Citizens United, Independent Expenditures, and Agency Costs: Reexamining the Political Economy of State Antitakeover Statutes

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We test the agency theory of corporate political activity by examining the association between the legality of independent expenditures and antitakeover lawmaking in the US states. Exploiting changes in state law regarding the use of corporate independent expenditures in the pre-Citizens United era, we estimate that a state is more likely to pass antitakeover statutes that entrench management when firms are allowed to make independent expenditures. We also find that this relationship is conditional on the competitiveness of a state’s electoral environment, suggesting that the threat of independent expenditures may move vulnerable legislators’ votes on less salient issues, such as corporate governance. These findings are robust to competing public interest and political economy explanations for antitakeover lawmaking, and they reveal that allowing independent expenditures may create additional agency costs for owners through public policy. Finally, these results strongly challenge the claim that state-level antitakeover laws are exogenous to firms’ activities. (JEL D72, G38, K20)

1. Introduction
The Supreme Court’s 2010 ruling in Citizens United v. Federal Election Commission (130 U.S. 876) 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 do not lead to the appearance or presence of corruption, meaning the state had no constitutionally compelling justification for limiting this spending. In addition to striking down the federal independent expenditure ban, *Citizens United* also effectively negated similar statutes in 23 of the 50 states by overturning its 1990 decision in *Austin v. Michigan Chamber of Commerce* (494 U.S. 652).¹

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. Beyond examining the potential electoral impacts of independent expenditure bans (e.g., Klumpp et al. 2013; La Raja and Shaffner 2014; Spencer and Wood 2014), little empirical work has explored what the policy effects of such bans are, however. This study offers an assessment of the potential effect of corporate independent expenditure bans on a public policy outcome of great interest to managers and owners: state-level antitakeover statutes. We find that when firms are allowed to make unlimited independent expenditures in a state, antitakeover statutes favor management. On average, when a state allows corporate political expenditures, it passes one additional antitakeover statute than it does when it bans these expenditures. Given that the mean state had 2.5 antitakeover statutes, this is a substantively significant effect. To identify this effect, we exploit variation within state laws on corporate independent expenditures during the height of the antitakeover era (1986–2001) by analyzing a dataset that covers the 38 states that had not adopted a corporate independent expenditure ban prior to 1986. We find further that this relationship is moderated by electoral competition.

These results are supportive of an agency cost interpretation of corporate political activity (CPA). Broadly, agency theory argues that managers use firms’ resources in a self-interested manner and extract rents from shareholders (Jensen and Meckling 1976). Within the realm of CPA, scholars argue that managers introduce agency costs not only by using more firm funds than are optimal in the political process but by using these funds to pursue public policies that benefit management. Antitakeover laws are perhaps the most prominent manifestation of such public policies. As Mallette and Spagnola (1994) document, following the invalidation of first-generation antitakeover statutes by the Supreme Court in 1982 in *Edgar v. MITE Corp.* (457 U.S. 624), several states responded by passing second-generation statutes. These statutes were upheld by the Court’s 1987 decision in *CTS Corp. v. Dynamics Corporation of America* (481 U.S. 69). The wave of adoptions that we analyze occurred across the states in the late-1980s and 1990s. If these statutes were the product of

¹ The Court made clear that the *Citizens United* decision applied to state laws in June 2012 through its *per curiam* opinion in *American Tradition Partnership, Inc. v. Bullock* (567 U.S. ____).
corporate pressure and also increased agency costs for shareholders, their adoption provides evidence for an agency interpretation of CPA.

Although we focus on a specific empirical manifestation of corporate political influence, our results speak to several broader debates and literatures. First, studies of the potential electoral consequences of *Citizens United* have reached mixed conclusions, but we find a substantial effect for corporate independent expenditure bans in the specific policy realm of corporate governance, which may help tailor responses at the firm level. Second, and as we discuss further in the next section, unlike most of the existing studies offering agency theory as an explanation for CPA, we provide a clear test of the framework’s expectations by examining both a source of campaign spending that is unambiguously subject to agency costs as our independent variable of interest and an outcome that also is a concrete manifestation of agency problems as our dependent variable. Third, beyond these contributions to the campaign finance literature, we also add to the field of corporate governance in two ways: (i) we revive and extend the literature on the political economy of antitakeover statutes, and (ii) we document that state-level antitakeover statutes are not exogenous to firms’ activities but rather are strongly and positively associated with various forms of CPA, including independent expenditures and lobbying efforts.

The rest of the article proceeds as follows. Section 2 provides additional background on agency theory as it relates to CPA and antitakeover statutes and articulates our theoretical expectations. Section 3 introduces our data, identification strategy, modeling approach, and hypothesis. Section 4 presents our results, and Section 5 provides two robustness checks of these results. Section 6 discusses the implications of our findings and concludes.

2. Agency Theory, Campaign Finance, and Antitakeover Statutes

We begin this section by briefly reviewing the literatures on agency theory and CPA, as well as agency theory and antitakeover laws. We then develop our theoretical argument regarding how corporate independent expenditures affect antitakeover lawmaking.

2.1 Agency Theory and Corporate Political Activity

Three theories have been offered to explain CPA. The first argues that firm engagement in politics is a strategically motivated investment that benefits firms and industries (e.g., Snyder 1990; Grier et al. 1994). This view suggests that firms’ patterns of giving reflect rational behavior to the point that the marginal cost of giving equals its marginal benefit. The second theory views campaign contributions as more akin to a classic consumption good for executives than as an investment by the firm since individuals’ monies fund corporate political action committee (PAC) contributions (Ansolabehere et al. 2003). Firms’ giving is thus neither
particularly beneficial nor terribly costly according to these authors, as their contributions are unlikely to have an impact on public policy or firm value since they constitute such a small share of the contribution pool (Ansolabehere et al. 2005). The final theory of CPA claims that firms gain little from their political engagement and argues that campaign contributions and other expenditures related to political activity represent agency costs generated by managers engaging in politics at their shareholders’ expense (e.g., Chappell 1982; Keim and Zardkoohi 1988; Aggarwal et al. 2012; Coates 2012; Hadani and Schuler 2012).

With regard to campaign finance regulation, agency costs have long been a concern. As Winkler (2004) and Mutch (1988) argue, many early regulations of CPA—including the ban on direct contributions from firms to candidates instituted by the Tillman Act of 1907—were motivated by the agency costs exposed through investigations into the role of money in politics in the Progressive Era. These historical accounts are consistent with studies of modern CPA and the freedom managers aim to secure by engaging in it. For example, Vogel (1978) notes that “The criterion by which business evaluates government policy has remained quite firm: does the proposed intervention strengthen or weaken the autonomy of management?” (51).

Despite the qualitative evidence in favor of an agency interpretation of CPA, most of the quantitative evaluations of the theory have failed to test it properly. Specifically, they have used invalid measures of agency costs as their key independent or dependent variable (or both). With regard to the former, the early studies in this vein developed with reference to corporate PACs since data on these entities were easily available, but PACs are legally segregated from their corporate sponsors and raise funds to contribute to candidates from a restricted class of individuals connected to the firm but not from the corporate treasury itself, meaning that PAC contributions cannot be considered agency costs. Only a few studies (Aggarwal et al. 2012; Coates 2012) have used measures of CPA (e.g., pre-2002 soft money contributions, lobbying expenditures) that actually come from the corporate treasury. These latter studies, however, largely use financial outcomes as their dependent variable, which implicitly assumes that if CPA does not increase shareholder value, it is wasteful and thus an agency cost. This use of financial outcomes can be problematic though, as much of CPA, including lobbying, is defensive in nature and designed

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2. In a speech given in the weeks following the decision in *Citizens United*, US Supreme Court Associate Justice Clarence Thomas advanced an alternative interpretation for the passage of Tillman Act. Thomas suggested that Tillman, a Democratic Senator from South Carolina, proposed the legislation not to combat corruption or address agency issues but to counter the influence of what he perceived to be the racial progressivism of corporations and their Republican allies (Liptak 2010). However, Winkler’s (2004) evidence both at the federal level and in states that passed campaign finance legislation before the federal government (e.g., New York) shows that scandals involving the use of “other people’s money” were typically the proximate cause of such legislation.
to preserve existing rents rather than to seek new ones (see, e.g., Baumgartner et al. 2009). Coates’ (2012) examination of managerial perquisites, such as jet use, to capture agency costs is an exception in this regard.

Despite these weaknesses in the agency theory CPA literature, we argue that this perspective is particularly important for understanding corporate behavior in environments like the one created by *Citizens United*. In contrast to PAC contributions, corporate independent expenditures come directly from corporate treasuries. They may heavily outweigh the amounts corporate PACs can give to individual candidates and the amount they contribute overall, which increases the risk that managers will raise their firms’ political involvement to levels beyond what is necessary to produce favorable outcomes for shareholders. Such a result would not square with the investment or consumption views of CPA. Furthermore, the use of or threat to use independent expenditures to secure public policies that protect management decision making from shareholders’ control is an outcome variable that is directly related to agency costs. Before articulating the logic of this relationship further, we briefly review how antitakeover statutes create agency costs.

### 2.2 Agency Theory and Antitakeover Statutes

As Barzuza (2009) notes, antitakeover law is one of the most studied and debated topics in corporate law. The existing literature focuses on questions related to interstate competition for such laws and the wealth effects of these laws on shareholders. The first of these bodies of literature is most relevant to our investigation. With regard to interstate competition, on one side of this debate, scholars argue that interstate competition leads to a race to the bottom, with states enacting antitakeover statutes that favor management at the expense of owners in order to raise revenue from franchise taxes (see, e.g., Cary 1974; Bebchuk 1992; Bebchuk and Ferrell 1999; Bebchuk et al. 2002; Subramanian 2004). On the other side, a second set of scholars argue that state competition appears to aid shareholders because capital markets pressure states to adopt quality law or see their domiciled corporations flee (see, e.g., Winter 1977; Romano 1985; Easterbrook and Fischel 1991; Romano 2006).

Those suggesting a race to the bottom argue that antitakeover statutes lessen the monitoring power of the takeover market (Manne 1965) by erecting artificial barriers that are not designed to extract additional value from bidders but rather to entrench managers. In turn, these statutes decrease incentives for managers to run their firms efficiently and thus create agency costs. Subramanian (2002) states that there is robust

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3. This is a necessarily brief summation of this literature; for a recent, extensive literature review on this topic, see, Davidoff (2012). Davidoff’s review covers the wealth effects of these state laws, as well as the research on the adoption and value effects of firm-specific, self-imposed antitakeover provisions (see, e.g., Gompers et al. 2003; Bebchuk et al. 2009).
evidence that antitakeover laws increase managerial agency costs with regard to incorporation decisions, and Bertrand and Mullainathan (1999) contend that these laws facilitate managers tunneling firm resources to themselves in the form of greater compensation.

There is nonetheless a debate as to whether the empirical evidence supports this view. Although Coates (2000) grants that the application of agency theory to antitakeover provisions is straightforward, he argues that the empirical literature on whether or not such laws introduce agency costs is not particularly persuasive, in part due to methodological issues. Principally, Coates argues that because of the way some antitakeover statutes interact and because of the fact that poison pills render most of the other statutes’ effects irrelevant, analyzing them as a package or concentrating on statutes other than those authorizing poison pills may be unnecessary. However, these statutes are nonetheless significant because they entrench management in ways that it cannot achieve through charter provisions alone (Matheson and Olson 1991), and as Bebchuk and Cohen (2003) note, these statutes are important both for what they actually measure and for what signals legislatures send in passing them. While the quality of this signal may be subject to manipulation by courts through statutory interpretation or degrade over time (Daines 2002), we are less concerned with this potential effect since we are studying the contemporaneous decisions of firms to lobby or campaign in favor of such statutes.

To address Coates’ concern regarding poison pills, we examine patterns specific to the adoption of laws authorizing this provision alone as a robustness check.

2.3 Independent Expenditures, Agency Costs, and Agency Outcomes

Despite this vast body of research, few studies examine the determinants of antitakeover laws. Although Barzuza (2009) makes an important contribution by examining the development of antitakeover case law across the states, her study focuses only on case law and not the enactment of the statutes that these state courts are interpreting. We are aware of only one prior published quantitative study on the determinants of legislative adoption of takeover statutes (Romano 1987), and that article does not consider the role that independent expenditures may have played in the passage of such laws. We reexamine the political economy of antitakeover statutes for two reasons. First, antitakeover statutes are a clear example of a policy outcome that writes agency problems into law, allowing us to use them as a dependent variable to test the agency theory of CPA.

4. Several additional studies provide qualitative examples of the adoption processes behind individual statutes (see, e.g., Butler 1988; Roe 1993), and Carney (1998) provides an excellent discussion on the diffusion patterns of corporate law generally and antitakeover provisions, specifically, across the states. With regard to the latter, Carney also identifies the “corporate sponsor” of several individual antitakeover statutes in various states. Additionally, Sitkoff (2002) engages in a theoretical discussion of the relationship between the regulation of political activity at the state level and the market for incorporation.
Second, the bulk of antitakeover statute lawmaking occurred after Romano’s study, suggesting that the dynamics she uncovered using only data from the mid-1980s might not fully capture the forces at play in this policy domain over time. In this subsection, we summarize Romano’s findings and offer a theoretical rationale for the existence of a negative relationship between a ban on corporate independent expenditures and antitakeover law passage.

Romano’s study of the political economy of state antitakeover statutes uses a cross-sectional model of the 50 states, with a dependent variable that captures the state’s rank order in adopting a second-generation antitakeover statute. Her focus is on determining which is more likely to explain the order in which states adopt antitakeover statutes: a broad coalition of political interests or splits within the business community created by the presence of domestic acquirers. She finds evidence that is more consistent with the latter explanation but cannot rule out the former. That is, the presence of acquiring firms in a state appears to be the key factor that affects the order in which a state adopts an antitakeover statute, but political factors may also have a marginal and positive effect on the adoption of such statutes. As a result of these findings, along with qualitative evidence on the adoption of such laws in Connecticut and other states, her results have been interpreted to suggest that antitakeover laws can be viewed as exogenous to the average firm’s political activity (see, e.g., Bertrand and Mullainathan 2003; Gompers et al. 2003; Armstrong et al. 2012).

In addition to the issues that arise from Romano’s study occurring at the beginning of the antitakeover wave, there are a number of other factors that may limit its current applicability. First, the model provides a simplified view of political activity, using only one categorical measure of the level of organized politics in the state. Such an approach is inadequate for capturing the diverse channels that firms, unions, and individuals have to influence state legislators. Second, the study’s dependent variable does not capture the degree of antitakeover lawmaking since only a simple binary indicator underlies the ordinal measure. Finally, the study’s data do not include a time dimension, so it cannot identify how changes in the political environment may have altered the incentives of legislators to enact antitakeover statutes.

Our study addresses many of these issues by analyzing a panel data set of 38 states over 15 years that includes a more richly informed set of covariates related to political activity and by examining a broader set of antitakeover laws that states can adopt. Nevertheless, our focus remains on whether or not allowing the use of independent expenditures affects antitakeover lawmaking in a state. To model this potential relationship, instead of conceptualizing the political economy of antitakeover laws as either the product of active lobbying by a coalition or of an attempt to “put out fires” created by divisions between acquirers and targets within
the business community, we view each state’s set of antitakeover laws as a product of supply and demand forces.

As Romano (1987, 1988) notes, there are a variety of factors that may affect a state’s willingness to supply antitakeover legislation. Principally, these include worsening economic conditions and the presence of an amateur state legislature, both of which should increase the probability of adoption. To these two, we would add increased interstate competition as a factor likely to increase adoption, and increases in the size of the state’s pension fund as a factor likely to decrease adoption. To the degree that the number of active takeover attempts of domestic firms may affect any of the above determinates, we might also consider it a supply-side factor, but it certainly has demand-side elements to it as well, as targets may mobilize politically to protect themselves via public policy.

The demand side dynamic of antitakeover laws is where both Romano (1987) and we focus. In particular, we are interested in how the availability of different political instruments affects the adoption process: that is, how do the different legal channels that allow unions, firms, and individuals to participate in politics affect the probability of statute adoption? Consistent with the broad-based coalition argument, we would expect that for both unions and firms, the more opportunities they have to participate, the more likely state legislators will be to pass antitakeover laws. For firms alone, ultimately, management will politically demand favorable antitakeover laws from state governments because advocating for these laws represents a less costly substitute for management (due to switching costs associated with public perception and time) to reincorporating in a new state with such laws already in place. In the remainder of this section, we further explain this logic, and we formally state our hypothesis in the next section.

The existing literature on corporate influence in politics suggests that business is likely to have its greatest impact in policy subsystems that are of low salience to the public (Hall and Wayman 1990; Smith 2000). For example, one study asserts that legal activity at the state Supreme Court level rarely attracts public attention and that campaign contributions appear to correlate with legal victories for firms in trials before courts when the courts’ membership is elected through a partisan process (Kang and Shepherd 2011). With regard to corporate governance, Romano (1988) found in a study of public opinion on corporate takeovers that even when this issue was at peak activity and heavily covered in the media, it remained of low public salience.

5. The availability of different jurisdictions complicates our welfare analysis; however, this is only to a matter of degree. Regardless of whether management reincorporates a firm in a different state (a process the board and shareholders have control over, unlike political activity) or management convinces a state’s legislature to pass antitakeover statutes, shareholders bear agency costs because management will be more entrenched in either scenario.
Given these circumstances, we argue that management’s ability to use or threaten to use independent expenditures could potentially play a role in persuading legislators to vote in favor of antitakeover statutes that entrench them. Independent expenditures are not subject to the limits that corporations face in the few states that allow direct corporate contributions, and unions may also support such laws but have fewer total funds to advocate for them. We therefore expect that allowing corporate independent expenditures has a larger impact than other campaign finance regulations in producing management-friendly public policy. Conversely, banning such expenditures would insulate legislators from potential managerial pressure to a greater degree. Activist investor Carl Icahn recently articulated a similar argument in a *Wall Street Journal* opinion column in which he argued that, “Years of lobbying by pro-management groups in state legislatures produced a thicket of laws that protect the impregnability of boards and CEOs” (Icahn 2013).

Additional anecdotal evidence suggests that these dynamics are plausible within the realm of antitakeover lawmaking. Romano’s quantitative analysis produced no evidence in favor of corporate political influence in antitakeover lawmaking; however, in her case study of Aetna’s lobbying for Connecticut’s fair price statute, several of her interviewees suggested that the rising costs of campaigns for office and the availability of corporate money made legislators more open to making pro-management votes. Although there is no specific discussion of independent expenditures in her study, we note that Connecticut did not adopt an independent expenditure ban until 2000, meaning that the potential use of independent expenditures could have been a factor in legislators’ decisions in the mid-1980s.

A similar legislative battle over antitakeover legislation played out in the 1980s in New York State, which also did not prohibit independent expenditures. The political pressure on legislators, including reported threats of electoral consequences, led the vice president of the state-level chamber of commerce to state that “This town [Albany] hasn’t seen this type of pressure in years” (Scott 1985). Ultimately, and in the view of some observers in part because of this pressure, the state legislature effectively banned takeovers funded by “junk” bonds.

Finally, in the battle over Delaware’s business combination statute in 1988, forces on both sides of the fight employed a variety of political tools. Activist investor T. Boone Pickens used independent expenditures to air television advertisements that encouraged citizens to lobby their legislators in opposition to the statute, and in response, the CEOs of two prominent Delaware firms (DuPont and Hercules) lobbied legislators (Hays 1988; Yang and Weber 1988). Pickens also reports that he learned in a conversation with the governor of Delaware that in addition to DuPont, which has a large physical presence in the state, other firms without a substantial domestic presence (e.g., General Motors,
Boeing, Texaco) exerted strong political pressure on the governor to sign the bill into law (Yago 1990). That Delaware’s lawmakers’ experience may have exhibited this level of political activity is particularly important to highlight for two reasons. First, Delaware has just under a 50% market share in incorporations of publicly traded firms (Barzuza 2009; Fisch 2000), meaning that its laws have broad economic implications. Second, the involvement of an array of firms and investor interest groups makes clear that Delaware’s antitakeover statute was not the product of a single firm’s demands. To see whether this experience was typical and whether or not corporate independent expenditure laws make a difference for antitakeover lawmaking, we turn to our quantitative analysis.

3. Data and Methodology
We investigate the effect of independent expenditure bans on the adoption of antitakeover statutes between 1987 and 2001. Although our sample period ends in 2001, this time series captures the most recent active period in the US market for corporate law (Bebchuk and Ferrell 1999). In this section, we introduce our dependent variable, our identification and modeling strategies, and our hypothesis and control variables.

3.1 Dependent Variable: Antitakeover Statutes and their Adoption
We use the Bebchuk and Cohen (2003) antitakeover statute index as our dependent variable. The dataset includes six variables, but as an index it only sums to five since two of the variables tap the same underlying legal concept (a business combination limitation) but differentiate on how strictly the state applies the concept (in number of years). The index has a Cronbach’s alpha 0.749, and a principal components analysis shows that only the first component has an Eigenvalue (2.51) greater than 1. Both of these statistics indicate that the index is strongly internally consistent. The index contains 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,
- other constituencies statute.

6. Our data series begins in 1986, but since we difference and lag our variables, 1987 is the first year we model.
Figure 1 plots the aggregate trends in adoptions for each of these types of statutes and reveals an early and rapid increase in adoptions and a leveling off after 1992.

3.2 Timing of Independent Expenditure Bans

We identify the effect of corporate independent expenditure bans based on changes to state laws. Table 1 reports which states had such bans, and the year they were first in place. During our sample period, seven states opted to ban unlimited corporate independent expenditures. To properly identify the effect of a ban, we exclude the 12 left-censored states that had such bans in place in or before 1986, leaving us 38 states to observe in a time-series cross-section approach. We coded a binary variable indicating whether or not in each state-year a corporate independent expenditure ban was in place using data from the National Conference of State Legislatures (NCSL).

Actual levels of corporate independent expenditures or corporate contributions to independent expenditure committees would be a more richly informed treatment variable, but we argue 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 38 states over 15 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 regulation of the campaign finance environment broadly captures the argument that business’ power stems not just from its actual political spending but also its potential spending and the threat of electoral intervention (Lindblom 1977). As we will show later, the threat of firms spending against vulnerable legislators appears to be a significant mechanism in shaping antitakeover legislation.

3.3 Error Correction Model

We ran an error correction model (ECM) using ordinary least squares regression with panel corrected standard errors since we are available to observe the entire population (all 50 states). Error correction modeling is a

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7. As Bebchuk and Cohen (2003) note, the vast majority of these statutes were adopted prior to 1992. All of the results we present here are robust statistically and substantively to limiting our analysis to the 1987–1992 time period.

8. 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 regression predicting whether a state had a ban in place prior to the start of our time series likely would not be correlated with the error terms of our regressions in Tables 3 and 4, as the second generation laws we model in our dependent variable were only definitively permitted by the Supreme Court in *CTS Corp. v. Dynamics Corp. of America*, which was decided in 1987, the same year our sample period begins. This result is largely due to our fixed effects approach in which each state serves as its own control. Nevertheless, we note here that including these 12 states would not change the statistical or substantive conclusions we reach but would weaken our identifying assumptions.
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. In terms of their substantive interpretation, the differenced measure captures the short-run adjustment caused by a change in the variable, and the lagged term, in combination with the lagged value for the dependent variable, captures the long-run dynamics of the relationship (Yasar et al. 2006). We chose to use an ECM over a simple differences-in-differences approach to account for the reality that even sharp reforms to electoral systems typically have effects that unfold over several years, as candidates and interest groups adjust their strategies in response to changes in electoral and campaign finance laws (see, e.g., Meirowitz 2008). This factor, combined with interstate competition in corporate governance, suggests that both short- and long-terms processes explain antitakeover lawmaking.

Following Wilson and Butler (2007), we conducted various diagnostic tests to arrive at our final model specification. Although the ECM captures the dynamics of our data, it does not take account of whether or not unit effects, period effects, autocorrelation, or heteroskedasticity need addressing. First, through a series of Hausman 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 we omit these coefficients, 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.\footnote{Since including a lagged dependent variable can lead to inconsistent estimators (Wooldridge 2002), especially when fixed effects are also present (Angrist and Pischke 2008), we note that in unreported specifications in which we dropped the lagged dependent variable, our results remain unchanged. For purposes of properly interpreting the substantive effects identified by the ECM though, we present the specifications that include the lagged dependent variable.} 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 specifications. Finally, tests revealed that our panel-corrected standard errors were heteroskedastic; we corrected for this violation by using panel corrected Huber-White robust standard errors.

\footnote{Data collected form the National Conference of State Legislatures. “ - ” denotes that the state did not implement a corporate independent expenditure ban prior to such bans being invalidated by Citizens United. States in italic\textit{s} were dropped from our sample since they adopted a ban prior to the beginning of our sample period; states in bold are those that we used for statistical identification since they adopted a ban during our sample period.}

Table 1. Timing of State Bans on Corporate Independent Expenditures

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<th>State</th>
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<td>Minnesota</td>
<td>1988</td>
<td>West Virginia</td>
<td>1908</td>
</tr>
<tr>
<td>Mississippi</td>
<td>-</td>
<td>Wisconsin</td>
<td>1973</td>
</tr>
<tr>
<td>Missouri</td>
<td>-</td>
<td>Wyoming</td>
<td>1977</td>
</tr>
</tbody>
</table>
Our estimated model in equation (1) is:

$$\Delta y_{it} = \lambda y_{it-1} + \beta \Delta X_{it-1} + \psi X_{i,t-1} + \alpha_i + \gamma_t + \tau_{it} + u_{it}$$

(1)

where, $\Delta y_{it}$ equals the change in the antitakeover index between year $t$ and year $t-1$, $y_{it-1}$ equals the antitakeover index in year $t-1$, $X$ represents a vector of independent variables that includes our corporate independent expenditure ban and additional controls described below, $\alpha_i$ captures our unit (state) effects, $\gamma_t$ captures our period (year) effects, $\tau_{it}$ captures a state-specific time trend, and $\mu_{it}$ is our error term.

3.4 Hypothesis

Corporate independent expenditure ban adoptions at the state-level have historically been justified as anticorruption measures. For example, Montana’s brief in defense of its ban in American Tradition Partnership, Inc. v. Bullock discusses how its ban resulted from the belief that mining interests historically dominated the state government. Additionally, in that case, 22 states and the District of Columbia filed an amicus brief advancing the argument that two anti-corruption justifications were behind the passage of their laws: (i) a desire to preserve the independence of elected members of the judiciary (a concern that did not apply at the federal level in Citizens United), and (ii) a desire to keep “foreign” (i.e., non-in-state) money out of state elections. This second justification is particularly important to highlight, as it suggests that anti-corruption sentiments and not anti-business attitudes led to the passage of these laws. Anecdotal evidence from states that adopted these bans during our sample period is also consistent with this justification: Iowa adopted its ban in 2003 in the wake of a national group highlighting how lax its campaign finance laws were (Eby 2004), and Connecticut adopted its ban in 2000 as part of a reform package that was motivated by a corruption scandal involving the state treasurer (Santora 2003).

Substantively, since both the dependent variable and the independent variable are legislative creations, it is unlikely that the relationship between the two is endogenous. That is, it is unlikely that trends in antitakeover statutes, which are purposefully chosen by the legislature and focus on changes in firm control, pressure these very same actors to change campaign finance law. Nevertheless, we take several steps to address the potential threat of legislative endogeneity. First, in Section 4.1, we conduct a test for reverse causality, which provides no evidence to suggest that changes in antitakeover laws drive campaign finance reform. Second, our state fixed effects and state-specific time trends should address concerns of causal simultaneity. The former capture omitted time-invariant or slow-moving variables that might affect the adoption of both our dependent variable and our key independent variable, and the latter capture similar across-time trends within each state. Finally, our main robustness check,
randomization inference, helps address concerns regarding selection on observables (see, e.g., Sekhon 2009).

As a result, we formally identify the treatment effect (ψ or β_{CorpIEBan})—that is, the average effect of a ban on corporate independent expenditures on the difference in the antitakeover index within a state—in equation (2) as:

$$
\beta \text{ or } \psi_{CorpIEBan} = E[\Delta y_{it}|y_{it-1}, \alpha_i, \gamma_t, \Delta X_{it}, X_{it-1}, \text{CorpIEBan}_{it} | \text{CorpIEBan}_{it-1} = 1] - E[\Delta y_{it}|y_{it-1}, \alpha_i, \gamma_t, \Delta X_{it}, X_{it-1}, \text{CorpIEBan}_{it} | \text{CorpIEBan}_{it-1} = 0]
$$

(2)

Testing whether or not these coefficients for a ban on corporate independent expenditures equal 0 provides a test of whether or not the presence or threat of independent expenditures affect our dependent variable, and depending upon which coefficient (or both) is significant, we will also know if the effect is immediate, more gradual, or has elements of both adjustments. We hypothesize that if banning independent expenditures reduces agency costs by limiting the potential for managers to use corporate resources to alter public policy in their favor, then when a state adopts such a ban, its antitakeover lawmaking should shift in favor of shareholders.

3.5 Control Variables

In addition to our treatment variable, we included controls for other state-level factors that might affect the demand for and supply of antitakeover statutes within a state. First, to fully capture the demands that the campaign finance regime may allow to be made, we included controls for whether the state had an independent expenditure ban that applied to unions, as well as whether the state allowed contributions to candidates from either corporations or unions.10 The data for these three variables came from the NCSL. We included the corporate contribution ban to capture other avenues for firm electoral influence, and we included variables capturing union regulation since unions are believed to have supported antitakeover statutes on the grounds that they protected jobs from being shed due to the downstream consequences of the buyouts of local firms by out-of-state actors (Romano 2006).11

10. Of the 23 states that had corporate independent expenditure bans, only 14 also banned union independent expenditures; further, the adoption of these bans did not always occur at the same time in states with both restrictions.

11. The increased political salience of corporate “downsizing” during the 1992 presidential election may have fractured the fragile alliance between unions and management on issues of corporate governance (see, Murphy 1995). In the split time sample models (pre- and post-1992) we describe in footnote 7, we found that although union independent expenditure bans remained insignificant in both time periods, the percent of the state workforce that was unionized (an additional control introduced in the next paragraph) was only positive and significant in the earlier sample. That is, this effect for 1987–92 is responsible for the result for union membership in the reported models that capture the entire sample period. Additionally, in the 1993–2001 model, allowing unions to make direct contributions is associated with the
In addition to including these binary variables, we also calculated an index of campaign finance laws using data from Primo and Milyo (2006). We construct a 0–4 index that is a sum of the presence/absence of the following five campaign finance provisions: limits on individuals’ contributions to candidates, limits on organizations’ contributions to candidates, partial or full public funding for legislative candidates, and partial or full public funding for gubernatorial candidates. To the degree that more regulation insulates public policymakers from political pressure, we would expect higher values of the scale to be associated with less antitakeover lawmaking.

Unions and firms may have demanded changes to antitakeover laws through non-electoral means as well. For unions, their membership is a significant source of political power (Francia 2006), so we included the annual percentage of each state’s nonfarm-based workforce that was unionized, using data collected by Hirsch and Macpherson (2003) and updates on their website. For firms, to capture other means by which management might attempt to influence policy, we constructed the log of the number of business associations registered to lobby in each state-year using data on association founding dates gathered by Spillman (2003). 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 as a result, the logged count of associations will better reflect their potential impact on policy making without producing a right skew in the variable’s distribution. Consistent with prior expectations, we expect that both of these variables will be positively associated with changes in the antitakeover index.

Management may be more likely to demand and a state more likely to supply antitakeover legislation when antitakeover activity increases. This relationship is expected by the extant literature and is supportive of the adoption of fewer antitakeover statutes. Although these results are not as well identified as our main analyses, they suggest that unions reversed their position on the value of antitakeover legislation in the early 1990s. These results do not affect the conclusions we reach regarding the effects of corporate independent expenditure bans.

12. Spillman collected these data from early-2000s editions of the Encyclopedia of Associations, National Trade and Professional Association Directory, and Associations Yellowbook 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 active in the 1980s 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, which would bias the estimated coefficient for this variable toward zero.
idea that many antitakeover statutes are the result of firm-specific demands when prominent firms fear being acquired or are an actual target (Butler 1988; Romano 1988; Roe 1993; Carney 1998). We controlled for this competing explanation by including the annual logged count of the number of active hostile takeover targets in the state using data we gathered from the Spectrum SDC database. If this “putting-out-fires” explanation is correct, then this variable should be positively associated with changes in the antitakeover index.

In addition to these demand-side variables, we included six variables that might affect how willing a state’s government is to supply antitakeover laws. First, the historical association between managers and the Republican Party might lead us to expect that states with liberal policy histories to be less friendly to management. However, to the degree that unions also may support such laws, ideology may play little role in policy adoption. Thus, given these potentially counteractive effects, we areagnostic as to the direction of liberalism’s potential effects on antitakeover lawmaking. To control for each state government’s predominant ideology, we used the NOMINATE version of Berry et al.’s (1998) institutional ideology score by state-year. This measure computes the relative liberalism and conservatism of the Democratic and Republican parties in each state based on roll-call voting records, and then weights these scores by the degree of Democratic control of the state legislature and the governorship. These common space ideological measures are then also pegged to national politics, obviating the need to adjust for the considerable ideological variation across state parties. The resulting variable ranges from 0 to 1, with higher values reflecting a more liberal environment.

A factor that may decrease a state’s willingness to supply antitakeover statutes is legislative professionalism. We measured legislative professionalism using Squire’s (2007) index of professionalism, which is based upon legislator salary, legislative staffing levels, and legislature time-in-session, and was calculated twice during our sample period, in 1986 and 1996. We expect more professional legislatures to be less susceptible to managers’ demands, as they are more likely to have a network of expertise upon which to draw, as well as offices within the legislature that can provide research and analysis.

An additional brake on a state’s willingness to supply statutes may come from within, in the form of lobbying by the state’s pension plan. To the degree that the state itself has ownership stakes in domestic firms or does not want to encourage a race to the bottom between states, it may be less inclined to increase agency costs by entrenching managers. Further, this reluctance likely increases with the size of the pension fund, as non-litigation activism of pension funds increases as they have more assets under management (Choi and Fisch 2008). Thus, we included the

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13. Since state policy liberalism may track with union preferences, we note that this control is not statistically significant in either of our split time sample models.
annual log of the market value of corporate stocks held by the state pension plan, using US Census data.

Additionally, we controlled for factors that might increase a state’s willingness to enact antitakeover statutes. States may be willing to pass such laws when the local economy is underperforming (Romano 1987). Thus, we included annual measures of the percent of adults over 18 years in the state who were unemployed, as well as the logged value of the state’s gross domestic product, using data from the Bureau of Economic Analysis.

The last variable we included forced us to estimate a second specification in which we drop our period fixed effects. As noted in the literature review, antitakeover laws may result from interstate competition or competitive federalism. Although we are agnostic as to whether there is a race between the states and, if so, what direction it goes in, we included a by-year, nationwide average antitakeover index value to capture this potential dynamic. We used a nationwide index of antitakeover statutes—computed as the 38-state average of the antitakeover index—because the decision of where to incorporate, as opposed to where to locate, rarely involves physical considerations for firms.¹⁴ The internal affairs doctrine in corporate law leads each state to respect the governance structures of firms incorporated in other states.

We provide descriptive statistics for all of these variables in Table 2. These statistics demonstrate substantial variation in the regulation of campaign finance concerning corporations, unions, expenditures, and contributions. They also display substantial variation in the adoption of antitakeover statutes.

4. Results and Extensions

We present first the results for our estimation of equation (1); we then turn to two extensions that build on these results and provide a fuller picture of the dynamics of antitakeover lawmaking.

4.1 Main Findings

Consistent with our hypothesis, we find significant and robust results for a corporate independent expenditure ban on the friendliness of antitakeover laws toward management. As Table 3 reveals, the lagged presence of a corporate independent expenditure ban in a state has a negative (for management) impact on the antitakeover index, whether we control for the nationwide antitakeover index directly or indirectly through period

¹⁴. Were we to use an antitakeover law index based upon neighboring states alone, our central findings would remain unaffected, and the variable capturing interstate competition would remain statistically significant (but its substantive magnitude would be reduced substantially). Additionally, the inclusion of controls for the adoption of an independent expenditure ban in a neighboring state produces insignificant coefficients and does not affect our central findings.
effects. Substantively, this long-run average effect is comparable to the legislature producing 1 less pro-management statute when a ban is in place, and given that the average state had 2.5 such statutes on the books during our sample period, this is an economically significant effect that does much to decrease potential agency problems.

This effect for banning corporate independent expenditures is the only campaign finance effect we uncover, which strengthens our agency cost interpretation. There are no significant associations between antitakeover laws and bans on corporate contributions (which likely are too small to have an effect on legislators’ votes or firm value) or bans on union contributions and independent expenditures, nor is there an association between these laws and the restrictions captured by the campaign finance index.

The other statistically significant variables are all consistent with our expectations. In the short run, antitakeover lawmaking is positively associated with increases in business’ lobbying activity, unemployment, the number of domestic hostile targets, and union membership. The last two of these variables also have longer-run effects, suggesting that they may have more substantively important associations with antitakeover lawmaking than the immediate demands politicians face due to increased lobbying or a weakening economy. Additionally, the positive and significant association for union membership, in contrast to the lack of results for the union-related campaign finance variables, suggests that union strength is better assessed in terms of membership levels than how favorable campaign finance regulation is toward unions.

On the supply-side, there is evidence to suggest that states’ willingness to create antitakeover statutes is not associated with their policy liberalism or their legislative professionalism but that their role as investors is

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>SD</th>
<th>Min.</th>
<th>Max.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Corporate independent expenditure ban</td>
<td>0.10</td>
<td>0.30</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Union independent expenditure ban</td>
<td>0.09</td>
<td>0.29</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Corporate contribution ban</td>
<td>0.24</td>
<td>0.43</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Union contribution ban</td>
<td>0.09</td>
<td>0.28</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Campaign finance index</td>
<td>1.49</td>
<td>1.17</td>
<td>0</td>
<td>4</td>
</tr>
<tr>
<td>% Unionized</td>
<td>14.00</td>
<td>6.17</td>
<td>3.30</td>
<td>30.30</td>
</tr>
<tr>
<td>Lobbying business associations (logged)</td>
<td>0.84</td>
<td>0.61</td>
<td>0</td>
<td>2.32</td>
</tr>
<tr>
<td>State policy liberalism</td>
<td>0.51</td>
<td>0.24</td>
<td>0</td>
<td>0.98</td>
</tr>
<tr>
<td>Legislative professionalism</td>
<td>0.20</td>
<td>0.12</td>
<td>0.03</td>
<td>0.66</td>
</tr>
<tr>
<td>Pension plan stock holdings (logged)</td>
<td>6.12</td>
<td>1.06</td>
<td>0</td>
<td>7.67</td>
</tr>
<tr>
<td>Hostile takeover targets (logged)</td>
<td>0.13</td>
<td>0.24</td>
<td>0</td>
<td>1.11</td>
</tr>
<tr>
<td>% Unemployed</td>
<td>5.22</td>
<td>1.67</td>
<td>2.20</td>
<td>12.60</td>
</tr>
<tr>
<td>Gross state product (logged)</td>
<td>10.92</td>
<td>0.46</td>
<td>9.96</td>
<td>12.13</td>
</tr>
<tr>
<td>Average antitakeover index</td>
<td>2.51</td>
<td>0.49</td>
<td>1.08</td>
<td>2.84</td>
</tr>
</tbody>
</table>
associated with decreases in such lawmaking: That is, an increase in a state’s pension plan’s holdings in corporate stocks is negatively associated with antitakeover protections. Unlike the other associations, however, this last result is not robust to dropping our period effects and instead including a nationwide average antitakeover index. This change, along with the statistical and substantive significance of the average antitakeover index,

Table 3. An Error Correction Model of State Antitakeover Statutes, 1987–2001

<table>
<thead>
<tr>
<th></th>
<th>Estimate (SE)</th>
<th>Estimate (SE)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Antitakeover laws L</td>
<td>-0.400***</td>
<td>-0.398***</td>
</tr>
<tr>
<td>Corporate independent L</td>
<td>-0.439*</td>
<td>-0.445*</td>
</tr>
<tr>
<td>Expenditure ban D</td>
<td>0.243</td>
<td>0.250</td>
</tr>
<tr>
<td>Union independent L</td>
<td>0.359</td>
<td>0.360</td>
</tr>
<tr>
<td>Expenditure ban D</td>
<td>0.453</td>
<td>0.460</td>
</tr>
<tr>
<td>Corporate contribution ban L</td>
<td>0.197</td>
<td>0.200</td>
</tr>
<tr>
<td>D</td>
<td>0.096</td>
<td>0.094</td>
</tr>
<tr>
<td>Union contribution ban L</td>
<td>0.054</td>
<td>0.055</td>
</tr>
<tr>
<td>D</td>
<td>0.461</td>
<td>0.463</td>
</tr>
<tr>
<td>Campaign finance index L</td>
<td>-0.037</td>
<td>-0.038</td>
</tr>
<tr>
<td>D</td>
<td>-0.042</td>
<td>-0.039</td>
</tr>
<tr>
<td>Business associations lobbying (logged) L</td>
<td>1.551</td>
<td>1.534</td>
</tr>
<tr>
<td>D</td>
<td>7.351***</td>
<td>7.294***</td>
</tr>
<tr>
<td>Hostile takeover targets (logged) L</td>
<td>0.241**</td>
<td>0.223**</td>
</tr>
<tr>
<td>D</td>
<td>0.162*</td>
<td>0.152*</td>
</tr>
<tr>
<td>% Unionized L</td>
<td>0.039*</td>
<td>0.039*</td>
</tr>
<tr>
<td>D</td>
<td>0.035*</td>
<td>0.037*</td>
</tr>
<tr>
<td>% Unemployed L</td>
<td>0.019</td>
<td>0.018</td>
</tr>
<tr>
<td>D</td>
<td>0.057*</td>
<td>0.062***</td>
</tr>
<tr>
<td>Gross state product (logged) L</td>
<td>0.588</td>
<td>0.559</td>
</tr>
<tr>
<td>D</td>
<td>1.024</td>
<td>0.785</td>
</tr>
<tr>
<td>State policy liberalism L</td>
<td>-0.132</td>
<td>-0.132</td>
</tr>
<tr>
<td>D</td>
<td>-0.185</td>
<td>-0.174</td>
</tr>
<tr>
<td>Legislative professionalism L</td>
<td>0.641</td>
<td>0.513</td>
</tr>
<tr>
<td>D</td>
<td>0.753</td>
<td>0.920</td>
</tr>
<tr>
<td>Pension plan stock holdings (logged) L</td>
<td>-0.020</td>
<td>-0.019</td>
</tr>
<tr>
<td>D</td>
<td>-0.065*</td>
<td>-0.062</td>
</tr>
<tr>
<td>Average antitakeover index L</td>
<td>0.457***</td>
<td>0.457***</td>
</tr>
<tr>
<td>D</td>
<td>1.270***</td>
<td>1.270***</td>
</tr>
</tbody>
</table>

State fixed effects? Y Year fixed effects? Y State-specific time trend? Y

n (observations) 570 570
n (states) 38 38
r² 0.434 0.433

=L Lag term; D Difference term

* p < 0.10, ** p < 0.05, ***p < 0.01.

Error correction OLS model with robust panel corrected standard errors. Dependent variable is the differenced state antitakeover law index by state-year. The 12 states that adopted a corporate independent expenditure ban prior to 1986 (see Table 1 for a list) were excluded from our sample.
suggests that states’ willingness to resist antitakeover lawmaking is negatively associated with interstate competition. That is, although it is not the focus of our study, we note that the positive sign on the antitakeover index variable is supportive of a race-to-the-bottom interpretation of interstate competition, at least with regard to antitakeover provisions.

To check our identification strategy, and as a falsification test of these panel results, we ran a conditional 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 a 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 and not simultaneous with the level of its antitakeover index (Wooldridge 2006). The results of this regression for both specifications 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 in both cases). 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.

4.2 Extensions

We extended our main analysis in two ways. First, we conducted a lead-and-lag analysis around the timing of the independent expenditure ban. Second, and based upon the results of our lead-and-lag analysis, we examined the role of electoral competition in mediating the relationship between campaign finance regulations that allow corporate independent expenditures and the extent of antitakeover lawmaking in a state.

4.2.1 Lead-and-Lag Analysis. Table 3 reveals that there is a statistically significant relationship between the presence of a corporate independent expenditure ban and a state’s antitakeover index, but 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 3 and included 10 binary indicators to capture the four years leading up to reform, the year of reform, and the five years following reform in each adopting state.

Rather than report these results in a table, we present them in Figure 2, which plots the point estimates and 95% confidence intervals for these additional year indicators and illustrates that in the 10 years surrounding a ban adoption, there is a downward trend in antitakeover lawmaking, with a significant break coming during the year of a ban. Further, the significant changes in antitakeover laws around the adoption of a corporate independent expenditure ban occur prior to reform. That is, before corporate independent expenditures are banned, legislatures engage in policy making that makes corporate governance laws more favorable to
management. The coefficients that achieve statistical significance are those for –3 and –1 years from ban adoption, but the point estimates across all of the years reveal that once an independent expenditure ban is enacted, a state’s legislature appears to halt adopting antitakeover laws that favor management at shareholders’ expense. Although this trend might raise concerns regarding legislative endogeneity, we note again that there is little substantive or statistical evidence for this, as bans on corporate independent expenditures were justified by supporters as a way to combat corruption and that the falsification test presented in the previous section suggested that the causal arrow runs only from independent expenditure bans to changes in antitakeover lawmaking.

4.2.2 Competitiveness as Mechanism. Since our lead-and-lag analysis revealed that the adoption of an independent expenditure ban serves more to halt antitakeover lawmaking than actually reverse it, in this section we explore the mechanism that leads from allowing independent expenditures to management-friendly votes in legislatures. We hypothesize that legislators will take the threat of corporate independent expenditures more seriously as their electoral vulnerability increases: that is, electoral competitiveness is the mechanism behind the dynamics of this relationship and likely moderates it. To test this hypothesis, we made two changes to our model. First, given the results of our lead-and-lag analysis and for ease of interpretation, we reversed the measurement of all of our campaign
finance variables, coding them such that they equal 1 when firms/unions/individuals are not restricted and 0 when they are. Second, using the Klarner et al. (2011) state legislative election data set, we added an indicator that captured the percentage of seats in the lower chamber of the state legislature that were competitive (defined as the winner having received less than 60% of the vote in the general election) in the most recent prior election, as well as interactions between this variable and our binary indictors for whether or not corporate and union independent expenditures were allowed.15

The results of these re-analyses of both specifications from Table 3 are reported in Table 4. For space reasons, we report only those coefficients
relevant to this extension; the unreported results are consistent with Table 3. After adding electoral competitiveness to our model, the finding of an independent effect for a corporate independent expenditure ban does not persist. However, both our measure of electoral competitiveness and the interaction between competitiveness and allowing corporate independent expenditures are statistically significant, with increasing competitiveness negatively affecting state antitakeover policy making on its own but positively affecting antitakeover lawmaking when management can make unlimited independent expenditures.

To assess the substantive impact of these seemingly opposite effects, Figure 3 presents an interaction plot that shows the combined effect of the three variables (corporate independent expenditures allowed, electoral competitiveness, and the interaction), as we vary electoral competitiveness. The figure reveals that when the percentage of competitive seats surpasses 22%, the combined effect of the variables on antitakeover lawmaking is positive and statistically significant. Such a threshold is not unreasonable in terms of having a real impact. As the box plot of the observed values of electoral competitiveness below the figure’s x-axis reveals, just under 50% of observed cases saw electoral competition at this threshold or above it. This result demonstrates that electoral competitiveness significantly mediates the relationship between corporate independent expenditures and antitakeover lawmaking, with legislators being more prone to enact statutes favorable to management when they feel more electorally vulnerable and thus perhaps more threatened by the potential use of independent expenditures.

5. Robustness Checks

In this section, we discuss two robustness checks to our empirical findings.\footnote{We conducted a third robustness check in which we rotated out each treatment state and then re-ran our various specifications. Our findings held in these leave-one-out tests, ensuring that our results are not driven by only one treated unit.} The first checks test the robustness of the findings in all four specifications of Tables 3 and 4, and the second check analyzes the adoption of poison pill statutes alone by the states.

5.1 Randomization Inference

As Bertand et al. (2004) and Conley and Taber (2011) demonstrate, standard errors in models with a differenced dependent variable may be incorrectly estimated when there is either a small number of observational units in total or a small number of observational units that are treated. The specifications in Tables 3 and 4 have 570 observations each; in each, 514 are in the control group, and 56 are in the treatment group. These 56 observations represent the seven states that adopted a corporate independent expenditure ban between 1986 and 2001.
To address this criticism, we employed randomization inference (Rosenbaum 2002) on the subsample of 31 states that had not adopted a corporate independent expenditure ban by 1986 and did not do so between 1986 and 2001. To do so, for each specification, we randomly generated 5000 sets of seven placebo bans in this subsample of 31 states and then re-estimated the specification on each of these 5000 sets. We assigned our seven placebo bans within each set such that their overtime distribution matched those of the seven states that changed their laws regarding corporate independent expenditures during this period (following, Frakes 2013). In addition to addressing issues related to the number of overall units and treated units, randomization methods ease concerns regarding selection on observables (Sekhon 2009) and can also produce higher quality inferences than those made using clustered standard errors based upon standard asymptotic assumptions (Barrios et al. 2012). This latter improvement is especially likely to occur when standard errors may be correlated not only within but also between clusters, as may be the case with state-level data.

The four panels in Figure 4 plot histograms of the coefficients we estimated via the 5000 regressions for each of the four specifications. Each
histogram also includes a vertical line that demarks where the specifications’ actual estimated coefficient falls in the distribution. In each histogram, the mass of the coefficients is centered on 0, and the specifications’ actual coefficients appear in the tails of the distribution, indicating that the values estimated using the actual data were unlikely to have been generated by chance. Using exact inference with these placebo distributions, we generated $p$-values for the coefficients in the four specifications. These values were 0.001, 0.001, 0.003, and 0.002 for specifications 1 and 2 in Table 3 and specifications 1 and 2 in Table 4, respectively. The results of these placebo tests reveal that, despite the small proportion of treated cases, the treatment effects estimated in Tables 3 and 4 are robust and strongly suggestive of a causal relationship between the implementation of a corporate independent expenditure ban in a state and less favorable antitakeover lawmaking.

Figure 4. Coefficient Estimates for the Presence of a Corporate Independent Expenditure Ban (panels a and b) and the interaction of the Absence of such a Ban and Electoral Competitiveness (panels c and d), as generated by 5000 Re-estimations of the Specifications in Tables 3 and 4, Respectively. For each Re-estimation, the Seven States that Adopted an Independent Expenditure Ban during the Sample Period were Dropped from the Sample, and Seven of the Remaining 31 States were Assigned “placebo” Changes in their Corporate Independent Expenditure Laws during the Sample Period. The Dashed Vertical Lines Demark where the Actual Estimated Coefficients from Tables 3 to 4 would Fall in these Distributions.
5.2 Poison Pill Statute Adoption

Our second robustness check responded to the claim advanced by Coates (2000), Daines (2002), and Kahan (2006), among others, that statutes that endorse firms’ adoptions of poison pills obviate the need for other antitakeover statutes. To address this criticism of our use of the broader index, we analyzed the impact of a corporate independent expenditure ban on the adoption only of a statute that endorses poison pill provisions. Ideally, we would have used a survival model to investigate the joint questions of whether and when states adopt such statutes, but such a model could not be estimated, as all 19 of the 38 states in our sample that adopted poison pill statutes between 1986 and 2001 allowed corporate independent expenditures. Since it lacks sufficient variation, our key independent variable simply drops from the survival model, but the lack of such statutes in states with bans is itself strongly suggestive of a negative association between the two.

In place of a regression model, we conducted a bivariate analysis of the association between the two variables using a contingency table and, since we had low expected counts across the combinations of corporate independent expenditure ban and poison pill adoption, a Fisher’s exact test for association. Given the theoretical arguments we developed in Section 2, we expected that such an association would be negative. The results for this test provided strong evidence (p < 0.001) in favor of a negative and significant association between the two, which corroborates our claim of a positive relationship between the legality of corporate independent expenditures and management entrenchment via state law.

6. Discussion and Conclusion

We conclude the article with discussions of the implications of our findings for campaign finance and corporate governance scholarship, along with several thoughts on how our results speak to broader discussions about business and politics.

6.1 Implications for Campaign Finance

Our study has two obvious implications for the study of money in politics. First, in terms of a theoretical contribution, this article provides the strongest test to date of the agency theory of CPA by using a key independent variable and a dependent variable that both capture agency-related problems. Unlike most studies in this tradition, our measure of CPA captures the potential for firms’ monies, as opposed to individuals’ monies bundled through a firm PAC, to be used in the electoral arena. Additionally, we examine an outcome variable that can accurately capture if CPA is being engaged in a manner contrary to shareholders’ interests.17 In

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17. Granting shareholders greater control over political activity may solve the agency problem we identified. However, such a reform could lead to more opportunistic behavior by controlling shareholders, who could use the political process to limit protections for minority shareholders.
demonstrating a link between independent expenditures being allowed and more management-friendly public policy making, we have provided strong empirical evidence in favor of an agency cost interpretation of this electoral form of CPA, in which firms’ own monies can be used.

Second, we also extend arguments from the money in politics literature that were developed with regard to PAC contributions to the new era of unlimited, nationwide independent expenditures. That is, with regard to business power, we find that corporate independent expenditures are most likely to have an effect on policy outcomes in low salience subsystems, such as corporate governance. Our significant results here stand in contrast to prior work on independent expenditure bans on state-level measures of policy liberalism and income inequality, as well as minimum wage rates (Werner and Coleman 2012), which finds no effect for independent expenditure bans on these highly salient policies. However, the present results are consistent with prior work on business’ influence on public policies that examined the effect of corporate PAC contributions on Congressional activity at the committee level (e.g., Hall and Wayman 1990). Combined, these findings suggest that in the newly liberalized campaign finance environment at the national level, the effects of independent expenditures will most likely be felt in insulated, low salience policy subsystems.

6.2 Implications for Corporate Governance

Our study has two implications for corporate governance scholars as well. First, and more narrowly, our results suggest that CPA plays an important role in shaping antitakeover legislation in the states. As a result, we would caution those scholars who rest on Romano’s (1987) findings to suggest that these laws are exogenously determined. Our findings strongly suggest that business affects antitakeover lawmaking both through campaign finance and lobbying. As a result, studies that make use of state antitakeover laws to test relationships between governance provisions and firm value should follow the lead of Armstrong et al. (2012) and Bertrand and Mullainathan (2003) and conduct sensitivity analyses to ensure that there is not an endogenous relationship between these laws and their outcome variables.

Second, and more broadly, our results also provide support to those who argue that scholars of corporate governance need to pay greater attention to the role of politics in the development of corporate law (see, e.g., Roe 2003; Gourevitch and Shinn 2005; Coates 2012; Werner 2012). Our

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18. One limitation on this claim though, is that management’s most natural competitor (labor) did not compete with it on this issue consistently during the sample period and instead supported such policies through 1992.

19. To gain a sense of how scarce such research is, one only need read Bischoff’s (2009) review of 141 articles on corporate governance published between 1997 and 2009, in which he highlights none that focus on the relationship between CPA and governance.
analysis here demonstrates that politics plays a central role in antitakeover lawmaking and that these policies are not simply the product of a state legislature translating the preferences of a state bar association into law. Further, the battles over the passage and implementation of the Sarbanes–Oxley and Dodd–Frank Acts provide warrants for this claim at the federal level, suggesting that politics’ role needs to be accounted for at all levels of government and across various policy making venues.

6.3 Conclusion
This study exploited changes in state-level campaign finance laws regarding the legality of corporate independent expenditures to demonstrate that changes in these laws alter states’ antitakeover statutes. Through a panel study of 38 states over 15 years, we found that when a state allows corporate independent expenditures and has a competitive electoral environment, it passes more antitakeover laws than when it bans such expenditures or has uncompetitive legislative elections. This result has important implications for scholars of campaign finance and corporate governance. In particular, this finding provides the best evidence to date in favor of an agency cost interpretation of corporate political activity. Further, our results should also be of interest to activists and advocates on various sides of the practical political, legislative, and regulatory debates (see, e.g., Bebchuk and Jackson 2010) concerning the role of corporate money in American politics.

References


