Is Campaign Spending a Cause or an Effect? Reexamining the Empirical Foundations of *Buckley v. Valeo* (1976)

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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 vote shares, increased Republican candidate entry, and decreased Democratic candidate entry in state legislative and gubernatorial elections in states affected by the ruling, as well as increased Republican House vote shares and the election of more conservative 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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1. INTRODUCTION

The Supreme Court's campaign finance jurisprudence rests on a doctrinal distinction between contribution and spending limits. According to this distinction, contribution limits can sufficiently serve the government's interest in preventing corruption so as to offset their potential negative impacts on speech rights. Spending limits, by contrast, do not serve the government's interest in preventing corruption. Moreover, the Court's majorities have failed to find any other governmental interest sufficiently served by spending limits so as to justify their potential negative impacts on speech rights (Alschuler et al. 2018). In practice, although contribution limits are often upheld, every spending limit that has come before the Court has been struck.[1]

In *Buckley v. Valeo*, 424 U.S. 1 (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*, 479 U.S. 238 (1986) (*MCFL*), striking restrictions on independent spending as applied to an antiabortion organization and declaring, "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

[1] The Court struck down spending limits in *Buckley v. Valeo*, 424 U.S. 1 (1976); *First National Bank of Boston v. Bellotti*, 435 U.S. 765 (1978); *FEC v. National Conservative Political Action Committee*, 470 U.S. 480 (1985); *Federal Election Commission v. Massachusetts Citizens for Life*, 479 U.S. 238 (1986); *Colorado Republican Federal Campaign Committee v. FEC (Colorado 1)*, 518 U.S. 604 (1996); and *Citizens United v. Federal Election Commission*, 558 U.S. 310 (2010).

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 v. Federal Election Commission*, 558 U.S. 310 (2010), striking restrictions on independent spending by corporations and unions in 23 states. Klumpp, Mialon, and Williams (2016), for example, found that the ruling increased the probability that Republican state legislative incumbents ran for reelection, decreased the probability that Democratic state legislative candidates contested races, and increased the probability that Republican state legislative candidates won election. Harvey and Mattia (forthcoming) further found that the ruling led to the election of more conservative Republican state legislators in states affected by the ruling relative to states unaffected by the ruling. 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 (as opposed to independent 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 (FECA) 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 vote shares 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 postruling election.

Further, *Buckley* had no effect on Republican vote shares in House elections held in states with no concurrent state elections but had effects of comparable magnitude to those observed for state elections in House elections held concurrently with state elections. DD and CEM analyses of House Dynamic Weighted Nominal Three-Step Estimation (DW-NOMINATE) scores indicate that *Buckley*'s ruling also appears to have led to the election of relatively more conservative House incumbents in the states both affected by the ruling and holding concurrent federal and state elections. These findings are robust to the exclusion of southern states and to preruling 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)

FECA, as amended in 1974, inter alia limited general and primary campaign expenditures by candidates for federal office to various specified amounts, depending on 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 did 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

Table 1. States with Mandatory Expenditure Limits in 1976

State	Date Statute(s) Enacted
Alabama	1950
Florida	1972
Indiana	1950
Iowa	1950
Kansas	1950
Maine	1972
Maryland	1950
Massachusetts	1950
Michigan	1950
Minnesota	1950
Mississippi	1970
Missouri	1950
Montana	1950
New Hampshire	1950
New Jersey	1950
New York	1950
North Dakota	1950
Ohio	1950
Oregon	1950
South Dakota	1950
Utah	1974
Vermont	1970
Wisconsin	1950
Wyoming	1950

Source: Primo and Milyo (2006).

election in which each statute took effect (with 1950 being the first year for which data are available). Figure A1 maps a reduced form of the pattern of enactment and repeal of campaign spending limits.

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 caused by an influx of increasingly conservative Republican freshmen. Congressional Democrats have moved only gradually to the left over the same period (see figs. 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

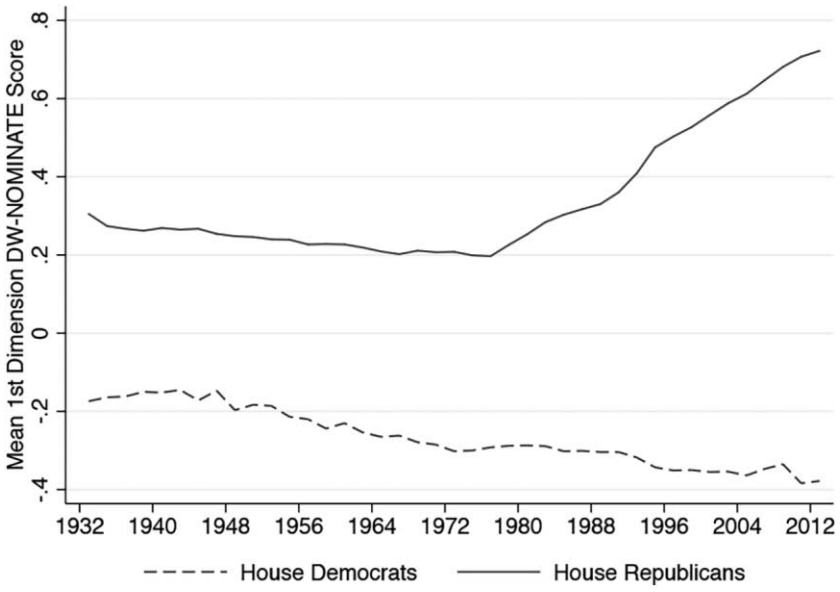

Figure 1. DW-NOMINATE party means (McCarty, Poole, and Rosenthal 1997). Color version available as an online enhancement.

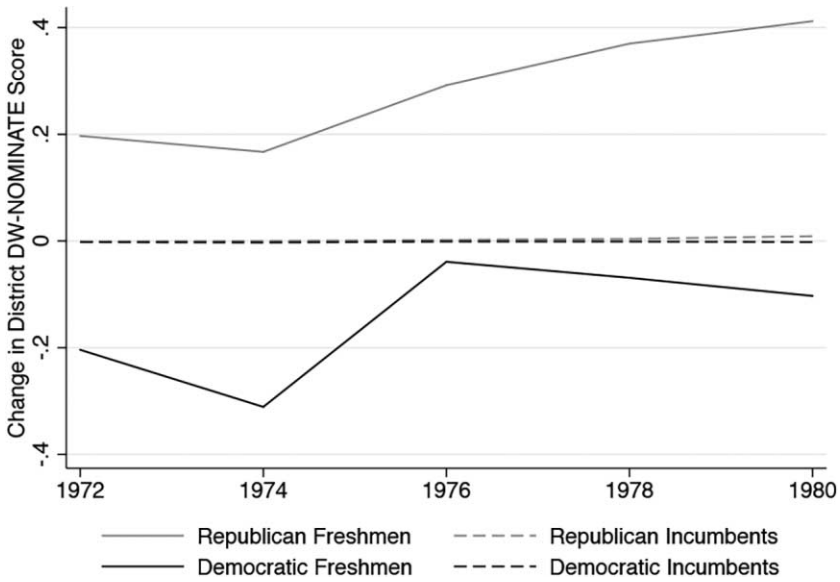

Figure 2. Interelection change in district-level House DW-NOMINATE scores. Color version available as an online enhancement.

the proportion of moderate House Democrats and the Democratic share of the House popular vote (see figs. 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 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 fig. A2). 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 latter 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

Numerous empirical studies have sought to estimate the impacts of campaign spending on outcomes (for reviews of this literature, see

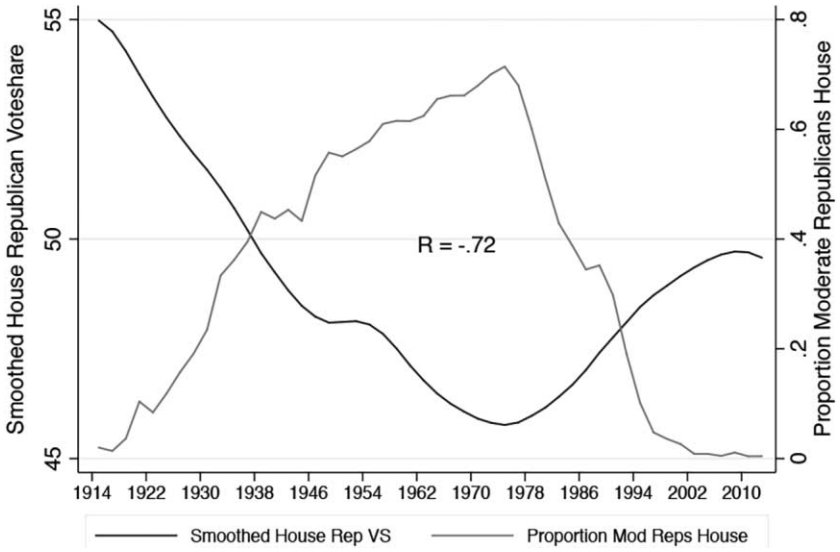

Figure 3. Republican (R) House vote shares (VS) and moderate (mod) Republicans (ICPSR 2013; McCarty, Poole, and Rosenthal 1997). Color version available as an online enhancement.

Ansolabehere, de Figueiredo, and Snyder 2003; Stratmann 2005, 2018). However, these studies have struggled with questions of causal identification (Stratmann 2018). Amassed campaign contributions can affect outcomes from the sidelines without ever being spent; the mere knowledge that a candidate has money to spend may affect an opposing candidate's entry and spending choices, confounding estimates of spending on outcomes (Fox and Rothenberg 2011). Conversely, candidates may be more likely to spend when they are falling behind in voter support, again confounding estimates of the possibly positive impacts of spending on vote shares (Stratmann 2018).

Some studies have used changes in state-level campaign finance statutes to identify effects of contributions and spending on outcomes. These studies have generally found that statutory limits on campaign finance 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 (2016) found that state statutes prohibiting corporate contributions between 1990 and 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

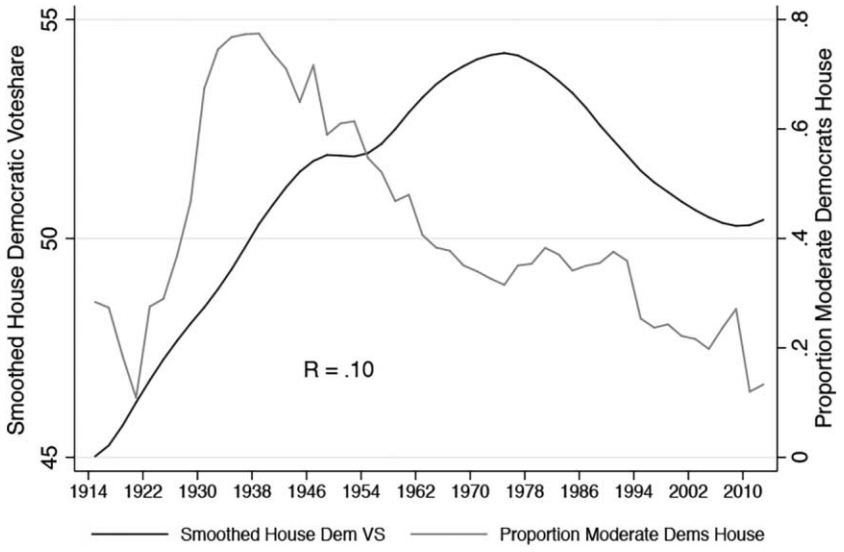

Figure 4. Democratic (D) House vote shares (VS) and moderate (mod) Democrats (ICPSR 2013); McCarty et al. 1997). Color version available as an online enhancement.

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. Harvey and Mattia (forthcoming) found that *Citizens United* (2010) led to the election of more conservative Republican state legislators in states affected by the ruling relative to states not affected by the ruling.

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 voter turnout (Enos and Fowler 2018; Green, McGrath, and Aronow 2013; Spenkuch and Toniatti 2018) and voter 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.[2] 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. As already noted, money can affect electoral outcomes from the sidelines without ever being 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 expenditures and outcomes do not imply causal effects (Stratmann 2018).

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 county-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

[2] For a similar approach applied to *Citizens United v. FEC* (2010), see Klumpp, Mialon, and Williams (2016) and Harvey and Mattia (forthcoming).

entries 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 4-year period between 1972 and 1975, inclusive; the posttreatment sample is correspondingly limited to the 6-year period between 1976 and 1981, inclusive. Nebraska is dropped because of its nonpartisan unicameralism. Races not held in single member districts are also dropped.[3] Gubernatorial election data are available from ICPSR Study 3371.[4]

Figures 5–8 and table A1 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 vote share remained unchanged between 1974 and 1976 in the control states. But in the treated states, the average Republican vote share increased by almost 5 percentage points. In gubernatorial elections, illustrated in figure 6, the average Republican vote share 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

[3] In the remaining panel, all states other than Alabama, Louisiana, Maryland, and Mississippi held lower chamber elections every 2 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 2 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.

[4] States holding gubernatorial elections every 2 years in even-numbered years during this time frame are Arkansas, Illinois, New Hampshire, Rhode Island, and Vermont. States holding gubernatorial elections every 4 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 Jersey and Virginia.

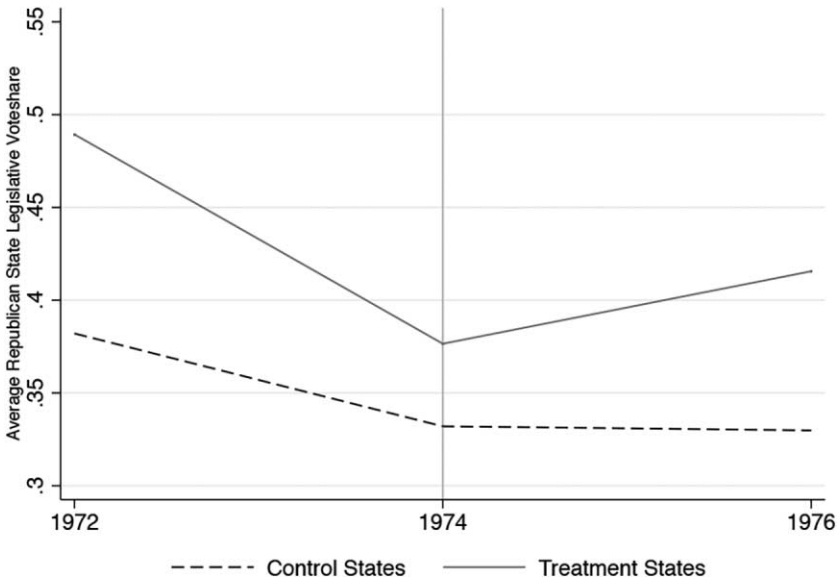

Figure 5. Average Republican state legislative vote shares. Color version available as an online enhancement.

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 either to have been losing electoral support faster in the treated states relative to the control states (Republican legislative vote shares and Democratic legislative candidate entry) or to have been losing electoral support at the same rate in the treated states as in the control states (Republican gubernatorial vote shares and Republican legislative candidate entry).[5]

[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, Poole, and Rosenthal 2006). States

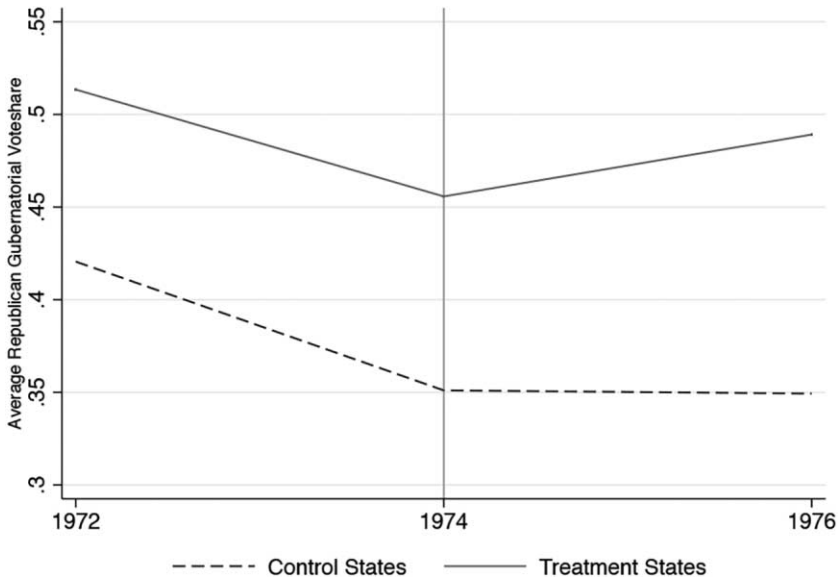

Figure 6. Average Republican gubernatorial vote shares. Color version available as an online enhancement.

The raw data, however, do not control for unit-specific variation in levels of or trends in pretreatment Republican electoral strength. The analyses in the next section attempt to address these issues.

4.2. Difference in Differences Analysis

The DD design, which uses the full span of years from 1972 to 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 vote shares in state legislative elections, county-level vote shares 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 defined by district i in state s during year t and are assumed to be generated by the following equation:

$$\text{Vote share/Entry}_{ist} = \beta[\text{Spending Limit}_s \times \text{Post-}Buckley_t] + \alpha_{is} + \mu_t + \varphi_s t + \varepsilon_{ist} \quad (1)$$

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.

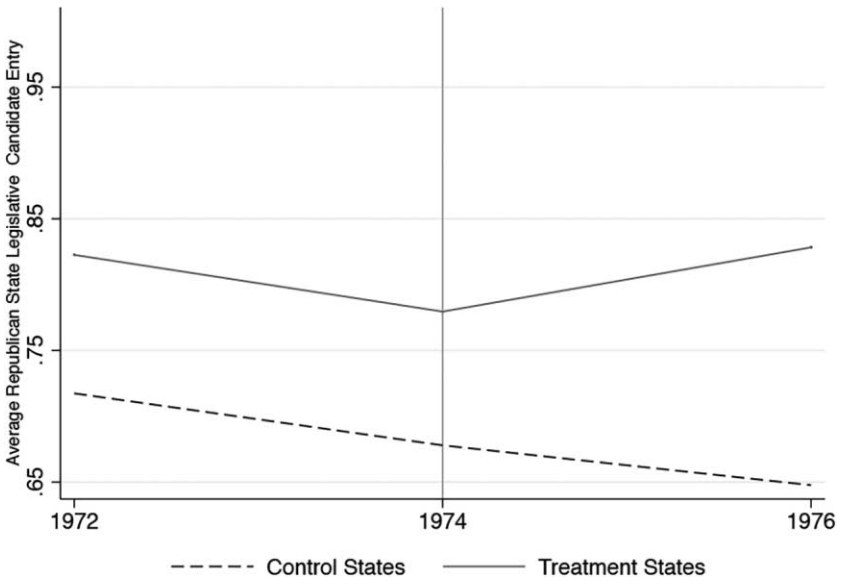

Figure 7. Average Republican state legislative candidate entry. Color version available as an online enhancement.

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 *Buckley*; Post-*Buckley*$_t$ is a dummy variable equal to 1 if the election year is 1976 or later. District/county fixed effects α_{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 figs. 5–8 and table A1). Election-year fixed effects μ_t are included to absorb partisan shocks specific to the election year unrelated to *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 pretreatment trends. To address the issue of nonparallel pretreatment trends, equation (1) includes state-specific linear time trends $\varphi_s t$. The error term is ε_{ist}. All models are estimated using ordinary least squares (OLS) with robust standard errors clustered on states.

The coefficient on the interaction term [Spending Limit$_s$ × Post-*Buckley*$_t$] estimates the average within-district/county posttreatment change in Republican electoral success in treated states relative to that in control states. Figure 9 reports these coefficients for

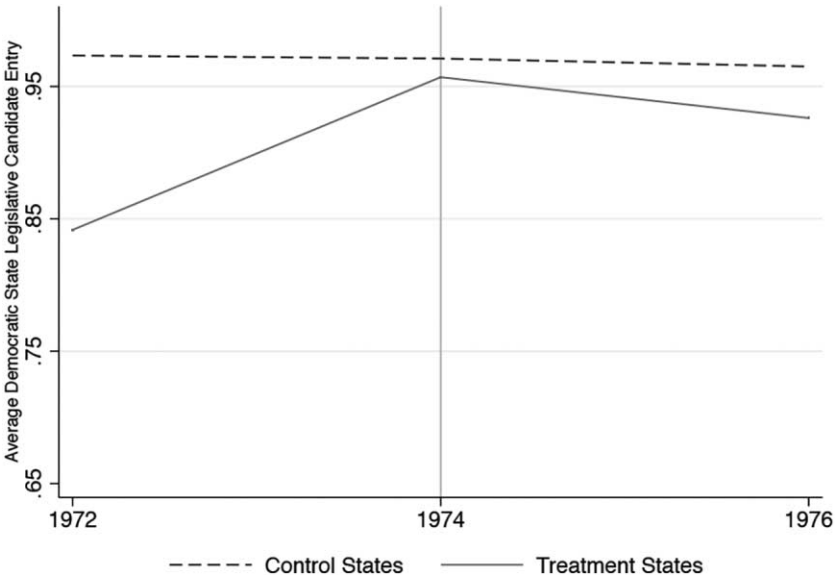

Figure 8. Average Democratic state legislative candidate entry. Color version available as an online enhancement.

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 vote shares are on average 3 percentage points higher in treated states posttreatment relative to control states posttreatment. Republican state legislative candidates were on average 8 percentage points more likely to contest races in the treated states posttreatment 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 posttreatment relative to the control states posttreatment, although this estimate is not distinguishable from 0 at conventional significance levels.

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

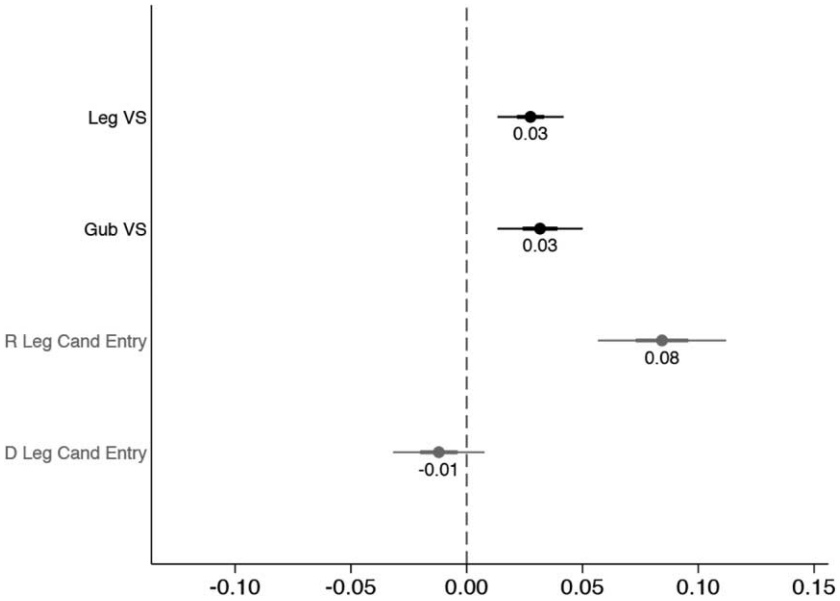

Figure 9. Republican electoral success in state elections, 1972–81. Ordinary least squares difference in differences estimates of the change in Republican electoral success in treated states posttreatment. Robust standard errors clustered on states. All models include district/county fixed effects, year fixed effects, and state-specific linear time trends. State legislative vote share, *n* = 20,488; gubernatorial vote share, *n* = 7,023; state legislative candidate entry, *n* = 21,561. Cand = candidate; D = Democratic; Gub = gubernatorial; Leg = legislative; R = Republican; VS = vote shares. Color version available as an online enhancement.

during the pretreatment 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.[6] 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 somewhat larger in state legislative districts won at least once by the Democratic Party during the pretreatment period, although its effect on Democratic candidate entry was clearly larger in state legislative districts always won by the Republican Party during the pretreatment period. Republican candidate entry is 9 percentage points higher in the treated states posttreatment in state legislative

[6] Other strategies for subsetting the data produce similar results.

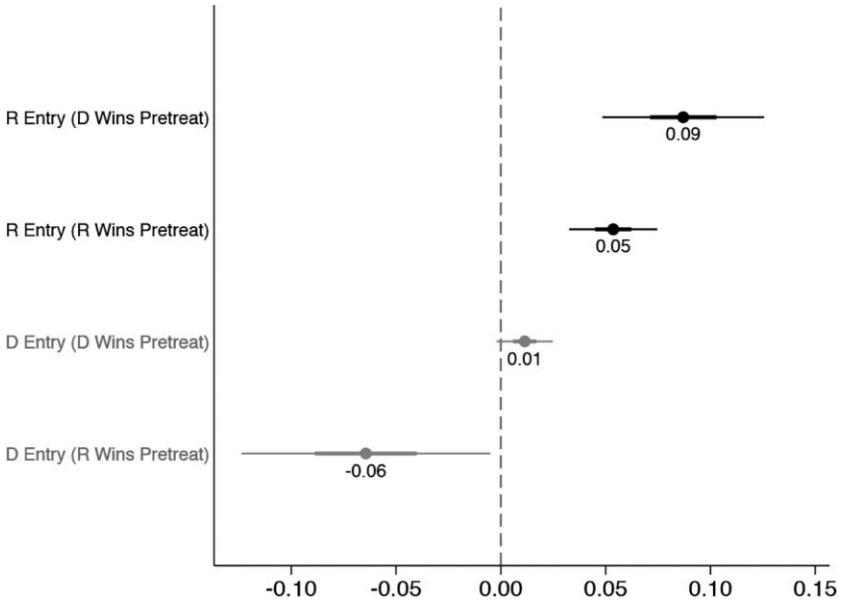

Figure 10. Candidate entry in state legislative elections (subsetted), 1972–81. Ordinary least squares difference in differences estimates of the change in state legislative candidate entry in treated states posttreatment. Robust standard errors clustered on states. All models include district fixed effects, year fixed effects, and state-specific linear time trends. "D Wins Pretreat" (n = 15,168) includes those state legislative districts wherein the Democratic Party won at least one pretreatment election; "R Wins Pretreat" (n = 6,393) includes those state legislative districts wherein the Democratic Party won no pretreatment elections. Color version available as an online enhancement.

districts won at least once by the Democratic Party during the pretreatment period and is 5 percentage points higher in the treated states posttreatment 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 posttreatment 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.

In short, the DD estimates indicate that the raw differences in vote shares 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 preruling period.

4.3. Coarsened Exact Matching

Equation (1) includes state-specific linear time trends to address the issue of nonparallel pretreatment 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 pretreatment 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, King, and Porro 2012).

Preprocessing is implemented on the cross-section of 2,646 state legislative districts for which we have vote share and candidate entry data for the 1972, 1974, and 1976 elections, and on the cross-section of 2,681 counties for which we have gubernatorial vote share data for the 1968, 1972, and 1976 elections or for the 1970, 1974, and 1978 elections. In the treated states are 1,360 of these 2,646 state legislative districts and 1,190 of these 2,681 counties (those with spending limits struck by *Buckley*).

Table A2 reports on the pretreatment 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 CEM on pretreatment levels of and trends in Republican electoral success. For state legislative elections, preprocessing using CEM was performed on the following district-level variables: average Republican state legislative vote shares for 1972 and 1974, change in Republican state legislative vote shares 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, preprocessing using CEM was performed on both averages of and changes in Republican gubernatorial vote shares between 1968 and 1972 or between 1970 and 1974, depending on a state's election cycle.[7]

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 vote shares, Republican candidate entry, and Democratic entry

[7] For both cross sections, Sturge's 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 nonmissing values for all variables were included in the CEM analyses.

between the 1974 and 1976 elections. For gubernatorial elections, the dependent variable is the county-level change in Republican vote shares between either 1976 and 1972 or 1978 and 1974. These differenced outcomes are assumed to be generated by the following equation:

$$\Delta\text{Vote share/Entry}_{is} = \beta\text{Spending Limit}_s + \gamma X_i + \varepsilon_{is} \quad (2)$$

The term γX_i represents the set (or a subset) of the district- or county-level pretreatment variables used to prune the two samples. Equation (2) is estimated using OLS with weights derived from CEM on all pretreatment variables reported in table A2.

Figures 11 and 12 report estimates of the coefficients on Spending Limit$_s$ 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

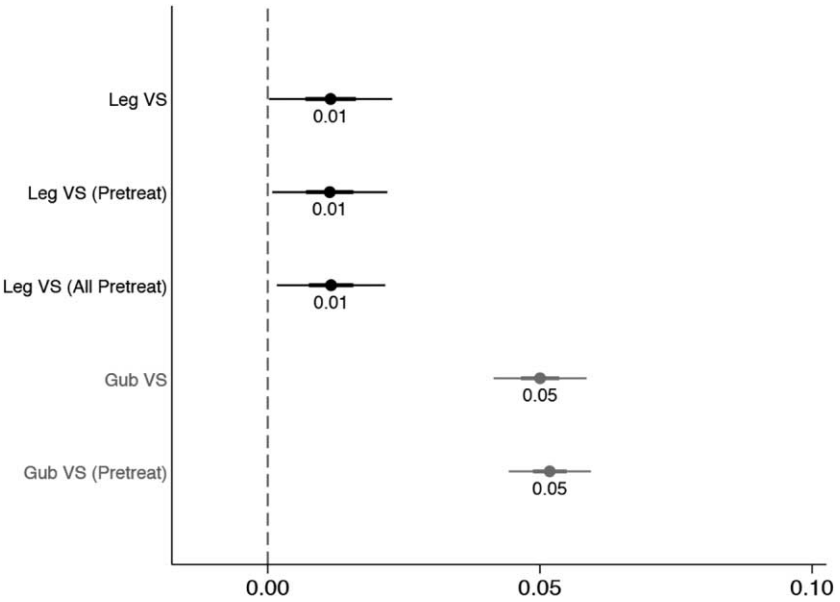

Figure 11. Change in Republican vote shares in state elections, coarsened exact matching (CEM) estimates. CEM-pruned ordinary least squares estimates of the relative change in Republican vote shares in treated states posttreatment. "Pretreat" models include levels of and trends in pretreatment vote shares; "All Pretreat" models include all pretreatment variables as controls. Legislative district, *n* = 2,613; gubernatorial county, *n* = 2,474. Gub = gubernatorial; Leg = legislative; VS = vote shares. Color version available as an online enhancement.

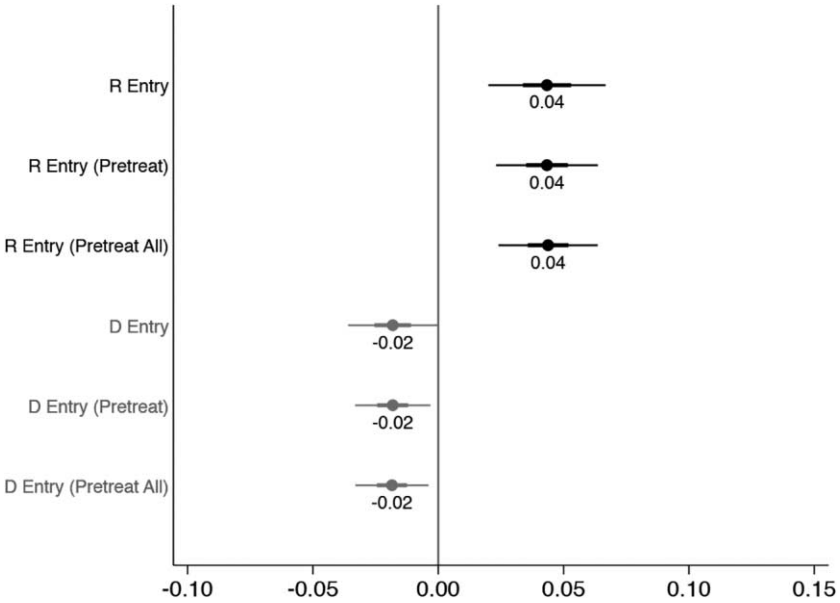

Figure 12. Change in candidate entry in state legislative elections, coarsened exact matching (CEM) estimates. CEM-pruned ordinary least squares estimates of the relative change in state legislative candidate entry in treated states posttreatment. "Pretreat" models include levels of and trends in pretreatment Republican (R)/Democratic (D) candidate entry; "Pretreat All" models include all pretreatment variables as controls. *n* = 2,613. Color version available as an online enhancement.

in figure 9. Republican vote shares 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 vote shares in gubernatorial elections rose by approximately 5 percentage points between the periods of 1972–76 and 1974–78 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.

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

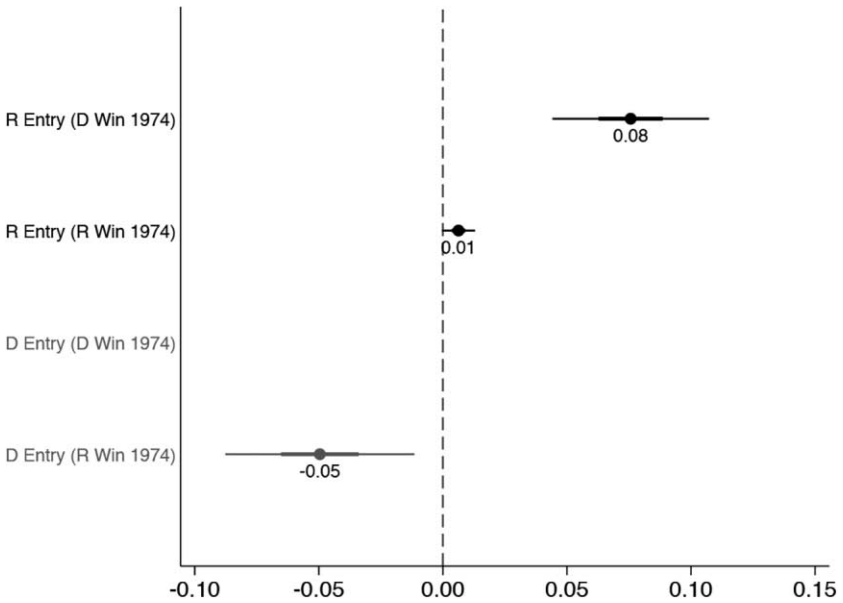

Figure 13. Change in candidate entry in state legislative elections, subsetted, coarsened exact matching (CEM) estimates. CEM-pruned estimates of the change in state legislative candidate entry in treated states posttreatment, for the subsamples of districts won in 1974 by the Democratic Party (D; *n* = 1,697) or the Republican Party (R; *n* = 920). All models include all pretreatment matching variables as controls. Color version available as an online enhancement.

prune the full sample.[8] 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 increased by 8 percentage points in 1976 in treated districts relative to control districts; in the latter districts Republican candidate entry increased by only 1 percentage point in treated districts relative to control districts.

Also as in figure 10, there is a larger effect of *Buckley* on Democratic candidate entry in districts won by the Republican Party in

[8] In the subsample of those 941 districts won by the Republican Party in 1974, the unmatched $\mathcal{L}_1$ is 0.20; after pruning 8 control and 13 treated districts using Sturge's rule tod bin the matching variables, the $\mathcal{L}_1$ statistic is reduced to 0.12. In the subsample of those 1,722 districts won by the Democratic Party in 1974, the unmatched $\mathcal{L}_1$ is 0.29; after pruning 11 control and 14 treated districts using Sturge's rule to bin the matching variables, the $\mathcal{L}_1$ statistic is reduced to 0.17.

1974 relative to districts won by the Democratic Party; in the former districts Democratic candidate entry decreased 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 nonsouthern 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 earlier. Figure A3 replicates figure 9, excluding the 11 former Confederate states. Results are generally similar to those reported in figure 9.

Second, we can further address the possibility that 1976 was an anomalous election by using 1978 as the first postruling election. Figure A4 replicates figure 9, using the changes between the 1974 and 1978 elections as the quantities of interest. Results are again generally similar to those reported in figure 9; although *Buckley*'s estimated effects on legislative and gubernatorial vote shares 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 A5 replicates figure 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 A6 replicates figure 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.

[9] The states included in this reduced sample are those shaded (treated) and white (control) in fig. A1.

[10] No states had public funding provisions for state elections in effect prior to 1976.

5. US HOUSE ELECTIONS

5.1. County-Level Vote shares 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 induced pro-Republican changes in 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 earlier 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 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 earlier 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.[11]

Figure 14 reports average county-level vote shares in House elections, using only the set of five states not holding concurrent state legislative or gubernatorial elections in 1976.[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 vote shares decreased more in this set of treated states after *Buckley* relative to this set of control states; the average Republican vote share increased by 12 percentage points between 1974 and 1976 in the control states but increased by only 2 percentage points in the treated states.[13]

[11] The concurrence of state electoral calendars with federal electoral calendars is almost perfectly uncorrelated with treatment status ($r = 0.01$ for concurrent federal/legislative elections and $r = 0.02$ for concurrent federal/legislative/gubernatorial elections).

[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.

[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 fig. A7.

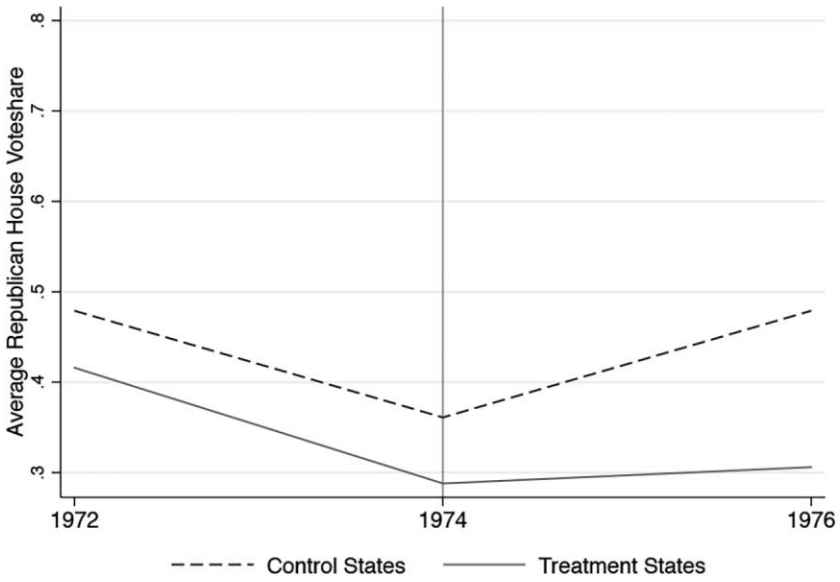

Figure 14. Average county-level Republican House vote share states with no concurrent 1976 state elections. Color version available as an online enhancement.

Figure 15 reports average county-level vote shares in House elections using only the set of 15 states holding concurrent state legislative and gubernatorial elections in 1976.[14] There is no evidence of a preruling pro-Republican trend in vote shares 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 vote share increased by 4 percentage points between 1974 and 1976 in the control states but increased by 5.5 percentage points in the treated states.

Figure 16 reports OLS and CEM-pruned estimates of the coefficients on Spending Limit$_s$ from equation (2), along with 90% confidence intervals, using the sample of counties for which we have vote share data for the 1972, 1974, and 1976 House elections. The dependent variable is the change in county-level Republican House vote shares between 1974 and 1976. The "no concurrent" coefficients are

[14] States holding state legislative and gubernatorial elections concurrently with federal elections in 1976 are New Hampshire, Rhode Island, Vermont, Delaware, Illinois, Indiana, Missouri, North Dakota, Arkansas, Louisiana, North Carolina, West Virginia, Montana, Utah, and Washington.

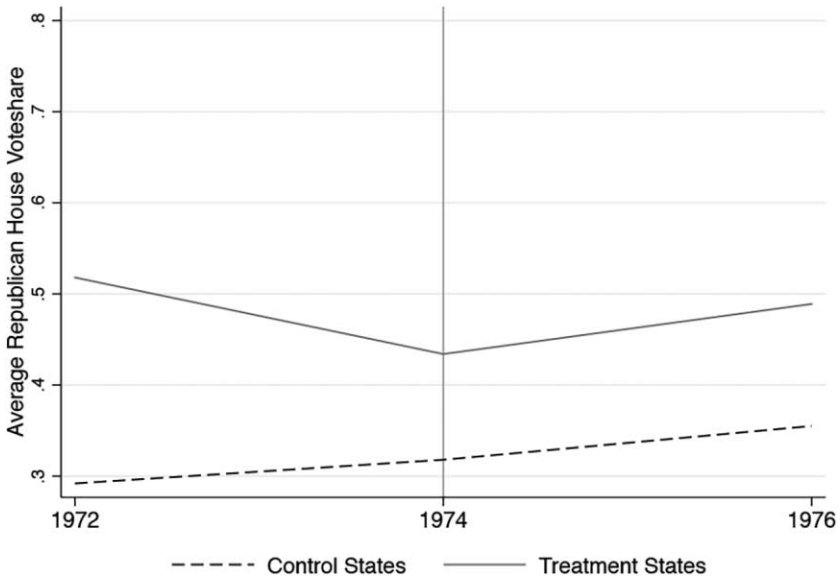

Figure 15. Average county-level Republican House vote share states with concurrent 1976 state elections. Color version available as an online enhancement.

estimated using only the sample of states not holding concurrent state legislative or gubernatorial elections in 1976. In the CEM model, this sample of 365 counties is preprocessed using CEM on the average levels of and changes in county-level Republican House vote shares in 1972 and 1974.[15] There is no evidence of increased Republican vote shares in the treated states posttreatment relative to the control states posttreatment. Instead, as was seen in figure 14, Republican vote shares actually decrease more in the treated states posttreatment relative to the control states posttreatment.

The "concurrent" coefficients are estimated using only the sample of states holding concurrent state legislative and gubernatorial elections in 1976. In the CEM model, this sample of 811 counties was also preprocessed using CEM on the average levels of and changes in county-level Republican House vote shares in 1972 and 1974.[16] Here we see increased Republican vote shares in the treated states posttreatment relative to the control states posttreatment; first differences in county-level Republican House vote shares between 1974

[15] The unmatched $\mathcal{L}_1$ is 0.42; after pruning 33 control and 9 treated counties using Sturge's rule to bin the matching variables, the $\mathcal{L}_1$ statistic is reduced to 0.24.

[16] The unmatched $\mathcal{L}_1$ is 0.51; after pruning 151 control and 11 treated counties using Sturge's rule to bin the matching variables, the $\mathcal{L}_1$ statistic is reduced to 0.28.

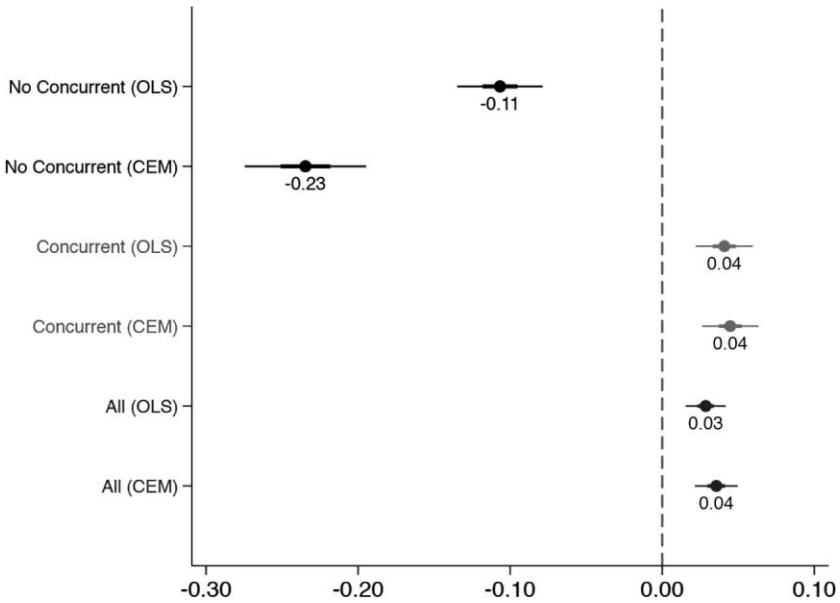

Figure 16. Change in county-level Republican House vote shares, ordinary least squares (OLS)/coarsened exact matching (CEM) estimates. OLS and CEM-pruned OLS estimates of the relative changes in county-level House Republican vote shares in treated states posttreatment. All models include all pretreatment matching variables as controls. Robust standard errors clustered on states; 90% confidence intervals. Color version available as an online enhancement.

and 1976 are 4 percentage points larger in the treated states relative to the control states.

Finally, the "all" coefficients are estimated using the full sample of counties, in which treatment is defined as a county's location in a state that had a limit on campaign spending struck by the Court's ruling in *Buckley* and that held state legislative and gubernatorial elections in 1976. This sample of 2,934 counties was again preprocessed using CEM on the average levels of and changes in county-level Republican House vote shares in 1972 and 1974.[17] Here again we see increased Republican vote shares posttreatment relative to all other states posttreatment. First differences in county-level Republican House vote shares between 1974 and 1976 are 3–4 percentage points higher in the treated states holding concurrent elections in 1976 relative to all other states. Figure A7 replicates the CEM models from

[17] The unmatched $\mathcal{L}_1$ is 0.51; after pruning 1,032 control counties using Sturge's rule to bin the matching variables, the $\mathcal{L}_1$ statistic is reduced to 0.28.

figure 16 using only the sample of nonsouthern states; results are nearly identical to those reported in figure 16.

These vote share estimates for concurrently held congressional elections are comparable in magnitude to the vote share estimates for gubernatorial elections, indicating large spillover effects of *Buckley* in concurrent congressional elections. This is consistent with greater pro-Republican campaign spending mobilizing more pro-Republican turnout in all concurrently held post-*Buckley* elections in the treated states relative to the control states (Spenkuch and Toniatti 2018) and/or inducing greater short-term increases in pro-Republican candidate preferences in the treated states relative to the control states (Gerber et al. 2011).

5.2. House DW-NOMINATE Scores

The vote share 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 posttreatment 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 the same. However, given the apparent spillover effects in concurrently held federal elections, we can estimate this effect on House incumbents. If state legislative and gubernatorial candidates moved to the right in the treated states posttreatment 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 House candidates to also move to the right in those treated states with posttreatment concurrent federal elections.

Figures 17 and 18 report average House DW-NOMINATE scores across treated and control states for the sets of states not holding and holding concurrent state legislative or gubernatorial elections in 1976. As we would expect, there is no apparent evidence of an effect of *Buckley* on DW-NOMINATE scores in the states not holding

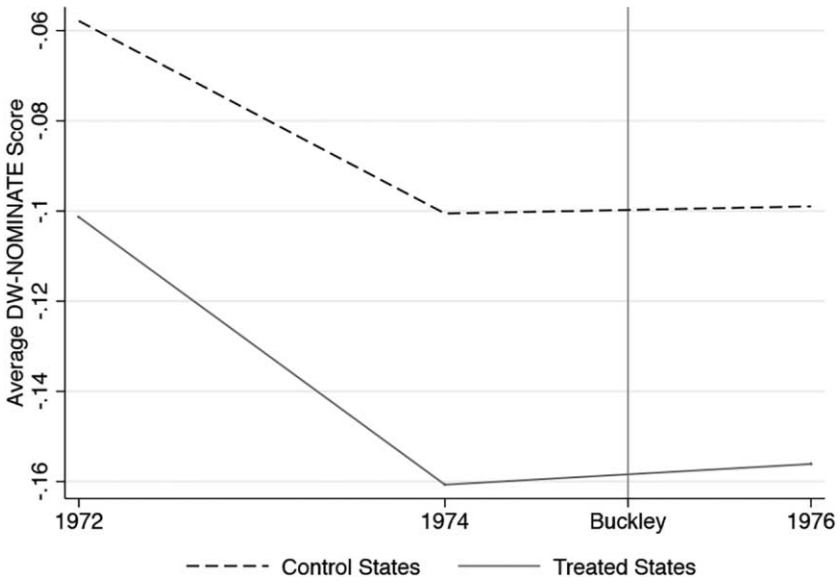

Figure 17. Average DW-NOMINATE scores in states with no concurrent 1976 state elections. Color version available as an online enhancement.

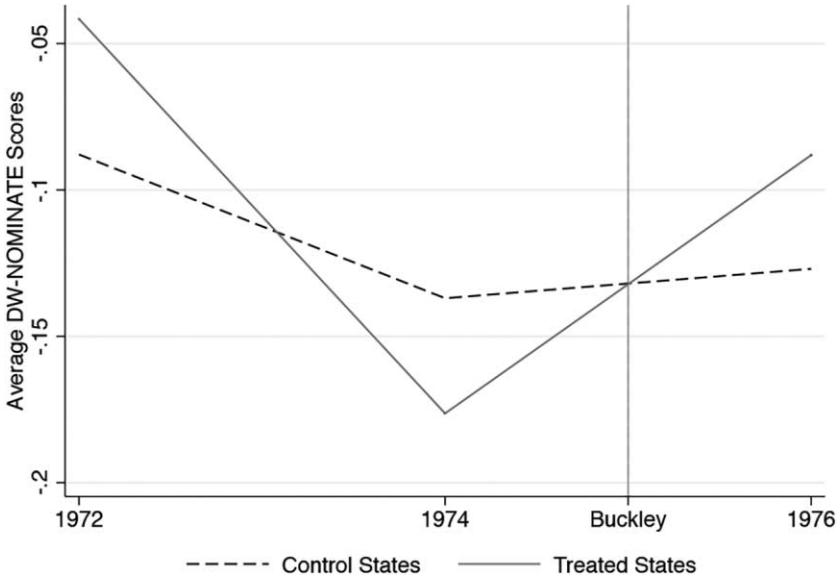

Figure 18. Average DW-NOMINATE scores in states with concurrent 1976 state elections. Color version available as an online enhancement.

concurrent elections in 1976; trends are almost precisely parallel across the two sets of states. However, there is a sharp movement rightward in DW-NOMINATE scores in the treated states postruling in states holding concurrent elections in 1976, a movement that is not mirrored in the control states.

Figure 19 reports DD estimates from equation (1) of the coefficient on Spending Limit$_s$ × Post-*Buckley*$_t$, along with 90% confidence intervals, for states not holding concurrent elections in 1976, states holding concurrent elections, and all states, in which treatment is defined as a district's location in a state 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. In states not holding concurrent state elections, DW-NOMINATE scores in treated states moved slightly to the left after the 1976 elections relative to scores in the control states. In states holding concurrent state elections, however, DW-NOMINATE scores moved 0.06 point to the right postruling in treated states relative to a baseline DW-NOMINATE average

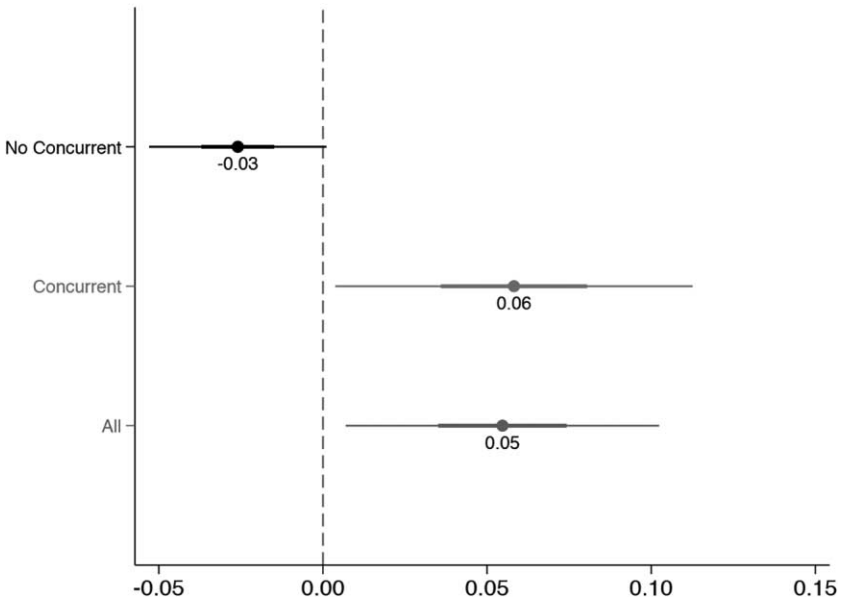

Figure 19. Difference in differences (DD) estimates; DW-NOMINATE scores 1972–80. Ordinary least squares DD estimates of the relative changes in district-level House DW-NOMINATE scores, 1972–80, for districts located in states not holding concurrent elections in 1976, districts located in states holding concurrent elections, and districts in all states, in which treatment is defined as location in a treated state holding concurrent elections in 1976. All models include district and year fixed effects; 90% confidence intervals reported. Color version available as an online enhancement.

of –0.10 point in the control states. The same pattern is seen in the estimates using all states, defining treatment as a treated state holding concurrent state elections in 1976; DW-NOMINATE scores moved 0.05 point to the right postruling in treated/concurrent states, again relative to a baseline DW-NOMINATE average of –0.10 in the control states.

Figure 20 reports OLS and CEM-pruned OLS estimates of the coefficients on spending limit$_s$ from equation (2), along with 90% confidence intervals. The dependent variable is the change in district-level DW-NOMINATE scores between 1974 and 1976. The "no concurrent" coefficients are estimated using only the sample of states not holding concurrent state legislative or gubernatorial elections in 1976; the "concurrent" coefficients are estimated using only the sample of

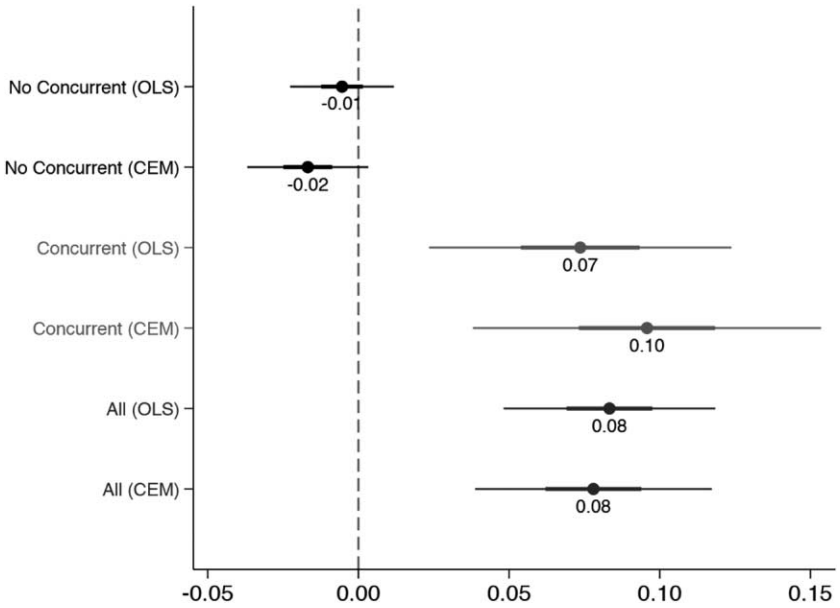

Figure 20. Ordinary least squares (OLS)/coarsened exact matching (CEM) estimates; change in DW-NOMINATE scores 1974–76. OLS and CEM-pruned OLS estimates of the relative changes in district-level House DW-NOMINATE scores between the ninety-fourth and ninety-fifth Congresses, for districts located in states not holding concurrent elections in 1976, districts located in states holding concurrent elections, and districts in all states, in which treatment is defined as location in a treated state holding concurrent elections in 1976. Districts in CEM models are matched based on average levels of and changes in DW-NOMINATE scores across the ninety-third and ninety-fourth Congresses. All models include preruling matching variables as controls; robust standard errors clustered on states; 90% confidence intervals reported. Color version available as an online enhancement.

states holding concurrent state legislative and gubernatorial elections in 1976; the "all" coefficients are estimated using the full sample of counties, in which treatment is defined as a district's location in a state that had a limit on campaign spending struck by the Court's ruling in *Buckley* and that held state legislative and gubernatorial elections in 1976. In the CEM models, samples are preprocessed using CEM on the average levels of and changes in district-level DW-NOMINATE scores in 1972 and 1974. All models include preruling matching variables as controls; robust standard errors are clustered on states.

There is no evidence of an effect of *Buckley* in the treated states posttreatment, relative to the control states posttreatment, in states not holding concurrent elections in 1976. However, in the models taking into account the concurrence of state and federal elections, DW-NOMINATE scores move 0.07–0.10 point more to the right in treated states posttreatment relative to a baseline increase in conservatism in the control states of 0.01 DW-NOMINATE point. The same pattern is seen in the estimates using all states, defining treatment as a treated state holding concurrent state elections in 1976; DW-NOMINATE scores moved 0.08 point more to the right postruling in treated/concurrent states relative to a baseline increase in conservatism in the control states of 0.01 DW-NOMINATE point.

5.3. Robustness

We can challenge the results reported in figure 20 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 A8 replicates figure 20 after excluding the southern states; results are nearly identical to those reported for the full sample.

Second, we can conduct a preruling placebo test, assuming that 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 20, 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 preruling trends. Figure A9 replicates figure 20 for the preruling period. Treated states are defined as those states with state-level campaign spending restrictions in 1974; concurrent states are defined as those states holding legislative and gubernatorial elections concurrently with federal elections in 1974. All CEM estimates were preprocessed using Sturge's rule on the average levels of and changes in districts' DW-NOMINATE scores in the ninety-second and ninety-third Congresses. There is no evidence of a preruling 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 have often been held to sufficiently serve the government's interest in preventing corruption so as to warrant their potential negative impacts on speech rights. Spending limits, by contrast, have not been seen as sufficiently deterrent of corruption so as 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 vote shares in state legislative and gubernatorial elections, to increased Republican candidate entry and decreased Democratic candidate entry in state legislative elections, and to both increased Republican House vote shares and the election of relatively more conservative 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.

APPENDIX

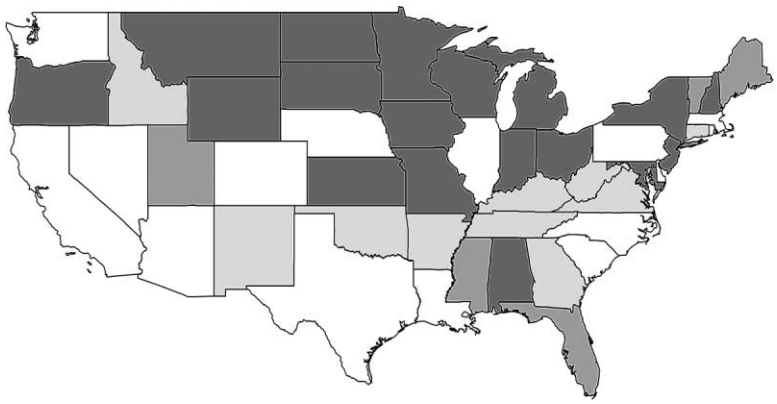

Figure A1. States enacting mandatory expenditure limits (Primo and Milyo 2006). Dark grey = states that enacted spending limits prior to 1950 and did not change them prior to 1976; medium grey = states that enacted spending limits between 1950 and 1976; light grey = states that had spending limits at some point between 1950 and 1976 but repealed them before *Buckley*; white = states that had no spending limits between 1950 and 1976. Color version available as an online enhancement.

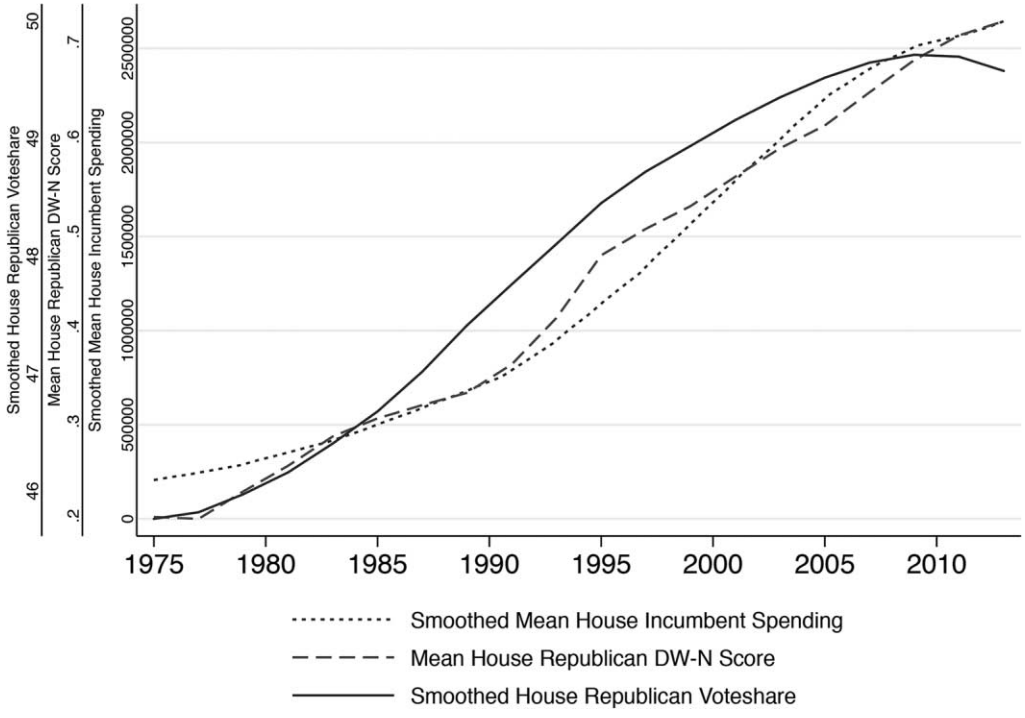

Figure A2. National Republican House vote share, mean House incumbent spending, and mean House Republican DW-NOMINATE scores (ICPSR 2013; Center for Responsive Politics 2015). Color version available as an online enhancement.

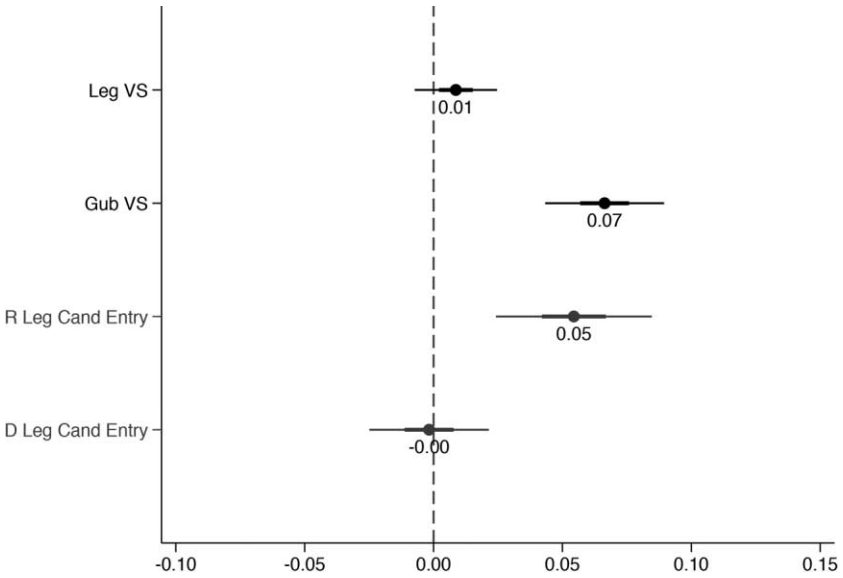

Figure A3. Republican electoral success in nonsouthern state elections, 1972–81. Ordinary least squares difference in differences estimates of the change in Republican electoral success in nonsouthern treated states posttreatment. All models include district/county fixed effects, year fixed effects, and state-specific linear time trends; 90% and 50% confidence intervals reported. Cand = candidate; D = Democratic; Gub = gubernatorial; Leg = legislative; R = Republican; VS = vote shares. Color version available as an online enhancement.

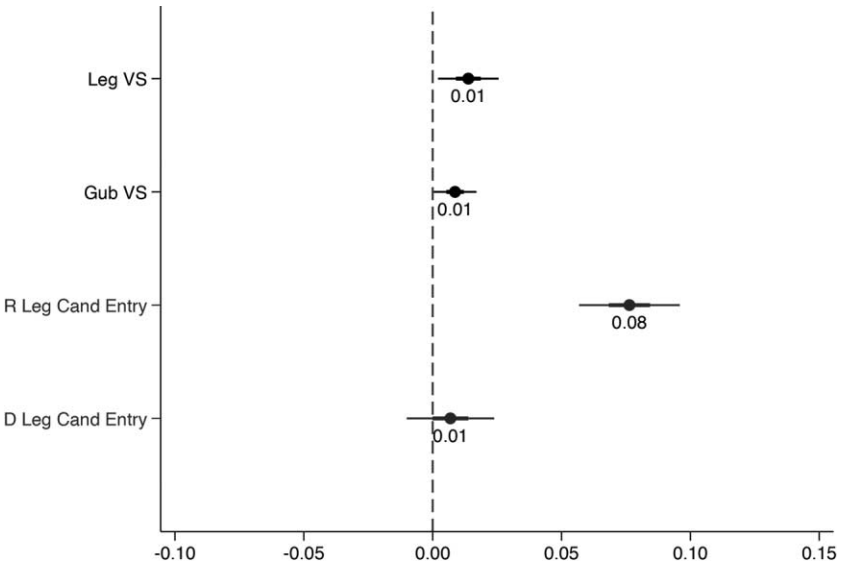

Figure A4. Change in Republican vote shares in 1978 and 1974 state elections, coarsened exact matching (CEM) estimates. CEM-pruned ordinary least squares estimates of the relative change in Republican vote shares in treated states posttreatment, using 1978 as first posttreatment election. "Pretreat" models include levels of and trends in pretreatment vote shares. "Pretreat All" models include all pretreatment variables as controls. Cand = candidate; D = Democratic; Gub = gubernatorial; Leg = legislative; R = Republican; VS = vote shares. Color version available as an online enhancement.

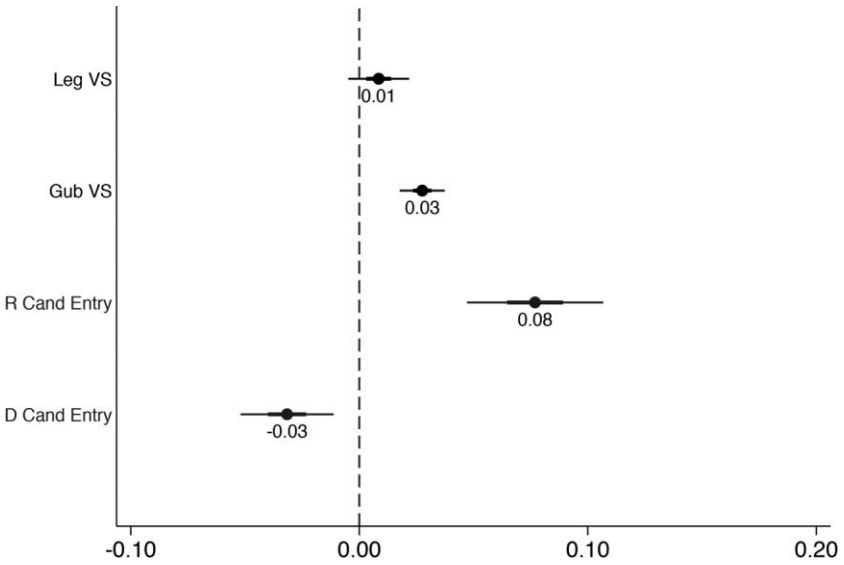

Figure A5. Change in Republican electoral success in state elections, coarsened exact matching (CEM) estimates, states with campaign spending statutes unchanged since 1950. CEM-pruned ordinary least squares (OLS) estimates of the change in Republican electoral success in state elections in treated states posttreatment, using only those states whose laws regulating campaign expenditure limits remained unchanged between 1950 and 1976. All results are from OLS regressions using weights derived from coarsened exact matching on average Republican vote share 1972–74 and change in Republican vote share 1972–74 (state legislative vote share, *n* = 1,927); average Republican vote share 1972–74 and change in Republican vote share 1968–72 or 1970–74 (gubernatorial vote share, *n* = 1,929); average Republican/Democratic legislative candidate entry 1972–74 and change in Republican/Democratic legislative candidate entry 1972–74 (candidate entry, *n* = 2,104). Cand = candidate; D = Democratic; Gub = gubernatorial; Leg = legislative; R = Republican; VS = vote shares. Color version available as an online enhancement.

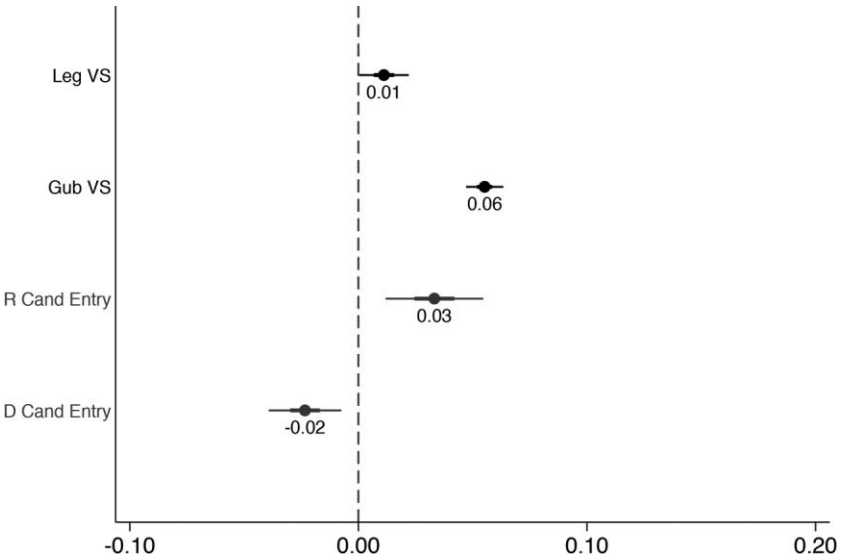

Figure A6. Change in Republican electoral success in state elections, coarsened exact matching (CEM) estimates, controlling for pretreatment presence of other campaign finance statutes. CEM-pruned ordinary least squares (OLS) estimates of the change in Republican electoral success in state elections in treated states posttreatment 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 CEM on all pretreatment variables reported in table A2. Legislative district, *n* = 2,613; gubernatorial county, *n* = 2,474. Cand = candidate; D = Democratic; Gub = gubernatorial; Leg = legislative; R = Republican; VS = vote shares. Color version available as an online enhancement.

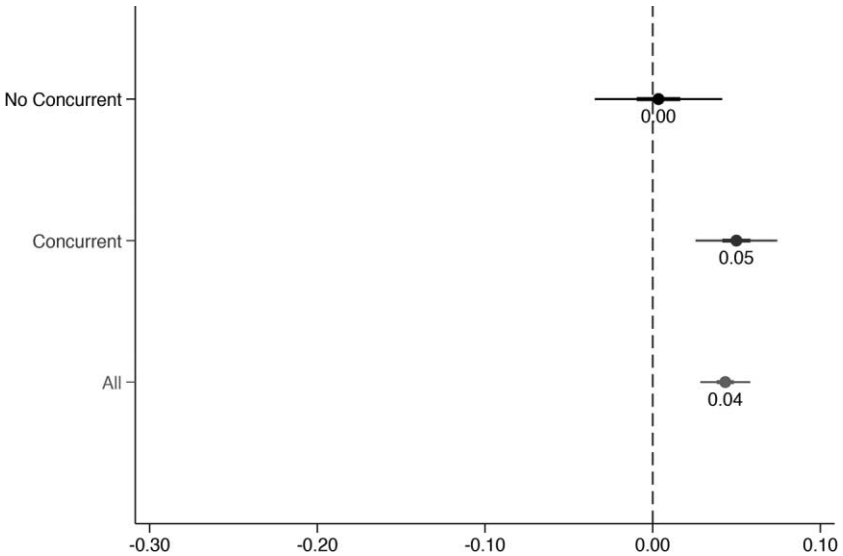

Figure A7. Change in nonsouthern county-level Republican House vote shares, coarsened exact matching (CEM) estimates. CEM-pruned ordinary least squares estimates of the relative changes in county-level House Republican vote shares in treated states posttreatment. All models include all pretreatment matching variables as controls. Southern states are excluded. Color version available as an online enhancement.

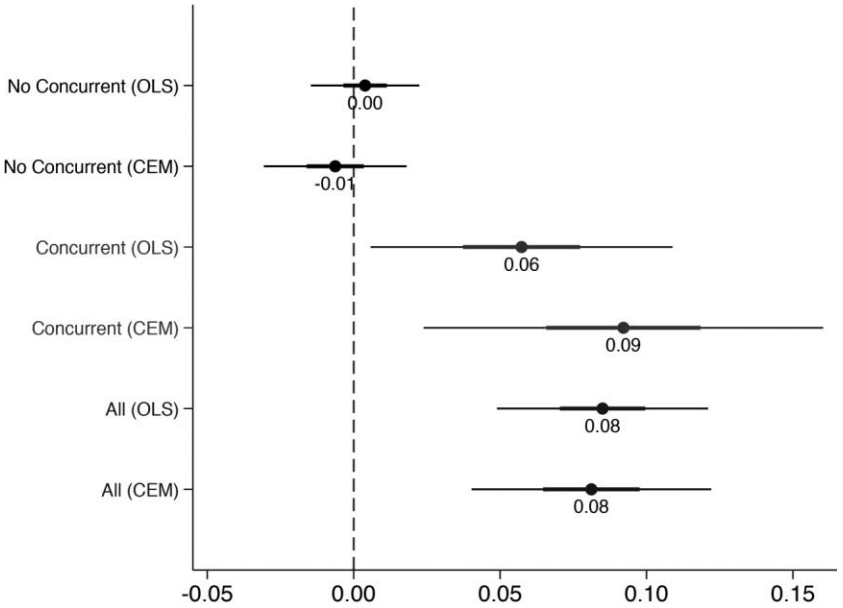

Figure A8. Ordinary least squares (OLS)/coarsened exact matching (CEM) estimates; change in DW-NOMINATE scores, 1974–1976, nonsouthern states only. OLS and CEM-pruned OLS estimates of the relative changes in district-level House DW-NOMINATE scores between the ninety-fourth and ninety-fifth Congresses, nonsouthern states only, for districts located in states not holding concurrent elections in 1976, districts located in states holding concurrent elections, and districts in all states, in which treatment is defined as location in a treated state holding concurrent elections in 1976. Districts in CEM models are matched based on average levels of and changes in DW-NOMINATE scores across the ninety-third and ninety-fourth Congresses. All models include prerul-ing matching variables as controls; robust standard errors clustered on states; 90% con-fidence intervals reported. Color version available as an online enhancement.

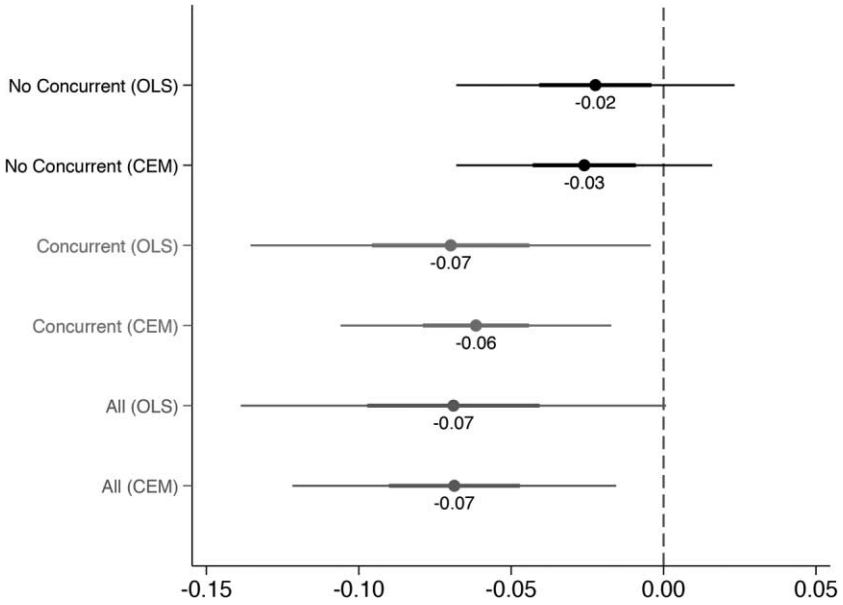

Figure A9. Ordinary least squares (OLS)/coarsened exact matching (CEM) estimates; change in DW-NOMINATE scores, 1972–1974, preruling placebo test. OLS and CEM-pruned OLS estimates of the relative changes in district-level House DW-NOMINATE scores between the ninety-third and ninety-fourth Congresses. All models include all pretreatment matching variables as controls. Robust standard errors clustered on states; 90% confidence intervals reported. Color version available as an online enhancement.

Table A1. Descriptive Statistics

	Control States	*n*	Treated States	*n*	Difference
Average Republican legislative vote share:					
1972	.38	1,834	.49	1,728	.11**
1974	.33	1,715	.38	2,290	.04**
1976	.33	1,779	.42	2,149	.09**
Average Republican gubernatorial vote share:					
1972	.42	696	.51	640	.09**
1974	.35	1,133	.46	934	.10**
1976	.35	443	.49	450	.14**

Table A1. (*continued*)

	Control States	*n*	Treated States	*n*	Difference
Average Republican legislative candidate entry:					
1972	.72	1,992	.82	1,939	.11**
1974	.68	1,869	.78	2,304	.10**
1976	.65	1,947	.83	2,166	.18**
Average Democratic legislative candidate entry:					
1972	.97	1,992	.84	1,939	.13**
1974	.97	1,869	.96	2,304	.01*
1976	.97	1,947	.93	2,166	.04**

State legislative/gubernatorial outcomes are averaged across all state legislative districts/counties reporting nonmissing data in specified years.
*$p < .05$.
**$p < .01$.

Table A2. CEM Balance Statistics

	Unmatched Sample	Matched Sample
State legislative elections:		
$\mathcal{L}_1$	.30	.19
Avg R VS, 1972–74 $\mathcal{L}_1$	.15	.05
Change in R VS, 1972–74 $\mathcal{L}_1$	.13	.08
Avg R cand entry, 1972–74 $\mathcal{L}_1$	.10	.00
Change in R cand entry, 1972–74 $\mathcal{L}_1$	.02	.00
Avg D cand entry, 1972–74 $\mathcal{L}_1$	.04	.00
Change in D cand entry, 1972–74 $\mathcal{L}_1$	.03	.00
Number of strata	64	48
n control	1,286	1,262
n treated	1,360	1,351
Gubernatorial elections:		
$\mathcal{L}_1$	.52	.26
Avg R VS, 1968–72 or 1970–74 $\mathcal{L}_1$	.38	.07
Change in R VS, 1968–72 or 1970–74 $\mathcal{L}_1$	.34	.09

Table A2. (*continued*)

	Unmatched Sample	Matched Sample
Number of strata	83	47
n control	1,491	1,366
n treated	1,190	1,108

CEM performed using Sturge's rule on all variables. The overall $\mathcal{L}_1$ 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. 2012). For example, before pruning, the overall $\mathcal{L}_1$ for the sample of state legislative districts is 0.30, indicating that only 70% of the two multidimensional distributions overlap. After pruning, the overall $\mathcal{L}_1$ is reduced to 0.19, indicating that 81% of the distributions overlap, with a loss of 9 treated and 24 control districts. Avg = average; cand = candidate; D = Democratic; R = Republican; VS = vote shares.

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