Using Repeat Challengers to Estimate the Effect of Campaign Spending on Election Outcomes in the U.S. House

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Previous studies of congressional spending have typically found a large positive effect of challenger spending but little evidence for effects of incumbent spending. Those studies, however, do not adequately control for inherent differences in vote-getting ability across candidates. "High-quality" challengers are likely to receive a high fraction of the vote and have high campaign expenditures, even if campaign spending has no impact on election outcomes. To avoid that bias, this paper examines elections in which the same two candidates face one another on more than one occasion; differencing eliminates the influence of any fixed candidate or district attributes. Estimates of the effects of challenger spending are an order of magnitude below those of previous studies. Campaign spending has an extremely small impact on election outcomes, regardless of who does the spending. Campaign spending limits appear socially desirable, but public financing of campaigns does not.

I. Introduction

Campaign finance has been the subject of political and academic debate almost continuously over the past few decades.[1] Accurate predic-

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[1] Congress passed five separate reforms between 1971 and 1979, though many of the early reforms were subsequently ruled unconstitutional by the Supreme Court in

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tions concerning the impact of the various policy reform proposals hinge on a clear understanding of the influence that campaign spending has on election outcomes. Yet despite an expansive literature devoted to that topic, the value of spending to candidates remains highly uncertain.

Almost without exception, previous studies on the House of Representatives have obtained a surprising result: campaign spending by challengers is found to have a large positive impact, whereas incumbent spending has little or no effect on election outcomes.[2] Such results, however, must be greeted with considerable skepticism since they are based primarily on cross-sectional analyses. Models estimated using cross-sectional data suffer from two unavoidable sources of bias: an inability to adequately measure candidate quality (i.e., intrinsic vote-getting ability) and the existence of district-specific factors that are omitted from the model. In the case of campaign spending, both of those biases are likely to exaggerate the effects of challenger spending while underestimating the impact of incumbent spending.

Failure to control for candidate quality will lead to an upward bias in the estimation of the impact of challenger spending because high-quality challengers will have a greater likelihood of winning and therefore will be able to raise a greater volume of campaign contributions (Snyder 1990). In contrast, the failure to include candidate quality will lead to an underestimate of the effects of incumbent spending since incumbents tend to increase campaign expenditures in response to a strong challenge.

Failure to control for district-specific factors will also lead to bias in cross-sectional regressions if districts differ systematically on characteristics that are correlated with both vote totals and campaign spending. Differences in partisanship across districts are an obvious source of such effects: a Democratic challenger in a staunchly Republican district will likely encounter great difficulty in raising campaign funds and will also obtain a low percentage of the vote. Since the race is unlikely to be close, the Republican incumbent's expenditure will

Buckley v. Valeo (424 U.S. 1 [1976]). Since that time, reform proposals have repeatedly been defeated (1986, 1988, and 1990). While political scientists have historically devoted a great deal of attention to the issue, only recently have economists turned their attention to the topic: Snyder (1989, 1990), Gerber (1992), Palda (1992), and Stratmann (1992).

[2] See, among others, Glantz, Abramowitz, and Burkart (1976), Jacobson (1978, 1980, 1985, 1990), Welch (1981), Green and Krasno (1988, 1990), Abramowitz (1991), and Erikson and Palfrey (1993). Among those papers, only Green and Krasno (1988, 1990) and Erikson and Palfrey (1993) have found evidence that incumbent spending has a strong effect on election outcomes. A number of papers, including Abramowitz (1988) and Gerber (1992), have also examined the effects of campaign spending on Senate elections.

also tend to be low. Thus, in a cross-sectional model, differences in partisanship across districts will lead to an upward bias in the measurement of the impact of challenger spending and a downward bias on the effects on incumbent spending.

Previous research has devoted only limited attention to those two sources of bias. On the issue of candidate quality, the paper by Green and Krasno (1988) is a notable exception. The authors develop an eight-point scale to proxy challenger quality (they do not attempt to control for incumbent quality). Although the proxy is statistically significant, its inclusion has only minor effects on the spending coefficients and does little to improve the fit of the model, only increasing the R^2 from .596 to .624.[3] It is difficult to believe that candidate quality differences could play such a minor role in determining election outcomes, particularly in light of the results reported in Sections III and IV of this paper. Rather, it appears that Green and Krasno's quality proxy is simply unable to fully capture the multidimensional concept of candidate quality.

Attempts to control for district-specific effects have typically been limited to the inclusion of the once-lagged congressional vote in the district. While the lagged vote is certainly correlated with a district's partisanship, it also reflects the quality of the candidates involved in the preceding election, the level of campaign spending in that contest, and the national political situation. For that reason, the lagged vote is unlikely to fully capture differences across districts.[4]

In this paper, I propose an alternative method for estimating the impact of campaign spending that avoids the pitfalls associated with unmeasurable candidate quality and district-specific effects. In particular, I use panel data, restricting my analysis to those elections in which the same candidates face one another on multiple occasions. Under the assumption that an individual candidate's quality is constant over time, a fixed-effects transformation eliminates all influences of quality, as well as any other district-specific fixed effects. Having controlled for other factors such as incumbency status and national-level partisan swings, one can obtain consistent estimates of the impact of campaign spending on election outcomes. Even if an individual candidate's quality does fluctuate, the method used will greatly reduce the bias found in cross-sectional studies as long as the

[3] The coefficient on challenger spending falls from .052 to .042; the coefficient on incumbent spending goes from $-.007$ to $-.009$. See table 2 of Green and Krasno (1988).

[4] Abramowitz (1991) uses a district's vote in the presidential contest. While this is presumably a better measure, this approach fails to reflect differences in party strength at the state and local levels.

variation in a given candidate's quality over time is small relative to quality differences across candidates.

The results I obtain differ sharply from those of previous studies in two respects. First, I find that campaign spending has an extremely small impact on election outcomes regardless of incumbency status. According to my estimates, an extra $100,000 (in 1990 dollars) in campaign spending garners a candidate less than 0.33 percent of the vote. Controlling for candidate quality and district fixed effects reduces estimates of the value of challenger spending to only one-tenth of the level typically obtained in previous cross-sectional studies. Despite relatively small standard errors on the estimates, I am unable to reject the null hypothesis that campaign spending has no effect on election outcomes. Second, while I find challenger spending to be marginally more productive than incumbent spending, the difference is greatly reduced compared to previous studies. Moreover, the differences in results between this paper and previous studies cannot be attributed to the subsample I use. When the standard methodology of previous cross-sectional studies is applied to my subsample, the results are very similar to those reported in the literature.

The paper is organized as follows. Section II develops the basic model, demonstrating how first-differencing eliminates problems that arise from unobservable candidate quality and district-specific effects. Section III describes and summarizes the panel data set employed (those elections in which the same opponents face each other on multiple occasions) and also presents the results obtained when the standard cross-sectional methodology is applied to the subsample. Section IV contains the empirical estimates obtained from the model as well as a number of tests and extensions. Section V discusses the implications of the model for current policy proposals. In stark contrast to previous work (Jacobson 1987), mandatory spending limits are found to provide only a modest benefit to incumbents. Public financing of campaigns does little to increase the competitiveness of elections and therefore appears to be socially wasteful unless justified on other grounds. Section VI offers a brief conclusion.

II. A Model of Election Outcomes

In this section, the basic model is developed. For simplicity of exposition the model is presented assuming a linear relationship between campaign spending and vote shares (results from alternate specifications are also reported in the following section). Let the Democratic share of the two-party vote in district i at time t be a function of a district-specific constant, the level of campaign spending by each of the candidates, national political events, and candidate quality:

$$V_{i,t} = \alpha_i + (\eta + \beta_1 \text{Incum\$}_{i,t} + \beta_2 \text{Chal\$}_{i,t}) \times I_{i,t} + \beta_3 \text{Open\$}_{i,t} + \gamma_t + \delta_1 \text{DemQual}_{i,t} - \delta_2 \text{RepQual}_{i,t} + \epsilon_{i,t}, \quad (1)$$

where $V_{i,t}$ is the Democratic share of the two-party vote; α_i is a district-specific constant; $\text{Incum\$}_{i,t}$ is campaign expenditure by the incumbent; $\text{Chal\$}_{i,t}$ is campaign expenditure by the challenger; $\text{Open\$}_{i,t}$ is net campaign expenditures by open-seat contenders (Democratic spending − Republican spending); $I_{i,t}$ is an indicator variable equal to one if the Democratic candidate is the incumbent, negative one if the Republican candidate is the incumbent, and zero otherwise; γ_t is nationwide partisan shock in year t (a dummy variable); $\text{DemQual}_{i,t}$ is the (unmeasured) quality level of the Democratic candidate; $\text{RepQual}_{i,t}$ is the (unmeasured) quality level of the Republican candidate; and $\epsilon_{i,t}$ is an error term, assumed to be independently and identically distributed normal.

The variables are interpreted as follows. The variable α_i, a district-specific constant, reflects the partisan alignment of district i and is assumed constant over the life of a district.[5] Districts that are more favorable to Democratic candidates have higher values of α_i.

The variables $\text{Incum\$}_{i,t}$, $\text{Chal\$}_{i,t}$, and $\text{Open\$}_{i,t}$ capture the effects of campaign spending on election outcomes. The first two variables are interacted with the incumbency status in the district to reflect the fact that the dependent variable is the *Democratic* share of the vote.[6] The variable $\text{Open\$}_{i,t}$ represents the difference between Democratic and Republican spending in pursuit of an open seat. If there is an incumbent in a race, $\text{Open\$}_{i,t}$ is equal to zero. The a priori expectation is that all three of the β parameters will be positive.

The variable $I_{i,t}$ is an indicator variable reflecting incumbency status in a district. It is equal to one if the Democratic candidate is the incumbent, negative one if the Republican candidate is the incumbent, and zero otherwise. The coefficient η captures all incumbency advantages except those due to campaign spending. The incumbency

[5] If the variables DemQual and RepQual are scaled so that each has mean zero, α_i is properly interpreted as the normal vote of Converse (1966). This point is not very important, however, since the value of α_i cannot be separated from quality considerations.

[6] The reader may wonder why the Democratic vote is used as the dependent variable rather than the incumbent's vote, a seemingly more natural choice that would alleviate the need to multiply the spending variables by the incumbency indicator variable. The primary reason is that the district constant α_i is appropriate only if the dependent variable is a given party's share of the vote. In addition, the specification employed here offers two advantages: (1) it allows inclusion of open-seat contests and (2) it facilitates the measurement of nationwide partisan shocks.

advantage is assumed to be constant across years, a reasonable approximation for the time period studied (Gelman and King 1990).

The variable γ_t is a dummy variable capturing nationwide partisan shocks (which are assumed to affect all districts identically). The likely sources of such shocks are national-level political and economic activities. In the political realm, presidential coattails (Calvert and Ferejohn 1983; Campbell 1986) and systematic presidential punishment at the midterm (Erikson 1988; Alesina and Rosenthal 1989; Levitt 1994) are two regular sources of distortions to congressional outcomes. From an economic standpoint, growth rates, unemployment, and inflation are sometimes said to have predictable effects on congressional elections (Tufte 1975; Fair 1978). For the purposes of this paper, however, it is enough to measure partisan shocks without concern for the ultimate source of the shocks.

The variables $\text{DemQual}_{i,t}$ and $\text{RepQual}_{i,t}$ reflect the intrinsic attractiveness of the respective candidates. These quality variables, however, are not directly observable.

If candidate quality were directly observable, equation (1) could be estimated using panel data on the full sample of all congressional elections after the removal of district fixed effects. In the absence of a good measure of candidate quality, however, attempts to estimate equation (1) will suffer from the same potential omitted variable bias that plagues previous models. Under the assumption that an individual candidate's quality is constant over time, the key parameters of the model can be estimated without bias if we restrict our focus to sets of elections in which the same two candidates face off on more than one occasion.

For simplicity, take the case in which the same two candidates face each other exactly twice, first at time t and again at time $t + 1$. First-differencing equation (1) yields

$$\begin{aligned}\Delta\text{DemVote}_i = {} & \beta_1\Delta(\text{Incum\$}_i \times I_i) + \beta_2\Delta(\text{Chal\$}_i \times I_i) \\ & + \beta_3(\Delta\text{Open\$}_i) + \eta(\Delta I_i) + \Delta\gamma + \Delta\epsilon_i,\end{aligned} \tag{2}$$

where Δ represents the difference between the value of the variable at time $t + 1$ versus time t. The district-constant α_i and the quality terms drop out of equation (2) because they remain constant across the two elections.

Estimation of equation (2) on the subset of elections in which the same two opponents meet on multiple occasions will now be free of the omitted variable biases caused by unobservable quality and district-specific factors as long as two conditions hold. First, the subset of elections that constitute the sample must be representative of House elections as a whole. Second, an individual candidate's quality

must be constant over time. (If candidate quality does fluctuate over time, the parameter estimates for challenger spending are likely to be biased upward whereas the incumbent spending parameters are biased downward, just as in the previous models. One would expect, however, that the size of the bias will be greatly reduced; the estimates obtained in Sec. IV support such a view.)

In the following section, summary statistics for the subsample of elections with repeat contenders are compared with statistics for the entire sample of House elections; the subsample appears to be broadly representative. More important, when the standard cross-sectional regression is run on the subsample, the results obtained are very similar to those in the previous literature.

III. The Data

The subsample used in the analysis consists of the 633 elections between 1972 and 1990 in which the same major party candidates faced one another in two or more general elections within a given district.[7] The subsample represents approximately 15 percent of the total congressional elections held during this time period.

Table 1 compares a number of descriptive statistics for a nearly complete[8] data set of contested elections held between 1972 and 1990 and the subsample used in this paper. Data for the subsample of 633 elections are further broken down into elections that represent the first time a pair of candidates face off (col. 2) and all later meetings (col. 3).

As panel A of table 1 demonstrates, the subsample appears generally representative of the elections as a whole. The average Democratic percentage of the vote in contested elections is 54 percent; in the subsample it is approximately 55 percent. The elections in the subsample are slightly more competitive than the typical uncontested elections. Incumbents won 66.8 percent of the vote in the broad sam-

[7] These 633 elections represent 299 pairs of opponents. A number of opponents faced one another three or more times. Cases in which the same two opponents met but redistricting intervened were discarded. In elections that directly followed redistricting, a candidate currently serving in the House was deemed an incumbent. While this could in theory understate the true incumbency advantage, empirically it had virtually no impact on the estimates obtained. Therefore, such observations were included in order to increase the available degrees of freedom.

[8] The sample used to compute descriptive statistics for overall contested elections is described in Levitt and Wolfram (1994). It includes all contested elections except (1) those elections directly following redistricting and (2) those elections in districts that existed for two or fewer elections before being redistricted. There is little reason to think that the descriptive statistics of this nearly complete sample differ systematically from those of the complete sample (which was not readily available to me).

TABLE 1

DESCRIPTIVE STATISTICS

	Overall Contested Elections (1)	Subsample of Repeat Meetings	
		First Meeting (2)	Later Meetings (3)
	A. Statistics		
Democratic percentage of vote	54.1	55.1	55.5
	(18.0)	(16.6)	(16.4)
Incumbent's percentage of vote	66.8	64.2	63.4
	(10.1)	(10.8)	(10.8)
Success rate for incumbents seeking reelection	94.8	94.1	89.5
	B. Breakdown by Status of Incumbent		
Democratic	52.8	55.5	62.6
Republican	36.2	34.8	37.4
Open seat	10.9	9.7	...
	C. Campaign Spending per Candidate (Thousands of 1990 Dollars)		
Incumbents	293	266	343
Challengers	136	134	173
Open seat	409	275	...
Observations	2,781	299	334

NOTE.—Numbers in parentheses are standard deviations. Col. 1, except for spending data, is drawn from the data set used in Levitt and Wolfram (1994). See n. 8 for further information. Spending data in col. 1 are unweighted averages of real spending for all major party candidates in general elections between 1972 and 1990. They are based on Common Cause (1974, 1976), Sorauf (1988, table 6-1), and multiple editions of the Federal Election Commission *Reports on Financial Activity*.

ple; in the subsample, this margin is reduced by about three percentage points. The percentage of beaten incumbents is higher in the subsample, especially when the opponents have met previously. The increased rate of challenger success in repeat bids is attributable to the fact that politicians appear to behave strategically (Jacobson 1989); repeat challenges are far more likely in those years in which national political conditions favor the challenger's party. For instance, in the aftermath of Watergate in 1974, 19 Democrats who had previously run for office chose to challenge again, compared to only three Republicans. Similarly, in the Reagan landslide of 1980, repeat Republican challengers outnumbered repeat Democratic challengers almost three to one. When national political conditions are controlled for in the regression analysis of the following section, the differences between first meetings and repeats disappear.

Panel B of table 1 breaks down the data according to incumbency status. Again, the subsample of repeat challengers is largely representative. The one notable difference is the absence of open-seat elections in column 3; except under unusual circumstances,[9] if two candidates are meeting for a second time within the same district, one of them will be the incumbent.

Panel C of table 1 compares mean campaign spending over the period (in 1990 dollars). Again the subsample appears generally representative. It is interesting to note that in second meetings between candidates both incumbents and challengers increase their spending by approximately 25 percent.

While the summary statistics broadly support the contention that the subsample is representative, more compelling evidence comes from a cross-sectional regression along the lines of those performed by Jacobson (1980, 1985, 1990) and others. The incumbent's share of the vote was regressed on incumbent spending, challenger spending, "competitor party strength" (CPS) in the district (proxied by a lagged vote share), and dummy variables reflecting the year of the election. The results of those regressions are presented in table 2. Elections involving the first meeting between candidates were separated from later meetings to isolate any systematic differences between the two sets of elections. Column 1 displays the results for the case in which candidates meet for the first time. Following Jacobson, I used the once-lagged congressional vote to represent CPS. For the set of repeat elections, two sets of estimates are provided. In column 2, competitor party strength is proxied by the vote percentage in the most recent congressional election in which the current challenger was not involved. Measuring CPS in that way ensures that the interpretation of the regression is comparable to previous cross-sectional analyses in which only a small fraction of the elections involve repeat challengers. In column 3, CPS is measured by the vote percentage obtained in the previous congressional election. Since the same two opponents ran in the preceding contest, column 3 will do a better job of controlling for candidate quality than either the first two columns of table 2 or past cross-sectional studies. Differences in the estimated impact of campaign spending between the first two columns and the third suggest that candidate quality is not adequately controlled for in the standard cross-sectional analysis.

In columns 1 and 2, the estimated impacts of campaign spending are identical: a $100,000 increase in challenger spending garners that

[9] Specifically, the winner of the first meeting of the two candidates has to subsequently lose the office to another candidate who later does not seek reelection or loses in a primary.

TABLE 2

CROSS-SECTIONAL ESTIMATES FOR SUBSAMPLE OF REPEAT CHALLENGERS: LINEAR MODEL

VARIABLE	FIRST MEETING (1)	LATER MEETINGS (2)	LATER MEETINGS (3)
Challenger spending	−2.7	−2.7	−.8
	(.3)	(.3)	(.2)
Incumbent spending	−.2	−.2	.1
	(.3)	(.2)	(.1)
Competitor party strength:			
Vote share (−1)	.54		.78
	(.07)		(.04)
Vote share in last election with different challenger		.21	
		(.05)	
Constant	34.0	48.0	14.2
	(5.2)	(7.8)	(3.2)
Adjusted R^2	.56	.55	.76

NOTE.—Dependent variable is incumbent's share of the two-party vote. Standard errors are in parentheses. Spending variables are in terms of $100,000 of 1990 dollars. Year dummies (not shown) were included in all regressions.

candidate 2.7 percent of the vote, whereas marginal spending by the incumbent has essentially no impact on the election outcome. There does not appear to be a systematic difference in the effects of campaign spending between the first time two candidates meet and subsequent elections. It is important to note that the results in columns 1 and 2 are indistinguishable from the results of previous studies using cross-sectional data, collected in table 3. As a consequence, any differences between the results obtained in applying the panel data model of the following section and the results of past cross-sectional analyses must be attributed to the methodological approach, not the sample being analyzed.

Column 3 of table 2 provides an informal test of the standard cross-sectional approach. If challenger quality were adequately controlled for using a cross-sectional approach, the results in columns 1–3 should be similar. Note, however, that in column 3, where a better control for challenger quality is available, the impact of challenger spending shrinks to less than one-third of the previous estimates. The proportion of the variance explained by the model rises substantially as well. The results of column 3 suggest that failure to adequately control for challenger quality in previous cross-sectional analyses has led to an upward bias in the effects of challenger spending. The estimates obtained in the following section further reinforce that conclusion.

TABLE 3

CAMPAIGN EXPENDITURE IMPACT ESTIMATES (Linear Models): PERCENTAGE OF TWO-PARTY VOTE OBTAINED THROUGH A $100,000 INCREASE (1990 Dollars)

Model	Time Period	Challenger	Incumbent
Jacobson:			
Ordinary least squares	1972–82	2.7	.2
Two-stage least squares	1972–82	4.2	. . .*
Green and Krasno:			
Ordinary least squares	1978	1.6	.1
Two-stage least squares	1978	2.4	2.2

SOURCE.—OLS: Jacobson (1980, pp. 142–43; 1985, p. 17) and Green and Krasno (1988, p. 899). TSLS: Jacobson (1985, p. 36) and Green and Krasno (1988, p. 899).

NOTE.—Estimates for Jacobson's model represent unweighted averages of estimates for the relevant time period.

* Jacobson does not estimate the impact of incumbent spending in the TSLS model because of imprecision arising from multicollinearity.

IV. Empirical Results Using Panel Data

The model presented in Section II was estimated using the set of 633 observations described above.[10] To account for the possibility of decreasing returns to campaign spending, two further regressions were run relaxing the assumption of a linear relationship between spending and vote shares. In those alternate specifications, the square root of spending and the log of spending were used, respectively, as independent variables.[11] Also, a scandal dummy was included in the regression.[12] All campaign expenditures have been transformed into 1990 dollars.

Although differencing the data, as this analysis requires, generally exacerbates any errors-in-variables problem, measurement error is unlikely to be a major concern here. Federal law requires disclosure and detailed accounting of every campaign expenditure over $200.

The basic regressions are presented in table 4. Columns 1, 2, and 3 correspond to the estimates obtained using spending in levels, square

[10] Because some candidates faced off on three or more occasions, the actual estimation required a fixed-effects transformation as opposed to first-differencing.

[11] For the log specification, any candidate that reported zero campaign spending was coded as having spent $1,000.

[12] Scandals were identified using candidate profiles found in the election preview published by the Congressional Quarterly. For the 633 contests, only seven scandals were uncovered: (1) Edward Patten (D-N.J.), 1978, Tongsun Park scandal, accused of violating House ethics standards; (2) Robert Bauman (R-Md.), 1980, solicited sex from a 16-year-old boy; (3) Frank Thompson (D-N.J.), 1980, Abscam; (4) George Hansen (R-Idaho), 1984, false financial disclosure led to House reprimand; (5) Pat Swindall (R-Ga.), 1988, money-laundering scheme, federal grand jury investigation; (6) Arlan Strangeland (R-Minn.), 1990, involvement with lobbyist, including after-hours phone calls and taxicabs charged to House credit card; (7) Denny Smith (R-Ore.), 1990, saving and loan related complaint to House ethics committee.

TABLE 4

RESULTS OF THE REGRESSION MODEL

Variable	Linear Spending (1)	Square Root Spending (2)	Log Spending (3)
Challenger spending	.30	.13	1.04
	(.19)	(.06)	(.50)
Incumbent spending	.09	.06	.61
	(.13)	(.06)	(.75)
Open-seat spending	.17	.09	.67
	(.44)	(.13)	(1.40)
Incumbency	3.2	3.7	3.5
	(.8)	(1.1)	(1.7)
Scandal dummy	4.8	4.8	5.0
	(1.4)	(1.4)	(1.4)
1990	...	...	...
1988	.6	.7	.6
	(.7)	(.7)	(.7)
1986	1.6	1.7	1.7
	(.9)	(.9)	(.9)
1984	−1.4	−1.3	−1.4
	(1.1)	(1.1)	(1.1)
1982	2.0	2.2	2.1
	(1.3)	(1.3)	(1.3)
1980	...	...	...
1978	2.3	2.3	2.5
	(.7)	(.7)	(.7)
1976	4.0	4.0	4.2
	(.9)	(.9)	(.9)
1974	5.0	4.9	5.1
	(1.1)	(1.1)	(1.1)
1972	−2.0	−1.9	−1.7
	(1.4)	(1.4)	(1.4)
Adjusted R^2	.24	.24	.24
F-test*	.85	1.64	1.36

NOTE.—The dependent variable is the Democratic percentage of the two-party vote. White heteroskedasticity-consistent standard errors are in parentheses. Spending variables are in terms of $100,000 of 1990 dollars. All variables except for year dummies are multiplied by the incumbency indicator variable (see Sec. III for further explanation). Year dummies for the 1970s are relative to 1980; year dummies for 1980s are relative to 1990. Adjusted R^2 value refers to the percentage of variance explained after the fixed-effects transformation. In col. 3, candidates spending less than $1,000 are treated as though they spent $1,000. Degrees of freedom are equal to 320 in all regressions.

* F-test of spending coefficients equal to zero.

roots, and natural logs, respectively.[13] Heteroskedasticity-consistent standard errors (see White 1980) are in parentheses. The adjusted R^2 values are similar across regressions, as are the values and stan-

[13] The model was also estimated using various permutations of the ratio of campaign spending between the incumbent and the challenger. The results of those regressions were completely consistent with the results presented below and are available from the author on request.

dard errors on the variables that are common to the three regressions. The reported adjusted R^2 values are the percentage of the variance explained by the regression after fixed effects are removed. (More than 95 percent of the total sample variation is eliminated through the removal of fixed effects, further reinforcing the contention that some combination of unobserved differences in quality across candidates and district-specific factors is driving the estimates obtained from cross-sectional models.)

Although the primary concern of the analysis is the impact of spending, it is useful to note first that the other parameters of the model are plausibly estimated. The incumbency advantage is significant and worth between three and four percentage points. Those numbers are in line with previous estimates that have measured the incumbency advantage when controlling for challenger quality (as the choice of sample explicitly does). Levitt and Wolfram (1994) find an incumbency advantage between 4 and 5 percent when controlling for challenger quality (compared to almost 9 percent before controlling for challenger quality). Not surprisingly, involvement in a scandal costs the incumbent almost five percentage points.

The nationwide partisan shocks are also presented in table 4. Some care must be taken in interpreting the partisan shocks since the parameter values are given with respect to a baseline year.[14] The values for the 1970s are relative to the year 1980; the values for the 1980s are relative to 1990. A positive value corresponds to a shock in favor of the Democrats. Once again, there is virtually no difference across specifications and few surprises. Republican congressional candidates performed strongly in 1972, 1980, and 1984, all years in which popular Republican presidents swept into office by landslides. Strong Democratic performances coincide with the aftermath of Watergate in 1974, the Carter victory in 1976, and the Reagan midterms in 1982 and 1986. The effect of national political events on congressional elections is substantial: with everything else held constant, a Democratic candidate for the House would have received an extra 4 percent of the vote running in 1976 versus 1980. The midterm cycle (Alesina and Rosenthal 1989) also emerges quite clearly.

Given that the other parameter values in the model are plausibly estimated, let us now focus on the effects of campaign spending, presented in the top three rows of table 4. All the spending coefficients enter with the expected sign. In all three regressions, campaign spending by challengers carries the largest coefficient. In contrast to the other variables in the regressions, however, the campaign spend-

[14] Computing year shocks in terms of baselines is required because of the perfect linear dependence across the year dummy variables within a decade.

ing variables are generally not significantly different from zero, despite standard errors that are small.[15] The *F*-tests of the null hypothesis that all spending coefficients are equal to zero are reported for each regression at the bottom of table 4. The critical value for rejection of the hypothesis at the .10 level is 2.10. Remarkably, none of the three specifications can reject the null that campaign spending has no effect.

The spending coefficients in column 1, the linear specification, have a direct interpretation: an extra $100,000 in campaign spending (with the opponent's spending held constant) garners a challenger 0.3 percent of the vote while adding less than one-tenth of a percentage point for incumbents. The marginal value of a dollar of spending varies with the level of spending in columns 2 and 3. The typical challenger in 1990 spent approximately $200,000; increasing this quantity by $100,000 buys the challenger 0.42 and 0.19 percentage points of the vote in columns 2 and 3, respectively. Even at low challenger spending levels, the effects of spending are extremely small. A challenger whose spending increased from $50,000 to $150,000 would gain only 0.68 and 0.49 percentage points according to the estimates in columns 2 and 3. The typical incumbent involved in a contested election in 1990 spent approximately $400,000 on reelection; increasing spending by $100,000 will improve the incumbent's tally by less than 0.2 percent of the vote (0.14 in col. 2, 0.09 in col. 3). The impact of spending by open-seat candidates falls in between that of incumbents and challengers. Regardless of who does the spending, the effects are small relative to the value of the incumbency advantage or the nationwide partisan shocks.

The results obtained in the regressions above contrast sharply with the previous results in the literature obtained using cross-sectional models (see table 3). Whereas previous models have found a $100,000 increase in challenger spending to result in a vote swing of between 1.6 and 4.2 percent, the model of this paper obtains an estimate that is an order of magnitude lower: 0.3 percent. Unobserved changes in candidate quality across elections cannot explain the difference between the two sets of results since such changes will lead to an *upward* bias in the estimated impact of challenger spending in the model estimated in this paper.

The effects of incumbent spending, on the other hand, are similar (i.e., nonexistent) when the fixed-effects and cross-sectional approaches are used. Ironically, my results suggest that the standard conclusion observed in the literature—namely, that the impact of

[15] The standard errors in this paper have approximately the same magnitude as those reported in Jacobson (1980, 1985) and Green and Krasno (1988).

challenger spending is measured accurately but the effects of incumbent spending are biased downward—should in fact be reversed. The large effects of challenger spending appear to be an artifact of model misspecification, whereas estimates of the impact of incumbent spending are not greatly affected by the choice of specification. Once candidate quality and other district-specific fixed effects are adequately controlled for, neither candidate's spending is very effective.

Tests and Extensions of the Basic Model

A number of steps were taken to test the accuracy of the underlying assumptions of the model as well as the robustness of the results to different specifications. First, the effects of spending were allowed to vary across the two parties. Although the parameter values corresponding to Democratic spending were generally slightly higher than those of the Republicans, in none of the three specifications could an F-test of the null hypothesis of no differences across parties be rejected.[16] Second, a dummy variable was included to capture any systematic changes in the performance of challengers when candidates met for a second time.[17] In no case was the value of that parameter significantly different from zero.

One potential explanation for the low impact of spending found in this paper is that election dynamics are somehow altered in such a way as to reduce the value of campaign spending when two candidates face each other for a second or third time. To test that hypothesis, the slopes of the spending parameters were allowed to vary between first elections and repeat challenges.[18] The F-values for the test

[16] The statistic for the test of no party differences is distributed $F_{[3,317]}$. The critical level for rejecting the null with 90 percent certainty is 2.08. The actual test statistics were 0.95 (in the linear case), 0.99 (in the square root case), and 0.93 (in the log case).

[17] Specifically, the dummy variable took on a value of zero if two opponents were meeting for the first time, a value of one when the candidates had met previously and the challenger was a Democrat, and a value of negative one when the candidates had met before and the challenger was a Republican.

[18] The specific form of the test is as follows. (For simplicity in exposition, only the case in which opponents meet exactly twice is presented here. The more general case follows directly.) Let election outcomes be described exactly as in eq. (1), except that in the second election the coefficient on incumbent spending is equal to $\beta_1 - \lambda_1$ and the coefficient on challenger spending is $\beta_2 - \lambda_2$. First-differencing in this case yields

$$\begin{aligned}\Delta\text{DemVote}_i = {} & \beta_1\Delta(\text{Incum\$}_i \times I_i) + \beta_2\Delta(\text{Chal\$}_i \times I_i) \\ & + \beta_3(\Delta\text{Open\$}_i) + \eta(\Delta I_i) - \lambda_1\text{Incum\$}_{i,t+1} \\ & - \lambda_2\text{Chal\$}_{i,t+1} + \Delta\gamma + \Delta\epsilon_i.\end{aligned} \tag{2'}$$

Equation (2') is identical to eq. (2) except that incumbent spending and challenger spending now enter both in levels and first-differenced. If spending matters less in the second election, the signs on Incum\$ and Chal\$ should both be negative. The null hypothesis of identical slopes across elections is that $\lambda_1 = \lambda_2 = 0$ and can be easily tested using an F-test.

of the null hypothesis that the spending parameters are identical across first and repeat elections were well within acceptable levels in all three specifications—support for the simple model presented in this paper.[19]

Another potential source of model misspecification is parameter values that change over time. To test for stability in the parameters, the sample was divided into two parts: 1972–80 and 1982–90. The model was then reestimated allowing all coefficients to differ across the two time periods. A Chow test could not reject the null hypothesis of no shift in the underlying model in any of the specifications.[20] The model was also estimated excluding the years 1972 and 1974 because of concerns about the quality of the campaign spending data in those years.[21] Again, there was little effect on the parameter values.

Finally, an attempt was made to determine whether campaign spending has a greater impact in highly competitive elections than in "noncompetitive" elections. As a crude proxy, any challenger spending less than $10,000 in 1990 dollars was deemed noncompetitive.[22] When only "competitive" elections were included, the point estimates for the effects of candidate spending were actually slightly *lower* but were not significantly different from the results obtained using the overall sample.

V. Policy Implications

In this section, the likely effects of three different public policy proposals involving mandatory spending limits or public financing of campaigns or both are examined. A simple and straightforward methodology is employed in what follows. The analysis assumes that the impact of spending in all districts is characterized by the parameters presented in table 4 above. The effect of a given policy proposal is then computed for *all* congressional elections involving an incumbent held between 1984 and 1990 (not just the subsample employed in obtaining the estimates), under the assumption that all factors ex-

[19] The statistic for the test is distributed $F_{[2,318]}$. The critical value for rejecting the null with 90 percent certainty is 2.30. The actual values were 0.23 (for spending in levels), 0.47 (for the square root case), and 0.59 (for the log case).

[20] The Chow test statistics (distributed $F_{[5,315]}$) were 0.91 for the linear case, 0.86 for the square root specification, and 0.34 for the log case. The critical value for rejection (at the .10 level) of the null hypothesis of no underlying shift is 1.88, well above the observed values.

[21] Data for 1972 and 1974 were compiled by Common Cause rather than the Federal Election Commission.

[22] In the 159 elections in which the challenger spent less than $10,000, incumbents received an average of 74.3 percent of the vote. In the 445 elections in which challenger spending exceeded $10,000, incumbents received an average of 60.0 percent. Open-seat elections were not included.

cept for spending levels would remain constant. Three possible limits/floors are considered for each policy proposal: $100,000, $200,000, and $400,000.[23] In order to slant the results in favor of a more pronounced impact of the policy proposals for the fixed-effects model, any election result that would have been affected according to *any* of the specifications (linear, square root, or log) is included in the tallies. For purposes of comparison, the same calculations were also performed using the point estimates obtained in the previous cross-sectional work of Jacobson (1980, 1985). In no case did one party systematically benefit from a policy proposal; therefore, predicted seat changes are aggregated across parties.

The obvious drawback to this type of analysis is that it captures only partial equilibrium effects. Implementation of policy changes may also have an important impact on strategic candidate decisions such as whether or not to enter a race or seek reelection. Therefore, the results that follow should be viewed only as first approximations of the potential policy effects.

Mandatory Spending Limits

While mandatory spending limits were ruled unconstitutional in *Buckley v. Valeo* (424 U.S. 1 [1976]), from an analytical point of view, such a policy is nonetheless of interest. A mandatory spending limit has two attractive features: it does not require public funding, and it effectively reduces overall levels of campaign spending. The primary argument made against spending limits (besides their unconstitutionality) is that they may reduce competition and reinforce the incumbency advantage (e.g., Jacobson 1987).

According to the estimates obtained using the fixed-effects model of this paper, the impact of spending caps on election outcomes is extremely small. Even for a cap of $100,000 (a 75 percent reduction for the average incumbent and a 50 percent reduction for the average challenger), only 15 election results are reversed over four sets of congressional elections—less than 1 percent of the elections held during the time period examined. Higher spending limits alter the outcome of even fewer elections. Spending caps only marginally benefit incumbents: a spending limit of $100,000 would have led to a net increase of seven victories for incumbents.

In contrast, Jacobson's cross-sectional estimates predict a decidedly pro-incumbent bias with spending caps. No challengers would have benefited from the caps, but 37 incumbents would have avoided de-

[23] Space limitations preclude a full presentation of all scenarios considered. Tables with a complete listing of all results are available from the author on request.

feat with a $100,000 cap. What makes the Jacobson prediction even more remarkable is that only 43 incumbents (out of 1,612 seeking reelection) were defeated in general elections between 1984 and 1990. According to Jacobson's model, the success rate of incumbents would have been over 99.5 percent with a $100,000 spending cap.

"Level Playing Field" Proposals: Mandatory Caps in Conjunction with Public Financing

I now consider a policy that enforces spending caps while also providing full public financing up to the level of the spending limit. The spending caps are assumed binding even if the candidate refuses public financing.[24] The difference between this policy and the mandatory spending limit proposal examined above is that candidates who fell short of the limit in the previous case are now subsidized up to that limit. As a consequence, this policy eliminates all incentives for private fund-raising.

A striking result emerges from the fixed-effects model: public financing up to $200,000 per candidate leads to almost exactly the same outcome as the spending caps without public financing examined above. In the case of public funds provision up to $100,000, the effects are identical to those of a straight spending limit; in the case of $200,000 in public funds, only one additional election outcome is affected, despite an estimated taxpayer cost of $700 million over the four sets of elections. Subsidizing candidates up to a $400,000 limit has an impact on an additional seven elections; all those benefiting from the public subsidy are challengers. Once again, however, it is clear that changing campaign spending patterns is a very blunt tool for affecting election outcomes. A government outlay of $400,000 per candidate would alter the results of less than 1 percent of the congressional elections in the sample while costing taxpayers over a billion dollars.

In contrast, the estimates using the cross-sectional parameters of Jacobson suggest that a generous "level playing field" policy would have a very favorable impact on challengers: 97 challengers would have benefited, but only 20 incumbents would have avoided defeat.

"Floors without Ceilings": Public Financing without Spending Limits

A "floors without ceilings" policy is one in which any candidate below a threshold level of spending is provided public funds to make up

[24] This policy, like the previous one, is likely to be considered unconstitutional but again provides a useful analytical benchmark.

the shortfall. Campaign spending of candidates above the threshold level remains unaffected.[25] While such a policy itself may not be particularly attractive, its effects on election outcomes are likely to be similar to schemes involving public financing in conjunction with voluntary spending restraints. A variety of experiences at the state level (see Sorauf 1988; Alexander 1991, 1992) suggest that candidates with the ability to raise funds above those limits are not likely to participate voluntarily in such programs if involved in close races.

According to the fixed-effects model estimates, the impact of such policies is virtually nonexistent, corroborating the earlier findings of Welch (1981). Over the last four years, raising the spending of all candidates who fell short of $200,000 up to that level without placing restrictions on the opponent would have altered the outcome of only two elections while costing taxpayers up to $400 million.[26] A floor of $400,000 would have altered eight elections (seven in favor of challengers and the other an open-seat contest) at an estimated taxpayer cost of over $1 billion. For the Jacobson cross-sectional estimates, a policy of floors without ceilings leads to results that are almost identical to those implied by a level playing field.

VI. Conclusions

This paper finds that once district-specific factors and the quality of the competing candidates are controlled for, the impact of campaign spending on election outcomes, regardless of incumbency status, is small but positive. These results contrast sharply with the existing literature in which challenger spending is found to have a large positive impact whereas incumbent spending has a negligible effect. In light of the results in this paper, one must question whether previous estimates are simply an artifact of cross-section modeling.

The estimates obtained in this paper have radically different implications for public policy than previous cross-sectional estimates. In contrast to the previous literature, my results suggest that spending caps may be desirable, but public financing of campaigns is clearly not justified. Excess fund-raising appears to be a socially wasteful activity that distracts representatives from their legislative duties, grants excessive power to political action committees (PACs), and discourages potential high-quality challengers who do not have ready

[25] The state of Montana has such a policy (see Sorauf 1988, p. 275).

[26] The estimate of taxpayer cost is based on the assumption that candidates spending less than $200,000 will take advantage of the policy by raising no funds privately, whereas candidates raising more than $200,000 currently would continue to do so. A less costly alternative to this program would be one involving matching funds rather than outright grants.

access to campaign funding and find fund-raising distasteful. Tight spending limits provide only a minor advantage to incumbents. If high-quality challengers, previously deterred by the war chests of incumbents, chose to run in the presence of spending limits, the success rate of incumbents might even be lowered.

Unfortunately, the Supreme Court has ruled spending caps unconstitutional unless accompanied by public financing. Given the limited impact of public financing on election outcomes, increased competitiveness of elections does not appear to justify the costs to taxpayers of funding such programs. Support for public spending on elections must be based on other factors such as the reduction in the influence of PACs, an issue about which the analysis of this paper can say nothing.

If campaign spending matters so little, as this paper asserts, why do politicians work so hard at fund-raising and spend so much money? There are two possible explanations. First, the opportunity cost of raising funds may be very low compared to the value of winning an election, so that even if there is only a small probability that spending affects the election outcome, it is worthwhile. Alternatively, it may simply be that politicians have confused correlation and causality when considering the relationship between spending and electoral success.

The analysis of this paper suggests that many of the current ills of our political system need not exist. If campaign spending has little impact on election outcomes, representatives should not feel unduly influenced by PACs. Campaign finance abuses such as "soft money" no longer appear worrisome if elections cannot easily be "bought." Finally, high levels of campaign spending or incumbent war chests, while perhaps socially wasteful, need not deter high-quality candidates from challenging incumbents.

Perception, however, is everything. The *belief* that money is the key to electoral success is almost as damaging as a scenario in which money really does matter. As long as conventional wisdom views money as critical, the patterns of behavior that have led to widespread criticism will remain.

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