After *Citizens United:* How Outside Spending Shapes American Democracy *

Nour Abdul-Razzak† Carlo Prato‡ Stephane Wolton§

July 5, 2020

Abstract

We study the political consequences of lifting restrictions on the funding of groups engaging in outside spending (e.g., independent political advertising) in elections. Theoretically, we assume that outside spending changes the salience of candidate-specific attributes relative to their party labels. Empirically, we employ a difference-in-differences design that exploits the removal of state-level restrictions on the funding of outside spending mandated by the federal-level rulings in both *Citizens United* and *SpeechNow.org v. FEC.* We find strong evidence that these regulatory changes increase the electoral success of Republican candidates, thereby leading to more ideologically conservative legislatures. We do not find an effect on polarization. Consistent with our theory, the effect of outside spending depends on the power of labor unions and the alignment of business interests with the Republican party.

Word Count: Approximately 9,975 for main text (including descriptive statistics in Appendix A).

---

*Carlo Prato gratefully acknowledges support from the Hoover Institution. Nour Abdul-Razzak gratefully acknowledges support from the National Science Foundation (NSF) Graduate Research Fellowship Program. Any opinions, findings, and conclusions or recommendations expressed in this material are the authors' and do not necessarily reflect the views of the NSF or the Hoover Institute.

†University of Chicago - Harris School of Public Policy. Email: abdulrazzak@uchicago.edu.

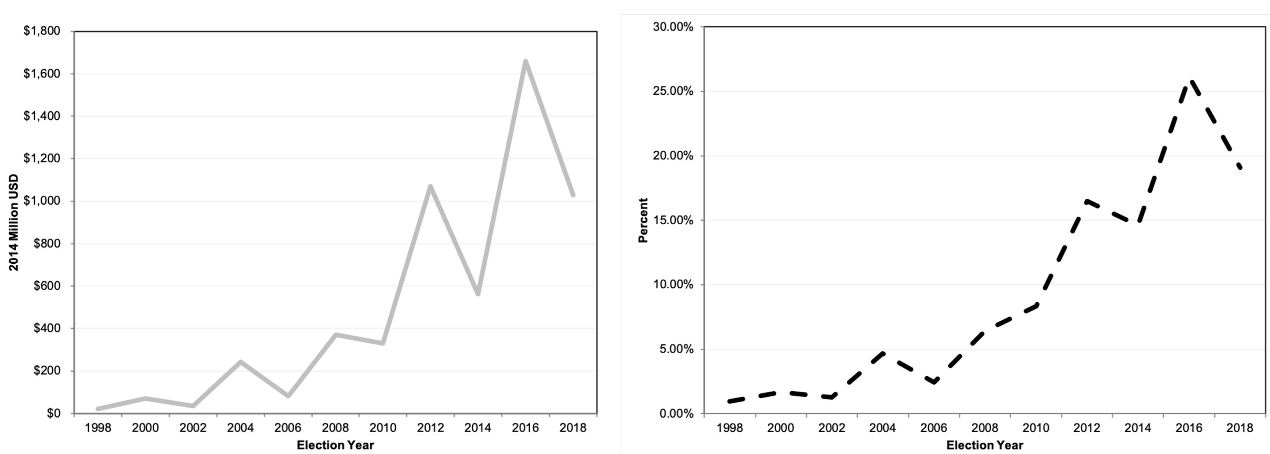
‡Columbia University. Email: cp2928@columbia.edu

§London School of Economics and Political Science. Email. S.Wolton@lse.ac.uk

According to the majority of Americans, wealthy individuals and special interest groups (SIGs) wield too much power over elections (e.g., Rasmussen Reports, 2016). In practice, these groups have two broad avenues of influence on electoral outcomes: they can contributes to candidates or parties, or they can engage in "outside spending," directly funding independent political communication.[1] While campaign contributions have been extensively studied, outside spending has received far less attention.

A comprehensive understanding of the consequences of outside spending has become more urgent in the wake of two judicial rulings issued in 2010: *Citizens United v. Federal Election Commission (FEC)* (558 U.S. 310) and *SpeechNow.org v. Federal Election Commission* (599 F.3d 686), with the former the key precedent and, thus, our point of reference. In this controversial decision, the U.S. Supreme Court ruled that limits on outside spending by any entity are unconstitutional.[2] In practice, this ruling lifted any restriction on the funding of groups engaged in this type of political advertising and led to a substantial increase in independent expenditures, which almost quadrupled (in absolute terms and as a percentage of total spending) between 2008 and 2016 (see Figure 1).

Figure 1: Outside Spending in Federal Elections



(a) Outside spending

(b) Outside spending as % of total electoral spending

Source: opensecrets.org.

[1] Outside spending encompasses independent expenditures and electioneering communication, two slightly different categories of political advertising in favor or against a candidate made independently of the candidate or party. Electioneering communication is defined as taking place 60 days prior to a general election (or 30 days prior to a primary election to federal office).

[2] *Citizens United* removed restrictions on the use of corporations and unions' general treasury funds for outside spending and *SpeechNow* removed restrictions on the use of funds from individuals.

In this paper, we investigate how outside spending affects the partisan and ideological composition of elected legislatures. To do so, we first leverage the implications of a novel theoretical model to design and interpret a number of empirical tests using U.S. state legislative elections. Our theory builds on the idea that outside spending changes the salience of a candidate's attributes (especially, her ideology) relative to her party label. We find that the lifting of funding restrictions on outside spending should electorally benefit the party aligned with the group whose budget increases most. Political polarization, in turn, should increase if and only if the party whose performance improves is more ideologically extreme before the regulatory changes.

Empirically, we perform a difference-in-differences estimation exploiting the fact that, as a result of *Citizens United*, some U.S. states had to strike down their own restrictions on the funding of outside spending. We find that as a result of these changes, (i) the vote shares (and seat shares) of the Republican party increased and (ii) the average ideological position of elected state legislators moved in a conservative direction. Conversely, we do not find an effect on ideological polarization.

In addition to performing a variety of robustness tests, we present evidence against potential confounders (e.g., the rise of the Tea Party) and alternative mechanisms (e.g., a drop in turnout) and in favor of our proposed mechanism. For instance, the pro-Republican effect of *Citizens United* is stronger in states where labor interests are relatively weaker and corporate interests relatively more aligned with the Republican party, consistent with the notion that Republican-aligned groups saw a greater increase in their budget following the Supreme Court's decision.

These results paint a comprehensive picture of how outside spending shapes American democracy. First, the improved performance of the Republican party suggests that conservative donors have more effectively taken advantage of the new opportunities for influence opened up by *Citizens United*. Second, the link between outside spending and ideological extremism is far more subtle and context-specific than often claimed. Our analysis suggests that *Citizens United* is *not* a leading cause of the increased polarization experienced by state legislatures in recent years.

1 Literature review

This paper belongs to a large literature attempting to quantify the influence of special interests. In her review piece, Beth Leech (2010, p.534) describes what has been largely an elusive quest: "[t]he search for a definitive statement about the power of lobbyists has become the Holy Grail of interest group studies. All seek it, but are forever being led astray." Of the three recognized avenues of influence available to interest groups—(i) the provision of information, (ii) the offer of personal reward, and (iii) electoral help (e.g., Leech, 2010; Giger and Klüver, 2016)—scholars have found convincing support for the first (de Figueiredo and Richter, 2014), some evidence in favor of the second (via, e.g., revolving door lobbying, analysed in Blanes i Vidal et al., 2012), and surprisingly little evidence that the third matters at all. Ansolabehere et al. (2003) find that contributions by interest groups do not affect how legislators vote and Baumgartner et al. (2009) find that elections are rarely mentioned in exchanges between groups and members of Congress.

The regulatory earthquake triggered by *Citizens United* opened up, suddenly and unexpectedly, a previously unavailable channel for candidate support. We exploit this seismic change to advance our knowledge of the electoral channel of influence in the era of Super PACs.

We are not the first to use time-series variation in state regulations to study SIG influence. Using changes in contribution limits by Political Action Committees to candidates, Barber (2016) shows how an influx of interest groups' money tends to reduce polarization, whereas large donations by individuals tend to increase it. Using a similar strategy, Hall (2013) finds that removing bans on state-level corporate campaign contributions can substantially alter parties' seat shares in the legislature.

Our paper follows a similar strategy, with the added advantage that the regulatory change we exploit was imposed on states by the U.S. Supreme Court. We significantly expand upon previous works on the impact of regulatory changes on outside spending which look at partisan government control, incumbency, and corporate taxation (Werner, 2011; La Raja and Schaffner 2014). Our work also complements a recent working paper by Petrova, Simonov, and Snyder (2019) that documents the effect of *Citizens United* on political advertising and on groups' political strategy at the state level.

Closely related to our paper, Klumpp et al. (2016) employ a similar design to study the consequences of *Citizens United* on Republican vote share, incumbency advantage, number of candidates, and campaign contributions. We contribute beyond Klumpp et al. in several ways. First, we look at outcomes (including legislators' ideology as well as polarization) that Klumpp et al. did not consider. Second, we perform all of our baseline analyses at the state level, the level of treatment assignment (this also avoids issues of redistricting), rather than at the district level. Third, we uncover substantively important heterogeneity in the effect of *Citizens United*. Fourth, we run a variety of robustness tests, as well as tests of alternative mechanisms and possible confounders that are fundamental to assess and interpret our estimated effects. Fifth, we document that the effect of *Citizens United* persists over time. Finally, we provide a theoretical model to guide our empirical tests as well as to help with the interpretation of the results.

Our theoretical framework is related to a large literature on special interest politics.[3] We build on several recent contributions. As in Aragonès et al. (2015) and Dragu and Fan (2016), we assume that advertising expenditures affect voters' perceptions of candidates, by emphasizing their individual traits or activating partisan cues. As in models of persuasive spending (Baron, 1994; Klumpp, 2014), SIG spending always benefits the candidate they support. As in models of informative spending (i.a., Grossman and Helpman, 2001 Chap 6; Coate, 2004; Ashworth, 2006; Prato and Wolton, 2019), the electoral consequences of advertising depend on candidates' attributes.

2 Background on federal and state regulations

Since the Taft-Hartley Act (1947), corporations and unions have been unable to use their general treasury funds to support federal candidates. In 1974, Congress created the FEC to ensure compliance with campaign finance laws and imposed strict limits on contributions and campaign expenditures. The expenditure limits were quickly overturned by the Supreme Court, which ruled in *Buckley v. Valeo* (424 U.S. 1) that independent expenditures are a form of free speech protected under the First Amendment.

Congress did not propose a major overhaul of campaign finance regulations until 2002's Bipartisan Campaign Finance Reform Act (BCRA). The law was an attempt to address the use of "soft money" (i.e., unlimited contributions to a party for the official purpose of party building) as a vehicle for circumventing existing restrictions on contributions. In an effort to prevent similar bypassing tactics, the BCRA also outlawed any form of funding of *electioneering communication*—ads aired

[3] To our knowledge, only Prato and Wolton (2017) and Klumpp (2014) explicitly model outside spending, under different assumptions and reaching normative conclusions.

in the 60 days before a general election that mention federal candidates—by corporations, unions, or non-profit groups.

This last provision quickly became the target of several legal challenges. Among them, in 2007, the conservative non-profit group Citizens United contested the decision by the FEC to prohibit the airing of a documentary opposing then presidential candidate Hillary Clinton. The case reached the Supreme Court and in January 2010, the verdict on *Citizens United* removed restrictions on the use of corporations and unions' budgets for the purpose of outside spending. Limits on individual-level contributions to groups engaged in outside spending were lifted in March by the D.C. Circuit Court of Appeals in *SpeechNow.org*.

These rulings not only affected federal regulation, but also state campaign finance laws as, prior to the ruling, 23 states had restrictions on the funding of outside spending by corporations and/or unions. This was well noticed by Justice Stevens who wrote in his dissenting opinion (page 7), "The Court operates with a sledge hammer rather than a scalpel when it strikes down [BCRA regulation on outside spending]. It compounds the offence by implicitly striking down a great many state laws as well." Our empirical analysis (see section 4) exploits pre-2010 state-level variation in funding restrictions on outside spending.

3 Theoretical framework

In this section, we present our theoretical framework. Our goal is to identify potential channels through which the lifting of restrictions on the funding of outside spending (henceforth, funding restrictions) affects political outcomes. To keep the technicality to a minimum, all proofs are relegated to Appendix B.

3.1 Set-up

Our model features a representative voter (she), two candidates D and R, and two groups d and r that favor D and R, respectively. These two groups can be individual donors or organized interests, and we remain agnostic about their identity. Throughout, we will use capital letters and superscript for candidates and lower-case letters and subscript for groups.

For ease of exposition, each candidate $J \in \{D, R\}$ is characterized by a degree of ideological extremism θ^J. A candidate with ideology $\theta^J = 0$ is a moderate; a candidate with ideology $\theta^J = 1$ is an extremist. It is common knowledge that the proportion of extremists in candidate J's party is given by p^J: $\Pr(\theta^J = 1) = p^J$.

The voter's evaluation of a candidate J depends on (i) the candidate's ideology, (ii) the appeal of candidate J's party brand, captured by the average extremism of its candidates p^J, and (iii) a taste parameter $\tau_J \in [\underline{\tau}, \overline{\tau}]$, which captures a combination of party match (reflecting a district's relative proximity to one party) and electoral advantages (arising, for example, from incumbency).

The voter does not weight these three elements equally. While τ^J receives a weight of one (for simplicity), the voter weights the candidate's own ideology by α^J and J's party brand by $(1 - \alpha^J)$. We refer to the weight α^J as the *salience* of candidate J's own attributes. The voter dislikes ideological extremism. Her payoff from voting for candidate J is then

$$V^J = -\alpha^J \theta^J - (1 - \alpha^J) p^J + \tau^J, \tag{1}$$

We also assume that voting behavior depends on idiosyncratic national-level factors that were unforeseen before the election. We capture such factors, in line with a long literature, with a zero-mean partisan shock ξ in favor of candidate R, drawn from a uniform with support $[-\frac{1}{2\psi}, \frac{1}{2\psi}]$.

Group j seeks to get its preferred candidate elected and its payoff is simply

$$U_j = \begin{cases} 1 & \text{if candidate } J \text{ is elected} \\ 0 & \text{otherwise} \end{cases}$$

Each group $j \in \{d, r\}$ has a budget B_j that can be used to change the relative salience of either candidate's attributes. Each group splits its budget B_j among four types of spending: candidate-specific spending targeting her preferred candidate J (share c_j^J) and the opposite candidate $-J$ (share c_j^{-J}), and party-specific spending targeting her preferred candidate J's party brand (share π_j^J) and the opposite candidate $-J$'s party brand (share π_j^{-J}). All shares must add up to one ($c_j^R + c_j^D + \pi_j^D + \pi_j^R = 1$), so group j cannot spend more than it raises (it never has any incentive to spend less). Candidate-specific spending $c_j^J B_j$ raises the relative salience of the attributes of candidate J, and party-specific spending $\pi_j^J B_j$ raises the relative salience of J's party label. In line with a long tradition of modeling campaign advertising as contests (e.g., Snyder, 1989; Skaperdas and Grofman, 1995), we assume that the different types of spending shape salience via a Tullock function:

$$\alpha^J = \frac{c_r^J B_r + c_d^J B_d}{c_r^J B_r + c_d^J B_d + \pi_r^J B_r + \pi_d^J B_d}$$

We model the effect of lifting funding restrictions as an increase in each group's budget from $B_j = B_j^0 > 0$ to $B_j = B_j^0(1+\beta_j)$. This parameterization has two advantages. First, it makes clear that outside spending pre-exists *Citizens United*. Second, it allows for between-group asymmetries in the level and in the growth of available funds. The parameter β_j corresponds to the budgetary expansion (in percentage term) of donor j's advertising budget as a result of the Supreme Court's decision.

The game proceeds as follows:

1. Nature draws candidates' ideology: $\theta^D \in \{0,1\}$ and $\theta^R \in \{0,1\}$.
2. Each group $j \in \{d,r\}$ observers θ^D and θ^R and chooses how to spend its budget $\{c_j^J, \pi_j^J\}_{J\in\{D,R\}}$.
3. The voter observes $\{\theta^J, \alpha^J\}_{J\in\{D,R\}}$ and computes V^D and V^R.
4. The shock ξ is realized, and the voter votes for one of the two candidates, who is then elected.
5. Payoffs are realized, and the game ends.

The equilibrium concept is Nash Equilibrium. As customary in probabilistic voting models, we impose parametric restrictions to ensure that for each possible value of V^D and V^R, candidates' winning probabilities are interior (see Online Appendix B for details).

A central component of our model is the salience weights modeled as payoff weights building on Aragonès et al. (2015). As they explain, salience weights relate to several foundational concepts in political psychology. Most importantly, they capture the idea that political advertising can influence *what* the voter thinks about, but not *how* she thinks about it (Cohen, 2015). Here, groups can use outside spending to shape how much weight the voter puts on candidates' attributes relative to their party labels, but they cannot change what those attributes are or what these labels represent. We collapse candidates' attributes into a single dimension: moderate/extremist. We recognize that personal advertising may focus on candidates' accomplishments or failures (Wichowsky, 2012), their personal strengths or weaknesses (Tinkher and Weaver-Lariscy, 1991) on top of their ideological centrism or extremism (Meirick et al., 2018). As long as the correlation between accomplishments/strengths and moderation is not too negative—i.e., moderates tend to be credit-claimers and extremists tend to be position-takers, as documented in Grimmer (2013)—, our results continue to hold. Further, our simplification has the advantage to improve the match between our theoretical framework and empirical analysis.

3.2 Analysis

We start with the voter's electoral decision. Given a payoff V^D from electing D and V^R from electing R and a particular partisan shock ξ, the voter votes for D if and only if $V_D - V_R \geq \xi$. Using Equation 1, this is equivalent to

$$\alpha^D(p^D - \theta^D) - \alpha^R(p^R - \theta^R) - (p^D - p^R) + \tau^D - \tau^R \geq \xi \tag{2}$$

Three elements affect the voter's electoral decision. First, the difference between candidates' extremism and their party brand $\alpha^D(p^D - \theta^D) - \alpha^R(p^R - \theta^R)$, which is weighted by the salience of the candidates' attributes. Second, the difference in party brands $(p^D - p^R)$, with better brand (lower p^J) improving a candidate's election chances. Third, the taste parameter differential $\tau^D - \tau^R$.

Since groups do not observe the shock ξ, they can only make inferences about their candidates' chances of winning based on V^D and V^R, which they can observe and can manipulate using outside spending. From the perspective of a donor j, the winning probability of candidate D with extremism θ^D wins against a candidate R with extremism θ^R—denoted $P^D(\theta^D, \theta^R)$—equals

$$\begin{aligned} P^D(\theta^D, \theta^R) =& \Pr(\xi \leq V_D - V_R) = \frac{1}{2} + \psi\Big[V_D - V_R\Big] \\ =& \frac{1}{2} + \psi\big[\alpha^D(p^D - \theta^D) - \alpha^R(p^R - \theta^R) - p^D + p^R + \tau^D - \tau^R\Big] \end{aligned} \tag{3}$$

From this, we can provide some insights into the strategy of the two groups. To do so, let's consider group d, who seeks to maximize the chances that its preferred candidate D wins. When its preferred candidate is more extreme than the party brand ($\theta^D = 1 > p^D$), the probability that D wins (P^D) decreases with α^D. Group d then tries to minimize the weight the voter puts on the candidate's attribute by running party-specific advertising campaigns: $\pi_d^D \geq 0$ and $c_d^D = 0$.

If instead candidate D is more moderate than the party brand ($\theta^D = 0 < p^D$), group d tries to raise the salience of the candidate's attributes in the eyes of the representative voter. To do so, its advertising is centered around candidate D: $\pi_d^D = 0$ and $c_d^D \geq 0$.

How should group d target candidate R? Obviously, group d always runs a "negative" campaign against candidate R: its advertising targeting R always decreases R's electoral chances. When candidate R is more extreme than her party brand ($\theta^R = 1 > p^R$), to improve D's electoral chances, group d chooses to raise the salience of candidate R's attributes: $c_d^R \geq 0$ and $\pi_d^R = 0$. Conversely, if candidate R is more moderate than her party brand ($\theta^R = 0 < p^R$), group d wants to distract voters from candidate R's attributes by focusing voters on R''s party label: $\pi_d^R \geq 0$ and $c_d^R = 0$.

We summarize these insights in the next lemma (where $-j$ denotes the group opposing j).

Lemma 1. *In every equilibrium, the groups' advertising strategy satisfies:*
(i) If $\theta^J = 1$, *then* $c_j^J = 0$ *and* $\pi_{-j}^J = 0$;
(ii) If $\theta^J = 0$, *then* $\pi_j^J = 0$ *and* $c_{-j}^J = 0$.

The 2018 special election in Pennsylvania's 18th Congressional District illustrates one prediction of Lemma 1. As described in Berry (2019), the Democratic party had in Conor Lamb a moderate candidate with broad appeal. In response, pro-Republican groups unleashed a barrage of ads linking Lamb to then-Democratic House Minority Leader Nancy Pelosi. As predicted by our model, the party, rather than the candidate, became the target of conservative groups' advertising.

While Lemma 1 holds for a large class of functional forms, we can make use of the Tullock function to offer some finer grained predictions on the groups' strategy.

Lemma 2. *Equilibrium adverting spending is allocated as follows:*
(i) when $\theta^D = 0$ *and* $\theta^R = 1$, $c_d^D = \frac{p^D}{p^D+1-p^R}$, $c_d^R = 1 - c_d^D$ *and* $\pi_r^R = \frac{1-p^R}{p^D+1-p^R}$, $\pi_r^D = 1 - \pi_r^R$;
(ii) when $\theta^D = 1$ *and* $\theta^R = 0$, $\pi_d^D = \frac{1-p^D}{1-p^D+p^R}$, $\pi_d^R = 1 - \pi_d^D$ *and* $c_r^R = \frac{p^R}{1-p^D+p^R}$, $c_r^D = 1 - c_r^R$;
(iii) when $\theta^D = 1$ *and* $\theta^R = 1$, $\pi_d^D = \frac{1-p^D}{1-p^D+1-p^R}$, $c_d^R = 1 - \pi_d^D$ *and* $\pi_r^R = \frac{1-p^R}{1-p^R+1-p^D}$,$c_r^D = 1 - \pi_r^R$;
(iv) when $\theta^D = 0$ *and* $\theta^R = 0$, $c_d^D = \frac{p^D}{p^D+p^R}$, $\pi_d^R = 1 - c_d^D$ *and* $c_r^R = \frac{p^R}{p^D+p^R}$, $\pi_r^D = 1 - c_r^R$.

Groups allocate their budget taking into account the electoral return of their advertising spending. Take an election with two moderate candidates. The electoral return from raising the salience of candidate D's attributes for group d is p^D. The electoral return for decreasing candidate R's attributes is p^R. If $p^D > p^R$ (i.e., party D is perceived as more extreme than party R), the group invests more in praising its favorite candidate. Otherwise, it mostly targets candidate R by linking him to his party brand.

Lemmas 1 and 2 offer theoretical predictions regarding groups' spending. The predictions they contain help to distinguish campaigns that focus on local factors (especially candidates' characteristics) from races that are instead dominated by national themes (Burden and Wichowsky, 2010). We use these preliminary results to study three aggregate quantities for which we have data: (i) vote shares of the Democratic and Republican parties, (ii) average ideology of elected legislators, and (iii) degree of polarization. Recall that in our model the lifting of funding restrictions corresponds to an increase by β_j percent of group j's budget. Regarding vote shares, we obtain Proposition 1.

Proposition 1. *The lifting of funding restrictions strictly improves the electoral chances of party $J \in \{D, R\}$ if and only if the budget of group $j \in \{d, r\}$ increases strictly more than the budget of group $-j$: $\beta_j > \beta_{-j}$*

On average (before the realization of types), the party aligned with the group who experiences the highest increase in budget benefits electorally.

Groups seek to move salience weights so as to improve their preferred candidates' chances. The greater the increase in a donor's budget, the more the group has the ability to change the voter's attention towards characteristics (candidates or party labels) that improve its preferred candidate's electoral fortune. Greater (relative) capacity to change the salience of attributes then translates into better electoral fortune for the favored candidate.

The greater electoral chances of (say) Republican candidates thanks to group r's bigger budget increase has a secondary effect if one assumes that Democrats and Republicans are on both sides of the ideological spectrum (so an extremist D can be interpreted as -1). As some Democrats are replaced by Republican legislators, the removal of funding restrictions moves the average ideology of the elected candidate in a conservative direction (for a formal statement and proof, see Proposition B.1 in Online Appendix B).

What about polarization? In line with the folded scale approach proposed by Barber (2016), in our set-up, polarization corresponds to the average extremism of the elected candidate. As the next proposition highlights, it is harder to predict how polarization (relative to other electoral outcomes) responds to the lifting of funding restrictions. Knowledge of which group benefits, in relative terms, is not enough. One also needs a measure of voters' perception of party brands when funding restrictions were in place.

Proposition 2. *The lifting of funding restrictions strictly increases polarization if and only if:* $(p^D - p^R)(\beta_d - \beta_r) > 0$.

The result is relatively intuitive. Say the Republican-aligned group benefits from the removal of funding restrictions: $\beta_r > \beta_d$. As a result, more Republican candidates are elected. If the Republicans are on average more moderate than Democrats ($p^D > p^R$), then this means that relatively moderate candidates are replacing relatively extreme candidates and, on average, polarization decreases. In turn, if Republicans are on average more extreme ($p^D < p^R$), then extremists are replacing moderates, and polarization increases.

Overall, our theoretical framework establishes two distinct sets of predictions. First, the lifting of funding restrictions on outside spending should have a clear effect on winning probabilities (and average ideologies), because its impact only depends on relative changes in budgets (Proposition 1). These predictions are reassuringly consistent with other models of influence such as models with impressionable voters (e.g., Baron, 1994). Second, the implications for polarization are more nuanced, because they depend on both relative changes in budgets and the political situation prior to the lifting of funding restrictions (Proposition 2). This also implies that the effect of regulatory changes is likely to be state-specific. If, say, Republican groups see a higher growth in their budget everywhere, but the Republican party is more extreme in some states and less extreme in others, the overall effect of lifting funding restrictions on polarization becomes even harder to predict.

In the absence of data on groups' revenues, we cannot directly estimate the effect of Citizens United on groups' budgets (the β's in our model). Several pieces of evidence, however, allow us to formulate an informed conjecture. At the time of the Supreme Court's decision, it was widely believed that groups aligned with the Republican Party would benefit most (e.g., Good, 2010). Subsequent empirical evidence (e.g., Klumpp et al., 2016) and our own analysis of the available data on spending is in line with that notion, but cannot fully prove it. Under that informed conjecture, our theory then predicts an increase in the Republican vote shares and in legislature' ideological conservativeness. As noted above, even knowing which side benefited the most is not sufficient to make predictions regarding polarization.

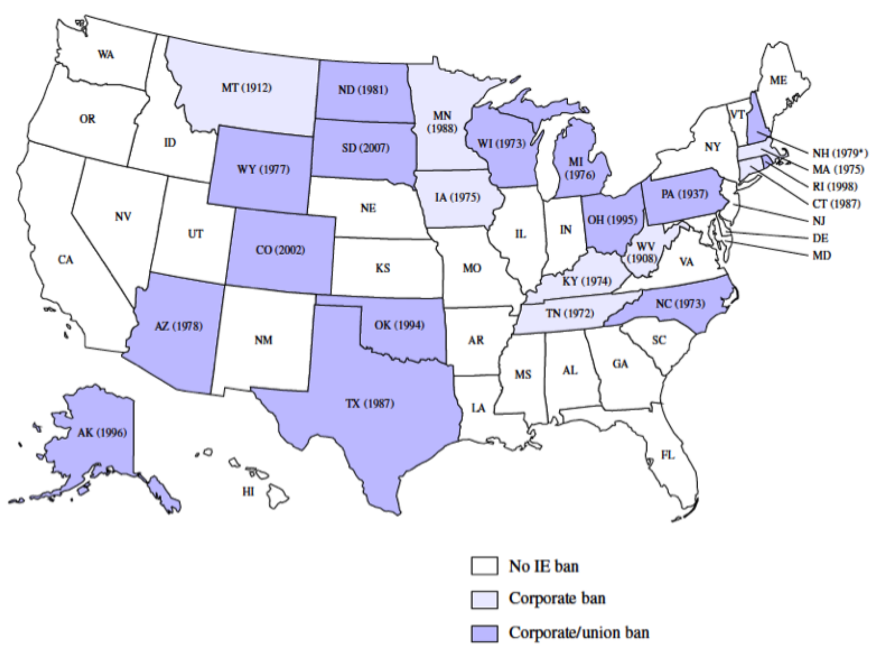
4 Empirical strategy

In this section, we describe our empirical strategy, specifications, and the assumptions required for causal interpretation.

4.1 Research design

To identify the effect of the removal of restriction on the funding of groups engaged in outside spending (again, shortened to funding restrictions in what follows) on electoral outcomes, we take advantage of the state-level variations in campaign finance regulation prior to 2010. Information on state-level funding restrictions were kindly provided by La Raja and Schaffner and augmented by our own research.

Figure 2: State campaign finance regulation before *Citizens United*



The map is reproduced from Klumpp et al. (2016). Years in parentheses indicate the years of introduction of the funding restrictions.

Prior to the Supreme Court's decision, 23 states had restrictions on corporations' and unions' ability to fund outside spending (see Figure 2). As discussed above, *Citizens United* forced these states to remove these restrictions. By the end of March 2010, more than half of these 23 states had officially changed their campaign regulations. By November 2010, all states (with the possible exception of New Hampshire) had complied with the new regulation (see Table A.1). It is therefore reasonable to conclude that these 23 states were "treated" from 2010 on (we nonetheless perform robustness tests excluding New Hampshire). We use 25 out of 27 of the remaining states that did not have to change their regulation to form our control group. We exclude Nebraska, which has a non-partisan unicameral legislature, and Louisiana, which elects its representatives via nonpartisan blanket primaries. Overall, our panel covers 48 states over a period of at least 20 years.

To estimate the impact of these regulatory changes, we use a difference-in-differences strategy. We compare the changes in our outcomes of interest (e.g., the Republican vote share) before and after *Citizens United* across the treatment and control states. This approach relies on a few key assumptions (e.g., the presence of common trends) that we review in the last part of this section after describing our empirical specifications and main data sources.

4.2 Electoral outcomes

We first consider the effect of outside spending on election outcomes using state legislative election returns compiled by ICPSR and purchased from Klarner Politics. Our dependent variable is the Republican share of state-wide votes in a chamber, state, and year denoted by $RepVS_{st}$ (in Online Appendix F, we also look at Republican seat share). We consider all elections from 1990 to 2018 (because data are aggregated at the state level, we do not lose observations due to redistricting). Our baseline specification is given by:

$$RepVS_{st} = \beta CitUn_t \times BanState_s + \gamma_s + \delta_t + \epsilon_{st} \tag{4}$$

$CitUn_t$ takes value 1 for post-2010 elections and 0 otherwise, $BanState_s$ takes value 1 if state s had funding restrictions on outside spending. The coefficient of $CitUn_t \times BanState_s$ thus captures the effect of lifting bans on contributions from corporations and unions to groups engaging in outside spending. Equation 4 also includes state (γ_s) and year (δ_t) fixed effects. The state fixed effect captures all time-invariant characteristics of a given state s (e.g., its pre-*Citizens United* partisan tilt). The time fixed effect deals with state-invariant common trends, such as national partisan waves.[4] Observe that we do not control for post-treatment variables such as contributions or the party of the governor because they could induce bias in our estimates.

The last term in Equation 4 (ϵ_{st}) is the residual. Due to the likelihood of serial correlation or state-specific clustering that could impact the precision of our estimates, we use robust standard errors clustered at the state level in this and all other specifications.

4.3 Ideology and polarization

To measure state legislators' ideology, two measures are available. The NPAT score, compiled by Boris Shor and Nolan McCarty and described in details in their 2011 article, and the DIME score, compiled by Adam Bonica and described in details in his 2014 article. The NPAT score is generated using a roll-call based strategy similar to Poole and Rosenthal's (1997) NOMINATE score, augmented by survey results from the Project Vote Smart National Political Awareness Test for comparability across chambers, states, and time. The DIME score, instead, is based on contributions to candidates.

The two measures each have advantages and shortcomings. On the one hand, the NPAT score is time-invariant and provides a unique (average) score for a legislator's tenure, whereas the DIME score is term-specific. On the other hand, NPAT is less vulnerable than the DIME contribution-based measure to SIGs' possible substitution from contributions to outside spending post-*Citizens United*. Due to this last concern, we use the NPAT score as our preferred measure and we relegate analyses performed using the DIME measure to Online Appendix E.5.

In our main specifications, we restrict the sample to the period 1996-2016 for which we have complete data available for all the states. First, we look at the effect of outside spending on a chamber's average ideology ($AvgIP_{st}$), with higher scores indicating a more conservative legislature.[5] Our baseline specification is as follows:

$$AvgIP_{st} = \beta CitUn_t \times BanState_s + \gamma_s + \delta_t + \epsilon_{st} \tag{5}$$

[4] Both fixed effects subsume $BanState_s$ and $CitUn_t$, which do not need to be included in Equation 4.

[5] We adjust the coding of $CitUn_t$ since candidates elected in November 2010 took office in January 2011. $CitUn_t$, thus, takes a value of 1 starting in 2011.

In addition to the chamber average ideal point, we also look at the chamber's median ideology, a more common measure of ideology less sensitive to outliers.

To measure polarization, we follow Barber (2016) and use a "folded" scale where we multiply the legislator's NPAT score for Democrats by -1 before taking the state-year average. When the distributions of ideologies do not overlap, this is equivalent to using the absolute value of ideology. When the distributions overlap, as discussed in Barber (2016), this approach guarantees that larger values still indicate more polarized legislatures. We let $Polarization_{st}$ denote the chamber average folded ideal point in state s in year t. Our baseline specification is then:

$$Polarization_{st} = \beta CitUn_t \times BanState_s + \gamma_s + \delta_t + \epsilon_{st} \tag{6}$$

4.4 Empirical strategy: assumptions and potential issues

Our estimates capture the causal effect of lifting funding restrictions on outside spending if (i) in the absence of *Citizens United*, outcomes in treated and control states would have followed common trends, and (ii) we can rule out potential confounders. We discuss the first point in detail below and tackle the second issue in Section 6.

While our approach allows us to parse out all time-invariant differences between treatment and control states and accounts for time-varying factors that are common to treated and control states (e.g., all states are becoming more polarized), it does not account for time-variant differences between states. This becomes a concern if these factors (i) *jointly* affected our outcomes (e.g. the Republican winning probability) *and* the adoption or removal of funding restrictions (e.g., the strength of local SIGs) and (ii) evolved differently over time in treated and control states.

We take a few steps to alleviate this potential problem. Historical research by La Raja and Schaffner (2014) as well as our own suggest that the adoption of funding restrictions on outside spending were introduced for reasons that are not explicitly partisan. The introduction of these restrictions spans over a century and clusters around three major nation-wide events: (i) the rise of the Progressive movement in the early 20th Century, (ii) the Watergate era in the mid-1970s, and (iii) the BCRA era in the late 1990s-early 2000s. Furthermore, stringent campaign finance laws were sometimes opposed by both parties, such as in Colorado in 2002 where they were passed by popular initiative (Hall 2013). When it comes to the lifting of these restrictions, treated states did not have a say. Changes were imposed by the federal Supreme Court whose justices evaluated the constitutionality of funding restrictions for *federal elections*.

We also depict trends graphically in our outcomes of interest to check visually for any pre-treatment differences in trends. In addition, we also rerun our main specifications with lead treatments to more robustly test for pre-trends (non-significant leads give us more confidence that the trends would have continued on this trajectory in absence of the treatment). Further, we augment our baseline analysis by adding two types of time trends to the specifications in Equations 4-6. First, we include a "Deep South" time trend (Alabama, Georgia, Mississippi, South Carolina, and Florida) to account for the fact that over our time period, these states were trending Republican at a faster rate than the rest of the country. Second, we conservatively add state-specific time trends (at the cost of decreasing statistical power).[6]

5 Empirical results

Table A.2 in Appendix A reports summary statistics (mean and standard deviation) for our dependent variables prior to *Citizens United.* These figures constitute the main benchmark for our estimates.

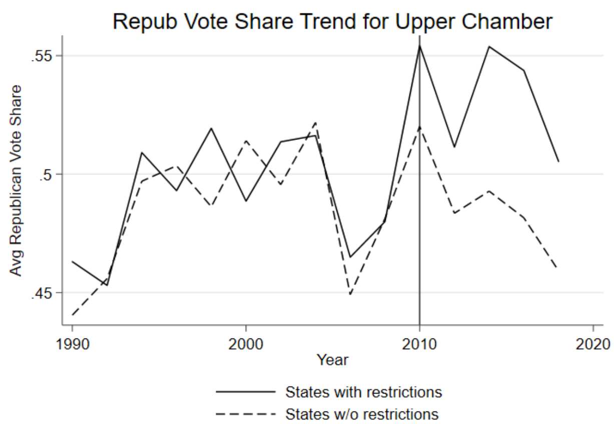
Republican vote

The trends in Republican vote share for the lower chamber in control (no restrictions) and treated (restrictions) states are documented in Figure 3.[7] Trends in both sets of states are relatively similar prior to *Citizens United*, which is consistent with the parallel trends assumption.

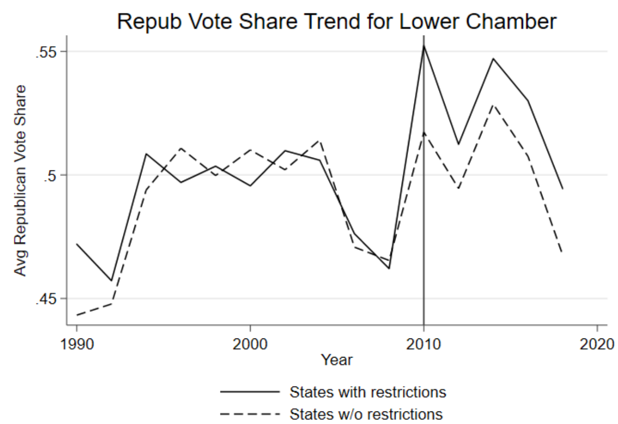
Table 1 presents the estimates of Equation 4. While our lower chamber baseline specification points to a positive but not statistically significant increase in the Republican vote shares (Column 1), the inclusion of state-specific time trends (Column 3), leads to a coefficient almost twice as large and statistically significant. Column 2 suggests that the different trajectory of the Deep South states—none of which had funding restrictions on outside spending in 2010 and all of which were trending Republican over the time period considered—accounts for most of the difference.

[6]The identification assumption then is that our outcomes of interest among the treated would not have deviated from their state-specific trends absent *Citizens United*.

[7]Figures depicts only states with elections in even years (some control states have elections in odd years).



(a) upper



(b) lower

Figure 3: Trends in average Republican vote share

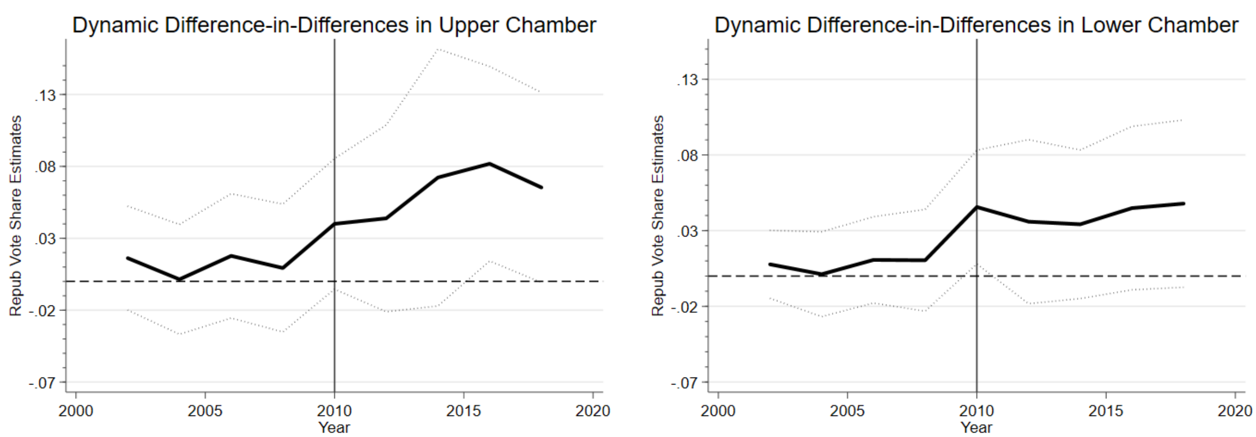
Table 1: Effect of *Citizens United* on Republican vote share

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Rep VS	Rep VS	Rep VS	Rep VS	Rep VS	Rep VS
CitUn×BanState	0.042*	0.056**	0.040**	0.020	0.038*	0.036**
	(0.023)	(0.023)	(0.019)	(0.020)	(0.020)	(0.015)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	650	650	650	696	696	696
R^2	0.679	0.693	0.791	0.804	0.830	0.919

Robust standard errors, clustered at state level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The implied magnitude of the effect is substantial, especially in the upper chamber. Relative to the pre-treatment baseline (49% Republican vote share in both chambers), lifting funding restrictions on outside spending produces an 8.2% increase in Republican vote share in the lower chamber and a 7.3% increase in the upper chamber. The effect exceeds one third of a standard deviation and, substantively, would be enough to change the election winners in 10% of the races in our sample. Consistent with these results, we show in Online Appendix F that *Citizens United* yielded an increase of approximately 11.5% in Republican seat shares (almost five seats in a 100-member state legislature).

Given the large electoral advantage to Republicans provided by lifting funding restrictions on outside spending, one may expect the effect to be short-lived as interest groups and party adjust to the new regulatory regime. In Figure 4, we display estimates from both the lead treatment indicators (pre-2010) and lag indicators (post-2011); point estimates and additional results can be found in Online Appendix E.1. For both chambers, lead estimates are close to zero (further validating the parallel trends assumption). In contrast, estimates on lag treatment suggest the effect persists over time. Together, these findings indicate that *Citizens United* may have produced a structural shift in the balance of power in state legislative politics, though we cannot rule out that the duration of the effect is reinforced by other factors such as the incumbency advantage.



(a) upper chamber

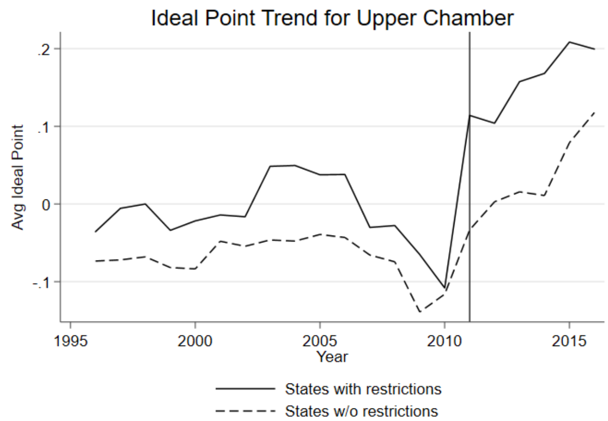
(b) lower chamber

Figure 4: Dynamic difference-in-differences estimates

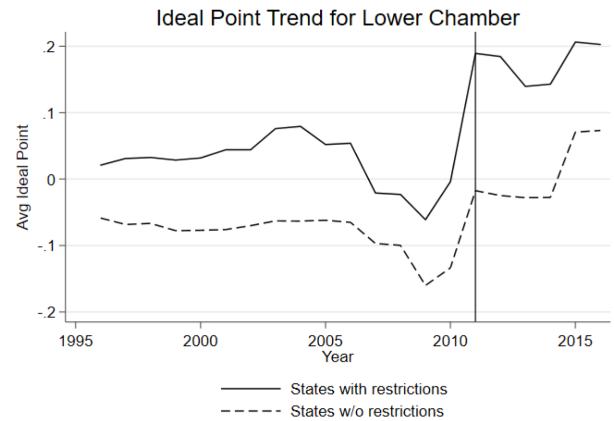
Ideology

As Figures 5 illustrates, chamber ideology (average and median) in control and treated states follow relatively similar trends over the period of study (1996-2016).

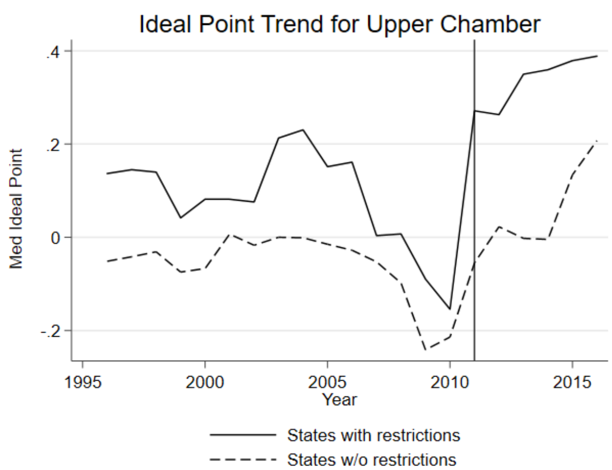
Consistent with the Republican gains described above, Tables 2 and 3 suggest that state legislatures became, on average, more conservative following *Citizens United.* While not all of our specifications yield statistically significant estimates (estimates are stronger using chamber medians than chamber averages), they all point in the same direction. Using the specification with state-specific trends, the results suggest that lifting funding restrictions on outside spending leads to an increase in conservatism of almost one quarter of a standard deviation.



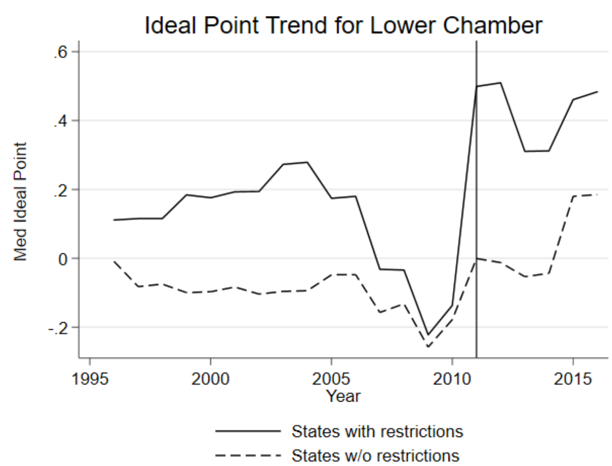
(a) Average ideology: upper chamber



(b) Average ideology: lower chamber



(c) Median ideology: upper chamber



(d) Median ideology: lower chamber

Figure 5: Trends in ideology

We again look at the effect over time in Figure 6 for average ideology. The increase in average ideology levels off, but it does not disappear. Table E.1 in Online Appendix E, where we use median ideology, confirms this conclusion.

Table 2: Effect of *Citizens United* on average ideology (NPAT scores)

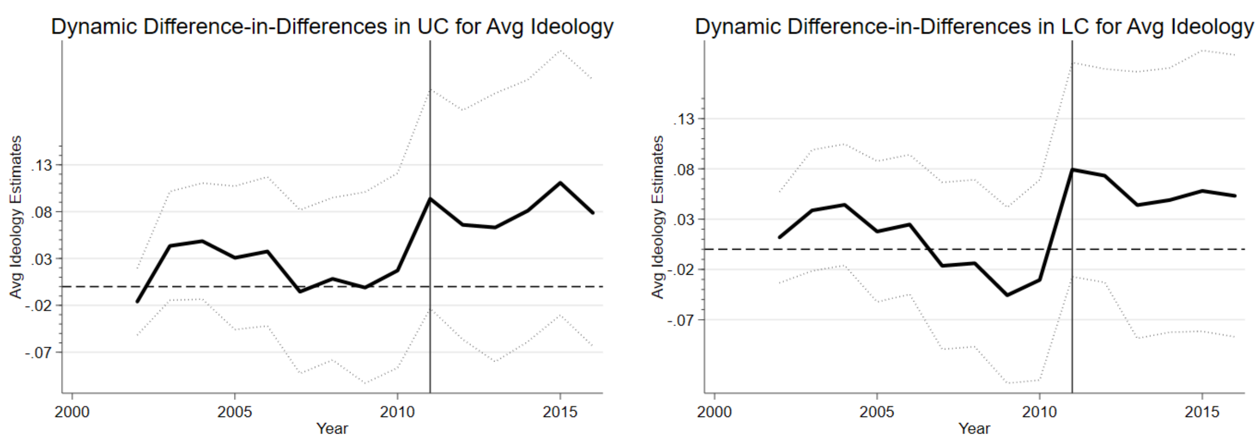
	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Average IP	Average IP	Average IP	Average IP	Average IP	Average IP
CitUn×BanState	0.042	0.070	0.067	0.034	0.056	0.074**
	(0.049)	(0.049)	(0.047)	(0.041)	(0.044)	(0.033)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.920	0.924	0.964	0.943	0.945	0.978

Robust standard errors, clustered at state level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effect of *Citizens United* on median ideology (NPAT scores)

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Median IP	Median IP	Median IP	Median IP	Median IP	Median IP
CitUn×BanState	0.102	0.189	0.193	0.131	0.208*	0.304**
	(0.127)	(0.124)	(0.133)	(0.112)	(0.116)	(0.126)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.775	0.788	0.882	0.765	0.776	0.876

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



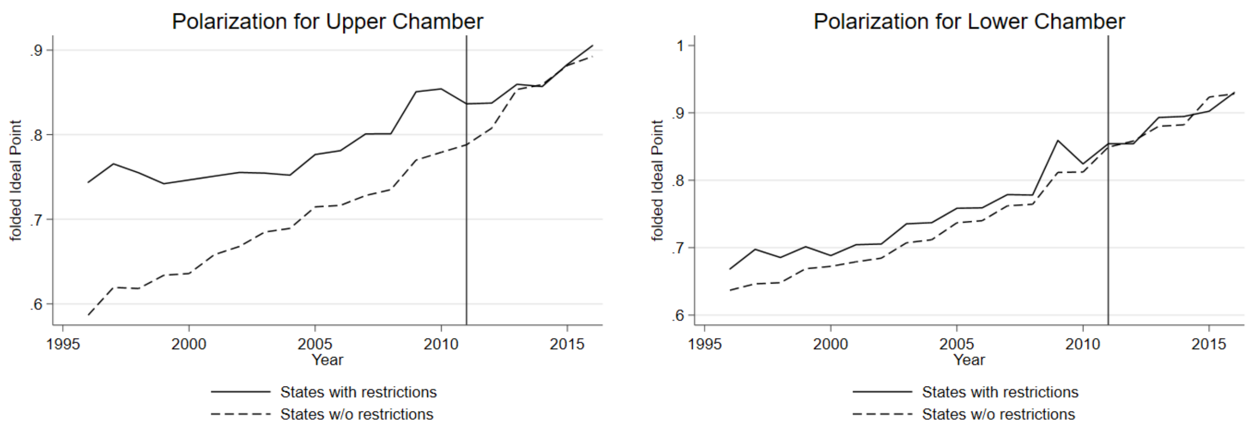
(a) upper chamber

(b) lower chamber

Figure 6: Dynamic difference-in-differences estimates

Polarization

Polarization exhibits similar pre-trends when we compare the treatment and control states over the years in Figure 7.



(a) upper chamber

(b) lower chamber

Figure 7: Trends in polarization ("folded" value of NPAT)

Table 4 suggests that removing funding restrictions on outside spending has no detectable effect on legislators' ideological polarization. The coefficients are very small in magnitude and very far from conventional levels of statistical significance.

Table 4: Effect of *Citizens United* on polarization

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Polarization	Polarization	Polarization	Polarization	Polarization	Polarization
CitUn×BanState	-0.062	-0.043	0.004	-0.024	-0.014	-0.007
	(0.042)	(0.041)	(0.032)	(0.046)	(0.047)	(0.034)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.891	0.895	0.966	0.909	0.910	0.975

Robust standard errors, clustered at state level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Online Appendix E.2 (Table E.4 and E.5), we also consider the effect of *Citizens United* on ideology within each party. Results are largely inconclusive due to lack of power, but the sign and magnitude of the effects coupled with our null polarization results are consistent with the notion that relatively moderate Democrats in competitive districts were replaced by moderate Republicans.

6 Additional evidence

In this section, we explore possible confounders and provide some evidence consistent with the mechanism proposed in our theory.

6.1 Potential confounders

Citizens United occurred at a tumultuous time in American politics. The election of Barack Obama, his administration's response to the Great Recession, and the passage of the Affordable Care Act triggered a strong political reaction, epitomized by the rise of the Tea Party. In addition, in 2009, the Republican State Legislative Committee began executing the *Redistricting Majority Project* (REDMAP), a strategy aimed at gaining control of key state legislatures (and the redistricting process post-2010). If these political phenomena disproportionately affected the treatment states, our estimates would incorrectly attribute the effect of increased gerrymandering or a more severe presidential midterm slump to *Citizens United*.

Here, we reproduce our empirical estimates controlling for the strength of the Tea Party in different states prior to *Citizens United*. As a proxy of strength, we use data employed in Madestam et al. (2013), kindly shared with us by Andreas Madestam. We construct a dummy variable, which takes a value of one in the 2009 and 2010 elections if the number of protest attendees in the state during the Tax day rally on April 15, 2009 was above the national average.

To address the potential confounding role of REDMAP, we combine information found in official documents to create a dummy variable ($REDMAP_{st}$) which equals one for states in which REDMAP spending exceeded USD $1M$ in 2010 (see `redistrictingmajorityproject.com` for details). Lastly, we also control for the existence of state legislative term limits, which could have exacerbated the Republican wave of 2010 by limiting incumbents' ability to run for office (19 states in our analysis have term limits at some point in the study period).

Table 5 shows that our results on vote shares remain virtually unchanged when we include these three controls. In Online Appendix C, we show that our estimates of the effect on ideology are also unaffected (Tables C.1 and C.2).

Table 5: Effect of *Citizens United* on Republican vote share with controls

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Rep VS	Rep VS	Rep VS	Rep VS	Rep VS	Rep VS
CitUn×BanState	0.042*	0.056**	0.041**	0.020	0.038*	0.036**
	(0.023)	(0.023)	(0.019)	(0.020)	(0.020)	(0.015)
Strong Tea Party	0.024	0.018	0.011	0.019*	0.010	0.008
	(0.017)	(0.017)	(0.016)	(0.010)	(0.011)	(0.010)
REDMAP	-0.024	-0.026	-0.015	0.000	-0.001	0.006
	(0.023)	(0.021)	(0.021)	(0.013)	(0.011)	(0.011)
Term Limit	0.002	-0.000	0.002	0.008	0.005	0.008
	(0.014)	(0.014)	(0.012)	(0.013)	(0.012)	(0.009)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	650	650	650	696	696	696
R^2	0.680	0.694	0.792	0.805	0.831	0.919

Robust standard errors, clustered at state level in parentheses

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

Finally, we investigate whether changes in state-level campaign finance regulation around the time of *Citizens United* might have affected our estimates. Using data from Barber (2016), we control for campaign contribution limits to individuals and political action committees for lower chamber races in each state over the period 1990-2012 (see Appendix C.1 for more details on the construction of our control variable). Again, we find that our estimates are robust to the addition of these potential confounders.

Table 6: Effect of *Citizens United* in Republican vote share in lower house controlling for contribution limits

	(1)	(2)	(3)
	Rep VS	Rep VS	Rep VS
CitUn×BanState	0.023	0.040**	0.034**
	(0.018)	(0.019)	(0.014)
State and Year FE	✓	✓	✓
Deep South Trend		✓	
State-specific Trends			✓
Contribution Limits Controls and Others	✓	✓	✓
Observations	556	556	556
R^2	0.830	0.853	0.923

Robust standard errors, clustered at state level in parentheses

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

Owing to data availability, our tests cannot rule out every potential confounding factor. However, they support our claim that our empirical strategy captures the impact of *Citizens United* above and beyond national trends.

6.2 Mechanism

Up to now, we have presented "macro" evidence on the effect of lifting funding restrictions on outside spending. In this section, we provide evidence that the impact we document is due to Republican-aligned groups benefiting more than Democratic ones.

Table 7 summarizes the available data on state-level outside spending, both before and after 2010. We see that (i) treated states experienced a substantially higher increase in outside spending (54%—51% per capita) than control states (4.8%—1.3% per capita, barely a change) and (ii) pro-Republican groups experienced a much larger increase in spending. These patterns are also consistent with the findings in Spencer and Wood (2014).[8]

Table 7: Outside spending in 18 states: total / per capita

		2006-09	2010-12	Change
Control (N=6)	Total	58.58 / 0.98	61.39 / 1.00	+4.8% / +1.3%
	Republican SIG	23.10 / 0.39	28.17 / 0.46	+22% / +18%
	Democrat SIG	34.24 / 0.58	31.84 / 0.52	-7% / -10%
Treatment (N=12)	Total	35.06 / 0.37	53.92 / 0.55	+54% / + 51%
	Republican SIG	11.14 / 0.12	21.72 / 0.23	+95% / +88%
	Democrat SIG	21.15 / 0.23	26.54 / 0.28	+26% / +21%

Source: followthemoney.org (2019 data). Aggregate figures are in millions USD; per capita in USD.

The evidence is only suggestive. While our analysis uses almost 20 years of election data from 48 states, Table 7 only covers the 18 states for which there exist some disclosure requirements, including 6 from the control group (CA, ID, ME, MO, VA, and WA) and 12 from the treated group (AK, AZ, CO, CT, MA, MI, MN, NC, OH, OK, TN, and TX). States' disclosure requirements also differ substantially, reducing comparability.[9] These limitations do not affect our empirical strategy. A strength of our approach is that it captures the total effect of *Citizens United*, through Super PACs as well as undisclosed sources like "dark money" (although we cannot distinguish between these channels).

To document empirically that Republican-leaning groups have enjoyed a higher boost from the lifting of funding restrictions on outside spending, we also exploit state-variations in the relative strength of organized labor and the alignment of corporate interests with the Republican party.

To measure union strength, we create a dummy variable, $Union_s$, that equals 1 when the pre-2010 union density in the state is above the median. Since Political Action Committees (PACs) associated with Unions contribute to Democrats more than to Republicans (according to opensecrets.org, 86%

[8]Democrat and Republican levels are based on targeted ads and differ from the total, which also includes issue advocacy. Using a different classification, Petrova et al. (2019) find no differential effect between Republicans and Democrats.

[9]For instance, nine states—AR, ME, MA, MI, MN, MO, TN, TX and WI—do not require disclosure of advertising spending in the weeks before the election, or do so only under restrictive conditions.

of their spending at the federal level went to Democratic candidates in 2015-16), we expect the pro-Republican treatment effect to be weaker in states with high union density.

PACs associated with corporations, conversely, usually contribute more to Republicans, at least at the federal level (66% in 2015-16). In the absence of a direct measure, we proxy the degree of alignment between business (corporations or wealthy donors) and the Republican party using a measure of industry-weighted exposure to regulation developed by Fouirnaies and Hall (who kindly shared their data with us). We create a dummy variable, $Corp_s$, which equals 1 if the pre-2010 state-specific weighted regulatory exposure is *above* the median.[10] We expect the pro-Republican treatment effect to be weaker in states with high regulatory exposure based on Hall and Fouirnaies' (2016: 2) finding that "firms exposed to more government regulation distribute their campaign contributions in a more access-seeking manner than do other firms, allocating more of their contributions based purely on incumbency status and not on the basis of ideology, district type, or other political factors." The two variables, *Union* and *Corp*, appear to capture distinct quantities (their correlation is only 0.12 when variables are dichotomized and 0.26 when we use the full variation).

In Table 8, we consider again the effect of lifting funding restrictions on Republican's vote share (VS) in state legislatures. For appropriate comparison, columns (1) and (3) reproduce our baseline specification with a Deep South Trend, for the years 1994 onward (Hall and Fouirnaies' regulatory exposure data are not available prior to 1994). Columns (2) and (4) include our two dummy variables and their interactions with the treatment variables. The coefficients all have the expected signs. In states where unions are weaker ($Union = 0$) and corporations are more aligned with the Republican party ($Corp = 0$), the effect of lifting funding restrictions on outside spending is more than twice our baseline estimate (almost 8 percentage points). Conversely, in states in which unions are stronger ($Union = 1$) and corporations are less aligned with the Republican party ($Corp = 1$), the effect is less than half of our baseline estimate and not statistically different from zero.

Table 9 reports heterogeneous effects for the chamber's average ideology. Columns (1) and (3) report our baseline estimates with a Deep South time trend. Columns (2) and (4) include the new

[10]Appendix D provides more details on the construction of this variable. There, we also show that all of our results are robust to using continuous measures rather than dichotomized ones.

Table 8: Effect of *Citizens United* on the Republican vote share

	Upper Chamber		Lower Chamber	
	(1)	(2)	(3)	(4)
	Rep VS	Rep VS	Rep VS	Rep VS
CitUn×BanState	0.053**	0.079***	0.039**	0.066***
	(0.022)	(0.021)	(0.018)	(0.018)
CitUn×BanState×Union		-0.042*		-0.038
		(0.024)		(0.025)
CitUn×BanState×Corp		-0.010		-0.029
		(0.064)		(0.054)
CitUn×BanState×Union×Corp		0.000		0.015
		(0.068)		(0.058)
State and Year FE	✓	✓	✓	✓
Deep South Trend	✓	✓	✓	✓
Observations	562.000	562.000	603.000	603.000
R-squared	0.718	0.723	0.865	0.870
F-stat p-val		0.001		0.002

Robust standard errors, clustered at state level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Columns 1 and 3 show the baseline effect in the modified time frame. Columns 2 and 4 include $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$. The F-statistics report the test of the null hypothesis of joint zero values of all four coefficients.

dummy variables and their interactions with the treatment. Again, the results are in line with our theoretical expectations. In states where unions are weaker ($Union = 0$) and corporations are more aligned with the Republican party ($Corp = 0$), the average conservative shift in the chamber's average ideology more than doubles relative to the baseline.

Yet, despite the consistency with our theoretical predictions, the findings above should be interpreted with caution for two reasons. First, these results do not prove that corporations always favor Republican candidates; instead, they only suggest that Republicans benefit more in situations where businesses weight ideology more than incumbency status. Second, our difference-in-differences strategy is unlikely to account for all possible confounders in these analyses (e.g., corporate interests more vulnerable to regulation may have a stronger incentive to influence the political process).

Table 9: Effect of *Citizens United* on the average ideology

	Upper Chamber		Lower Chamber	
	(1)	(2)	(3)	(4)
	Avg Ideal Point	Avg Ideal Point	Avg Ideal Point	Avg Ideal Point
CitUn×BanState	0.070	0.181**	0.056	0.100*
	(0.049)	(0.077)	(0.044)	(0.053)
CitUn×BanState×Union		-0.176**		-0.041
		(0.071)		(0.053)
CitUn×BanState×Corp		-0.044		-0.043
		(0.100)		(0.129)
CitUn×BanState×Union×Corp		-0.014		-0.026
		(0.103)		(0.136)
State and Year FE	✓	✓	✓	✓
Deep South Trend	✓	✓	✓	✓
Observations	945.000	945.000	952.000	952.000
R-squared	0.924	0.930	0.945	0.946
F-stat p-val		0.003		0.157

Robust standard errors, clustered at state level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Columns 1 and 3 show the baseline effect in the modified time frame. Columns 2 and 4 include $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$. The F-statistics report the test of the null hypothesis of joint zero values of all four coefficients.

We also rule out one important alternative mechanism, namely voter turnout. In Table 10, we obtain a precisely estimated null effect, thus ruling out that *Citizens United* had an effect on participation.[11]

Table 10: Effect of *Citizens United* on turnout

	(1)	(2)	(3)
	Turnout	Turnout	Turnout
CitUn × BanState	-0.004	0.004	-0.007
	(0.011)	(0.011)	(0.010)
State and Year FE	✓	✓	✓
Deep South Trend		✓	
State-specific Trends			✓
Observations	750	750	750
R^2	0.880	0.884	0.918

Robust standard errors, clustered at state level in parentheses.

[11]We obtained data on turnout from the United States Elections Project: `http://www.electproject.org/`. To measure turnout, we use total ballots counted. For the states that do not report total ballots counted, we use highest office ballot counts.

Though our empirical findings suggest that Republicans benefited from groups advertising on their behalf (in line with Dowling and Wichowsky, 2016), our results do not reproduce the turnout-reducing effect of negative ads documented in Ansolabehere and Iyengar (1995) or the more positive impacts found in Franz et al. (2009). This null effect, however, may also be consistent with a reduction in turnout by Democratic voters that is precisely matched by an increase in turnout by Republican voters (a conjecture not too dissimilar from our salience framework). Lack of data availability prevents us from testing this possibility.

6.3 Robustness

In this subsection, we briefly discuss our placebo and robustness tests.

In Online Appendix E.3, we reproduce our analysis for electoral outcomes at the district-level and find similar results (though not statistically significant for the upper chamber). Our results are also robust to using alternative samples (e.g, dropping NH and NC which may have updated their regulation after the 2010 elections) or an alternative definition of our treatment (e.g., separating restrictions affecting corporations and restrictions affecting union), with details available upon request.

As mentioned above, we also use Bonica's (2014) DIME database whenever possible as an alternative measure of ideology. We find that all of our results are robust, and some even stronger, when we measure ideology with this alternative data source (see Online Appendix E.5 for details). We also conduct additional tests to confirm our (possibly surprising) null effect of lifting funding restrictions on polarization. In addition to taking advantage of the DIME database, we also use the share of moderate legislators an alternative measure of polarization and still find a null result (see Online Appendix E.4).

7 Conclusion

This paper provides a theoretical framework and an empirical approach to understand and evaluate the consequences of outside spending on electoral outcomes, the ideology of elected legislators, and polarization. Our theory, based on the idea that outside spending affects the salience of candidates' attributes (relative to their party labels), suggests that lifting restrictions on the funding of groups engaging in outside spending should increase the electoral strength of the candidates aligned with the groups experiencing the largest budgetary expansion as a result of the regulatory change. We estimate a large and robust pro-Republican effect and offer additional evidence supporting the notion that on average pro-Republican groups experienced a larger increase in funding. The effect on polarization is expected to be more nuanced as it depends on the changes in groups' budgets as well as the political situation pre-removal of funding restrictions on outside spending. Empirically, we document a null effect of *Citizens United* on polarization. We also rule out some possible confounders and alternative mechanisms that could explain our findings. Overall, our results suggest that, with the right tools, interest groups have the ability to shape electoral outcomes.

While we document a significant and persistent positive effect on Republican vote shares, our data do not allow us to fully explore why. We can, here, offer one conjecture. Unions, the traditional allies of Democrats, may not have had the required funds to counter the offensive from Republican donors (wealthy individuals, corporations) in the wake of the"right-to-work" laws which have weakened them and changed voting patterns (Feigenbaum et al., 2018). Our finding that the effect of the Supreme Court's decision is almost null in states with strong unions (Tables 8) is consistent with this interpretation, but more work is needed to uncover why Republicans gained so much for so long electorally. Another promising avenue of future research consists in analyzing the downstream policy consequences of the Supreme Court's decision. Following others (e.g., Hacker and Pierson, 2014; Anzia, 2019), we believe state-level politics, especially in the aftermath of *Citizens United*, presents a unique opportunity to better understand the influence of interest groups on elected officials.[12]

[12] Our own preliminary analysis of the effect of *Citizens United* on public good provision revealed no effect (results available upon request). But we conjecture this may be due to current data limitation.

References

Ansolabehere, Stephen, John M. De Figueiredo, and James M. Snyder Jr. 2003. "Why is there so little money in US politics?." Journal of Economic perspectives 17(1): 105-130.

Ansolabehere Stephen and Shanto Iyengar. 1995. "The Effectiveness of Campaign Advertising: It's All in the Context." In Candice Nelson and James A. Thurber eds., *Campaigns and Elections American Style*. Boulder, CO: Westview Press. 1995.

Anzia, Sarah F. 2019. "Looking for influence in all the wrong places: How studying subnational policy can revive research on interest groups." The Journal of Politics, 81(1), 343-351.

Aragonès, Enriqueta, Micael Castanheira, and Marco Giani. 2015. "Electoral competition through issue selection." American Journal of Political Science 59(1): 71-90.

Ashworth, Scott. 2006. "The Economics of Campaign Finance" in *The new Palgrave Dictionary of economics* (2nd edn). Basingstoke : Palgrave Macmillan.

Barber, Michael J. 2016. "Ideological Donors, Contribution Limits, and the Polarization of American Legislatures." The Journal of Politics 78(1): 296-310.

Baron, David P. 1994. "Electoral competition with informed and uninformed voters." American Political Science Review 88(1): 33-47.

Baumgartner, Frank R., Jeffrey M. Berry, Marie Hojnacki, Beth L. Leech, and David C. Kimball. 2009. *Lobbying and policy change: Who wins, who loses, and why.* University of Chicago Press, 2009.

Berry, Jeffrey M. 2019. "Interest Groups and Elections" in *The Oxford Handbook of Electoral Persuasion*, Elizabeth Suhay, Bernard Grofman, and Alexander H. Trechsel eds. Oxford, UK: Oxford University Press. Available at https://www.oxfordhandbooks.com/view/10.1093/oxfordhb/9780190860806.001.0001/oxfordhb-9780190860806-e-31.

Blanes i Vidal, Jordi, Mirko Draca, and Christian Fons-Rosen. 2012. "Revolving door lobbyists." American Economic Review 102(7): 3731-48.

Bonica, Adam. 2014. "Mapping the ideological marketplace." American Journal of Political Science 58(2): 367-386.

Buckley v. Valeo. 75-436 (424 U.S. 1 1976).

Burden, Barry C. and Amber Wichowsky. 2010. "Local and National Forces in Congressional Elections." In *The Oxford Handbook of American Elections and Political Behavior,* Jan E. Leighley, editor. 453-470. Oxford, UK: Oxford University Press.

Coate, Stephen. 2004. "Pareto-improving campaign finance policy." The American Economic Review 94(3): 628-655.

Citizens United v. Federal Electoral Commission. 08-205 (558 U.S. 310 2010).

Cohen, Bernard Cecil. 2015. *Press and foreign policy.* Princeton, NJ: Princeton University Press.

De Figueiredo, John M., and Brian Kelleher Richter. 2014. "Advancing the empirical research on lobbying." Annual Review of Political Science 17: 163-185.

Dragu, Tiberiu, and Xiaochen Fan. 2016. "An Agenda-Setting Theory of Electoral Competition." The Journal of Politics 78(4): 1170-83.

Feigenbaum, James, Alexander Hertel-Fernandez and Vanessa Williamson. 2018. "From the Bargaining Table to the Ballot Box: Political Effects of Right to Work Laws" NBER Working Paper No. 24259. http://www.nber.org/papers/w24259.

Franz, Michael M., Paul B. Freedman, Kenneth M. Goldstein, and Travis N. Ridout. 2009. *Campaign advertising and American democracy.* Philadelphia, PN: Temple University Press.

Giger, Nathalie, and Heike Klüver. 2016. "Voting against your constituents? How lobbying affects representation." American Journal of Political Science 60(1): 190-205.

Good, Chris. 2010. "Citizens United Decision: Republicans Like It, Liberals Don't." *The Atlantic*, January 21. https://www.theatlantic.com/politics/archive/2010/01/citizens-united-decision-republicans-like-it-liberals-dont/.

Grimmer, Justin. 2013. "Appropriators not Position Takers: The Distorting Effects of Electoral Incentives on Congressional Representation." American Journal of Political Science, 57(3): 624-642.

Grossman, Gene M., and Elhanan Helpman. 2001. *Special interest politics.* Cambridge, MA: MIT press.

Hall, Andrew B. 2013. "Systemic Effects of Campaign Spending: Evidence From Corporate Campaign Contribution Bans in State Legislatures." Political Science Research & Methods 4(2): 343-359.

Hall, Andrew and Alexander Fouirnaies. 2016. "The Exposure Theory of Access: Why Some Firms Seek More Access to Incumbents Than Others." Working Paper. https:dropbox.com/Fouirnaies_Hall_Regulation.pdf/

Hirsch, Barry T., David A. Macpherson, and Wayne G. Vroman. 2001. "Estimates of Union Density by State", *Monthly Labor Review*, Vol. 124(7): 51-55.

Klumpp, Tilman. 2014. "Populism, Partisanship, and the Funding of Political Campaigns." University of Alberta Working Paper. https://sites.ualberta.ca/ klumpp/docs/populism.pdf

Klumpp, Tilman, Hugo M. Mialon, and Michael A. Williams. 2016. "The Business of American Democracy: Citizens United, Independent Spending, and Elections." Journal of Law and Economics, 59, 1?43.

La Raja, Raymond J., and Brian F. Schaffner. 2014. "The effects of campaign finance spending bans on electoral outcomes: Evidence from the states about the potential impact of Citizens United v. FEC." Electoral Studies 33: 102-114.

Leech, Beth L. 2010. "Lobbying and influence." In *The Oxford Handbook of American Political Parties and Interest Groups*, L. Dandy Maisel, Jeffrey M. Berry, and George C. Edwards III, eds. 535-552. Oxford, UK: Oxford University Press.

Madestam, Andreas, Daniel Shoag, Stan Veuger, David Yanagizawa-Drott. 2103 "Do Political Protests Matter? Evidence from the Tea Party Movement." The Quarterly Journal of Economics 128(4): 1633–1685.

Meirick, Patrick C., Gwendelyn S. Nisbett, Lindsey A. Harvell-Bowman, Kylie J. Harrison, Matthew D. Jefferson, Tae-Sik Kim, and Michael W. Pfau. 2018. "To Tell the Truth: Ad Watch Coverage, Ad Tone, and the Accuracy of Political Advertising." Political Communication 35(3): 450-469.

North Dakota Attorney General. 2010. "Press Release: STENEHJEM CREATES ELECTION LAWS TASK FORCE." January 22. https://www.ag.nd.gov/NewsReleases/2010/01-22-10.pdf.

Petrova, Maria, Andrei Simonov, James M. Snyder Jr. 2019. "The Effects of *Citizens United* on U.S. State and Federal Elections." Unpublished manuscript. Available at https://0ec87679-a-62cb3a1a-s-sites.googlegroups.com/site/mariapetrovaphd/Citizens%20United%20draft.pdf.

Poole, Keith T., and Howard Rosenthal. 1997. *Congress: A Political-economic History of Roll Call Voting.* Oxford: Oxford University Press.

Prato, Carlo, and Stephane Wolton. 2017. "Citizens United: A Theoretical Evaluation." Political Science Research & Methods 5(3): 567-74.

Prato Carlo, and Stephane Wolton. 2019. "Campaign Cost and Electoral Accountability." Political Science Research & Methods 7(1): 1-21.

Rasmussen Reports Poll. 2016. "Political Influence." Feb. 9-10. http://www.rasmussenreports.com/public_content/politics/general_politics/february_2016/voters_say_money_media_have_too_much_political_clout

Shor, Boris, and Nolan McCarty. 2011. "The ideological mapping of American legislatures." American Political Science Review 105(3): 530-551.

Skaperdas, Stergios, and Bernard Grofman. 1995. "Modeling negative campaigning." American Political Science Review 89(1): 49-61.

Snyder, James. 1989. "Election Goals and the Allocation of Campaign Resources."Econometrica 57(3): 637-660.

Speech Now v Federal Election Commission. 08-5223 (1:08-cv-00248-JR 2010).

Spencer, Douglas M., and Abby K. Wood. 2014. "Citizens United, states divided: an empirical analysis of independent political spending." Indiana Law Journal 89: 315-372.

Stern, Robert M. 2012. "Sunlight State by State After *Citizens United*." Corporate Reform Coalition, June. https://www.citizen.org/documents/sunlight-state-by-state-report.pdf.

Tinkham, Spencer F., and Ruth Ann Weaver-Lariscy. 1991. "Advertising message strategy in US congressional campaigns: Its impact on election outcome." Current Issues and Research in Advertising 13(1-2): 207-226.

Werner, Timothy. 2011. "The sound, the fury, and the nonevent: Business power and market reactions to the Citizens United decision." American Politics Research 39(1): 118-141.

Wichowsky, Amber. 2012. "District Complexity and the Personal Vote." Legislative Studies Quarterly 37(4): 437–63.

Appendix

(FOR publication)

A Descriptive statistics

Table A.1: State Responses to Citizens United

State	Restrictions removed on
Alaska	February 19, 2010
Arizona	April 1, 2010
Colorado	May 25, 2010
Connecticut	June 8, 2010
Iowa	April 8, 2010
Kentucky	March 24, 2010
Massachusetts	February 8, 2010
Michigan	January 21, 2010
Minnesota	May 18, 2010
Montana	October 18, 2010
New Hampshire	
North Carolina	July 10, 2010
North Dakota	January 22, 2010
Ohio	February 26, 2010
Oklahoma	February 1, 2010
Pennsylvania	March 4, 2010
Rhode Island	July 13, 2010
South Dakota	March 11, 2010
Tennessee	June 23, 2010
Texas	April 21, 2010
West Virginia	March 13, 2010
Wisconsin	May 10, 2010
Wyoming	January 21, 2010

Source: Spencer and Wood (2014) except for Montana (Klumpp et al., 2015), North Carolina, North Dakota, and Rhode Island (authors' research). For Montana, the date is based on an injunction from the Montana District Court ruling (Klumpp et al., 2015). The court injunction was reversed by the Montana Supreme Court in 2011 only to be upheld by the US Supreme Court in 2012 so Montana is considered as treated in all elections. For North Carolina, the date is based on House Bill 748 (though it was approved by the Department of Justice. House Bill 748 (section 5 of the Voting Rights Act requires) in 2011. For North Dakota, the date is the date of a task force recognizing that the state laws were void (North Dakota Attorney General, 2010). For Rhode Island, the source is Stern (2012). An exact date could not be found for New Hampshire.

Table A.2: Pre-Treatment Summary Statistics

(a) Republican vote share

	With Restrictions		Without Restrictions	
	Mean	St. Dev.	Mean	St. Dev.
lower chamber	0.489	0.097	0.486	0.088
upper chamber	0.490	0.100	0.484	0.092

(b) Average Ideology (NPAT)

	With Restrictions		Without Restrictions	
	Mean	St. Dev.	Mean	St. Dev.
lower chamber	0.029	0.345	-0.080	0.373
upper chamber	-0.008	0.354	-0.067	0.337

(c) Median Ideology (NPAT)

	With Restrictions		Without Restrictions	
	Mean	St. Dev.	Mean	St. Dev.
lower chamber	0.133	0.535	-0.095	0.561
upper chamber	0.103	0.601	-0.046	0.515

(d) Polarization (NPAT–“Folded” Measure)

	With Restrictions		Without Restrictions	
	Mean	St. Dev.	Mean	St. Dev.
lower chamber	0.733	0.200	0.709	0.262
upper chamber	0.771	0.211	0.679	0.249

Online Appendix

(Not for publication)

B Proofs of formal model

Throughout this Appendix, we assume that

Assumption B.1. *The distribution of the partisan shock satisfies:*

$$\psi \leq \frac{1}{2}\frac{1}{\overline{\tau}+1-\underline{\tau}}$$

Since $-1+\underline{\tau} \leq V^J \leq \overline{\tau}$, Assumption B.1 guarantees that the probability that a candidate wins is always interior (between zero and one) for all advertising spendings by the groups. Hence, the problem of the group is well behaved as a result.

Proof of Lemma 1

Direct from observation of P^D (and $1-P^D$ for group r). □

Proof of Lemma 2

For each case, we write P^D and the FONC for each group (note that r maximizes $P^R = 1 - P^D$). The reader can verify that each group's problem is strictly concave, so the FONC implicitly define the optimal spending allocation. Throughout, we make use of Lemma 1 (recall that the proportions spent on each category sum to one).

(i) $\theta^D = 0, \theta^R = 1$. In this case

$$\begin{aligned}
P^D(0,1) &= \frac{1}{2} + \psi(\tau^D - \tau^R) + \psi p^D \alpha^D + \psi(1-p^R)\alpha^R - p^D + p^R \\
\alpha^D &= \frac{c_d^D B_d}{c_d^D B_d + (1-\pi_r^R)B_r} \\
\alpha^R &= \frac{(1-c_d^D)B_d}{(1-c_d^D)B_d + \pi_r^R B_r}
\end{aligned}$$

The associated FONC are

$$p^D \frac{(1-\pi_r^R)B_r}{\left[c_d^D B_d + (1-\pi_r^R)B_r\right]^2} - (1-p^R)\frac{\pi_r^R B_r}{\left[(1-c_d^D)B_d + \pi_r^R B_r\right]^2} = 0$$
$$p^D \frac{c_d^D B_d}{\left[c_d^D B_d + (1-\pi_r^R)B_r\right]^2} - (1-p^R)\frac{(1-c_d^D)B_d}{\left[(1-c_d^D)B_d + \pi_r^R B_r\right]^2} = 0$$

Taking the ratio of the FONC yields $c_d^D = 1-\pi_r^R$, substituting into the first FONC yields $c_d^D = \frac{p^D}{p^D+(1-p^R)}$.

(ii) $\theta^D = 1, \theta^R = 0$. In this case

$$\begin{aligned}
P^D(1,0) &= \frac{1}{2} + \psi(\tau^D - \tau^R) - \psi(1-p^D)\alpha^D - \psi p^R \alpha^R - p^D + p^R \\
\alpha^D &= \frac{(1-c_r^R)B_r}{(1-c_r^R)B_r + \pi_d^D B_d} \\
\alpha^R &= \frac{c_r^R B_r}{c_r^R B_r + (1-\pi_d^D)B_d}
\end{aligned}$$

The associated FONC are

$$(1-p^D)\frac{\pi_d^D B_d}{\left[(1-c_r^R)B_r + \pi_d^D B_d\right]^2} - p^R \frac{(1-\pi_d^D)B_d}{\left[c_r^R B_r + (1-\pi_d^D)B_d\right]^2} = 0$$
$$(1-p^D)\frac{(1-c_r^R)B_r}{\left[(1-c_r^R)B_r + \pi_d^D B_d\right]^2} - p^R \frac{c_r^R B_r}{\left[c_r^R B_r + (1-\pi_d^D)B_d\right]^2} = 0$$

Taking the ratio of the FONC yields $c_r^R = 1-\pi_d^D$, substituting into the first FONC yields $c_r^R = \frac{p^R}{1-p^D+p^R}$.

(iii) $\theta^D = 1, \theta^R = 1$. In this case

$$\begin{aligned}
P^D(1,1) &= \frac{1}{2} + \psi(\tau^D - \tau^R) - \psi(1-p^D)\alpha^D + \psi(1-p^R)\alpha^R - p^D + p^R \\
\alpha^D &= \frac{(1-\pi_r^R)B_r}{(1-\pi_r^R)B_r + \pi_d^D B_d} \\
\alpha^R &= \frac{(1-\pi_d^D)B_d}{(1-\pi_d^D)B_d + \pi_r^R B_r}
\end{aligned}$$

The associated FONC are

$$
\begin{aligned}
(1-p^D)\frac{(1-\pi_r^R)B_r}{\left[(1-\pi_r^R)B_r+\pi_d^D B_d\right]^2}-(1-p^R)\frac{\pi_r^R B_r}{\left[(1-\pi_d^D)B_d+\pi_r^R B_r\right]^2}&=0\\
(1-p^D)\frac{\pi_d^D B_d}{\left[(1-\pi_r^R)B_r+\pi_d^D B_d\right]^2}-(1-p^R)\frac{(1-\pi_d^D)B_d}{\left[(1-\pi_d^D)B_d+\pi_r^R B_r\right]^2}&=0
\end{aligned}
$$

Taking the ratio of the FONC yields $\pi_r^R = 1-\pi_d^D$, substituting into the first FONC yields $\pi_r^R = \frac{1-p^R}{1-p^D+1-p^R}$.

(iv) $\theta^D = 0, \theta^R = 0$. In this case

$$
\begin{aligned}
P^D(0,0) &= \frac{1}{2}+\psi(\tau^D-\tau^R)+\psi p^D\alpha^D-\psi p^R\alpha^R-p^D+p^R\\
\alpha^D &= \frac{c_d^D B_d}{c_d^D B_d+(1-c_r^R)B_r}\\
\alpha^R &= \frac{c_r^R B_r}{c_r^R B_r+(1-c_d^D)B_d}
\end{aligned}
$$

The associated FONC are

$$
\begin{aligned}
p^D\frac{(1-c_r^R)B_r}{\left[c_d^D B_d+(1-c_r^R)B_r\right]^2}-p^R\frac{c_r^R B_r}{\left[c_r^R B_r+(1-c_d^D)B_d\right]^2}&=0\\
p^D\frac{c_d^D B_d}{\left[c_d^D B_d+(1-c_r^R)B_r\right]^2}-p^R\frac{(1-c_d^D)B_d}{\left[c_r^R B_r+(1-c_d^D)B_d\right]^2}&=0
\end{aligned}
$$

Taking the ratio of the FONC yields $c_r^R = 1-c_d^D$, substituting into the first FONC yields $c_r^R = \frac{p^R}{p^D+p^R}$. □

Proof of Proposition 1

To prove this result, we employ the following Lemma

Lemma B.1. *For all positive constants γ_d, γ_r, the ratio*

$$
\frac{B_d\gamma_d - B_r\gamma_r}{B_d+B_r}
$$

(strictly) increases as a result of the lifting of funding restrictions if and only if $\beta_d > (\geq)\beta_r$.

Proof. Notice that without funding restrictions, we have

$$\frac{B_d\gamma_d - B_r\gamma_r}{B_d + B_r} = \frac{B_d^0(1+\beta_d)\gamma_d - B_r^0(1+\beta_r)\gamma_r}{B_d^0(1+\beta_d) + B_r^0(1+\beta_r)}$$

and with funding restrictions on outside spending, we have

$$\frac{B_d\gamma_d - B_r\gamma_r}{B_d + B_r} = \frac{B_d^0\gamma_d - B_r^0\gamma_r}{B_d^0 + B_r^0}.$$

Their difference equals

$$\begin{aligned}
&\frac{B_d^0(1+\beta_d)\gamma_d - B_r^0(1+\beta_r)\gamma_r}{B_d^0(1+\beta_d) + B_r^0(1+\beta_r)} - \frac{B_d^0\gamma_d - B_r^0\gamma_r}{B_d^0 + B_r^0} \\
=&\frac{B_d^0(\frac{1+\beta_d}{1+\beta_r})\gamma_d - B_r^0\gamma_r}{B_d^0(\frac{1+\beta_d}{1+\beta_r}) + B_r^0} - \frac{B_d^0\gamma_d - B_r^0\gamma_r}{B_d^0 + B_r^0} \\
\propto& B_d^0\gamma_d\frac{1+\beta_d}{1+\beta_r}B_r^0 - B_r^0B_d^0\gamma_r + B_d^0\frac{1+\beta_d}{1+\beta_r}B_r^0\gamma_r - B_d^0B_r^0\gamma_d \\
\propto& 2\frac{1+\beta_d}{1+\beta_r} - 2
\end{aligned}$$

Hence, the lifting of funding restrictions increases the ratio if and only if $\beta_d > \beta_r$. □

Substituting the equilibrium spending decisions from Lemma 2 into $P^D(\theta^D, \theta^R)$ yields

$$P^D(0,1) = \frac{1}{2} + \psi(\tau^D - \tau^R) - p^D + p^R + \psi(p^D + 1 - p^R)\frac{B_d}{B_r + B_d} \tag{B.1}$$

$$P^D(1,0) = \frac{1}{2} + \psi(\tau^D - \tau^R) - p^D + p^R - \psi(1 - p^D + p^R)\frac{B_r}{B_r + B_d} \tag{B.2}$$

$$P^D(1,1) = \frac{1}{2} + \psi(\tau^D - \tau^R) - p^D + p^R + \psi\frac{(1-p^R)B_d - (1-p^D)B_r}{B_r + B_d} \tag{B.3}$$

$$P^D(0,0) = \frac{1}{2} + \psi(\tau^D - \tau^R) - p^D + p^R + \psi\frac{p^D B_d - p^R B_r}{B_r + B_d} \tag{B.4}$$

This implies that the ex-ante expected winning probability of D equals

$$\begin{aligned}
P^D =&(1-p^D)p^R P^D(0,1) + p^D(1-p^R)P^D(1,0) + p^D p^R P^D(1,1) + (1-p^D)(1-p^R)P^D(0,0) \\
=&\frac{1}{2} + \psi(\tau^D - \tau^R) - p^D + p^R + \psi(p^D(1-p^D) + p^R(1-p^R))\frac{B_d - B_r}{B_r + B_d}
\end{aligned}$$

The result then follows from Lemma B.1. □

In the next proposition, we look at average ideology in the legislature. For this, as explained in the main text, we code Democrat's extremism as -1 and Republican's extremism as 1. As a result, positive numbers correspond to a more conservative legislature. We obtain:

Proposition B.1. *Average ideology strictly increases after the lifting of funding restrictions if and only if $\beta_r > \beta_d$.*

Proof. Using Equation B.1-Equation B.3 and the discussion above, the average ideology is:

$$
\begin{aligned}
\mathcal{I} =&(1-p^D)p^R(1-P^D(0,1)) + p^D p^R(1-2P^D(1,1)) - p^D(1-p^R)P^D(1,0)\\
=&\frac{(1-p^D)p^R - p^D(1-p^R)}{2} - \psi(\tau^D - \tau^R)(p^R + p^D)\\
&+\psi\frac{B_r}{B_r+B_d}\big(p^D(1-p^D) + p^D(1-p^D)p^R + p^D p^R(1-p^R)\big)\\
&-\psi\frac{B_d}{B_d+B_r}\big(p^R(1-p^R) + p^D(1-p^D)p^R + p^D p^R(1-p^R)\big)
\end{aligned}
$$

The result follows, again, from Lemma B.1. □

Proof of Proposition 2

Recall that polarization is measured as the average expected extremism of the winning candidate. This quantity equals

$$\Theta^W = p^D p^R + p^D(1-p^R)P^D(1,0) + (1-p^D)p^R(1-P^D(0,1))$$

An extremist is elected with probability one if two extreme candidates compete with each other (probability $p^D p^R$) or when an extremist faces a moderate and wins (prob. $p^J(1-p^{-J})P^J(1,0)$). Using Equation B.1 and Equation B.2, this is equivalent to

$$
\begin{aligned}
\Theta^W =&p^R + \frac{p^D(1-p^R) + p^R(1-p^D)}{2} + (p^D(1-p^R) - (1-p^D)p^R)\psi(\tau^D - \tau^R)\\
&-\psi p^D(1-p^R)(1-p^D+p^R)\frac{B_r}{B_d+B_r} - \psi(1-p^D)p^R(p^D+1-p^R)\frac{B_d}{B_d+B_r}
\end{aligned}
$$

After substituting for B_d and B_r with or without funding restrictions and re-arranging, we obtain that expected extremism post-lifting of funding restrictions increases if and only if

$$\begin{aligned} & \big(p^D(1-p^R)((1-p^D)+p^R)-(1-p^D)p^R(p^D+(1-p^R)))\big)(\beta_d-\beta_r)>0 \\ \Leftrightarrow \quad & \big(p^D(1-p^D)(1-2p^R)+p^R(1-p^R)(2p^D-1)\big)(\beta_d-\beta_r)>0 \end{aligned}$$

Note that the function $G(p^D)=p^D(1-p^D)(1-2p^R)+p^R(1-p^R)(2p^D-1)$ equals zero when p^D is equal to p^R, is concave in p^D on the interval $[0,1]$, $G(0)=-p^R(1-p^R)<0$ and $G(1)=p^R(1-p^R)>0$. Hence, there exists a unique solution to $G(p^D)=0$ in $[0,1]$ (two solutions would violate concavity), which is $p^D=p^R$, and for all $p^D>p^R$, $G(p^D)>0$. So $G(p^D)$ has the same sign as p^D-p^R.
As a result, polarization without funding restrictions is strictly greater than polarization with lifting of funding restrictions if and only if $(p^D-p^R)(\beta_d-\beta_r)>0$ as claimed. $\square$

C Confounders

C.1 Details on our analysis of campaign finance laws

Our analysis of the possible confounding effect of campaign finance laws follows Barber (2016)'s model specification and uses his original data set of campaign contribution limits for the lower houses of various state legislatures. We include the following controls: indicator if the state had unlimited individual contributions in a given year; indicator if the state had unlimited PAC contributions in a given year; an interaction terms that interacts a dummy variable that is equal to 1 when the state has any limit on individual contributions with the log of the limit amount; and an interaction term that interacts a dummy variable that is equal to 1 when the state has any limit on PAC contributions with the log of the limit amount. This allows the model to simultaneously estimate the effect of no contribution limits while also estimating the marginal effect of lowering limits conditional on a limit existing. Note that, as indicated in the text, information for campaign finance laws are only available for the lower house (hence, we only carry the analysis for this chamber).

C.2 Tea Party, REDMAP, and term limits

We complement our analysis of our main outcomes adding controls for the tea party, the Republican's party strategy REDMAP, and term limits. We find that controlling for these time-varying indicators does not change the impact the lifting of spending bans has on average and median ideology, despite some of these variables having independent impacts on these outcomes (see Tables C.1 and C.2).

Table C.1: Impact on Average Ideology with controls

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP
CitUn×BanState	0.037	0.065	0.064	0.029	0.050	0.079**
	(0.048)	(0.049)	(0.047)	(0.040)	(0.043)	(0.032)
Strong Tea Party	0.061	0.046	0.006	0.036	0.026	0.002
	(0.040)	(0.038)	(0.036)	(0.030)	(0.030)	(0.022)
REDMAP	0.034	0.034	0.048	0.004	0.002	-0.001
	(0.058)	(0.053)	(0.036)	(0.028)	(0.025)	(0.017)
Term Limit	0.040	0.036	-0.006	0.070**	0.066*	0.027
	(0.034)	(0.036)	(0.029)	(0.033)	(0.034)	(0.023)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.921	0.925	0.964	0.945	0.947	0.978

Robust standard errors, clustered at state level in parentheses

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

Table C.2: Impact on Median Ideology with controls

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Med IP	Med IP	Med IP	Med IP	Med IP	Med IP
CitUn×BanState	0.087	0.173	0.185	0.113	0.188	0.321**
	(0.125)	(0.122)	(0.132)	(0.108)	(0.113)	(0.128)
Strong Tea Party	0.137	0.091	0.033	0.167*	0.132	0.068
	(0.102)	(0.095)	(0.065)	(0.099)	(0.098)	(0.086)
REDMAP	0.247	0.246	0.213**	0.037	0.031	0.029
	(0.158)	(0.147)	(0.093)	(0.092)	(0.081)	(0.079)
Term Limit	0.078	0.064	0.013	0.198*	0.186	0.109
	(0.096)	(0.098)	(0.065)	(0.115)	(0.119)	(0.111)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.778	0.790	0.883	0.773	0.782	0.877

Robust standard errors, clustered at state level in parentheses

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

D Mechanism

We provide additional details on how we constructed the *Union* and *Corp* variables used in Section 6.2.

Union membership density data (from 1964-2015) used represents the percentage of each state's nonagricultural wage and salary employees who are union members. Data was obtained from Hirsch et al. (2001),[13] and was created using the Current Population Survey and the discontinued BLS publication *Directory of National Unions and Employee Associations*. To approximate which states at the time of *Citizens United* had a stronger union presence, we use data up to 2010 to construct the median percentage membership within each state.

Our *Corp* variable is based on an industry weighted index of exposure to regulation. Industry weights were obtained from disaggregated state level GDP data from the Bureau of Economic Analysis. The measure of industry level regulatory exposure developed by Hall and Fouirnaies, is based on publicly traded firms' annual 10-K filings with the SEC. Using text analysis, Hall and Fouirnaies construct an industry-level measure of self-reported regulatory risk. We refer the interested readers to Hall and Fouirniaies's (2016) paper for further details.[14]

Further, we also replicate the analysis using the continuous standardized version of these variables. Tables D.1-D.3 show that the standardized variables yields similar patterns to the ones displayed in Section 6.2: stronger unions decrease the pro-Republican impact of lifting restrictions on the funding of outside spending.

[13]Hirsch, Barry T., David A. Macpherson, and Wayne G. Vroman. 2001. "Estimates of Union Density by State", *Monthly Labor Review*, Vol. 124(7): 51-55.

[14]Hall, Andrew and Alexander Fouirnaies. 2016. "The Exposure Theory of Access: Why Some Firms Seek More Access to Incumbents Than Others." Working Paper. https:dropbox.com/Fouirnaies_Hall_Regulation.pdf/

Table D.1: Effect of *Citizens United* on the Republican vote share

	Upper Chamber		Lower Chamber	
	(1)	(2)	(3)	(4)
	Rep VS	Rep VS	Rep VS	Rep VS
CitUn × BanState	0.053**	0.052**	0.039**	0.038*
	(0.022)	(0.023)	(0.018)	(0.019)
CitUn × BanState×Union_std		-0.008		-0.008
		(0.013)		(0.012)
CitUn × BanState×Corp_std		-0.018		-0.018
		(0.016)		(0.016)
CitUn × BanState×Union_std×Corp_std		0.008		0.006
		(0.012)		(0.013)
State and Year FE	✓	✓	✓	✓
Deep South Trend	✓	✓	✓	✓
Observations	562	562	603	603
R-squared	0.718	0.721	0.865	0.868
F-stat p-val		0.021		0.033

Robust standard errors, clustered at state level in parentheses.

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

Columns 1 and 3 show the baseline effect in the modified time frame. Columns 2 and 4 include $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$, where $Union$ and $Corp$ variables have been standardized. The F-statistics report test of the null hypothesis of joint zero values of all four coefficients.

Table D.2: Effect of *Citizens United* on Legislators' Ideology (Average)

	Upper Chamber		Lower Chamber	
	(1)	(2)	(3)	(4)
	Avg IP	Avg IP	Avg IP	Avg IP
CitUn × BanState	0.070	0.072	0.056	0.057
	(0.049)	(0.045)	(0.044)	(0.046)
CitUn × BanState×Union_std		-0.080**		-0.015
		(0.034)		(0.026)
CitUn × BanState×Corp_std		-0.040		-0.051
		(0.038)		(0.046)
CitUn × BanState×Union_std×Corp_std		0.000		0.001
		(0.031)		(0.037)
State and Year FE	✓	✓	✓	✓
Deep South Trend	✓	✓	✓	✓
Observations	945	945	952	952
R-squared	0.924	0.930	0.945	0.947
F-stat p-val		0.002		0.009

Robust standard errors, clustered at state level in parentheses.

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

Columns 1 and 3 show the baseline effect in the modified time frame. Columns 2 and 4 include $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$, where $Union$ and $Corp$ variables have been standardized. The F-statistics report test of the null hypothesis of joint zero values of all four coefficients.

Table D.3: Effect of *Citizens United* on Legislators' Ideology (Median)

	Upper Chamber		Lower Chamber	
	(1)	(2)	(3)	(4)
	Med IP	Med IP	Med IP	Med IP
CitUn × BanState	0.189	0.199	0.208*	0.215
	(0.124)	(0.125)	(0.116)	(0.132)
CitUn × BanState×Union_std		-0.167		-0.009
		(0.115)		(0.100)
CitUn × BanState×Corp_std		-0.041		-0.091
		(0.151)		(0.174)
CitUn × BanState×Union_std×Corp_std		-0.021		-0.018
		(0.116)		(0.139)
State and Year FE	✓	✓	✓	✓
Deep South Trend	✓	✓	✓	✓
Observations	945	945	952	952
R-squared	0.788	0.796	0.776	0.777
F-stat p-val		0.177		0.089

Robust standard errors, clustered at state level in parentheses.

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

Columns 1 and 3 show the baseline effect in the modified time frame. Columns 2 and 4 include $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$, where $Union$ and $Corp$ variables have been standardized. The F-statistics report test of the null hypothesis of joint zero values of all four coefficients.

E Robustness

E.1 Placebo test on electoral outcomes

Here, we include the point estimates for the dynamic difference-in-differences depicted in the main text (Figures 4 and 6). To perform these tests, we augment our specifications by the leads and lags of the treatment variable. In line with the parallel trends assumption, the coefficients on all lead treatments are nearly zero (Tables E.1). We obtain similarly reassuring results when looking at ideology. We do find an effect for the lower chamber in years 2009 and 2010, but the effect is *negative*: suggesting that prior to 2010, legislatures in treated states were becoming more liberal due to reasons not accounted for in our specifications. Our findings indicate that the lifting of funding restrictions on outside spending has fully reversed this trend (see Tables E.2 and E.3).

Table E.1: Dynamic Difference-in-Differences for Republican Vote Share

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Rep VS	Rep VS	Rep VS	Rep VS	Rep VS	Rep VS
2002 lead	0.009	0.016	0.020	-0.001	0.008	0.022
	(0.019)	(0.018)	(0.023)	(0.011)	(0.011)	(0.016)
2004 lead	-0.007	0.001	0.008	-0.010	0.001	0.020
	(0.020)	(0.019)	(0.029)	(0.015)	(0.014)	(0.020)
2006 lead	0.007	0.018	0.026	-0.003	0.011	0.034
	(0.024)	(0.022)	(0.033)	(0.017)	(0.015)	(0.024)
2008 lead	-0.003	0.009	0.016	-0.005	0.010	0.037
	(0.023)	(0.023)	(0.034)	(0.019)	(0.017)	(0.026)
2010 Treatment	0.026	0.040*	0.052	0.027	0.046**	0.077**
	(0.024)	(0.023)	(0.041)	(0.019)	(0.019)	(0.031)
2012 lag	0.028	0.044	0.051	0.016	0.036	0.070*
	(0.033)	(0.033)	(0.049)	(0.028)	(0.028)	(0.036)
2014 lag	0.052	0.072	0.085	0.010	0.034	0.075*
	(0.045)	(0.046)	(0.067)	(0.026)	(0.025)	(0.042)
2016 lag	0.062*	0.082**	0.090	0.021	0.045	0.087*
	(0.035)	(0.035)	(0.061)	(0.028)	(0.028)	(0.046)
2018 lag	0.044	0.065*	0.086	0.019	0.048*	0.097*
	(0.034)	(0.034)	(0.066)	(0.029)	(0.028)	(0.050)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	650	650	650	696	696	696
R^2	0.680	0.695	0.793	0.804	0.831	0.920

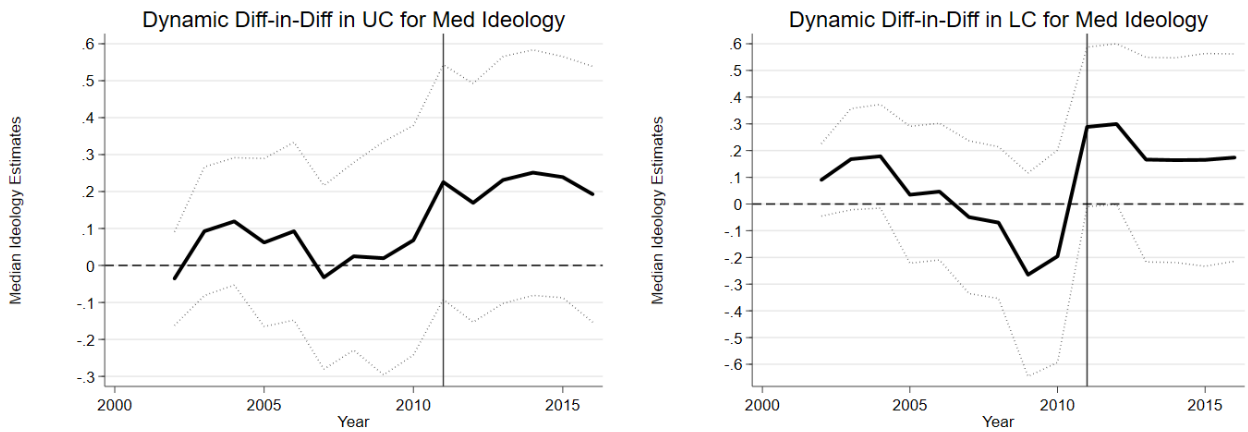
Robust standard errors, clustered at state level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

For completeness, we also represent the dynamic difference-in-differences for median ideology in Figure E.1 with the point estimates displayed in Table E.3

Table E.2: Dynamic Difference-in-Differences for Average Ideology

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP
2002 lead	-0.025	-0.016	0.004	0.005	0.012	0.000
	(0.017)	(0.018)	(0.021)	(0.022)	(0.023)	(0.014)
2003 lead	0.031	0.043	0.068	0.030	0.039	0.023
	(0.028)	(0.030)	(0.050)	(0.029)	(0.031)	(0.025)
2004 lead	0.034	0.049	0.079	0.033	0.044	0.025
	(0.030)	(0.032)	(0.058)	(0.029)	(0.031)	(0.028)
2005 lead	0.013	0.031	0.067	0.005	0.018	-0.005
	(0.038)	(0.039)	(0.066)	(0.034)	(0.036)	(0.037)
2006 lead	0.018	0.038	0.079	0.010	0.025	-0.001
	(0.039)	(0.041)	(0.073)	(0.034)	(0.035)	(0.041)
2007 lead	-0.028	-0.005	0.041	-0.033	-0.016	-0.046
	(0.044)	(0.045)	(0.068)	(0.040)	(0.042)	(0.049)
2008 lead	-0.017	0.008	0.061	-0.033	-0.014	-0.047
	(0.043)	(0.044)	(0.074)	(0.040)	(0.042)	(0.055)
2009 lead	-0.031	-0.001	0.051	-0.068	-0.046	-0.081
	(0.053)	(0.052)	(0.079)	(0.042)	(0.045)	(0.066)
2010 lead	-0.014	0.017	0.079	-0.056	-0.030	-0.076
	(0.053)	(0.053)	(0.088)	(0.048)	(0.051)	(0.070)
2011 Treatment	0.060	0.094	0.167	0.055	0.079	0.039
	(0.060)	(0.060)	(0.124)	(0.050)	(0.054)	(0.079)
2012 lag	0.031	0.066	0.153	0.046	0.073	0.026
	(0.061)	(0.062)	(0.127)	(0.049)	(0.054)	(0.086)
2013 lag	0.025	0.063	0.152	0.015	0.044	-0.003
	(0.071)	(0.073)	(0.121)	(0.063)	(0.068)	(0.093)
2014 lag	0.040	0.081	0.176	0.019	0.049	-0.001
	(0.069)	(0.071)	(0.126)	(0.061)	(0.067)	(0.099)
2015 lag	0.064	0.111	0.204	0.024	0.058	0.012
	(0.070)	(0.072)	(0.138)	(0.064)	(0.071)	(0.108)
2016 lag	0.030	0.079	0.191	0.017	0.053	0.007
	(0.068)	(0.073)	(0.144)	(0.064)	(0.072)	(0.114)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.921	0.925	0.965	0.944	0.946	0.979

Robust standard errors, clustered at state level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



(a) upper chamber (b) lower chamber

Figure E.1: Dynamic Difference-in-Difference estimates: Median Ideology

E.2 Average Ideology Impacts by party

In Tables E.4 and E.5, we break down the impact of average ideology by party. They indicate that the average Republican is slightly more moderate after *Citizens United* and the average Democrat is slightly more liberal. Since NPAT only provides one ideology per legislator (not a time-varying one), these results indicate that moderate Democrats (relative to all Democrats) tend to leave our sample and relatively moderate Republicans tend to enter it. That is, consistent with our interpretation in the text, moderate Democrats are replaced by moderate Republicans. Though, as we noted there, estimates are too noisy to warrant any definitive conclusion.

Table E.3: Dynamic Difference-in-Differences for Median Ideology

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Med IP	Med IP	Med IP	Med IP	Med IP	Med IP
2002 lead	-0.064	-0.035	0.020	0.067	0.090	-0.022
	(0.063)	(0.065)	(0.070)	(0.067)	(0.069)	(0.047)
2003 lead	0.056	0.093	0.162	0.137	0.168*	0.022
	(0.089)	(0.089)	(0.139)	(0.092)	(0.097)	(0.080)
2004 lead	0.075	0.119	0.202	0.142	0.179*	-0.000
	(0.087)	(0.088)	(0.157)	(0.093)	(0.099)	(0.096)
2005 lead	0.009	0.062	0.159	-0.009	0.035	-0.177
	(0.116)	(0.116)	(0.205)	(0.127)	(0.131)	(0.155)
2006 lead	0.032	0.093	0.204	-0.004	0.047	-0.199
	(0.122)	(0.123)	(0.229)	(0.126)	(0.131)	(0.172)
2007 lead	-0.101	-0.032	0.093	-0.106	-0.049	-0.327
	(0.127)	(0.127)	(0.201)	(0.142)	(0.146)	(0.215)
2008 lead	-0.052	0.025	0.164	-0.133	-0.070	-0.381
	(0.129)	(0.129)	(0.227)	(0.141)	(0.145)	(0.233)
2009 lead	-0.072	0.020	0.166	-0.339*	-0.265	-0.609**
	(0.166)	(0.161)	(0.244)	(0.190)	(0.194)	(0.292)
2010 lead	-0.029	0.068	0.235	-0.282	-0.196	-0.588*
	(0.161)	(0.158)	(0.267)	(0.196)	(0.203)	(0.307)
2011 Treatment	0.123	0.226	0.425	0.205	0.289*	-0.113
	(0.167)	(0.162)	(0.352)	(0.144)	(0.152)	(0.273)
2012 lag	0.061	0.170	0.407	0.208	0.300*	-0.138
	(0.167)	(0.165)	(0.365)	(0.144)	(0.153)	(0.295)
2013 lag	0.115	0.231	0.488	0.070	0.166	-0.299
	(0.173)	(0.170)	(0.388)	(0.187)	(0.195)	(0.361)
2014 lag	0.127	0.251	0.526	0.061	0.164	-0.334
	(0.171)	(0.169)	(0.407)	(0.186)	(0.195)	(0.382)
2015 lag	0.096	0.239	0.520	0.050	0.165	-0.344
	(0.166)	(0.166)	(0.421)	(0.187)	(0.203)	(0.380)
2016 lag	0.045	0.193	0.509	0.051	0.174	-0.374
	(0.171)	(0.176)	(0.439)	(0.179)	(0.198)	(0.399)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.776	0.789	0.884	0.772	0.781	0.881

Robust standard errors, clustered at state level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E.4: Effect of *Citizens United* on ideology–Republicans

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP
Cit_United x BanState	-0.072	-0.081	0.000	-0.007	-0.017	-0.028
	(0.050)	(0.053)	(0.036)	(0.047)	(0.049)	(0.033)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State Specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.891	0.891	0.961	0.952	0.953	0.988

Robust standard errors, clustered at state level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E.5: Effect of *Citizens United* on ideology–Democrats

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP
Cit_United x BanState	0.017	-0.012	-0.062	-0.015	-0.032	-0.030
	(0.046)	(0.045)	(0.041)	(0.048)	(0.048)	(0.039)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State Specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.930	0.934	0.973	0.945	0.947	0.983

Robust standard errors, clustered at state level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.3 District-level analysis of electoral outcomes

To analyze the impact of outside spending on district level electoral outcomes, we define a dummy variable, $RepWins_{dst}$, which takes the value 1 (0) when a Republican candidate wins (respectively loses) district d in state s in year t. Due to redistricting issue, we only consider the years 2002 to 2010 (9 states experience mid-decade redistricting and we use only the years with similar district as in 2010). Since North Carolina experiences multiple redistricting episodes up to 2009, we drop it from the analysis. Furthermore, we exclude multi-member districts (approximately 5% of our sample) unless they have staggered elections or separate ballots so we can attribute electoral outcomes to a single candidate.

Overall, we are left with 19,794 observations for the lower chamber and 5,363 for the upper chamber. We run the following baseline specification, with standard errors clustered at the state level:

$$RepWins_{dst} = \beta CitUn_t \times BanState_s + \gamma_{ds} + \delta_t + \epsilon_{dst} \tag{E.1}$$

The results of the baseline and additional estimations (with Deep South Trend and State-specific trends) are summarized in Table E.6.

	lower chamber			**upper chamber**		
	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	Rep Win	Rep Win	Rep Win	Rep Win	Rep Win	Rep Win
CitUn×BanState	0.030	0.039*	0.057**	0.032	0.052	0.049
	(0.021)	(0.022)	(0.027)	(0.048)	(0.044)	(0.054)
District & Year FE	X	X	X	X	X	X
Deep South Trend		X			X	
State-specific Trends			X			X
Observations	19,794	19,794	19,794	5,363	5,363	5,363
R-squared	0.831	0.831	0.835	0.862	0.863	0.868

Robust standard errors clustered at state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.6: Effect of *Citizens United* on Republicans' winning probability at the district level

Across the columns, we find a consistent positive effect of the lifting of spending bans on the probability that a Republican is elected. Notice that the coefficient (and statistical significance) of our estimates is different when we augment the baseline specification in column 1 with State-specific time trends (column 3). Columns 2 and 3 show that this discrepancy can be reasonably attributed to a few Deep South states that are already trending Republican during this time period and experience no change in the spending bans, inducing a downward bias in our first specification. When we correct for this in column 2, we see that the coefficient becomes significant and closer in magnitude to column 3's specification.

Using our preferred specification in column 3, we can see that outside spending increases the probability of Republican victory in the lower house by approximately 5.7 percentage points. The effect is not significant when looking at the upper chamber, but across specifications coefficients are remarkably similar to the ones for the lower chamber. Our estimates are comparable to, but slightly smaller than Klumpp et al.'s (2015) who use a similar approach (see Table 3 in their paper). The difference could be due to two factors. First, we exclude districts which experienced mid-decade redistricting so our number of observations is lower than theirs (19,794 against 21,656 for the lower chamber). Second, we take a more parsimonious approach omitting controls such as the number of candidates running due to the risk that such variables induce post-treatment bias.

E.4 Alternative measure of polarization

Here, we check whether the null effect of *Citizens United* is robust to an alternative measure of polarization. Following Hirano et al. (2010), we use the share of moderates in the legislature, with increased polarization proxied by a decreased in the proportion of say moderates. We define moderates as legislators whose score is between the 45^{th} and 55^{th} percentile prior to 2010 (that is, a NPAT score between $-.197$ and $.15$ for the lower chamber and between $-.169$ and $.145$ for the upper chamber). Table E.7 contains summary statistics for this measure.

Table E.7: Pre-Treatment Summary Statistics (NPAT-Share of Moderate Legislators)

	With Restrictions		Without Restrictions	
	Mean	St. Dev.	Mean	St. Dev.
lower chamber	0.081	0.088	0.114	0.119
upper chamber	0.068	0.093	0.112	0.145

Table E.8 displays our difference-in-differences estimates. It confirms that the removal of bans had no statistically discernible effect on the share of moderate legislators (if anything, the coefficients seem to point towards an increase in the share of moderates). The results are robust to alternative cutoffs for moderation (e.g., the 47.5^{th} and 52.5^{th} percentiles; details available upon request).

Table E.8: Effect of *Citizens United* on average share of moderate legislators

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Mod Share	Mod Share	Mod Share	Mod Share	Mod Share	Mod Share
CitUn×BanState	0.023	0.018	-0.008	0.032*	0.023	-0.002
	(0.019)	(0.021)	(0.014)	(0.016)	(0.016)	(0.014)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	945	945	945	952	952	952
R^2	0.888	0.889	0.963	0.890	0.895	0.960

Robust standard errors, clustered at state level in parentheses.

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

Moderate Legislators have NPAT scores in $[-.158, .256]$ for the Lower Chamber and in $[-.123, .239]$ for the Upper Chamber.

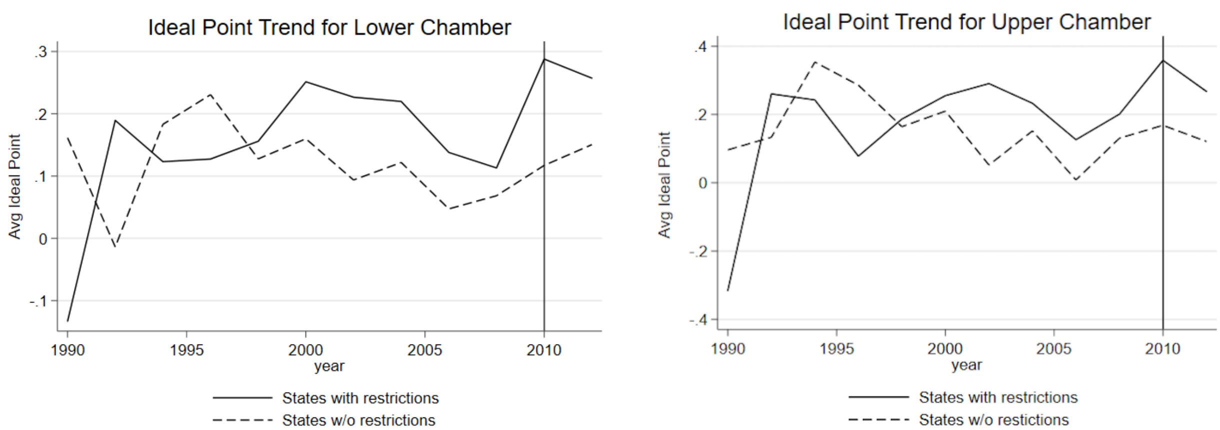
E.5 Alternative measure of ideology

In this subsection, we look at the effect of lifting funding restrictions on ideology using Bonica's (2014) DIME scores. Table E.9 first describes the summary statistic for this variable to benchmark our estimates.

Table E.9: Pre-Treatment Summary Statistics (Average Ideology DIME)

	With Restrictions		Without Restrictions	
	Mean	St. Dev.	Mean	St. Dev.
lower chamber	0.190	0.408	0.097	0.426
upper chamber	0.221	0.438	0.116	0.453

Figure E.2 shows that trends between control and treated states look markedly different before 2000. Upon further inspection of the database, we noticed incomplete data in the years prior to 2000. Due to the incomplete data, we restrict the samples to the year 2000-2014 (ideology values are not available after this date). While the trends seem more parallel in the period 2000-2014, there still exist visible differences between control and treated states which we address using state-specific trends.



(a) lower chamber

(b) upper chamber

Figure E.2: Trends for Average Ideology using DIME

In Bonica's DIME, state legislators' ideology is measured each election year (rather than averaged across their whole tenure). As a result, the statistical power of our tests is significantly higher than in the baseline analysis. However, as explained above, ideal points are measured using contributions which are affected by the regulatory change (see Klumpp et al., 2015). The results should thus be interpreted with caution.

Table E.10 confirms the results in the main text: legislatures become more conservative as a result of the lifting of funding restrictions. The point estimates are slightly larger and of higher statistical significance.

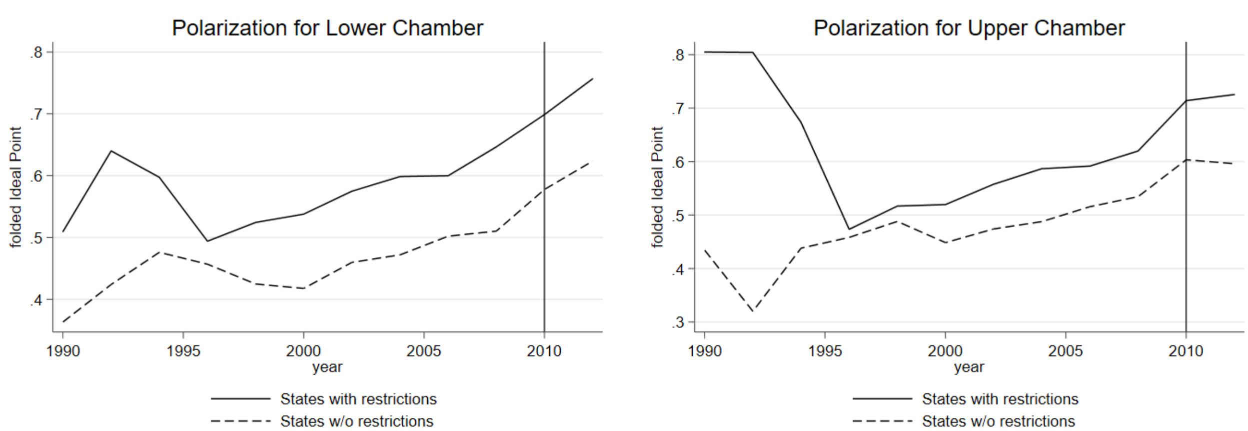
Table E.10: Effect of *Citizens United* on average ideology (DIME database)

	Lower Chamber			Upper Chamber		
	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP	Avg IP
CitUn×BanState	0.047	0.069*	0.108**	0.072*	0.087**	0.113*
	(0.034)	(0.036)	(0.043)	(0.039)	(0.041)	(0.066)
State and Year FE	X	X	X	X	X	X
Deep South Trend		X			X	
State-specific Trends			X			X
Observations	306	306	306	278	278	278
R-squared	0.955	0.957	0.973	0.929	0.930	0.945

Robust standard errors, clustered at state level in parentheses.

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

Regarding polarization, we recover the same problem as for average ideology (see Figure E.3). In Tables E.11 and E.12, we again limit our analysis using data from 2000-2014. We again recover a null effect of *Citizens United* whether we measure polarization using Barber's folded scale or the share of moderates.



(a) lower chamber (b) upper chamber

Figure E.3: Trends in polarization ("folded" value of DIME)

Table E.11: Effect of *Citizens United* on polarization using DIME scores in Lower Chamber

	Lower Chamber					
	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	Polarization	Polarization	Polarization	Moderates	Moderates	Moderates
CitUn × BanState	0.008	0.016	0.005	0.002	-0.003	-0.002
	(0.026)	(0.023)	(0.034)	(0.018)	(0.015)	(0.020)
State and Year FE	X	X	X	X	X	X
Deep South Trend		X			X	
State-specific Trends			X			X
Observations	306	306	306	306	306	306
R-squared	0.936	0.937	0.969	0.728	0.731	0.860

Robust standard errors, clustered at state level in parentheses.

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

Table E.12: Effect of *Citizens United* on polarization using DIME scores in Upper Chamber

	Upper Chamber					
	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	Polarization	Polarization	Polarization	Moderates	Moderates	Moderates
CitUn × BanState	0.041	0.051	0.036	-0.022	-0.029	-0.020
	(0.046)	(0.044)	(0.041)	(0.018)	(0.019)	(0.030)
State and Year FE	X	X	X	X	X	X
Deep South Trend		X			X	
State-specific Trends			X			X
Observations	278	278	278	278	278	278
R-squared	0.813	0.815	0.886	0.612	0.616	0.716

Robust standard errors, clustered at state level in parentheses.

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

Finally, we conduct our heterogeneous analysis in Table E.13 using the DIME score instead of NPAT score. Patterns are very consistent with the main text. The effect is stronger in places where corporations are well aligned with the Republican party and unions are weak.

Table E.13: Heterogeneous effect of *Citizens United* on average ideology (DIME)

	Lower Chamber		**Upper Chamber**	
	(1)	(2)	(3)	(4)
VARIABLES	Avg Ideal Point	Avg Ideal Point	Avg Ideal Point	Avg Ideal Point
CitUn×BanState	0.069*	0.058	0.087**	0.138**
	(0.036)	(0.036)	(0.041)	(0.059)
⋯×Union		0.026		-0.073
		(0.058)		(0.073)
⋯×Corp		-0.079		-0.115
		(0.099)		(0.118)
⋯×Union×Corp		0.109		0.143
		(0.140)		(0.153)
State and Year FE	X	X	X	X
Deep South Trend	X	X	X	X
Observations	306	306	278	278
R-squared	0.957	0.958	0.930	0.932
F-statistic p-value		0.124		0.093

Robust standard errors, clustered at state level in parentheses.

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

Columns 1 and 3 show the baseline effect. Columns 2 and 4 include additional independent variables $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$. The F-statistics reported are testing the null hypothesis of joint zero values of all four coefficients.

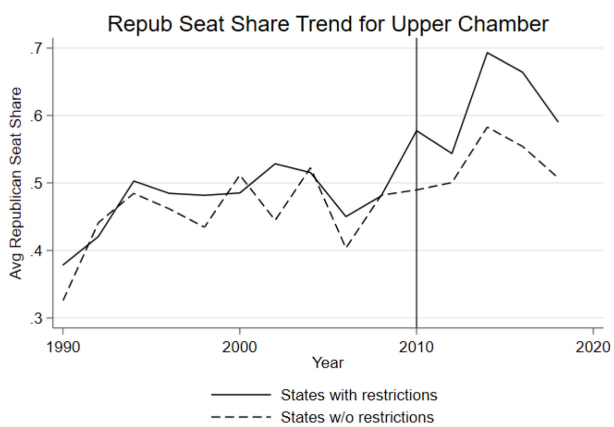
F Additional results: Republican seat shares

In this subsection, we look at the electoral effect of *Citizens United* using Republican seat shares (Rep SS) rather vote shares. Table F.1 presents the summary statistics for this variable.

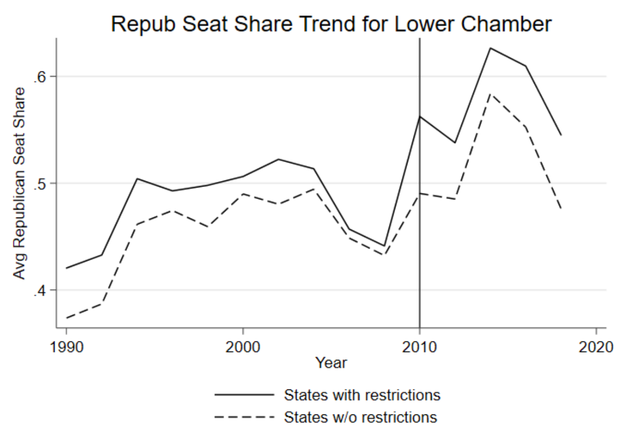
Table F.1: Pre-Treatment Summary Statistics: Seat Share

	With Restrictions		Without Restrictions	
	Mean	St. Dev.	Mean	St. Dev.
lower chamber	0.479	0.157	0.453	0.166
upper chamber	0.472	0.175	0.454	0.192

Figure F.1 displays tends before the Supreme Court's decision. As for vote shares (Figure 3), trends appear to be relatively parallel prior to 2010.



(a) upper



(b) lower

Figure F.1: Trends in average Republican seat share

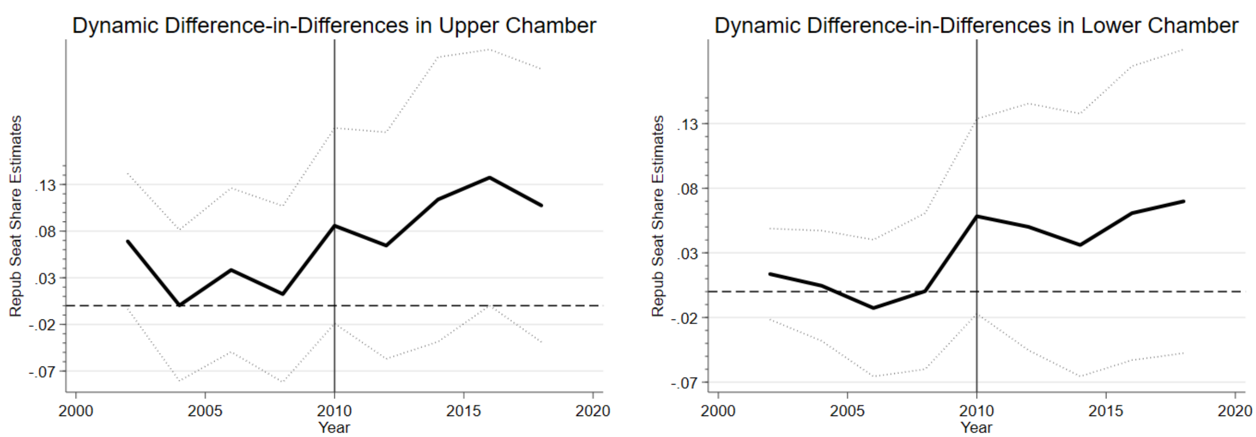
Table F.2 looks at the effect of *Citizens United* on Republican seat shares. The effect is large and approximates 5.5% in both chambers. This corresponds to an increase of more than 11% in Republican seat shares in both chambers (0.054/0.479 for the lower house and 0.055/0.472 for the upper house). It also represents more than 30% of a standard deviation.

Table F.2: Effect of *Citizens United* on Republican seat share

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Rep SS	Rep SS	Rep SS	Rep SS	Rep SS	Rep SS
CitUn×BanState	0.066	0.089*	0.055*	0.029	0.054	0.054**
	(0.047)	(0.047)	(0.032)	(0.039)	(0.041)	(0.026)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	650	650	650	696	696	696
R^2	0.692	0.702	0.819	0.799	0.816	0.928

Robust standard errors, clustered at state level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As for vote shares, the impact of *Citizens United* is not temporary and seems to persist over many elections as Figure F.2 shows (estimates can be found in Table F.3).



(a) upper chamber

(b) lower chamber

Figure F.2: Dynamic Difference-in-Difference estimates: Seat shares

We also reproduce our re-analysis with controls (Tea party, REDMAP, Term limit) in Table F.4. Again, our results are robust to the inclusion of theses controls.

Finally, we look at the heterogeneous effect of *Citizens United* as a function trade unions strength and the alignment of the corporate interests with the Republican party (proxied by exposure to regulation). Table F.5 shows a similar pattern as in the main text (Table 8). The electoral conse-

Table F.3: Dynamic Difference-in-Differences for Republican Seat Share

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Rep SS	Rep SS	Rep SS	Rep SS	Rep SS	Rep SS
2002 lead	0.058	0.069*	0.069	0.002	0.014	0.016
	(0.036)	(0.037)	(0.043)	(0.018)	(0.018)	(0.019)
2004 lead	-0.013	0.000	0.001	-0.011	0.005	0.008
	(0.041)	(0.041)	(0.047)	(0.023)	(0.022)	(0.026)
2006 lead	0.021	0.038	0.040	-0.032	-0.013	-0.008
	(0.046)	(0.045)	(0.060)	(0.029)	(0.027)	(0.036)
2008 lead	-0.007	0.013	0.013	-0.021	0.000	0.004
	(0.048)	(0.048)	(0.064)	(0.032)	(0.031)	(0.042)
2010 Treatment	0.062	0.086	0.089	0.032	0.058	0.066
	(0.054)	(0.053)	(0.075)	(0.038)	(0.038)	(0.047)
2012 lag	0.039	0.065	0.062	0.023	0.050	0.055
	(0.061)	(0.062)	(0.080)	(0.048)	(0.049)	(0.058)
2014 lag	0.080	0.114	0.117	0.003	0.036	0.046
	(0.076)	(0.078)	(0.103)	(0.049)	(0.052)	(0.071)
2016 lag	0.106	0.137*	0.132	0.027	0.061	0.066
	(0.067)	(0.070)	(0.101)	(0.054)	(0.058)	(0.076)
2018 lag	0.072	0.107	0.119	0.029	0.070	0.082
	(0.073)	(0.075)	(0.113)	(0.058)	(0.060)	(0.078)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	650	650	650	696	696	696
R^2	0.694	0.705	0.821	0.799	0.816	0.929

Robust standard errors, clustered at state level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

quences are stronger in states where unions are weak and corporate interests more ideological (using continuous values rather than dichotomized one broadly shows the same patter, see Table F.6).

Table F.4: Impact on Republican Seat Share with controls

	Upper Chamber			Lower Chamber		
	(1)	(2)	(3)	(4)	(5)	(6)
	Rep SS	Rep SS	Rep SS	Rep SS	Rep SS	Rep SS
CitUn×BanState	0.064	0.086*	0.056*	0.027	0.052	0.056**
	(0.046)	(0.047)	(0.033)	(0.039)	(0.041)	(0.027)
Strong Tea Party	0.037	0.028	0.004	0.030	0.017	0.004
	(0.032)	(0.031)	(0.030)	(0.022)	(0.020)	(0.016)
REDMAP	-0.008	-0.012	0.007	-0.008	-0.010	-0.009
	(0.047)	(0.042)	(0.036)	(0.027)	(0.021)	(0.013)
Term Limit	0.044	0.040	0.013	0.033	0.029	0.016
	(0.035)	(0.035)	(0.025)	(0.026)	(0.027)	(0.014)
State and Year FE	✓	✓	✓	✓	✓	✓
Deep South Trend		✓			✓	
State-specific Trends			✓			✓
Observations	650	650	650	696	696	696
R^2	0.696	0.705	0.819	0.802	0.818	0.929

Robust standard errors, clustered at state level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F.5: Effect of *Citizens United* on the Republican seat share

	Upper Chamber		Lower Chamber	
	(1)	(2)	(3)	(4)
	Rep SS	Rep SS	Rep SS	Rep SS
CitUn×BanState	0.081*	0.153***	0.053	0.098**
	(0.044)	(0.051)	(0.040)	(0.042)
CitUn×BanState×Union		-0.095*		-0.037
		(0.050)		(0.043)
CitUn×BanState×Corp		-0.043		-0.062
		(0.100)		(0.106)
CitUn×BanState×Union×Corp		-0.029		-0.006
		(0.113)		(0.114)
State and Year FE	✓	✓	✓	✓
Deep South Trend	✓	✓	✓	✓
Observations	562.000	562.000	603.000	603.000
R-squared	0.720	0.732	0.829	0.835
F-stat p-val		0.017		0.023

Robust standard errors, clustered at state level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Columns 1 and 3 show the baseline effect in the modified time frame. Columns 2 and 4 include $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$. The F-statistics report test of the null hypothesis of joint zero values of all four coefficients.

Table F.6: Effect of *Citizens United* on the Republican seat share

	Upper Chamber		Lower Chamber	
	(1)	(2)	(3)	(4)
	Rep SS	Rep SS	Rep SS	Rep SS
CitUn × BanState	0.081*	0.081*	0.053	0.053
	(0.044)	(0.044)	(0.040)	(0.040)
CitUn × BanState×Union_std		-0.031		-0.004
		(0.024)		(0.021)
CitUn × BanState×Corp_std		-0.053		-0.054
		(0.034)		(0.033)
CitUn × BanState×Union_std×Corp_std		0.008		0.005
		(0.026)		(0.025)
State and Year FE	✓	✓	✓	✓
Deep South Trend	✓	✓	✓	✓
Observations	562	562	603	603
R-squared	0.720	0.728	0.829	0.835
F-stat p-val		0.033		0.010

Robust standard errors, clustered at state level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Columns 1 and 3 show the baseline effect in the modified time frame. Columns 2 and 4 include $Union$, $Union \times CitUn$, $Corp$, $Corp \times CitUn$, $Corp \times Union$, $Corp \times CitUn \times Union$, where $Union$ and $Corp$ variables have been standardized. The F-statistics report test of the null hypothesis of joint zero values of all four coefficients.