

# *A Return to Normalcy? Revisiting the Effects of Term Limits on Competitiveness and Spending in California Assembly Elections*

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## **ABSTRACT**

Term limits advocates argued that their reform would make state legislative campaigns more competitive and less expensive, and limited early studies suggested that it may have achieved those goals. But now, with evidence from more than a decade of experience with reform, we re-examine the effects of terms limits on electoral competitiveness and campaign spending in California Assembly elections. We find that while term limits initially suppressed campaign spending, they did not check its growth for long. Today, California's state legislative elections are as expensive in real dollars as they have ever been. In terms of electoral competitiveness, state legislative incumbents are in no more danger of losing their seats today than they were in the pre-term limits days of the late 1980s. Furthermore, open-seat races are not any more competitive under term limits than before them; however, we do find a modest, but significant, decline in incumbents' average winning margin since the imposition of term limits. But since term limits have made fewer incumbents eligible to run for office, this incumbency advantage helps fewer people than it once did. Yet, for the most part, rather than being supplanted by citizen-legislators, career politicians have simply adapted to the constraints imposed by term limits.

TERM LIMITS WERE IMPOSED on many state legislatures in the anti-incumbent frenzy of the early 1990s with the promise to "restore citizen control of government" (U.S. Term Limits 2006). "It's time to drain the swamp," declared term limits advocate John Van de Kamp, then California's attorney general (Price and Bacciocco 1990). Paul Weyrich of the Free Congress Foundation predicted, "Yes, we'll lose some good people, but that will be more than offset by the advantage of fresh thinking" (Taylor 1990, A18). According to U.S. Term Limits president Stacie Rumenap, "With new people in office, you have people with real world experience. Under term limits, you might have a schoolteacher sitting on the education committee" (Smith, Greenblatt, and Buntin 2005, 209). Term limits advocates promised no less than the

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restoration of competitive elections, an infusion of energy and enthusiasm in public life, and the resurrection of the citizen-politician model of public service (Price and Bacciocco 1990).

Yet, just 15 years after the adoption of term limits in many states, would-be reformers are once again bemoaning entrenched politicians and the influence of special interests. Even in California, which has one of the strictest term limits statutes in the country, the governor is calling for reforms to drive professional politicians from the state legislature.<sup>1</sup> So, what is the legacy of the term limits movement? In this article, we consider one aspect of this legacy, term limits' effects on elections. In particular, we ask: Have term limits made state legislative elections more competitive and reduced campaign spending, as reformers had hoped?

While 16 states adopted term limits between 1990 and 2000, we test reformers' predictions using data only from California for several reasons. California has the most populous electoral districts of any state to have adopted term limits (California Senate districts are larger than United States House of Representatives districts), and its legislature has been the highest paid and most professionalized in the nation, both before and during term limits.<sup>2</sup> Prior to term limits, California's Assembly and Senate were characterized by careerism and professionalism. Furthermore, before term limits, California legislators spent large sums, often raised from interest groups, on their campaigns. In short, the California Legislature was exactly the sort of body that term limits advocates hoped to reform.

Initial studies of term limits' effects were encouraging for reformers, showing some elections to be less expensive and more competitive (Daniel and Lott 1997; Carey, Niemi, and Powell 2000; Van Vechten 2001; Ensley and Tofias 2002). But have these effects been sustained over time? We have now seen more than a decade of elections under term limits in some states, providing a much more secure vantage point from which to judge the reform's effectiveness. Our findings suggest that while term limits may initially have caused an increase in electoral competitiveness and a decline in campaign spending, those changes were largely ephemeral. California politics today is marked by the same low competitiveness and high spending that defined it prior to the passage of term limits.

Of course, term limits have had some effects on state legislative elections. Almost by definition, there are more open seats each election cycle than there were prior to term limits, and open seat caucuses remain more competitive than re-election races in California. We find that even if incumbents rarely lose their seats, they win them by less comfortable margins than they once did. However, our findings suggest that the early enthusiasm for term limits'

effects was premature. In short, the reform was a temporary shock to the system, one to which professional politicians and parties have been able to adapt.

#### TERM LIMITS: EXPECTATIONS AND EFFECTS

Term limits advocates promised that their reform would make elections both more competitive and less expensive, throwing entrenched politicians out on the streets and allowing a more diverse cast of citizen-legislators to serve (Price and Bacciocco 1990; Taylor 1990). Have term limits actually met these lofty goals? The evidence to date has been mixed. Some scholars have found that term limits, by creating more open-seat races, have helped women and racial minorities win office more frequently (Thompson and Moncrief 1993; Caress 2001), and Bratton and Haynie (2001) have shown that women and minority legislators have a better chance of being selected for leadership positions in term-limits states. The good news for women is far from conclusive, however. Carey, Niemi, and Powell (2000) find that women were already being elected in higher numbers in term-limits states prior to the reform, while Carroll and Jenkins (2001) maintain that female candidates actually underperform in elections in term-limits states.

Still other evidence has cast doubt on whether term limits have had any effect on the composition of state legislatures (Carey, Niemi, and Powell 1998). But even if the same types of people are winning elections, they may be behaving differently in office after term limits. For example, Carey, Niemi, and Powell (1998) found that term-limited legislators were less interested in procuring special projects for their districts and more interested in asserting their consciences in the face of constituent sentiment. More generally, it appears that term limits may have made legislators less interested in serving their constituents (Niemi, Carey, Moncrief, and Powell 2003).

Turning to elections, there has been some evidence that term limits have made politics more competitive. For example, term limits have forced more career politicians to run for higher office (Ensley and Tofias 2002), resulting in more crowded primaries for those offices (Van Vechten 2001). In addition, term limits might make state legislators' seats appear less secure, encouraging the opposition party to contest races they would not have otherwise (Carey, Niemi, and Powell 2000).

One of the most encouraging pieces of research for term limits reformers on their effects on elections was Daniel and Lott's 1997 study. They found that, as a result of term limits, California statehouse elections had become more competitive and that less money was being spent in campaigns for

them. This was exactly the result that reformers predicted, and the study was widely cited as justifying the reform (Besley and Case 2003; López 2003; Smart and Sturm 2004). But the data used by Daniel and Lott only ran through the 1994 election, just two elections into the term limits era and two years before even the first legislator was forced from office by the limits. But now, years after term limits were actually implemented in California and several other states, we are in a much better position to assess the effects of this reform.

## METHODOLOGY

The methods and data we use to assess the effects of term limits are similar, but not identical, to those used by Daniel and Lott (1997). One difference is that those authors measured electoral competitiveness with a logit regression model of the likelihood that an incumbent would be defeated. We use ordinary least squares (OLS) regression to estimate the incumbency advantage, as explained in the following section. We use this approach because the probability of almost any incumbent's defeat is very low; we were more curious about shrinking victory margins, which may induce greater responsiveness among elected officials even if very few of them actually lose their seats. Otherwise, the two methods are substantively similar. A more significant difference is that Daniel and Lott pool all assembly and senate results for primary and general contests, whereas we analyze only assembly general elections. While more cases from the same population are always desirable, primary and general elections may be affected very differently by term limits, and if so, pooling them would be inappropriate. Furthermore, Daniel and Lott may have had to pool across chambers and election types simply to get enough cases to analyze, given that they only had two election cycles of term-limits data. There have now been seven election cycles under term limits, making it possible to calculate reliable estimates of the incumbency advantage for the more homogeneous set of assembly general elections alone.<sup>3</sup>

Many studies of legislative elections employ normal-vote calculations to account for year-to-year shifts in the parties' fortunes (Carter and Schap 1990; Ansolabehere and Snyder 2002). Following Levitt and Wolfham (1997), we estimate a fixed-effects regression model that controls for variations in district competitiveness and demographic profiles, as well as temporal shifts in party performance. The fixed-effects model is consistent with the notion of a normal vote, but it avoids the difficult measurement issues associated with normal-vote models.

## ELECTORAL COMPETITIVENESS UNDER TERM LIMITS

While term limits may have increased electoral competitiveness in California, it is not obvious how such increased competitiveness would manifest itself. It may be that:

Hypothesis 1: Incumbents lose their re-election bids more often.

Hypothesis 2: Margins of victory in all races are smaller.

Hypothesis 3: Margins of victory for incumbents are smaller relative to those of non-incumbents.

Hypothesis 4: Neither open-seat nor incumbent races are any more competitive under term limits. However, since incumbents enjoy larger victory margins than non-incumbents and since term limits have created more open-seat races by periodically turning out incumbents, elections appear more competitive because more of them are for open-seat races.

First, a simple before-and-after comparison of the data from California Assembly elections does not support Hypothesis 1. As we can see in the top row of Table 1, incumbents actually lose at a considerably lower rate under term limits than they did before term limits. This conclusion stands in clear contrast to Daniel and Lott's (1997) finding that more incumbents lost their re-election bids after term limits were adopted. Of course, Daniel and Lott's data ended with the 1994 election. By extending the years of analysis to 2004, we see that while there was a decrease in incumbent job security under term limits, it was localized in the early 1990s, concurrent with, but probably not caused by, term limits. Once term limits were implemented, incumbents actually became safer in their seats.

The second row of Table 1 shows that these data do not support Hypothesis 2 either; while victory margins in all assembly races did decline by 5.5 points on average in the early 1990s, they returned to roughly their original levels once term limits had been implemented. Thus, both the incumbent

*Table 1.* Indicators of District Competitiveness and Extremism in Three Eras in the California Assembly

	1976–90	1992–94	1996–2004
Percentage of incumbents who lost re-election	3.7%	7.2%	1.2%
Average victory margin for all races	38.1	32.6	37.7
Average victory margins for contested, open-seat races	22.1	22.4	29.2
Ideological moderation, rank (out of 80) <sup>a</sup>	37th	39th	42nd

a. Districts were ranked in terms of their distance from the median district in the Democratic vote for governor in the most recent gubernatorial election. Number one would thus be the median district--the most moderate in terms of the gubernatorial vote. Eighty would be the most ideologically extreme district in the state.

loss and the victory margin figures suggest only a short-term increase in electoral competitiveness in the early 1990s, perhaps simply a by-product of the anti-incumbent mood of the time. But once term limits were actually in effect, incumbents became as safe, or even safer, than they were previously.

Conversely, when we look at the change in competitiveness for only open seats (Table 1, row three), the trend is reversed. Since the imposition of term limits, elections for open, contested assembly seats have been won by roughly seven percentage points more than they were previously. Why would term limits have made open-seat races less competitive? Perhaps because a wider variety of districts began to have open-seat races regularly after term limits went into effect. For example, Democratic Assemblyman Willie Brown's San Francisco district would be very safe for any Democrat, but it never saw an open-seat race during Brown's three decades in office. Under term limits, such safe seats come open nearly as often as those from more marginal districts. We can test whether this effect is responsible for the decline in competitiveness by looking at the ideological extremity of the districts, as measured by their distance from the median district in the Democratic vote share in quadrennial gubernatorial elections.<sup>4</sup> We expect that more ideologically extreme districts will tend to be contested less frequently and less effectively by the out party. As the bottom row of Table 1 shows, prior to term limits, the average open-seat district was the 37th (out of 80) most moderate ideologically, but that ranking dropped to 39<sup>th</sup> in the early 1990s and 42<sup>nd</sup> once term limits were implemented. Thus, at least some of the decrease in open-seat competitiveness can be explained by the addition of these safe districts to the pool of open seat races. In fact, controlling for district extremism there is no decline in open seat competitiveness.

As a result, if all races have become more competitive on average but open-seat races are no more competitive, then incumbent races must be the source of these declining victory margins, as Hypothesis 3 suggests. To test this, we examined changes in the incumbency advantage, defined as the difference in the average victory margins of incumbent races and open-seat races. To test whether the adoption of term limits in California did, indeed, depress the incumbency advantage, we compared the incumbent advantages from the pre-term-limits era (1976–90) and the term-limits era (1992–2004). To provide an appropriate comparison between these periods, we excluded uncontested races<sup>5</sup> and those races that included incumbents who had served more than three terms.<sup>6</sup>

Any effective method of estimating incumbency advantage must account for district- and election-specific lopsidedness in the vote. Following Levitt and Wolfram (1997), we use a fixed-effects model to separate these district-

and election-specific effects on vote margins from the effects of term limits. In particular, our equations include district-by-districting plan effects. That is, we estimate separate effects for each district under each decennial districting plan. We also include fixed effects for each election year. The dependent variable in the regression model is the *Democratic vote share*. The *incumbent* variable is coded as 1 if the incumbent is a Democrat, -1 if a Republican, and 0 if there is no incumbent in the race. This way, all outcomes are coded in the Democratic direction. We also include a variable for the party of the incumbent (*Democratic incumbent*), allowing us to control for any inter-party differences in incumbency advantage. In none of our regression specifications do we find a statistically significant inter-party difference in incumbency advantage.

By including fixed effects for each district under each districting plan, our results are not confounded by differences in the districts across time.<sup>7</sup> Instead, our estimates of incumbency advantage reveal the average amount by which an incumbent would be expected to out-poll a candidate of the same party, in the same district, in the same election, in an open-seat contest. In the results presented here, we omit uncontested races, but in a separate analysis (not presented), we find a generally similar pattern of change in the incumbency advantage over time, even with uncontested races included in the dataset, although, as one would expect, all of the estimated advantages are greater than those estimated without the uncontested races.

In Table 2, we see a modest, but clear, difference in the incumbency advantage between the two eras. The incumbency advantage declined by 41 percent with the advent of term limits, from 5.4 percentage points in 1976–90

Table 2. Effect of Incumbency on Vote Margin in California Assembly Races, 1976–2004—Fixed-Effects Model

	1976–1980	1982–1990	1976–1990	1992–2004
Incumbent	.068** (.019)	.044** (.013)	.054** (.011)	.032** (.005)
Democratic incumbent	-.001 (.028)	.029 (.022)	.016 (.017)	.005 (.008)
Constant	.505** (.015)	.528** (.016)	.516** (.011)	.507** (.004)
Adjusted R-squared	.483	.594	.453	.316
N	159	164	323	473

\*p<.05; \*\*p<.01

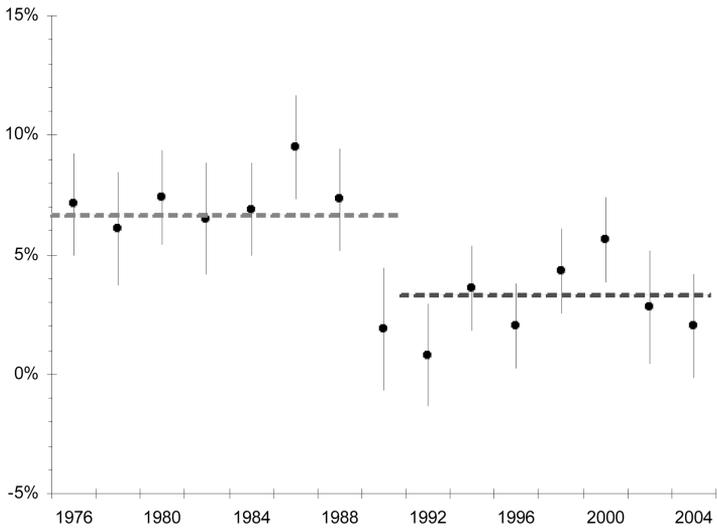
Note: The dependent variable is the Democratic share of the assembly race vote. Races that were uncontested or that involved a member with more than three terms of experience are excluded. Cell entries are ordinary least squares coefficients, with standard errors appearing in parentheses. Fixed-effects coefficients (districts by decade and year) and year dummy coefficients are not shown.

to 3.2 in 1992–2004. Thus, all else being equal, it seems that incumbents were substantially less secure in their positions under term limits.

To better understand the impact of term limits on the incumbency advantage, we re-estimated our model with a term for an interaction between incumbency and each election year. In this way, we were able to estimate the size of the incumbency advantage in each election while controlling for partisan tides and district composition. The results of this analysis are given in Table A1 in the Appendix. Figure 1 plots the estimated incumbency advantage in each year from 1976 to 2004. The vertical bars in the graph show 95 percent confidence intervals for each estimate. The two horizontal lines show the average estimated effect for 1976–90 and 1992–2004, respectively.

Figure 1 shows that the incumbency advantage was relatively large during the 1980s, often around 7 percent. However, the advantage plummeted by decade’s end, and by 1990 and 1992, it was statistically indistinguishable from zero. The incumbency advantage rebounded in the late 1990s, and by 2000, it was close to the size of the incumbency advantage of the 1980s. But

Figure 1. Incumbency Advantage in California Assembly Elections, 1976–2004



Note: Each point on the graph is the estimated electoral value of incumbency in a given year. The vertical lines are the 95 percent confidence intervals for each estimate. These estimates were derived from the regression model in Table A1. The dotted horizontal lines are the average estimated incumbency advantage for the 1976-90 (pre-term limits) and 1992-2004 (term limits) periods.

this upward trend was forestalled after 2000, and the incumbency advantage has remained relatively low to date. It is difficult to conclude from these data whether term limits caused only a short-lived reduction in incumbency advantage or whether the passage of campaign finance reforms in California in 2000 caused a resurgence of the incumbency advantage by limiting incumbent resources. We consider the role of campaign finance reform below.

The figures in Figure 1 and Table 1A require further explanation since the steep drop in incumbency advantage seems to have occurred in 1990, even though term limits were not even imposed until 1992. This very low figure in 1990 suggests that this drying up of incumbent advantage in these races may be attributed not to term limits, but the general anti-incumbent movement in American politics of the period, fueled by the Congressional banking scandal and other undesirable incumbent behavior. Although the vast majority of congressional incumbents retained their seats in 1990, 16 House members and one senator lost re-election, and many who won did so by reduced margins (Anonymous 1990). In California 1990 indeed saw the passage of Proposition 140, adopting state legislative term limits. As a result, a reduced incumbency advantage in 1990 is not surprising; however, what remains clear is that the incumbency advantage is significantly lower under term limits than it was previously, and this decline coincided with the adoption of term limits. Whether it was actually caused by term limits or whether both term limits and the reduced incumbency advantage were simply caused by the same catalyst—the anti-incumbent mood of the 1990s—remains an open question.

Hypothesis 4 suggests that term limits increased competitiveness simply by turning more incumbents out of office. It seems quite clear that this straightforward effect of term limits has occurred. There were an average of 11.6 open assembly seats from 1976 to 1992. Since the adoption of term limits, an average of 27.3 of the assembly's 80 seats have been open in each election, and that figure has not fallen below 23.0 in any year since 1992. In other words, roughly a third of assembly seats have been regularly open in elections under term limits. Since non-incumbents almost invariably average lower victory margins than incumbents, this trend has obviously caused some increase in perceived competitiveness.

In summary, the electoral data in the California Assembly races suggest that term limits have not led to more incumbent defeats, but only that there has been a modest increase in overall competitiveness as measured by vote margins. This small increase in competitiveness stems from two sources. One is the mechanical effect of simply turning out more incumbents. This effect could exist without any of the changes in behavior or motivation of

office-seekers posited by term limits advocates. But we have also seen that the vote margins of incumbents declined after the imposition of term limits, suggesting that either incumbents are putting less effort into pursuing re-election or their resource advantage over non-incumbents has narrowed as the result of term limits. Either scenario is consistent with a modest reduction in the electoral value of holding office, an important objective of term limits reformers. Interestingly, the greatest reduction in incumbency advantage was observed at the precise moment that term limits were adopted (1990), and there is some evidence that the advantage rebounded in the late 1990s, just as limits were actually implemented. This result suggests that the decline in incumbency advantage that has been correlated with the imposition of term limits in California may prove transitory.

#### TERM LIMITS AND SPENDING IN CALIFORNIA ASSEMBLY ELECTIONS

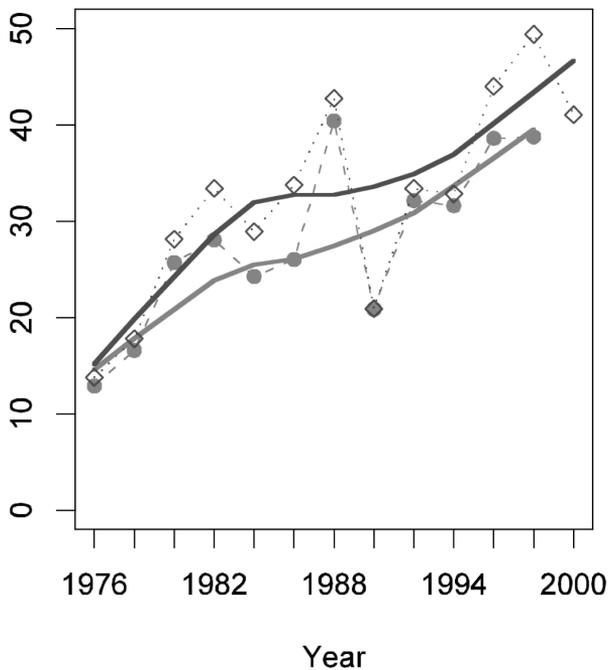
Next, we test the prediction that term limits would reduce spending on campaigns. Term limits advocates claimed that the reform would help reduce spending in elections through two mechanisms, one indirect and one direct. The indirect mechanism was based on the assumption that term limits would remove entrenched officeholders who stood in the way of serious campaign spending limitations; thus, with term limits in place, other reforms could proceed apace. The direct mechanism was based on the fact that it costs a terrific sum even to challenge an incumbent credibly; by having fewer incumbents running for re-election, seats could be contested and won for far less money (Anonymous 1991). Underlying this direct mechanism is the assumption that term-limited seats have a lower electoral value, reducing the amount that prospective candidates would be willing to spend to win them (Squire 1992). Thus, even without the passage of campaign finance reform, campaigns would become less expensive under term limits.

Unlike in the previous section, the data to test the campaign spending hypothesis end with the 2000 election. The enactment of campaign finance reforms in California makes the inclusion of more recent data impossible. Proposition 34, passed by California's voters in 2000, capped the amount any individual or non-party committee could donate to a state legislative candidate and limited transfers from one candidate committee to another. (Indeed, as noted above, these restrictions may be partially responsible for the decline in incumbent re-election margins in 2002 and 2004.) Therefore, the financial data from post-2000 elections and those from prior elections are not comparable and cannot be pooled.

Daniel and Lott (1997) found that California's legislative campaigns had become less expensive after term limits were adopted but before they were implemented. But a review of campaign spending over a longer period suggests that their conclusions were premature. Figure 2 shows the trend in total spending by all assembly candidates in each election in real terms.<sup>8</sup> The upper line shows the trend in gross expenditures and the lower line shows the trend in net expenditures. Net expenditures do not include transfers from one assembly campaign to another, which would double-count contributions for our purposes.

These trend lines offer no suggestion that term limits affected campaign spending in California Assembly races. Aside from a large upward swing in 1988 and then downward in 1990, the growth in real campaign spending has been consistent over time. Of course, these summary totals could be obscuring real, but countervailing, effects of term limits. For example, on

Figure 2. Total Spending in California Assembly General Elections, 1976–2000



Note: The top solid line shows the trend in total gross spending over the period. The bottom solid line shows the trend in total spending net of transfers between assembly candidates. The solid circles show the actual net spending in each year. The open diamonds show the actual gross spending in each year. All amounts are in 2000 dollars.

one hand, the reformers' hypothesis that term limits would reduce spending by reducing the value of these seats may indeed be correct. On the other hand, if term limits also caused elections to become more competitive, as reformers hoped, then this increased competition might lead to increased campaign spending (Coyte and Landon 1989).

To sort out these potential effects, we estimate regression models for total spending in each assembly race from 1976 to 2000, following Daniel and Lott's model closely. These model estimates are reported in Table 3. We find only mixed evidence to support Daniel and Lott's (1997, 182) claim that term limits "dramatically reduced campaign expenditures."

If no account for the general growth in spending over the entire period is made (Table 3, Model 1), then this model makes it appear as if term limits

Table 3. Regression Analysis of Real Total Expenditures in California Assembly General Elections, 1976-2004

Variable	Model		
	(1)	(2)	(3)
Term limits	150,152** (32,530)	148,490** (58,996)	74,102 (67,454)
Year	—	22,953** (3,810)	18,814** (4,221)
1988	—	—	149,581* (66,189)
Toss up	743,786** (56,800)	782,763** (56,209)	782,859** (56,095)
Leans	448,392** (45,949)	464,643** (45,249)	468,865** (45,196)
Leader	147,023 (127,476)	153,355 (125,315)	146,155 (125,102)
Majority party leader	753,417** (56,800)	737,572** (148,255)	740,885** (147,962)
Power committee chair	194,109** (60,004)	171,094** (59,109)	170,522** (58,989)
Constant	78,020 (70,661)	45,400,000** (7,556,856)	37,200,000** (8,371,202)
R-squared	0.31	0.33	0.34
N	1,040	1,040	1,040

\*p<.05;\*\*p<.01

Note: The dependent variable is real total spending in each assembly race. Also included in the regression, but not shown in the table, are controls for whether the race was for an open seat, the stated occupations of the candidates, whether the race was in a presidential election year, and whether the race involved a committee vice chair. The only occupations that were related to greater spending to a statistically significant degree were Democratic legislative staffers and teachers/union organizers. In most specifications, these occupations are associated with approximately 200 to 250 thousand dollars more spending. In some specifications, uncontested seats have significantly less spending than safe, but contested, districts. OLS estimated coefficients are reported, with standard errors appearing in parentheses.

have increased campaign spending by about \$100,000 to \$200,000 per race, controlling for electoral competitiveness, for whether the incumbent held an important leadership or committee position, and the occupation of the challengers (or of both candidates in an open-seat race). To the contrary, if we allow for a linear increase in spending over time (Table 3, Model 2), term limits are estimated to have suppressed spending by \$30,000 to \$280,000 per contest. Therefore, even if spending has increased since the adoption of term limits, the reform may have stemmed the overall rate of increase.

We also estimate that spending in these races increased at a rate of about \$15,000 to \$31,000 per year, controlling for these other factors.<sup>9</sup> In addition, this analysis reveals that campaign spending was not evenly distributed across these races. For example, races rated as “toss ups” by the *California Journal* had average campaign spending that was \$620,000 to \$860,000 greater than that in more lop-sided races. When the majority party leader sought re-election, expenditures averaged about \$750,000 greater than when a rank-and-file member sought re-election.<sup>10</sup> While toss-up races had greater spending because the parties directed resources and effort toward those races, the extra spending in races involving a majority leader was caused by a different process. The majority leader’s spending reflects his or her efforts to help other candidates by transferring money to their campaigns. The same process may also have been at work when a race involved the chair of a powerful committee, where spending is estimated to have been \$50,000 to \$290,000 greater. Thus, by comparison, the estimated effect of term limits on campaign spending in Model 2 is substantial, almost the size of the estimated effect of having a powerful committee chair in the race.

The large year-to-year fluctuations in aggregate campaign spending may account for this apparent impact of term limits on spending. In particular, the result largely pivots on the 1988 election. As Figure 2 shows, 1988 is something of an outlier in this dataset, with spending in that year being much greater than in the several elections that preceded or followed it. When we control for the unusually high spending in 1988 (Table 3, Model 3), the apparent effect of term limits is more than halved, and it is no longer statistically distinguishable from zero. Of course, 1990 seems to be something of an outlier on the lower spending side, and if a control dummy is included for the 1990 election, the size of the apparent term-limits effect grows (not shown), although it remains smaller than when no account is taken of either 1988 or 1990.

## A CAVEAT: REDISTRICTING

As noted above, decennial redistricting potentially complicates the interpretation of our analyses. Redrawing legislative districts could affect both incumbent re-election margins and campaign spending, although the expected magnitude, and even the direction, of such effects are not obvious. For example, the 1982 redistricting was a highly partisan process, designed to increase the number of state legislative districts winnable by Democrats (Basehart and Comer 1991). But it is not clear how such a partisan redistricting would affect competitiveness and spending overall. For example, some marginal majority-party-held districts and safe minority-party-held districts might be expected to become less competitive under a partisan plan, while safe majority party districts and marginal minority party districts would be expected to become more competitive. It might also be the case that when districts are drawn to protect incumbents, such as in the 2002 California state legislative plan (Plendl 2002), the incumbent advantage increases. On the other hand, since redistricting alters constituencies, it may rob incumbents of one of the chief campaign benefits of holding office: name recognition (Ansolabehere, Stewart, and Snyder 2000). The absence of this benefit would cause the incumbent advantage to drop somewhat in the subsequent election.

The problem for a test of the electoral impacts of term limits is that their adoption in California in 1990 practically coincides with the 1992 redistricting. And the likely electoral effects of the 1992 redistricting plan, which was drawn by a panel of judges rather than the legislature, are not obvious. While there is no particular reason to predict that the races in these districts would be particularly competitive or uncompetitive, it remains at least possible that some of our results maybe attributable to this contemporaneous redistricting rather than to term limits. Because our measure of incumbency advantage directly accounts for redistricting, redistricting cannot explain the variations in incumbency advantage that we find. Moreover, the abrupt shifts in campaign spending, electoral competitiveness, and incumbency advantage in the early 1990s and their rebounding later in the decade differ from the patterns observed subsequent to the redistrictings in the 1970s and 1980s. This suggests that it is term limits, which were unique to the 1990s, and not redistricting, that account for our results.

## CONCLUSION

To date, the evidence of the electoral effects of California's experiment with term limits has been mixed. The incumbency advantage—the bonus that

re-election-seekers receive simply by holding office—decreased in assembly elections around the time that term limits became law, suggesting that the reform had, indeed, suppressed the value of incumbency. In addition, term limits apparently reduced campaign spending in these elections, perhaps because fewer contributors were interested in currying favor with such short-term incumbents. Under term limits, officeholders appear to be less secure and to have to fight harder to keep their jobs. These are effects that term limits advocates would likely applaud (Price and Bacciocco 1990).

Yet each of these findings has important caveats. While incumbents were involved in closer races, the rate of incumbent defeat actually dropped slightly in California Assembly races once term limits were adopted. Potentially even more damaging to the argument that term limits had the desired effects, we find that some of these effects actually appear to have occurred prior to their supposed cause, the enactment of term limits. Perhaps it was apparent to members of the state legislature and their challengers in 1990 that term limits would soon be law, and they began acting accordingly. But we would be much more confident in any conclusion about term limits impacts had the effects we find followed the presumptive cause.

Perhaps our most important and well-supported conclusion is that the initial changes detected by Daniel and Lott (1997) in the California Legislature—less incumbency advantage and decreased campaign spending—appear to have been short-lived, at best. By the mid-1990s, spending and incumbency advantage were on the rise again, and in the 2000 races, spending was as high as it had been at its peak in the late 1980s, even in constant dollars. Daniel and Lott's (1997) analysis may simply have come too early for a fair assessment of term limits' effects, probably capturing only the anticipatory effects caused by their adoption, rather than the effects (if any) of their actual implementation.

In the end, it appears that the hopes of term limits advocates, like those of the advocates of so many other political reforms, have been, to a large extent, overcome by ambitious and creative politicians. Although California's term limits experiment is far from complete, it appears that the system may have found a new equilibrium in which incumbents once again have access to the resources they need to protect their jobs—if only for a more limited time.

## APPENDIX

*Table A1.* The Effect of Incumbency on Vote Shares—Fixed-Effects Model with Year and Incumbency Interactions

Variable	Coefficient
Incumbent × 1976	.071** (.011)
Incumbent × 1978	.061** (.012)
Incumbent × 1980	.074** (.010)
Incumbent × 1982	.065** (.012)
Incumbent × 1984	.069** (.010)
Incumbent × 1986	.095** (.011)
Incumbent × 1988	.073** (.011)
Incumbent × 1990	.019 (.013)
Incumbent × 1992	.008 (.011)
Incumbent × 1994	.036** (.009)
Incumbent × 1996	.020* (.009)
Incumbent × 1998	.043** (.009)
Incumbent × 2000	.056** (.009)
Incumbent × 2002	.028* (.012)
Incumbent × 2004	.020 (.011)
Constant	.512** (.004)
R-squared	.425
N	880

\*p<.05; \*\*p<.01

*Note:* The dependent variable is the Democratic share of the assembly race vote. Races that were uncontested or that involved a member with more than three terms of experience were excluded. Cell entries are OLS coefficients, with standard errors in parentheses. Fixed-effects coefficients (districts by decade and year) and the coefficient for the variable *Democratic incumbent* are not shown.

## ENDNOTES

1. In a flurry of ballot initiatives in a 2005 special election, California's Governor Arnold Schwarzenegger pushed for redistricting reform (to remove, or at least threaten, incumbents in safe districts) and merit pay for school teachers (to disempower teachers' unions and the legislators who benefit from their largesse) (Anonymous 2005, G-2).

2. Michigan and Florida are the only other full-time legislatures to have adopted term limits. California has the largest number of staff per legislator (19.7 compared to 7.8 in Michigan and 11.3 in Florida in 2003). California also had the highest legislative salaries of any state legislature in 2003 (\$99,000 per year compared to \$77,000 per year in Michigan and \$28,000 per year in Florida) (see the National Conference of State Legislatures' website for further details, <http://www.ncsl.org/>).

3. Also note that assembly general election races would comprise the majority of a pooled dataset. There are 80 assembly general elections biennially, while there are only 20 biennial senate general elections due to staggered terms. In addition, many primary election races in both chambers go uncontested in California, far more than uncontested general election races. For example, in 2002 there were only 50 contested major party primaries in the senate and assembly combined.

4. Because gubernatorial elections in California occur every four years and assembly elections occur every two years, we've matched off-years to the previous gubernatorial vote to measure the ideological extremism of the district. The one exception was 1992, which occurred shortly after a redistricting, making the 1990 gubernatorial vote inappropriate. For that session, we used the gubernatorial vote in 1994. "The Supplemental Statement of the Vote," published biennially by the California Secretary of State's Office, breaks down gubernatorial votes by assembly district.

5. Since it is almost invariably incumbents who have the good fortune to run unopposed in our dataset, the size of the incumbency advantage will be inflated in years with unusually high numbers of uncontested races.

6. Otherwise, we would be comparing members with 16, 18, or more years of service in the pre-term-limits era to those after 1990 who could serve no more than six years. But we find substantively similar results if all incumbents from the pre-term-limits era are included.

7. In separate analysis (not presented), we also controlled for gubernatorial vote in the district, which allowed us to control for partisan changes in district composition over the 10-year life of each district under each districting plan. This analysis yielded nearly identical estimates of incumbency advantage to those presented.

8. Dollar quantities are denominated in constant 2000 dollars using the Bureau of Economic Analysis's Gross Domestic Product price deflator.

9. Again, these figures are in constant 2000 dollars.

10. Similar figures could not be calculated accurately for the assembly speaker, since only one sitting speaker, Fabian Nuñez, has been eligible to seek re-election since the imposition of term limits. Furthermore, Nuñez's re-election was in 2004, which is outside the timeframe of our data because of campaign finance reforms passed in 2000.

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