

**FOUR ESSAYS IN POSITIVE
POLITICAL ECONOMY**

by

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Abstract

Understanding the role of government and its impact on the economy is a central focus of both economics and political science. Economists have traditionally devoted a great deal of attention to measuring the effects of government activities and the optimal design of such programs. Political scientists, meanwhile, have obtained a great deal of insight into the question of how decisions are actually made within the government. A range of critically important issues, however, have received less study precisely because they lie at the intersection of the two disciplines. This dissertation focuses on two such questions: whose interests do elected officials actually represent, and what factors determine election outcomes.

The first chapter examines the relationship between campaign spending and election outcomes. Reliable estimates of the effects of campaign spending are difficult to obtain due to the inability to control for unmeasured differences in the inherent ability of candidates to attract votes. To avoid the bias that arises from unobserved candidate quality, this chapter looks only at pairs of elections in which the same two candidates run against each other on more than one occasion. Under the assumption that an individual candidate's quality remains constant over time, a fixed-effects panel model using only those elections will eliminate the bias from unobserved candidate quality. In stark contrast to previous work, campaign spending is found to have an extremely small impact on election outcomes regardless of incumbency status. My estimates of the impact of challenger spending are an order of magnitude below those of cross-sectional studies. Despite relatively small standard errors on the estimates, the null hypothesis that campaign spending has no effect on election outcomes cannot be rejected.

Chapter 2 focuses on the electoral advantage enjoyed by incumbents in the House. This chapter builds on many of the insights of Gelman and King(1990), but by extending the analysis from a cross-sectional approach to panel data, has a number of important

advantages. First, panel data avoids possible biases due to district-specific characteristics and serially correlated errors. Equally important, the model presented here allows the various sources of the incumbency advantage to be disentangled. Both perks of office and the ability to deter high-quality challengers are important components of the observed incumbency advantage, with the latter being the primary source of growth over the last two decades.

Chapter 3 tests competing explanations of the midterm gap. In the thirty-two midterm elections since the Civil War, the party of the incumbent president has lost seats in the House all but once. There are three leading explanations for the midterm gap: withdrawn coattails, systematic presidential punishment, and reversion to the mean in economic performance. The results of the chapter suggest that each of the three theories has an explanatory role. The impact of each, however, is substantially smaller than previous estimates would suggest. Interestingly, voters appear to systematically punish the party of the incumbent president not only at the midterm, but also in on-year elections. That result is interesting not only because it is in stark contrast to the incumbency advantage in the House, but also because it is difficult to reconcile with rational voting behavior.

The final chapter examines the question of whose interests senators represent. This chapter develops a methodology for estimating the relative weights that senators place on various factors that does not require senator ideologies to be observed in order to yield consistent estimates. The key assumptions required to identify the model are that senator ideologies do not change over time, that state voter preferences are adequately proxied by the voting behavior of a state's House delegation, and that a senator's core constituency can be approximated by the senator's party within the state. If those assumptions hold, the absence of a proxy for senator ideologies does not pose a problem. In fact, estimated ideologies can be backed out from the parameter estimates. Senators appear to be heavily influenced by their own ideologies, assigning approximately thirty to forty percent of the weight to that factor. While some weight is assigned to the preferences of voters in the state, but outside of the senator's "core constituency," approximately three times as much weight is given to the preferences of voters within the "core constituency." Estimates of the importance of the national party line vary from about twenty-five percent of the senators' weight down to ten percent. The weight given to state voter preferences rises sharply as elections approach, accompanied by a decline in the importance of the party line.

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Of course, all remaining errors are my own.

Biographical Information

Steve Levitt was born in Boston on May 29, 1967. He grew up in Minneapolis, Minnesota, where he attended Saint Paul Academy High School. Upon graduation from high school, he went to Harvard College, where he earned a bachelors degree in economics, graduating Summa Cum Laude in 1989.

For the following two years he worked as a management consultant for the Boston-based firm Corporate Decisions, Inc. In 1991, he returned to academics, enrolling in the graduate economics program at MIT. He has studied at MIT until the present time, May 1994. Besides the work in this thesis, his research conducted at IT includes the following unpublished manuscripts:

"The Effect of Police on Crime." 1994.

"Congressional Delegation Seniority and State Economic Performance." 1994.
(with James M. Poterba)

"Optimal Incentive Schemes when Only the 'Best' Agent's Output Matters to the Principal." 1994.

"Political Parties and the Distribution of Federal Outlays." 1994.
(with James M. Snyder, Jr.)

"Congressional Campaign Finance Reform." 1994.

"Private Information as an Explanation for the Use of Jail Sentences Instead of Fines." 1993.

"Labor Mobility, Spillovers, and the Inefficiency of Local Public Good Provision." 1993.

"The Transition from Hunting and Gathering to Agriculture: Explaining the Paradox of Declining Health." 1993.

Steve Levitt is currently a Ph.D. candidate at MIT. Beginning in July 1994, he will begin a three year appointment as a junior fellow with the Harvard Society of Fellows.

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Introduction

Understanding the role of government and its impact on the economy is a central focus of both economics and political science. Economists have traditionally devoted a great deal of attention to measuring the effects of government activities and the optimal design of such programs. Political scientists, meanwhile, have obtained a great deal of insight into the question of how decisions are actually made within the government. A range of critically important issues, however, have received less study precisely because they lie at the intersection of the two disciplines. This dissertation focuses on two such questions: whose interests do elected officials actually represent, and what factors determine election outcomes.

The standard model used by economists to describe the behavior of elected officials is the median voter theorem. Elected officials who care only about holding office and choose policies in a one dimensional space will pursue the path most preferred by the median voter. One implication of the median voter theorem is policy convergence; all candidates representing the electorate will behave identically. As a consequence, the outcome of elections are unimportant. In practice, however, the median voter theorem does not appear to be a realistic description of actual behavior on the part of politicians. One clear counter-example to the median voter theorem is the U.S. Senate. Senators from the same state --and therefore representing exactly the same electorate-- have radically different voting records.

In response to the failure of the median voter theorem as a positive description

of politician behavior, a number of alternative theories have been proposed. Fiorina (1974) develops a theory of "dual constituencies" in which elected officials represent only the interests of those voters who supported them in the previous election. If contributions of time or money are important inputs to successful campaigning, cultivating a "core constituency" may be necessary for re-election. A second alternative, proposed by Kalt and Zupan(1984), applies the principal-agent model to politicians. If politicians have objectives that do not perfectly match the median voter's preferences, and there is electoral slack, then the opportunity arises for "legislator shirking." Such electoral slack may be the consequence of institutional factors that induce an incumbency advantage (such as the seniority system, the franking privilege, or fundraising advantages), or lack of information on the part of voters. A third prominent alternative to the median voter theorem is the party based model of Cox and McCubbins(1993). In this explanation, parties serve as a means of creating and distributing political rents. In order to be included in the sharing of those rents, elected officials must abide by party interests. One of the chapters of this dissertation attempts to discriminate empirically between these competing explanations using roll-call voting data from the U.S. Senate. A common theme of each of the theories described in the preceding paragraph is that, unlike the median voter theorem, elections matter. The identity of the party or individual that wins an election can have a dramatic effect on government decisions, and ultimately, on the economy. As a consequence, the factors that affect election outcomes are of interest not only to political scientists, but to also to economists. Three separate aspects of

the election process are examined in this dissertation. Chapter one considers the impact of campaign spending on election outcomes. Chapter two focuses on the measurement, sources, and implications of incumbency advantage in the U.S. House. The third chapter analyzes the midterm cycle in American politics. The party of the president loses seats in the House at the midterm elections almost without exception. While a number of theories have been proposed to explain that result, limited empirical work has been devoted to testing the competing hypotheses.

The papers contained in this dissertation fall squarely into a recently emerging field of research known as "positive political economy" that has its roots in the seminal contributions of Arrow, Buchanan, Downs, Olson, and Tullock. Alt and Shepsle(1990) characterize positive political economy as follows:

"... although ultimately interested in real phenomena, positive political economy is explicitly theoretical. Its focus is on microfoundations, and it is grounded in the rational-actor methodology of microeconomics. Thus, its most distinguishing characteristics are its coherent and unified view of politics and economics, its strongly interdisciplinary nature, and its concern with explaining empirical regularities."

While still in its infancy, the advances embodied in the initial wave of research in positive political economy suggest that it represents a valuable alternative to the typical isolation of economists and political scientists. Moreover, the quantity of work in the area is still quite limited; the returns to further research along these lines appear to be substantial.

Overview of the Chapters

The remainder of this introduction is devoted to overviews of the individual

chapters, as well as directions for future research. These summaries provide context for the chapters vis-a-vis the previous literature and highlight their methodological advance.

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

Reliable estimates of the effects of campaign spending are difficult to obtain due to the inability to control for unmeasured differences in the inherent ability of candidates to attract votes. At any given level of campaign spending, a high quality candidate will attract more votes than a low quality candidate. The same factors that make a candidate attractive to voters, however, will also make fundraising easier. As a consequence, 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. In contrast, the failure to include candidate quality may lead to an underestimate of the effects of incumbent spending since incumbents tend to increase campaign expenditures in response to a strong challenge. In light of those potential biases, the existing literature, which typically finds a large positive effect of challenger spending, but little evidence for effects of incumbent spending, must be viewed with great skepticism.

To avoid the bias that arises from unobserved candidate quality, this chapter looks only at pairs of elections in which the same two candidates run against each

other on more than one occasion. Under the assumption that an individual candidate's quality remains constant over time, a fixed-effects panel model using only those elections will eliminate the bias from unobserved candidate quality. Even if a candidate's quality does fluctuate, the method used will greatly reduce the bias found in cross-sectional studies as long as the variation in a given candidate's quality over time is small relative to quality differences across candidates.

In order to demonstrate that the sub-sample of elections employed in this analysis is representative, the standard cross-sectional methodology is applied to the sub-sample. The results obtained mirror those of previous studies. When the fixed-effects model is estimated, however, the results are strikingly different. In stark contrast to previous work, campaign spending is found to have an extremely small impact on election outcomes regardless of incumbency status. My estimates of the impact of challenger spending are an order of magnitude below those of cross-sectional studies. Despite relatively small standard errors on the estimates, the null hypothesis that campaign spending has no effect on election outcomes cannot be rejected.

Chapter II: A Panel-Data Approach to Measuring the Incumbency Advantage in the U.S. House

By all accounts, the electoral advantage enjoyed by incumbents in the House is substantial and has increased dramatically in the post-war period. Over the last two

decades, incumbents have been virtually unbeatable in the House, winning approximately ninety-five percent of the elections in which they have run.

Although a large empirical literature has been devoted to the topic of the incumbency advantage, Gelman and King(1990) demonstrate that all previous estimates suffer from various sources of bias. This chapter builds on many of the insights of Gelman and King(1990), but by extending the analysis from a cross-sectional approach to panel data, has a number of important advantages. First, panel data avoids possible biases due to district-specific characteristics and serially correlated errors. Equally important, the model presented here allows the various sources of the incumbency advantage to be disentangled. In particular, by varying the sample considered (and controlling for sample selection bias), the overall incumbency advantage can be broken down into three components: direct office-holder benefits (such as the franking privilege, fundraising advantages, etc.), incumbents' ability to scare off high quality opponents, and higher average quality among incumbents. As was the case in Chapter I, the key to obtaining those various estimates is choosing the appropriate sample in order to control for unobserved candidate quality, where quality is again defined as inherent vote-getting ability.

The results of the chapter suggest that the overall incumbency advantage has increased from 3.4 percent in the 1950s to 8.0 percent in the 1980s. There is little evidence to support the claim that incumbents are, on average, of higher quality than the typical open-seat candidate. When a candidate wins an open seat election and faces the same challenger again in the future, the incumbency advantage is

substantially smaller than is otherwise the case, even after controlling for sample selection bias. One interpretation of this result is that the ability to deter high quality challengers is a major indirect benefit of incumbency. Moreover, virtually all of the growth in the incumbency advantage since the 1960s appears to be attributable to a reduction in the relative quality of challengers. Finally, increases in the incumbency advantage over time are found to be capable of explaining all of the observed decline in the competitiveness of elections.

Chapter III: An Empirical Test of Competing Explanations for the Midterm Gap in the U.S. House

In the thirty-two midterm elections since the Civil War, the party of the incumbent president has lost seats in the House all but once. There are three leading explanations for the midterm gap. The first is "withdrawn presidential coattails." If popular presidential candidates provide a boost to congressional candidates when elections are held concurrently, then the absence of coattails at the midterm will tend to hurt the president's party. The second explanation of the midterm gap is based on mean reversion in economic performance. According to this explanation, the party of the president is held accountable for the state of the economy. Therefore, the victorious party in presidential elections will generally benefit from the state of the economy. If, however, the economy is only average at the midterm, or systematically underperforms, the vote for the president's party will decline at the midterm. The third explanation of the midterm gap is systematic presidential

punishment, i.e. even after controlling for other effects, the party of the president will lose votes. Alesina and Rosenthal(1989) develop a rational expectations model with polarized parties that predicts such an outcome.

Previous empirical work on the midterm gap has been quite limited. A handful of studies use national-level time series data. With only 23 congressional elections since World War II, however, results are quite sensitive to modelling assumptions. Moreover, such studies typically test only one or two explanations of the midterm gap without adequately controlling for competing hypotheses and other influences on election outcomes. As a consequence, researchers have tended to overstate the importance of their own explanations; combined, past estimates of the three competing theories are capable of explaining the midterm gap twice over.

This chapter uses district-level panel data rather than aggregate time-series data to circumvent some of the problems that have affected previous studies. The competing explanations are tested in a nested model that controls for the incumbency advantage in the U.S. House, something that previous studies have ignored.

The results of the chapter suggest that each of the three theories has an explanatory role. The impact of each, however, is substantially smaller than previous estimates would suggest. Interestingly, voters appear to systematically punish the party of the incumbent president not only at the midterm, but also in on-year elections. That result is interesting not only because it is in stark contrast to the incumbency advantage in the House, but also because it is difficult to reconcile with rational voting behavior.

Chapter IV: How Do Senators Vote: Disentangling the Role of Party
Affiliation, Voter Preferences, and Senator Ideology

There is a great deal of both academic and public policy interest in the question of whose interests elected officials represent. As discussed earlier, there are a number of competing theories of political behavior. The fundamental obstacle to discriminating between those explanations is the lack of observability of the variables in question, in particular candidate ideology. Since a candidate's ideology is likely to be correlated with both party affiliation and voter preferences, reliable estimates are difficult to obtain.

This chapter develops a methodology for estimating the relative weights that senators place on various factors that does not require senator ideologies to be observed in order to yield consistent estimates. The key assumptions required to identify the model are that senator ideologies do not change over time, that state voter preferences are adequately proxied by the voting behavior of a state's House delegation, and that a senator's core constituency can be approximated by the senator's party within the state. If those assumptions hold, the absence of a proxy for senator ideologies does not pose a problem. In fact, estimated ideologies can be backed out from the parameter estimates.

The model is estimated using ADA scores as measures of voting behavior for the period 1970-1991. Senators appear to be most influenced by their own ideologies, assigning approximately forty to fifty percent of the weight to that factor. While some weight is assigned to the preferences of voters in the state, but outside of the

senator's "core constituency," approximately three times as much weight is given to the preferences of voters within the "core constituency." Estimates of the importance of the national party line vary from about twenty-five percent of the senators' weight down to close to zero.

While the precise estimates obtained must be interpreted with some caution due to the reliance on proxies, the framework also yields estimates of the change in the relative weights placed on the various factors by senators over the election cycle and along the career path that are likely to be more robust. First-term senators appear to put more weight on voter preferences outside their constituency, but within the state. The weight given to state voter preferences rises sharply as elections approach, accompanied by a commensurate decline in the importance of the party line. Retiring senators are found to exhibit little systematic change in behavior.

This chapter has a number of important implications for political science research. First, the important role of ideology in determining voting behavior calls into question any voting study that does not explicitly control for ideology, especially since the estimated ideologies obtained in this chapter are strongly correlated with the other variables of interest. Secondly, these results call into question the applicability of the median voter theorem since state voter preferences play a small role in predicting senator voting patterns.

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Chapter 1

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

1.1 Introduction

Campaign finance has been the subject of political and academic debate almost continuously over the past few decades.¹ Accurate predictions 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, while incumbent spending has little or no effect on election

¹ Congress passed five major reforms between 1971 and 1979, though many of the early reforms were subsequently ruled unconstitutional by the Supreme Court in *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).

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 is an obvious source of such effects: a Democratic challenger in a

² See, among others, Abramowitz(1991), Erikson and Palfrey(1993), Glantz, Abramowitz, and Burkart (1976), Green and Krasno(1988,1990), Jacobson(1978,1980,1985,1990), and Welch(1981). Among those papers, only Erikson and Palfrey(1993) and Green and Krasno(1988,1990) 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.

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 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, Green and Krasno (1988) is a notable exception. Green and Krasno develop an eight-point scale to proxy challenger quality (they do not attempt to control for incumbent quality). Although statistically significant, inclusion of the proxy has only minor effects on the spending coefficients and does little to improve the fit of the model, increasing the R^2 only 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 multi-dimensional 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

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).

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

The results I obtain differ sharply from 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 one-third of one percent of the vote. Controlling for candidate quality and district fixed-

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

effects reduces estimates of the value of challenger spending to only a 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. Secondly, 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 outline of the paper is as follows. Section II develops the basic model, demonstrating how first differencing eliminates problems arising from unobservable candidate quality and district-specific effects. Section III describes and summarizes the panel data set employed (those elections where 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.

1.2 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:

$$\begin{aligned} V_{i,t} = & \alpha_i + (\eta + \beta_1 \text{Incum}\$_{i,t} + \beta_2 \text{Chal}\$_{i,t}) * I_{i,t} \\ & + \beta_3 \text{Open}\$_{i,t} + \gamma_t + \delta_1 \text{DemQual}_{i,t} \\ & - \delta_2 \text{RepQual}_{i,t} + \epsilon_{i,t} \end{aligned} \tag{1}$$

where

$V_{i,t}$ = Democratic share of the two party vote,

α_i = a district-specific constant,

$\text{Incum}\$_{i,t}$ = campaign expenditure by the incumbent,

$\text{Chal}\$_{i,t}$ = campaign expenditure by the challenger,

$\text{Open}\$_{i,t}$ = net campaign expenditures by open seat contenders (Democratic spending - Republican spending),

$I_{i,t}$ = indicator variable equal to 1 if the Democratic candidate is the incumbent, -1 if the Republican candidate is the incumbent, and 0 otherwise,

γ_t = nationwide partisan shock in year t (a dummy variable),

$\text{DemQual}_{i,t}$ = (unmeasured) quality level of the Democratic candidate,

$\text{RepQual}_{i,t}$ = (unmeasured) quality level of the Republican candidate, and

$\epsilon_{i,t}$ = an error term, assumed to be i.i.d. normal.

The interpretation of the variables is as follows:

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 $\text{Open}\$_{i,t}$ 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.

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 of little import, 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, 2) it facilitates the measurement of nationwide partisan shocks.

$I_{i,t}$ is an indicator variable reflecting incumbency status in a district. It is equal to 1 if the Democratic candidate is an incumbent, -1 if the Republican candidate is the incumbent, and 0 otherwise. The coefficient η captures all incumbency advantages except those due to campaign spending. The incumbency advantage is assumed to be constant across years, a reasonable approximation for the time period studied (Gelman and King 1990).

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 (Alesina and Rosenthal 1989; Erikson 1988; 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.

$DemQual_{i,t}$ and $RepQual_{i,t}$ 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 where the same two candidates face each other exactly twice, first at time t and again at time $t+1$. First differencing equation (1) yields

$$\Delta \text{DemVote}_i = \beta_1 \Delta(\text{Incum}\$_i * I_i) + \beta_2 \Delta(\text{Chal}\$_i * I_i) + \beta_3(\Delta \text{Open}\$_i) + \eta(\Delta I_i) + \Delta \gamma + \Delta \epsilon_i \quad (2)$$

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 comprise the sample must be representative of House elections as a whole. Secondly, 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 upwards while the incumbent spending parameters are biased downwards, just as in the previous models. One would expect, however, that the size of the bias will be greatly reduced; the estimates

obtained in Section IV support such a view.)

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

1.3 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 fifteen percent of the total Congressional elections held during this time period.

Table 1 compares a number of descriptive statistics for a nearly complete⁸ data set of contested elections held between 1972 and 1990 and the subsample used in

⁷ These 633 elections represent 299 pairs of opponents. A number of opponents faced one another three or more times. Cases where 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 elections or less 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 the author).

this paper. Data for the subsample of 633 elections is further broken down into elections that represent the first time a pair of candidates face-off (column 2), and all later meetings (column 3).

As 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 fifty-four percent of the vote; in the subsample it is approximately fifty-five percent. The elections in the subsample are slightly more competitive than the typical uncontested elections. Incumbents win 66.8 percent of the vote in the broad sample; 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 when national political conditions favor the challenger's party. For instance, in the aftermath of Watergate in 1974, nineteen 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.

The middle section 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.

The bottom portion 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 when candidates meet for the first time. Following Jacobson, the once-lagged congressional vote is used 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 percent in the most recent congressional election in which the current challenger was not

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.

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 percent 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.

Focusing first on columns (1) and (2), the estimated impacts of campaign spending are identical: a \$100,000 increase in challenger spending garners that candidate 2.7 percent of the vote, while 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. Importantly, the results in columns (1) and (2) are indistinguishable from 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 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.

1.4 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.

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) Patten (D-NJ), 1978, Tongsun Park scandal, accused of violating House ethics standards; 2) Bauman (R-MD), 1980, solicited sex from 16 year-old boy; 3) Thompson (D-NJ), 1980, ABSCAM; 4) Hansen (R-ID), 1984, false financial disclosure leads to House reprimand; 5)

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 roots, and natural logs respectively.¹³ White-heteroscedasticity consistent standard errors are in parentheses. The adjusted R^2 values are similar across regressions, as are the values and standard 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 removing fixed effects. (More than ninety-five 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.

Swindall (R-GA), money laundering scheme, federal grand jury investigation; 6) Strangeland (R-MN), 1990, involvement with lobbyist, including after-hours phone calls and taxi cabs charged to House credit card; 7) Smith (R-OR), 1990, S&L related complaint to House ethics committee.

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.

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) finds an incumbency advantage between four and five percent when controlling for challenger quality (compared to almost nine 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 given are 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: holding everything else constant, a Democratic candidate for the House would have received an extra four percent of the vote running in 1976 versus 1980. The midterm cycle (Alesina and Rosenthal 1989) also emerges quite clearly.

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

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 of 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 spending variables are generally not significantly different from zero, despite standard errors that are small.¹⁵ 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 absolutely no effect.

The spending coefficients in column (1), the linear specification, have a direct interpretation: an extra \$100,000 in campaign spending (holding the opponent's spending constant) garners a challenger three-tenths of a 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

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

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 re-election; increasing spending by \$100,000 will improve the incumbent's tally by less than two-tenths of a percent of the vote (0.14 in column (2), 0.09 in column (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 above regressions 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., non-existent) using the fixed-effects and cross-sectional approaches. Ironically, my results suggest that the standard conclusion observed in the literature, namely that the impact of challenger spending is measured accurately while the effects of incumbent spending are biased downwards, 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/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 Secondly, 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.

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 linear case), 0.99 (in square root case), and 0.93 (in 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.

To test that hypothesis, the slopes of the spending parameters were allowed to vary between first elections and repeat challenges.¹⁸ F-values for the 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 re-estimated 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

¹⁸ The specific form of the test is as follows. (For simplicity in exposition, only the case of opponents who meet exactly twice is presented here. The more general case follows directly.) Let election outcomes be described exactly as in equation (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

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

Equation (2') is identical to equation (2) except that incumbent spending and challenger spending now enter both in levels as well as first-differenced. If spending matters less in the second election, the sign 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.

¹⁹ The statistic for the test is distributed $F_{[2,318]}$. The critical value for rejecting the null with ninety 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.

The model was also estimated excluding the years 1972 and 1974 due to 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 "non-competitive" elections. As a crude proxy, any challenger spending less than \$10,000 in 1990 dollars was deemed non-competitive.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.

1.5 Policy Implications

In this section, the likely effects of three different public policy proposals involving mandatory spending limits and/or public financing of campaigns 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 of the preceding section. 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 sub-sample employed in

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

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

obtaining the estimates), under the assumption that all factors except 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 re-election. 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*,²⁴ from an analytical point of view, such a policy is nonetheless of interest. A

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.

²⁴ *Buckley v Valeo* (424 U.S. 1, 1976).

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 seventy-five percent reduction for the average incumbent; a fifty percent reduction for the average challenger), only 15 election results are reversed over four sets of congressional elections -- less than one 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 benefitted from the caps, but thirty-seven incumbents would have avoided defeat with a \$100,000 cap. What makes the Jacobson prediction even more remarkable is that only forty-three 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.²⁵ 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 fundraising.

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 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 dollars over the four sets of elections. Subsidizing candidates up to a \$400,000 limit has an impact on an additional seven elections; all of those benefitting 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 one percent of the Congressional elections in the sample while costing tax payers over a billion dollars.

In contrast, the estimates using the cross-sectional parameters of Jacobson

²⁵ This policy, like the previous one, is likely to be considered unconstitutional, but again, provides a useful analytical benchmark.

suggest that a generous "level playing field" policy would have a very favorable impact on challengers: ninety-seven challengers would have benefitted, while only twenty incumbents would have avoided defeat.

"Floors Without Ceilings": Public Financing Without Spending Limits

A "floors without ceilings" policy is one where any candidate below a threshold level of spending is provided public funds to make up the shortfall. Campaign spending of candidates above the threshold level remains unaffected.²⁶ 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 Alexander(1991,1992) and Sorauf(1988)) 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 non-existent, 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 tax payers up to 400 million dollars.²⁷ A floor

²⁶ The state of Montana has such a policy. See Sorauf(1988, p.275).

²⁷ The estimate of tax-payer cost is based on the assumption that candidates spending less than \$200,000 will take advantage of the policy by raising no funds privately, while candidates raising more than \$200,000 currently would continue to do so. A less costly alternative to this

of \$400,000 would have altered eight elections (seven in favor of challengers and the other an open seat contest) at an estimated tax payer 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."

1.6 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 while 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. Unlike 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 PACs, and discourages potential high-quality challengers who do not have ready access to campaign funding and find fundraising distasteful. Tight spending limits provide only a minor advantage to incumbents. If high-quality

program would be one involving matching funds rather than outright grants.

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 on 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 where 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.

Table 1: Descriptive Statistics

<u>Statistic</u>	<u>Subsample of Repeat Meetings</u>		
	(1) <u>Overall Contested Elections</u>	(2) <u>First Meeting</u>	(3) <u>Later Meetings</u>
Democratic Percentage of Vote	54.1% (18.0)	55.1% (16.6)	55.5% (16.4)
Incumbent's Percentage of Vote	66.8 (10.1)	64.2 (10.8)	63.4 (10.8)
Success Rate for Incumbent's Seeking Reelection	94.8	94.1	89.5
<hr/>			
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	----
<hr/>			
Campaign Spending per Candidate: (in Thousands of 1990 Dollars)			
Incumbents	293	266	343
Challengers	136	134	173
Open Seat	409	275	---
<hr/>			
N	2781	299	334

Notes: Numbers in parentheses are standard deviations. Column (1), except for spending data, is drawn from the data set used in Levitt and Molfram(1994). See note 8 for further information. Spending data in column (1) is an unweighted average of real spending for all major party candidates in general elections between 1972 and 1990. Spending data is based upon Sorauf(1988, Table 6-1), Common Cause(1974,1976), and FEC Reports on Financial Activity (multiple editions).

Table 2: Cross-Sectional Estimates for Sub-sample of Repeat Challengers
Linear Model

<u>Variable</u>	<u>First Meeting</u>		<u>Later Meetings</u>	
	(1)	(2)	(3)	(3)
Challenger Spending	-2.7‡ (0.3)	-2.7‡ (0.3)	-0.8‡ (0.2)	
Incumbent Spending	-0.2 (0.3)	-0.2 (0.2)	0.1 (0.1)	
Competitor Party Strength:				
Vote Share(-1)	.54 (.07)		.78 (.04)	
Vote Share in Last Election with Different Challenger		.21 (.05)		
Constant	34.0 (5.2)	48.0 (7.8)	14.2 (3.2)	
Adj. R-squared	.56	.55	.76	

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

Table 3: Campaign Expenditure Impact Estimates (Linear Models)
 Percent of Two-party vote Obtained through a \$100,000 increase in 1990 dollars

<u>Model</u>	<u>Time Period</u>	<u>Challenger</u>	<u>Incumbent</u>
Jacobson(1980,1985) OLS	1972-1982	2.7%	0.2%
Jacobson(1985) TSLs	1972-1982	4.2%	----- ¹
Krasno and Green(1988) OLS	1978	1.6%	0.1%
Krasno and Green(1988) TSLs	1978	2.4%	2.2%

Notes: Estimates for Jacobson model represent unweighted averages of estimates for the relevant time period. Estimates for Jacobson OLS model taken from Jacobson(1980, pp. 142-43) and Jacobson(1985, p.17). Estimates for Jacobson TSLs model taken from Jacobson(1985, p.36). Estimates for Krasno and Green models taken from Krasno and Green(1988, p. 899).

¹Jacobson does not estimate the impact of incumbent spending in the 2SLS model due to a lack of precision arising from multicollinearity

Table 4: Results of the Regression Model

Variable	(1) Linear Spending	(2) Square Root Spending	(3) Log Spending
Challenger Spending	.30 (.19)	.13 (.06)	1.04 (.50)
Incumbent Spending	.09 (.13)	.06 (.06)	.61 (.75)
Open Seat Spending	.17 (.44)	.09 (.13)	.67 (1.40)
Incumbency	3.2 (0.8)	3.7 (1.1)	3.5 (1.7)
Scandal Dummy	4.8 (1.4)	4.8 (1.4)	5.0 (1.4)
1990	----	----	----
1988	0.6 (0.7)	0.7 (0.7)	0.6 (0.7)
1986	1.6 (0.9)	1.7 (0.9)	1.7 (0.9)
1984	-1.4 (1.1)	-1.3 (1.1)	-1.4 (1.1)
1982	2.0 (1.3)	2.2 (1.3)	2.1 (1.3)
1980	----	----	----
1978	2.3 (0.7)	2.3 (0.7)	2.5 (0.7)
1976	4.0 (0.9)	4.0 (0.9)	4.2 (0.9)
1974	5.0 (1.1)	4.9 (1.1)	5.1 (1.1)
1972	-2.0 (1.4)	-1.9 (1.4)	-1.7 (1.4)
Adj. R ²	.24	.24	.24
F-test of Spending coeffs. equal to zero	0.85	1.64	1.36

Notes: The dependent variable is the Democratic percentage of the two-party vote. White-heteroscedasticity consistent standard errors in parentheses. Spending variables are in terms of \$100,000 of 1990 dollars. All variables except for year dummies multiplied by incumbency indicator variable (see Section III for further explanation). Year dummies for the 1970s are relative to 1980; year dummies for 1980s are relative to 1990. Adjusted R² value refers to the percentage of variance explained after the fixed-effects transformation. In column (3), candidates spending less than \$1,000 are treated as if they spent \$1,000. Degrees of freedom equal to 320 in all regressions.

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A Panel-Data Approach to Measuring the Incumbency Advantage in the U.S. House

2.0 Introduction

Incumbency status is a critical determinant of success in elections to the US House of Representatives. Over the last two decades, well over ninety percent of the incumbents seeking re-election have been successful. Even in 1992, after widespread redistricting and a groundswell of public disillusionment fueled by the House Banking scandal, ninety-three percent of the incumbent candidates were successful in the general election.

Although a large literature has been devoted to the topic of the incumbency advantage in the U.S. House,¹ Gelman and King(1990) demonstrate that previous estimates of the incumbency advantage suffer from various sources of bias. Gelman and King propose their own cross-sectional model and demonstrate that it is unbiased under certain conditions. Since that paper, little attention has been devoted to the

¹ This literature includes, but is not limited to, Alford and Hibbing(1981), Collie(1981), Cover(1977), Cover and Mayhew(1977), Erikson(1971), Krashinsky and Milne(1993), Mayhew(1974), Nelson(1979), Payne(1980).

topic with the exception of Krashinsky and Milne (1993).

This paper builds on many of the insights of Gelman and King (1990), but by extending the analysis from a cross-sectional approach to panel data, has a number of important advantages compared to their earlier work. The use of panel data avoids the possible biases that may arise in their model due to district-specific characteristics (Hausman and Taylor 1981) and serial correlation in the error term. Moreover, the model developed here allows us to at least partially disentangle the various sources of the incumbency advantage. In what follows, we obtain rough estimates of the extent to which the incumbency advantage is due to direct office-holder benefits (such as the franking privilege, fundraising advantages, etc), the ability of incumbents to scare off high quality challengers, or simply the result of incumbent candidates generally being more highly qualified.

The results of this paper generally confirm the findings of Gelman and King concerning both the overall magnitude of the incumbency advantage and its time-path. Using a fixed-effects panel data model, we estimate that the incumbency advantage has risen from 3.4 percent in the 1950s to 8.0 percent in the 1980s.

By using a sophomore surge analysis (Erikson 1971), modified to offset the sources of bias as suggested by Gelman and King (1990), we are able to control for the inherent quality of the incumbents, and find that only a negligible fraction of the overall incumbency advantage can be attributed to the fact that incumbents are on average of higher quality than the typical open-seat candidate.

In contrast, when one looks at elections where the same two candidates meet

on multiple occasions, as in Levitt(1994a), the incumbency advantage is greatly reduced. One explanation for that finding is that a sizable portion of the incumbency advantage results from an incumbent's ability to scare off high quality challengers, rather from direct perks of office such as franking privileges or the opportunities to provide constituency services.

In addition to estimating parameter values for the incumbency advantage, the paper also considers the impact of the incumbency advantage on the competitiveness of elections. We find that essentially all of the observed decline in the competitiveness of congressional elections is attributable to increases in the incumbency advantage.

The paper is organized as follows. Section I develops the basic model. Section II presents the results of estimating that model on the full sample of post World War II elections, and summarizes the results of numerous tests of the model specification. Section III uses a modified sophomore surge analysis to measure the incumbency advantage controlling for incumbent quality. Section IV provides estimates of direct office-holder benefits by looking at elections in which the same two candidates meet on more than one occasion. Section V analyzes the impact of the incumbency advantage on the competitiveness of elections. Section VI provides a brief conclusion.

2.1 A Model of the Incumbency Advantage

The outcome of a congressional election is assumed to be a function of

district-specific characteristics (i.e. the normal vote), incumbency status in the district, the quality of the competing candidates, and national-level political forces. Formally, the percentage of the two-party vote obtained by the Democratic candidate in district i and year t is written as follows:

$$V_{it} = N_i + \gamma_t I_{it} + Q_{it}^d - Q_{it}^r + \gamma_t + \epsilon_{it} \quad (1)$$

where

V_{it} = percentage of the two-party vote accruing to the Democratic candidate in district i in year t ,

N_i = a district-specific constant corresponding to district i 's normal vote,

I_{it} = incumbency status in district; equal to 1 if the Democratic candidate is an incumbent, -1 if the Republican candidate is an incumbent, 0 otherwise,

Q_{it} = quality of the candidate seeking office, with superscripts corresponding to party,²

γ_t = national partisan swing across all districts in a given year, and

ϵ_{it} = district-specific shock, assumed to be normally distributed with mean zero.

The difference in quality between two candidates running for a given seat is not directly observable. To understand more clearly the effect of unobserved quality

² Because the choice of scaling on the candidate quality variables is arbitrary, there is no loss of generality from having Q_{it} enter directly into (1) rather than being scaled by a coefficient.

on our estimates, let the expected quality difference between two candidates for a particular seat be modeled as the gap between the average quality of candidates available to each party in that district adjusted to take into account the incumbency status in the district. Because high quality candidates are more likely to win elections the typical incumbent will have above average quality as a consequence of selection bias. Conversely, if strong challengers are deterred by the presence of an incumbent, the typical challenger will have lower quality than the average open-seat candidate. In mathematical symbols,

$$Q_{it}^d - Q_{it}^r = (\psi_i^d - \psi_i^r) + (\theta_i^I + \theta_i^C)I_{it} + \eta_{it} \quad (2)$$

where

ψ_i = average quality level of open-seat candidates from a given party in district i ,

θ_i^I = difference between quality of the average incumbent and the average candidate for an open seat,

θ_i^C = difference between quality of the average challenger to an incumbent and the average candidate for an open seat,

η_{it} = a noise term, assumed to be normally distributed and uncorrelated with ϵ_{it} and the regressors in equation (1).

Combining equations (1) and (2) yields,

$$V_{it} = [N_i + (\psi_i^d - \psi_i^r)] + [\delta_i + (\theta_i^I + \theta_i^C)]I_{it} + \gamma_i + (\eta_{it} + \epsilon_{it}) \quad (3)$$

Equation (3) can be estimated using panel data by regressing the congressional vote on the incumbency status in the district with year dummies and a fixed effect for each district also included. The first term in brackets on the right-hand side is the expected vote in the district taking into account candidate quality. The second bracketed term is what we term the incumbency advantage accruing to the party. It reflects the increase in the vote that a party can expect compared to an open-seat contest when represented by an incumbent. The incumbency advantage to the party includes both direct office-holder benefits (δ), as well as any differences in candidate quality. The size of the incumbency advantage is free to vary by year (as reflected by the subscript t on the coefficients) in order to capture changes in the magnitude of the incumbency advantage over time. Within a particular year, however, all incumbents are assumed to benefit equally from holding office. When neither candidate is an incumbent, there is no incumbency advantage in the district. The third term, γ_t , captures nationwide partisan swings, 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) 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 (Tufté 1975). For the purposes of this paper, however, it is enough to be able to measure partisan swings without concern for the ultimate source

of those swings. Section II contains the empirical estimates obtained using equation (3).

While estimates of the incumbency advantage to the party are certainly of interest in and of themselves, one would also like to know the relative magnitudes of the various sources of that advantage. For instance, controlling for incumbent quality in equation (3) provides an estimate of the size of the "incumbency advantage to the individual" (i.e., the boost that a *given* candidate can expect when incumbency status changes), which may be a more useful measure in practice. To achieve that goal, the results of a modified sophomore surge analysis proposed by Gelman and King(1990), but never previously estimated, are presented in Section 3. A second advantage of our sophomore surge analysis over previous studies is that it is done in a regression framework and therefore provides us with reliable standard errors on our estimates.

The magnitude of direct office-holder benefits (δ) is also of interest. To obtain such estimates, one must control for the quality of both candidates. A method for doing that, based on elections involving repeat challengers, is developed and estimated in Section 4.

2.2 Empirical Estimates of the Incumbency Advantage Accruing to the Party

Equation (3) above was estimated using district-level congressional elections

from 1948 to 1990.³ Before estimation could proceed, practical decisions with respect to redistricting, the length of time over which the normal vote in a district can be presumed constant, and uncontested elections were required. On the first point, whenever redistricting occurred, the resulting district was treated as an entirely separate district. Results from the first election in a district following redistricting therefore were not included in our sample due to the indeterminacy of the incumbency status in those elections. On the second issue, due to concerns over possible drift of the normal vote in a district over time, the normal vote was required to remain constant for a maximum of ten years. For those districts that were not redistricted within ten years, separate estimates of the normal vote were obtained for each decade. Finally, we exclude uncontested elections from our sample because the true proportion of the vote that would have gone to the two parties had there been a major party opponent is not observed.⁴

The year-by-year estimates of the incumbency advantage, along with White-heteroscedasticity consistent standard errors are displayed in Table 1, and also pictorially in Figure 1. The coefficients represent the extra percent of the vote that a

³ Because existing databases were not in a convenient format for our analysis, we compiled our own database of post-World War II election results. Our source of vote data was America Votes (multiple editions). We used the Biographical Directory of the United States Congress 1774-1989 and the Congressional Quarterly (multiple editions) for information on membership changes within a congress following retirements or deaths.

⁴ Excluding uncontested elections is an example of sample selection on the dependent variable, a practice that typically leads to bias. Using very different techniques, however, both Gelman and King(1990) and an earlier version of this paper demonstrate that the extent of the bias is empirically quite small (less than half a percentage point).

party with an incumbent seeking reelection can expect to receive in a given district vis-a-vis an open seat election. With the exception of the years 1972 and 1982, when widespread redistricting limited the number of available observations, the estimates are quite precise. In all cases, the incumbency advantage is significantly positive, and exhibits a clear upward trend over time. Averaged over decades, the incumbency advantage has grown from 3.4 percent in the 1950s to 8.0 percent in the 1980s.⁵ Nonetheless, the increase we find is less pronounced than almost all previous analyses (see, for instance, Mayhew 1974; Cover and Mayhew 1977; Alford and Hibbing 1981; Collie 1981).

The estimates of the national partisan swings between presidential election years and the ensuing midterm are presented in Table 2. There is strong evidence for the midterm cycle. In each case, the party of the president loses votes between the on and off-year elections, with the average loss being 4.4 percent of the vote.⁶ The estimates appear quite plausible, and are also quite precise except when the election cycle coincides with widespread redistricting in 1972 and 1982. The largest swings -- approximately seven percent of the vote-- follow the Johnson landslide in 1964 and the Reagan victory in 1980. In contrast, the Bush, Carter, and Kennedy administrations experience midterm gaps of a much smaller magnitude.

⁵ For classification purposes, we include 1948 in the 1950s, and we include 1990 in the 1980s.

⁶ Levitt(1994b) provides an empirical test of competing explanations of the midterm gap.

Tests/Extensions of the Basic Model

A number of steps were taken to check the adequacy of the assumptions underlying the basic specification estimated above. As one test of the model specification, we include the lagged congressional vote as a regressor. There are at least two reasons for suspecting that the lagged vote may belong in our model. First, a district's normal vote may exhibit a trend over time rather than being constant, as we assume. Secondly, if a Representative's incumbency advantage grows over his or her career, the lagged vote may also matter. When we include the lagged vote, however, it is completely insignificant, carrying a coefficient of $-.005$ with a standard error of $.020$. Clearly, in our model, which includes fixed effects, the lagged congressional vote is of little relevance in predicting the upcoming election.

A number of other variables were added to the basic model to test for potential misspecification. One possible bias arises from the endogeneity of the incumbent's decision concerning whether to seek reelection. If incumbents sensing negative *district-specific*⁷ shocks in upcoming elections frequently choose not to run, our measure of the incumbency advantage will likely overstate the true value. To test this hypothesis, we include a dummy variable to reflect districts where the incumbent did not seek reelection. The coefficient on that variable, however, is extremely small and indistinguishable from zero (-0.4 percent with a standard error of 0.3 percent).

A dummy variable representing sophomore Representatives is included in the

⁷ No problems of bias arise if the decision to retire is based upon the national shock since that is controlled for in the regression.

regression to test the hypothesis that the incumbency advantage increases with seniority. Not surprisingly, sophomores receive a somewhat smaller incumbency advantage: 0.9 percentage points less than other incumbents (with a standard error of 0.3 percent). That result suggests that, as a measure of the overall incumbency advantage, sophomore surge analyses may be biased even further downward than is commonly recognized.

The magnitude of the incumbency advantage appears to differ slightly across parties. When we allow the incumbency advantage to vary by party, Republican incumbents enjoy an advantage that is 1.4 percentage points greater than Democrats (with a standard error of 0.6). There is no evidence that the size of the incumbency advantage varies systematically between on-year and off-year elections.

2.3 Estimating the Incumbency Advantage to the Individual using a Modified Sophomore Surge Analysis

To obtain accurate estimates of the benefits that incumbency provides to a particular individual (as opposed to the that candidate's party), one must control for the quality of the incumbent, a task that is difficult since quality is not directly observable. Assuming that a candidate's intrinsic quality remains constant across elections, the solution to this difficulty is to employ a sophomore surge analysis, i.e. compare the vote shares received in an incumbent's first bid for reelection to the vote share obtained when that candidate ran as an open-seat candidate. While standard sophomore surge estimates are biased downwards (Erikson 1971), Gelman and

King(1990) develop a method to correct for that bias, the intuition for which is presented below.

Let congressional elections be modeled as before by equation (1). To simplify exposition, we will focus on the case where the candidates contend for an open seat at time t , and the Democratic candidate is the victor.⁸ The relevant equation describing the sophomore surge (obtained by subtracting equation (1) in time period t from equation (1) in time period $t+1$) is

$$\Delta V_{i,t+1} = [\delta_{t+1} + \theta_{t+1}^c] \Delta I_{i,t+1} + \Delta \gamma_{t+1} + (\eta_{i,t+1} + \epsilon_{i,t+1}) - (\eta_{i,t} + \epsilon_{i,t}) \quad (4)$$

where Δ represents the change in between time t and $t+1$. Equation (4) is simply the change in the vote share between the two elections. When applied to the set of elections where an incumbent seeks reelection for the first time at $t+1$, it is a measure of sophomore surge. Note that the district-specific constant and incumbent quality coefficient disappear because they are assumed constant across the two elections.

Direct estimation of equation (4) will be biased due to a sample selection problem. We only observe as incumbents at time $t+1$ those candidates who were successful in winning elections at time t . Among this set of candidates, Democrats are likely to have received positive error terms at time t , while Republicans are likely

⁸ The logic is identical when considering Republican rather than Democratic incumbents (although the formulas change slightly).

to have negative errors. As a consequence, estimates of the incumbency advantage to the individual obtained by applying OLS to equation (4) will be biased downwards.

Correcting the sample selection bias in equation (4) requires the elimination of any correlation between $I_{i,t+1}$ and the error terms. To accomplish that, an estimate of the expected value of the error terms *conditional on the realization of $I_{i,t+1}$* is required. Using the assumption of normality, the conditional expectations of the error terms (which are distributed as truncated normals) can be expressed as follows

$$\begin{aligned} E[\eta_{it} + \epsilon_{it} | V_{it} > .50] &= \sigma_t \lambda(\psi_{it}) \\ E[\eta_{i,t+1} + \epsilon_{i,t+1} | V_{it} > .50] &= \rho(\sigma_t \lambda(\psi_{it})) \end{aligned} \tag{5}$$

where

σ_t = the standard error of the error term from equation (1),

$$\lambda(\psi_{it}) = \frac{\phi(\psi_{it})}{1 - \Phi(\psi_{it})},$$

$\phi(\)$ = the density function of the standard normal,

$\Phi(\)$ = the standard normal cumulative distribution function,

$$\psi_{it} = (.50 - \mu_{it}) / \sigma_{it},$$

μ_{it} = the expected Democratic share of the vote in district i in year t , and

ρ = the serial correlation in the error term in equation (1).

The function λ is sometimes known as an inverse Mills ratio (Greene 1990). Note

that the conditional expectation of the time $t+1$ error will be equal to zero if there is no serial correlation in the error terms.

Computing equation (5) requires unbiased estimates of the expected Democratic share of the vote in district i at time t , the variance of the error term σ_i , and the serial correlation ρ in the error term. The results of Section II meet the first two requirements. Bhargava et al. (1982) present an alternative to the Durbin-Watson statistic that is applicable to panel data models. Their methodology can be used to obtain unbiased estimates of ρ from the residuals obtained in Section II.

Subtracting equation (5) from both sides of equation (4) yields an equation that is free of sample selection bias

$$\Delta V_{i,t+1} - (1-\rho)(\sigma_i \lambda(\psi_i)) = [\delta_{i,t+1} + \lambda \theta_{i,t+1}^c] \Delta I_{i,t+1} + \Delta \gamma_{i,t+1} + \pi_{i,t+1} \quad (6)$$

where π is shorthand for a complicated expression for the error term, which is mean-zero and uncorrelated with the right-hand side variables. Consequently, estimation of equation (6) will lead to unbiased estimates of the incumbency advantage to the individual.⁹

Empirical Results

⁹ More accurately, the results provide unbiased estimates of the incumbency advantage obtained by *sophomores*. To the extent that more senior incumbents enjoy differing magnitudes of an incumbency advantage (as suggested by the evidence in Section 2), the estimates of this section do not directly generalize to all incumbents.

Equation (6) was estimated using the 684 observations in our sample involving sophomore representatives who were contested in their first two elections. For purposes of computing the sample selection bias correction, the estimate of σ_t was allowed to vary by decade. According to our estimates, election outcomes have become more variable over time, confirming Jacobson(1987).¹⁰ The technique of Bhargava et al.(1982), a generalization of the Durbin-Watson statistic, yields an estimated serial correlation parameter $\rho = .36$.¹¹

Estimates for both the standard sophomore surge, as well as our modified sophomore surge measure are presented in Table 3. Column (1) contains the most primitive measure, controlling for neither national partisan swings nor sample selection bias. Column (2) controls for partisan swings, but does not take into account sample selection bias. Column (3) controls for both national partisan swings and sample selection bias.

Because the year-by-year parameter estimates are imprecise (standard errors ranging from 0.8 percent to 6.2 percent), the results in the table are aggregated by decade. As was expected, the estimates in columns (1) and (2) exhibit substantial downward bias. Estimates of the sophomore surge are more than two percentage

¹⁰ The estimates of σ are .036 for the 1950's, .040 in the 1960's, .053 for the 1970's, and .051 in the 1980's.

¹¹ The Bhargava et al. technique is designed for a balanced panel. To apply their method to our unbalanced panel, separate estimates of ρ were computed for districts with 3, 4, and 5 contested elections respectively. The estimated values of ρ ranged from .30 to .43. A weighted average of those estimates yielded the value .36 used in the following analysis.

points lower in column (1) than column (3).¹² Most of that difference results from the sample selection correction rather than controlling for partisan swings.

The modified sophomore surge measure in column (3) yields results that are strikingly similar to those obtained in Section 2 when estimating the incumbency advantage to the party. For none of the four decades can we reject the null hypothesis that the incumbency coefficients from the two different models are identical. Any differences between the two sets of estimates, aside from noise, should be attributable to systematic quality differences between incumbents and the typical open seat candidate. The fact that the results correspond quite closely across the specifications implies that any such quality differentials are small. That conclusion is further strengthened when one considers that sophomores were found to enjoy an incumbency advantage approximately one percent smaller than other incumbents in Section 2.

To the extent that there is any evidence of a quality differential, it is apparent only in the 1950's and 1960's. One possible explanation for that pattern is as follows. From the 1970's onward, the large incumbency advantage has meant that the winner of an open seat election, unless involved in a scandal, is practically assured of a seat in congress for life. Consequently, a victory in an open seat contest is

¹² The failure to control for sample selection also explains why previous sophomore surge analyses have found a greater percentage increase in the incumbency advantage than we observe: the sample selection correction has grown much more slowly than the unadjusted sophomore surge measure. For instance, between the 1950's and the 1980's, the unadjusted sophomore surge measure grew almost 400 percent (from 1.1 percent to 5.2 percent). By comparison, our corrected sophomore surge measure slightly more than doubled (from 3.4 percent to 7.6 percent).

extremely valuable, and the parties are able to field uniformly strong candidates in such elections. In contrast, the smaller incumbency advantage of the 1950s implied a lower value to winning an open seat election. As a result, the parties sometimes failed to attract high quality candidates in open seat elections.

2.4 Estimating the Magnitude of Direct Office-Holder Benefits

Using Elections with Repeat Challengers

The analysis of the previous section demonstrates strong evidence for a sophomore surge. From those estimates, however, it is impossible to determine whether the incumbency advantage is the result of direct office-holder benefits, or the ability to scare off high-quality challengers. Office-holder benefits include franking privileges and opportunities to provide constituent services, as well as the indirect benefits of media exposure, potential advantages in fund raising, and experience in running successful campaigns (Mayhew 1974; Cover and Mayhew 1977; Fiorina 1977). The ability to deter high-quality challengers arises because strong challengers may be hesitant to engage in costly and time-intensive campaigns against incumbents, especially given the high rates of reelection for incumbents and the possibility that losing hinders future political advancement (Mann and Wolfinger 1980; Collie 1981; Jacobson and Kernell 1981).

Separating these two sources of advantage requires controlling for challenger quality. Previous analyses have attempted to construct indexes of candidate quality (Krasno and Green 1988). We take a more direct approach, focusing on pairs of

congressional contests that satisfy two conditions: (1) the same two candidates face one another in both elections, and (2) their incumbency status differs across the two elections.¹³ Under the assumption that candidate quality remains constant across elections, pairs of elections satisfying those two conditions are described by the following equation:

$$\Delta V_{it+1} = (\delta_{t+1})\Delta I_{it+1} + \Delta \gamma_{t+1} + (\eta_{it+1} + \epsilon_{it+1}) - (\eta_{it} + \epsilon_{it}) \quad (7)$$

where all quality terms have now been eliminated. Estimation based on equation (7) will suffer from sample selection bias for the same reasons cited with respect to equation (4) in the previous section; namely, candidates who receive favorable electoral shocks at time t and more likely to be incumbents in period $t+1$. Therefore, the following sample selection correction is made prior to estimation

$$\Delta V_{it+1} - (1-\rho)(\sigma_{it}\lambda(\psi_{it})) = (\delta_{t+1})\Delta I_{it+1} + \Delta \gamma_{t+1} + \pi_{it+1} \quad (8)$$

where all variables are defined as previously, and the error terms from equation (8) are condensed into π_{it+1} . Estimation of equation (8) on the sample of elections where two candidates meet on multiple occasions between which incumbency status has

¹³ Two situations fit these conditions. In one case, two candidates vie for an open seat in an election, and the loser returns to challenge the winner (who now benefits from an incumbency advantage) in the following election. In the second case, an incumbent is defeated and returns to challenge the candidate who he or she lost to earlier (despite the fact that he or she has lost the incumbency advantage while the opponent has gained it).

changed provides unbiased estimates of direct office-holder benefits for that subset of incumbents,¹⁴ provided two further conditions are met. First, potential candidates must not be able to determine whether a given incumbent's advantage will be greater than or less than the average incumbency advantage. If such information is available, losing candidates may be more likely to run again if the incumbent's advantage is small, leading to a downward bias in the estimates. Secondly, for similar reasons, candidates must not be able to determine the sign or magnitude of local shocks to elections. While it certainly is possible for an opponent to estimate the likely vote an incumbent will receive, such estimates will be based primarily on historical voting patterns in the district, expected national-level partisan swings, and the average incumbency advantage. Since most of the cases observed involve first-term incumbents, it is difficult to imagine how prospective opponents would have specific knowledge concerning the relative size of the Representative's incumbency advantage. Moreover, we found that the loser's decision to run again was highly correlated with the national political landscape. In our sample, 73 percent of those seeking to avenge a defeat chose to do so in a year that had a more favorable national partisan swing than the year in which they had lost; it appears to be national rather than district-level events that lead a loser to challenge an incumbent again. Nonetheless, in light of those informational requirements, the results of this section are substantially more speculative than those of the previous sections.

¹⁴ If this subset of incumbents is not representative of incumbents as a whole, the results of this section cannot be directly generalized to all incumbents.

Empirical Results

There were 122 pairs of elections in which the same two candidates faced one another before and after a change in incumbency status. Using those elections, we estimated equation (8). Because of the limited number of observations, we restricted the partisan swing coefficients γ_i to take on the estimated values obtained from regressions of equation (3) in Section II. Column (4) of Table 3 contains the results. Those candidates who face repeat challengers -- and thus garner only the direct office-holder benefits of incumbency -- attain significant, but substantially reduced, incumbency advantages. In contrast to the other measures of the incumbency advantage, direct office-holder benefits appear to have declined in the 1980's. Comparing the 1960's to the 1980's, office-holder benefits have increased by less than one percentage point, explaining less than one-sixth of the increase we found over the same period using our modified sophomore surge estimator in column (3). Rather, it appears that most of the increase in the incumbency advantage is due to an increased ability of incumbents to deter high quality challengers.

There are a number of plausible explanations for why fewer strong challengers are observed in recent years. First, the decline may be related to the dramatic increase in campaign spending (which more than doubled in real terms over the last two decades). Some high-quality challengers may choose not to enter a race because they are unable to raise the necessary funds to be competitive, or find such activities distasteful. A second possible explanation is that the stigma attached to losing has grown over time. As the stigma increases, candidates with higher political aspirations

avoid challenging incumbents, instead waiting for an open-seat contest. Finally, the increasing incumbency advantage may simply be a self-fulfilling prophecy. If strong challengers choose not to run based on the (perhaps mistaken) belief that incumbents are invincible, the resulting electoral success of incumbents will validate those expectations.

2.5 The Incumbency Advantage and Declining Competitiveness

In this section, we consider whether the incumbency advantage is capable of explaining the downward trend in the number of competitive elections over this time period. Several researchers have examined the fall in the number of closely contested congressional districts (Mayhew 1974; Cover and Mayhew 1977; Cover 1977; Ferejohn 1977; Fiorina 1977; Bauer and Hibbing 1989). While the explanations for how and why incumbents have played a part in this phenomena vary, there is general agreement that the decline in competitiveness is inextricably linked to the rise in the incumbency advantage since the mid-1960s.¹⁵ We use results from our panel data model to quantify the extent to which the incumbency advantage explains declining competitiveness.

Most previous analyses of the trend in competitiveness have focused on margins in election outcomes. These studies have classified as competitive all districts in which the winner received less than a certain percentage of the vote, for

¹⁵ Not all scholars agree that elections have become less competitive in recent years. See Collie (1981) and Jacobson (1987).

instance, less than 60 percent (Tufté 1973; Tufté 1974; Mayhew 1974; Collie 1981). We take a slightly different approach in order to account for the increased variability in election outcomes. We designate a "noncompetitive" election as one in which the winning margin was more than 1.28 standard deviations away from 50 percent, implying that there was less than a ten percent chance (based on the ex post outcome) that the winning candidate would have lost the election. Because the standard errors have grown over time, later elections had to be won by slightly larger margins to qualify as noncompetitive. In the 1950s and 1960s, our division between noncompetitive and competitive elections occurs at around 55 percent; in the 1970s and 1980s the cut-off point is near 57 percent.

We consider all of the contested elections involving incumbents in our data set. Our results are summarized in Table 4. Column (1) lists the percent of actual elections that fall into our competitive category. Column (2) reflects our estimates of the percentage of election outcomes that would have been competitive if there were no advantage to being an incumbent, holding all else constant. (To attain the figures in column (2), we deducted the estimated incumbency advantage in each year from the actual election outcomes and counted the percent of these hypothetical election results that fell in the competitive range.)

Comparing column (1) to column (2), it is clear that the incumbency advantage greatly reduces the number of competitive elections. For example, if there were no incumbency advantage in the 1980s, over twice as many elections would have been competitive (34.1% versus 15.3%).

Table 4 also demonstrates, as have previous studies, that the number of competitive elections has declined over time. Looking down column (1), we see that approximately eight percent more contests fall into the competitive category in the 1950s (23.0%), than in the 1980s (15.3%), despite controlling for the increased variability in election outcomes.

If the incumbency advantage were responsible for all of the decline in competitiveness, we would expect the numbers in column (2), which control for the incumbency advantage, to be constant over time. As column (2) demonstrates, that claim is borne out by the data: there is no apparent trend in the values of column (2). Between the 1950's and 1980's, the percentage of competitive elections has actually risen slightly once we control for incumbency. Thus, in contrast to King and Gelman(1991), we conclude that the observed decline in the competitiveness of congressional elections over time can be fully attributed to the rising incumbency advantage.¹⁶

2.6 Conclusion

Our approach to measuring the incumbency advantage, utilizing both panel data and a modified sophomore surge analysis, provides unbiased measures of both the incumbency advantage to the party and to the individual candidate. Not surprisingly, we find that both measures have grown since the 1950s. The close

¹⁶ Another piece of evidence to support our conclusion is the fact that there has not been a clear trend in the competitiveness of open seat elections.

correspondence between the two measures suggest that incumbents are, on average, not of substantially higher quality than the typical open-seat candidate. We also find that while direct office-holder benefits are substantial, a large fraction of the incumbency advantage is the result of incumbents' apparent ability to deter high-quality challengers. Virtually all of the growth in the incumbency advantage since the 1960s is attributable to a reduction in the relative quality of challengers. Finally, we find that increases in the incumbency advantage over time are capable of explaining all of the observed decline in the competitiveness of elections.

To the extent that incumbents succeed in deterring strong challengers, a large and growing incumbency advantage can become a self-fulfilling prophecy driven by the strategic behavior of politicians. As the apparent electoral advantage of incumbents grows over time, more and more potentially viable challengers decide not to enter races against incumbents, further reinforcing the apparent invincibility of incumbents. As a consequence, the incumbency advantage continues to grow, though direct office-holder benefits have contributed little to the growth. Developing a clearer understanding of the explanations for the disappearance of the high-quality challenger remains an important topic for future research.

Figure 1: Incumbency Advantage Of the Party

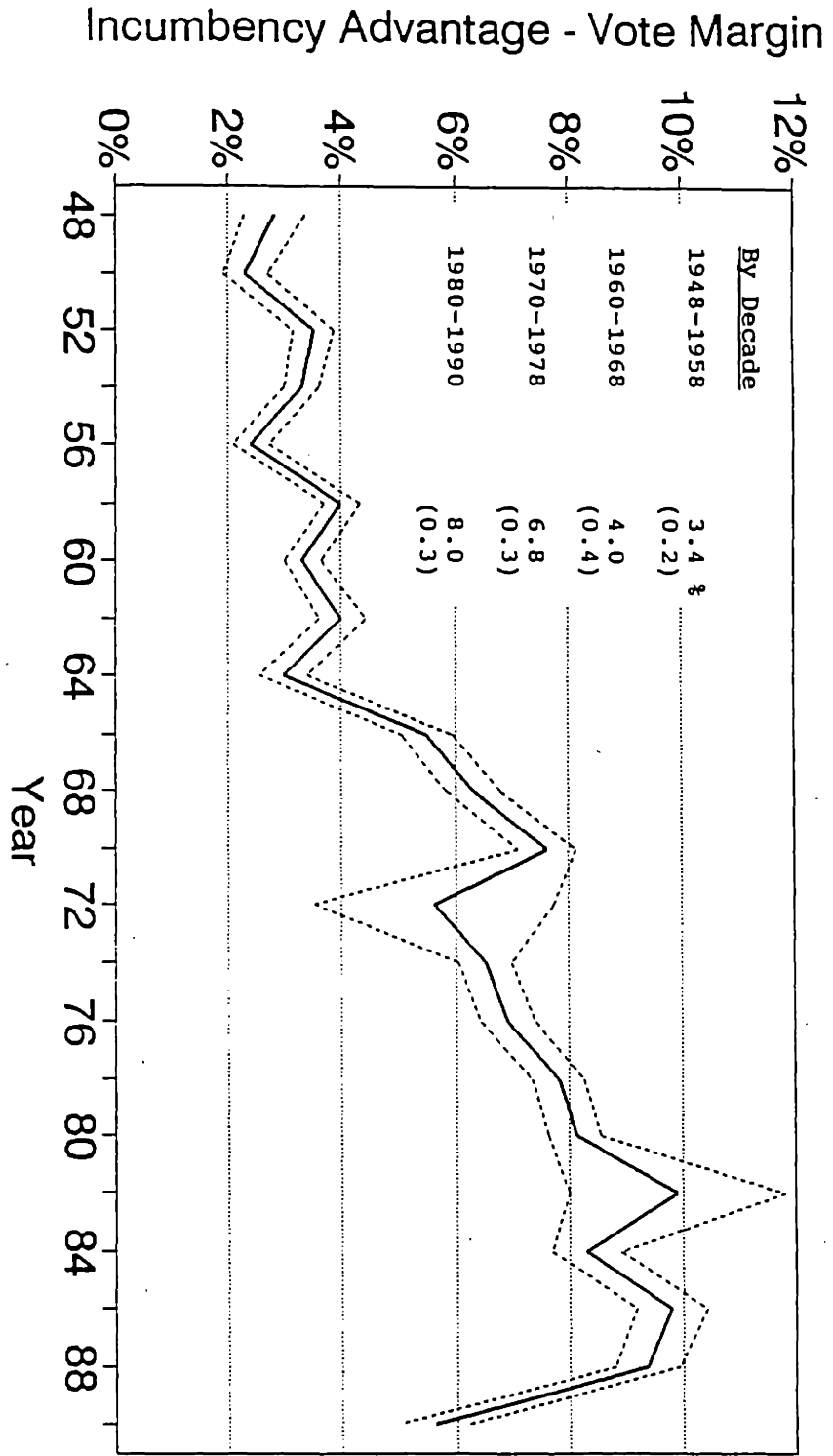


Table 1: The Incumbency Advantage to The Party

<u>Year</u>	<u>Incumbency Advantage to the Party</u>	<u>Year</u>	<u>Incumbency Advantage to the Party</u>
1990	5.6‡ (0.6)	1968	6.3 (0.5)
1988	9.4 (0.6)	1966	5.5 (0.4)
1986	9.8 (0.6)	1964	3.0 (0.4)
1984	8.3 (0.6)	1962	4.0 (0.4)
1982	9.9 (1.9)	1960	3.3 (0.3)
1980	8.1 (0.5)	1958	4.0 (0.3)
1978	7.8 (0.5)	1956	2.4 (0.3)
1976	6.9 (0.5)	1954	3.3 (0.3)
1974	6.5 (0.5)	1952	3.5 (0.4)
1972	5.6 (2.1)	1950	2.3 (0.4)
1970	7.6 (0.5)	1948	2.8 (0.5)

Notes: Estimates based on the specification in equation (3). Fixed-effects for each district and year dummies also included in regression. White-heteroscedasticity consistent standard errors in parentheses. Uncontested elections were eliminated from the sample, as were the first election following redistricting, leaving 5,760 observations. Adjusted R² for the regression is .89, including the fixed-effects. The incumbency variables and year dummies explain 36 percent of the variance that remains after fixed-effects are removed.

Table 2: Estimated Partisan Swings between Elections

<u>Election Cycle</u>	<u>Partisan Swing</u>	<u>Party Benefitting</u>	<u>Party of the President</u>
1988-90	0.6% (0.4)	DEMOC	REPUB
1984-86	2.9 (0.4)	DEMOC	REPUB
1980-82	7.0 (2.6)	DEMOC	REPUB
1976-78	2.2 (0.4)	REPUB	DEMOC
1972-74	5.0 (1.3)	DEMOC	REPUB
1968-70	3.9 (0.5)	DEMOC	REPUB
1964-66	7.1 (0.5)	REPUB	DEMOC
1960-62	1.2 (0.5)	REPUB	DEMOC
1956-58	6.5 (0.4)	DEMOC	REPUB
1952-54	4.4 (0.5)	DEMOC	REPUB
1948-50	3.6 (0.6)	REPUB	DEMOC

Notes: Estimates based on the specification in equation (3). Fixed-effects for each district and incumbency variables also included in regression. See Table 2 for further details of estimation.

Table 3: Estimates of the Sophomore Surge and Direct-Office Holder Benefits

<u>Years</u>	<u>Sophomore Surge Estimates</u>			<u>Direct Office-Holder Benefits</u>
	(1)	(2)	(3)	(4)
1980-90	5.2 (0.6)	5.5 (0.6)	7.6 (0.7)	3.7 (1.0)
1970-78	4.7 (0.5)	5.3 (0.6)	7.4 (0.7)	5.1 (0.7)
1960-68	1.0 (0.4)	2.3 (0.5)	3.6 (0.5)	3.1 (0.4)
1948-58	1.1% (0.4)	2.0% (0.5)	3.4% (0.6)	1.3% (0.6)
<u>Control for:</u>				
partisan swings	No	Yes	Yes	Yes
sample selection	No	No	Yes	Yes
challenger quality	No	No	No	Yes
Observations	684	684	684	122
Adjusted R ²	.23	.34	.47	.42

Notes: Sophomore Surge estimates are based on equation (6), and reflect the change in vote percentages between open-seat contests and the winner's first bid for re-election. Direct office-holder benefit estimates are based on equation (8), and reflect the results in elections involving the same candidates, but with a change in incumbency status. White-heteroscedasticity consistent standard errors in parentheses.

Table 4: Impact of Incumbency Advantage on Competitiveness of Elections
Percentage of Incumbents Involved in "Competitive" Elections

<u>Years</u>	(1) <u>Actual Election Results</u>	(2) <u>Controlling for Incumbency Advantage</u>
1980-88	15.3	34.1
1970-78	20.4	37.3
1960-68	22.3	34.6
1948-58	23.0 %	32.4

Notes: An election is considered competitive if the election outcome is within 1.28 standard deviations of 50 percent, implying at least a 10 percent chance of losing the election. For the 1950s and 1960s, the cutoff for competitiveness is approximately 55 percent; in the 1970s and 1980s the cutoff is approximately 57 percent.

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Chapter 3

An Empirical Test of Competing Explanations for the Midterm Gap in the U.S. House

3.1 Introduction

In the thirty-two midterm elections since the Civil War, the party of the incumbent president has lost seats in the U.S. House of Representatives all but once (1934). The average swing over the period 1856 to 1990 has been close to thirty seats, and the president's party has seen an average decline of approximately four percent of the congressional vote between the presidential election and the following midterm. The purpose of this paper is to test empirically several competing theories of the midterm gap.

There are three leading explanations for the midterm gap. The first is based on presidential coattails (Campbell (1966), Calvert and Ferejohn (1983)). According to this hypothesis, popular presidential candidates provide a coattail boost to congressional candidates of their party when elections are held concurrently. It is

hypothesized that the absence of coattail effects during midterm elections explains the loss of votes for the president's party vis-a-vis the previous presidential election.

The second theory of the midterm gap is based on mean reversion in economic performance. According to this explanation, the party of the president is held accountable for the state of the economy, perhaps because it reflects the administration's competence (Rogoff and Sibert (1988)). As a result, the victorious party in presidential elections will tend to benefit from the state of the economy (i.e., the current administration wins re-election when the economy is strong, but is replaced when the economy is weak). If the economy is only average at the midterm, or systematically underperforms (Nordhaus (1975)), the vote for the president's party will decline at the midterm relative to the preceding election.

A third explanation for the midterm gap is systematic presidential punishment (Alesina and Rosenthal (1989), Erikson (1988)). These theories predict that the party of the president will lose votes, even after controlling for the effects of withdrawn coattails, poor economic performance, presidential approval ratings, and any other relevant factors. In the Alesina and Rosenthal (1989) model of presidential constraint, for instance, polarization of political parties results in pivotal voters whose preferences are more moderate than those of the party in power. As a result, moderate voters shift allegiance away from the president's party at the midterm, generating the familiar midterm effect.

In contrast to the theoretical explanations for the midterm gap, the empirical literature is quite limited. A handful of studies use national-level time series data

(Alesina and Rosenthal (1989), Alesina, Londregan, and Rosenthal (1991), Campbell (1985), Erikson (1988)). Such studies are severely restricted by the lack of available observations -- only 23 elections (11 election cycles) since World War II.

Consequently, the empirical results are not only extremely sensitive to modeling assumptions, but also typically test only one or two explanations of the midterm gap without adequately controlling for competing hypotheses and other potential influences on election outcomes. Coattail effects have also been measured using individual voter survey responses (Calvert and Ferejohn (1983)).

Despite the limited volume of empirical work, the existing literature nonetheless suffers from an embarrassment of riches: together the three theories mentioned earlier appear to account for a midterm gap that is substantially larger than the 3.5 percent average decline actually observed in the post-war era. Erikson (1988) finds that systematic presidential punishment accounts for a decline of 8.8 percentage points, while Alesina and Rosenthal (1989) find presidential punishment equal to 1.8 percent of the vote. Calvert and Ferejohn(1983) find that withdrawn coattails explain a 3.1 percent decline at the midterm. Campbell (1985) estimates that coattail effects account for about half of the seat losses between on-year and off-year elections. Finally, the estimates of Tufte (1975) and Born (1986) suggest that changes in the performance of the economy between the presidential and midterm elections are capable of explaining a greater than one percent decline between the on-year and the

midterm.¹⁷ Combined, past estimates of the three competing theories are capable of explaining the midterm gap more than twice over. Data limitations and failure to control for competing theories appear to have resulted in a set of empirical results that is not mutually consistent.¹⁸

This paper circumvents the data limitations of previous work by using district-level panel data instead of national aggregates. As a result, the number of available observations rises from 23 elections to almost 6,000. District-level data permit the development of a far richer model and nested hypothesis testing of competing explanations. In addition, the panel data model presented here not only provides far more precise estimates, but is also likely to be much less sensitive to the particular choice of specification.

The empirical results obtained in this paper find each of the three prominent theories share an explanatory role. The impact of each, however, is generally smaller than previous estimates might suggest. On average, withdrawn coattails and systematic presidential punishment at the midterm each account for a two percentage

¹⁷ The effect of the economy on the midterm gap is obtained by first computing the deviation from trend in the relevant economic variable between the on-year and off-year elections. That value is then multiplied by the estimated coefficient on the economic variable in the model and averaged over all election cycles.

¹⁸ In the presence of a congressional incumbency advantage, one would expect these three theories to account for greater than one hundred percent of the observed midterm gap. If a party performs well in the on-year election (e.g. due to coattails or a strong economy), that party will have an increased number of incumbents running in the following midterm election and therefore will have a stronger performance than otherwise would be the case. Back of the envelope calculations, however, suggest that the incumbency advantage reduces the observed midterm gap by less than one percentage point.

point decline in the vote for the party of the president at the midterm. The performance of the economy explains roughly one percentage point of decline.

This paper also finds evidence that voters systematically punish the party of the incumbent president in *on-year* elections. That result is interesting not only because it is in stark contrast to the incumbency advantage accruing to members of the US House of Representatives, but also because it is difficult to reconcile with rational voting behavior.

The outline of the paper is as follows. Section two presents the model used to distinguish between the competing hypotheses. Section three presents the empirical findings and interpretations. Section four offers concluding thoughts.

3.2 A Model for Differentiating Competing Explanations of the Midterm Gap

The outcomes of congressional elections are characterized by equations (1) and (2) as follows with the subscript i reflecting the district and t referring to the year of the election.

Presidential Years

$$\text{DemVote}_{i,t} = \alpha_i + \lambda_t \text{IncumStatus}_{i,t} + \beta_1 \text{PresParty}_t + \beta_2 \text{Coattail}_t + \beta_3 \text{GNP}_t + \epsilon_{i,t} \quad (1)$$

Midterm Years

$$\text{DemVote}_{i,t} = \alpha_i + \lambda_1 \text{IncumStatus}_{i,t} + \gamma_1 \text{PresParty}_t + \gamma_2 \text{Gallup}_t + \gamma_3 \text{GNP}_t + \eta_{i,t} \quad (2)$$

where,

$\text{DemVote}_{i,t}$ = Democratic share of the congressional two-party vote in district i in year t ,

α_i = a district-specific constant,

$\text{IncumStatus}_{i,t}$ = incumbency status in district i in year t ; equal to 1 if the Democratic congressional candidate is an incumbent, -1 if the Republican candidate is an incumbent, 0 otherwise,

PresParty_t = the party of the president at the time of the year t election; equal to 1 if a Democratic administration, -1 if Republican,

Coattail_t = a proxy variable for the impact of the presidential race on congressional elections in year t (see text for possible choices of this variable),

GNP_t = the deviation from the post WWII average annual real growth rate in GNP in year t (multiplied by 1 if a Democratic president; -1 if a Republican president),

Gallup_t = the last presidential approval rating prior to the election at time t ; measured as the deviation from sample average 1948-1990 (multiplied by 1 if a Democratic president, -1 if a Republican president), and

$\epsilon_{i,t}$ and $\eta_{i,t}$ = district-specific shocks, assumed to be independently, but not

necessarily identically, distributed with mean zero.

The specifications for on-year and off-year elections are the same except that the coattail variable is replaced by a measure of presidential approval at the midterm. Although the other independent variables are included in both specifications, there is no a priori reason to believe that the independent variables will have an identical effect on the vote in on-year and off-year elections. Therefore, different parameterizations are used in the two specifications.

Where noted, the right-hand side variables are multiplied by negative one when there is a Republican president; such an adjustment is necessary since the dependent variable is in terms of the Democratic share of the vote.

The interpretation of the right-hand side variables is as follows:

α_i reflects the partisan affiliation of district i . α_i is assumed to be constant across the life of a district. When all variables are defined as deviations from means, as is done in the empirical portion of this paper, α_i is properly interpreted as the "normal vote" (Converse (1966)) of a district. Districts that are more favorable to Democratic candidates have higher values of α_i .

The variable IncumStatus controls for the congressional incumbency advantage (Erikson (1971); Gelman and King (1990)). The size of the incumbency advantage is free to vary by year, as reflected by the subscript t on λ . The incumbency status in a district is not only an extremely important determinant of the vote, but also has the potential to distort measurement of the causes of the midterm gap if not accounted for

properly.

The PresParty variable captures the net effect on a House candidate of having his or her party's president in office prior to an election. Institutional biases (e.g. increased fund raising capabilities, or photo opportunities with the president) suggest a positive coefficient; systematic presidential punishment would produce a negative value. The presidential constraint hypothesis predicts a negative sign on γ_1 . None of the theories make a strong prediction on β_1 .¹⁹

Three alternatives are available for estimating the extent of the coattail effect; while none are ideal, together they should provide upper and lower bounds on the true value.²⁰ The presidential vote is one possible option. Estimates of the coattail effect using the presidential vote may be biased upward, however, if there are common factors underlying both presidential and congressional elections that are not properly controlled for in the regression (such as the anti-Republican sentiment in 1976 in the wake of Watergate). The other two proxies are drawn from the estimates of Fair(1988) and Alesina, Londregan, and Rosenthal(1991) (hereafter referred to as ALR). Both of those papers decompose the presidential vote into two elements: the

¹⁹ Note that β_1 captures a very different phenomenon than the coattail effect. The coattail effect refers to the benefit accruing to the party of the *winning* presidential candidate. In contrast, β_1 reflects the benefit or loss associated with being in the *incumbent* president's party. Punishment or rewards in on-year elections are unlikely to be an important component of the observed midterm gap since the incumbent president's party has historically maintained the White House in only about half of the elections.

²⁰ It should be noted that the model presented here assumes that there are no reverse coattails, i.e. the outcome of congressional elections do not affect the presidential race. That assumption appears to be supported empirically (Calvert and Ferejohn 1983).

expected vote based on observable economic/historic conditions, and a residual assumed to capture the intrinsic attractiveness of the presidential candidate himself.²¹ The coattail effect is hypothesized to be a function only of that residual component.²² The shortcoming of the latter two proxies is the possible existence of measurement error. In any case, the true magnitude of the coattail effect is likely to fall in between the estimates obtained using the two decompositions of the presidential vote and the presidential vote itself.

GNP captures the extent to which the party of the current administration is held accountable for the performance of the economy. The variable GNP is defined such that a positive value of the variable reflects either above average growth under a Democratic president, or below average growth for a Republican administration. The model was also estimated using the inflation rate, the unemployment rate, and various combinations of the three economic variables.

GALLUP controls for presidential popularity at the midterm. A positive value of GALLUP reflects either a popular Democratic president or an unpopular Republican. The expected sign on γ_2 is positive.

²¹ The actual breakdown of the data into predicted versus residual components is contained in Table 5 of ALR(1991). With the exception of 1988, for which there were no updated results, Fair's data was used for the estimate of the Fair residuals. For 1988 only, ALR's estimate of the Fair specification based upon their own data is used as a proxy.

²² This implies that the regression coefficient on the expected portion of the presidential vote should be near zero. If that is not the case, either the model in this paper is not well-specified, or the coattail proxies are measured with error.

3.3 Empirical Analysis

Equations (1) and (2) were estimated jointly using a panel data set of congressional election results from 1948-1990. Summary statistics for the sample used are presented in column (1) of Table 1. The normal vote α_i was constrained to be constant over the life of a district by removing all fixed effects at the district level²³, but the other parameters were allowed to vary between on-year and off-year elections. Whenever redistricting occurred, the resulting district was treated as a new and separate district. Because of the indeterminacy of the incumbency status in the first election following redistricting, those observations were discarded. Uncontested elections were also removed from the sample, leaving 5,760 observations. Removing uncontested elections does little to alter the observed midterm gap; the average midterm gap is 3.4 percent in the sample used for estimation compared to an actual value of 3.5 percent for all elections in the post-war period.

Columns (2)-(4) of Table 2 present point estimates for all parameters except the year-by-year incumbency advantages (which are displayed in Figure 1). Heteroskedasticity-robust White standard errors are in parentheses.²⁴ Three different specifications are presented corresponding to the three proxies for the coattail effect.

²³ Removing fixed-effects is equivalent to including a constant for each district. The fixed-effects transformation is required because α_i is not observable. For a review of panel data estimation, see Greene(1990). An alternative specification in which the partisan affiliation of long-lived districts was only constrained to be constant for ten year periods was also estimated with virtually no change in the results.

²⁴ The standard errors reported in columns (2) and (3) may slightly understate the true errors due to the inclusion of estimated values as regressors.

The parameter estimates are quite precise and generally highly significant. All of the variables enter with the expected sign. The choice of the coattail variable has little impact on either the fit of the model or the estimates of the other parameters.

The variables representing the party of the president enter negatively, implying that voters systematically punish the incumbent president's party in *both* on-year and off-year elections. Holding everything else constant, control of the White House costs congressional candidates of that party about two percent of the vote in midterm elections. While that result provides strong evidence for systematic presidential punishment at the midterm, the results are not fully consistent with specific explanations in the literature such as the presidential constraint hypothesis. The fact that the incumbent president's party also does poorly in on-year elections (receiving between 0.3 and 1.0 percentage points less of the congressional vote than would otherwise be expected) suggests that at least part of the observed midterm punishment appears to be attributable to a broader pattern of negative voting aimed towards the incumbent president (Kernell (1977)), rather than a specific attempt to moderate the president's policies. Such anti-incumbent sentiment directed at the president is difficult to reconcile with existing models of rational voters.²⁵

Presidential approval ratings have a relatively small independent impact on election outcomes. A ten point swing in a president's approval rating translates into

²⁵ Nor does that finding provide support for the Fiorina (1988) model of split-ticket voting to constrain the president. That type of behavior will be reflected in a negative value on the coefficients of the coattail proxies (either the predicted or the residual components of the presidential vote). Those values are not negative, suggesting that coattail effects far outweigh split-ticket voting to constrain the president.

about one percentage point of change in the congressional vote. In the case of George Bush, whose approval fell from 76 percent to 53 percent in the two months preceding the midterm in 1990, the loss to the Republican party is estimated to have been just over two percentage points.

Based on the point estimates, the performance of the economy appears to have a greater direct impact on elections at the midterm than in presidential years (although in none of the three regressions can the null hypothesis that changes in GNP have the same effect on midterm and on-year elections be rejected at the .05 level). A one point increase in the growth rate of GNP in the year preceding the midterm election garners the president's party between four-tenths and one half of a percent of the congressional vote. In presidential election years, the direct effect of the economy on congressional outcomes is about half that size. That discrepancy is not surprising since economic factors also work indirectly via presidential coattails in on-year elections. The estimates presented here are all well below those obtained by Tufte (1975) and Born(1986), but are in line with Erikson(1990). In separate regressions (not shown), the inflation and unemployment rates were used as measures of economic performance. The impact of those two variables was generally in the proper direction, but smaller than estimates obtained using changes in GNP. Estimates of the other parameters in the model were generally steady across the specifications.²⁶

²⁶ Estimates of the magnitude of presidential punishment at the midterm ranged from 1.8 percent to 2.4 percent across the specifications. Estimates of presidential punishment in on-years ranged from 0.0 to 1.1 percent. The coefficient on the presidential vote ranged from .38 to .44.

In order to understand the role of economic factors in explaining the midterm gap, the point estimates obtained in Table 1 must be linked to the actual path of the economy. Column (1) of Table 2 presents the impact of the economy on the midterm gap for each election cycle in the post-war period.²⁷ A positive value means that the economy contributed to the midterm gap in a given election cycle. The economy has consistently worked to increase the midterm gap when Republicans hold the White House, but has reduced the midterm gap for the four Democratic incumbents, as previously noted by Hibbs (1977) and Alesina and Sachs (1988). The economy had its greatest impact in the 1980-82 election cycle, accounting for a 3.7 percentage point decline in the Republican congressional vote between the on-year and off-year elections. As a challenger in 1980, Ronald Reagan benefitted from the slow growth preceding the 1980 election; as the incumbent president two years later, his party was held responsible for the sharp recession that began in the summer of 1981 and ran through the midterm election. The Republican party also suffered substantially in the 1972-74 election cycle when economic growth turned negative after the oil shocks. Averaging over the eleven election cycles, economic factors contribute 0.8 percentage points to the midterm gap.

The three proxies for the coattail effect all enter positively. For each

The Fair predicted and residual components had estimates ranging from .43 to .69 and .22 to .39 respectively. The ALR predicted and residual components had estimates from .56 to .60 and -.02 to -.15. The coefficient on GALLUP ranged from 0 to .18.

²⁷ The estimates in column (1) of Table 3 are based upon the point estimates for changes in GNP in the first column of Table 2. The results are altered only slightly when the other specifications are used.

percentage point swing in the presidential vote, four-tenths of a percent of the congressional vote swings to that party according to column (2) of Table 1.

Decomposing the presidential vote into the predicted portion and a residual leads to results that fit fairly well with the theory. The residual component of the presidential vote -- reflecting presidential coattails -- should have a positive impact on congressional elections, while the coefficient on the predicted portion of the presidential vote should be equal to zero as long as common factors underlying both presidential and congressional election outcomes are adequately controlled for and the expected presidential vote is accurately measured. Those predictions are strikingly borne out in column (4) of Table 1 using the ALR breakdown of the presidential vote. Over half of those votes obtained by the presidential candidate due to personal characteristics are translated into congressional coattails, while the predicted portion of the presidential vote has no impact whatsoever. The Fair model in column (3) of Table 1 is less successful; both the residual and the predicted portion of the presidential vote positively affect congressional elections. One possible explanation for the somewhat disappointing results using the Fair model is that some of the variables included on the right-hand side in the Fair model may be endogenous -- a claim made previously by Alesina and Rosenthal (1989). If that were the case, some portion of the true residual would be mistakenly included in the predicted component of the Fair model, leading to the observed results.

Columns (2)-(4) of Table 2 translate the point estimates of the impact of the presidential vote into estimates of the magnitude of withdrawn coattails in each

election cycle. The values in column (2) of Table 2 are obtained by multiplying the coefficient on PresVote by the actual deviation of the presidential vote in a given year from the post-war average (52.6 percent of the vote for the Republican candidate). Columns (3) and (4) are simply the estimated residuals from the Fair and ALR models multiplied by the relevant coefficient from Table 1. The year-by-year estimates are fairly similar across the three columns (the covariance across the three columns is approximately .70). Lyndon Johnson's 1964 coattail boost of between 3.3 and 5.8 percentage points is the largest value across all three specifications. The winning presidential candidate is estimated to have received positive coattails in almost all instances.

The average coattail boost is substantial: 1.9 percent of the vote according to column (2), 0.6 percent in column (3), and 2.0 percent in column (4). The fact that the average coattail effect is virtually identical in columns (2) and (4) is encouraging since there are reasons to believe that these two sets of estimates might be biased upwards and downwards respectively. The similarity in the estimates suggests that common factors underlying both presidential and congressional elections are well controlled for in the regression. In contrast, the lower average value in column (2) provides further evidence that the Fair estimates of the predicted presidential vote may be measured with error, leading to an underestimate of the coattail effect.

Figure 1 presents the year-by-year estimates of the House incumbency advantage along with standard error bands. The incumbency advantage has exhibited a clear upward trend, averaging approximately four percent of the vote in the 1950s

and nine percent in the 1980s. The pattern of estimates obtained are consistent with those of Gelman and King (1990) and Levitt (1993).²⁸

Estimates of the incumbency advantage are somewhat sensitive to the assumption that a district's normal vote is constant over the life of the district. While the actual estimates of the incumbency advantage are of limited interest for the topic at hand, the possibility that other parameter estimates in the model might also be sensitive to that assumption is a quite serious concern. The model was therefore reestimated allowing the normal vote in long-lived districts to change over time. In particular, the normal vote was constrained to be constant for a maximum of ten years.²⁹ Relaxing the assumptions in that way had virtually no effect on any of the coefficients of the model except for the estimates of the incumbency advantage.³⁰ It is clear that the conclusions of this paper concerning the midterm gap are not at all sensitive to the assumption that district normal votes are constant.

3.4 Conclusions

This paper has attempted to gauge the relative importance of competing

²⁸ See Levitt (1993) for a detailed examination of the magnitude, sources, and implications of the incumbency advantage in the context of a similar model.

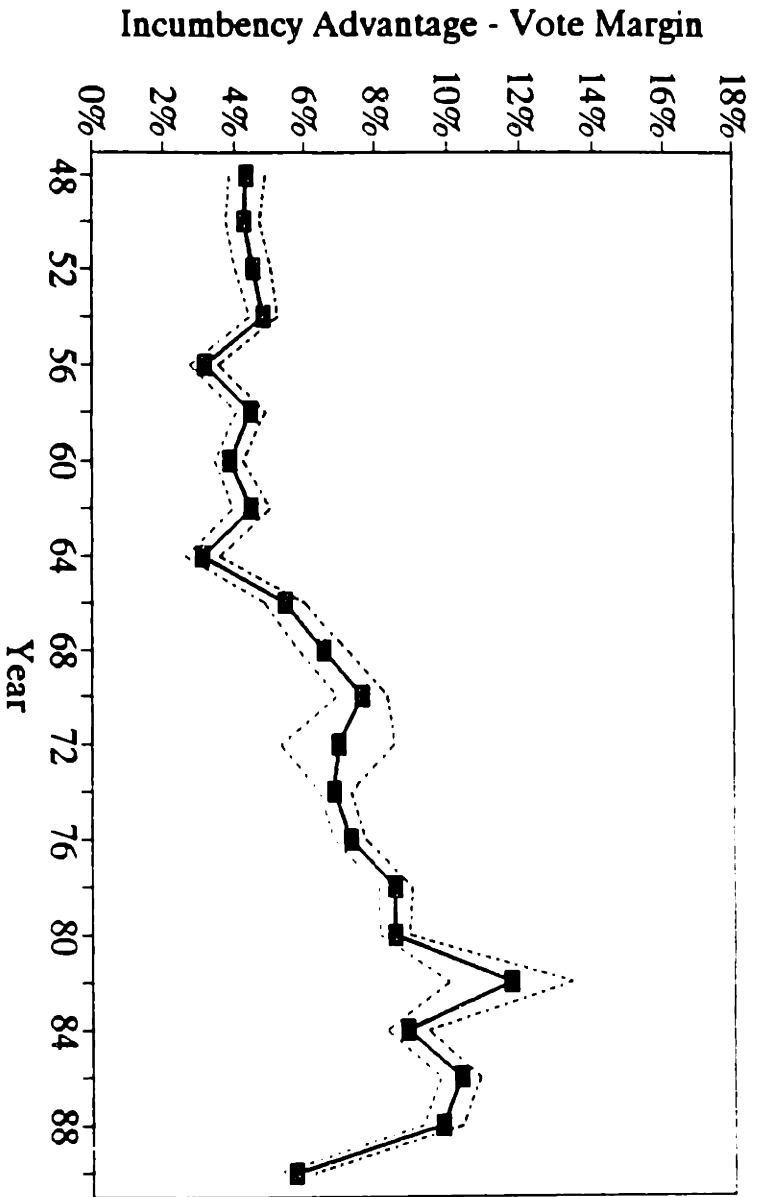
²⁹ Slightly more than sixty percent of the observations in the sample are drawn from districts that existed for ten years or less; these observations are not affected when the assumption in question is relaxed. Approximately eighty percent of the observations are from districts lasting 14 years or less. Ninety five percent of the data is from districts spanning twenty years or less.

³⁰ Full results are available from the author on request.

explanations of the midterm gap. Withdrawn coattail effects and systematic presidential punishment at the midterm each account for approximately two percentage points of decline for the party of the president at the midterm. Economic factors explain one percentage point. Together, those three theories more than explain the observed midterm gap since other factors, in particular the congressional incumbency advantage, systematically work in the opposite direction. Nonetheless, this paper finds the estimated impact of each of the three primary explanations to be smaller than previous estimates in the literature. By reporting results that are sensitive to the choice of specification and neglecting to control for other potential theories, previous researchers have often been too generous in attributing the midterm gap to their theory of choice.

Perhaps the most far reaching finding in the paper is the evidence that voters systematically punish the party of the president in both on-year *and* off-year elections. While some theories predict the latter result, the existence of punishment in presidential year elections is surprising. It not only runs contrary to the large incumbency advantage observed in the U.S. House of Representatives, but also is hard to explain using rational voter models. To the extent that such behavior is also observed elsewhere, for instance in midterm by-elections in the United Kingdom (Mughan(1986)), further study of systematic voter punishment of the incumbent party appears well-warranted.

Figure 1: Incumbency Advantage
1948 - 1990



Notes: Estimates of incumbency advantage based on specification presented in column (2) of Table 1. Dashed lines represent white-heteroscedasticity consistent standard error bands.

Table 1: Estimates of the Determinants of the Midterm Gap

<u>Variable</u>	(1)	(2)	(3)	(4)
	<u>Sample Means</u>			
PresParty at Midterm		-1.9* (0.1)	-1.9* (0.1)	-2.0* (0.1)
PresParty in On-year		-1.0* (0.1)	-0.9* (0.1)	-0.3 (0.2)
Gallup	52.0 (8.0)	.11* (.02)	.10* (.02)	.09* (.02)
GNP at Midterm	2.11 (3.44)	.50* (.06)	.49* (.06)	.41* (.06)
GNP in On-year	3.86 (1.81)	.23* (.09)	.26* (.09)	.36* (.09)
PresVote	55.4 (4.0)	.42* (.03)		
Fair Residual	1.30 (2.24)		.49* (.07)	
Fair Predicted Vote			.37* (.06)	
ALR Residual	3.41 (2.87)			.57* (.03)
ALR Predicted Vote				-.03 (.07)
Adj. R ²		.35	.36	.36

Notes: Dependent variable is Democratic share of the two-party vote in the district. White-heteroskedasticity robust standard errors in parentheses. Asterisk denotes significance at the .01 level. Number of observations is 5760. Degrees of freedom equal to 4231 in column (1), 4230 in columns (2) and (3). Where appropriate, right-hand side variables are multiplied by -1 when the incumbent representative is a Republican. All district-level fixed-effects removed prior to estimation. Yearly incumbency status variable also included in all regressions (see Figure 1). Values in column (1) refer to winning candidate totals where applicable. Voting data and information on redistricting are drawn from America Votes (multiple editions). The Biographical Directory of the United States Congress 1774-1989 and Congressional Quarterly (multiple editions) were used to identify membership changes within a congress. Economic data is from Economic Report of the President, 1992. Presidential approval ratings are taken from The Gallup Poll (multiple editions).

Table 2: Impact of Economic Factors and Withdrawn Coattails

By Election Cycle

<u>Election Cycle</u>	<u>Change in GNP</u>	<u>Withdrawn Coattails</u>		
		<u>PresVote</u>	<u>Fair</u>	<u>ALR</u>
		(1)	(2)	(3)
1948-50	-2.5	2.1	0.5	4.2
1952-54	2.4	1.2	0.5	2.6
1956-58	1.8	2.1	0.7	1.0
1960-62	-1.3	1.1	0.4	2.0
1964-66	-0.8	5.8	3.3	5.1
1968-70	2.0	-0.9	0.6	0.3
1972-74	2.3	3.9	0.7	2.7
1976-78	-1.4	1.6	0.7	2.3
1980-82	3.7	1.1	0.0	1.5
1984-86	1.1	2.8	0.8	0.6
1988-90	1.4	0.6	-1.5	-0.7
Average	0.8	1.9	0.6	2.0

Notes: All estimates based on coefficients reported in Table 2. A positive value implies that a factor contributes to the midterm gap; a negative value means that the factor offsets the midterm gap. The bottom row is an unweighted average of the eleven election cycles. Column (1) is the estimated difference between the actual election results and the hypothetical outcomes if economic growth had equalled its post-war average in the year of both elections in the cycle.

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Chapter 4

How Do Senators Vote: Disentangling the Role of Party Affiliation, Voter Preferences, and Senator Ideology

4.0 Introduction

Whose interests do elected officials represent? The extent to which members of congress' votes are influenced by the preferences of their electorate, specific constituencies within their electorate, the party line, or their own ideological beliefs remains an open question.¹ The primary difficulty that arises in attempting to answer that question is the lack of observability of the variables in question, especially

¹ The literature addressing this question is voluminous. Notable recent contributions include, but certainly are not limited to Bender (1991), Bullock and Brady (1983), Davis and Porter (1989), Jung et al. (1991), Kalt and Zupan (1984, 1990), Lott and Davis (1991), McArthur and Marks (1988), Nelson and Silberberg (1987), Peltzman (1984), Shapiro et al. (1990), Thomas (1985), Van Beek (1991), Wright (1989), and Zupan (1990).

ideology. An elected official's ideology is likely to be correlated with both party affiliation and voter preferences. Therefore, if one does not observe ideology, estimates of the effect of other variables on voting behavior are likely to be biased. This paper develops a methodology for estimating the relative weights that senators place on various factors that does not require senator ideologies to be observed in order to yield consistent estimates, and then proceeds to estimate the model on data from 1970-1991.

There are a number of competing theories to explain the voting patterns of senators. The median voter theorem predicts that senators will represent the median voter among the state electorate. If he consistently fails to support the position favored by the median voter, he will be voted out of office. Actual voting patterns in the Senate, however, contrast sharply with that simple prediction. Senators representing the same states in the same years -and thus facing the same median voter- exhibit radically different voting behavior when not affiliated with the same party (Poole and Rosenthal 1984). As Table 1 demonstrates, when a state has a mixed-party senate delegation (i.e. one republican senator, one democrat), those senators' votes are only slightly more similar than one would expect from a random draw of two senators.

The "dual constituency" hypothesis (Fiorina 1974) has been offered as an alternative to the median voter theorem. That hypothesis predicts that elected officials will place extra weight on the preferences of their supporters within the electorate. Possible explanations for such behavior are the existence of primaries and the

likelihood that financial support is concentrated in this group. The close correspondence in Table 1 between ADA scores of senators from the same state and party, who are likely to share quite similar constituencies, is consistent with such a hypothesis.

Another possibility, argued most recently by Cox and McCubbins (1993), ascribes a strong role to national parties. If party allegiance is an important determinant of an official's success in the congress (as seems to be the case, for instance, in terms of committee assignments in the House (Rohde and Shepsle 1973, Smith and Ray 1983), then differences in voting behavior among senators from the same state, but different parties, might be causally attributed to senators altering their voting patterns to more closely match the party line. The fact that members of the same party exhibit substantially less variability than do senators as a whole in Table 1 is consistent with the existence of strong national parties.

An alternative to all of the above hypotheses, however, is that senators simply vote their own ideologies without regard for the interests of the electorate or the party line. The observation that senators from the same state and party tend to vote similarly may reflect the fact that they are drawn from a pool of candidates with relatively similar ideologies, rather than being evidence that they weigh heavily the interests of their constituents. Along the same lines, the observation that Democrats across states tend to have similar voting records may simply reflect the fact that liberal candidates tend to run as Democrats while conservatives run as Republicans.

The analysis of this paper attempts to separate correlation and causality. The

key identifying assumption is that a senator's ideology remains fixed over time.

Under that assumption, using roll-call voting behavior of a state's House delegation to proxy voter preferences and the roll-call votes of party leaders to proxy the party line, it is demonstrated that the relative weights assigned to the various factors can be ascertained even though senator ideology is not observed.

The ability to estimate explicit weights for the competing factors in the senator's decision function is the primary methodological advance of the paper. In addition, the framework developed here is ideal for testing a wide variety of hypotheses concerning senator voting behavior. For instance, the issue of "political shirking" (Kalt and Zupan 1990, Zupan 1990, Lott and Davis 1991) can be addressed by comparing changes in the weights assigned to state voters as election years or retirement approach. The extent to which any systematic differences exist between first-term senators and others, Democrats and Republicans, and senators holding "safe" versus marginal seats can also be analyzed within the framework. Finally, explicit estimates of senator ideologies are obtained, which may prove useful to future researchers.

My results suggest that senators are most influenced by their own ideologies, assigning approximately forty to fifty percent of the weight to that factor. State voter preferences are assigned a relatively minor weight. Preferences of voters in a senator's own constituency are three times more important than preferences of state voters outside the senator's constituency in determining the senator's voting behavior. Estimates of the importance of the national party line vary from about twenty-five

percent of the senator's weight down to close to zero.

While interpretation of the actual weights must be approached with some caution, the relative patterns in the weights across different situations are likely to be more robust. The weight that senator's place on the preferences of state voters rises sharply as an election approaches, with a commensurate decline in the importance of the party line. That result suggests that senators are at least somewhat constrained by elections. Furthermore, the fact that changes in senator voting are concentrated in the year of the election implies that senators consider voters to be quite myopic. Senators do not, however, appear to dramatically alter their voting patterns in the year preceding retirement. In contrast to more senior senators who weigh the interests of voters in their own constituency more heavily, first-term senators appear to put roughly weight on the preferences of all voters in the state. There is no apparent change in the weight assigned to the party line as tenure increases, but senators in "safe" seats place a higher weight on the party line. Somewhat surprisingly, weights do not appear to vary systematically across party line, or between Northern and Southern Democrats.

The paper is organized as follows. Section I develops the model underlying the empirical specification, and demonstrates that the model can be estimated without directly observing senator ideologies. Section II describes the choice of proxy variables and the sample to be analyzed. Section III presents empirical estimates of the basic specifications, while section IV considers extensions of the model. Section V offers a brief set of conclusions.

4.1 The Model

It is assumed that senators potentially take into account four different sets of interests when determining where to position themselves in the policy space:

- 1) Overall preferences of the state electorate
- 2) Preferences of his/her particular constituency within the state electorate
- 3) The party line
- 4) The senator's personal preferences or ideology

Arguments for including those four factors in a senator's decision calculus are straightforward and well established in the political science literature.

In analyzing the influences on a senator's voting, I focus on the overall positioning of a senator's voting record in policy space, rather than on the breakdown on any specific vote. Solely for simplicity and consistency with the empirical estimation that follows, I assume that the policy space is unidimensional. The underlying logic readily generalizes to n-dimensional space. The problem is formalized by assuming that the senator minimizes a weighted average of the squared distances from the bliss points of the four different sets of interests as follows:

$$U_i = - [\alpha(V_i - S_i)^2 + \beta(V_i - C_i)^2 + \gamma(V_i - P_i)^2 + (1 - \alpha - \beta - \gamma)(V_i - Z_i)^2]$$

(1)

where

V_i = senator i's voting profile in year t,

S_{it} = the bliss point of the voters in state i in year t ,

C_{it} = the bliss point of the senator's particular constituency within the state in year t ,

P_{it} = the bliss point of senator i 's party in year t ,

Z_i = the senator's ideological bliss point, assumed to be constant over time.

Two comments concerning the specification of the utility function are warranted. First, the use of squared distances in the utility function is only appropriate if all of the bliss points and the voting profile V_{it} are measured in the same units. While this is a seemingly minor point, it will in fact limit the flexibility available in choosing variables to include when estimating the model. Secondly, since utility functions are defined only up to an affine transformation, there is no loss of generality implied by the fact that the decision weights are constrained to sum to one; this is simply a convenient re-scaling, and has no bearing on what follows.

Maximizing the above function with respect to the senator's vote V_{it} yields a senator's optimal voting record V_{it}^* which is simply a weighted average of the four bliss points:

$$V_{it}^* = \alpha S_{it} + \beta C_{it} + \gamma P_{it} + (1-\alpha-\beta-\gamma)Z_i \quad (2)$$

The basic problem in applying equation (2) to actual data is that the bliss

points are not directly observable to the researcher. The strategy for overcoming this difficulty has two elements. First, where reasonable proxy variables are available (i.e. for state voter preferences, constituency preferences, and party preferences), they are employed. Concerns over possible endogeneity and/or errors-in-variables can be dealt with empirically through the use of instrumental variables.

For senator ideologies, however, no reasonable proxy is available.² Given the availability of proxies for the other variables, however, this absence does not pose a problem. Rewriting equation (2) in indicator variable notation

$$V_{it}^* = \alpha S_{it} + \beta C_{it} + \gamma P_{it} + [(1-\alpha-\beta-\gamma)Z_i] * I_{it} \quad (3)$$

where I_{it} equals one if the observation in question is for senator i , and zero otherwise.

Equation (3) can be estimated by including a senator-specific constant, which is equivalent to a fixed-effect for each senator. The estimates of the coefficients associated with the fixed-effects are comprised of two components: the senator's ideology, and the weight the senator places on his own ideology in his utility function. Because estimates of the weighting parameters α , β , and γ are obtained from a regression of equation (3), the weight a senator places on his own ideology ($1-\alpha-\beta-\gamma$) can be determined. Knowing that weight, parameter estimates of each senator's ideology can also be obtained. Put another way, equation (3) is identified in

² In particular, a senator's past voting record is not a proxy for ideology since it is a function of the three other factors as well as ideology.

the formal econometric sense, even though senator ideology is unobserved.

4.2 Data Choices

In applying the model of the previous section to the data, three sets of choices are required. First, the units of measure must be defined. Secondly, proxy variables must be selected. Finally, the appropriate sample needs to be identified. This section outlines those three choices.

Units of Measure

The logical choice for a unit of measure is the voting scores compiled annually by the ADA.³ ADA scores have been the standard measure in the political shirking literature (e.g., Kau and Rubin 1979, Kalt and Zupan 1990, Lott and Davis 1991) and are available for both the House and the Senate over a long time period, as this analysis requires.⁴

ADA scores have three potential shortcomings. First, they assume that the relevant policy space is unidimensional along a liberal-conservative scale. Poole and Rosenthal(1985, 1991) demonstrate, however, that the relevant policy space,

³ The ADA bases their rating on approximately twenty roll call votes each year. Those votes are selected for their importance and a well-defined liberal position on the issue. Scores are scaled such that a senator who votes with the liberal position on every vote receives a score of 100, while a senator who always opposes the liberal position receives a 0.

⁴ There has been a heated debate over whether ADA scores are a measure of ideology. That criticism of ADA scores is not relevant here since ADA scores are just used here as summary statistics for roll call voting, and are explicitly differentiated from ideology in the model.

especially in the period examined here (1970-1991), is quite well captured by a single dimension. ADA scores are highly correlated with other interest group ratings and the NOMINATE scores obtained by Poole and Rosenthal.⁵

A second drawback of ADA scores is that they exhibit censoring (i.e., scores are restricted to fall between zero and one hundred) that may lead to inconsistent parameter estimates. In practice, however, only about ten percent of the senators receive scores of 0 or 100 in a given year. As a check for bias induced by censoring, the basic specifications of the following section were replicated using Symmetrically Trimmed Least Squares (Powell 1986), an estimation technique that is robust to censoring. In all cases, the estimates of the weighting parameters in the utility function were virtually unchanged, suggesting that censoring is not a critical issue.⁶

A final criticism of the ADA measure is that it, like other interest group scores, is subject to artificial extremism (Snyder 1992). While that is a potential problem, it is unclear a priori what type of bias that might introduce into the current analysis. To test this possibility, the author is in the process of applying the methodology developed here using NOMINATE scores, which are not subject to

⁵ To investigate this issue further, however, the author is currently undertaking a study employing a similar methodology, but using *National Journal* ratings as a unit of measure. *National Journal* ratings rank members of Congress along three dimensions: economic, social, and foreign policy.

⁶ In contrast to the weighting parameters, the estimates of the senator ideologies were somewhat affected by censoring. When censoring was taken into account, estimates of the ideologies were more extreme than in the results reported in the following section. The relative ordering of senators was virtually unaffected, however. The correlation between the estimated ideologies using Two Stage Least Squares and Symmetrically Trimmed Least Squares is greater than .95.

artificial extremism.

Choice of Proxy Variables

Proxy variables are needed for state voter preferences, constituency preferences within the state, and the national party line. The framework developed in Section I imposes an important restriction: in order for the model to be identified, all of the proxies must be scaled by the same units as the dependent variable, namely ADA scores.

A logical proxy for state voter preferences in a given year is the mean ADA score across the state's House delegation in that year.⁷ The mean House ADA score is strongly correlated with other possible measures of a state's liberal-conservative position such as the percent of the state's presidential vote cast for the Democratic party. Moreover, there is a high degree of overlap between the issues covered by the roll call votes that are used to calculate the two House and Senate ADA scores in a given year. Therefore, to the extent that a single ADA score reflects not only overall liberal tendencies, but also stands on issues of particular interest to a state, the mean House ADA score may better capture state voter preferences than would measures based off the presidential vote.⁸

⁷ This analysis requires that ADA scores in the House and the Senate be comparable. Poole and Daniels(1985) provide extremely convincing evidence to support that claim.

⁸ Survey data provides a more direct measure of constituency preferences. The problem with survey data in this context is that there is no simple way to scale survey responses in units of ADA scores. While survey responses could be used as instruments for the state voter preference proxies, I have not done so.

One possible concern with using mean House ADA scores as a proxy is that they may be distorted by the party composition of a state's delegation. For a given set of underlying voter preferences, a House delegation with more Democrats is likely to have a higher mean ADA score. Two measures are taken to minimize that potential problem. First, states with small House delegations (where the distortion is likely to be greatest) are excluded from the analysis. The second approach is to use instrumental variables. If there are variables that are correlated with a state's true voter preferences, but not correlated with the measurement error in the proxy, then instrumental variable techniques can be used to circumvent the errors-in-variables problem (Greene 1991). In particular, I instrument for state voter preferences using the Democratic share of the state's presidential vote in presidential elections, and the ADA score for the other senator from the state.

The preferences of a senator's particular constituency within a state is proxied using the mean House ADA score among members of that senator's state *and* party. That variable is somewhat crude, overlooking the possibility that two senators from the same state and party may represent different constituencies, or that constituencies may cross party lines. While more direct measures of constituency would clearly be preferred, the restriction that the explanatory variables be scaled in ADA scores precludes that possibility. The fact that senators from the same state and party typically exhibit quite similar voting patterns (see Table 1) somewhat lessens concern

on that score.⁹

Two proxies are considered for the national party line: the mean ADA score of the party leadership¹⁰ in a given year, and the mean ADA score of all party members in that year. While the voting scores of the party leadership correspond more closely to the theoretical notion of a party line, concerns over possible noise in that measure due to the small number of senators upon which it is based makes use of a broader measure of party preferences attractive. In any case, the two proxies are highly correlated ($\rho > .90$).

The possible endogeneity of both party proxies is an important concern. What one hopes to capture with a party line variable is the extent to which senators alter their behavior based upon pressure from other members of the party (relative to how they would have behaved in the absence of party pressure). If, however, there are common "shocks" to ADA scores across senators of a given party (due, for instance to the particular set of votes included in the calculation of ADA scores that year), these shocks will mistakenly be attributed to the party line in the regression. Thus, in the presence of common party shocks, the coefficient on the party proxy is likely to be biased upwards using OLS. The use of instrumental variables is once again the way to eliminate this source of bias. The party proxies are therefore instrumented

⁹ In contrast to the case of overall state preferences, the party composition of a state's House delegation is not likely to greatly distort this proxy.

¹⁰ The party leadership was defined as the floor leader, the party whip, the chairman and secretary of the conference committee, the President Pro Tempore, and the chairman of the Republican policy committee.

using their once lagged values in what follows. The lagged values are likely to be excellent instruments since they are highly correlated with the current value of the proxies, but are not contaminated by the particular set of votes used to compute ADA scores in the current year.

The Choice of Sample

Senator voting records over the twenty-two year period 1970-1991 are considered. Because of concerns over errors-in-variables in the state voter preference proxy (discussed above), senators representing states with House delegation of three members or less are excluded.¹¹ This exclusion reduces the sample size from 2,200 to 1,488.

A second consideration in choosing a sample is the fact that an estimate of each senator's ideology must be obtained. For those senators who serve for only a few years in my sample (either because their term(s) only partially overlap with my sample, or because of death, resignation, or appointment to a partial term), those estimates are quite imprecise. Moreover, the imprecision of those estimates has an adverse impact on the standard errors on the estimates of the weights of the utility functions. For that reason, the sample is restricted to include only those senators for which there are at least six years of observations in the sample. While that could

¹¹ The cutoff for House delegation size was chosen based on the following logic. The errors-in-variables problem suggests that the coefficient on the proxy will be biased downwards. Therefore, I estimated the regressions, sequentially eliminating the one member delegations, two member delegations, etc. The elimination process was stopped when the coefficient on the state voter preference proxy no longer increased when another round of delegations was removed.

potentially induce sample selection bias, the regressions in the following section were replicated without this sample restriction with virtually identical point estimates.¹² Including only senators that serve a minimum of six years in the sample reduces the number of parameters to be estimated by 54, while lowering the available observations from 1,488 to 1,329. Finally, the observations on James Buckley (Conservative-NY) are eliminated from the sample because he was not affiliated with a major party, leaving 1,323 observations.¹³ Summary statistics for the variables used in the analysis, defined over this restricted sample of 1,323 observations, are provided in Table 2.

4.3 Empirical Estimates

Regression estimates of the basic model, using the variables and sample defined in the previous section, are presented in Table 3. In addition to the variables specified in equation (3), year dummies were also included in all specifications to capture any systematic variation in ADA scores over time.

Columns (1)-(3) of Table 3 use the ADA scores of party members as the proxy for the party line; columns (4)-(6) use ADA scores of party leaders as a proxy.

Columns (1) and (4) are OLS estimates. Columns (2) and (5) instrument for the party

¹² Note that sample selection bias is not a problem for senators excluded at the tail end of the sample, but rather is only a potential problem for those senators who retire or lose at the very beginning of the sample period.

¹³ Including Buckley as a Republican had no impact on the parameter estimates or standard errors.

proxies using lagged values of those proxies to eliminate potential endogeneity. Columns (3) and (6) instrument for both party and state voter preferences, with the latter variable being instrumented by the average deviation from the national mean Democratic share of the presidential vote in the state in the two most proximate presidential elections¹⁴ and the ADA score of the other senator in the state. In all cases, the sum of the weights in the utility function were restricted to equal one, and senator ideologies were assumed to be constant over time.

Looking across all of the specifications, it is reassuring to note that all of the weights are positive, although that restriction was not imposed. The high adjusted R^2 values in the OLS cases imply that the regressions are able to explain virtually all of the variation in senator ADA scores.¹⁵ Estimated weights for state voter preferences and a senator's constituency within the state are not very sensitive to either the choice of proxy for the party line, or instrumenting. In contrast, the weights on party line and ideology do vary significantly across the specifications. In particular, the estimated weights placed on the party line fall dramatically when the party line proxy is instrumented using lagged values (columns (2)-(3) and columns (5)-(6)). That result is precisely what one would expect to observe if there are common shocks to ADA scores across senators of the same party. Consequently, I will largely ignore the OLS results in the analysis that follows since those results appear to be biased by

¹⁴ Because of complications caused by the Wallace candidacy in 1968, only the 1972 presidential vote was used for observations in 1970-1972.

¹⁵ R^2 is not a meaningful statistic when IV is employed. For instance, it is not bounded between zero and one.

the endogeneity of the party proxies.

In contrast to the party proxies, instrumenting for state voter preferences (columns (3) and (6)) has little impact on the parameter estimates, suggesting that possible errors in these proxy variables is not leading to large biases in the coefficients. As would be expected, however, the standard errors on the estimates increase because the correlation between the instruments and the proxy, while high, is less than perfect.¹⁶

Regardless of specification, senators appear to place relatively little weight on the preferences of state voters. Whereas the median voter theorem would predict a coefficient near one on that variable, the estimates are generally less than .20. This implies that two senators from the same party and sharing identical ideologies, whose state voters are 50 ADA points apart, will on average differ by only 10 points in their ADA scores. The low weight on state voter preferences is consistent with the evidence for political shirking elsewhere in the literature.

The weight placed on a senator's constituency within the state approximately matches that assigned to state voters as a whole. The equality of those parameters, however, is somewhat deceptive since the specific constituency is also included in the overall state voter preferences. For example, assume that a senator's constituency comprises fifty percent of the overall state electorate. Using the estimates in column (2), if the preferences of everyone in the senator's constituency increase by one ADA

¹⁶ Tests of the overidentifying restrictions in columns (3) and (6) are just able to reject the assumption of exogeneity of the overidentified instruments at the .05 level. Therefore, some caution in interpreting the coefficients in those columns is warranted.

point, the senator will alter his voting position by .265 points (.18 directly through the parameter on constituency, and $.5 \times .17$ since the constituency is half of the overall state electorate). If everyone in the state *outside* the senator's constituency changed their preferences by one ADA point, the senator's position would shift by only .085 points ($.5 \times .17$ since those outside the constituency are one half of the overall state electorate). Thus, the results of the regressions suggest that senators place upwards of three times as much weight on the preferences of those within their constituency as they do on those outside their constituency.¹⁷ These results therefore provide strong support for the dual constituency hypothesis.

The weight devoted to the party line is consistently greater when mean ADA score of party members are used (columns (1)-(3)) instead of mean ADA score of party leaders (columns (4)-(6)). The most plausible interpretation of this result is that there is a substantial amount of noise in the latter measure inducing the familiar downward bias often caused by errors-in-variables problems. If one accepts voting by party members as the proxy for the party line, parties appear to exhibit a substantial influence on senator behavior, receiving a greater weight than either of the variables corresponding to voter preferences. Thus, it appears that party membership is an important determinant of senator voting, even after controlling for constituency preferences and senator ideology.

With the exception of the OLS estimates, which are likely to be biased,

¹⁷ Moreover, if the proxy that I am using for constituency is imperfect, that ratio is likely to be even more extreme.

ideology is the single most important determinant of senator voting patterns, receiving a weight of between .37 and .54. Nonetheless, these weights are well below one, implying that senators do not simply vote as they please. While maintaining a large degree of independence, senators do adjust their voting behavior to conform to the wishes of their constituents, party, and to a lesser extent other voters in the state.

The regressions in Table 3, in addition to providing estimates of the weights in the utility function, also generate estimates of the senator ideologies. There are two reasons why some caution must be used in interpreting those estimated ideologies too literally. First, there is censoring in the dependent variable, which will tend to compress the estimated ideologies.¹⁸ Secondly, the actual estimates of the ideologies appear to be fairly sensitive to the particular specification (although the rank order of the ideologies is virtually unchanged across specifications). Nonetheless, summary statistics for estimated ideologies obtained from the specifications in columns (2) and (5) are presented in Table 4. Both mean ideologies and the percentage of the estimates that fall below zero, between zero and one hundred, and above one hundred are presented.¹⁹ The means of the estimated ideologies appear reasonable. The mean senator ideology (scaled by ADA scores) is estimated to be approximately 54 in

¹⁸ As one would expect, when procedures that account for censoring, such as Tobit, were used, the estimates of the ideologies were more extreme. The rank order of the ideologies was virtually unchanged, however, and the correlation between the sets of estimates was approximately .97.

¹⁹ Ideologies are allowed to be unbounded. An ideology greater than 100 implies that a senator is even more liberal than the ADA. A value less than zero suggests that even if a group less liberal than the ADA chose the votes from which the voting record was determined, the senator's ideology would lead him to vote against all of the liberal positions.

both specifications. This value is slightly higher than the mean senator voting score (46.6) in the sample. The gap between the ideologies across parties is more extreme than is the gap in actual voting patterns. Democratic senators are on average ideologically further to the left than their voting patterns might suggest, whereas Republican ADA scores are on average quite close to the mean estimated ideology. In contrast to the estimated means, the span of the distribution of ideologies varies substantially across specifications. The estimates in column (1) of Table 3 are much more dispersed than are the values in column (2).

A full listing of estimated ideologies for senators in the sample is provided in Appendix A. John East (R-NC) is the most conservative senator in the sample with an estimated ideology of -16. Tom Harkin (D-IA) is the most liberal senator with a score of 126. In general, there are few surprises. One point of interest is that most of the serious Democratic presidential candidates that have emerged from the Senate (Humphrey, Kennedy, Tsongas, Hart) are closely clumped with estimated ideologies between 90 and 100. Walter Mondale, at 116, is an exception to that pattern.

The estimates obtained here important implications for a wide range of political science research. An on ongoing controversy in the political science literature concerns the extent to which voting records are satisfactory proxies for ideology (as, for instance, Peltzman(1984) assumes). The large estimated weight on ideology obtained in this paper suggests that voting records are a fairly good proxy for ideology. Furthermore, the raw correlation between estimated senator ideologies and voting records is even higher than the weights in Table 3 might suggest:

approximately .90 in my sample. The high correlation is a consequence of the fact that party affiliation and voter preferences are each correlated with both estimated ideologies and voting records.

The flip side of the coin, however, is that studies that use voting patterns as a dependent variable, but do not directly control for ideology (as virtually none have due to the lack of available proxies), may be seriously flawed due to that omission. The fact that my estimated ideologies are strongly correlated with party affiliation ($\rho = .46$), state voter preferences ($\rho = .53$), and with preferences of a senator's constituency within the state ($\rho = .72$), intensifies concern over the likelihood and magnitude of bias resulting from the omission of ideology as an explanatory variable.

4.4 Extensions of the Basic Specification

Due to the possible existence of errors-in-variables, the estimated weights presented in the preceding section must be interpreted with some caution. By comparison, differences in the estimated weights across subsets of senators, or as elections near, are likely to be more robust. This section analyzes a number of different hypotheses concerning senate voting patterns. In all cases, specifications are estimated using the two different party proxies (instrumented by their once-lagged values). Therefore the results reported below are variations on the results reported in columns (2) and (5) of Table 3. In all regressions, senator ideologies were constrained to be constant over time.

One extension of the basic specifications is to allow the weights of first-term

senators to differ from those of more senior senators. Results are presented in Table 4. Columns (1) and (2) use party members as the proxy for the party line; columns (3) and (4) use party leaders as the proxy. The initial estimate of weight on the party in column (3) was negative, but insignificantly different than zero (-.07 with a standard error of .12). In the results reported, however, the regression was re-estimated constraining that weight to be greater than or equal to zero. Only for the second pair of regressions can the null hypothesis that the parameters for first-termers and all others are identical be rejected.²⁰

While the estimates on voter preferences are fairly precise and consistent across the two sets of estimates, the weights on the party line and ideology are volatile and imprecisely estimated. The primary conclusion from Table 5 is that first-term senators put substantially more weight on state voter preferences overall, but are less responsive to the specific interests of their own constituency. For instance, assuming that a senator's constituency represents one-half of the electorate, the coefficients in column (1) suggest that first-term senators weigh the interests of all voters in the state approximately equally. A one ADA point change in preferences for voters outside the constituency leads to a .135 point change in the senator's position, whereas the same shift for voters in the constituency leads to .175 point change on the part of the senator. In contrast, column (2) implies that more senior senators place three times as much weight on members of their constituency (.085

²⁰ The test statistics (distributed $F_{3,1190}$) for the tests of identical coefficients for first-termers and all others are respectively 1.85 and 6.40.

outside the constituency, .295 within the constituency).

Table 5 compares the estimated weights of senators who are running for reelection in a given year, senators who are retiring,²¹ and all others.²² Columns (1)-(3) use party members as the proxy for the party line; columns (4)-(6) use party leaders as the proxy. The patterns obtained are quite pronounced and are present in both sets of regressions. With the exception of column (3), the null hypothesis that senators' behavior does not change in election years (whether running or retiring) is rejected at the .05 level.

Senators who are running for reelection significantly increase the weight they place on state voter preferences, while lowering their allegiance to the party line. The tendency for senators to weigh the interests of their own constituency more heavily than other voters in the state is only slightly attenuated in election years; columns (2) and (4) imply that even in election years, constituent interests are more than twice as influential as those of voters outside the constituency. Interestingly, the weight that senators place on their own ideology is not affected by elections.

The weighting profile of senators whose retirement is imminent is quite similar to that of senators in non-election years. Any difference between retiring senators and non-election year senators would appear to be an increase in weight devoted to

²¹ A senator is classified as retiring if he does not run in the primary or general election in the last year of his term, and does not run for either the presidency or the governorship.

²² Initially, parameter weights were allowed to vary according to the number of years to election. For 2-6 years to election, however, there were small differences in the point estimates across years, and the hypothesis of no difference between the parameters across those years could not be rejected.

the party line, accompanied by a reduced weight on ideology. That result is quite counter-intuitive. In light of the imprecision with which the weights on party and ideology are estimated, and the limited number of observations on retiring senators in the sample (26), strong conclusions do not appear justified.²³

The results of Tables 4 and 5 are relevant to the current debate on term limits. Term limits are likely to have two effects: increasing the number of first-term senators, and increasing the number of senators that are not up for future reelection. The imposition of term limits, based on the coefficients obtained in this analysis, would have little effect on the overall weight assigned to the various factors in the senator's utility function. Since first-termers tend to vote in a fashion quite similar to candidates up for re-election, an increase in the former and a decrease in the latter are likely to largely offset.

Table 6 compares the weights of senators holding "contested" versus "safe" seats. The distinction between contested and safe is necessarily somewhat arbitrary: a seat was taken to be safe if the senator received greater than sixty percent of the popular vote in the last election, and was otherwise classified as contested. The estimate of the weight given to party was initially negative, but insignificantly different from zero (-.13, with a standard error of .11) in column (3), and was constrained to be greater than or equal to zero in the results reported.

²³ One possible explanation for this finding, suggested by Zupan(1990), is that retiring senators are not representative of the sample of senators as a whole. Retiring senators tend to be quite senior. In order to have survived the selection process, retiring senators may as a group tend to put less weight on their own ideology.

While similar patterns emerge in both sets of estimates, differences across safe and contested seats are much more pronounced in columns (3) and (4).²⁴ Senators holding safe seats put substantially more weight on the party line. That result is consistent with the reduced weight placed on the party line by senators that are up for re-election in Table 5. Adherence to the party line appears to be a political luxury; when re-election is in question other factors take precedence.

Senators holding contested seats appear to place more weight on the preferences of their particular constituency within the state, but are no more responsive to voters outside their constituency. It is unclear, however, which way the causality runs. Senators in contested seats may more closely abide by their constituency interests in order to guarantee financial support for upcoming elections. On the other hand, some senators may simply put less value on re-election, and therefore are willing to serve their own constituents interests more closely, even if the likelihood of re-election is lowered.

Finally, parameter weights in the utility function were allowed to vary across Northern Democrats, Southern Democrats, and Republicans.²⁵ Perhaps surprisingly, the null hypothesis that the weights are identical across those three groups could not be rejected at the .10 significance level using either of the party proxies. Moreover, no systematic patterns emerge in comparing the coefficients obtained using the

²⁴ The null hypothesis of no difference between safe and contested seats can be rejected at the .10 level in columns (1) and (2), and at the .01 level in columns (3) and (4).

²⁵ The definition of Southern was the same as that used by *Congressional Quarterly*.

different party proxies and either OLS or the various instruments available, further reinforcing the conclusion that the weights do not vary across parties.

4.5 Conclusions

This paper has attempted to disentangle the relative weights that senators assign to various factors in choosing a voting record. The primary methodological contribution of this work is that consistent estimates are attainable even though senator ideologies are not observed. State voter preferences are shown to play a relatively minor role. Senators place approximately three times as much weight on the preferences of voters in their own constituency relative to other voters in the state. Discriminating between the weight assigned to a senator's ideology and the party line proves to be somewhat difficult. While the party line appears to exhibit some independent influence on senate voting, ideology generally is more important. In most specifications, ideology is the most important determinant of voting patterns.

Senators systematically change their behavior in election years, weighing the preferences of state voters more heavily prior to elections, while reducing the emphasis placed on the party line. First-term senators also weigh state voter preferences more heavily than do more senior senators. Retiring senators are found to exhibit little systematic change.

The results of this paper provide some support for each of the prominent explanations of voting behavior on the part of elected officials. The small, but positive, weight associated with state voter preferences suggests that the median voter

theorem, while not irrelevant, is only a piece of the puzzle. The dual constituency hypothesis receives fairly strong support, since voters in the senator's constituency receive much more weight than those in the opposing party. The evidence for party-centric theories is mixed. In some specifications, the party line is an important determinant of voting behavior. Overall, however, it is clear that the strong correlation between party affiliation and roll call voting primarily reflects similar ideologies and constituency preferences among party members, rather than an independent influence of party pressure.

The apparent importance of ideology in explaining senator voting has implications for voting study research. On the one hand, these results suggest that past voting records are quite closely correlated with ideology, and may therefore be a reasonable proxy for ideology. On the other hand, ideologies are strongly correlated with party affiliation as well as voter preferences both inside and outside of the constituency. Therefore, any analysis that purports to attribute a causal role to any of those factors without explicitly controlling for ideology is unlikely to obtain reliable results due to omitted variable bias. The estimated ideologies obtained in this analysis could be used by researchers to control for senator ideology.

Table 1: Variation in Senator Voting Records
 Measured by ADA Scores

<u>Senator Classification</u>	<u>Group Mean</u>	<u>Standard Deviation</u>
All Senators	44.7	31.7
Democrats	62.7	24.8
Republicans	23.3	23.3
Within a State:		
Both Democrats	61.1	9.9
Both Republicans	22.0	9.3
One Democrat, One Republican	Dem: 64.8 Rep: 24.5	29.9

Notes: Based on ADA rankings for all senators, 1970-1991. Standard deviations are the weighted (by number of senators) average of yearly standard deviations by category. ADA rankings range from zero to one-hundred, with higher numbers signifying a more liberal voting record.

Table 2: Summary Statistics for Sample Used

<u>Variable</u>	<u>Mean</u>	<u>Standard Error</u>	<u>Minimum</u>	<u>Maximum</u>
Senate ADA Scores	46.8	31.4	0	100
Democrats	60.7	27.6	0	100
Republicans	27.7	25.9	0	95
House Delegation ADA (state means)	42.6	18.7	1.2	90.9
House Democrats	56.4	24.2	2.1	94.5
House Republicans	16.9	14.1	0	73.5
Party Leaders				
Democrat	63.8	13.9	40.0	86.7
Republican	15.4	7.2	7.0	31.2

Notes: For the period 1970-1991. Summary statistics based only on data for senators meeting the following criteria: i) senator served at least six years in sample, ii) at least four members in the state's House delegation. Statistics for the House refer to state delegations rather than individual members of the House.

Table 3: Estimated Weights in Senator Decision Functions

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
<u>Weight on:</u>						
State Voter Preferences	.18 (.05)	.17 (.05)	.16 (.22)	.17 (.05)	.16 (.05)	.26 (.23)
Constituency within State	.13 (.05)	.18 (.06)	.20 (.07)	.21 (.04)	.25 (.05)	.24 (.07)
Party Line	.45 (.08)	.28 (.12)	.24 (.12)	.23 (.04)	.05 (.10)	.02 (.10)
Personal Preferences of Senator	.24 (.08)	.37 (.09)	.40 (.22)	.40 (.06)	.54 (.10)	.48 (.22)
Proxy for Party Line		Members	Members	Leaders	Leaders	Leaders
Instrument for:						
Party	No	Yes	Yes	No	Yes	Yes
State Preferences	No	No	Yes	No	No	Yes
Adjusted R ²	.97			.97		

Notes: Dependent variable is senator ADA score. To be included, senator had to serve at least six years between 1970-1991, and had to represent a state with a House delegation with at least 4 members. Standard errors in parentheses. Year dummies included in all specifications. Number of observations is 1,329 in all specifications. Decision weights constrained to sum to one in each column.

Table 4: Distribution of Estimated Senator Ideologies

	(1)	(2)
<u>All Senators</u>		
Mean	55.1	55.0
< 0	22.7%	11.8%
0-100	45.5%	70.9%
> 100	31.8%	17.3%
<u>Democrats</u>		
Mean	75.9	74.3
< 0	6.5%	0%
0-100	48.4%	75.8%
> 100	45.1%	24.2%
<u>Republicans</u>		
Mean	28.2	30.2
< 0	42.9%	28.6%
0-100	42.8%	63.2%
> 100	14.3%	8.2%
<hr/>		
Party Proxy	Members	Leaders
Instrument for Party	Yes	Yes
State Preferences	No	No
<hr/>		

Notes: Estimates of personal preferences are on the ADA scale. Estimates based on specifications in columns (2) and (5) of Table 3. Each senator's ideology is constrained to be constant over time. Ideologies obtained by dividing the coefficient on each senator's indicator variable by the estimated weight placed on ideology (as explained in Section I of the text). See the notes to Table 3 for further information concerning the underlying regression specification. A full listing of estimated ideologies is provided in the Appendix.

Table 5: Comparison of Decision Weights of First-Termers vs All Others

	(1)	(2)	(3)	(4)
	<u>First-Term</u>	<u>Later Terms</u>	<u>First-Term</u>	<u>Later Terms</u>
<u>Weight on:</u>				
State Voter Preferences	.27 (.08)	.17 (.05)	.28 (.06)	.14 (.05)
Constituency within State	.04 (.08)	.21 (.06)	.11 (.07)	.22 (.05)
Party Line	.51 (.48)	.48 (.36)	.00*	.06 (.04)
Personal Preferences of Senator	.18 (.47)	.14 (.35)	.61 (.06)	.58 (.06)
<hr/>				
Party Proxy	Members	Members	Leaders	Leaders
<hr/>				

Notes: Dependent variable is senator ADA score. To be included, senator had to serve at least six years between 1970-1991, and had to represent a state with a House delegation with at least 4 members. Standard errors in parentheses. Year dummies included in all specifications. Party line is instrumented using the once lagged value. Number of observations is 1,323 in all specifications. Decision weights are constrained to sum to one moving down the columns. Senator ideologies constrained to be constant over time.

*The weight on party for first-term senators was estimated to be negative, but insignificantly different from zero (-.07 with a standard error of .13). The regression was re-estimated constraining the weight to be greater than or equal to 0.

Table 6: Election Proximity, the Decision to Retire, and Estimated Weights in Senator Decision Functions

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Election Year	Election Year, Run	Election Year, Retire	Non-Election Year	Election Year, Run	Election Year, Retire
Weight on:						
State Voter Preferences	.15 (.05)	.26 (.06)	.14 (.12)	.14 (.05)	.24 (.06)	.20 (.11)
Constituency within State	.18 (.06)	.17 (.06)	.17 (.14)	.26 (.05)	.21 (.06)	.15 (.15)
Party Line	.31 (.12)	.22 (.12)	.44 (.13)	.07 (.10)	.01 (.11)	.28 (.12)
Personal Preferences of Senator	.36 (.11)	.35 (.10)	.25 (.09)	.53 (.10)	.54 (.11)	.37 (.10)

Proxy for Party Line Members Members Members Leaders Leaders Leaders

Notes: Dependent variable is senator ADA score. To be included, senator had to serve at least six years between 1970-1991, and had to represent a state with a House delegation with at least 4 members. Standard errors in parentheses. Year dummies included in all specifications. The party proxies are instrumented using once-lagged values in all columns. Number of observations is 1,323. Decision weights constrained to sum to one in each column.

Table 7: Comparison of Decision Weights for Senators in "Safe" vs. "Contested" Seats

	(1)	(2)	(3)	(4)
	<u>Contested</u>	<u>Safe Seat</u>	<u>Contested</u>	<u>Safe Seat</u>
<u>Weight on:</u>				
State Voter Preferences	.17 (.05)	.18 (.05)	.16 (.05)	.18 (.05)
Constituency within State	.16 (.06)	.13 (.06)	.24 (.05)	.14 (.05)
Party Line	.25 (.14)	.35 (.11)	.00*	.19 (.04)
Personal Preferences of Senator	.42 (.12)	.34 (.10)	.60 (.06)	.49 (.05)
<hr/>				
Party Proxy	Members	Members	Leaders	Leaders

Notes: Dependent variable is senator ADA score. A seat is defined as "safe" if the senator received greater than sixty percent of the popular vote in the last election, otherwise it is classified as contested. To be included, senator had to serve at least six years between 1970-1991, and had to represent a state with a House delegation with at least 4 members. Standard errors in parentheses. Year dummies included in all specifications. Party line is instrumented using the once lagged value. Number of observations is 1,329 in all specifications. Decision weights are constrained to sum to one moving down the columns. Senator ideologies constrained to be constant over time.

*The weight on party for first-term senators was estimated to be negative, but insignificantly different from zero (-.07 with a standard error of .13). The regression was re-estimated constraining the weight to be greater than or equal to 0.

Appendix: Estimated Ideologies for Senators in Sample

Estimated Ideology	Senator Name	State	Party	Estimated Ideology	Senator Name	State	Party
-16	East	NC	R	32	Morgan	NC	D
-11	Jepsen	IA	R	32	Heflin	AL	D
-9	Nickles	OK	R	33	Taft	OH	R
-9	Gramm	TX	R	34	Stone	FL	D
-7	Armstrong	CO	R	35	Boren	OK	D
-7	Helms	NC	R	37	Danforth	MO	R
-6	McConnell	KY	R	38	Scott, H.	PA	R
-5	Kasten	WI	R	39	Nunn	GA	D
-3	Denton	AL	R	44	Johnston	LA	D
-2	Mattlingly	GA	R	46	Bentsen	TX	D
-2	Quayle	IN	R	53	Deconcini	AZ	D
-1	Grassley	IA	R	53	Hollings	SC	D
0	Thurmond	SC	R	54	Byrd, R.	WV	D
1	McClellan	AR	D	55	Dixon	IL	D
2	Lugar	IN	R	59	Kassenbaum	KS	R
4	Allen	AL	D	59	Randolph	WV	D
4	Trible	VA	R	59	Chiles	FL	D
6	Griffin	MI	R	60	Jackson	WA	D
6	Tower	TX	R	60	Hartke	IN	D
7	Eastland	MS	D	61	Huddleston	KY	D
8	Dole	KS	R	63	Durenburger	MN	R
9	Hayakawa	CA	R	67	Heinz	PA	R
10	Bartlett	OK	R	68	Pearson	KS	R
10	Cochran	MS	R	69	Ford	KY	D
10	D'Amato	NY	R	72	Packwood	OR	R
13	Warner	VA	R	72	Magnuson	WA	D
13	Byrd, H.	VA	D	73	Proxmire	WI	D
16	Hawkins	FL	R	75	Percy	IL	R
16	Baker	TN	R	79	Schweicker	PA	R
17	Scott, W.	VA	R	80	Glenn	OH	D
17	Gorton	WA	R	81	Gore, Jr.	TN	D
17	Brock	TN	R	81	Pryor	AR	D
18	Stennis	MS	D	81	Weicker	CT	R
21	Boschwitz	MN	R	83	Specker	PA	R
22	Long	LA	D	83	Dodd, C.	CT	D
23	Talmadge	GA	D	86	Symington	MO	D
27	Beall	MD	R	87	Rockefeller	WV	D
29	Sparkman	AL	D	88	Sasser	TN	D
29	Bellman	OK	R	91	Ribicoff	CT	D

<u>Estimated Ideology</u>	<u>Senator Name</u>	<u>State</u>	<u>Party</u>
91	Bayh	IN	D
92	Moynihan	NY	D
93	Humphrey, H.	MN	D
95	Kennedy	MA	D
96	Brooke	MA	R
97	Tsongas	MA	D
97	Hart, G.	CO	D
97	Kerry	MA	D
98	Tunney	CA	D
98	Bradley	NJ	D
99	Simon	IL	D
99	Stevenson	IL	D
100	Haskell	CO	D
102	Williams, H.	NJ	D
102	Bumpers	AR	D
103	Mathias	MD	R
104	Hatfield, M.	OR	R
104	Eagleton	MO	D
104	Levin	MI	D
105	Riegle	MI	D
106	Javits	NY	R
106	Cranston	CA	D
107	Lautenberg	NJ	D
109	Nelson	WI	D
110	Culver	IA	D
112	Hart, P.	MI	D
118	Mondale	MN	D
118	Sarbanes	MD	D
119	Clark	IA	D
123	Metzenbaum	OH	D
124	Case	NJ	R
126	Harkin	IA	D

<u>Estimated Ideology</u>	<u>Senator Name</u>	<u>State</u>	<u>Party</u>
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