Did Money Polarize the Republican Party?
Estimating the Causal Impact of Citizens United on State Legislative Preferences

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Abstract
A prominent claim in the popular press is that wealthy donors have purchased the election of economically conservative legislators in order to protect donor wealth (e.g. Mayer 2016). The campaign finance literature has, however, struggled to identify causal effects from money in elections. Extending recent work by Klumpp et al (2016), we leverage the Supreme Court’s ruling in Citizens United v. FEC (2010) to achieve causal identification. Estimates consistently suggest that the Citizens United-induced removal of state bans on independent spending by corporations and unions led to increased conservatism among newly elected Republican state legislators, and to decreased per capita state expenditures on redistribution. These estimates, which are robust to a series of matching and placebo exercises, may provide support for the claim that campaign money has the net effect of shifting outcomes in a more conservative direction.

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A spending edge is the only thing that gives a Republican a chance to compete.

Senator Mitch McConnell, Jr. (R, KY)\[1\]

1 Introduction

Over the last two decades the Republican party has experienced remarkable success in state legislative races. Before the 1992 elections the party held majorities in 31% of state legislative chambers. After the 2016 elections, the party held majorities in 68% of state legislative chambers. The largest single gain during this period was during the 2010 elections, when the proportion of state legislative chambers with Republican party majorities increased from 37% to 59% (see Figure 1).

![Proportion of State Legislative Chambers With Republican Majorities](image1)

Figure 1: Proportion of State Legislative Chambers With Republican Majorities
Source: National Conference on State Legislatures

The growth in the number of state legislative seats held by Republican incumbents has been widely noted in the popular press. What has been less widely noted is that Republican-majority state legislative chambers have grown not only in number, but also in conservatism. As reported in Figure 2, using NPAT Common Space scores (Shor and McCarty 2011), the average median legislator in Republican-majority state legislative chambers has become significantly more conservative over the last twenty years.

![Average State Legislative Chamber Median Chambers With Republican Majorities](image2)

Figure 2: Average State Legislative Chamber Median Chambers With Republican Majorities
Source: Shor and McCarty (2011)

Republican candidates have moved to the right and simultaneously won more votes in federal elections as well. For example, the Republican share of the House national popular vote grew

\[1\] Mayer 2016, p. 234.
from 43.2 to 53.4 percent between 1976 and 2010, while the average DW-NOMINATE score of Republican House incumbents increased from .20 to .71 over the same time period (McCarty et al 1997). The phenomenon of Republican polarization coupled with increasing Republican electoral success, a phenomenon largely unobserved for the Democratic party, has been widely noted but remains generally unexplained (Barber and McCarty 2015).

A currently prominent story in the popular press is that increasingly wealthy donors have used increasingly large sums of money to buy the election of increasingly conservative Republican representatives, largely in order to protect donor wealth. Jane Mayer’s journalistic *Dark Money* (2016), for example, recounts a coordinated effort by extraordinarily wealthy individuals to elect conservative Republican legislators at all levels of government beginning in the 1970s, an effort that gained steam as success led to policies which increased donor wealth, enabling the donation of even larger sums of money to the cause: "In the course of a few decades a handful of enormously rich right-wing philanthropists had changed the course of American politics" (Mayer 2016, 375).

Mayer’s story is troubling, yet exceedingly difficult to test empirically. It is true that the flow of money into campaigns has increased dramatically since the mid-1970s. However, efforts to identify causal relationships between campaign money and outcomes suffer from significant identification problems (Hall 2015).

Extending recent work by Klumpp et al (2016), we estimate the causal impact of the Supreme Court’s ruling in *Citizens United v. FEC* (2010) on state legislative preferences and policy outcomes in state legislatures. In *Citizens United*, the Court struck federal restrictions on independent spending by corporations and unions that had been in force since 1947. Because the ruling in *Citizens United* was based on a sweeping First Amendment argument, the ruling also struck statutes in 23 states banning independent spending by corporations and unions in state legislative and gubernatorial races; many of these statutes pre-dated the federal statute. As of the 2010 state legislative and gubernatorial elections, these spending bans were no longer in effect.

We estimate the effects of *Citizens United* using both conventional difference in differences (DD) designs as well as matching on pretreatment trends in the LHS variables. We find that the removal of state bans on independent campaign spending led to the election of more conservative Republican state legislators, to more conservative state legislatures, and to less redistributive state spending.
Pre-ruling placebo tests indicate that these effects are not found in any years prior to 2010. Placebo tests using the 5-point ideology question on the Cooperative Congressional Election Study (CCES) suggest that the observed treatment effects were not due to differential evolution in voters’ long-term ideological preferences across control and treatment states. We conclude with a discussion of the possible relevance of the findings to the claim that campaign money has the net effect of shifting outcomes in a more conservative direction.

2 Identifying the Effects of Money in Elections

The Dark Money claim is that American democracy has been captured by very wealthy economic conservatives. The latter are said to have used their wealth to secure the election of a sufficient number of conservative Republican legislators, at all levels of government, so as to move policy in a less redistributive direction.

In the language of social science, the claim is that, in a right-skewed income distribution, we would expect the median donor to favor less redistribution, relative to the median voter, and therefore to disproportionately support less-redistributive candidates (Feddersen and Gul 2014). As the income distribution shifts further to the right, we would expect the median donor to support candidates who are even more conservative, relative to the position of the median voter (Ibid). Further, we know from both experimental and quasi-experimental evidence that campaign funds can affect electoral outcomes. For example, campaigns can have significant effects on turnout (Green, McGrath, and Aronow 2013, Enos and Fowler 2016). Radio and television ads have also been shown experimentally to have short-term effects on voters’ preferences (Gerber et al 2011). Greater access to campaign money by more conservative candidates may enable relatively more effective efforts to increase the turnout of more conservative voters, and/or to shift the short-term preferences of voters in a more conservative direction.

Yet we have little reliable evidence on the actual effects of money in elections, despite a large literature on same. Canonical papers in this literature include Jacobson 1978, Abramowitz 1988, Green and Krasno 1988, Jacobson 1990, Levitt 1994, Gerber 1998, Erikson and Palfrey 2000, and Gerber 2004. Virtually all of the findings in this literature are unidentified, however; candidates’

\footnote{In empirical work conducted simultaneously with our own, Abdul-Razzak et al (2018) also find that Citizens United is associated with increased relative conservatism of Republican state legislative incumbents.}
fundraising and spending choices are almost certainly endogenous to their likely electoral success. Some studies have attempted to use the variation in state-level campaign finance laws in an effort to achieve identification (Besley and Case 2003, Stratmann 2006, Stratmann and Aparicio-Castillo 2006, La Raja and Schaffner 2014, Hall 2015, Barber 2016). However, these findings are vulnerable to the endogeneity of campaign finance laws to state-level covariates. Recent experimental work on the effects of money in elections shows great promise, yet findings are as yet relatively limited in scope (Panagopolous and Green 2008, Kalla and Broockman 2016).

Following Klumpp et al. (2016), we turn to the Supreme Court’s largely unanticipated ruling in *Citizens United v. FEC* (2010), repealing *inter alia* 23 state statutes banning certain kinds of independent spending in state elections, to achieve identification.

### 3 *Citizens United v. FEC* (2010)

Prior to their Court-induced repeal in 2010, prohibitions on independent campaign spending by corporations and unions were of long standing. At the federal level, the Taft-Hartley Act of 1947 had prohibited any corporation or labor organization from making a "contribution or expenditure in connection with any election" for federal office. The Federal Election Campaign Act Amendments of 1974 incorporated this prohibition into Section 441b, while creating the option for corporations and unions to set up political action committees permitted to accept voluntary donations from corporate employees or union members, for the purpose of making contributions or independent expenditures in elections. Donations, contributions, and expenditures were all strictly limited under this option.

In 1986, the Supreme Court narrowed the reach of Section 441b’s ban on corporate or union independent expenditures in elections to include only those expenditures constituting "express advocacy" for or against a clearly identified candidate, and carved out an exception to the ban for nonprofit voluntary political associations (*FEC v. Massachusetts Citizens for Life, Inc.* (1986)). In 1990 the Court rejected a First Amendment challenge to a state-level ban on independent corporate expenditures for or against candidates for office (*Austin v. Michigan Chamber of Commerce* (1990)).

In 2002 the Congress passed the Bipartisan Campaign Reform Act (BCRA) to extend Section 441b’s ban on independent expenditures by corporations and unions to include expenditures that, while avoiding the so-called "magic words" of express advocacy, nonetheless were the functional
equivalent of express advocacy. These tv or radio ads, termed "electioneering communications," were defined by whether a candidate was mentioned by name, the closeness of a primary or general election, and the size of the audience reached. In 2007 the Supreme Court narrowed this provision to prohibit only corporate- or union-sponsored electioneering communications that were "susceptible of no reasonable interpretation other than as an appeal to vote for or against a specific candidate" (FEC v. Wisconsin Right to Life, Inc. (2007)).

As recounted by Spencer and Wood (2014), the Court’s ruling in *Citizens United v. FEC*, 558 U.S. 310, issued on January 21, 2010, was largely unexpected. The appellant in the case, a small nonprofit conservative organization that had accepted a negligible amount in corporate contributions, had not originally posed a First Amendment challenge to independent spending in support of express advocacy by corporations and unions. The communication in question, a feature length film about Hillary Clinton to be broadcast on demand, did not meet the traditional definition of express advocacy. The question posed by the appellant was merely whether the film could receive an exemption from the BCRA’s ban on electioneering communications, whether because the film was capable of an interpretation other than as an appeal to vote against Hillary Clinton, because video on demand is not broadcast television, because the likely audience would have been sufficiently small, or because Citizens United had received only a trivial amount in corporate contributions.

But in a sweeping First Amendment ruling, the Supreme Court held that no independent expenditures of any kind could be constitutionally restricted under the First Amendment: "We now conclude that independent expenditures, including those made by corporations, do not give rise to corruption or the appearance of corruption" (558 U.S. 331).3

The broad First Amendment ruling in *Citizens United* also repealed the bans on corporate and union independent spending in state legislative and gubernatorial races on the books in 23 states in 2010, rendering these bans invalid for the remainder of the 2010 state election cycle.4 Many of these bans had pre-dated the Taft-Hartley Act. Figure 18 in the Appendix maps the states that had such bans in 2010, and the date of the first election in which each statute took effect.

*Citizen United*’s unexpected repeal of these state-level independent spending bans created a

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3 In *SpeechNow.org et al v. FEC*, decided on March 26, 2010, the D.C. Circuit Court held that *Citizens United* also invalidated any restrictions on contributions by individuals to independent expenditure-only political committees.

4 We follow Klumpp et al (2016) in coding New Hampshire’s $5,000 cap on corporate independent spending as effectively a ban, and also in coding Montana’s statute as having been repealed for the 2010 elections, due to the injunction granted on October 18, 2010 to suspend the statute.
natural experiment. States with independent spending bans were forced to repeal them without having chosen to do so voluntarily; indeed, elected officials in repeal states were vocal in expressing their hostility to the Court’s ruling (Spencer and Wood 2014). The ruling thus offers the opportunity, in a perhaps narrow yet identified context, to test the Dark Money story. Did the repeal of state-level restrictions on independent spending, by presumably increasing the availability of campaign funds for more conservative candidates, increase the conservatism of successful Republican candidates for state legislative office, and/or decrease state per capita redistributive spending, in those states affected by the ruling?

We focus on these reduced form questions, rather than on the flow of money that we presume mediated between the bans’ repeal and electoral and policy outcomes, because of the difficulties inherent in identifying the causal impact of campaign money. Money can affect electoral outcomes from the sidelines, without ever being contributed and/or spent; the mere knowledge that unlimited sums could be raised and spent independently after Citizens United may have affected outcomes (Fox and Rothenberg 2011). Conversely, the fact that money is raised and spent does not imply an effect on outcomes (Hall 2015).

We nonetheless note empirical evidence that is not inconsistent with the proposition that Citizens United disproportionately increased the availability of campaign funds for more conservative state legislative candidates. As reported in Figure 3 prior to Citizens United, reported outside spending in federal races was dominated by groups predominantly supporting liberal (Democratic) candidates. However, after Citizens United, reported outside spending in federal races has been dominated by groups predominantly supporting conservative (Republican) candidates.\footnote{The CRP reports total outside spending as an aggregation of independent expenditures, electioneering communications, and communications costs; more details on the definitions of these categories can be found at \url{http://www.opensecrets.org/outsidespending/cycle_tots.php}. Cooper, Gulen and Ovtchinnikov (2010) also found that between 1979 and 2004, corporations donated disproportionately to Republican candidates in federal elections. Likewise, Klumpp et al (2016) found that Republican Super PACs raised about 50% more, and spent about 100% more, than Democratic Super PACS in the post-Citizens United 2010 and 2012 federal elections.}
Likewise, as reported in Figure 4, the ratio of independent spending in support of Republican state legislative candidates to that in support of Democratic state legislative candidates grew more in the first post-

_Citizens United_ elections in treated states, relative to the same ratio in control states.\footnote{The National Institute on Money in State Politics reports independent spending in support of state legislative candidates for those states with disclosure requirements, which here include AR, CA, ID, IL, ME, MO, and WA (control states) and AK, AZ, CO, CT, IA, KY, MA, MI, MN, MT, NC, NH, OH, OK, PA, RI, TN, TX, WI, WV, and WY (treated states). Also see Spencer and Wood (2014), who found larger increases in per capita independent expenditures in state elections between 2006 and 2010 in treated states, relative to control states. Besley and Case (2003) likewise found that state statutes restricting corporate campaign contributions were positively associated with the Democratic seat share in both chambers of state legislatures between 1950 and 2000, and Hall (2015) found both that corporations disproportionately contributed to the Republican party in state legislative races from 1990-2012, and that state statutes prohibiting corporate contributions during this period were associated with higher Democratic seat shares in state legislatures.}

We emphasize, however, that drawing inferences from spending is suspect for the reasons discussed above; we instead look at the direct effects of the repeal of state independent spending bans on state legislative outcomes. Using a similar reduced form design, Klumpp, Mialon, and Williams (2016) found that _Citizens United_ was associated with increases in the probabilities that Republican incumbents ran for reelection and that Republican candidates won office in state lower house legislative elections between 2000 and 2012, and with decreases in the probability that Democratic candidates contested races in state lower house legislative elections over the same period. We extend...
this design to estimate the effect of *Citizens United* on state legislative preferences.

![Graph showing the ratio of Republican to Democratic independent spending from 2008 to 2012. The graph includes two lines: one representing treated states and the other representing control states. The X-axis represents the years 2008, 2010, and 2012, and the Y-axis represents the ratio of Republican to Democratic independent spending.](image)

Figure 4: Reported Outside Spending in Support of State Legislative Candidates

Source: National Institute on Money in State Politics

4 State Legislative Preferences

4.1 Data

Our first question is whether the *Citizens United*-induced repeal of state statutes prohibiting independent spending by corporations and unions in state elections increased the conservatism of newly elected Republican candidates.

We use two different measures of state legislators’ preferences, estimated from two different kinds of underlying data. Common Space Campaign Finance (CF) scores are estimated from the common space of donors who give to multiple candidates, and candidates who receive campaign funds from multiple donors, and are reported in the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica 2015). These estimates, scaled to lie between -2 (most liberal) and 2 (most conservative), tell us how state legislators differ from one another in terms of their respective populations of donors. NPAT Common Space scores are estimated within states from the roll call votes cast by state legislators during each legislative year, and are then transformed into the common space of Project Vote Smart’s National Political Awareness Test (now called the Political Courage
These estimates, scaled to lie between -1 (most liberal) and 1 (most conservative), tell us how state legislators differ from one another in terms of their roll call voting behavior.

The ruling in *Citizens United* was in effect for all state elections held in 2010. Elections held in 2012 were held under post-2010 redistricting plans. Likewise, elections held before 2002 were generally held under post-1990 districting plans. We restrict our attention to the elections held between 2002-2010, inclusive (and therefore to the state legislative terms of 2003-2011, inclusive), and to those districts in the CF data with complete biennial preference data for the election cycles of 2002-2010, inclusive, and those districts in the NPAT data with complete annual preference data for the years 2003-2011, inclusive. Treated states are those states identified in Figure 18 as having a ban on independent spending in effect at the time of *Citizens United*; control states are all other states.

Both the Common Space CF and the NPAT Common Space scores assign a single preference estimate to a legislator over the course of his or her legislative career. As a result, we gain no information about the effect of *Citizens United* in districts wherein incumbents retained their seats in the 2010 elections. We thus further restrict our attention to districts wherein freshmen legislators were elected in the 2010 elections.

Figure 5 reports averaged Common Space CF scores by year, party, and treatment status for the 1056 districts electing freshman state legislators in 2010 for which we have complete preference data; Figure 6 reports the analogous data for the 645 districts for which we have complete NPAT Common Space data. In these figures, 2003-2010 represent pre-treatment years, and 2011 represents the first post-treatment year.

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7 Common Space CF election years have been translated into NPAT Common Space legislative years.
In both figures, all districts electing freshman Republicans moved to the right after the 2010 elections. However, districts in the states affected by *Citizens United* moved more sharply to the right, relative to districts in states unaffected by the ruling. These post-ruling trends in Republican state legislators’ estimated preferences were not simply the continuation of pre-ruling trends. In both figures, districts in the treated states that elected freshmen Republicans in 2010 were trending in a more liberal direction prior to *Citizens United*, relative to their counterparts in the control states.

Also in both figures, districts electing freshman Democratic legislators in 2010 move slightly to the left in the 2010 elections. Democratic freshmen elected in 2010 in the treated states appear to move their districts somewhat less to the left, relative to their Democratic freshmen counterparts in the control states. However, these differences are small.

Table [1] in the Appendix, reporting pre-ruling covariate balance tests for each measure of legislative preferences, indicates that control and treated districts differed on several covariates prior to the Court’s ruling in *Citizens United*. In subsequent analyses we both control for and match on pre-ruling covariates.
4.2 Difference in Differences (DD) and Coarsened Exact Matching (CEM) Regressions

We are interested in the differences between the estimated preferences of freshman legislators elected in 2010 and the incumbents they replaced. The primary challenge to inference is that pre-ruling trends in state legislative preferences were not parallel across control and treated districts, as evident in Figures 5 and 6 and Table 1. We take two different approaches to modeling these pre-treatment trends. In the DD analysis, the outcomes of interest are the estimated preferences of state legislators, $\text{Leg Prefs}_{ist}$, which are defined by district $i$ in state $s$ during year $t$. These outcomes are assumed to be generated by the following equation:

$$
\text{Leg Prefs}_{ist} = \beta [\text{Spending Ban}_s \times \text{Post-CU}_t] + \gamma X_{st} + \alpha_{is} + \mu_t + \varphi_s t + \epsilon_{ist} \tag{1}
$$

$\text{Spending Ban}_s$ is a dummy variable equal to 1 if state $s$ had a ban on independent spending in place before the Court’s ruling in Citizens United. $\text{Post-CU}_t$ is a dummy variable equal to 1 if the election cycle is 2010. $X_{st}$ is a vector of time-varying state-level covariates, namely median household income, percent African American, percent bachelor’s degree, percent aged 18-24, and percent aged 25-44. District fixed effects $\alpha_{is}$ (which subsume state and chamber fixed effects) are included to address fixed differences in preferences across districts. Year fixed effects $\mu_t$ are included to absorb year-specific shocks unrelated to Citizens United. State-specific linear time trends $\varphi_s t$ control for trends in state legislators’ preferences unrelated to Citizens United. $\epsilon_{ist}$ is the error term. We estimate this model for the full sample and for the subsets of districts won by the Democratic and Republican parties in 2008, respectively. All models are estimated using OLS with robust standard errors clustered on states.

In Equation 1 we address the potential confounder of non-parallel pre-treatment trends by controlling for state-specific time trends. However, this strategy allows for the inclusion of districts in both the treatment and control groups that are sufficiently anomalous in their pre-treatment trends as to have no clear counterparts in the corresponding group. The inclusion of these districts may bias estimates. Pre-processing the data to prune these anomalous districts can improve estimates of causal effects (Ho et al. 2007, Iacus et al. 2011). Matching treatment and control districts on their district-level pre-treatment trends also improves on techniques that match units on the basis of static
measures of pre-treatment covariates, for example through propensity score matching, because these
techniques do not address pre-treatment trends in the data (Dimmery 2015).

We can estimate most of the variation in pre-ruling trends in state legislators’ preferences with a
simple linear regression of district-level preferences on time. We match districts on these estimated
coefficients using Coarsened Exact Matching (CEM) and Sturge’s rule to bin observations. Table
2 in the Appendix reports pre- and post-matching differences in means for pre-ruling trends in
district-level state legislative preferences. Matching eliminates any significant pre-ruling differences
in means for these estimated trend coefficients.

For the CEM regressions, our outcome of interest is the difference in the estimated preferences of
a freshman legislator elected in 2010, and the incumbent s/he replaced ($\Delta LegPrefs_{is}$). We assume
this outcome is generated by the following equation, which is defined by state legislative district $i$
in state $s$:

$$\Delta LegPrefs_{is} = \beta SpendingBan_{s} + \lambda Z_{is} + \gamma X_{s} + \varepsilon_{is}$$

(2)

$Z_{is}$ represents a vector of district-level pre-ruling covariates, namely the estimated pre-ruling trend
in district-level legislative preferences and a district’s mean pre-ruling legislative preference. $X_{s}$ is
a vector of state-level pre-ruling covariates, namely pre-ruling average state-level median household
income, percent African American, percent with a bachelor’s degree, percent aged 18-24, and percent
aged 25-44. $\varepsilon_{is}$ is the error term. Equation 2 is estimated using OLS with weights derived from
Coarsened Exact Matching on pre-ruling trends in district-level legislative preferences, with robust
standard errors clustered on states.

Figure 7 reports the coefficients on the interaction term $SpendingBan_{s} \times Post-CU_{i}$, and Figure
8 reports the coefficients on $SpendingBan_{s}$, for both measures of legislative preferences, by party
of the freshman legislator elected in 2010, along with 95% confidence intervals.

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8 Results are robust to other coarsening strategies, including Scott’s rule, the Freedman-Diaconis rule, and Shimazaki-Shinomoto’s rule.

9 In Figure 7, Democratic Freshmen (CF) N = 1,719; Democratic Freshmen (NPAT) N = 1,719; Republican
Freshmen (CF) N = 3,695; Republican Freshmen (NPAT) N = 4,086. In Figure 8, Democratic Freshmen (CF) N = 315; Democratic Freshmen (NPAT) N = 182; Republican Freshmen (CF) N = 731; Republican Freshmen (NPAT) N = 449.
Republican freshman state legislators elected in 2010 in the treated districts move their districts more sharply to the right, by .14 - .44 CF points, and by .16 - .61 NPAT points, relative to the district-level changes in state legislative preferences observed in the control districts. Democratic freshman state legislators elected in 2010 in the treated districts also appear to move their districts more to the right, relative to their counterparts in the control districts, but these movements are small and not distinguishable from zero at conventional thresholds.

5 Pre-Ruling Placebo Tests

An alternative strategy to assess whether pre-ruling trends are confounding identification is to conduct placebo tests during the pre-ruling period. We construct a series of placebo tests on our LHS variables of interest by implementing a placebo Citizens United ruling in each year prior to the actual ruling for which data is available, using Equation 1 to estimate each DD parameter of interest.\textsuperscript{10} For each placebo model the year of the placebo ruling represents the last year of data used to estimate the model.


\textsuperscript{10}For the 2004 placebo tests, for which there are only two years of data, we drop the year fixed effects and the state-specific linear time trends.
set of districts electing freshman Republican state legislators in the year of the ruling (actual or placebo).

The Common Space CF models are estimated using only the set of districts holding biennial elections throughout the period of interest; the NPAT Common Space models are estimated using only the set of districts for which there are annual preference estimates. There are again significant treatment effects only after the actual *Citizens United* ruling, not after any of the placebo rulings.

### 6 Voter Ideology Placebo Tests

We can also challenge our identification strategy through the use of voters’ ideological preferences. If the repeal of state-level prohibitions on independent spending by corporations and unions in January of 2010 had an immediate impact on the state elections held in the fall of 2010, we would not expect the causal path to have run through voters’ relatively slow-moving ideological preferences (Gerber et al 2011). Rather, it is more plausible that the causal path ran through shorter-term campaign-induced factors such as differential turnout and/or priming of voters’ candidate preferences.

If we were to find, therefore, that voters’ overall ideological dispositions became more conservative post-treatment in the treated states, relative to the control states, this would strongly suggest

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11 Common Space CF Full sample N = 3,695; 2008 Placebo sample N = 1, 444; 2006 Placebo sample N = 1,014; 2004 Placebo sample N = 1,018. NPAT Common Space Full sample N = 4,086; 2010 Placebo sample N = 184; 2009 Placebo sample N = 1,687; 2008 Placebo sample N = 582; 2007 Placebo sample N = 1,405; 2006 Placebo sample N = 76; 2005 Placebo sample N = 1,140; 2004 Placebo sample N = 108.
that we are picking up not the short-term effect of *Citizens United*, but rather some longer-term process of ideological change differentiating the treated states from the control states. We can then use voters’ ideological dispositions in a placebo test. If our primary findings in fact represent short-term effects of *Citizens United*, rather than a longer-term effect of ideological change, then we would not expect voters’ ideological dispositions to follow the same pattern as legislators’ partisan affiliations and ideological preferences.

We source data on voters’ ideological dispositions from the Cooperative Congressional Election Studies (CCES) for the years 2006-2010, inclusive. Figure 11 reports respondents’ average responses to a 5-point ideology question, by treatment status, for the full sample of respondents and for those identifying as Democrats and Republicans on the CCES 7-point party identification question. There appear to be few differences across respondents in control and treated states.

![Figure 11: Average Responses to CCES 5-Point Ideology Question](image)

The outcome of interest for this placebo test is a respondent’s self-identified ideological preference, defined by respondent \( i \) in state \( s \) during year \( t \). This outcome is assumed to be generated

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12 The 5-point ideology question asks, "Thinking about politics these days, how would you describe your own political viewpoint?" Possible answers are: Very Liberal, Liberal, Moderate, Conservative, Very Conservative, and Not Sure. We code Not Sure as missing; coding these responses alternatively as Moderate does not qualitatively change results. The five remaining answers are coded from 1 to 5, increasing in conservatism. The 7-point party identification question asks, "Generally speaking, do you think of yourself as a [REPUBLICAN/DEMOCRAT]? Would you call yourself a strong [REPUBLICAN/DEMOCRAT] or a not very strong [REPUBLICAN/DEMOCRAT]? If Independent, do you think of yourself as closer to the Republican or Democratic party?"
by a slightly modified version of Equation 1, where \( Z_{ist} \) is a vector of individual-level covariates, namely indicator variables corresponding to categories for respondents’ household income, level of education, race, gender, employment status, response to the 7-point party identification question, as well as a continuous variable for respondent birth year. \( \alpha_s \) are state fixed effects, and \( \varphi_{st} \) are state-specific linear time trends in respondents’ ideology. We estimate this model for the full sample of respondents and for those identifying as Democrats and Republicans on the 7-point party identification question (dropping the party ID variable for the two subsamples). All models are estimated using OLS with robust standard errors clustered on states.\(^{13}\)

Figure 12: Estimated Post-Citizens United Change in CCES Respondent Ideology

Figure 12 reports the coefficients on the interaction term \( Spending\ Ban_s \times Post-CU_t \) for these three models, along with 90% confidence intervals. There are no significant post-ruling differences between respondents in treated and control states.

7 State Chambers and Budgets

We would finally like to know whether the apparent effects of Citizens United in state legislative elections had downstream effects on state budgets. The Supreme Court’s repeal of state bans on independent spending in state elections appears to have led to the election of both more Republican

\(^{13}\)Full sample \( N = 137,624 \); Democratic subsample \( N = 47,261 \); Republican subsample \( N = 42,698 \).
state legislators (Klumpp et al 2016), and more conservative state legislators. These effects may have been present in a sufficient number of districts to move chamber medians to the right and/or to flip chambers from Democratic to Republican majorities. These state-level effects may in turn have led to changes in state policymaking.

We investigate these possible state-level effects of *Citizens United* using the NPAT Common Space chamber medians estimated and reported by Shor and McCarty (2011), the identity of the party holding a majority of seats in each legislative chamber as reported by the National Conference of State Legislatures, and the state governmental expenditure data reported annually by the Census Bureau.

### 7.1 State Legislative Chamber Medians and Party Majorities

#### 7.1.1 Data

Figure 13 reports the average NPAT chamber median, and Figure 14 reports the average number of state legislative chambers with Republican majorities (ranging from 0 to 2), for control and treated states from 2003 through 2011. Nebraska’s nonpartisan unicameral state legislative chamber is omitted from Figure 14.

![Figure 13: Average Chamber Medians NPAT Common Space Scores](image1)

![Figure 14: Average Number of Republican Chamber Majorities](image2)

In the 2010 elections there are increases in the average conservatism of state legislative chamber medians, and in the number of state legislative chambers with Republican majorities, in both the control and treated states. However, in both cases, these increases are much larger in the states
affected by *Citizens United*.

### 7.1.2 CEM Regressions

For the CEM regressions we first match control and treated states on their pre-ruling linear trends in the LHS variables, using CEM and Sturge’s rule to bin observations. Our outcomes of interest are the differences in a) a state’s average chamber median in legislative year 2011, relative to 2010 ($\Delta \text{Average Median}_s$), and b) a state’s average number of Republican chamber majorities in 2011, relative to 2010 ($\Delta \text{Repub Majorities}_s$). We assume these outcomes are generated by a slightly modified version of Equation 2, where $X_s$ represents a vector of state-level pre-ruling covariates, namely the estimated pre-ruling trend in a state’s average chamber median and its average pre-ruling average chamber median, or the estimated pre-ruling trend in a state’s number of Republican chamber majorities and its average pre-ruling number of Republican majorities, in addition to pre-ruling average state-level median household income, percent African American, percent with a bachelor’s degree, percent aged 18-24, and percent aged 25-44. Both models are estimated using OLS with weights derived from Coarsened Exact Matching on pre-ruling trends, with robust standard errors.\(^1\)

\[\text{Figure 15: CEM-Weighted Estimated Post-Citizens United Changes in State Legislative Chambers}\]

\(^1\)In Figure 15, Average Chamber Median $N = 28$; Republican Chamber Majorities $N = 49$. 

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Figure 15 reports the coefficients on Spending Ban$_s$, for both measures of state legislative outcomes, along with 90% confidence intervals. Relative to control states, treated state chamber median averages became .43 NPAT points more conservative, and treated states’ average number of Republican chamber majorities increased by approximately .5.

7.2 State Welfare Spending

7.2.1 Data

We would like to know whether the Citizens United-induced changes in state legislative chambers affected state-level policy making. The Census Bureau reports state expenditures by functional areas (e.g., corrections, highways, natural resources, etc.). While some expenditures within these functional areas may have redistributive impact, others may not; it is difficult to predict what impact a more conservative legislature would have on these expenditures. However, the functional area of public welfare spending clearly reports state expenditures with redistributive impact. State public welfare spending is reported in thousands of current dollars.$^{15}$

The Census Bureau collects data on state governmental expenditures for each fiscal year. With only four exceptions, state fiscal years close on June 30 of the Census survey year (e.g. 2011 Census data covers FY 2011, closing on June 30, 2011). The four exceptions are New York (March 31), Texas (August 31), and Alabama and Michigan (September 30). State legislators elected in the 2010 elections, taking office in January 2011, would have had no impact on FY 2011 expenditures, and only a few months to enact policies affecting FY 2012 expenditures. The first fiscal year in which we would expect to see an impact of legislators elected in post-Citizens United elections would have been FY 2013. By extension, the first fiscal year in which we would expect to have seen the effect of policy choices made by state legislators elected in the 2002 elections would have been FY 2005.

$^{15}$The Bureau provides the following definition for this category of expenditures: “Support of and assistance to needy persons contingent upon their need. Excludes pensions to former employees and other benefits not contingent on need.” The category aggregates “cash assistance paid directly to needy persons under the categorical programs Old Age Assistance, Temporary Assistance for Needy Families (TANF) and under any other welfare programs; Vendor payments made directly to private purveyors for medical care, burials, and other commodities and services provided under welfare programs.” Additionally, this category also “includes payments to other governments for welfare purposes, amounts for administration, support of private welfare agencies, and other public welfare services.” While the argument could be made that other spending categories, such as hospitals, include redistributive spending, the categorization strategy utilized by the Census Bureau leads many of these policies to fall into the welfare spending bin. For example, the hospitals category excludes “federal aid for medical care under public assistance programs such as Medicaid even if received by a public hospital, for care in nursing homes not associated with hospitals, or for payments to vendors for medical care in public assistance cases.” Instead, these expenditures are categorized under welfare spending. https://www.census.gov/govs/state/definitions.html.
We match state legislative chamber data in year $t$ with per capita state welfare spending (in thousands) in year $t + 2$. Figure 16 reports average per capita state welfare spending, by control and treated states, for state legislative chamber years 2003 through 2011, inclusive (and thus welfare spending data from FY 2005 through FY 2013, inclusive). Per capita state welfare spending is generally increasing throughout this time period, and is generally increasing by at least as much if not more in the treated states, relative to the control states. However, in the first fiscal year likely to have been affected by the legislators elected in the first post-	extit{Citizens United} elections, per capita welfare spending appears to increase sharply less in the treated states, relative to the control states.

![Figure 16: Per Capita State Welfare Spending](image)

1.2 1.3 1.4 1.5 1.6 1.7

**Average Per Capita State Welfare Spending (in thousands)**

- Control States
- Treated States

**Figure 16: Per Capita State Welfare Spending**

### 7.2.2 CEM Regressions

We estimate both the direct (reduced form) effect of 	extit{Citizens United} on per capita state welfare spending, as well as the indirect (two-stage) effect as mediated through both the predicted average state NPAT chamber median and the predicted number of Republican chamber majorities.

For the CEM regressions we first match control and treated states on their pre-treatment linear trends in per capita state welfare spending, using CEM and Sturge’s rule to bin observations. Our outcome of interest is the difference in a state’s per capita level of welfare spending in FY 2013, relative to FY 2012. We assume this outcome is generated by a slightly modified version of
Equation 2 where we define the primary RHS variable of interest, \( Y_s \), as either 1) \( Spending Ban_s \), thus implementing the reduced form equation; 2) the predicted change in a state’s average NPAT chamber median as instrumented by \( Spending Ban_s \); or 3) the predicted change in a state’s number of Republican chamber majorities as instrumented by \( Spending Ban_s \). \( X_s \) represents a vector of state-level pre-ruling covariates, namely the estimated pre-ruling trend in a state’s per capita welfare spending for FY 2005 - FY 2012, a state’s pre-ruling average level of per capita welfare spending for FY 2005 - FY 2012, and pre-ruling average state-level median household income, percent African American, percent with a bachelor's degree, percent aged 18-24, and percent aged 25-44. Models are estimated using OLS or 2SLS with weights derived from Coarsened Exact Matching on pre-ruling trends in per capita state welfare spending, and robust standard errors.

Figure 17 reports the coefficients for the three different versions of \( Y_s \), along with 90% confidence intervals.

\[ \text{Figure 17: CEM-Weighted Estimated Post-Citizens United Changes in Per Capita Welfare Spending} \]

16 In Figure 17 the Reduced Form coefficient reports the estimate from a CEM-weighted OLS regression of the change in per capita state welfare spending on \( Spending Ban_s \) (\( N = 49 \)). The 2SLS: Average Median coefficient reports the second stage estimate from a CEM-weighted 2SLS regression of the change in per capita state welfare spending on the change in the state chamber median average two years prior, where the latter is instrumented by \( Spending Ban_s \) (\( N = 27; 1st \) stage \( F = 5.04 \)). The 2SLS: Republican Majorities coefficient reports the second stage estimate from a CEM-weighted 2SLS regression of the change in per capita state welfare spending on the change in the number of Republican chamber majorities two years prior, where the latter is instrumented by \( Spending Ban_s \) (\( N = 48; 1st \) stage \( F = 2.82 \)).

In the reduced form model, the change in per capita state welfare spending between the 2012 and 2013 fiscal years is estimated to be $70 less in the treated states, relative to the control states. The 2SLS models suggest that this relative reduction in per capita welfare spending worked through changes in the composition of state legislatures. The Average Median coefficient indicates that, for every 1-point increase in the predicted average NPAT median, per capita welfare spending decreased by $170. As shown in Figure [15], treated states saw an estimated .43 increase in their average medians in the first post-

Citizens United elections, relative to control states; this relative increase in conservatism is then associated with an estimated relative decrease in per capita state welfare spending of $73. The Republican Majorities coefficient, although less precisely estimated, indicates that, for every 1-unit increase in the predicted number of chambers with Republican majorities, per capita welfare spending decreased by $200. As shown in Figure [15], treated states saw an estimated increase of .46 in the number of Republican chamber majorities in the first post-

Citizens United elections, relative to control states; this estimated relative increase in Republican party control is then associated with an estimated relative decrease in per capita state welfare spending of $92.

8 Discussion

Increasingly large sums of money have flowed into U.S. elections since the 1970s. As illustrated in Figure [19] in the Appendix for House elections, the amount of money spent in these campaigns is closely correlated with both the Republican share of the national popular vote and mean Republican DW-NOMINATE scores.

The Dark Money claim is that these trends are not just correlated but are in fact causally related, that money is buying the election of more and more conservative Republican legislators. Yet estimating the causal impact of money in elections is notoriously plagued by multiple problems of endogeneity, leading to a host of contradictory findings (Hall 2015).

In this paper we leveraged the Supreme Court’s ruling in Citizens United (2010) to identify the effect of campaign money in a particular context: state legislative elections in states whose bans on independent spending were struck by the Supreme Court. Using both traditional difference in differences (DD) designs as well as matching on pretreatment trends in the LHS variables, we found that the Citizens United-induced removal of state bans on independent campaign spending appears to have led to the election of more conservative Republican state legislators. These district-level
effects appear to have been of sufficient magnitude to produce more conservative state legislative medians, which in turn appear to have produced less redistributive spending per capita in states affected by the ruling. These findings do not appear in years prior to the Court’s ruling in Citizens United, and do not appear to have been generated by long-term shifts in voters’ ideological dispositions.

While our findings are limited to the specific empirical context necessitated by our research design, they may provide a causal underpinning for the more general observed correlations between campaign money, votes for Republican candidates, and the conservatism of Republican candidates. Money may indeed shift outcomes in a more conservative direction, a troubling prospect for a democracy based on the foundational principle that political influence is a property of citizenship, not of wealth.
References


Fox, Justin and Lawrence Rothenberg. 2011. “Influence Without Bribes: A Noncontracting Model


Figure 18: Bans on Independent Spending in 2010
Source: Klumpp et al (2016)
### Table 1: Covariate Balance
District-Level Legislative Preferences

<table>
<thead>
<tr>
<th></th>
<th>Control Districts</th>
<th>Treated Districts</th>
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</thead>
<tbody>
<tr>
<td><strong>Common Space CF Scores</strong></td>
<td></td>
<td></td>
</tr>
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<td><strong>Districts Electing Freshman Democrats in 2010</strong></td>
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<td>9.89**</td>
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* p<.10, ** p<.05, *** p<.01 (two-tailed tests), reported for differences between control and treated districts. Trend measures report estimated coefficients of linear trends in pre-ruling preference measures.
### Table 2: Differences in Means
Pre-Ruling Trends in District-Level Legislative Preferences

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<td>Control</td>
<td>Treated</td>
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* p<.10, ** p<.05, *** p<.01 (two-tailed tests), reported for differences between control and treated districts.

![Graph](image-url)  
**Figure 19:** National Republican House Voteshare, Mean House Incumbent Spending, and Mean House Republican DW-NOMINATE Scores. Sources: ICPSR (2013), Center for Responsive Politics (2015)