Partisan Effects of Legislative Term Limits

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Abstract

Term limits remain a popular policy reform and have generated a great deal of scholarship as a result. Although many predicted that term limits would benefit the Republican party, the literature finds no marked partisan effects, possibly because termed-out legislators have largely been replaced by co-partisans. This article demonstrates that term limits have indeed had partisan effects—just not on electoral outcomes. Term limits have caused a significant reallocation of institutional power from Democrats to Republicans (as measured by contributions from access-oriented interest groups), in large part because they have removed more senior Democrats than Republicans. The partisan effects of term limits therefore point to the institutional value of seniority. Already in the early months of 2013, a number of state and municipal governments across the U.S. have considered new proposals to implement term limits.¹ A few examples include the Texas state senate, which voted on a resolution to impose term limits on the executive office,² the village of Niles, in Cook County, Illinois, which voted to limit its trustees and president,³ and the Los Angeles Board of Education, which voted to term limit its president.⁴ As of 2013, fifteen U.S. states have legislative term limits in place.⁵ What are the effects of these major interventions? For purposes of institutional design, this question is itself interesting. In addition, term limits provide an unusual opportunity to study the value of legislative seniority, which otherwise rarely varies in a random fashion.

In this article, I employ a difference-in-differences design in U.S. state legislatures, 1990–2010, to assess the causal effects of term limits on the distribution of power in the legislature, following previous literature in using access-oriented interest group contributions as a proxy for institutional power. I show that term limits have caused a significant reallocation of power away from the Democratic party and towards the Republican party, at least in part because term limits erode the Democrats' typically large advantage in legislative seniority. Because term limits do not appear to have any electoral effects, and do not change the parties' share of the legislature on average, the findings suggest that seniority *per se* carries institutional power and is valuable to strategic interest groups.

Political science research on legislative term limits has been fruitful, assessing their effects on the distribution of power within the legislature and across branches of government (e.g., Apollonio and La Raja 2006; Cain and Kousser 2004; Cain and Wright 2007; Carey, Niemi, and Powell 1998; Carey et al. 2006; Kousser 2005; Miller, Nicholson-Crotty, and Nicholson-Crotty 2011), the encouragement of more diverse candidates (e.g., Bowser 2003; Carey, Niemi, and Powell 1998; Carey et al. 2006; Moncrief, Powell, and Storey 2007), electoral competition (e.g., Bowser 2003; Daniel and Lott Jr 1997; Masket and Lewis 2007), fiscal policy (e.g., Lewis 2012), and a host of other outcomes (for reviews see Cain and Levin 1999; Carey et al. 2006; Mooney 2009). But the empirical literature has not devoted much

time to the *partisan* effects of term limits,⁶ perhaps because legislative term limits have had no discernible partisan electoral effects (Powell 2008).⁷

In the early 1990s, when states were first implementing term limits, many observers predicted that they would benefit the Republican party, who had fewer incumbents at risk of being "termed out" than the Democratic party (e.g., Karp 1995; Moncrief and Thompson 1991). Carey, Niemi, and Powell (1998) report that "many Democrats suspected that widespread Republican support for the reforms in the early 1990s was driven by the fact that there were far more Democratic than Republican incumbents in both state legislatures and Congress" (276). Donovan and Snipp (1994) likewise present evidence that Republican voters supported term limits more than Democrats – possibly in part because of the strategic implications. They explain: "Thus, tenure limitations might be viewed as advantageous to Republicans since most incumbents losing seats could be Democrats" (494). This view was hardly limited to academics. In California, for example, the Democratic majority in the legislature hired lawyers to argue that the term limits, passed by ballot initiative, were unconstitutional. Reporting on this law suit, *The New York Times* explained that the Democratic legislature "views [term limits] as an attack on the continued dominance of the Democratic Party" (Bishop 1991).

However, there are also good reasons to explain why term limits would have no such partisan effects. Many state legislative districts are "safe" (Squire 2000; Weber, Tucker, and Brace 1991) – especially those with senior members affected by term limits, since to become senior one must be reelected repeatedly, an easier feat for party-matched candidates in partisan districts. These districts may be likely to stick with candidates from one party regardless of their seniority, thus dampening the effect of term limits on electoral outcomes. In addition, challengers may be of lower quality at the same time as incumbents are being termed out (Gilmour and Rothstein 1994; Powell 2008), preventing the Republicans from gaining many new seats. Once in office, the new incumbents, even though they are less senior and less experienced, might be able to fulfill many of the same duties as their termed-out predecessors, allowing the majority party to continue business as usual.

The article is organized as follows. The next section lays out the empirical strategy and presents estimated results revealing the partisan effects of term limits. I use a differencein-differences design to explore the causal effects of term limits on interest group campaign donations, showing that term limits have had a profound effect on Democratic receipts from interest groups but no such effect for the Republican party. Subsequently, I show that the results are robust and that the identifying assumption of the empirical model is plausible. States that implement term limits are not trending more conservatively, and the model is not sensitive to the inclusion of variables to relax the so-called "parallel trends" assumption of the diff-in-diff. Then, I consider implications of the findings. Why have term limits sapped Democratic power so much? I present evidence that term limits disproportionately hurt Democrats because Democratic legislators held a disproportionate amount of seniority in the legislature before term limits. This effect appears strongest in more professionalized legislatures, where seniority ought to matter more. Finally, I conclude.

Empirics

An important literature justifies the idea that strategic, access-oriented interest group campaign donations can measure institutional power, both in theory (e.g., Hall and Wayman 1990; Kroszner and Stratmann 1998) and empirically (e.g., Grier and Munger 1991; Snyder 1992). The key link in the argument is that access-oriented interest groups act as *investors*. Snyder (1992) presents strong evidence for this view, showing that access-oriented interest groups prefer to donate to MCs more likely to provide long-term value to investors. Ansolabehere and Snyder (1998) proposes leveraging access-oriented group donations to find the sources of institutional power. If these groups act as investors, their money should flow to those with power in the legislature. This is the pattern of evidence they discover for access-oriented interest group donors (and not for other types). The strategy of using campaign donations to measure institutional power has already enjoyed great success in the term limits literature, too. Apollonio and La Raja (2006) use campaign donations as a measure of institutional power to demonstrate the ways term limits have shifted power within the legislature and across the different branches of government. As the authors write, "In short, contributions should reflect the influence of legislators and their ability to control policy" (262). Following this logic, I investigate the effect of term limits on interest group campaign donations in order to test for partisan changes in the distribution of power. To strengthen the interpretation of the findings, I replicate the analysis with ideological interest groups as well, showing that term limits have no similar partisan effects on these other donors.⁸

I collect data on campaign donations to every state legislative candidate by year, 1990-2010.⁹ This dataset is freely available from the National Institute on Money in State Politics (http://www.followthemoney.org/). The dataset breaks down donations by donor and recipient and codes donors by industry and sector, making it possible to separate out donations from individuals and donations from groups (e.g. corporations, labor unions, etc.). Most importantly, Follow the Money identifies individual donors, as opposed to group donors, by always including a comma in their name (e.g., "Smith, John").¹⁰ I remove all donations from individuals using this formatting regularity.

To isolate the strategic behavior of access-oriented interest groups, I also remove donations that come from party organizations, candidate self-finance, single-issue ideological groups, and public funding, consistent with the method proposed in Snyder (1992). These sources are readily identifiable in the Follow the Money data, using the sector codings.¹¹ In the Appendix, I replicate the paper's main analysis on ideological interest groups, as coded by Follow the Money. No partisan effects are found, suggesting that Follow the Money has provided accurate codings of interest group sectors.¹²

[Table 1 about here.]

Term limits are measured as taking effect in the first year any legislator can be prevented from running due to previous terms served, consistent with previous literature. Table 1 lists the states with active term limits, along with the years in which term limits take effect.¹³ Figure 1 summarizes the main results. The red solid line presents the difference (in thousands of dollars) between average interest group donations to Democratic and Republican candidates for state legislators in term-limited states, in the years directly preceding and following the implementation of term limits. Although different states implement term limits in different years, I re-center the data so that years are measured as years before and after term limits are implemented.¹⁴ The blue dotted line represents the same partian differential in interest group donations for states that never implement term limits, averaged over the same periods of times as the term-limited states for comparability.¹⁵ As the figure shows, the partian differential is extremely similar before term limits in both term-limited and non-term-limited states; after term limits, however, the Democratic party experiences a pronounced disadvantage, only in term-limited states. A tentative conclusion, then, is that term limits have caused a significant shift in partian interest group contributions. The econometric analysis reinforces this conclusion from Figure 1.

[Figure 1 about here.]

To test econometrically for the partian effects of term limits on campaign donations, and therefore on institutional power, it is tempting to compare term-limited states to nonterm-limited states directly. If we find that the Democrats are receiving fewer interestgroup donations in term-limited states, we might conclude that term limits have caused this decrease. This comparison is clearly biased, however, because term-limited states may differ from non-term-limited states in many unobserved ways. To control for time-invariant differences between the two groups, we can employ state fixed effects to look only at changes *within* a given state, comparing Democratic donation receipts before and after term limits are implemented. However, this specification may still be biased if the outcome variable is trending. Suppose, for example, that voters implement term limits when Democrats are already losing campaign donations, and that term limits have no effect on this trend. Then the fixed-effects model will attribute a spurious causal effect to term limits, because the switch to term limits will be associated with a decrease in donations. To control for this trend problem I add year fixed effects, using a "difference-in-differences" strategy to compare the changes before and after term limits in treated states to the same changes in non-termlimited states, thus differencing out the trend.

Therefore, I estimate models of the form

$$Log \ Donations_{scit} = \beta_0 + \beta_{rlim} Limit_{sct} + \beta_d Dem_{scit} + \beta_{dlim} Dem_{scit} \cdot Limit_{sct} + \delta_s + \gamma_t + \epsilon_{scit}, \quad (1)$$

where $Log \ Donations_{scit}$ represents the log of access-oriented interest group donations to legislator *i* in chamber $c \in \{lower, upper\}$ in state *s* in time t.¹⁶ The variable $Limit_{sct}$ takes the value one if chamber *c* in state *s* has a term limit in effect in year *t*, and zero otherwise. The variable Dem_{scit} is a dummy indicating that legislator *i* is a Democrat. Finally, δ_s and γ_t represent state and year fixed effects, respectively, and ϵ_{scit} is the disturbance term.

The quantity of interest is β_{dlim} , the coefficient on the interaction term between *Limit* and *Dem* that tests for the difference in the effect of term limits on Democrat and Republican campaign receipts. This coefficient measures the average differential causal effect of term limits on Democratic candidates under the assumption of common counterfactual trends. Placebo tests presented below establish the plausibility of this assumption.¹⁷ Because the treatment is fixed at the state level and errors are potentially correlated at the state level, all estimation uses robust standard errors clustered at the state level. Results are robust to the use of various forms of the block bootstrap recommended in Bertrand, Duflo, and Mullainathan (2004) and Cameron, Gelbach, and Miller (2008) for estimating standard errors in difference-in-differences estimation.¹⁸

[Table 2 about here.]

Table 2 presents estimated results using logged interest group donations, with and without controls.¹⁹ The quantity of interest is the coefficient on the interaction term, "Democrat \cdot Limit," β_{dlim} from Equation 1. In the second column of the table, I include controls for the presence of bans on corporate and/or union contributions ("Ban") and a public campaign funding program ("Public"), both of which affect interest group campaign donation behavior. The inclusion of these controls does not affect the estimate of the quantity of interest.²⁰ There is a marked disparity in the effect of term limits on the two parties. While Republican legislative candidates see a possible increase in interest group donations, Democrats see interest group donations decrease by about 22%. This number comes from adding the coefficient on Limit in the full specification (second column) to the coefficient on its interaction with the Democrat dummy (0.415 + -0.666 = -0.251), and then using the approximation for the coefficient on a dummy variable with a logged outcome variable (exp(-.251) - 1 \approx -0.22).²¹

In both cases, the difference between the Democratic and Republican response to term limits is highly statistically significant. Additionally, the removal of Maine and Arizona from the analysis, both states which undertook campaign finance reform at the same time as they passed term limits, does not substantively change the estimated results, which remain statistically significant and very similar in magnitude.

This partian effect of term limits is not driven by any fundamental differences between term-limited and non-term-limited states, which are accounted for by the state fixed effects. This effect is also not the result of any national anti-Democratic trend because the trends of non-term-limited states are used as the baseline against which term limits are assessed. Furthermore, in the Appendix Table 6 I show that there is no evidence that term-limited states are trending differently than non-term-limited states before term limits are introduced. The inclusion of state-specific linear time trends does not affect the estimates presented above, suggesting that non-term-limited states provide a plausible counterfactual trend for term-limited states. These results do not depend on term limits having no effect on campaign donations more broadly. Indeed, we have good reason to believe that term limits will directly affect overall campaign donation behavior (Carey, Niemi, and Powell 2000; Powell 2012). Because this analysis looks at the *differential* effect between the two parties, such overall shifts are canceled out.²²

Counterarguments Considered

The causal inference in this article depends upon the assumption that the treatment, the implementation of term limits, is exogenous to trends in partisan campaign donation behavior. That is to say, the article's findings rest on the assumption that non-term-limited states provide a plausible counterfactual trend for term-limited states. This does not require the treatment to be unrelated to fundamental characteristics of the states that influence donation behavior, but only that the treatment is unrelated to the *trends* in campaign donation behavior, a much weaker and far more plausible assumption. This assumption is also remarkably weak in the sense that, to be problematic, any unobserved, time-varying factor must not only affect campaign donation behavior, but must do so in a partisan manner, since the analysis examines the *difference* in donations to the two parties.

Nevertheless, this assumption is violated if term-limited states differ systematically from non-term-limited states in a way that influences the decision to implement term limits, affects partisan campaign donations, and varies over time. What if states that implement term limits do so because they are becoming more conservative, in a way that non-term-limited states are not? This could explain the changing composition of legislative donations rather than the shock to the partisan seniority distribution. This explanation is unlikely because term-limited states are not trending unusually (relative to non-term-limited states) in their Republican presidential vote share, a fact I document through a difference-in-differences regression.²³ The results, presented in Table 3, show that the implementation of term limits is not associated with any meaningful changes in state-level conservativeness. This is consistent with voter survey evidence presented in Carey et al. (2006).

[Table 3 about here]

That term limits are not more common in more conservative states, even though they were pushed by the Republican party, may not be so surprising in light of evidence in the literature. Cain and Levin (1999) summarizes the situation: "Term limits became the catch basin for the flow of public discontent unleashed in the post-Watergate period and fed by frustration in the 1980s over divided government" (167). In surveys of voters, while conservativeness was a good predictor of support for term limits (e.g., Donovan and Snipp 1994), there was significant support from "cynical" and "underrepresented" groups (e.g., Karp 1995; Southwell 1995). While Republicans may have wanted term limits, their likelihood for successfully implementing them varied based on factors such as voter cynicism that apparently do not correlate with state-level ideology. Bolstering this claim, Mooney (2009) shows that treated and control states are extremely similar across a variety of important covariates including demographics, per-capita income, gross state product, voter turnout, and ideology.²⁴

Most important for the purposes of this article, term limits are typically passed by *initiative*, not by the members of the legislature itself.²⁵ Obvious inferential difficulties would result if strategic politicians were choosing when to limit themselves. Because the voters of the state as a whole impose the treatment, the strategic considerations of a legislator, who represents only a small portion of the state, are washed out.

Another concern is that the results, while valid, are not limited to strategic interest groups. Perhaps all donors are behaving in the same way, which would change the interpretation of the results. To address this concern, Appendix Table 5 replicates the main analysis using ideological interest group donations.²⁶ Follow the Money provides sector codings that identify explicitly ideological groups. These groups are primarily those organized for a single issue, e.g., abortion, gun control, etc. No partisan effects of term limits are found using these contributions, and in fact the coefficient of interest – on the differential effect of term limits on Democratic donations – is slightly positive instead of negative. The partisan pattern appears to be limited to strategic donors, highlighting the shift in institutional power.²⁷

Finally, the analysis only estimates the *average* effect of term limits on partisan campaign donations. Term limits clearly have different effects across states. Substantive work has demonstrated tremendous heterogeneity in campaign finance across states, for example (Thompson and Moncrief 1998). Furthermore, the intensity of treatment differs across states; some have term limits that forbid legislators from ever running for the office again, while others only require that the legislator sit out a term.²⁸ Nevertheless, the average establishes that term limits have clear partisan effects. While one source of heterogeneity – the professionalization of state legislatures – will be considered in the next section, many others are left for future research. Although they may be of separate interest, their omission does not bias any of the estimated results presented herein.

Implications

Why do term limits have such strong partian effects on institutional power? It is not because they alter electoral fortunes.²⁹ In an earlier study, Powell (2008) shows that term limits have no effect on average Democratic vote share or on the Democratic share of the legislature. Taking advantage of the passage of time, I verify these null findings on a larger electoral dataset in Appendix Table 7. A likely alternate explanation is that seniority is valuable for interest groups. In this section, I provide support for this theory by showing that Democrats possess a pronounced seniority advantage, one which term limits remove.

An old and lasting literature in political science links seniority to expertise (e.g., Luce 1926; McConachie 1898). Goodwin (1959) offers some reasons for the advantage of seniority. Senior member are "experienced both in the subject matter of the committee on which they have served so long, and in legislative procedure" (419). Not only are senior members among the most high quality members, as evidenced by their ability to continue to be reelected, but they gain valuable experience in the legislature over time. Indeed, Robert Luce – himself a congressman – held that experience was "the most important factor" in "deciding merit" (Luce 1926: 9).³⁰ These ideas underpin more recent theories of information in the legislature

(e.g., Gilligan and Krehbiel 1987; Krehbiel 1991), which also hold that senior members are valuable for their policy expertise. The loss of its most expert members may therefore be a significant loss for both for the party and for its interest group supporters.

To investigate the loss of seniority, I use ICPSR dataset 34297 on state legislative elections, 1967-2010 (Klarner et al. 2013). Using names and legislative districts, I track winning candidates over time to measure their chamber seniority. Prior to the implementation of term limits, Democrats possessed a remarkable seniority advantage in many legislatures. Figure 2 plots the ratio of the number of Democrat and Republican legislators at each level of seniority, pooling over all observations in states that go on to have term limits, but do not have them in place yet. The graph shows a tremendous seniority advantage for the Democratic party. At higher levels of seniority, Democrats outnumber Republicans two to one. Term limits remove all of these extremely senior legislators.

[Figure 2 about here.]

In Table 4, I verify that term limits disproportionately remove senior Democrats by rerunning the diff-in-diff model from Equation 1 with legislator seniority, measured as the number of terms of service, as the outcome variable. In the second column, I add controls for contribution bans and public funding programs, to mirror the previous analysis. In both cases, term limits are seen to cause a marked decrease in seniority for both parties, not surprisingly. But the effect is almost twice as strong for the Democratic party. Term limits cause a decrease in the average Democratic seniority level of almost two terms of service.

[Table 4 about here.]

Although there are no doubt many factors that could contribute to the loss of Democratic power in the legislature, there are good reasons to believe that the loss of its most senior members affected the Democrats. Indeed, this seniority advantage was not lost on the Republican supporters of term limits, who knew that "there were far more Democratic than Republican incumbents" (Carey, Niemi, and Powell 1998: 276). There is another testable implication of the seniority link. If term limits affect partisan fortunes because seniority is valuable, then the partisan effect of term limits ought to be strongest in more professionalized legislatures. It is in these more professional legislatures that the political process becomes "institutionalized" (e.g., Berry, Berkman, and Schneiderman 2000). With full-time salaries and staff, senior committee members in these professionalized legislatures possess more power, power which is valuable to access-oriented interest groups. The overall effects of term limits no doubt differ across levels of professionalization for many other reasons; here, however, interest is on the *differential* effect of term limits across the two parties.

First, I calculate the difference-in-differences effect for each state that implements term limits within the sample. This does not include term-limited states that are always term limited in the dataset; there is no way to calculate a difference-in-differences estimate for these states.³¹ Specifically, I carry out a regression like Equation 1 separately for each "treated" state, including only it and all non-term-limited states, storing the estimated value for β_{dlim} as the state-specific effect of term limits on the difference between Democratic and Republican interest group campaign contributions.

Using the up-to-date measure from the Legislative Professionalization index (Squire 2012), each state is ranked in terms of its average legislative professionalization, with 1 being the least professionalized state and 50 being the most. Figure 3 plots the professionalization rank of each of the nine states with variance in the treatment against the diff-in-diff state-specific effects. As is apparent from the graph, the Democratic disadvantage from term limits appears most pronounced in more professional legislatures. The picture does not allow for any strong conclusions, but a correlation is evident.

[Figure 3 about here.]

We can also leverage the within-state variation in professionalization over time. To do so, I estimate equations of the form

$$Dem \ Money \ Share_{scit} = \beta_1 Limit_{sct} + \beta_2 Prof_{st} + \beta_3 Limit_{sct} \cdot Prof_{st} + \delta_s + \gamma_t + \epsilon_{sct}$$
(2)

where *Dem Money Share*_{sct} measures the Democratic share of all access-oriented interest group donations in state s, chamber c in election t and $Prof_{st}$ is the professionalization of state s at time t, standardized to have mean 0 and standard deviation 1, and all other variables are defined as before. I use the share of money instead of the log here to make the results easier to interpret by avoiding a triple-interaction; results are robust to the logged specification, however.

In this setup, term limits are estimated to cause a 7.7 percentage-point decrease (t = -1.86; state-clustered standard errors) in the Democratic share of contributions in a state with average professionalization (Prof = 0, corresponding to β_1 in the regression). The coefficient on the interaction term (β_3) is -1.3, indicating that term limits in a state with a professionalization index one standard deviation above the mean are expected to cause approximately a 9 percentage point decrease in the Democratic share of access-oriented interest group contributions. This effect is not statistically significant (t = -.045; state-clustered standard errors) but it is substantively meaningful. The lack of precision likely stems from the fact that the professionalization data is measured at a lower frequency than the election data.³²

In this section, I have focused on a key mechanism explaining term limits' effect on the partisan distribution of power in the legislature. When it loses its seniority advantage, the Democratic party loses legislative power, as measured by access-oriented campaign contributions. This is somewhat surprising because, at the same time, term limits do not affect the Democratic party's majority-party status or share of the legislature, on average. If seniority's value came purely through the access it provides to, for example, committees or

leadership positions, then term limits should have no effect on the distribution of power; the new Democratic replacement incumbents should, on average, continue to offer the same value to access-oriented donors. As such, the findings suggest that seniority *per se* has sway in the legislature (and thus has value to access-oriented groups) separate from its role as an institutional criterion for distributive politics.

In addition, I have shown that the effect of term limits appears largest in professionalized legislature, where the value of seniority is likely to be highest. This reinforces the notion that term limits' effect on the partisan distribution of power flows, at least in part, through its effect on the distribution of seniority. Taken together, the analyses in this section present strong, quasi-experimental evidence that parties possess more institutional power when their members have more experience in the legislature.

Conclusion

Many politicians and observers expected term limits to have partian consequences in line with the observation that Republicans, both politicians and their supporters, led the term limits movement. Although term limits have turned out to have no discernible partian effects on electoral outcomes, they have had major effects on the internal workings of legislatures. Term limits have caused a significant reduction in Democratic campaign contributions from interest groups relative to their Republican colleagues.

A plausible explanation for this effect is that Democrats tended to be more senior than Republicans before term limits. Seniority is a valuable characteristic in the legislature. Although the new legislators may be of the same partisan stripe, they cannot match the expertise and knowledge of their senior predecessors. As a result of this and other factors, they do not possess the same institutional power and therefore are not as valuable to strategic interest groups. All else equal, a party appears to lose a degree of institutional power even when it retains majority status if its seniority advantage weakens. What are the sources of seniority's power? Further research should investigate precisely which characteristics of more experienced members bring the most legislative power and thus provide the most benefit to access-oriented groups.

More broadly, the results highlight the inevitably partian nature of electoral reforms. In *Small Change*, Raymond La Raja describes this fundamental issue: "Reform of any kind tends to favor one party or the other because laws enhance or diminish the value of particular electoral resources relative to others" (5). The distribution of valence characteristics across the two parties – for example, the distribution of seniority across the two parties before term limits – help determine which of the parties will support what reforms. In considering the effects of any electoral reform, we need to consider both its effects on institutional features, and, separately, its partian effects. These effects may manifest themselves in the electoral arena, or, as is the case in the present analysis, they may appear only in the internal operations of the legislature.

	House		Senate	
State	Year	Limit	Year	Limit
ME	1996	8	1996	8
CA	1996	6	1998	8
CO	1998	8	1998	8
AR	1998	6	2000	8
MI	1998	6	2002	8
FL	2000	8	2000	8
OH	2000	8	2000	8
SD	2000	8	2000	8
MT	2000	8	2000	8
AZ	2000	8	2000	8
MO	2002	8	2002	8
OK	2004	12	2004	12
NE			2006	8
LA	2007	12	2007	12
NV	2010	12	2010	12

 $\label{eq:table_table_table_table} \textbf{Table 1} - \text{Term limits by state and chamber}.$

Year represents the year the limit first takes effect legally, and Limit gives the number of years state legislators are limited to. Table is reproduced from http://www.ncsl.org/Default.aspx?TabId=14844.



Figure 1 – Compares the change over time in the partisan differential in average interest group campaign donations (Democrat minus Republican) in term-limited states vs. non-term-limited states.

	Log Contributions	Log Contributions
Limit	0.0934	0.415
	(0.222)	(0.341)
Democrat	0.282^{*}	0.282^{*}
	(0.166)	(0.166)
$Democrat \cdot Limit$	-0.666***	-0.666***
	(0.234)	(0.234)
Ban		-0.678
		(0.456)
Public Funding		-2.941**
0		(1.427)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
N	1350	1350
# of States	44	44

 ${\bf Table} \ {\bf 2} - {\rm Term} \ {\rm Limits} \ {\rm and} \ {\rm Interest} \ {\rm Group} \ {\rm Campaign} \ {\rm Contributions}$

State-clustered standard errors in parentheses.

Analysis excludes AR, CA, LA, ME, NE, and SD. See foonotes 13 and 19. * p<0.10, ** p<0.05, *** p<0.01

	Dem Pres Vote Share (0-100)
Limit	-0.948
	(2.087)
State Fixed Effects Year Fixed Effects	Yes Yes
Ν	789
# of States	50

 ${\bf Table} \ {\bf 3}-{\rm Term} \ {\rm Limits} \ {\rm and} \ {\rm Republican} \ {\rm Sentiment}$

State-clustered standard errors in parentheses.

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

The Democratic Seniority Advantage Before Term-Limits



Figure 2 – Plots the ratio of the number Democrat and Republican legislators at each level of seniority, pooling over all term-limited state-years before term limits are implemented. Democrats are disproportionately represented at the higher seniority levels.



Figure 3 – State-specific estimates of β_{dlim} from Equation 1 across levels of professionalism, for states that *switch* to term limits within the sample (this excludes, for example, California). States are ranked from least to most professionalized (larger values mean more professional). The negative effects of term limits on Democratic interest group campaign donations appears stronger in more professionalized legislatures.

	Seniority	Seniority
Democrat	0.670***	0.669***
	(0.196)	(0.195)
Limit	-0.995***	-1.011***
	(0.210)	(0.221)
${\rm Democrat}\cdot{\rm Limit}$	-0.805***	-0.804***
	(0.220)	(0.220)
Ban		0.179
		(0.191)
Public Funding		0.0889
-		(0.196)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
N	240827	240827
# of States	49	49

 ${\bf Table} \ {\bf 4} - {\rm Term} \ {\rm Limits} \ {\rm and} \ {\rm Partisan} \ {\rm Seniority}$

State-clustered standard errors in parentheses. Excludes NE (non-partisan). * p < 0.10, ** p < 0.05, *** p < 0.01

Notes

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²http://www.dallasnews.com/news/politics/headlines/20130319-texas-senate-approvesterm-limits-for-state-executives.ece

³http://articles.chicagotribune.com/2013-03-26/news/ct-tl-niles-referendumpreview-20130328_1_electricity-aggregation-term-limits-new-electricity-supplier

⁴http://latimesblogs.latimes.com/lanow/2013/03/la-school-board-targets-garciawith-term-limits-vote.html

⁵For one overview of the term limits movement, see http://www.ncsl.org/legislatureselections/legisdata/legislative-term-limits-overview.aspx.

⁶While it is not the focus of their article, Apollonio and La Raja (2006) do find a persistent decrease in campaign contributions to Democrats after term limits in a study of four term-limited and four non-term-limited states.

⁷In the Appendix, I verify the non-result from Powell (2008) using updated data and a difference-in-differences design.

⁸Results for these estimates are available in the Appendix.

⁹The actual year ranges in the dataset differ by state. Ranges can be seen online at http://followthemoney.org/database/state_overview.phtml.

¹⁰I have confirmed this record-keeping strategy with Follow the Money in personal correspondence.

¹¹Specifically, the computer code drops any donations with the following sector codings: "Candidate Contributions", "Uncoded", "Party", "Public Subsidy", "Ideology/Single Issue", "Unitemized Contributions."

¹²Modest effects are found using individual donations. This is likely due to the fact that a significant portion of individual donors are also members of interest groups. While Follow the Money does code individual's interests, as well, these are likely to be less reliable (they are self reported). As a result, the safest strategy is to remove individual donations from the analysis, following previous literature.

¹³Though listed in the table for formality's sake, Nebraska and Louisiana are not used in the analysis due to their unique electoral setups.

¹⁴This is straightforward. Suppose an observation is from year 1998 and the state implemented term limits in 1996. The year of the observation is thus re-coded to the value 2.

¹⁵Each treated state is paired to the year-by-year mean of the control states over the same time period, before the re-centering is performed. Otherwise, incorrect control observations might be used, because not all states are treated in the same year.

¹⁶For two reasons, I focus on the "intention to treat" effect of term limits. First, the general equilibrium effects of term limits on the entire legislature are of primary interest, which this specification measures. Second, selecting on termed-out legislators would induce potentially severe selection bias. While the presence of term limits in the diff-in-diff may be exogenous, the choice of an individual legislator to stand for reelection in the shadow of term limits is highly strategic.

¹⁷In the Appendix Table 6, I show that relaxing this assumption by using state-specific linear time trends, as recommended in Angrist and Pischke (2009), does not affect the results.

¹⁸Code and estimated standard errors for these block bootstraps are available from the author upon request.

¹⁹In general, the inclusion of treated units that have no variance in the treatment variable will not affect estimation of the treatment effect in a fixed-effects or diff-in-diff framework. However, because partisanship varies within unit and year, the inclusion of always-treated units can affect estimation in unpredictable ways. The best solution to this problem is to get campaign donation data for earlier years, so that there are no term-limited states for which I have only post-term-limits data. Unfortunately this appears to be impossible. Therefore I omit states that are always term-limited in my dataset for this part of the analysis.

²⁰Under the diff-in-diff design, controls are not necessary for "as-if" random assignment, but can increase precision. See for example Angrist and Pischke (2009).

²¹An alternate specification is to run the diff-in-diff directly on the Democratic share of all interest group donations. This is a less desirable specification because it assumes parallel trends in the share variable instead of in the log of contributions. Nevertheless, it paints a similar story. In this alternate setup, term limits are estimated to cause a 7.8 percentagepoint decrease in the Democratic share of interest group donations.

²²One more feature deserves mention: the model does not include variables indicating majority party status, incumbency, or any other electoral features because the realizations of these variables occur subsequent to treatment assignment (i.e., the implementation of term limits).

²³Data on presidential vote share was provided by James Snyder and comes from Ansolabehere, Snyder, and Stewart (2001) as updated to current day by those authors. ²⁴The most notable commonality among term-limited states is the inclusion of ballot initiatives in their constitutions, a factor steeped in history and likely exogenous to the outcomes studied in this article.

²⁵See for example http://www.ncsl.org/legislatures-elections/legisdata/statewidevotes-on-term-limits.aspx.

²⁶I do not perform this "placebo test" with individual donations because many individuals donate on behalf of, or in coordination, with groups (for some discussion see Apollonio and La Raja 2006). When I replicate the main analysis with individual contributions, I continue to find a negative effect for term limits on the partian differential. However, the effect (-.4) is quite a bit smaller than for interest groups (-.66).

²⁷For reasons explained earlier, testing for the effect on individual donors is biased because many individuals are also members of interest groups in the Follow the Money data.

²⁸For details on how term limits vary by state, see http://www.ncsl.org/legislatureselections/legisdata/chart-of-term-limits-states.aspx.

²⁹It may seem surprising that term limits can hurt the Democratic party monetarily but not electorally, if money has any link to electoral outcomes. Recall first that I am only measuring the effect of term limits on *interest group* donations, not all donations. More importantly, it is likely that the candidates/districts that are losing contributions are safe districts for Democrats. Districts with very senior Democrats – those who will be termed out – may be especially safe districts.

 $^{30}\mathrm{This}$ quote is featured in the analysis of Goodwin (1959) as well.

³¹One could make a pooled comparison across states based on professionalism, but such a test would be probably be biased due to the unobserved heterogeneity across states, which the diff-in-diff addresses.

 32 I "fill in" the professionalization data for each year in between its actual measurement, to ensure that I am able to merge it with the contribution data. This reduces variance in the professionalization variable and thus reduces precision.

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Appendix

Placebo Test

To verify that the observed change in interest group donations is indeed strategic in nature, I replicate the main analysis using *ideological* interest group donations instead of strategic interest group donations as the outcome variable. To calculate this outcome variable, I take donations only from interest groups that Follow the Money has coded as being in the "Ideological" sector. As Table 5 shows, there is no analogous result using these less strategic donors (indeed, the coefficient goes from negative, using strategic donors, to positive, but it is extremely small and indistinguishable from zero).

	(1)	(2)
	Log Contributions	Log Contributions
Limit	-0.212	-0.0345
	(0.373)	(0.362)
D	0.105	0.100
Democrat	0.127	0.129
	(0.135)	(0.136)
Democrat · Limit	0.0374	0.0393
	(0.236)	(0.237)
Ban		-0 563*
Dan		(0.220)
		(0.330)
Public Funding		-1.505***
		(0.263)
State Fixed Effects	Voc	$\mathbf{V}_{0\mathbf{C}}$
State Fixed Effects	res	res
Year Fixed Effects	Yes	Yes
Ν	1304	1304
# of States	44	44

 Table 5 – Term Limits and Interest Group Campaign Contributions

State-clustered standard errors in parentheses

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

Relaxing the Diff-in-Diff Assumption Parametrically

The identifying assumption of diff-in-diff is that the non-term-limited states provide an accurate counterfactual for the trend in Democrat and Republican donations in term-limited states. While this is a plausible assumption in many settings, we can also try to relax it by including state-specific linear time trends. If the addition of these trends does not affect the estimated effect, we can take this as supporting evidence that term-limited states do not appear to have different trend behaviors than non-term-limited states (e.g., Angrist and Pischke 2009).

	Log Contributions (\$)	Log Contributions (\$)
Limit	-0.056	0.250
	(0.183)	(0.280)
Democrat	0.282	0.282
	(0.168)	(0.168)
$Democrat \cdot Limit$	-0.666***	-0.666***
	(0.238)	(0.238)
Ban		-0.161
		(0.452)
Public Funding		-2.400*
0		(1.268)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Specific Time Trends	Yes	Yes
N	1350	1350
# of States	44	44

Table 6 – Term Limits and Interest Group Campaign Contributions

State-clustered standard errors in parentheses.

Analysis excludes AR, CA, LA, ME, NE, and SD. See footnotes 13 and 19. * p < 0.10, ** p < 0.05, *** p < 0.01

The Non-Effects of Term Limits on Electoral Outcomes

To verify that term limits have little to no effect on electoral outcomes, I re-run the diff-in-diff with a variety of electoral variables as the outcome. In the first column, I use Democratic vote share from Klarner et al. (2013). In the second two columns, I use the Democratic share of the lower and upper houses, respectively. The information for this variable comes from Dubin (2007), who compiled the information from primary sources, as coded and extended to subsequent years by Folke, Hirano, and Snyder (2011). In all three cases, point estimates are small, in conflicting directions, and not statistically significant.

	Dem Vote Share	Dem Seat Share (Lower)	Dem Seat Share (Upper)
Limit	$1.148 \\ (3.123)$	-2.261 (5.154)	-3.272 (5.869)
State Fixed Effects Year Fixed Effects	Yes Yes	Yes Yes	Yes Yes
N # of States	$\begin{array}{c} 101716\\ 49 \end{array}$	$\frac{1446}{49}$	$1355\\49$

Table 7 – Term Limits and Electoral Outcomes

State-clustered standard errors in parentheses. Outcomes range from 0 to 100. Excludes NE (non-partisan). * p < 0.10, ** p < 0.05, *** p < 0.01