

WORKING PAPER

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Spending in House Elections:
A Natural Experiment Approach

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1. Introduction

The public debate over campaign finance often ignores the one fact that most academic researchers who work in this field agree upon: it is not yet known to what extent campaign finance drives either public policy decisions or electoral outcomes. This is not for want of trying; there is an extensive literature (in both political science and economics) on both campaign contributions and campaign spending. However, evidence on the claim that money plays an important role in either policy formation or electoral outcomes can be fairly described as, at best, “mixed.” To be sure, there is anecdotal evidence for both the corrupting effect of campaign contributions and the efficacy of certain political advertisements; further, there is some systematic evidence consistent with the twin hypotheses that campaign contributions function as if they were bribes and that campaign spending drives electoral success. However, several authors have argued that these same patterns in the data are also consistent with a less dominant (and therefore less nefarious) role for money in politics [e.g., Wright (1989), Wright (1996), Sorauf (1992), Levitt (1995) and Milyo (1997c)].

Rather than rehash this debate, this paper presents the results of several recent empirical studies, which, when taken together, provide fairly consistent evidence that marginal campaign spending by incumbents has little impact on electoral outcomes. This in turn suggests that campaign contributions are not very effective as bribes, since campaign contributions are limited to small amounts by current law. A contribution of five or ten thousand dollars to a member of

the House is unlikely to solicit much gratitude, as this amount of marginal campaign spending has essentially no effect on a candidates chances for success.¹

This paper examines several “natural experiments” on the effects of campaign spending in House elections. Each of the experiments is by itself a weak test of the presence of the electoral effects of marginal campaign spending, but taken together they provide a fairly consistent and coherent counterpoint to the conventional wisdom that campaign spending drives electoral success. In the least, this exercise suggests that researchers, reformers and reform opponents should exercise caution in predicting the likely effects of campaign finance reform on either elections or policy-making.

2. A Brief Literature Review: Does Campaign Spending Affect Electoral Outcomes?

There exists a plethora of studies by political scientists and economists that attempt to measure the electoral effects of Congressional campaign spending; each of these studies must confront the fact that the candidate spending is not just a determinant of electoral success, but that expected electoral success determines the fund-raising ability and spending decisions of the candidates as well.² The empirical problem is made more complex by the presence of important unobservable determinants of both campaign spending and electoral success.³ The “first generation” of academic studies [e.g., Jacobson (1978) and Green and Krasno (1988)] did not address this latter point well; the “second generation” of studies confronts the twin problems of reverse causality and omitted variables [e.g., Grier (1989), Erickson and Palfrey (1993), Gerber (1994), Levitt (1994) and Ansolabehere and Snyder (1996b)], though not all do so equally well. Not surprisingly then, the literature in this area displays an impressive diversity in both the

specification of statistical models and the estimated effects of campaign spending by incumbents and challengers (both within and across “generations”).

Several studies argue that campaign expenditures are beneficial for challengers but not for incumbents [Glantz, Abramowitz, and Burkhart (1976), Jacobson (1978, 1980, 1985, and 1990), Abramowitz (1988), and Abramowitz and Segal (1992)]. On the other hand, Green and Krasno (1990) and Grier (1989) find that incumbent expenditures have positive effects on incumbent vote share, but that expenditures by challengers are more productive. Not to be outdone, Green and Krasno (1988), Gerber (1994) and Ansolabehere and Snyder (1996b) all argue that incumbent and challenger spending are equally productive, while Levitt (1994) finds that incumbent and challenger spending are equally *unproductive*. To make the confusion complete, Erickson and Palfrey (1993) argue that incumbent expenditures have a significant impact on vote shares, but challenger expenditures do not.

Grier (1989), Gerber (1994), Levitt (1994) and Ansolabehere and Snyder (1996b) all argue fairly convincingly that much of the existing empirical work on the electoral effects of campaign spending is fatally flawed. The nature of the critique is fairly technical, but the intuition can be conveyed in a relatively painless manner. In order to isolate the direct effects of spending on election outcomes, a researcher must examine variations in candidate spending that are not caused by factors which also directly affect the election outcome (i.e., “instrumental variables” or “instruments”). For instance, suppose that personable candidates have an advantage in raising funds. Further, suppose that these personable candidates exercise this advantage and spend more on their campaigns. If one observes that the subset of personable candidates fares better in elections than do other candidates it might be attributable to the fact that such candidates spend

more money. However, it may be the case that money itself has little impact on the election outcome and that voters simply prefer more personable candidates. Therefore, variations in candidate personality would not provide a good instrument for uncovering the electoral effects of campaign spending. On the other hand, committee assignments might be an example of a good instrument. If voters are ignorant of the committee assignments of their representatives, then such assignments may not have a direct causal effect on electoral outcomes. If, in addition, committee assignments are key to candidate fundraising, it may then be possible to isolate changes in campaign spending attributable to committee assignments. Examining variations in spending caused by the instrumental variable (committee assignments), it may then be possible to discover changes in electoral outcomes attributable to the instrumented changes in campaign spending (a.k.a., “exogenous” changes in campaign spending). This need for instrumental variables has been recognized by most researchers, although nearly all of these studies stumble on the next point.

Instrumental variables must not only be a direct cause of spending and not a direct cause of electoral success, but they must not be correlated with omitted causes of electoral success. Again, consider candidate personality; such a thing is no doubt difficult to quantify, and so is usually an “omitted variable” in analyses of campaign spending. This is of little concern when omitted variables are either unimportant or effectively random influences (e.g., the color of a candidates’ eyes is also an omitted variable). However, in studies of elections, personality traits (integrity, honesty, etc.) are non-trivial omitted variables; further, these omitted variables are likely to be correlated with potential instrumental variables. Again, for example, committee assignments may be won or lost based on a candidate’s personality; the same characteristics that

enable someone to rise through the ranks in political organization may also enable that candidate to sway voters. So when one observes that candidates on powerful committees raise more money and are more successful in elections, it does not necessarily follow that it was campaign spending that caused the observed electoral effects. Instead, it may be the case that unobserved candidate attributes were a cause of both electoral success and committee assignment (it may even be the case that committee assignment had no direct effect on fundraising; all of the observed differences might be caused by the omitted variable, candidate personality).

The “second generation” authors argue that past studies employ instruments that are correlated with important omitted variables, and therefore do not capture the true electoral effects of campaign spending. For instance, Green and Krasno (1988) use previous campaign spending as an instrument for current campaign spending. They argue that variations in candidate spending are caused in part by a candidates’ “taste” for fund-raising; if this taste or propensity to spend could be measured, it would be a valid instrument. Green and Krasno use campaign spending in previous elections as a proxy for the propensity to spend, and this is their downfall. Even if past spending is caused in part by some “taste” for spending, it is also caused in part by candidate personality or other factors. However, such candidate specific factors are omitted variables in their analysis. Therefore, past campaign spending is correlated with omitted variables which cause current electoral success. The apparent effect of campaign spending (instrumented by past campaign spending) on electoral outcomes will be exaggerated because it is also picking up the effect of the important correlated omitted factors. It is not surprising then that Green and Krasno (1988) claim to find relatively large effects of candidate spending on vote share. However, their results are not informative due to this “omitted variable bias”.

If one could somehow measure how personable a candidate is (or is not) and then account for the direct effect of personality on both electoral success and fundraising, it might then be possible to calculate the separate and direct effect of spending on electoral success. But ignoring factors which jointly determine spending and electoral outcomes will confound the measurement of the effects of spending on electoral outcomes, even when instrumental variables are available. Unfortunately, most studies of campaign spending do not appropriately account for this particular form of omitted variable bias.⁴

This paper will focus on two recent second generation studies of House elections: Levitt (1994) and Ansolabehere and Snyder (1996b). Despite the fact that these authors make very similar critiques of the extant literature, they attempt to solve the omitted variable problem in very different ways. Levitt examines repeat meetings of candidates in order to control for candidate specific qualities, while Ansolabehere and Snyder adopt Gerber's (1994) approach in examining novel sources of variation in candidate spending (each of these studies is discussed in more detail below). So how do these "second generation" authors answer the question, "Can candidates buy their way into office with campaign expenditures?" A&S say, "Yes, especially for the case of challengers", while Levitt says, "No." So even though the understanding of the difficulties inherent in estimating the electoral effects of campaign spending has improved, the existing literature does not really inform the policy debate over the desirability of either public financing or further regulation of campaign finance. However, in the subsequent discussion this paper will present evidence which strongly refutes the claims made in Ansolabehere and Snyder (1996b).⁵

3. Natural Experiments on the Electoral Effects of Campaign Spending

The challenge and frustration of social science is that in most instances the luxury of testing hypotheses in controlled and replicable settings is not available. The statistical methods of social science are really tools designed to uncover experiments in an otherwise too noisy environment. Sometimes circumstances provide episodes that closely resemble the conditions of a good experiment. These “natural experiments” are never perfect, but they do afford an opportunity to look at the workings of social relationships from a new perspective.

In this section, several natural experiments on the electoral effects of campaign spending will be examined. None of these episodes are by themselves convincing, but taken together they offer some insight into the counterfactual question: “What would happen to a candidate’s share of the vote if a money-fairy gave that candidate an extra \$100,000 and we then re-ran the election?”

Enid Greene Waldholtz and Karen Shepard

The first experiment to be considered is really a case study of a single House race, but it is a useful starting point for considering the pros and cons of more systematic evidence. In this case, the con is Joe Waldholtz, who also appears in the role of the money-fairy.

In 1992, Enid Greene and Karen Shepard vied for the open second congressional district seat in Utah. Shepard defeated Greene by four percentage points. In 1994, the incumbent Shepard was again challenged by (a newly married) Enid Greene Waldholtz. These two elections have the potential for providing a controlled experiment, since unobservable characteristics of the candidates and district are controlled by the rematch itself. All that is lacking is an exogenous shock to candidate spending.

In 1994, the newly married Enid Greene Waldholtz’s campaign treasurer was her new husband, Joe Waldholtz. During the course of the year, Joe stole several million dollars from his

new father-in-law and apparently funneled about \$1.8 million of the loot into Enid's campaign.⁶ This is about as close as we could hope to come to an exogenous shock to campaign spending: the Waldholtz campaign received a windfall of (illegal) contributions quite independent of Enid's efforts at fund raising or contributors' expectations about her prospects for victory.⁷ Further, the change in Waldholtz's campaign spending from 1992 to 1994 was just about equal to the amount of illegal contributions (see Table One).

With the help of a late media blitz, Enid Waldholtz defeated Karen Shepard in the 1994 midterm election. The final tally gave Waldholtz the nod by ten percentage points. On the face of it, this appears to be an example of campaign spending that had a substantial impact on the election outcome. This is certainly what Karen Shepard was telling anyone with a microphone soon after the news of Joe's creative fund raising surfaced in late 1995. Several opinion writers echoed Shepard's complaints that the election was "bought" with "dirty money." But there is more to the story.

Even if the entire decline in Shepard's vote share (14 percent) is attributed to the illegal shock to her opponent's spending, the implied change in vote share amounts to less than one percentage point for each one hundred thousand dollars in spending. However, Table One shows that Waldholtz's vote share did not change from 1992 to 1994, despite the large increase in her campaign spending. Clearly, many other factors were at work here.

Some of these factors suggest a larger impact of Waldholtz's illegal spending. First, newly-elected members of the House usually improve their vote share in their first run for re-election. Brady, Cogan, and Rivers. (1996) estimate that in 1994 the expected sophomore surge for Democrats was about two percent of the two-party vote. So absent all other factors we might

have expected Shepard's 1994 vote share to be 52 percent, not 36 percent. This unexplained change of sixteen percentage points might well have been caused by Waldholtz's illegal spending. Further, even though Waldholtz's vote share was constant at 46 percent, the anti-Shepard was much probably higher than that in 1994. An independent candidate, Merrill Cook, won 18 percent of the vote. Cook's candidacy probably lowered Waldholtz's vote share, since he was courting Republican voters (as evidence of this, Cook was elected to this seat as the Republican nominee in 1996). If we assign all of Cook's votes to Waldholtz and account for the expected sophomore surge for Shepard, then the apparent change in Waldholtz's vote share from 1992 to 1994 is eighteen percentage points. However, whether we prefer the 16 percentage point figure or the 18 percentage point figure, this still amounts to a little more than one percentage point change for each one-hundred thousand dollars of illegal campaign spending. But even this should be considered a generous upper bound estimate of the effect of campaign spending on vote share.

The preceding analysis ignored several factors which probably served to reduce Shepard's vote share, so it greatly exaggerates the impact of Waldholtz's campaign spending. Cook spent almost \$900,000 in 1996, so not all of the change in Shepard's vote share can be attributed to increased spending by Waldholtz. Further, Shepard's campaign spending also increased from 1992 to 1994. Considering all campaign spending in 1992 and 1994, the net change in non-Shepard spending was about \$2 million. Further, independent candidates won four percent of the vote in 1992. If this vote would have been divided evenly between the two major party candidates, then the unexpected shift in Shepard's vote share shrinks to fourteen percentage points. Further, it is not quite fair to consider all of Cook's votes as "anti-Shepard", some of those votes were surely "anti-Waldholtz", as well. Suppose that the Cook voters would have

gone 2-1 for Waldholtz over Shepard had Cook not run; this would make the unexplained change in vote even lower, about eight percentage points. Just considering these factors, the net change in non-Shepard spending of about \$2 million yields only an eight percentage point decrease in her vote share, or less than one-half of one percentage point per one-hundred thousand dollars.

Once again, even this estimate is probably high, as Shepard alienated many voters by breaking several campaign promises (Milyo and Groseclose, 1996). Nevertheless, this case study is instructive: it suggests that an extra hundred thousand dollars in campaign spending yields at most one percentage point in vote share, but more likely less than half that amount. The only existing study which comes close to predicting this small of an impact of a change in challenger spending is Levitt (1994), so that study merits a closer look.

Repeat Meetings of Incumbents and Challengers

Levitt (1994) also examines a kind of natural experiment on the effects of campaign spending; he focuses on repeat meetings of candidates (like Shepard and Greene/Waldholtz) in order to control for unobserved candidate characteristics. For example, candidates who are articulate and personable probably fare better in elections (all else constant), but these personal characteristics are difficult to measure objectively. By comparing identical pairings of candidates in different elections, Levitt is able to observe the effect of changes in candidate spending on changes in vote share. As long as the candidates personalities do not change from election to election, this omitted determinant of electoral success is irrelevant as a determinant of *changes* in electoral success.

Levitt argues that the previous literature on the electoral effects of candidate spending has not accounted for unobserved heterogeneity in challenger quality. This omission causes ordinary

least squares regression (OLS) estimates of the electoral impact of challenger spending to be biased upward. Levitt estimates the effects of changes in spending on changes in vote share for repeat meetings (controlling for year, party, year*party, incumbency and scandals); this “fixed-effects” model also controls for any time-invariant unobserved heterogeneity in challenger quality (e.g., “personality”).

Levitt finds extremely small coefficients for the candidate spending variables. Further, the coefficients on incumbent and challenger spending are neither significantly different from each other nor jointly significantly different from zero.⁸ From this, Levitt concludes that previous estimates derived from cross-sectional analysis are biased upward and that marginal campaign spending actually has little or no impact on House races.

These findings warrant more scrutiny; Levitt does not control for several potentially important factors and does not check for possible sample selection bias. Accordingly, it is desirable to repeat Levitt’s analysis and check the sensitivity of results to changes in specification which address these criticisms.

This will be accomplished by examining repeat meetings of incumbents and challengers from 1984 to 1992 (n=181). However, this exercise differs from Levitt in two important respects. First, freshmen are omitted. Voters learn a great deal about newly elected representatives from their behavior once in office (e.g., Karen Shepard). This is potentially an important source of omitted variable bias since it is difficult for the researcher to quantify the extent to which constituents are either pleasantly surprised or seriously disappointed by the behavior of their new representative. The data set here covers a shorter time period than Levitt’s, but it includes all House members. So, unlike Levitt, this paper tests for sample selection bias by examining the

process by which challengers decide to run a second time for the same seat.

Table 2 lists some descriptive statistics for repeat and non-repeat challengers (for all House races involving non-freshmen incumbents from 1984-1992). In this table and in all subsequent tables, all dollar amounts are adjusted for inflation and expressed in 1990 dollars. From the table it is clear that repeat challengers tend to spend more and fare worse in terms of vote share than non-repeat challengers. Thus it is perhaps no surprise that Levitt finds little effect from changes in challenger spending. However, it also is the case that repeat challengers are more successful at defeating their incumbent opponents than are non-repeat challengers. Further, seven of the thirteen victorious repeat challengers over this period had prior political experience. This strongly suggests that not all repeat challengers are alike, so it is important to consider the reasons why some challengers decide to run again, while others do not.

The results of several regressions are reported in Table 3; the sources for all variables used in these and subsequent regressions are described in the appendix. Throughout this paper, only coefficient estimates of interest are reported, but full results are available upon request.⁹

In Table 3, column (1) reports estimates for a specification which is very similar to that used by Levitt. The only control variables are dummies for *Year*Party* effects, a continuous variable to control for the fallout of the House Bank scandal of 1992 and *Redistricting*, a dummy variable which takes the value one if the incumbent's district was altered significantly between the 1990 and 1992 elections. The estimated values of the point estimates for spending on vote share reported in column (1) are slightly higher than those found by Levitt (about .004 for all types of candidates versus .001 for incumbents and .003 for challengers), but as in Levitt, the null hypothesis (no effects from spending) can not be rejected at the standard confidence levels.

In column (2), some additional control variables are added into Levitt's basic specification. *Tenure* (in years) is included to capture any life-cycle or learning-by-doing effects for the incumbent. *Coattails* is the share of the two-party vote in the district won by the Presidential candidate of the incumbent's party in the current or most recent election. This variable is interacted with *Party* and *Year* dummies to capture any district-specific (post-) coattail effects from Presidential contests.¹⁰ *Change in Primary Challenge* is a differenced dummy which takes the value one if the incumbent was previously unchallenged in a primary but currently challenged. This variable takes the value negative one if the opposite is true, and it is zero otherwise. It is important to control for the presence of primary challenges because FEC data do not distinguish between money spent in a primary versus in a general election. Contrary to what might have been expected, the only effect of adding these additional controls is to make it even more difficult to reject the null hypothesis that both spending coefficients are zero.

The descriptive statistics for repeat challengers showed that those with political experience were far more successful than other repeat challengers. However, the inclusion of a dummy variable for the eighteen repeat challengers with prior political experience does not alter the conclusion made above.

Finally, in column (3) the results of estimation are given for the same specification in column (2) after controlling for sample selection bias. A conventional two-stage estimator is used. In the first stage the sample considered is all incumbents opposed in their previous race; a probit is estimated for whether the same challenger runs again in the current race. The control variables in this first stage include all of the controls in specification (2) plus the incumbent's vote share in the previous race and dummy variables describing the quality of the previous challenger

(prior political experience or major party endorsement). In the second stage, the null hypothesis of no sample selection bias is strongly rejected, but the correction for this selection has little impact on the estimated effects of candidate spending. In general, the estimated effects of spending are within two one-hundredths of a percentage point of those found by Levitt (1994).

This analysis has also been conducted using the natural logarithm of spending and using the logs of both spending and vote share. In both cases, negative and insignificant effects of incumbent spending and small but significant effects for challenger spending are found. Nevertheless, the results of these estimations are not substantively different from the linear case. For instance, the implied effect of a \$100,000 increase in challenger spending on incumbent vote share (in a competitive race) is about $-.003$, or just about the same as in the linear model.

All in all, Levitt's results seem quite robust to changes in specification and correction for sample selection bias. Further, these results are consistent with the case study of Utah's second district elections discussed above. Still, some caution is in order, if only because the sample of repeat challengers is quite small.¹¹

Instrumental Variables as Experiments:

Ansola-behere and Snyder (1996b) examine House races from 1980-1994; like most previous authors (Levitt is an exception), they use an instrumental variable approach applied to cross-sectional regressions to measure the effects of spending on vote share. However, unlike most previous studies, A&S follow Gerber (1994) and test the validity of several proposed instruments. The most important contribution of their study is the finding that instruments used in previous studies (e.g., lagged vote share or lagged spending) are invalid because they proxy for omitted variables in the vote equation. This implies that most prior studies of House elections

which employ instrumental variables should be ignored.

The set of instruments used by A&S are incumbent war chests, dummies for wealthy challengers or incumbents, a dummy for challengers who are political insiders and dummies for committee assignments. They argue that these variables are true exogenous causes of higher candidate spending and thus an experiment can be performed by comparing the electoral success of candidates with these qualities to those without.

Since it is typically difficult and unpleasant for candidates to raise money, the existence of a large war chests or a personal fortune acts to lower the candidate's cost of raising funds (they simply dis-save from these sources). The political insider variable is meant to capture the fact that many candidates are well connected in party circles but not known to voters. Former House staffers or party officials may thus have an advantage in fund raising without any direct advantage of name recognition among voters.¹² Finally, candidates may be able to parlay important committee assignments into higher campaign receipts, but voters rarely know the committee assignment of their representatives. Consequently, A&S argue that each of these proposed instruments provide a natural experiment: they affect campaign spending but do not directly affect candidate vote share.

This study has replicated the results of A&S for the 1992 House elections; the only fact worth noting about this exercise is that their results do seem to depend on taking logarithms of both vote share and candidate spending. This particular transformation of variables provides estimates of the "elasticity" of vote share with respect to changes in candidate spending, or the percentage change in vote share for a percentage change in spending. A&S find elasticities in the neighborhood of .06 to .08. Thus a 100 percent increase in spending by either incumbents or

challengers will lead to only a six to eight percent increase in that candidates vote share (note: this is an eight percent increase in vote share, not an increase of eight percentage points!). Nevertheless, many challengers spend very little on their campaigns, so an across-the-board increase of \$100,000 in all challenger spending would yield many instances of very large decreases in incumbent vote share (and many more defeats for incumbents); this is in marked contrast to the negligible effects found by Levitt (1994).

This striking difference in results suggest that there may be some problem with the instruments proposed by A&S. In order to explore this possibility, this study next examines how the proposed instruments fare in reduced form equations for a linear specification after first-differencing. In other words, the analysis focuses on the the effect of the levels and changes in the proposed instrumental variables on the levels and changes in candidate receipts, expenditures and vote share. Since many other factors change over time, several other control variables are included (unless noted otherwise, all reported results are robust to minor changes in the set of control variables).

Because reduced-form specifications are examined, the resulting coefficient estimates describe the net effects of exogenous variables on each dependent variable. This exercise is similar to Levitt's analysis in that unobserved incumbent-specific or district-specific characteristics are swept out by the first-differencing. However, it departs from Levitt's approach in one important aspect: since the identity of challengers changes from year to year in most cases, this study can not control for challenger fixed effects. Consequently, the interpretation of results must proceed with caution.

This exercise is similar to A&S's in that the focus is on potentially exogenous changes in

spending and their effects on vote share. However, unlike A&S this analysis also looks for shocks to fund raising and spending. One advantage of examining reduced-forms is that if the proposed instruments affect vote share directly (or if the basic instruments are themselves invalid), then the coefficients in the reduced-form represent *over-estimates* of the direct spending effects of changes in these proposed instruments.

Candidate Wealth as an Experiment

A basic instrument employed in A&S is candidate wealth. They define a candidate as wealthy if a candidate is ever mentioned in *Who's Who of Wealthy People* or is ever described as wealthy in *Congressional Quarterly*. This binary definition of wealth is by construction time-invariant, making it impossible to disentangle the effects of wealth itself from any omitted personal attributes that might be either correlated with or causes of personal wealth (e.g., intelligence, perseverance, education, a famous family name and so on). This would require longitudinal data on a continuous measure of candidate wealth. Such data is not publically available for challengers, but incumbents do file annual financial disclosure reports.

Groseclose (1994) examines incumbent wealth from the 1992 financial disclosure reports of House members. Unfortunately, this data is quite tedious to collect, so a longitudinal data set is not yet available. Consequently, it is not currently possible to examine the effects of changes in wealth on changes in fund raising and electoral success.

Nevertheless, this wealth data can be used to explore how wealthy incumbents fare in their re-election bids. A&S claim that wealth is an instrumental variable for candidate spending in that wealthy candidates may spend more on their campaigns, all else being constant. They argue that wealthy candidates have this advantage for two reasons: 1) current law permits candidates to

spend unlimited amounts of own-source funds on their campaigns and 2) social connections may enhance the ability of wealthy candidates to generate contributions from other wealthy individuals. However, A&S do not offer any evidence of these claims.

Milyo and Groseclose (1998) employ the wealth data from 1992 to show that wealthy incumbents do not appear to have any electoral or fund-raising advantage. While they do find that there exists a great deal of variation in the personal wealth of incumbents, they find this variation has no statistically significant effect on any measures of the electoral or fund-raising abilities of incumbents. Further, if one ignores the conventional constraint of statistical significance, wealthy incumbents actually raise and spend *less* campaign money than their less wealthy counterparts!

A note of caution is in order: if incumbent wealth deters opponents from challenging the incumbent, then one might see these same patterns in the data. However, Milyo and Groseclose also explore this possibility. They find that experienced challengers, usually considered the most formidable opponents, are slightly *more* likely to challenge more wealthy incumbents. Further, using other definitions of challenger quality does not alter the conclusion that incumbent wealth has no discernable deterrent effect on challengers.

The results in Milyo and Groseclose (1998) are from only one election cycle, so they must not be given too much weight. Nevertheless, the finding that incumbent wealth does not deter potential challengers is consistent with several studies that show that the campaign war chests of incumbents do not deter potential challengers (e.g., Ansolabehere and Snyder, 1996a). Below, this analysis provides additional evidence of this and demonstrate that incumbents with large war chests, like wealthy incumbents, do not possess an electoral or fundraising advantage.

Incumbent War Chest as an Experiment

Incumbents often carry over funds from one election cycle to the next; these war chests are potential instrumental variables, since incumbents with large war chests probably spend more than other incumbents, all else being constant. This is the argument made by both Gerber (1994) and Ansolabehere and Snyder (1996b); however, in neither case do the authors provide evidence for this claim. From the descriptive statistics in Table Four, it appears to be the case that incumbents with large war chests fair better than other incumbents in that they are slightly less likely to be defeated and receive a higher share of the vote. However, these incumbents do not raise or spend more money, rather their success appears to be attributable to the fact that their opponents are of lower quality (they spend less money and are less likely to have prior political experience). These descriptive statistics run counter to the claims in both Ansolabehere and Snyder, 1996a and 1996b.

Table Five provides more systematic evidence on the question of whether incumbent war chests deter quality challengers. This table and all subsequent tables reporting regression or probit results show the estimated coefficient for the independent variable of interests for two different sets of control variables. One set is sparse; it contains controls for only year effects, party effects, the interaction of year and party effects and tenure effects. The other set is rich; it also contains controls for committee and leadership positions in the House interacted with party (these are described in detail below), the previous vote share of the incumbent (a proxy for the baseline popularity of the incumbent), and dummy variables for the presence and quality of challengers (as described in Table Four). As there is no obviously correct set of control variables, results are presented for these two different extreme cases. Fortunately, the substantive interpretations of the estimated coefficients of interest are always robust to the choice of control

variables. For example, in Table Five, whether one prefers the specification with more or fewer controls for other factors, a \$100,000 change in an incumbent's war chest has only a trivial effect on the probability that the incumbent is opposed or opposed by a high quality challenger (defined as a challenger with prior experience in elective office or some celebrity status). Recall from Table Four that the mean incumbent war chest is about \$100,000; so these results suggest that even dramatic changes in incumbent war chests have negligible effects on the presence or quality of challengers. This is difficult to reconcile with the notion that campaign spending drives electoral success, but it is (once again) consistent with previous studies and with the "non-deterrence" findings for incumbent wealth. However, the claim that incumbents with large war chests spend more does not receive such a reprieve once we control for other factors.

The results in Table Six show that incumbents with large war chests do win reelection by greater margins, but they do not spend more money than other incumbents. This may be an artifact of omitted variables. Presumably, "good" incumbents are more likely to have had the luxury of building up a large war chest. But these same "good" incumbents also are more likely to win reelection with less effort (spending). Consequently, if incumbent quality is not fully accounted for in the regression analysis, the apparent effects of the war chest variable on spending will be biased downward (by this same reasoning, the apparent effects of the war chest variable on vote share will be biased upward).

To better control for unobserved incumbent quality, consideration is given to the effects of changes in an incumbent's war chest on changes in vote share and changes in campaign spending. The results in Table Seven do show that those incumbents who have greater increases in their war chests do spend more money in their subsequent reelection bids. However, these

same incumbents actually fare worse in terms of vote share. How can this be? It appears that incumbents who grow their war chest end up facing better challengers, since the change in war chest variable is associated with a significant increase in challenger spending. This implies that incumbents save campaign funds for the future in anticipation of a high quality challenge to their next reelection bid.

This anticipation of future challenges may explain why it appears that incumbent war chests do not appear to deter challengers. But this reverse causality should not confound the results for incumbent wealth; so this does not completely salvage the claim that incumbent war chests play some role in deterring challengers. More importantly for Ansolabehere and Snyder (1996b), these results show that either the incumbent war chest variable is not a good instrument for uncovering the direct effects of campaign spending on vote share, or if it is a good instrument, the apparent effects on vote share from this exogenous shock to spending are non-positive! In either case, the second generation research that argues for significant electoral effects of campaign spending receives a second blow.

Committee and Leadership Positions as Experiments

It is well documented that party leaders and members of certain committees raise more money than other incumbents [e.g., Grier and Munger (1991a and 1991b) and Romer and Snyder (1994)]. Ansolabehere and Snyder (1996b) argue that committee assignments and leadership positions have little direct influence on voters, therefore these are good candidates for instrumental variables. A&S focus on three committee assignments as particularly useful for raising money: Appropriations, Energy and Commerce and Ways and Means; following A&S, a dummy variable is constructed which indicates an assignment to these “good committees”. Since

the minority party is less likely to reap the benefits of the committee system, an interaction is included for Republicans on one of these good committee. For similar reasons, two dummy variables are created for party leaders (defined as the Speaker, the Majority and Minority Party Leaders, committee chairmen and ranking members and all members of the Rules Committee).

The regression results in Table Eight demonstrate that Democrats on good committees or in leadership positions do raise and spend significantly more than other incumbents. However, in neither case do these privileged members fare significantly better in their vote share. Further, as more control variables are added, the effect on vote share becomes negative. This is not strong support for the claim that committee assignments or leadership positions are useful instrumental variables.

The point has been previously raised that assignment to committees or promotion to leadership may itself be influenced by unobserved characteristics of the incumbent. To the extent this is true, the results in Table Eight actually *exaggerate* the financial and electoral effects of committee assignments and leadership. As before, this omitted variable problem can be mitigated by examining the effects of changes in committee assignments and leadership positions on changes in campaign finance and vote share. The results in Table Nine do show a far more modest effect of committee assignments and leadership on fundraising. In fact, the changes in receipts are statistically significant in only one case and the changes in expenditures are not significant in any case. Further, the effects on vote share remain insignificant and negative. Once again, we are left to conclude that either committee assignments and leadership positions are not good instruments, or that marginal campaign spending by incumbents has no discernable effect on incumbent vote shares.

Shocks to Committee Power as an Experiment

Elsewhere (Milyo, 1997b), it is argued that the major budget and tax reforms of the mid-eighties, Gramm-Rudman-Hollings I and the Tax Reform of 1986, caused windfall contributions to the members of the House Budget and Ways and Means committees, respectively.¹³ Both of these pieces of legislation were developed and passed between the 1984 and 1986 elections. Consequently, this episode of budget and tax reform offers a natural experiment on the effect of an increase in marginal campaign spending by incumbents. Incumbents on either of these two committees throughout this time period should realize a shock to fundraising, which may lead to higher campaign spending and vote share.

Table 10, examines changes in campaign finance and vote share from 1984 to 1986. The independent variables of interest are two dummy variables indicating continuous membership on either the Budget or Ways and Means committees during this time period.

The regression results confirm that members of Budget and Ways and Means each realized a large and significant positive shock to receipts during the 1986 election cycle. However, there is no statistically significant shock to either campaign spending or vote share. Further, the coefficients on vote share are negative. As in the previous examples, the results hold with either more or fewer control variables.

Milyo (1997b) also shows that Budget and Ways and Means members appear to have saved much of their increased receipts (this is also implied by the results in Table 10). Thus a major drawback of all the “experiments” presented thus far is that even if shocks to fund raising ability can be found, the incumbent’s choice to raise and spend more money is determined by the quality of the incumbent’s opponent. One may identify a shock to fund-raising, but that does not

necessarily imply a shock to spending. For the case of a windfall of campaign receipts, secure incumbents will save more of the windfall, while insecure incumbents will spend more of it. It is therefore crucial to control for the quality of the incumbent and the challenger. Otherwise, the estimated coefficient of interest will merely be an average of these two contradictory cases, leading to an underestimate of the direct effects of (instrumented) spending on vote share.

However, the controls that have been selected for incumbent and challenger quality are fairly typical for the literature on the electoral effects of campaign spending. Therefore, the only way to render consistent the results of the experiments presented here with the existence of significant electoral effects of campaign spending is to accept the claim that the included controls for challenger quality are not adequate. But this is exactly the argument made by Levitt (1994) and the motivation for his examination of repeat meetings of candidates. So the very attempt to dismiss the findings here of negligible effects of marginal incumbent spending serves to strengthen the case for Levitt's finding of negligible effects of either incumbent or challenger campaign spending (marginal and inframarginal).

4. Conclusion

There is a common methodological approach taken in most studies of the electoral consequences of campaign spending: instrumental variable estimation of cross-sectional data on candidate vote shares across races.¹⁴ To the vexation of researchers, it is difficult to find legitimate instruments for candidate spending. It is hard to imagine a determinant of campaign spending in House races which is not itself either a determinant of electoral success or a proxy for some unobserved determinant of electoral success (integrity, leadership skills, wisdom and so on).

Like Grier (1989), Gerber (1994) and Levitt (1994) before, Ansolabehere and Snyder (1996b) have done a great service to students of campaign finance and elections by articulating in a very clear and accessible fashion the reasons that previous attempts to measure the electoral effects of candidate spending are flawed. Together, Gerber and A&S have proposed and examined some novel instruments: wealth, war chests and committees. Employing these instruments they find relatively large and significant effects of challenger and incumbent spending. However, the analysis here has shown that at least for the case of House incumbents, these instruments are not associated with increases in incumbent vote share. This implies that either or both of the following is true: marginal campaign spending by incumbents has little effect on electoral outcomes, or the proposed instruments are invalid. In either case, the empirical findings of A&S are overturned. However, the same can not be said of Grier (1989) or Gerber (1994); future work should explore similar natural experiments in Senate elections.

To avoid the conclusion that marginal campaign spending by incumbents has negligible effects on vote share, one must embrace the claim that the proposed instruments (wealth, war chests and committees) are invalid because they are correlated with important omitted variables, for instance unobservable attributes of candidate quality. However, this study has also demonstrated that changes in war chests, committees and leadership have no effect on vote share. To the extent that unobservable attributes are constant across time, this again suggests that marginal incumbent spending has little impact on vote share.

The empirical strategy of examining differences in instruments on differences in vote share is subject to the important criticism that in most cases the identity of the challenger changes from election to election. Consequently, to the extent that challenger quality is difficult to observe and

quantify, these regressions do not appropriately control for challenger quality. This is problematic, since the presence of a “surprisingly” (or unobserved) high quality challenger can be expected to have a positive effect on incumbent effort (spending) and a negative effect on incumbent success (vote share). Therefore, the repeated findings that shocks to incumbent spending are not associated with positive shocks in incumbent vote share may be attributable to the repeated mis-measurement of challenger quality.

But this is exactly the motivation for Levitt’s (1994) study of repeat meetings of candidates ---- and he finds negligible effects of spending on vote share *in general*, not just for marginal spending by incumbents. Of course, Levitt’s study is subject to criticism, but this paper has demonstrated that his results are quite robust to alternative specifications.

This study has also presented a case study of a substantial shock to challenger spending that seems to have had limited impact on vote shares. Further, neither incumbent wealth nor incumbent war chests appear to be important deterrents to potential challengers or potential high quality challengers. All of this is consistent with the existence of limited electoral effects of at least marginal campaign spending.

Lessons for Future Research

There are several lessons here for future research. First, the fact that shocks to fundraising ability do not necessarily translate into significant shocks to expenditures suggests that not all incumbents attempt to maximize vote share in the current election. The allocation of incumbent effort between reelection and other goals is a possible confounding factor that has received little attention (for an exception, see Milyo, 1997a). Future studies of campaign spending must take account of the fact that many incumbents do not run “all out.” Next, it is

imperative to account for unobserved candidate quality; simply including control variables for selected and easily observable attributes of candidates appears to be insufficient in these natural experiments. Third, researchers need to be more entrepreneurial in constructing truly exogenous variables which determine candidate spending but not vote share. Measures of candidate wealth hold some promise. This wealth data needs to be collected for more years to permit a more detailed analysis. Fourth, any studies which show significant effects of campaign spending should also be able to explain the failure to find such effects among repeat challengers or any of the other experiments considered here. On the other hand, studies which show negligible effects of spending in House elections must explain why Senate elections appear to be different.

Finally, if money doesn't matter "much", then why do candidates work so hard to raise funds? Perhaps it is because candidates can do little else to try to influence voters (Levitt, 1995), but this is mere speculation in need of empirical study. Besides, if campaign spending does not influence electoral outcomes, then campaign contributions are ineffective either as a way to influence elections or as a way to influence legislator actions. Despite this, Americans are contributing increasing amounts to political campaigns. Any claim that money does not matter in politics should also explain why money flows to campaigns and why the flow is increasing.

In summary, academic research on money in politics has a long way to go. Money may well play an important role in elections and politics in general, but we just don't have much good evidence in support of that conventional wisdom. Some scholars of campaign finance have opined that the last word on this subject has already been said; if nothing else, this study should convince the reader otherwise.

Lessons for Reform Policy

Campaign finance reform has always presented difficult trade-offs between the ideals of equality of participation and freedom of speech. But recent research on campaign finance suggests that some modest reforms exist which do not do violence to freedom of speech.

The evidence presented here strongly suggests that marginal spending by incumbents has little impact on their electoral success. Even shocks to spending of \$100,000 or more produce no discernable impact on incumbent vote shares. Since (hard money) campaign contributions are limited by law to \$10,000 per election cycle per candidate for political action committees and \$2,000 for individual contributors, it is difficult to imagine that any single campaign contribution can elicit much genuine gratitude from an incumbent.

It is nevertheless plausible that voters, contributors and perhaps even candidates are misinformed about the effects of marginal campaign spending. One avenue for reform then, is simply to disseminate the results of recent research. This alone could ease the wide-spread discomfort with the existing system of campaign financing of Congressional elections and perhaps even alter the undesirable behavior of some candidates and contributors.

The second potential reform is to encourage more individuals to make campaign contributions (much in the same way that individuals are encourage to go to the polls on election day). Since the marginal value of campaign spending decreases with additional campaign spending (Milyo, 1997a), an increase in available campaign funds reduces the value of any particular campaign contribution. In addition, an increase in “dis-interested” individual contributions from ordinary citizens also lowers the marginal value of money spent on unregulated activities (soft money contributions to parties and generic issue advocacy campaigns). In contrast, limits on candidate expenditures or campaign contributions serve only to raise the value

of these unregulated activities. Since fewer than five percent of adults currently make political contributions of any kind (Milyo, 1997c), there is the potential for a dramatic increase in contributions from ordinary citizens. By encouraging a flood of dis-interested money, it is possible to reduce the influence of interested money without sacrificing freedom of speech.

Notes

1. This refers to legal contributions to candidates, or “hard money”; large soft money contributions to political parties or large “issue-advocacy” expenditures are likely to be more efficacious in influencing party leaders.
2. This reverse causality works to understate the apparent effect of incumbent spending on incumbent vote share and to overstate the apparent effect of challenger spending on incumbent vote share. For example, the coefficient of correlation between incumbent spending and incumbent vote share is $-.43$, while the coefficient of correlation between incumbent spending and challenger spending is $.52$ (for all House races involving incumbents from 1984-1992).
3. See note 4, below.
4. In technical jargon: in the presence of unobserved fixed effects, lagged endogenous variables (e.g., previous spending) are not valid instruments for candidate spending.
5. The findings of this paper are also in contrast to those of the second generation studies of Senate elections, Grier (1989) and Gerber (1994). However, a fair criticism of those studies must examine Senate elections.
6. News reports place the illegal contribution between \$1.7 million and \$1.9 million; the exact amount is not known for certain.
7. This is not a perfect experiment though; perhaps Joe bilked his father-in-law because he thought that Enid’s chances of winning were slim. In that case the windfall was not truly exogenous. However, Joe Waldhotz appears to have squirreled away several million for himself (still unaccounted for). Further, it has since been revealed that he is a professional con man who married Enid Greene under false pretenses. Finally, when Federal officials started asking questions about irregularities in Enid Waldholtz’s campaign finances, Joe went left, abandoning Enid and their newborn infant. Given these facts, it is doubtful that Joe’s sole or even primary motive was to help Enid win office.
8. Levitt also finds that the effects of campaign spending in the second meeting are not significantly different than in the first meeting.
9. For all regression results, the reported standard errors are heteroskedastic-consistent (White’s method).
10. A control for Ross Perot’s district vote share in 1992 is also included.
11. The small sample makes it impossible to use variables such as changes in committee assignment or promotions to leadership positions to instrument for changes in spending, since

promotions and transfers are extremely rare events. This is unfortunate, since changes in candidate spending are likely to be correlated with omitted time-variant factors (although does not appear to have been the case with Karen Shepard).

12. However, endorsements and press coverage are likely to provide name recognition to these political insiders.

13. It is no surprise that major tax reform should cause interested parties to contribute more to members of Ways and Means, but it is somewhat surprising that GRH would cause a similar windfall for the members of the Budget committee. However, GRH expanded the scope and improved the enforcement of the budget resolution, which is in the jurisdiction of the Budget committee.

14. Most studies of campaign finance examine House races simply because there are more races per election year than for the Senate. However, Senate elections do offer an advantage: more data on possible control variables is available at the state level than the district level.

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Appendix

Data Sources:

Incumbent wealth data was provided by Tim Groseclose; he collected this information from financial disclosure reports on file with the Clerk of the U.S. House. All information on campaign finance is from the Federal Election Commission. Information on challenger backgrounds, number of checks kited and redistricting is taken from various issues of *Congressional Quarterly Weekly Reports*. All other data is taken from various editions of the *Almanac of American Politics*.

Table 1: Second Congressional District of Utah, 1992 - 1994

Candidate	1992 Open Seat Election		1994 Rematch	
	Vote	Expenditures	Vote	Expenditures
Karen Shepard (D)	50%	\$650,000	36%	\$1,045,000
Enid Greene/Waldholtz (R)	46	470,000	46	2,176,000
Merrill Cook (I)	--	--	18	879,000
Others	4	11,000	--	--

Table 2: Descriptive Statistics for Repeat Challengers
 (Non-Freshman Incumbents Opposed in Two Consecutive Races; n=1331)

Means and Standard Deviations (except as noted)	Different Challengers	Same Challenger as Previous Race	H ₀ : Same
Number	1150	181	n.a.
Incumbent Vote Share (top two vote-getters only)	.68 (.11)	.66 (.11)	p<.01
Incumbent Expenditures (constant 1990 dollars)	443,180 (317,044)	471,858 (394,839)	p<.20
Challenger Expenditures (constant 1990 dollars)	111,784 (190,528)	182,463 (284,117)	p<.01
Incumbent War Chest (beginning cash on hand less debts)	117,446 (185,118)	127,214 (159,990)	p<.30
Incumbent Expenditures ÷ Incumbent Receipts	.85 (.16)	.80 (.20)	p<.01
Number of Challengers with Prior Political Experience (%)	170 (13%)	18 (10%)	p<.20
Incumbent Defeats (%)	35 (3%)	13 (7%)	p<.01

Notes: “H₀: Same” reports the p-value from a one-tailed difference in mean or proportion test.

Table 3: Regression Estimates for Repeat Challengers
(Non-freshmen incumbents running for re-election, 1984-1992; n=181)

Coefficient Estimates (Standard Errors)	(1) Change in Vote Share	(2) Change in Vote Share	(3) Change in Vote Share
Incumbent Expenditures (\$100,000)	.0041 (.0025)	.0032 (.0027)	.0024 (.0023)
Opponent Expenditures (\$100,000)	-.0036 (.0031)	-.0030 (.0032)	-.0049 (.0028)
H ₀ : Spending Coefficients = 0	p<.20	p>.30	p<.20
R ²	.28	.32	.49
Controls for:			
Year*Party	yes	yes	yes
Redistricting	yes	yes	yes
Log(1+Bad Checks)	yes	yes	yes
Tenure	no	yes	yes
Coattails*Party*Year	no	yes	yes
Change in Primary Challenge	no	yes	yes
Correct for Sample Selection	no	no	yes
Mean of Dependent Variable (Standard Deviation)	.0090 (.0863)	.0090 (.0863)	.0090 (.0863)

Notes: **p<.01; *p<.05 (two tailed test). Vote share is the incumbent's share of the total votes for the top two vote-getters. Sample selection correction is performed with a two-stage estimation (the excluded variables are previous vote share, war chest and quality of previous challenger). The null hypothesis of no sample selection bias is rejected (t=6.1). All regression results correct for heteroskedasticity using White's method.

Table 4: Descriptive Statistics on Incumbent War Chests and Electoral Success
(Incumbents Running for Reelection, 1984-1992; n= 1924)

	All	Incumbent War Chest:	
		Above Median vs. Below Median	Top Quintile vs. Bottom Quintile
	Mean (Standard Deviation):	Difference in Means	
War Chest	\$104,861 (178,536)	223,968**	608,718**
Incumbent Vote Share	.71 (.14)	.05**	.07**
Incumbent Expenditures	\$439,430 (319,957)	-29,984*	-131,329**
Incumbent Receipts	\$474,768 (295,817)	-32,963**	-105,908**
Challenger Expenditures	\$124,690 (219,100)	-85,894**	-195,090**
	Proportion of Incumbents:	Difference in Proportions	
Opposed in Primary	.29	-.07**	-.13**
Opposed in General Election	.90	-.05**	-.04
Opposed by a Major Party Challenger	.85	-.06**	-.05
Opposed by a Challenger with Prior Political Experience	.13	-.08**	-.17**
Defeated	.04	.03**	-.05*

Notes: **p<.01; *p<.05 (one-tailed test). War Chest is ending cash on hand less debts reported at the end of the previous election cycle. The median war chest is \$60,300; The cut-off for the top quintile is \$305,000 and the cut-off for the bottom quintile is \$-28,820. All dollar amounts are in constant 1990 dollars.

Table 5: Estimated Effects of War Chests on the Presence and Quality of Challengers
(All incumbents running for reelection, 1984-1992; n=1924)

Probit Results Coefficient Estimate (Standard Error)	Dependent Variables:			
	(1) Opposed in Primary	(2) Opposed in General	(3) Major Party Challenger	(4) Experienced Challenger
War Chest (\$100,000)	-0.07** (0.02)	-0.04* (0.02)	-0.04* (0.02)	-0.11** (0.03)
Change in Probability:				
Increase in War Chest	-2.4%	-0.7%	-0.9%	-2.2%
Decrease in War Chest	2.4%	0.6%	0.8%	2.6%
Other Independent Variables: Year*Party, Tenure and Tenure Squared				
	(5)	(6)	(7)	(8)
War Chest (\$100,000)	-.06** (0.02)	-0.01 (0.03)	-0.00 (0.02)	-0.07** (0.03)
Change in Probability:				
Increase in War Chest	-2.1%	-0.2%	-0.1%	-1.4%
Decrease in War Chest	+2.1%	0.2%	0.1%	1.5%
Other Independent Variables: Year*Party, Tenure, Tenure Squared, Committee*Party, Leadership*Party, Previous Vote Share for Incumbent and Previous Presence and Quality of Challengers				
Mean of the Dependent Variable	.29	.90	.85	.13

Notes: **p<.01; *p<.05 (two-tailed test). Change in probability is calculated for an incumbent with a base probability equal to the mean of the dependent variable (e.g., for a \$100,000 increase in War Chest, the probability that the incumbent is opposed in the primary decreases from 29% to either 26.6% or 26.9%).

Table 6: Estimated Effects of War Chests on Spending and Vote Share
 (All incumbents running for reelection, 1984-1992; n=1924)

Regression Results	Dependent Variables:			
	(1)	(2)	(3)	(4)
Coefficient Estimate (Standard Error)	Incumbent Expenditures	Incumbent Receipts	Challenger Expenditures	Incumbent Vote Share
War Chest (\$100,000)	5,323 (5,510)	5,753 (5,589)	-17,568** (3910)	.011** (.002)
	R ² =.10	R ² =.06	R ² =.06	R ² =.08
Other Independent Variables: Year*Party, Tenure and Tenure Squared				
	(5)	(6)	(7)	(8)
War Chest (\$100,000)	-1,861 (5,167)	8,144 (5432)	-1,345** (371)	.007** (.001)
	R ² =.22	R ² =.16	R ² =.18	R ² =.64
Other Independent Variables: Year*Party, Tenure, Tenure Squared, Committee*Party, Leadership*Party, Presence and Quality of Challengers				
Mean of the Dep. Variable (Standard Deviation)	\$439,430 (319,957)	\$474,768 (295,817)	\$124,690 (219,100)	.71 (.14)

Notes: **p<.01; *p<.05 (two-tailed test).

Table 7: Estimated Effects of Changes in War Chests on Changes in Spending and Vote Share
(All incumbents running for reelection, 1984-1992; n=1924)

Regression Results	Dependent Variables:			
	(1)	(2)	(3)	(4)
Coefficient Estimate (Standard Error)	Change in Incumbent Expenditures	Change in Incumbent Receipts	Change in Challenger Expenditures	Change in Incumbent Vote Share
Change in War Chest (\$100,000)	82,932** (9,791)	3,852 (8,616)	41,153** (10,698)	-.023** (.004)
	R ² =.13	R ² =.06	R ² =.03	R ² =.07
Other Independent Variables: Year*Party and Tenure				
	(5)	(6)	(7)	(8)
Change in War Chest (\$100,000)	68,719** (8,763)	-6,864 (8,189)	31,365** (9,923)	-.009** (.002)
	R ² =.24	R ² =.15	R ² =.13	R ² =.65
Other Independent Variables: Year*Party, Tenure, and changes in {Committee*Party, Leadership*Party and Presence and Quality of Challengers}				
Mean of the Dep. Variable (Standard Deviation)	\$56,800 (303,440)	\$48,343 (262,587)	-\$778 (275,118)	.0009 (.1499)

Notes: **p<.01; *p<.05 (two-tailed test).

Table 8: Effects of Committee and Leadership Positions on Spending and Vote Share
(All incumbents running for reelection, 1984-1992; n=1924)

Regression Results	Dependent Variables:		
	(1) Incumbent Expenditures	(2) Incumbent Receipts	(3) Incumbent Vote Share
Coefficient Estimate (Standard Error)			
Good Committee	94,315** (19,768)	92,329** (18,231)	.005 (.009)
Good Committee*Republican	-92,331** (31,581)	-84,756** (28,930)	.013 (.014)
Leadership	139,962** (35,976)	144,105** (34,905)	.008 (.013)
Leadership*Republican	-138,386** (46,769)	-107,364* (45,329)	-.008 (.018)
	R ² =.12	R ² =.08	R ² =.06
Other Independent Variables: Year*Party, Tenure and Tenure Squared			
	(5)	(4)	(6)
Good Committee	106,436** (18,589)	99,100** (17,458)	-.001 (.006)
Good Committee*Republican	-102,674** (29,713)	-94,118** (27,637)	.019* (.008)
Leadership	143,702** (34,696)	144,961** (33,689)	-.001 (.008)
Leadership*Republican	-142,079** (44,909)	-110,411* (44,269)	.009 (.011)
	R ² =.22	R ² =.22	R ² =.64
Other Independent Variables: Year*Party, Tenure, Tenure Squared, War Chest, Presence and Quality of Challengers			
Mean of the Dep. Variable (Standard Deviation)	\$124,690 (219,100)	\$474,768 (295,817)	.71 (.14)

Notes: **p<.01; *p<.05 (two-tailed test).

Table 9: Effects of Transfers and Promotions on Changes in Spending and Vote Share
(All incumbents running for reelection, 1984-1992; n=1924)

Regression Results	Dependent Variables:		
	(1)	(2)	(3)
Coefficient Estimate (Standard Error)	Change in Incumbent Expenditures	Change in Incumbent Receipts	Change in Incumbent Vote Share
Transfer onto Good Committee (n=74)	76,993 (56,810)	85,871 (54,710)	-.033 (.023)
{Transfer onto Good Committee} *Republican (n=30)	-45,919 (71,139)	-49,109 (65,902)	.010 (.035)
Promotion to Leadership (n=79)	32,539 (43,439)	40,382 (39,959)	-.007 (.027)
{Promotion to Leadership} *Republican (n=30)	27,806 (60,827)	19,095 (52,661)	-.002 (.037)
	R ² =.14	R ² =.07	R ² =.06
Other Independent Variables: Year*Party and Tenure			
	(4)	(5)	(6)
Transfer onto Good Committee (n=74)	87,685 (50,547)	94,814* (49,203)	-.009 (.149)
{Transfer onto Good Committee} *Republican (n=30)	-87,621 (64,858)	-80,814 (61,386)	-.000 (.022)
Promotion to Leadership (n=30)	29,137 (40,809)	37,936 (38,076)	-.014 (.016)
{Promotion to Leadership} *Republican (n=79)	18,439 (57,058)	11,191 (51,079)	.003 (.020)
	R ² =.24	R ² =.15	R ² =.65
Other Independent Variables: Year*Party, Tenure and changes in {War Chest and the Presence and Quality of Challengers}			
Mean of the Dep. Variable (Standard Deviation)	\$56,800 (303,440)	\$48,343 (262,589)	-.001 (.012)

Notes: **p<.01; *p<.05 (two-tailed test).

Table 10: Estimated Effects of Shocks to Committees Power
on Changes in Spending and Vote Share
(All incumbents running for reelection, 1984-1986; n=385)

Regression Results	Dependent Variables:			
	(1)	(2)	(3)	(4)
Coefficient Estimate (Standard Error)	Change in Incumbent Expenditures	Change in Incumbent Receipts	Change in Challenger Expenditures	Change in Incumbent Vote Share
Ways & Means, 1984-1986 (n=25)	113,653 (62,300)	126,710* (54,170)	36,727 (34,148)	-.041 (.034)
Budget, 1984-1986 (n=15)	67,104 (72,663)	142,601* (67,174)	-1,113 (36,338)	-.054 (.038)
	R ² =.05	R ² =.07	R ² =.02	R ² =.06
Other Independent Variables: Party and Tenure				
	(5)	(6)	(7)	(8)
Ways & Means, 1984-1986 (n=25)	78,893 (50,418)	111,105* (50,181)	1,927 (33,239)	-.009 (.011)
Budget, 1984-1986 (n=15)	42,720 (68,922)	122,486 (63,133)	-23,001 (35,080)	-.016 (.034)
	R ² =.21	R ² =.17	R ² =.08	R ² =.72
Other Independent Variables: Party, Tenure and changes in {Committee*Party, Leadership*Party, War Chest and the Presence and Quality of Challengers}				
Mean of the Dep. Variable (Standard Deviation)	\$33,860 (236,249)	\$39,140 (215,122)	\$-22,949 (245,053)	.035 (.144)

Notes: **p<.01; *p<.05 (two-tailed test). Two incumbents are members of both Ways and Means and Budget throughout this period.

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