Is Campaign Spending a Cause or an Effect?

Anna Harvey*

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**Abstract**

The Supreme Court’s campaign finance jurisprudence rests on a distinction between spending restrictions (generally struck) and contribution restrictions (often upheld). In *Buckley v. Valeo* (1976), the case originating this distinction, the majority rejected an “anti-distortion” rationale for spending restrictions, claiming that campaign spending is merely an *effect* of candidate support, not a *cause* of candidate support. If this claim is true, then removing restrictions on campaign spending should have no discernible causal impacts. This article tests the *Buckley* majority’s empirical claim using its own ruling, which struck limits on campaign spending in state elections in 26 states. Estimates consistently suggest that the *Buckley*-induced removal of state limits on campaign spending led to increased Republican voteshares, increased Republican candidate entry, and decreased Democratic candidate entry in state legislative and gubernatorial elections in states affected by the ruling, and to both increased Republican House voteshares and the election of more conservative freshman Republican House incumbents in states both affected by the ruling and holding concurrent federal and state elections. These findings suggest that the rationale for the core distinction in the Supreme Court’s campaign finance jurisprudence has little empirical foundation.

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*Professor of Politics, Department of Politics, New York University, 19 West 4th Street, New York, NY 10012; Email: anna.harvey@nyu.edu.
1 Introduction

The Supreme Court’s campaign finance jurisprudence rests on a doctrinal distinction between contribution and spending limits. Contribution limits may sufficiently serve the government’s interest in preventing corruption to warrant their potential negative impacts on speech rights. Spending limits, by contrast, have not been seen as sufficiently deterrent of corruption to warrant the same treatment. Moreover, the Court’s majorities have failed to find any other governmental interest sufficiently served by spending limits to justify their potential negative impacts on speech rights (Alschuler et al 2018). In practice, while contribution limits are often upheld, every spending limit that has come before the Court has been struck.¹

In *Buckley v. Valeo* (1976), the case originating this distinction, the majority considered and rejected an “anti-distortion” governmental interest in restricting spending in elections, claiming that campaign spending is unlikely to distort electoral outcomes because it is an effect of candidate support, not a cause of candidate support. According to the *Buckley* majority, “the financial resources available to a candidate’s campaign, like the number of volunteers recruited, will normally vary with the size and intensity of the candidate’s support. There is nothing invidious, improper, or unhealthy in permitting such funds to be spent to carry the candidate’s message to the electorate” (424 U.S., at 56). In a footnote, the *Buckley* majority cited with approval an opinion dissenting in part from the appellate court’s ruling in the case, stating that, “If a senatorial candidate can raise $1 from each voter, what evil is exacerbated by allowing that candidate to use all that money for political communication? I know of none” (424 U.S., at 56, quoting 171 U.S. App. D.C., at 268, 519 F.2d, at 917 (Tamm, J.)). Ten years later, the Court repeated its empirical claim in *Federal Election Commission v. Massachusetts Citizens for Life*, (1986), striking restrictions on independent spending as applied to an anti-abortion organization, declaring that, “Political ‘free trade’ does not necessarily require that all who participate in the political marketplace do so with exactly equal resources. See NCPAC, supra (invalidating limits on independent spending by political committees); Buckley, 424 U.S., at 39-51 (striking down expenditure limits in 1971 Campaign Act). *Relative availability of funds is after all a rough barometer of public support*” (479 U.S., at 258, emphasis added).

If, in fact, the “relative availability of funds” to a campaign is “a rough barometer of public support,” as declared by the MCFL majority, then spending on behalf of a candidate, whether by the candidate’s own campaign or by independent organizations, is an effect of support for that candidate. It does not cause support for the candidate. If this claim is true, then removing spending restrictions in elections should have no causal impacts.

Recent studies, however, have identified substantial causal impacts from the Court’s ruling in Citizens United, leveraging the ruling’s incidental invalidation of restrictions on independent spending by corporations and unions in 23 states. Klumpp et al (2016), for example, found that the ruling was associated with increases in the probabilities that Republican state legislative incumbents ran for reelection, decreases in the probabilities that Democratic state legislative candidates contested races, and increases in the probabilities that Republican state legislative candidates won election of approximately 4 percentage points overall, and of 10 or more percentage points in several states. Harvey and Kaslovsky (2018) further found that the ruling led to the election of more conservative Republican state legislators, to more conservative state legislatures, and to less redistributive state spending per capita. These findings indicate that the removal of restrictions on independent spending by corporations and unions in the wake of Citizens United in fact had significant causal impacts. At least in this context, spending is not merely an effect of preexisting candidate support, but rather causes increased candidate support.

To date, however, no study has examined the Court’s empirical claim in the context of the removal of campaign spending restrictions. This article uses the Buckley ruling itself to test its majority’s assertion that the removal of restrictions on campaign expenditures will not impact electoral outcomes. In Buckley, the Court struck the limits on campaign spending in federal elections that had been enacted in the Federal Election Campaign Act Amendments of 1974. Because the 1974 amendments were challenged immediately after they became effective in January 1975, and because the Court’s ruling in Buckley was issued in January 1976, the campaign spending limits never took effect for federal elections. Yet because the ruling in Buckley was based on a sweeping First Amendment argument, the ruling also struck statutes in 26 states limiting campaign spending in state legislative and gubernatorial races. As of the 1976 state legislative and gubernatorial elections, these spending caps were no longer in effect.

The effects of Buckley’s ruling are estimated using both conventional difference in differences
(DD) designs and Coarsened Exact Matching (CEM) on pretreatment levels of and trends in Republican electoral success and Republican extremism. The results suggest that the *Buckley*-induced removal of state limits on campaign spending led to increased Republican voteshares in state legislative and gubernatorial elections, and to increased Republican candidate entry and decreased Democratic candidate entry in state legislative elections. *Buckley*’s effects on candidate entry were of the largest magnitude in districts won by the opposing party in the previous election, a finding consistent with the greater insulation of incumbents from the effects of changes in campaign spending restrictions. Results are generally robust to the exclusion of Southern states, to the restriction of the sample to only those states whose laws regulating campaign spending restrictions remained unchanged between 1950 and 1976, to the inclusion of indicators for the pretreatment presence of other state-level campaign finance statutes, and to the use of 1978 rather than 1976 as the first post-ruling election.

Further, *Buckley* had no effect on Republican voteshares in federal elections held in states with no concurrent state elections, but had effects of comparable magnitude to those observed for state elections in federal elections held concurrently with state elections. OLS and CEM analyses of changes in House DW-NOMINATE scores for those districts electing freshman incumbents in 1976 indicate that *Buckley*’s ruling also appears to have led to the election of relatively more conservative freshman Republican House incumbents in the states both affected by the ruling and holding concurrent federal and state elections, and that the *Buckley*-induced rightward moves for Republican House freshmen in the treated/concurrent states were larger than the rightward moves made by Democratic House freshmen in the same states. These findings are robust to the exclusion of Southern states, and to pre-ruling placebo tests. Overall, the findings suggest that the rationale for the core distinction in the Supreme Court’s campaign finance jurisprudence, subjecting spending restrictions to greater scrutiny than contribution restrictions, has little empirical foundation.

2 *Buckley v. Valeo* (1976)

The Federal Election Campaign Act of 1971, as amended in 1974, *inter alia* limited general and primary campaign expenditures by candidates for federal office to various specified amounts, depending upon the office sought. In *Buckley v. Valeo*, 424 U.S. 1, decided on January 30, 1976, the Supreme Court ruled that "the First Amendment requires the invalidation of the Act’s...ceilings
on over-all campaign expenditures, since those provisions place substantial and direct restrictions on the ability of candidates, citizens, and associations to engage in protected political expression, restrictions that the First Amendment cannot tolerate" (424 U.S. 3). Because the 1974 FECA amendments had not become effective until January 1975, the campaign spending limits never took effect for federal elections.

But the sweeping First Amendment ruling in *Buckley* did impact the campaign spending ceilings for state legislative and gubernatorial races on the books in 26 states in 1976, rendering these ceilings invalid for the 1976 state election cycle. Table 1 reports the states that had mandatory expenditure limits in 1976 and the date of the first election in which each statute took effect (with 1950 being the first year for which data is available). Figure 22 in the Appendix maps a reduced form of the pattern of enactment and repeal of campaign spending limits.

Table 1: States With Mandatory Expenditure Limits in 1976

<table>
<thead>
<tr>
<th>State</th>
<th>Date statute/s enacted</th>
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<tbody>
<tr>
<td>Maine</td>
<td>1972</td>
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<td>Vermont</td>
<td>1970</td>
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<tr>
<td>New Hampshire</td>
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<td>1950</td>
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<tr>
<td>Wyoming</td>
<td>1950</td>
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<tr>
<td>Utah</td>
<td>1974</td>
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<td>Oregon</td>
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<tr>
<td>Maryland</td>
<td>1950</td>
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<tr>
<td>Florida</td>
<td>1972</td>
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<tr>
<td>Alabama</td>
<td>1950</td>
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<tr>
<td>Mississippi</td>
<td>1970</td>
</tr>
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<td>Missouri</td>
<td>1950</td>
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</tbody>
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Source: Primo and Milyo (2006)

According to the *Buckley* majority, its ruling striking these state-level spending restrictions should have had little impact. There is, however, substantial circumstantial evidence suggesting that the majority may have been wrong. In the first congressional election held after the ruling, the
conservatism of congressional Republicans began what would become a steady and steep increase, an increase largely due to an influx of increasingly conservative Republican freshmen. Congressional Democrats have moved only gradually to the left over the same period (see Figures 1 and 2). These phenomena have been widely noted yet remain largely unexplained (Barber and McCarty 2015).

Despite relatively strong theory predicting electoral returns to moderation (Downs 1957), Republican House candidates began to attract more electoral support as they became more conservative in the mid-1970s. As the proportion of moderate House Republicans began a sustained decline with the cohort elected in the 1976 elections, the Republican share of the national House popular vote began a period of relatively sustained growth, reaching a 64-year high in 2010. We do not observe the same negative correlation between the proportion of moderate House Democrats and the Democratic share of the House popular vote (see Figures 3 and 4). The apparent correlation in the aggregate between increasing Republican extremism and increasing Republican electoral success, and the absence of this correlation for Democrats, also remain unexplained.
A prominent anecdotal explanation for these trends is that, in approximately the mid-1970s, increasingly wealthy donors began to more aggressively promote the election of increasingly conservative Republican representatives, largely in order to protect donor wealth (Mayer 2016). It is true that the flow of money into federal campaigns has increased dramatically since the mid-1970s (see Figure 23 in the Appendix). Given the nature of the findings about the impact of *Citizens United*, and given the timing of the Court’s ruling in *Buckley*, it is perhaps not implausible that the ruling may have contributed causally to these trends. Yet to date there is no evidence of the *Buckley* ruling’s causal impacts.

### 3 Identifying the Causal Impact of Campaign Spending Restrictions

Empirical studies of campaign finance restrictions have generally found that these restrictions tend to benefit Democratic candidates. Besley and Case (2003) found, for example, 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. Hall (2015) found that state statutes prohibiting corporate contributions between 1990-2012 were associated with higher Democratic seat shares in state legislatures. As already noted, Klumpp et al (2016) found that *Citizens United* (2010), striking bans on corporate and union independent expenditures, 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 decreases in the probability that Democratic candidates contested races in state lower house legislative elections over the same period. Harvey and Kaslovsky (2018) found that *Citizens United* (2010) led to the election of more conservative state legislators, to more conservative state legislatures, and to decreases in state-level welfare spending.

Restrictions on money in elections might disproportionately benefit liberal candidates for two reasons. First, 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). Second, money spent during campaigns can affect electoral outcomes. Both experimental and quasi-experimental evidence suggests that campaigns can have significant effects on both turnout (Green, McGrath, and Aronow 2013, Enos and Fowler 2016, Spenkuch and Toniatti 2016) and voters' preferences (Gerber et al. 2011). Greater access to campaign money by more conservative candidates might then 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.

We might then expect that the Court’s ruling in *Buckley*, repealing *inter alia* 26 state statutes restricting campaign spending in state elections, would likewise have increased the probability of Republican electoral victories, Republican candidate entry, and the conservatism of Republican incumbents, and have decreased the probability of Democratic electoral victories and Democratic candidate entry. The empirical focus here is on these reduced form questions, rather than on the flow of money that presumably mediated between the restrictions’ repeal and electoral outcomes, because of the difficulties inherent in identifying the causal impact of campaign money. Disclosure of money in state elections is uneven, and those few states with thorough disclosure in the 1970s do not have digitized records available for this period (Spencer and Wood 2014). Perhaps more importantly, even if these records were fully available, we would still be unable to make inferences from them. Money can affect electoral outcomes from the sidelines, without ever being contributed and/or spent; the mere knowledge that state campaigns could spend without limit after *Buckley* may have affected outcomes (Fox and Rothenberg 2011). Conversely, correlations between campaign donations and outcomes do not imply causal effects (Hall 2015).

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4 State Legislative and Gubernatorial Elections

Our first question of interest concerns Buckley’s impact on state legislative and gubernatorial elections.

4.1 Data

District-level Republican shares of the two-party vote in state legislative elections and state-level Republican shares of the two-party vote in gubernatorial elections (both scaled to lie between 0 and 1) are here used as measures of net Republican electoral success in state elections. Republican vote shares are presumably endogenous to the particular candidates contesting any given race (Hall and Snyder 2015). Yet candidate entry may itself have been endogenous to restrictions on campaign spending. If Republican candidates in state elections stood to benefit disproportionately from unrestricted campaign spending after Buckley, then the lifting of restrictions on such spending may have led to increased Republican and decreased Democratic candidate entry. District-level Republican and Democratic candidate entry in state legislative elections, measured as dummy variables equal to 1 if there is a Republican (Democratic) candidate contesting a race, are also analyzed independently as measures of Republican electoral success.

State legislative election data are available at the district level from Klarner et al (2013). Because of the possibility of redistricting occurring as of the 1972 election cycle, the pretreatment sample is limited to the four year period between 1972 and 1975, inclusive; the posttreatment sample is correspondingly limited to the four year period between 1976 and 1979, inclusive. Nebraska is dropped because of its nonpartisan unicameralism. Races not held in single member districts are also dropped. Gubernatorial election data are available from ICPSR Study 3371.

In the remaining panel, all states other than Alabama, Louisiana, Maryland, and Mississippi held lower chamber elections every two years during the period of interest, with only Kentucky and Virginia holding their biennial elections, and Louisiana and Mississippi holding their quadrennial elections, in odd-numbered years. All states other than Alabama, Louisiana, Maryland, Mississippi, Michigan, Kansas, South Carolina, Minnesota, New Jersey, Virginia, and Hawaii also held upper chamber elections every two years during the period of interest, with only Kentucky holding its biennial elections, and Louisiana, Mississippi, New Jersey, and Virginia holding their quadrennial elections, in odd-numbered years.

States holding gubernatorial elections every two years in even-numbered years during this time frame are Arkansas, Illinois, New Hampshire, Rhode Island, and Vermont. States holding gubernatorial elections every four years starting in 1972 are Delaware, Indiana, Missouri, North Dakota, Louisiana, North Carolina, West Virginia, Montana, Utah, and Washington; starting in 1974 are Connecticut, Maine, Massachusetts, New York, Pennsylvania, Michigan, Ohio, Wisconsin, Iowa, Kansas, Nebraska, South Dakota, Alabama, Florida, Georgia, South Carolina, Texas, Maryland, Oklahoma, Tennessee, Arizona, Colorado, Idaho, Nevada, New Mexico, Wyoming, California, Oregon, Alaska, and Hawaii; starting in 1971 are Kentucky, Louisiana, and Mississippi; and starting in 1973 are New York, Pennsylvania, Michigan, Ohio, Wisconsin, Iowa, Kansas, Nebraska, South Dakota, Alabama, Florida, Georgia, South Carolina, Texas, Maryland, Oklahoma, Tennessee, Arizona, Colorado, Idaho, Nevada, New Mexico, Wyoming, California, Oregon, Alaska, and Hawaii. States holding gubernatorial elections every two years in even-numbered years during this time frame are Arkansas, Illinois, New Hampshire, Rhode Island, and Vermont. States holding gubernatorial elections every four years starting in 1972 are Delaware, Indiana, Missouri, North Dakota, Louisiana, North Carolina, West Virginia, Montana, Utah, and Washington; starting in 1974 are Connecticut, Maine, Massachusetts, New York, Pennsylvania, Michigan, Ohio, Wisconsin, Iowa, Kansas, Nebraska, South Dakota, Alabama, Florida, Georgia, South Carolina, Texas, Maryland, Oklahoma, Tennessee, Arizona, Colorado, Idaho, Nevada, New Mexico, Wyoming, California, Oregon, Alaska, and Hawaii; starting in 1971 are Kentucky, Louisiana, and Mississippi; and starting in 1973 are New York, Pennsylvania, Michigan, Ohio, Wisconsin, Iowa, Kansas, Nebraska, South Dakota, Alabama, Florida, Georgia, South Carolina, Texas, Maryland, Oklahoma, Tennessee, Arizona, Colorado, Idaho, Nevada, New Mexico, Wyoming, California, Oregon, Alaska, and Hawaii.
Figures 5-8, and Table 2 in the Appendix, report averaged outcome data by year and treatment status for the 1972, 1974 and 1976 elections. On every measure, Republicans experienced gains in the 1976 state legislative and gubernatorial elections held in the treated states, relative to those held in the control states. In state legislative elections, illustrated in Figure 5, the average Republican voteshare remained unchanged between 1974 and 1976 in the control states. But in the treated states, the average Republican voteshare increased by almost 5 percentage points. In gubernatorial elections, illustrated in Figure 6, the average Republican voteshare also remained unchanged between 1974 and 1976 in the control states, but increased by almost 4 percentage points in the treated states. Republican candidate entry in state legislative elections, illustrated in Figure 7, decreased by 3 percentage points between 1974 and 1976 in the control states, but increased by 5 percentage points in the treated states. Democratic candidate entry in state legislative elections, illustrated in Figure 8, remained unchanged between 1974 and 1976 in the control states, but decreased by 3 percentage points in the treated states.

Figures 5-8 also suggest that states that had enacted campaign spending limits were not trending in a more pro-Republican direction prior to Buckley, relative to states that had not enacted spending limits. In fact, in the state elections held just prior to Buckley, Republicans appear to have either been losing electoral support faster in the treated states, relative to the control states (Republican legislative voteshares and Democratic legislative candidate entry), or were losing electoral support at the same rate in the treated states as in the control states (Republican gubernatorial voteshares and Republican legislative candidate entry).\(^5\)

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5 Independently of these trends, Republicans experienced higher average levels of pretreatment electoral success in states that had enacted campaign spending limits, relative to states that had not. This may be because most of the state campaign spending limits in force in the treated states prior to the ruling in Buckley were enacted prior to the 1950s, during a period in which the racial/regional dimension of conflict in American politics was both important and on which the Republican party was located to the left of the Democratic party (McCarty et al 2006). States enacting campaign spending limits during this period may have been those with greater support for the Republican party’s more liberal position on this dimension. As noted in the text, it would be inappropriate to make inferences about the effects of campaign spending limits from the voluntary enactment of state statutes during the pretreatment period.
The raw data, however, do not control for unit-specific variation in levels of or trends in pre-treatment Republican electoral strength. The analyses below attempt to address these issues.

4.2 Difference in Differences (DD) Analysis

The DD design, which uses the full span of years from 1972-1981, addresses the issue of national-level trends that might have affected Republican electoral success in state legislative and gubernatorial races in the 1970s.

The outcomes of interest are district-level Republican voteshares in state legislative elections, county-level voteshares in gubernatorial elections, Republican candidate entry (0/1) in state legislative elections, and Democratic candidate entry (0/1) in state legislative elections. These outcomes
are assumed to be generated by the following equation, which is defined by district/county \( i \) in state \( s \) during election year \( t \):

\[
\text{Voteshare/Entry}_{ist} = \beta [\text{Spending Limit}_s \times \text{Post-Buckley}_t] + \alpha_{is} + \mu_t + \varphi_{st} + \varepsilon_{ist},
\]

(1)

\( \text{Spending Limit}_s \) is a dummy variable equal to 1 if state \( s \) had a spending limit in place before the Court’s ruling in \( \text{Buckley} \); \( \text{Post-Buckley}_t \) is a dummy variable equal to 1 if the election year is 1976 or later. District/county fixed effects \( \alpha_{is} \) (which subsume state fixed effects) are included to address the fixed differences in levels of Republican electoral support across both states and districts/counties (as evident in Figures 5-8 and Table 2 in the Appendix). Election year fixed effects \( \mu_t \) are included to absorb election year-specific partisan shocks unrelated to \( \text{Buckley} \).

Figures 5-8 appear to suggest that, on average, the treated states were not trending in a more Republican-friendly direction prior to treatment, relative to control states. However, it is also the case that control and treated states do not appear to have had parallel pre-treatment trends. In order to address the issue of non-parallel pre-treatment trends, Equation 1 includes state-specific linear time trends \( \varphi_{st} \). \( \varepsilon_{ist} \) is the error term. All models are estimated using OLS with robust standard errors clustered on states.

The coefficient on the interaction term \( \text{Spending Limit}_s \times \text{Post-Buckley}_t \) estimates the average within-district/county post-treatment change in Republican electoral success in treated states, relative to that in control states. Figure 9 reports these coefficients for the four outcomes of interest, along with 95% confidence intervals. The raw differences evident in Figures 5-8 largely survive the DD analysis. After including district/county and year-specific fixed effects and state-specific time trends, both Republican state legislative and Republican gubernatorial voteshares are on average 3 percentage points higher in treated states post-treatment, relative to control states post-treatment. Republican state legislative candidates were on average 8 percentage points more likely to contest races in the treated states post-treatment, relative to the control states post treatment. Finally, Democratic state legislative candidates were on average 1 percentage point less likely to contest races in the treated states post-treatment, relative to the control states post treatment, although this estimate is not distinguishable from zero at conventional significance levels.
OLS DD estimates of the change in Republican electoral success in treated states post-treatment. Robust standard errors clustered on states. All models include district/county fixed effects, year fixed effects, and state-specific linear time trends. State legislative voteshare N = 20488; gubernatorial voteshare N = 7023; state legislative candidate entry N = 21561.

Figure 9: Republican Electoral Success in State Elections 1972-1981

Pooling districts typically won by the Republican party with those typically won by the Democratic party may be obscuring effects on candidate entry. With their institutional means of promoting their candidacies, incumbents are likely to be less sensitive to changes in campaign spending regulations (Benoit and Marsh 2008). We would expect to see Buckley’s largest effects on candidate entry in state legislative districts typically won by the opposing party.

Figure 10 subsets state legislative races by the identity of the typical winning party during the pre-treatment period, grouping state legislative districts wherein the Democratic party won at least one pretreatment election and districts wherein the Democratic party won no pretreatment elections. All models include district and year-specific fixed effects and state-specific time trends; coefficients on the interaction term are reported with 95% confidence intervals.

Figure 10 suggests that Buckley’s effect on Republican candidate entry was in fact largest in state legislative districts won at least once by the Democratic party during the pretreatment period, while its effect on Democratic candidate entry was largest in state legislative districts always won by the Republican party during the pretreatment period. Republican candidate entry is 9 percentage

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6 Other strategies for subsetting the data produce similar results.
points higher in the treated states post-treatment in state legislative districts won at least once by the Democratic party during the pretreatment period, and is 5 percentage points higher in the treated states post-treatment in state legislative districts always won by the Republican party during the pretreatment period. Democratic candidate entry is 6 percentage points lower in the treated states post-treatment in state legislative districts always won by the Republican party during the pretreatment period, and is essentially unchanged in state legislative districts won at least once by the Democratic party during the pretreatment period.

The DD estimates indicate that the raw differences in voteshares across control and treatment states observed in Figures 5-8 survive a more demanding econometric analysis largely intact, with the caveat that *Buckley*’s effects on Democratic candidate entry appear to be limited to districts won by the Republican party during the pre-ruling period.

![Graph showing candidate entry in state legislative elections](image)

**Figure 10: Candidate Entry in State Legislative Elections (Subsetted), 1972-1981**

4.3 **Coarsened Exact Matching (CEM)**

Equation 1 includes state-specific linear time trends in order to address the issue of non-parallel pre-treatment trends. However, this strategy allows for the inclusion of districts or counties 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. Pruning these anomalous districts/counties from the sample can improve estimates of causal effects (Ho et al. 2007, Iacus et al. 2011, Dimmery 2015).

Pre-processing is implemented on the cross-section of 2646 state legislative districts for which we have voteshare and candidate entry data for both the 1974 and the 1976 elections, and on the cross-section of 2681 counties for which we have gubernatorial voteshare data for the 1972 and 1976 elections, or for the 1974 and 1978 elections. 1360 of these 2646 state legislative districts, and 1190 of these 2681 counties, are in the treated states (those with spending limits struck by *Buckley*).

Table 3 in the Appendix reports on the pre-treatment similarities between the treated and control districts or counties in these two samples, reporting balance statistics for both the full cross-sections and the cross-sections resulting from pruning using Coarsened Exact Matching (CEM) on pretreatment levels of and trends in Republican electoral success. For state legislative elections, pre-processing using CEM was performed on the following district-level variables: average Republican state legislative voteshares for 1972 and 1974, change in Republican state legislative voteshares between 1972 and 1974, average Republican state legislative candidate entry for 1972 and 1974, change in Republican state legislative candidate entry between 1972 and 1974, average Democratic state legislative candidate entry for 1972 and 1974, and change in Democratic state legislative candidate entry between 1972 and 1974. For counties, pre-processing using CEM was performed on average Republican gubernatorial voteshares between 1972 and 1974, and change in Republican gubernatorial voteshares between 1970 and 1974 or 1968 and 1972, depending on a state’s election cycle.\footnote{For both cross-sections, Sturges’ rule was used to coarsen or bin these variables, but results are robust to other coarsening strategies, including Scott’s rule, the Freedman-Diaconis rule, and Shimazaki-Shinomoto’s rule. Only observations with non-missing values for all variables were included in the CEM analyses.}

For the CEM analyses the dependent variables from Equation 1 are transformed into first differences. For state legislative elections, the dependent variables are the district-level changes in Republican voteshares, Republican candidate entry, and Democratic entry between the 1974 and 1976 elections. For gubernatorial elections, the dependent variable is the county-level change in Republican voteshares between either 1976 and 1972, or 1978 and 1974. These differenced outcomes
are assumed to be generated by the following equation:

\[ \Delta \text{Voteshare}/\text{Entry}_{it} = \beta \text{Spending Limit}_{it} + \gamma X_i + \varepsilon_{is} \] (2)

\( \gamma X_i \) represents the set (or a subset) of the district- or county-level pre-treatment variables used to prune the two samples. Equation 2 is estimated using OLS with weights derived from Coarsened Exact Matching on all pre-treatment variables reported in Table 3 in the Appendix.

Figures 11 and 12 report estimates of the coefficients on Spending Limit, from Equation 2, along with 95% confidence intervals, using the samples of state legislative districts and counties pruned by CEM. Estimates of Buckley’s effect are comparable to those reported in Figure 9.

![Graph showing change in Republican voteshares in state elections, CEM Estimates](image)

CEM-pruned OLS estimates of the relative change in Republican voteshares in treated states post-treatment. 'Pretreat' models include levels of and trends in pre-treatment voteshares; 'Pretreat All' models include all pre-treatment variables as controls. Legislative district N = 2613; gubernatorial county N = 2474.

Figure 11: Change in Republican Voteshares in State Elections, CEM Estimates

Republican voteshares in state legislative districts rose by approximately 1 percentage point between 1974 and 1976 in states affected by Buckley, relative to states not affected by the ruling. County-level Republican voteshares in gubernatorial elections rose by approximately 5 percentage points between the periods of 1972-1976 and 1974-1978 in states affected by Buckley, relative to states not affected by the ruling. Republican candidate entry in state legislative elections increased by approximately 4 percentage points between 1974 and 1976 in states affected by Buckley, relative to
states not affected by the ruling. Finally, Democratic candidate entry in state legislative elections decreased by 2 percentage points between 1974 and 1976 in states affected by *Buckley*, relative to states not affected by the ruling.  

CEM-pruned OLS estimates of the relative change in state legislative candidate entry in treated states post-treatment. 'Pretreat" models include levels of and trends in pre-treatment Republican/Democratic candidate entry; "Pretreat All" models include all pre-treatment variables as controls. N = 2613.  

Figure 12: Change in Candidate Entry in State Legislative Elections, CEM Estimates  

Figure 13 reports estimates of *Buckley's* effect on legislative candidate entry, with 95% confidence intervals, using samples subsetted by the winning party in 1974, and pruned by CEM implemented separately on each subsample, using the six pretreatment variables used to prune the full sample.  

The estimates are again qualitatively similar to those reported in Figure 10. As in Figure 10, we see that there is a larger effect of *Buckley* on Republican candidate entry in districts won by the Democratic party in 1974, relative to districts won by the Republican party; in the former districts Republican candidate entry increases by 8 percentage points in 1976 in treated districts, relative to control districts; in the latter districts Republican candidate entry increases by only 1 percentage point in treated districts, relative to control districts.  

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8In the subsample of those 941 districts won by the Republican party in 1974, the unmatched $L_1$ is .20; after pruning 8 control and 13 treated districts using Sturge’s rule to bin the matching variables, the $L_1$ statistic is reduced to .12. In the subsample of those 1722 districts won by the Democratic party in 1974, the unmatched $L_1$ is .29; after pruning 11 control and 14 treated districts using Sturge’s rule to bin the matching variables, the $L_1$ statistic is reduced to .17.
CEM-pruned estimates of the change in state legislative candidate entry in treated states post-treatment, for the subsamples of districts won in 1974 by the Democratic party (N = 1697) or the Republican party (N = 920). All models include all pretreatment matching variables as controls.

Figure 13: Change in Candidate Entry in State Legislative Elections, Subsetted, CEM Estimates

Also as in Figure 10, there is a larger effect of Buckley on Democratic candidate entry in districts won by the Republican party in 1974, relative to districts won by the Democratic party; in the former districts Democratic candidate entry decreases by 5 percentage points in 1976 in treated districts, relative to control districts; in the latter districts, there is insufficient variation in Democratic candidate entry to be able to estimate the model (virtually all such districts saw Democratic candidate entry in 1976).

4.4 Robustness

We can challenge these estimates in several ways. First, states that enacted campaign spending caps were more likely to be drawn from the non-Southern states; control states were more likely to be drawn from the Southern states (defining the South as the 11 former Confederate states). With Jimmy Carter on the top of the Democratic ticket in 1976, voters in Southern states may have been less likely to vote Republican down the ticket; this might produce the findings reported above. Figure 24 in the Appendix replicates Figure 9, excluding the 11 former Confederate states. Results are generally similar to those reported in Figure 9, although Buckley’s effect on state legislative voteshares, while remaining positive, is now indistinguishable from zero.
Second, we can further address the possibility that 1976 was an anomalous election by using 1978 as the first post-ruling election. Figure 25 in the Appendix replicates Figure 9, using the changes between the 1974 and 1978 elections as the quantities of interest. Results are generally similar to those reported in Figure 9; although *Buckley*’s estimated effects on legislative and gubernatorial voteshares are smaller than those reported in Figure 9, they remain significant at conventional levels.

Third, restricting the sample to only those states whose statutes regulating campaign expenditure limits remained unchanged from the start of data availability in 1950 to the Court’s ruling in *Buckley* can partially address the possibility that states may have responded to partisan electoral trends by changing their statutes.\(^9\) Figure 26 in the Appendix replicates 9 using this reduced sample. Results remain largely unchanged.

Finally, adding indicators for the pretreatment presence of other state-level campaign finance statutes addresses one possible source of omitted variable bias. Figure 27 in the Appendix replicates 9, controlling for the presence in 1974 of state statutes mandating disclosure, restricting individual donations to candidates, and restricting corporate and union donations to candidates.\(^10\) Again, estimates are qualitatively similar to those reported without these controls.

## 5 U.S. House Elections

### 5.1 County-Level Voteshares in House Elections

*Buckley*’s strike of state statutes restricting campaign spending had no direct effect on federal elections. But it may have had indirect spillover effects, if increased spending by Republican candidates in state legislative and gubernatorial elections in the treated states increased pro-Republican turnout and/or short-term voter preferences in federal elections being held concurrently with state elections.

The possibility of spillover effects in those federal elections held concurrently with state elections offers an opportunity to further probe the possibility that the findings reported above were simply the result of differing trends in voter preferences, trends unrelated to the Supreme Court’s ruling in *Buckley v. Valeo*. If this were the case, presumably we would see similar patterns in Republican

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\(^9\) The states included in this reduced sample are those shaded blue (treated) and white (control) in Figure 22 in the Appendix.

\(^10\) No states had public funding provisions for state elections in effect prior to 1976.
electoral success across treated and control states even in those federal elections held in states with no concurrent state legislative or gubernatorial elections. Conversely, if the findings reported above were in fact the product of Buckley’s ruling, we would expect to see effects in federal elections similar to those observed for state elections only in those states holding concurrent state and federal elections, and not in those states with no concurrent state legislative or gubernatorial elections.\textsuperscript{11}

Figure 14 reports average county-level voteshares in House elections, using only the set of states not holding concurrent state legislative or gubernatorial elections in 1976.\textsuperscript{12} There is no apparent evidence of an effect of Buckley in these elections, analogous to that observed in state legislative and gubernatorial elections. If anything, Republican voteshares decrease more in this set of treated states after Buckley, relative to this set of control states; the average Republican voteshare increased by 12 percentage points between 1974 and 1976 in the control states, but increased by only 2 percentage points in the treated states.\textsuperscript{13}

\vspace{10pt}

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figure14.png}
\caption{Average County-Level Republican House Voteshare States With No Concurrent 1976 State Elections}
\end{figure}

Figure 15 reports average county-level voteshares in House elections using only the set of states holding concurrent state legislative and gubernatorial elections in 1976, and Figure 16 reports av-

\textsuperscript{11}The concurrence of state electoral calendars with federal electoral calendars is almost perfectly uncorrelated with treatment status ($r = .01$ for concurrent federal/legislative elections and $r = .02$ for concurrent federal/legislative/gubernatorial elections).

\textsuperscript{12}These states are New Jersey, Maryland, Kentucky, Virginia, and Alabama. The analogous sample for Senate elections is not reported due to small sample size.

\textsuperscript{13}Although this difference may suggest a difference between treated and control states not due to Buckley, this difference disappears when we remove Southern states from the sample, reported in Figure 28 in the Appendix.
verage county-level House voteshares using the full sample of states, defining a treated state as one having both a state campaign spending limit struck by Buckley and concurrent state legislative and gubernatorial elections in 1976.\textsuperscript{14} In neither figure is there evidence of a pre-ruling pro-Republican trend in voteshares in the treated states, relative to the control states. However, we see apparent evidence of a small effect of Buckley analogous to that observed in state legislative and gubernatorial elections. In the sample of states holding concurrent legislative and gubernatorial elections, the average Republican voteshare increased by 4 percentage points between 1974 and 1976 in the control states, but increased by 5.5 percentage points in the treated states. In the full sample of states, the average Republican voteshare increased by 3 percentage points between 1974 and 1976 in the control states, but again increased by 5.5 percentage points in the treated states.

Figure 15 reports CEM-pruned estimates of the coefficients on Spending Limit\textsubscript{s} from Equation 2, along with 95\% confidence intervals, using the sample of counties for which we have voteshare data for the 1972, 1974 and 1976 House elections. The dependent variable is the change in county-level Republican House voteshares between 1974 and 1976. The coefficient in black is estimated using only the sample of states not holding concurrent state legislative or gubernatorial elections in 1976. This sample of 365 counties is preprocessed using CEM on the average levels of and changes in county-level Republican House voteshares in 1972 and 1974.\textsuperscript{15} There is no evidence of


\textsuperscript{15}The unmatched $L_1$ is .42; after pruning 33 control and 9 treated counties using Sturge’s rule to bin the matching variables, the $L_1$ statistic is reduced to .24.
increased Republican voteshares in the treated states post-treatment, relative to the control states post-treatment. Instead, as was seen in Figure 14, Republican voteshares actually decrease more in the treated states post-treatment, relative to the control states post-treatment.

![Figure 17: Change in County-Level Republican House Voteshares CEM Estimates](image)

CEM-pruned OLS estimates of the relative changes in county-level House Republican voteshares in treated states post-treatment. All models include all pretreatment matching variables as controls.

The coefficient in red is estimated using only the sample of states holding concurrent state legislative and gubernatorial elections in 1976. This sample of 811 counties was also preprocessed using CEM on the average levels of and changes in county-level Republican House voteshares in 1972 and 1974. Here we see increased Republican voteshares in the treated states post-treatment, relative to the control states post-treatment; first differences in county-level Republican House voteshares between 1974 and 1976 are 5 percentage points larger in the treated states, relative to the control states.

Finally, the coefficient in green is estimated using the full sample of counties, where treatment is defined as a county’s location in a state that a) had a limit on campaign spending struck by the Court’s ruling in *Buckley* and b) held state legislative and gubernatorial elections in 1976. This sample of 2934 counties was again preprocessed using CEM on the average levels of and changes

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16The unmatched $L_1$ is .51; after pruning 151 control and 11 treated counties using Sturges’s rule to bin the matching variables, the $L_1$ statistic is reduced to .28.
in county-level Republican House voteshares in 1972 and 1974. Here again we see increased Republican voteshares post-treatment, relative to all other states post-treatment. First differences in county-level Republican House voteshares between 1974 and 1976 are 4 percentage points higher in the treated states holding concurrent elections in 1976, relative to all other states.

These voteshare estimates for concurrently held congressional elections are comparable in magnitude to the voteshare estimates for gubernatorial elections, indicating large spillover effects of *Buckley* in concurrent congressional elections. This is consistent with a causal mechanism such as differential partisan turnout, with more conservative/Republican voters being mobilized to vote in all concurrently held post-*Buckley* elections in the treated states (Spenkuch and Toniatti 2016).

Figure 28 in the Appendix replicates Figure 17 using only the sample of nonsouthern states; results are nearly identical with the exception that, for the nonsouthern states, there is no difference in House Republican voteshares between the control and treated states for those states with no concurrent state and federal elections.

### 5.2 House DW-NOMINATE Scores

The voteshare evidence is consistent with the hypothesis that *Buckley*’s repeal of state-level campaign spending limits benefitted not only Republican candidates running in state legislative and gubernatorial elections in the repeal states, but also Republican candidates running in those federal elections being held concurrently with state elections in the repeal states. In the repeal states with concurrent federal elections, relative increases in Republican spending in state elections may have generated relative increases in pro-Republican turnout and/or short-term voter preferences, increases that may have benefitted all Republican candidates on the ballot in those states.

The remaining hypothesis of interest is that the increased ability to spend in the repeal states may have generated more conservative policy positions among post-treatment Republican state candidates vying for the support of newly relevant conservative donors. We lack the estimates of state legislators’ preferences for this period that would allow us to estimate *Buckley*’s effect on same. However, given the apparent spillover effects in concurrently held federal elections, we can

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17 The unmatched $L_1$ is .51; after pruning 1032 control counties using Sturge’s rule to bin the matching variables, the $L_1$ statistic is reduced to .28.

18 Democratic candidates in the repeal states may have also moved to the right, relative to Democratic candidates in the control states, but should have moved less to the right than Republican candidates in these states (Feddersen and Gul 2014).
estimate this effect on Republican House candidates. If Republican state legislative and gubernatorial candidates moved to the right in the treated states post-treatment, and were using donor funds to generate voter turnout and/or short-term preferences for their more conservative candidacies, these more conservative electorates may have induced Republican House candidates to also move to the right in those treated states with post-treatment concurrent federal elections.

Because we obtain little information from congressional districts wherein an incumbent retained his or her seat in the 1976 elections, we restrict our attention to the 73 districts sending freshman incumbents to the 95th Congresses.\footnote{\textsuperscript{19}} While this sample is too small to permit isolation of states with either no concurrent state elections, or both concurrent gubernatorial and legislative elections, we can look at changes in the DW-NOMINATE scores of districts that elected freshman incumbents in the 1976 elections and that were located in either a) states that had a limit on campaign spending struck by the Court’s ruling in \textit{Buckley} and held state legislative and gubernatorial elections in 1976 (Treated/Concurrent States) or b) or all other states (Control States).

Figure 18 reports average DW-NOMINATE scores for the 73 districts that elected freshman House incumbents in the 1976 elections. Districts in the treated/concurrent states that elected freshmen in the 1976 House elections were not trending in a more conservative direction prior to \textit{Buckley}, relative to the analogous districts in the control states; districts in the treated/concurrent states saw their incumbents become on average .07 DW-NOMINATE points more liberal between the 93d and 94th Congresses, while districts in the control states saw their incumbents become on average only .02 DW-NOMINATE points more liberal over the same period. But those districts electing freshmen in the 1976 House elections in the treated/concurrent states saw their incumbents become on average .25 DW-NOMINATE points more conservative between the 94th and 95th Congresses, while the analogous districts in the control states saw their incumbents become on average only .03 DW-NOMINATE points more conservative over the same period.

\footnote{\textsuperscript{19}DW-NOMINATE scores assume a fixed ideal point that can vary only as a linear function of time over the course of a member’s entire congressional career.}
Although the samples are small, Figures 19 and 20 separate these districts by the party of the freshman House incumbents elected in the 1976 elections. Both Democratic and Republican freshmen elected in the 1976 elections in the treated/concurrent states moved their districts on average in a more conservative direction, relative to freshmen in the control states. However, Republican freshmen in the treated/concurrent states moved their districts more to the right, relative to Republican freshmen in the control states, than did Democratic freshmen in the treated/concurrent states, relative to Democratic freshmen in the control states. Those districts in the control states electing Democratic freshmen in the 1976 House elections saw their incumbents become on average .06 DW-NOMINATE points more liberal between the 94th and 95th Congresses, while the analogous districts in the treated/concurrent states saw their incumbents become on average .09 DW-NOMINATE points more conservative, for a treatment effect of .15. But those districts in the control states electing Republican freshmen in the 1976 House elections saw their incumbents become on average .25 DW-NOMINATE points more conservative between the 94th and 95th Congresses, while the analogous districts in the treated/concurrent states saw their incumbents become on average .5 DW-NOMINATE points more conservative, for a treatment effect of .25. In neither figure is there much evidence of a pre-ruling conservative trend in the treated/concurrent states, relative to the control states.
Figure 19: Districts Electing Freshman Democrats in 1976 House Elections

Figure 20: Districts Electing Freshman Republicans in 1976 House Elections

Figure 21 reports OLS and CEM-pruned OLS estimates of the coefficients on Spending Limits from Equation 2, along with 90% confidence intervals, where Spending Limits is equal to 1 for those districts located in states that both had a limit on campaign spending struck by the Court’s ruling in Buckley and held state legislative and gubernatorial elections in 1976, using the sample of House districts that both sent freshman incumbents to the 95th House, and for which we have House DW-NOMINATE scores from the 94th and 95th Congresses. The coefficients plotted in black report estimates for all districts electing freshman incumbents in the 1976 elections; the coefficients plotted in blue report estimates for districts electing Democratic freshman incumbents in the 1976 elections; the coefficients plotted in red report estimates for districts electing Republican freshman incumbents in the 1976 elections. All CEM estimates were preprocessed using Sturje’s rule on the average levels of and changes in districts’ DW-NOMINATE scores in the 93d and 94th Congresses.20

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20 The unmatched $L_1$ for the full sample of 73 districts is .48; after pruning 25 control districts the $L_1$ statistic is reduced to .1. The unmatched $L_1$ for the sample of 51 districts electing Democratic freshmen in 1976 is .57; after pruning 25 control districts the $L_1$ statistic is reduced to 0. The unmatched $L_1$ for the sample of 22 districts electing Republican freshmen in 1976 is .5; after pruning 10 control districts the $L_1$ statistic is reduced to .17.
OLS and CEM-pruned OLS estimates of the relative changes in district-level House DW-NOMINATE scores between the 94th and 95th Congresses, for all districts electing freshmen incumbents in the 1976 elections, for districts electing Democratic freshmen in the 1976 elections, and for districts electing Republican freshmen in the 1976 elections. All models include all pre-treatment matching variables as controls.

Figure 21: Estimated Changes in DW-NOMINATE Scores Between 94th and 95th Congresses
Districts Electing Freshman Incumbents in 1976 House Elections

Both the OLS and the CEM-pruned OLS estimates tell the same story. In the treated states holding concurrent state legislative and gubernatorial elections in 1976, freshman incumbents elected in that year’s House elections moved their districts further to the right than did their counterpart freshman incumbents elected in the control states, with an estimated relative rightward move of approximately 9% of the DW-NOMINATE space. Republican freshman incumbents in the treated/concurrent states moved their districts even further to the right, relative to their Republican freshman counterparts in the control states, with a estimated relative rightward move of approximately 11% of the DW-NOMINATE space, than did their Democratic freshman colleagues in the treated/concurrent states, relative to their Democratic freshman counterparts in the control states, with an estimated relative rightward move of approximately 4% of the DW-NOMINATE space.

5.3 Robustness

We can challenge the results reported in Figure 21 in two ways. First, we can exclude Southern states, on the theory noted earlier that the presence of Jimmy Carter at the top of the ticket may be
a confounder. Figure 29 in the Appendix replicates Figure 21 after excluding the Southern states; results are nearly identical to those reported for the full sample.

Second, we can conduct a pre-ruling placebo test, assuming a placebo Buckley v. Valeo ruling took place between the 1972 and 1974 congressional elections. If we see estimates similar to those reported in Figure 21, we can assume that the latter estimates were not in fact the result of the Court’s ruling in January 1976, but instead were simply the continuation of pre-ruling trends. Figure 30 in the Appendix replicates Figure 21 using the sample of House districts that both sent freshman incumbents to the 94th House, and for which we have House DW-NOMINATE scores from the 93d and 94th Congresses. Treatment is defined as those states with both state-level campaign spending restrictions in 1974 and legislative or gubernatorial elections held concurrently with federal elections in 1974. The coefficients plotted in black report estimates for all districts electing freshman incumbents in the 1974 elections; the coefficients plotted in blue report estimates for districts electing Democratic freshman incumbents in the 1974 elections; the coefficients plotted in red report estimates for districts electing Republican freshman incumbents in the 1974 elections. All CEM estimates were preprocessed using Sturge’s rule on the average levels of and changes in districts’ DW-NOMINATE scores in the 92d and 93d Congresses. There is no evidence of a pre-ruling conservative shift in DW-NOMINATE scores in the treated states, relative to the control states.

6 Discussion

In the Supreme Court’s campaign finance jurisprudence, contribution limits may sufficiently serve the government’s interest in preventing corruption to warrant their potential negative impacts on speech rights. Spending limits, by contrast, have not been seen as sufficiently deterrent of corruption to warrant the same treatment. Moreover, the Court’s majorities have failed to find any other governmental interest sufficiently served by spending limits to justify their potential negative impacts on speech rights. In Buckley v. Valeo (1976), the majority considered and rejected an “anti-distortion” governmental interest, claiming that campaign spending is unlikely to distort electoral outcomes because it is an effect of candidate support, not a cause. As the Court stated this empirical claim in MCFL (1986), the “relative availability of funds is after all a rough barometer of public support” (479 U.S., at 258).
If, in fact, the “relative availability of funds” to a campaign is “a rough barometer of public support,” as declared by the MCFL majority, then removing spending restrictions in elections should have no causal impacts. Of specific interest here, the Supreme Court’s ruling in *Buckley*, striking campaign spending restrictions in 26 states, should have had no causal impacts.

Yet the findings reported here indicate that the *Buckley* ruling in fact had significant impacts in state legislative, gubernatorial, and congressional elections. The *Buckley*-induced removal of state limits on campaign spending led to increased Republican voteshares in state legislative and gubernatorial elections, to increased Republican candidate entry and decreased Democratic candidate entry in state legislative elections, and to the election of relatively more conservative freshman Republican House incumbents in the states both affected by the ruling and holding concurrent federal and state elections. Overall, the findings suggest that the rationale for the core distinction in the Supreme Court’s campaign finance jurisprudence, subjecting spending restrictions to greater scrutiny than contribution restrictions, has little empirical foundation.
References


Hall, Andrew B. and James M. Snyder Jr. 2015. “How Much of the Incumbency Advantage is Due to Scare-Off?” *Political Science Research and Methods* 3(3):493–514.


Blue = states that enacted spending limits prior to 1950 and did not change them prior to 1976, green = states that enacted spending limits between 1950 and 1976, gray = states that had spending limits at some point between 1950 and 1976 but repealed them before *Buckley*, white = states that had no spending limits between 1950 and 1976.

Figure 22: States Enacting Mandatory Expenditure Limits
Source: Primo and Milyo (2006)

Figure 23: National Republican House_Voteshare, Mean House Incumbent Spending, and Mean House Republican DW-NOMINATE Scores
Sources: ICPSR (2018), Center for Responsive Politics (2015)
Table 2: Descriptive Statistics

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<th>N</th>
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<td>.97</td>
<td>1947</td>
<td>.93</td>
<td>2166</td>
<td>.04***</td>
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</table>

State legislative/gubernatorial outcomes are averaged across all state legislative districts/counties reporting non-missing data in specified years. * p<.10, ** p<.05, *** p<.01.
## Table 3: CEM Balance Statistics

<table>
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<th><em>State Legislative Elections</em></th>
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<th>Matched Sample</th>
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<td>Avg R VS, 1972-1974 ( L_1 )</td>
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<td>.05</td>
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<td>.08</td>
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<td>.00</td>
</tr>
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<td>Avg D Cand Entry, 1972-1974 ( L_1 )</td>
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<td>.00</td>
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<td>.00</td>
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</tr>
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<td>Control=1262, Treated=1351</td>
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<td>.26</td>
</tr>
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<td>.09</td>
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</table>

CEM performed using Sturge’s rule on all variables. The overall \( L_1 \) statistics measure the distance or lack of overlap between the multidimensional distributions of these variables across the treatment and control groups, within each cross-section (Iacus et al 2011). For example, before pruning, the overall \( L_1 \) for the sample of state legislative districts is .30, indicating that only 70% of the two multidimensional distributions overlap. After pruning, the overall \( L_1 \) is reduced to .19, indicating that 81% of the distributions overlap, with a loss of 9 treated and 24 control districts.
OLS DD estimates of the change in Republican electoral success in non-Southern treated states post-treatment. All models include district/county fixed effects, year fixed effects, and state-specific linear time trends. 90% and 50% confidence intervals reported.

Figure 24: Republican Electoral Success in Non-Southern State Elections 1972-1981

CEM-pruned OLS estimates of the relative change in Republican voteshares in treated states post-treatment, using 1978 as first post-treatment election. "Pretreat" models include levels of and trends in pre-treatment voteshares; "Pretreat All" models include all pre-treatment variables as controls.

Figure 25: Change in Republican Voteshares in 1978 and 1974 State Elections, CEM Estimates

Figure 26: Change in Republican Electoral Success in State Elections, CEM Estimates, States With Campaign Spending Statutes Unchanged Since 1950

CEM-pruned OLS estimates of the change in Republican electoral success in state elections in treated states post-treatment, controlling for the presence in 1974 of statutes requiring disclosure, restricting individual contributions to candidates, and restricting corporate and union contributions to candidates. All results are from OLS regressions using weights derived from Coarsened Exact Matching on all pre-treatment variables reported in Table 3. Legislative district N = 2613; gubernatorial county N = 2474.

Figure 27: Change in Republican Electoral Success in State Elections, CEM Estimates, Controlling for Pretreatment Presence of Other Campaign Finance Statutes

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CEM-pruned OLS estimates of the relative changes in county-level House Republican voteshares in treated states post-treatment. All models include all pretreatment matching variables as controls. Southern states are excluded.

Figure 28: Change in Nonsouthern County-Level Republican House Voteshares
CEM Estimates

OLS and CEM-pruned OLS estimates of the relative changes in district-level House DW-NOMINATE scores between the 94th and 95th Congresses, for all nonsouthern districts electing freshmen incumbents in the 1976 elections, for nonsouthern districts electing Democratic freshmen in the 1976 elections, and for nonsouthern districts electing Republican freshmen in the 1976 elections. All models include all pre-treatment matching variables as controls.

Figure 29: Estimated Changes in DW-NOMINATE Scores Between 94th and 95th Congresses
Nonsouthern Districts Electing Freshman Incumbents in 1976 House Elections
OLS and CEM-pruned OLS estimates of the relative changes in district-level House DW-NOMINATE scores between the 93d and 94th Congresses, for all districts electing freshmen incumbents in the 1974 elections, for districts electing Democratic freshmen in the 1974 elections, and for districts electing Republican freshmen in the 1974 elections. All models include all pre-treatment matching variables as controls.

Figure 30: Estimated Changes in DW-NOMINATE Scores Between 93d and 94th Congresses
Districts Electing Freshman Incumbents in 1974 House Elections