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Congress, Representation, and Participation:
The Influence of Voter Turnout on Legislative Behavior in the House of Representatives

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ABSTRACT

Congress, Representation, and Participation:

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In this dissertation I examine the relationship between voter turnout and legislative representation in the United States Congress. My main contention is that political elites react to electoral participation in formulating public policies. I posit that the institutional advantages of incumbency in the House of Representative reduces turnout in congressional elections. This relationship, in turn, affects the ideological composition of the electorate and ultimately the legislative behavior of elected representatives. I begin by measuring the ideological differences between voters and non-voters using the results of a wide range of public opinion surveys taken over the past thirty years. I then analyze House election results from 1972 to 2000 to determine whether certain institutional characteristics, such as the incumbency advantage and longer tenure length in Congress, affect voter turnout and electoral outcomes. Drawing on the same empirical material, I also assess the influence of political participation on legislative behavior by measuring how turnout rates and electoral competition appear to influence the roll-call votes of members of the House of Representatives. I draw from these analyses the conclusion that higher levels of voter participation tend to move both Republican and Democratic lawmakers to the left, in the sense that Republicans become less conservative and more centrist, while Democrats become more liberal and less centrist. These findings lead me to conclude that a surge in turnout will induce lawmakers to be more responsive to the needs of constituents with lower

socioeconomic status. Overall, my dissertation broadens our understanding of the link between electoral participation and democratic governance in the United States.

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À mes parents.

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CHAPTER 1

Introduction

Today more than ever, legislators appear to be shaping public policy so as to favor particular interests, rewarding politically active and organized citizens at the expense of those who do not vote or otherwise participate in politics.[1] The following project addresses this apparent democratic deficit by analyzing the relationship between voter turnout rates and legislative behavior within the United States House of Representatives. Contemporary democratic theory generally assumes that elected officials represent the interests of their constituents (Schumpeter, 1942; Pitkin, 1967; Dahl, 1971). When political participation is low, the monitoring and effective punishment of legislators may become the privilege of a select group: those who turn out to vote. Because congressional behavior is affected by the broader political environment (Ansolabehere, Snyder, & Stewart, 2001; Erikson & Wright, 2000), we should expect declining voting rates to reduce electoral accountability. In fact, the main contention of this dissertation is that the level of voter participation makes a difference in the formulation of public policy by House members, and by democratic political elites more generally.

Congressional scholars have shown that legislators consider constituent preferences when deciding how to cast their roll-call votes (e.g. Mayhew, 1974). The underlying logic is that district-by-district competition exerts some pressure on candidates to align themselves with the ideological disposition of a majority of their constituents. Indeed, standard electoral theory holds

[1] As reported by the Task force on Inequality and American Democracy by the American Political Science Association (2004).

that legislators who vote consistently outside the mainstream of their party (to the extreme right in the case of the Republicans and to the extreme left in the case of Democrats) will experience a reduction in electoral support (Canes-Wrone, Brady, & Cogan, 2002; Brady, Cogan, Gaines & River, 1996). With popular attitudes on political issues more evenly distributed today than a generation ago, we should expect re-election minded lawmakers to have a strong incentive to avoid taking controversial positions (Fiorina, Abrams, & Pope, 2005).

Yet the reality is quite different. Parties in the House and Senate have become more internally cohesive and more ideologically distinct in recent years (McCarthy, Poole, & Rosenthal 2006; Aldrich & Rhode 2000; Jacobson 2000). Since the overall political attitudes of Americans have remained stable since the 1980s (DiMaggio, Evans, & Bryson, 1996; Evans, 2003), the challenge is to explain why the behavior of an important group of lawmakers in Congress deviates from the preferences of the average or median constituent. One of the keys to understanding these ostensibly inconsistent trends lies not in the general public as standard electoral theories would imply, but rather in a subsample of the voting age population: those who are more likely to participate in elections. That is, even if incumbents espouse ideological positions consistent with a majority of their constituents, they may still see their electoral support decrease. This could happen if the most ideologically extreme segments of their district vote in disproportionately high numbers. Consequently, by demonstrating that the ideological character of active voters is significantly different than that of sporadic voters or nonvoters, I help explain why certain members of Congress have chosen to adopt a less moderate legislative record in recent years.

1.1 Overview

Although the idea of linking legislative behavior and voter turnout is not new, there are few systematic analyses that aim to determine the extent of the relationship between legislative representation, electoral accountability, and political participation. One strand of research argues that abstention has no clear legislative consequence since voters and nonvoters only differ at the margins (Wolfinger & Rosenstone, 1980; Shaffer, 1982; Gant & Lyons, 1993; Teixeira 1992; Highton & Wolfinger, 2001). Following these observations, I will begin by measuring the differences in policy preferences between voters and nonvoters at both the national and state levels. Using different measurement methodologies, I review more than 30 years of surveys from the National Election Studies (1972–2002). I also adopt a new approach to measuring state-level opinion by using propensity score weighting estimators combined with exit polls and census data. This method allows me to calculate the difference between voters and nonvoters in their partisanship, ideology, and presidential voting for the elections from 1994 to 2000, while controlling for possible selection-bias effects. In this manner, my analysis helps to determine whether there is really a significant divergence of opinion between these two groups. The answer is that there is. I also show that the divergence varies from state to state and from election to election. By demonstrating that the ideological character of active voters is different than that of sporadic voters and nonvoters, chapter 2 outlines the importance of understanding how political institutions influence the level of electoral participation, and how those institutions are later transformed by the subset of the electorate that does vote.

This also explains why chapter 3 focuses on how longer congressional careers affect electoral behavior. Because the incumbent reelection rate in contemporary congressional elections is so high (99% in the House for the 2002 and 2004 election), open-seat elections are

typically the only remaining competitive contests. If we consider that there are fewer competitive races in the House today, we should find that certain types of voters, like political independents or lower socio-economic status voters, are less likely to turn out on Election Day (DeNardo, 1980, Converse, 1966). Should we also expect higher retention rates in Congress to exert a downward pressure on political participation? In order to answer this question, chapter 3 presents a simple incumbency theory in which legislators protect their seats by deterring quality challengers and by keeping the cost of political participation high. The theory is tested using a dataset of House elections covering over 15 elections (1972–2000). The analysis demonstrates that the presence of incumbents and longer tenure lengths reduces turnout in congressional elections. The results also indicate that challenger quality and campaign spending have a positive influence on turnout. Most important, the chapter demonstrates that an increase in participation brings with it a decrease in electoral support for incumbents and a reduction in their chances of being reelected. This inverse relationship is shown to strengthen with the number of terms served by House representatives.

Chapter 4 expands on the previous analysis by directly measuring how electoral competitiveness and varying turnout rates influence the behavior of members of Congress. If we follow the conventional theory of "electoral connection," we should compare the voting records of members of Congress and electoral turnout in order to determine how much participation levels affect legislative behavior. Greater turnout should theoretically push members toward more mainstream policy positions, and lower turnout should push them toward more extreme positions (left of center for Democrats, right of center for Republicans). But here we see a paradox: more extreme legislative behavior is supposed to decrease the likelihood of retaining

office (e.g., Canes-Wrone, Brady, & Cogan, 2002). Since we know that an extreme legislative record is likely to reduce electoral support, the question remains why would an incumbent choose to adopt this type of behavior in the first place?

The aim of chapter 4, then, is to determine whether political participation can help solve this paradox. The chapter begins with a review of existing theories of electoral accountability and legislative responsiveness in order to improve our understanding of the relationship between fluctuating political participation rates and roll-call behavior. Using the House election data from chapter 3, the study shows that legislative behavior affects turnout and incumbent support. On the other hand, the results also show that voting and participation significantly affect roll-call behavior. In fact, more extreme legislative records increase the level of political participation, and higher levels of turnout induce legislative moderation. However, this result is only found to be true in the case of Republican incumbents. This counterintuitive finding is explained by the fact that people who are less likely to vote usually hold slightly more liberal preferences. Hence, in most conservative districts, higher turnout is associated with an increase in the number of liberal voters. Conversely, in Democratic districts, higher levels of political participation increase the number of liberal voters and encourage more extreme legislative behavior. Both of these seemingly complementary findings explain why a surge in turnout will induce lawmakers to be more responsive to the needs of constituents of a lower socio-economic status.

Since turnout is so strongly related to electoral competition, chapter 4 also evaluates how electoral demands are affected by legislative behavior. The findings demonstrate that previous roll-call extremism reduces the incumbent's electoral security. This relationship is explained by past legislative records: either the incumbent's own record or that of his predecessor. An

important exception to this rule is found when we focus on freshmen representatives who entered the House by defeating an incumbent (or the incumbent party's candidate). In such cases, moderate incumbents are more likely to be replaced by extreme candidates, and extreme incumbents are more likely to be replaced by moderate candidates. The analysis presented in chapter 4 will highlight the role of electoral participation and competitiveness as a moderating influence on the voting record of House members.

Overall, this dissertation aims to broaden our understanding of the link between electoral participation and democratic governance. In the final chapter (5), I evaluate some of the consequences of low turnout on public policy and representative democracy in the United States. This chapter also includes a brief critique of several of the principal arguments presented in the analysis.

1.2 Theoretical Considerations

The remaining portion of the introduction presents a brief overview of the theory of turnout and incumbency which informs and links the different sections and empirical analyses presented in the dissertation. This summary serves as a roadmap for the reader to clarify the logic behind the relationship between incumbency, legislative responsiveness, and turnout. Whenever necessary, I will identify the assumptions of the theory and its testable propositions.

To begin, I assume that incumbent and challenger candidates are rational actors who aim to maximize their expected utility (in this case, winning elections and implementing public policies, see Milyo, 2001). As I will show in chapter 3, the candidate's utility is determined by three factors: the benefit received from being elected (and for holding office), their outside utility or reserve value, and the cost of campaigning. The theory assumes that challengers have different

levels of outside utility. The theory also assumes that the institutional advantages of holding a House seat will raise the cost of entering the electoral contest for challenger candidates (see also Erikson, 1971; Gelman & King, 1990; Cox & Katz, 1996; Jacobson, 2004).

Since incumbents indirectly introduce barriers to entry (by building a brand name, raising a campaign war chest, or by providing constituency services), I show that candidates who challenge a returning House member must overcome a higher cost of entering the electoral market (for complete proofs see Baskan & Godbout, 2006). Challengers of quality usually have a stronger incentive not to run against a strong incumbent because the utility obtained for not running is higher than for challengers of lower quality (e.g., they may already hold an elected office, see Epstein & Zemsky, 1995 and Banks & Kiewiet, 1989 for similar argument). Therefore, I demonstrate in chapter 3 that only challengers with lower levels of outside utility enter races against high quality incumbents (and quality challengers enter races against weaker incumbents).[2]

This theory leads me to formulate the following testable propositions. First, I expect that in campaigns where the incumbent is running for re-election, we should find a reduction in the overall level of campaign activity. In this context, lower quality challengers will have more difficulty raising campaign funds. By extension, I also expect that the level of electoral competition and turnout in this type of election will be reduced since it will be more difficult for low quality challengers to mobilize potential supporters. Thus, in equilibrium, my theory of incumbency shows that competition and turnout is lower in elections where an incumbent is present. All of these propositions are tested empirically in the third chapter of the dissertation.

[2] I assume that in this later case, the quality challenger will win the nomination over the weaker challenger.

The theory has so far been silent about the circumstances under which an incumbent might become vulnerable and affect the entry decision of challenger candidates. My theory assumes that the costs of entering the electoral market for all challengers will be lowered when an incumbent is unresponsive to his or her constituency preferences (Canes-Wrone, Brady, & Cogan, 2002). The theory also assumes that incumbents can become vulnerable at different stages of their career (Diermeier, Keane, & Merlot, 2003). In this context, we should find an increase in the probability of seeing the emergence of a quality challenger in the next election. Hence, I expect that the previous equilibrium of low turnout/low quality challenger will shift under a high intensity election. In this type of campaign, spending, competition and turnout will increase (this is also confirmed empirically in chapter 3).

The problem remains to determine whether lower levels of turnout and competition have an influence on electoral accountability and political representation in the U.S. Congress. As noted, chapter 2 demonstrates that nonvoters are more likely to prefer redistributive policies and to originate from the lower socio-economic segment of the district. This is an assumption of my theory. However, it is supported by empirical evidence (presented in chapter 2, and also by studies done by Griffin & Newman, 2005; Leighley & Nagler, 1992; Wolfinger & Rosenstone, 1980). This finding leads me to expect that greater political competition, all else being equal, will increase the level of turnout and the number of voters who normally abstain from participating in a low intensity election.

This is explained by the fact that higher levels of competition and campaign spending generally favor the mobilization of independents and lower socio-economic status constituents (DeNardo, 1980; Rosenstone & Hansen, 1993). These voters are more likely to have lower levels

of political interest and to have weaker feelings of attachment to either of the political parties (Campbell, 1960). Thus, members of this group are the first to leave the voting electorate in low intensity election campaigns. As DeNardo (1980) explains, "when turnout falls, the loss of the independent vote often deprives the minority party of a much larger proportion of its support than it does the majority, since independents tend to divide their votes equally [between the minority and majority party]" (p.413).

The preceding empirical finding leads to a final implication of the theory relating to electoral accountability and legislative responsiveness. I expect that an increase in turnout will affect the distribution of voter preferences in the district. Higher levels of political participation will displace the median voter and force re-election minded incumbents to pay more attention to a greater pool of electors. Incumbents wishing to maximize their utility (and remain in office) will need to be responsive to the needs of their altered reelection constituency.

One of the main predictions of the spatial theory of voting is that competing candidates will converge on the district median in order to maximize their likelihood of election. This will be true for national and local elections.[3] An extension of the median voter theorem stipulates that convergence should be observed in any type of electoral district. In other words, in conservative constituencies, Democratic candidates will tend to adopt more moderate positions, while in liberal constituencies, Republican candidates will tend to adopt more moderate positions (Erikson & Wright, 1980, 2000).

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[3] Since candidates rarely propose platforms at the district level, proxies of candidate positioning have generally been used, such as surveys (Ansolabehere, Snyder, & Stewart, 2001; Sullivan & Uslaner, 1978), interest groups scores (Canes-Wrone, Brady, & Cogan, 2002), or roll call voting records (Poole & Rosenthal, 1997; Heckman & Snyder, 1997).

The median voter theorem also predicts that legislators will benefit electorally from moderation, especially when representing unsafe districts (where the previous margin of victory was small). To remain in office, successful candidates can broaden their electoral appeal by avoiding controversial legislative positions in constituencies evenly divided between Republicans and Democrats. And this is most likely to be achieved through the adoption of a more moderate legislative record, where the roll-call votes of the incumbent tend to support an equal number of Republicans and Democratic bills.

When an incumbent is unresponsive to the average district preference, I expect to find that turnout and competitiveness will increase in the subsequent election. If the incumbent wins the subsequent re-election, then I expect that this lawmaker will readjust his or her legislative behavior to accommodate this new influx of voters (and to avoid future strong electoral challenges). As I will demonstrate in chapter 4, incumbents who fail to alter their legislative record under the higher turnout scenario will significantly increase their likelihood of being removed from office.

This modification of legislative behavior will take different forms depending on whether the incumbent is a Republican or a Democrat. Because higher levels of turnout imply more voters, we should find that greater participation rates increase the number of constituents who prefer redistributive policies. This assumption follows directly from the empirical results presented in chapter 2 showing that nonvoters are more likely to be liberal and to identify with the Democratic Party. In districts where a Republican incumbent is reelected, this will mean that there was an increase in the number of voters who prefer redistributive policies, and in Democratic districts this will mean an even higher number of voters who favor redistributive

policies. If we assume that Republican candidates are more conservative than their liberal counterparts (Ansolabehere, Snyder, & Stewart, 2001) we should find that the best response, for a Democratic incumbent in the case of an increase in the level of turnout will be to favor more liberal policies in the next legislative session; and for Republican incumbents, it will be to moderate their legislative behavior (by also favoring more redistributive policies).

Empirically, this implies that the average legislative behavior of Democratic incumbents who are representing high turnout/high competitiveness districts will tend to be more extreme than Democratic incumbents who represent low turnout/low competitiveness districts. On the other hand, I also expect to find that Republican incumbents who represent high turnout/high competitiveness districts will tend to be more moderate than Republican incumbents who represent low turnout/low competitiveness districts.[4] This is explained by the fact that in competitive elections, we will see a greater number of lower socio-economic status voters. Hence, successful incumbents in high turnout high competitiveness election will need to adjust their legislative behavior accordingly, or risk being removed from office.

To summarize, the theory of incumbency and turnout presented in this dissertation demonstrates that the institutional advantages of incumbency in the House of Representatives reduces district competitiveness and turnout among lower socio-economic status voters. This in

[4] Because I am using cross-district analyses of congressional elections, the following empirical study can only conclude that more extreme legislators tend to represent districts where turnout is high. Since my analysis does not include a measure of average district party positions for individual congressional districts (like Schmidt, Kenny, & Morton, 1996), I cannot conclude with certainty that extremism vis-à-vis the average incumbent party or the challenger candidate is causing sitting members of Congress to lose elections or to alter their legislative behavior (see also Ansolabehere, Snyder, & Stewart, 2001 for example of this type of analysis, but also see Appendix 4.B for analyses with challenger candidate positions). The main empirical findings in this dissertation simply entail that extremism vis-à-vis the average House legislative record (as measured by an interest group rating of roll call voting) reduces the incumbent's candidates vote share, and increases the overall level of turnout.

turn affects the distribution of voters' preferences in the constituency. The main consequence of this contraction will be a reduction in the legislative representation of electors who have a lower probability of voting.

CHAPTER 2

Measuring Voter and Nonvoter Policy Preferences

Many scholars have set out to determine whether lower participation rates affect election results and, more broadly, public policy. The underlying logic has generally been that because voters and nonvoters differ in many socio-economic characteristics (income, age, ethnicity, education), the gap in political representation would favor the more privileged segment of the American population. And since it was assumed that nonvoters were more likely to identify with the Democratic Party, conventional wisdom held that a surge in participation would help the Democrats. Nevertheless, more than thirty years of empirical study of political preferences has consistently indicated that there is almost no difference of attitude and opinion between those who participate in elections and those who abstain.

The fact that voters and nonvoters differ significantly in their socio-economic characteristics (Wolfinger & Rosenstone, 1980, Rosenstone & Hansen, 1993), but not in their expressed policy or partisan preferences, constitutes a paradox. As Downs (1957) explains, "citizens who abstain exercise less influence than those who vote, [hence] low-income groups in society are likely to have less political power than their numbers warrant, and high income group more" (p.273). If Congress is a creature of its broader political environment (Jacobson, 2004; Aldrich & Rhode, 2000; Cox & McCubbins, 1993), then one would expect declining turnout rates to have an important limitation on legislative responsiveness. And since wealthier, higher status, older, and more educated citizens are over-represented in the electorate, one would also

expect the distribution of power to be skewed against the interest of citizens of lower socio-economic status.

But if voters and nonvoters differ only at the margins (Highton & Wolfinger, 2001; Rosenstone & Hansen, 1993), should we conclude that there is no partisan and ideological participation bias in the United States? Put differently, should we assume that the median location of voters and nonvoters is similar on a left–right continuum and that measuring the preferences of a random sample of citizens is enough to provide us with an accurate estimate of the "voting" population preference? In response to these questions, I argue that most existing studies have been unable to detect significant differences between voters and nonvoters because of conceptual and measurement errors, not because the differences are not there.

This chapter provides an attempt to correct some of those shortcomings. This dissertation employs several unique empirical strategies to estimate the differences between voters and nonvoters in a series of public opinion studies, spanning more than 30 years. The results demonstrate a stable and significant difference between voter and nonvoter attitudes. One will also find that partisan and ideological preferences vary across states in a statewide public-opinion study combining census and exit poll data for the 1994, 1996, 1998 and 2000 elections. The identified gap is generally detrimental to Democrats, but in some states this trend is reversed and seems to hurt Republicans. The chapter is organized as follows. The first part is an assessment of the ongoing debate surrounding the significant disparity between voters' and nonvoters' partisan preferences and attitudes on public policies. The second part presents empirical data to assess some of the conflicting claims associated with voters' and nonvoters' political preferences. I begin by conducting an analysis of individual surveys collected during

more than 30 years using American National Election Studies.[1] I then proceed to include a more detailed analysis of census and exit-poll data in order to estimate the ideological and partisan preferences of voters and nonvoters in elections from 1994 to 2000 in a variety of states. The final part draws conclusions from the findings.

2.1. Preferences of Voters and Nonvoters: Review of Literature

Scholars have been quick to point out that there is not much difference between the policy preferences of voters and nonvoters, as expressed in response to survey questions about their attitudes about welfare and other public policies (Ellis, Ura, & Ashley-Robinson, 2006; Teixeira 1992; Highton & Wolfinger, 2001; Wolfinger & Rosenstone, 1980).[2] All previous analyses indicate that nonvoters are slightly but insignificantly more liberal than voters with respect to the role government ought to play in the economy. Other individual-level data research found similar patterns: more nonvoters would support the Democratic Party, but not in great enough numbers to make much of a difference (Petrocik, 1987). Recent analyses by Citrin, Schickler, and Sides (2003) and by Brunell and DiNardo (2004) have also shown that an increase in participation, or even full turnout, would only marginally help the Democrats. As the authors explain, the consensus seems to be that "the differences [among voters and nonvoters] that do emerge are too small and inconsistent for compulsory voting to transform the political agenda or have drastic electoral consequences" (Citrin, Schickler, & Sides, 2003, p. 76).

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[1] From 1972 to 2002, Cumulative data file.

[2] The literature on turnout is voluminous. No attempt to summarize its content will be made here. For a review see Blais (2000) and Schlozman (2002). There exists additional studies that demonstrate that unequal voting participation is associated with policies that favor privilege voters—e.g., Hill and Leighley (1992), Leighley (1995), Mebane (1994). However, Bennett and Resnick (1990) and Calvert and Gilchrist (1991) conclude that non-voting does not have a large impact on domestic policy orientation.

Aggregated studies of voting patterns at the Congressional level tell a similar story (DeNardo, 1980, 1987; Nagel & McNulty, 1996), and studies examining specific electoral reforms such as the "motor voter" legislation of 1993 also have concluded that the impact of rising electoral participation does not favor Democrats more than Republicans (Nagel & McNulty, 2000; Highton & Wolfinger, 1998; Knack & White, 2000). Consequently, there does not appear to be a constant linear association between higher turnout rates and increase in the Democratic Party vote. Most of the existing empirical evidence complicates the assumption, defended by Lijphart (1997), that higher turnout would favor the Democratic Party and more liberal policies.

Since the opinions, preferences, and vote choices expressed by voters do not differ substantially from the ones expressed by nonvoters, many scholars have concluded that declining turnout rates should not be perceived as a threat to American democracy (e.g., Teixeira, 1992). Just as in the case of the apparent ignorance of the American public and its lack of political knowledge, the aggregation of all voters appears to be the safeguard against biased participation and representation (Page & Shapiro, 1991). Is it reasonable to conclude, like Highton & Wolfinger, (2001) that nonvoters appear well represented by those who vote? Based on the evidence provided here, the answer is no.

But how can we reconcile the fact that voters and nonvoters are so distinctive when we compare their socio-economic status and more or less similar when we consider their policy preferences, or their partisanship? And what ought we to assume when considering that there is a "modest difference" in opinion between voters and nonvoters, or that universal turnout would bring only "modest change" to electoral outcome? Several explanations have been put forward to

account for these discrepancies and the remainder of this section will provide a brief summary of the most commonly cited.

Lijphart (1997) provides one of the most theoretically appealing explanations. The author accounts for the lack of difference between the opinions of voters and nonvoters by stipulating that those who abstain are typically uninformed, un-mobilized, and lack "class consciousness," which can explain why they are not in effect "voting correctly," or favoring more progressive or redistributive policies.. Lijphart did not empirically test any of his hypotheses, but recent studies by Lassen (2005), Dee (2004) and Milligan, Morretti, and Oreopoulos (2004) have confirmed that higher levels of information or education increase the likelihood of voting. This strand of research does not, however, directly look at the impact of political knowledge on policy preferences or partisan support.

Studies by Bartels (2005, 1996), Lau and Redlawsk (1997), Delli Carpini and Keeter (1996) and Gilens (2001) examine the impact of political information on policy preferences and voting, without taking into account the potential interaction effect of political participation. These analyses conclude that there is a systematic difference between the policy preferences of highly and poorly informed citizens. But, as Bartels (2005) explains, there are profound limitations to the transformative power of political information when it comes to public opinion about complex policy issues. It is, thus, not surprising that the only study that directly tests Lijphart's class-based explanation of participation concludes that nonvoters display few signs of undeveloped class consciousness (Highton & Wolfinger, 2001).

So far, information does appear to have a positive impact on turnout, but none of the previous studies provide any convincing evidence to demonstrate that political knowledge leads

individuals to vote according to their "underlying class interest." This type of explanation for the gap in political participation does not withstand further scrutiny when considering a more rational approach to voting. Rational choice models of turnout predict that voters will abstain when the expected utilities derived from participating is outweighed by the costs of voting. Similarly, abstention may be caused by alienation if the distance between a voter's policy preferences and the candidate's proposed platforms is too great. In this context, certain individuals can choose to abstain from voting even though it may prove not to be in their best economic interest (Downs, 1957; Davis, Hinich, & Ordeshook, 1970; Hinich & Ordeshook, 1969).

Assuming full turnout, the rational model of voting predicts that more extreme and alienated voters will participate in greater numbers, and that the number of indifferent voters will also increase. In this context, it is not evident that the electorate would favor more liberal or redistributive policies. An increase in political participation would not necessarily be concentrated in a specific segment of the American electorate. As Highton and Wolfinger (2001) explain, the party of nonvoters is heterogeneous. The poor, the minorities, and the less educated are overrepresented. "But the young and the transient are even more numerous (p.192)." It is not evident that all of these citizens, if they were forced to vote, would support liberal policies and the Democratic Party. In addition, Plane and Gershtenson (2004) have further demonstrated that turnout is lower in midterm senatorial elections when candidates are not close to the median voter. Their analysis suggests that extreme senatorial contestants will increase alienation and reduce participation in the middle. So in considering the rational model of voting, an increase in turnout would not automatically transform into a more liberal electorate.

Other idiosyncratic factors such as regional disparities provide some additional explanations as to why some privileged citizens support the Democratic Party while some less advantaged citizens decide to cast their vote in favor of the Republican Party. For example, in the Jim Crow South, lower status white voters have tended to support the Republican Party and upper-middle class and economically conservative southerners almost unanimously have supported the Democratic Party (Nadeau & Stanley, 1993). In this context, one would not necessarily assume that full turnout will favor the Democratic Party. Several studies on voting patterns arrive to this conclusion by demonstrating that higher turnout in the former Confederate states favored Republicans in the first part of the twentieth century, while this effect shifted later in favor of the Democratic party in the last few decades (Nagel & McNulty, 2000; Erikson, 1995).

The theory of surge and decline proposed by Campbell (1985) may also shed some additional light on the complex relationship between participation and partisanship. It asserts that House candidates sharing the same political party as the president are advantaged in presidential election years, and share a greater risk in the subsequent midterm election. The logic is that the lower turnout in the midterm election, combined with the increase in attention given to presidential elections, are detrimental to congressmen who benefit from the presidential coattail effect. As Carson, Finocchiaro, Leoni, and Rhode (2001) explain, the Republicans were advantaged in the 1994 election by a decline in turnout among Democratic loyalists, while the Democrats were helped in the 1990 midterm election. Here again, some additional forces, beyond simple socio-economic characteristics, play a role in determining the ideological and partisan support of nonvoters.

Taken as a whole, the previous evidence suggests that Lijphart's class consciousness approach is inadequate for explaining why voters and nonvoters do not significantly differ in their policy preferences. The bulk of the existing empirical reality demonstrates that voters and nonvoters diverge slightly in their policy preferences when their responses to a series of survey questions about attitudes on government welfare and other policy are measured. However, recent analyses comparing voters and nonvoters preferences disaggregated at the state level reveal that the expressed preferences of voters and nonvoters might well be less similar than originally assumed (Griffin & Newman, 2005; Citrin, Schickler, & Sides, 2003). Using a novel methodology which simulated the outcomes of Senate elections under alternative turnout scenarios, Citrin, Schickler, and Sides (2003) demonstrate that the partisan effects of higher turnout varied across states, time, and electoral context in the 1994, 1996, and 1998 elections. Similarly, Griffin and Newman (2005) show that different turnout rates in senatorial elections had a significant impact on public policy orientation. And finally, Brunell and DiNardo (2004) demonstrate, with an extension of the Citrin, Schickler, and Sides methodology, that full turnout would slightly benefit the Democratic Party. If, indeed, the balance of partisan preferences among nonvoters fluctuates significantly across certain states, then the link between social class, policy preference, and voting fails to provide a convincing explanation as to why only marginal differences are observed between expressed policy preferences of voters and nonvoters in public opinion surveys across the United States. It is possible that some other factors might be at play.

These recent findings suggest, contrary to much conventional wisdom, that nonvoters are not necessarily adequately represented by the interests of voters. Perhaps Lijphart is correct in asserting that who does and does not vote has important consequences on the content of public

policy. But unlike Lijphart, I argue that the reason why the majority of individual survey level analyses have systematically been unable to detect any significant differences between voters and nonvoters is not a consequence of class consciousness. It is rather attributable to some of the empirical, methodological, and theoretical flaws found in the existing literature.

Notwithstanding Citrin, Schickler, and Sides (2003) and Griffin and Newman (2005), all of the previous individual level studies of voting and turnout focus on national samples. These analyses are incapable of accounting for some of the differences in registration laws and in the particular configuration of ideological preferences found across states. Furthermore, the existing literature generally relies on a very small number of surveys to measure the opinion of voters and nonvoters. Teixeira (1992), the most cited study on voter and nonvoter attitudes, bases his conclusions only on two public opinion analyses (1984 and 1988 NES). Similarly, Highton and Wolfinger limit their work to the 1992 and 1996 NES studies, while Verba, Schlozman, and Brady (1995) focus on a two-stage survey conducted in 1989 and 1990. Since Citrin, Schickler, and Sides (2003) demonstrated that partisan preferences among voters and nonvoters varies considerably across years, and because NES has conducted additional surveys since 1996, it may prove worthwhile to expand the scope of the analysis on policy preference and participation. The NES has data available on many policy issues which were recorded as far back as 1972. In reality, more than 30 years of public opinion research are available, providing a unique opportunity to study a valuable time series dataset.

The fact that Citrin, Schickler, and Sides (2003), demonstrate that there are significant differences between voter and nonvoter partisan support—when we consider disaggregated opinion at the state level, combined with the limited scope of some of the national individual

studies on participation—raises several important questions. First, do voters differ in their policy preferences when compared to nonvoters? Though it would appear that this is not the case, in order to establish if this holds true over time, a broader analysis is needed that incorporates more than one or two elections. Second, can we identify statewide differences in voters' and nonvoters' policy preferences as well? It is useful to recall that Citrin, Schickler, and Sides (2003) build a study on simulated voter choice. Would the same patterns hold when looking at statewide differences in partisanship or ideology? And finally, can some of the causes of this participation gap be identified? If there is in fact a significant difference in attitudes between voters and nonvoters, then it becomes important to find some of the origins and implications of the participation gap.

The remainder of this chapter attempts to shed some light on the aforementioned questions. Through a comprehensive review of the existing data supplied by the National Election Studies for the 1972 and 2002 period, my first task is to explore the extent to which there is a difference of opinion between voters and nonvoters. The chapter will also use a dataset constructed with census and exit poll data in order to examine the idiosyncratic statewide differences in ideology and partisan preferences in the elections between 1994 and 2000.

2.2. Descriptive Analyses of Voters' and Nonvoters' Preferences

My first task is to review the oft used American National Election Studies (NES) dataset. NES provides more than thirty years of public opinion data and will allow an estimation of the difference between the expressed partisanship, ideology, and policy preferences of voters and

nonvoters. I use the data from the cumulative file, ranging from 1972 to 2002, since the ideology scale question was not asked in earlier surveys.

As mentioned, NES is the data source used by almost all of the individual level analyses that record very little difference between voters and nonvoters. Consequently, the following empirical analysis provides, to the best of my knowledge, the first attempt to review the policy preferences of voters and nonvoters in over more than fifteen elections.

The first Appendix (2.A) to this chapter contains six very basic histograms showing the distribution of voters' and nonvoters' ideological preferences and partisan identification (on a seven point scale, 1 being extreme liberal/strong Democrat and 7 being extreme conservative/strong Republican). In terms of ideology, the mean of the whole population and that of the voters is almost equal (4.3); in the case of nonvoters, it is slightly on the left (4.2). Looking now at partisan identification, there is little difference between voters and nonvoters. In general, the population as a whole leans toward the Democratic Party; voters are a slightly closer to the center (3.7), and nonvoters are more likely to be Democratic identifiers (3.5).

At first glance, it would appear that Teixeira (1992) and Highton and Wolfinger (2001) are right to assert that there is almost no difference between citizens who participate in elections and the rest of the population, at least in terms of ideology and partisanship. However, these results are limited since they do not consider public policy preferences, and do not account for any yearly fluctuations in turnout between 1972 and 2002. The rest of my analyses therefore present the results of each NES election sample individually.

Below, I summarize the data in line charts, where the *y* axis represents the year of the NES survey, and the *x* axis measures the average ideological or partisan scores of voters and nonvoters. These figures also contain a series of public policy questions.

American National Election Studies Average Seven Points Ideology Scale

Figure 2.1. Voters and nonvoters, 1972–2002.

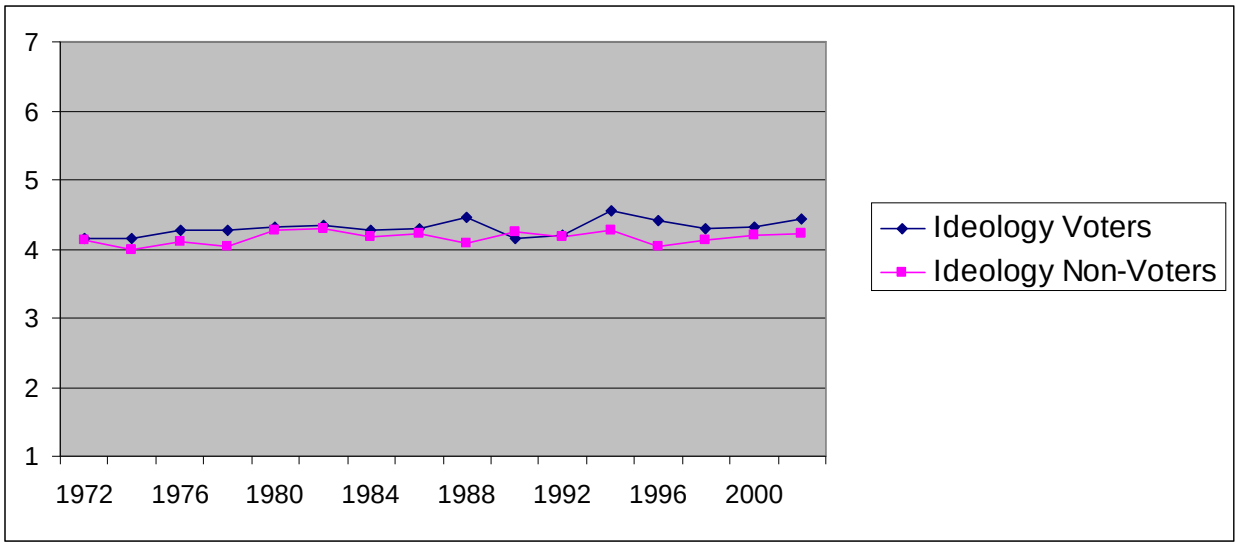


Figure 2.2. Voters and nonvoters, verified, 1976–1990.

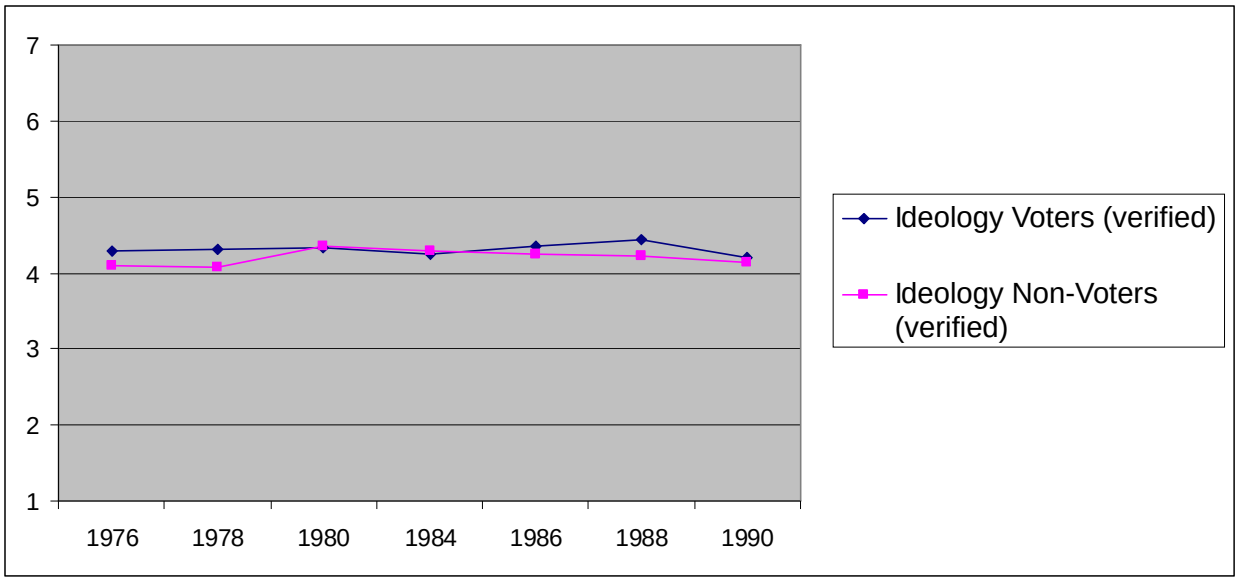


Figure 2.3. Probability of voting 3 categories, 1972–2002.

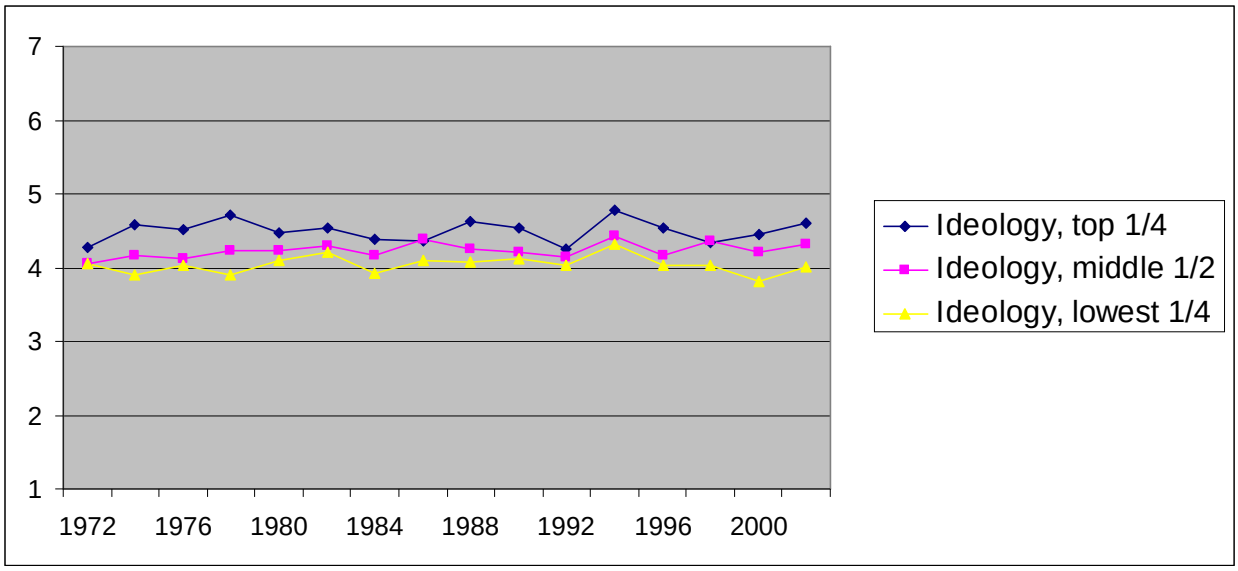
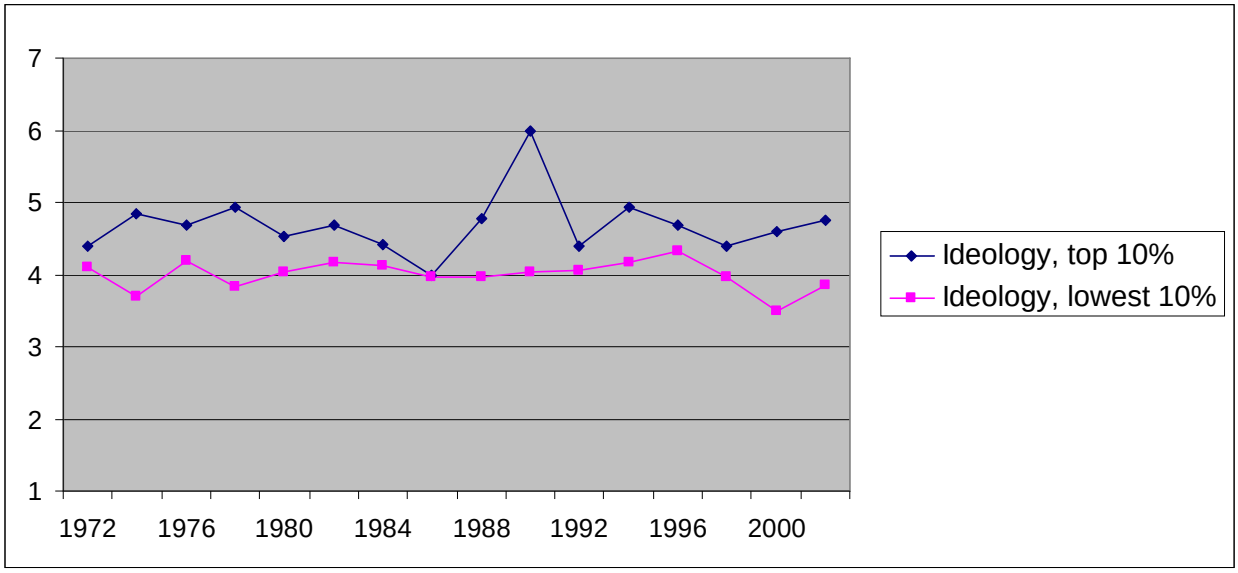


Figure 2.4. Top 10% and lowest 10% probability of voting.



American National Election Studies Average Seven Points Party Identification Scale

Figure 2.5. Voters and nonvoters, 1972–2002.

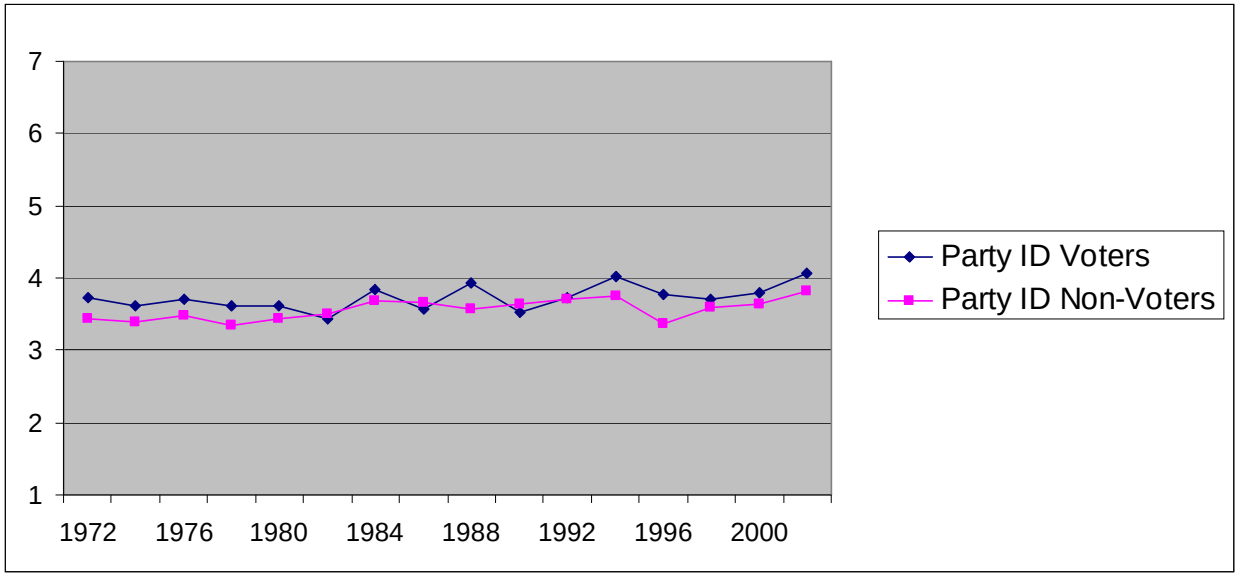


Figure 2.6. Voters and nonvoters, verified, 1976–1990.

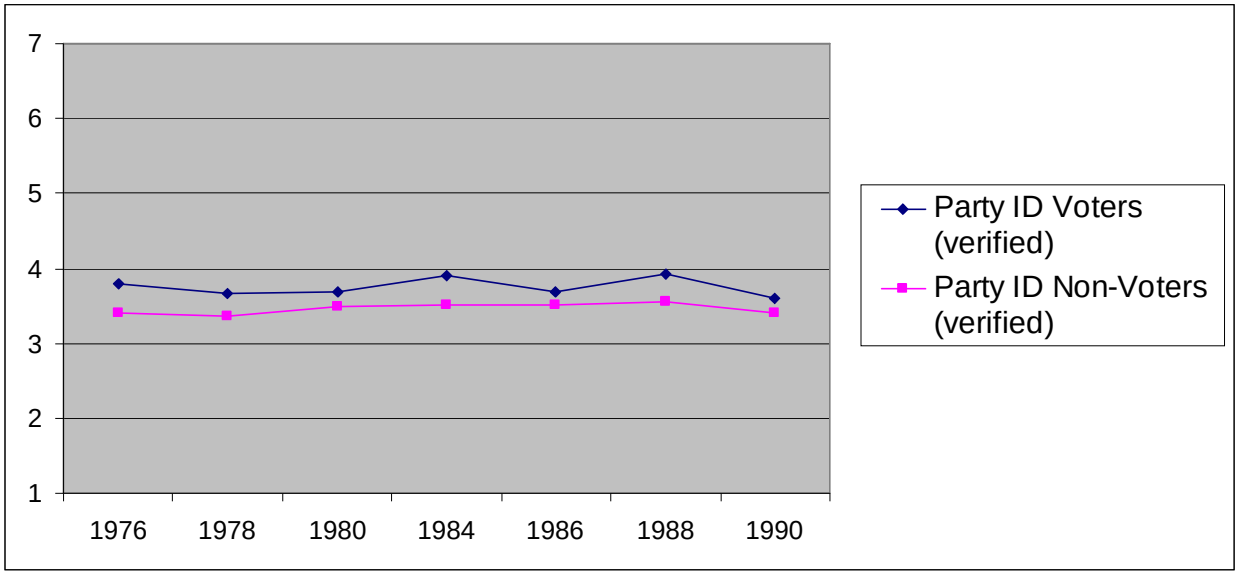


Figure 2.7. Probability of voting 3 categories, 1972–2002.

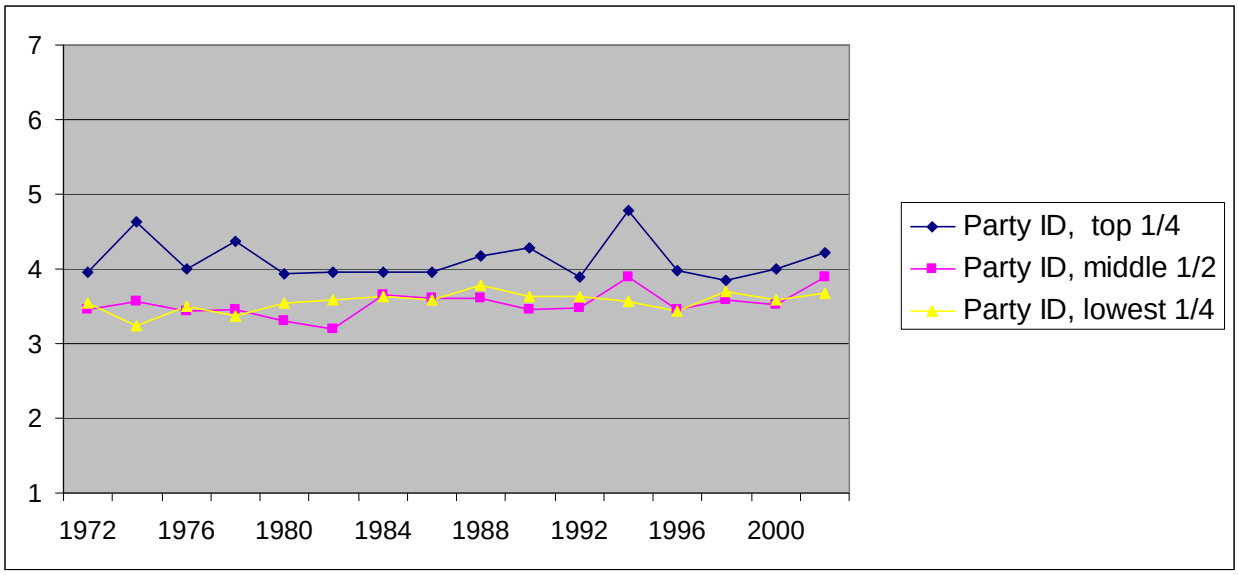
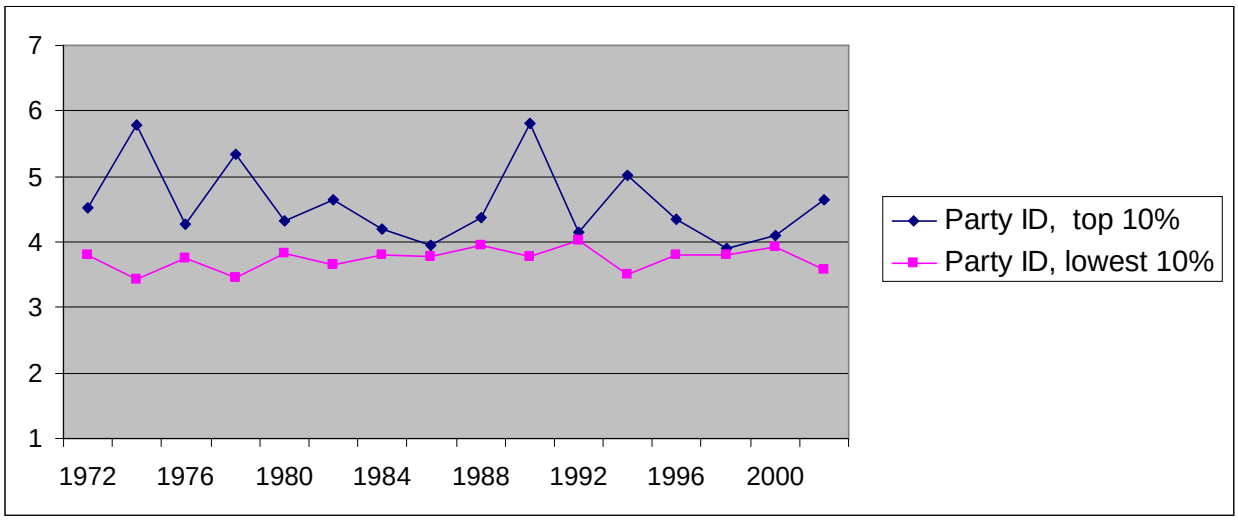


Figure 2.8. Top 10% and lowest 10% probability of voting, 1972–2002.



The data is organized in the following fashion: I first review the difference between voters and nonvoters in their ideological self-placement on a seven-point scale, followed by their partisan identification. I also review the graphs of figures 2.A7 to 2.A18 (see Appendix 2.A) that look at different public policy questions, mainly related to individual preferences over specific

government programs. Each variable is coded according to a seven-point scale, where a low score corresponds to an increase in preferences over stronger government spending/intervention; higher scores increasingly favor limited government. The policy issues included in the analysis are, in order: *government help,* which measures if respondents agree with the statement that government should provide fewer or more services; *health insurance,* which measures if respondents are in favor of universal health care; and finally *jobs,* which measures if respondents agree with the statement that the government should see to it that everyone has a job.

The final series of graphs (figures 2.A19-2.A27 of Appendix 2.A) examine spending preferences over specific government programs (does the respondent favor an increase, a reduction, or the status quo?). The reviewed programs are social security, child spending, food stamp, welfare, crime spending, programs to help the poor, schools, African Americans, and the environment. In these last graphs, the *y* axis represents the percentage of respondents (voters or nonvoters) in favor of cutting, increasing, or maintaining the funding of these specific programs.

All of the preceding ideology, partisanship, and policy scale graphs report three different measures of electoral participation. The first measure is the classical NES question, which asks in the post-election survey if respondents voted in the last elections. As Burden (2000), Abramson and Aldrich (1982), Teixeira (1987) Campbell et al. (1960) demonstrate, the NES has a tendency to overestimate the rate of electoral participation in its surveys, which is why, for reference, a verified voting variable is also reported, which is only available between 1976 and 1990 (1982 is missing).[3] Each respondent's individual probability of voting is also reported, as

[3] In these reported years, NES has validated the reported voting by cross-referencing the individual level data with state electoral registries.

calculated by a logistic regression model containing a series of socio-demographic variables.[4] Two types of classification are used in the regression analyses. The first combines quartiles (1/4, 2/4 and 3/4, 4/4 of the distribution), the second only contains two categories (top decile and lowest decile of the distribution of probabilities of voting).[5]

It may appear unusual to report the individual probabilities of voting since it is known, at least between 1976 and 1990, the validated turnout rate in each of the sample years. But if one acknowledges with DeNardo (1980) and Fenno (1978) that the electorate is constituted of core and peripheral voters, and that an increase in turnout brings forth a surge in the number of marginal voters with weaker partisan attachment to the polls, then assigning a probability of voting to each respondent could help distinguish between these two types of electors.

Assigning probabilities of voting makes intuitive sense when one considers some of the basic principles found in the political marketing literature—see also Rosenstone and Hansen (1993) for similar arguments on targeting mobilization by elected officials. Focusing on the supply side perspective of political campaigns, knowledge about which voter is more likely to participate on Election Day is crucial. But since it is impossible to know this answer in advance and with certainty, campaign strategists rely on individual probabilities of voting when

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[4] The model follows Deufel and Kedar (2000) analysis on voting probabilities. It includes the following variables: home ownership; southerner; gender; church attendance scale; strong partisan identifiers; age by categories (17–24, 25–34, 35–44, 55–64, 65–00), education scale, and African American. Transformed log-odd ratios are used to obtain the prediction vector. The probability of voting is calculated in a separate model for each election year, in order to control for punctual effects.

[5] I use quartile and decile because I believe that the actual probability number is arbitrary. This also allows me to get enough respondents in each category. By focusing on the distribution, we can construct intervals that include a specific amount of voters. The lowest quartile corresponds to 0 to .5 probability of voting, second and third is .51 to .86, and the highest quartile is .87 to 1. The lowest decile corresponds to respondents who have between 0 and .30 chance of voting, and the top decile of the distribution contains respondent with a probability of voting higher than .93.

approaching the issue of vote targeting. In this way, the probabilities of voting measures the value associated to each voter by specific candidates. Knowing that voter *A* has a .90 probability of voting makes him more "valuable" than voter *B* who has a .51 probability of turning out. Following this example, if campaign strategists mail one hundred .90 probability voters, they should expect to reach 90 voters; which is more cost efficient than mailing one hundred .50 probability voters (Malchow 2003). When considering the voting decision as a yes/no proposition, it is impossible to construct an efficient campaign communication strategy, since the optimal allocation of scarce campaign resources is almost impossible.

It follows that the opinions of individuals who have a higher probability of voting may matter more from the supplier's perspective. Even though no one can actually have a one hundred percent probability of voting, it makes more sense for a politician to tailor his/her campaign message to appeal to voters who have a higher probability of turning up on Election Day, especially when turnout is expected to be low (i.e., in midterm elections, or in primary elections). So by targeting campaign expenditures in order to raise turnout, strategists generally segment the electorate and focus their GOTV effort on groups where the percentage of supporters have a higher propensity to vote. The median preference of registered voters is what matters more to candidates when they are campaigning. Consequently, by stratifying the electorate and looking at individual probability of voting, the following analysis draws from the same kind of information that is available to politicians in the course of an election campaign.

2.3. Findings

Let us now begin the analysis by looking at voter ideology. Figures 2.1 and 2.2 show that there is indeed a very small difference between voters and nonvoters, and that this difference is not enhanced if we observe the distribution of validated voters. In effect, the (rounded) average ideological score for voter across all the years is 4.3 and, and 4.2 for nonvoters. There is a bigger tilt on the left when looking at the average partisanship scores, but here again the average difference is extremely small, at 3.7 for voters versus 3.6 for nonvoters on the seven point scale. By looking at the distribution of voting probabilities and the average ideological score, one notes a small but constant difference between the lowest quartile and the highest quartile, where the remaining half fits squarely in the middle. This distinction is even more substantial when we focus on the top and lowest decile of the distribution of the probabilities of voting. Here the cumulative average score difference is more than .7.

Observing such a small difference on the ideology scale between voters and nonvoters is not that troubling if one considers the fact that the distribution of the respondents' ideological scores is normal.[6] In effect, more than 66% of the sample either gave a three, four, or five on the 7-point ideology scale. So expecting to find differences in the order of two or more points seems highly unreasonable. More than 68% of the observations fall between the scores of 2.93 and 5.67 on the scale. A shift of .5 from the mean (4.3 to 4.8 for example) must involve the displacement of more than 11% of the sample (which corresponds to a .27 standard deviation point from the mean). Small differences are indeed important if one considers the shape of the ideology scale distribution.

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[6] One-Sample Kolmogorov-Smirnov Test significant, $Z = 25$.

In turning attention to government help, health insurance, and the job scale variable, it becomes apparent that the gap between voters and nonvoters is widening (figures 7-18, Appendix 2.A). The difference is even larger when looking at the probabilities of voting. On average, there is a .8 divergence between the lowest and highest probability of voting quartiles when looking at the health insurance scale (Figure 14, Appendix 2.A).

When considering the preferences of voters and nonvoters on specific federal government programs, one can also notice that nonvoters are generally in favor of more spending. For instance, on average 68% of nonvoters professed to be in favor of increasing federal spending on social security, while 52% nonvoters supported an increase (Figure 19, Appendix 2.A). Similarly, on average 37% of the respondents declared to be in favor of cutting the food stamp funding, while 31% of nonvoters favored a similar position (Figure 24, Appendix 2.A).

The key findings of the preceding graphs are generalizable into three distinctive conclusions. First, there exists a small, but constant difference in the expressed opinion of voters and nonvoters. This difference appears rather stable across time and the observed variations seem to be the result of a simple stochastic process. Second, the distinction in opinions between voters and nonvoters is actually more pronounced if attention is focused on specific government spending and programs. The finding is probably explained by the discrepancy found between operational and symbolic ideology. As Ellis and Stimson (2005) explains, most Americans prefer a government that does more rather than less, which make them operationally liberal. But when asked to describe their political ideology, these same individuals will choose the label conservative. In this way, the fact that a stronger difference is found between voters and

nonvoters by looking at their preferences over government programs just confirms the individual cognitive dissonance found within the liberal and conservative ideological labels.

Finally, and most importantly, I find that as the probability of voting increases, the likelihood of having conservative views increases. What is more, a little over half of the respondents (the second and third quartile) sit squarely in the middle between the bottom and top quarter of the distribution of the probabilities of voting on many policy issues. If one considers that politicians partially weight the values of a vote by the individual's probability of voting, it would appear that conservative voters are a better investment. And this fact transcends partisanship, since we are really looking at the electorate at large. Nationally, at least, more conservative policies should appeal to a broader segment of the voting electorate.

This difference holds at the national level. But as mentioned, one of the weaknesses of previous analyses of electoral participation and individual policy preferences stems from an inability to account for idiosyncratic regional differences. DeNardo (1980) and later Citrin, Schickler, and Sides (2003) have found that the partisan effects of higher turnout can vary across states. In some districts dominated by Democrats for example, higher turnout can actually benefit the Republican Party. In this vein, one could also assume the presence of a negative relationship between the likelihood of voting and the ideological/partisan identification with the minority party in a specific jurisdiction. The only way to get at this type of regional fluctuations in the balance of preference of nonvoters is to conduct statewide level analyses of turnout.

2.4. Statewide Analyses of Political Participation 1994–2000: Overview

In the following section, I present and evaluate different methods of calculating the impact of an increase in participation on the ideological, partisanship and voting distributions across states in the 1994, 1996, 1998 and 2000 elections. Because I am interested in analyzing the effects of turnout on public policy preferences at the state level, relying on national surveys like the NES here is problematic. These election year surveys contain at best 2,500 respondents, which is not sufficient to provide large enough statewide samples. In order to assess if there is a difference in voters' and nonvoters' preferences across states, the study uses election year samples that combine data from exit polls and the Bureau of the Census. I improve on the methodology employed by Citrin, Schickler, and Sides (2003) to simulate the effect of an increase in turnout on the statewide distribution of ideological, partisan, and voting.[7] However, the analysis offers more than a simple update to the Citrin, Schickler and Sides paper. Here, not only more preference variables are observed (partisanship, ideology, and presidential vote when relevant) and at least one more electoral contest (2000), but I am also analyzing all the states included in the exit poll analyses with a more precise simulation approach (the previous analysis only focused on states where senate elections were held). And as the results will demonstrate, I employ a more valid method to estimate the distributions of voters and nonvoters policy preferences.

Like Citrin, Schickler, and Sides, I rely on the November Voter Supplement data of the Census Bureau's Current Population Survey (CPS) which include questions measuring political participation in every midterm and presidential election. With a sample of around 90,000

[7] The 2002 exit poll did not contain statewide samples (only a large national sample).

respondents, the CPS gets on average a little over 800 respondents per state (and even more in populous states like New York or California). Using the CPS allows to get a fairly accurate estimate of statewide individual probability of voting. The CPS reports a count of the number of citizens who are qualified to vote. This measure does not overestimate the actual abstention rate as in the case of the Census Bureau Voting Age Population's measure, which includes felons and noncitizens (McDonald & Popkin, 2002).

Since I am using government data, the CPS is of limited use because it does not contain any information on respondents' policy preferences and voting intentions. To get at these statistics, I rely on the Voter News Services (VNS) exit polls data series, obtained from individual states in the four elections covered by this study. The VNS questionnaires include information on reported voting, partisanship, and ideology on a sample of voters in each state. It also incorporates socio-demographic measures that are similar to the ones found in the CPS.

The method is straightforward. With the help of the CPS data, it is possible to compare the behavior of voters and nonvoters by looking at the preferences of certain voters who share with nonvoters the same socio-demographic characteristics. In other words, since the VNS data provides a randomized sample of voters only, a method is needed to adjust for the absence of nonvoters. This is a basic problem of incomplete data in survey sampling, and weighting adjustments techniques are usually applied to tackle this non-response issue. To substitute for the absence control units (nonvoters) in the VNS dataset, I rely on the census data to estimate individual probability of voting. Dehejia and Wahba (1999), also combine the treated units from a randomized evaluation of the National Supported Work with non-experimental comparison units drawn from census data, so our methods are similar in this respect.

The question now is to determine which method is the most efficient and precise in adjusting for the missing data in this sample. There are quite a few statistical strategies available to estimate counterfactuals when presented with incomplete data. Each of these estimation methods represents a tradeoff between simplicity, accuracy, and controlling for potential observable and unobservable selection effects. A review of four different methods is offered before conducting the statewide simulations for the 1994–2000 elections. In order, these methodologies are a basic probability classification weight, two propensity score weighting schemes, and finally the Heckman two-step selection model. I first present a general description of these "different ways" of calculating the counterfactuals. Later each method is tested with altered NES data (where nonvoters are assumed to be missing) in order to determine which technique produces the most precise and consistent estimators. Briefly, I compare the counterfactuals with the actual distribution of preferences through simulating the behavior of nonvoters by alerting the partisanship and ideological preferences of voters, and comparing these counterfactuals with the real preferences of nonvoters in the NES samples. These robustness tests allow one to identify the most precise estimation method to tackle the problem of non-response in the exit poll surveys. I then proceed to present the results of the simulation analyses combining the exit poll data and the Current Population Survey for the four elections under study.

The analyses of simulated statewide full turnout scenario demonstrate that there are fluctuations in partisan support within and across elections. The general impression is that the Democratic Party is the big winner in a majority of the states under full turnout. Nonetheless, I

also find confirmation of the DeNardo (1980) and Citrin, Schickler, and Sides (2003) theses, that the Republican Party could sometime benefit from higher electoral participation.

2.5. Probability Selections

The most straightforward method one can employ to adjust for the absence of nonvoters is the one commonly used by political marketing and campaign experts (Malchow, 2003). It consists of estimating, in the first part, the probability of voting for each respondent of the exit poll, and to compare, in the second part, respondents who have a high probability of voting with respondents who have a low probability of voting (the surrogate nonvoters). This method was employed in the previous section to compare voters with a high and low probability of voting in the NES data. In this context, one only needs to estimate the probability of voting for each respondent, and classify each voter in two groups according to a certain cutoff point (say a .5 probability of voting).

In this study, the CPS data allows one to calculate the statewide voting probability functions with the help of specific socio-demographic indicators (using the $n \times k$ CPS statewide data matrix). The parameter vector obtained from the probability functions can be transposed to the $n \times k$ VNS statewide data matrix. This procedure computes the probabilities of voting for all exit poll respondents in the elections and re-classifies them accordingly (high or low probability of voting). In the end, the $n \times 1$ column vector of voting probability obtained with the VNS data can be used to compare the partisanship and ideology of voters who have a high probability of voting with voters who have a low probability of voting.

It is important to note that there are several methodological problems associated with the use of this particular type of simulations. First, as Citrin, Schickler, and Sides (2003) rightly point out in their own simulation analyses, when calculating the voting probabilities, there are limitations to the demographic variables commonly found in both the CPS and VNS datasets. In the model presented here, age, race, income, gender, as well as education, marital status, and union membership represent all of the available cofactors. So it is clear that these socio-demographic indicators cannot predict turnout perfectly. Citrin, Schickler, and Sides provide a detailed explanation to justify the use of such an underspecified model to predict voting intentions. I will not repeat their arguments here, but note in passing that turnout is highly correlated with socio-economic status, which fits perfectly with the underlying premise of this model (for similar arguments, see Brunell and DiNardo, 2004). Citrin, Schickler, and Sides, estimate voter preference for the voting population with the exit poll data and use the coefficient obtained to predict the behavior of voters and nonvoters in the census.[8] This method presupposes a relationship between a voter's demographic characteristics and his/her preference over candidates. There is no doubt that such a relationship exists, but as Brunell and DiNardo (2004) explain, this well known estimation method developed by Blinder (1973) and Oaxaca (1973) is somewhat less efficient in estimating categorical outcomes (i.e., when there is more than two choices, like party identification, ideology or voting for more than two candidates). For a different methodological approach to this type of estimation problem, one could also use multinomial logit estimations (e.g., Martinez and Gill, 2005).

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[8] That it to say that the parameter vector obtained from the exit poll regressions (where senate voting is the dependent variable) is transposed and matched to the statewide census data. The subsequent *n* x 1 vector of predicted values gives the overall statewide full turnout senatorial vote.

A second problem with the CPS data relates to the fact that it is not, by itself, immune to over-reporting when estimating the rates of electoral participation. Because I am interested in calculating the probability of voting, the possibility that the dependent variable is biased raises an important problem with regards to the validity of the results. The fact that the opinions of validated voters and nonvoters in the NES samples do not differ greatly when compared to unverified voters and nonvoters provides us with some evidence that over-reporting should not skew the results of the simulations. And as Citrin, Schickler, and Sides explain, the overestimation of turnout rates in the CPS is about ten percentage points lower than the one found in NES. So the problem of over-estimation is actually lower in the census studies. At least in theory, the over-reporting of voting should not distort the results of the simulations in favor of the research hypotheses: respondents who falsely declare having voted will actually help make the group of voters similar to the group of nonvoters, not different, which is what I am trying to determine.

A more fundamental problem in this type of probability analysis is related to the sample selection bias. This can render some of the assumptions used to calculate the probabilities of voting invalid. Here, the main concern over non-responses is the risk that nonvoters differ from voters with regards to their voting, partisanship and ideological orientations.

As Dehejia and Wahba (2002) explain, "it is well recognized that the estimate of a causal effect obtained by comparing a treatment group with a non-experimental treatment group could be biased because of problems such as self-selection [. . .] (p.1)." In this analysis, the treatment I am interested in is whether someone voted or not. That is, voters are part of the "treatment group" and nonvoters are part of the "comparison groups." If voting was a randomized

experiment—i.e., if citizens were randomly assigned to participate in an election—one could compare the partisanship of voters and nonvoters, and generalize any of the findings to the population at large (this is what Citrin, Schickler, & Sides, do). But since voters and nonvoters systematically differ in their socio-demographic characteristics, and because individuals who decide to vote do so voluntarily, voting should be considered as a nonrandomized event.

It follows that comparing the opinion of high and low probability voters, or the Citrin et al. approach contain an inherent selection bias. Assigning a voting probability to exit poll voters, based on the records found in the census data violates one of the fundamental assumptions underlying the central limit theorem. In effect, one cannot assume that the data is generated from a random sample of size *n,* and that the observations in *y* (the dependent variable, voting) are statistically independent from each other. The sheer fact that a sample of voters is used to estimate the preferences of nonvoters can potentially create a major estimation bias. Consequently, it is necessary to employ a method to correct for this sample selection problem. I will focus attention on this particular issue below. I begin by presenting two similar methods to adjust for selection biases on observational cofactors present in the analysis and follow by introducing a methodology to control for selection biases arising from unobservable cofactors.

2.6. Propensity Score Approach

There exist numerous statistical approaches that provide a natural weighting scheme to correct for selection problems and biased estimates. The most popular, propensity score matching, developed by Rosenbaum and Rubin (1983), has been widely used by medical and labor economic scholars. In political science, propensity score matching is still relatively new,

although recent work by Lassen (2005) on voting and information, by Herron and Wand (in press) on voting technologies and Brunell and DiNardo (2004) on turnout more specifically, demonstrates that it is gaining in popularity.

"Matching involves pairing treatment and comparison units that are similar in terms of their observable characteristics. When the relevant differences between any two units are captured in the observable (pre-treatment) covariates, which occurs when outcomes are independent of assignment to treatment conditional on pre-treatment covariates, matching methods can yield an unbiased estimate of the treatment impact (Dehejia & Wahba, 2002, p.1)." In other words, propensity score matching is a way to correct for the estimation of treatment effects controlling for the existence of confounding factors. The process controls for the selection bias by making treated and control subjects as similar as possible (Becker and Ichino, 2002). The propensity score approach infers how a nonvoting individual would have voted by studying the behavior of a voter who shares similar socio-demographic characteristics.

Because certain socio-demographic indicators like age, income, education, gender, and race, are all predictors of turnout, it is necessary to make the assignment to treatment (i.e., voting) as random as possible. This process will allow me to ascertain the behavior of nonvoters from a sample of voters while controlling for the fact that both groups differ significantly in their socio-demographics characteristics. By comparing a voter and a nonvoter belonging to the same race, age, gender, or education strata for example, the propensity score approach permits one to obtain the unbiased probability of voting, just as if both individuals had been randomly assigned to participate in the election (see Dehejia & Wahba, 2002 for proofs). Propensity score as

defined by Rosenbaum and Rubin (1983) is, as such, the conditional probability of receiving a treatment given the pre-treatment characteristics.

In my electoral study, propensity scores should measure the conditional probability that someone voted, given their socio-economic characteristics. The propensity score matching (PSM) technique begins by obtaining the probability of selection in the sample (here the probability of voting). The algorithm then organizes blocs to group respondents by their propensity score (or their probability to vote). The respondents are therefore stratified according to the same distribution of observable characteristics. This is done independently of whether they participated in the experiment or not (in this case, if they voted or not). In other words, for a given propensity score, exposure to treatment is random and therefore treated and control units should be on average observationally identical (Becker & Ichino, 2002). PSM re-classify respondents by their probability of selection in the sample. The classification is done by stratum, and it allows me to compare high propensity score respondents with low propensity score respondents (as in the probability selection model presented earlier).[9]

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[9] Formally this methodology would work as follows:

1. The probit (or logit) model is calculated:

   $\Pr\{Di = 1|Xi\} = \Phi(h(Xi))$

   Where Φ denotes the normal (logistic) c.d.f. and h(Xi) is a starting specification which includes all the covariates as linear terms. In our model, Di is equal to voting and h(Xi) is equal to the socio-economic covariates commonly found in the VNS and CPS.

2. The program then splits the sample in k equally spaced intervals of propensity scores.
3. Within each interval, the algorithm tests that the average propensity score of treated and control units do not differ.
4. If the test fails in one interval, the algorithm splits the interval in halves and test again.
5. It continues until, in all intervals, the average propensity score of treated and control units do not differ. In other words, the algorithm classifies the respondents according to their likelihood of voting based on their socio-economic characteristics.

It is important to note that since there are no treatment measures (i.e., reported voting, party identification, and ideology) for the nonvoters in the census dataset, the size of the stratum is limited since only the opinion of voters is measured. Because the VNS data contains only information on individuals who voted, matching these individuals according to their propensity to vote with the CPS respondents creates a censored data problem, which adds yet another lawyer of complexity to the theoretical framework.

This problem can be curbed by using a slightly different method proposed by DiNardo, Fortin, and Lemieux (1996) and Brunell and DiNardo (2004). Brunel and DiNardo have developed a propensity score re-weighing (PSW) approach to estimate the voting behavior of nonvoters in NES surveys. The logic behind this type of adjustment is that the distribution of preferences of the unobservables is equal to the distribution of the appropriately weighted preferences of the observables (see Brunell & DiNardo, 2004 for proofs). In the more classical PSM method, similar treatment and comparison units are paired in terms of their observable characteristics. When the means of one or more of the pairing characteristics differ, the algorithm must re-estimate the propensity score with a less parsimonious specification. This re-estimation process is somewhat arbitrary (the researcher must select which variable to remove), and when there are more than a few variables to be matched, it is difficult to determine along which dimension to match units or which weighting scheme to use (Dehejia & Wahba, 2002).

The advantage of using the re-weighting approach is that the researcher has only to worry about correctly estimating the probability of selection in the sample (in this case, the probability

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In effect, this procedure allows us to assign a probability of voting to all of the respondents of the exit polls, controlling for the fact that they all voted. The algorithm allows us to match voters from the VNS according to their socio-demographic characteristics, with voters and nonvoters from the CPS.

of voting). Weighting adjustment technique compensates for non-response by applying differential weight adjustments across the specific classes of respondents (i.e., the individual weight being applied in proportion to the inverse of the rate of selection). For instance, let us assume a population where a sample is drawn to get an estimate of the population mean ($\overline{Y}$). In this context, $\overline{Y}s$ and $\overline{Y}m$ are the means for the response and non-response stratum. In this case, *p* is the proportion of each stratum in the population. Formally:

$$\overline{Y} = p_s \overline{Y}s + p_m \overline{Y}m$$

(1) *where*:

$$p_s + p_m = 1$$

If we estimate the difference between the sample mean $\overline{Y}s$ and the population mean $\overline{Y}$:

$$\begin{aligned} \overline{Y}s - \overline{Y} &= \overline{Y}s - (p_s \overline{Y}s + p_m \overline{Y}m) \\ &= \overline{Y}s(1 - p_s) - p_m \overline{Y}m \\ &= p_m(\overline{Y}s - \overline{Y}m) \end{aligned} \quad \textbf{(2)}$$

The selection bias will equal $p_m(\overline{Y}s - \overline{Y}m)$. One way to compensate for this potential non-response bias would be to use weights that are inversely proportional to the selection probability. In this case multiplying $\overline{Y}s$ by $\frac{1}{p_s}$ and $\overline{Y}m$ by $\frac{1}{p_m}$ should put the proportion of both stratums equally, and hence take care of the selection bias (see Brunell and DiNardo for complete proofs).

By making each case equal to the reciprocal of the probability of voting, the appropriate counterfactuals can be generated. The question now is how to estimate these weights? By using the same variables available in the PSM approach and in the simple probability selection model (age, education, race, income, gender, working, union, and married). We can then compute the individual probability of voting in the CPS dataset using logistic regression. Like in the previous

methodologies, the coefficients obtained from the logit models can be transposed in the Exit Poll dataset to get at the counterfactuals. So the distribution of preferences for nonvoters will be equal to the distribution of voter preferences appropriately weighted (Brunell and DiNardo p. 32 for proofs). Given the capability of obtaining a fairly accurate estimation of the individual voting probabilities in each state with the census data, computing the counterfactuals becomes a simple question of correctly weighting each case.

2.7. Heckman Selection Model

The principal limitation of calculating counterfactuals with the previous methods relates to the existence of a selection bias arising from unobservable cofactors. In effect, unbiased estimates require that, conditional on the observed covariates, the process by which units are selected into treatment remains unrelated to unmeasured variables that affect the outcome under study. The preceding methodologies assume that the relationship between individual preferences and socio-demographic characteristics is the same for both voters and nonvoters. The PSM and PSW adjust for the bias in the sample by correcting for selection effects related to observable characteristics (e.g., age, income, etc.). However, both methods are incapable of guaranteeing against a "hidden bias" from unobserved variables related to both the assignment and the outcome variables. And because the PSM and PSW are based on the assumption that respondents and non-respondents have identical unobservable characteristics, one cannot ignore the fact that the predictions may produce incorrect estimates of the individual probabilities of voting (Heckman, 1979).

In this study, I predict the likelihood of voting with socio-demographics variables. It is probable that other unobservable variables, such as intensity of partisanship, or closeness of the

election race, also play a major role in the individual's decision to participate in the election. The previous methods are incapable of controlling for such selection effects. In other words, the analysis fails to control for unobservable cofactors simultaneously correlated with participation and voting preferences. So when I base my estimates of the behavior of nonvoters with the behavior of voters, I assume that there are no unobservable differences between these two groups. This potential bias in the post-estimation calculation of counterfactuals actually represents yet another limitation in the methodological framework.

Luckily, the use of the Heckman selection model (Heckman, 1976; Heckman; 1990, Van de Ven & Van Praag, 1981) can provide consistent and asymptotically efficient estimates for the parameters in a model where the dependant variable is not always observed. In this study, I am only observing data on voting, partisan and ideological preferences in the sample of exit poll voters. The dependent variables are present in the dataset if the selection equation (voting probability) is greater than 0. Citizens choose to vote, and thus whether we observe their preferences in the exit poll is dependent on this action. If some respondents decide to vote because of some unobservable cofactor (like intensity of partisanship), one can control for this bias by identifying some variables that strongly affect the probability of selection in the sample, but not the outcome of interest. In the case of election participation, there are limitations because of the variables available in the CPS. Since the CPS is not a political survey per se, I have identified the length of residency and voter registration as potential instruments that may be correlated with voting but not with voting choice, partisanship, or ideology.[10]

[10] We can think that Republicans are more likely to have been living at the same address or are more likely to be registered voters. But we suppose that this effect is insignificant at the margins.

The selection variables (length of residence and registration) predict if the dependent variable is observed or not. The principal equation measures the relationship between the socio-demographic variables (age, income, gender, education, union, married, and working) and the dependent variables. Following a two-step parameter estimation, Heckman derives consistent estimates of predicted values of the dependent variable observed in the absence of a selection effect. So the default predicted statistics produced by Heckman is the expected value of the dependent variables regardless of whether they were observed to participate in the election or not (see Heckman, 1976, 1979 for specific proofs).

Of course the Heckman selection model is highly sensitive to the correct specification of the selection equation. The process depends critically on the correct identification of the exclusion instruments; here that means the variables that enter the choice equation (i.e., the decision to vote), but not the outcome equation (i.e., party identification, ideology, voting). In the case presented here, registration and length of residency provide a very basic model to predict the likelihood of voting. And the absence of any specific measures on the variables of interest in the census dataset requires us to estimate the values of ideology, partisanship and voting preferences with the exit poll data using the Blinder/Oaxaca methodology, an estimation framework employed by Citrin, Schickler, and Sides (2003).[11] The tally from the complete set of predictions subsequently needs to be re-estimated using the Heckman selection model. Only then are unbiased counterfactuals being provided. Needless to say, this two-step approach of selection on observables has never been attempted in the labor economic literature. By using two separate

[11] In other words, we need to calculate with the exit poll data three different models where the dependent variables are ideology, partisanship and voting with matched socio-demographic indicators. The coefficients obtained need to be transposed to the census datasets to obtain the predicted values of the three dependent variables.

levels of estimations, the variance and the likelihood of generating measurement errors in the statistics are being increased. But to my knowledge, this is the only existing methodology that permits the control for biases in the selection equation when not all the data is observed.

2.8. Comparing the Methods

The absence of a post-experimental measurement of treatment in the census dataset complicates the task of estimating statewide turnout rates. The methods I have suggested above are not relevant in all situations. As was indicated, there is a strong possibility that one may find important unobservable and observable confounding effects. However, rather than abandoning the project, or relying on assumptions about the unobserved variables, I believe that there is a considerable reward in exploring the information contained in the variables that were conjointly observed in the CPS and VNP datasets. In the best circumstances, I would have a sufficiently large random sample of voters and nonvoters in each state, with a battery of questions measuring their opinions on a series of public policy issues. This type of data is, unfortunately, not available.

The question now is which method should one employ to estimate the counterfactual and to control for the fact that I only have information on partisanship and ideology from the exit poll dataset. Of course, each of the previous estimation methodology contains numerous flaws. The tradeoff is between simplicity (the probability selection model and the PMW approach) and complexity (the PSM and the Heckman selection model). It is also necessary to account for the selection bias on observable and unobservable cofactors.

Formally, the propensity score re-weighting approach proposed by Brunell and DiNardo and the method employed by Citrin, Schickler, and Sides (the Blinder/Oaxaca framework) should produce the same estimates. However, it is possible that some unobservable variables correlated with participation and related to voter preferences might create a bias in my analyses (hence the Heckman methodology). What is needed here is an empirical framework to test for the robustness of the predicted counterfactuals in each of the four methodologies, as well as the Blinder/Oaxaca framework employed by Citrin, Schickler, and Sides (2004). In this context, I will at least be capable of assessing the internal validity of my estimation methods.

In order to do that, I have used seven samples from the National Election Studies (1976, 1978, 1980, 1984, 1986, 1988, 1990) in which I have altered the meaning of the data. In each sample, the data was censored by removing all nonvoters from the studies (I used the verified voting variable in the Election Studies which is only available in those years). The logic here is to calculate the counterfactuals with all five methodologies and compare the estimates with the "true" sample statistics of the whole uncensored data. I use the data available for voters to estimate the behavior of nonvoters, and compare the censored statistics with the overall sample (combining both voters and nonvoters). In order to make these simulations realistic, the models employ the same variables which are available in the VNS and CPS datasets; that is age, income, gender, education, race, working status, union and married (variables are coded similarly). For the Heckman simulation, the selection equation also included the official registration and the length of residency variables. In all of these cases, I obtain the probability of voting for the whole sample, and then drop all of the nonvoters from the analysis. The counterfactuals are then calculated using one of the five methodologies. The average difference is reported between the

partisanship, ideology, and voting intentions of voters and nonvoters. The results of the analyses are presented in Table 2.1 and Table 2.2.

Table 2.1

Simulation Censored National Election Studies, A

Year	Blinder / Oaxaca Method	Heckman Method	PSW
1976	0.095566	0.169767	0.016215
1978	0.197715	0.327513	0.00882
1980	0.125827	0.201113	0.011215
1984	0.154239	0.230322	0.016185
1986	0.188184	0.154842	0.014456
1988	0.147547	0.089763	0.008521
1990	0.166233	0.230037	0.008521
Average	0.153616	0.20048	0.011991

Note: Cell entries are the combined average of the differences between reported NES estimates and simulated censored estimates for ideology, partisanship, and presidential voting (respondents who voted are compared with overall NES sample).

Table 2.2

Simulation Censored National Election Studies, B

Year	Selection	PSM	Probability of Voting Method
1976	High Voted	0.016492	0.014349
	Low Voted	0.038775	0.071749
1978	High Voted	0.019883	0.017693
	Low Voted	0.028718	0.016275
1980	High Voted	0.072115	0.073168
	Low Voted	0.076828	0.072519
1984	High Voted	0.031694	0.151261
	Low Voted	0.038862	0.040147
1986	High Voted	0.057556	0.057556
	Low Voted	0.017761	0.017761
1988	High Voted	0.177173	0.025665
	Low Voted	0.015513	0.147547
1990	High Voted	0.013638	0.043933
	Low Voted	0.029727	0.010839
Average	-	0.045338	0.054319

Note: Cell entries are the combined averages of the differences between reported NES estimates and simulated censored estimates for ideology, partisanship, and presidential voting (respondents who voted are compared with overall NES sample). The probabilities are obtained with the propensity score matching algorithm where strata are divided at the median. The probability voting method percentages are obtained with a probit function, cutoff point is .5.

The PSM and probability selection methods present the results in two stratums. That is, respondents who have a high probability/propensity of voting are compared with actual voters, and respondents who have a low probability/propensity of voting are compared with actual nonvoters; remember that all the estimates are calculated with the sample of voters only (so nonvoters are actual voters with a low probability of voting). The Blinder/Oaxaca, PSW, and Heckman methods compare the estimated average of the censored sample (with voters only)

adjusting for the probability of voting (obtained with the data of voters and nonvoters) with the overall average of the sample (including voters and nonvoters).

The results demonstrate that the best estimates of the counterfactuals are provided by the PSW approach proposed by Brunell and DiNardo (2004). The worst estimates are generated by the Heckman method, probably because the selection equation in the model is under-specified—that is, there are other variables than length of residency and registration that can predict the likelihood of voting. It also appears that the use of the standard selection mechanism with the probabilities of voting (high and low) is more accurate in predicting ideology, voting, and party identification, than the Blinder/Oaxaca and Heckman models. In the case of ideology especially (not reported in the table), the difference is quite substantive simply because predicting ideology with socio-demographic variables is not ideal. I also note in passing the very poor performance of the Blinder/Oaxaca methodology employed by Citrin, Schickler, and Sides (2003) which raises doubts about the validity of some of the predictions in their full statewide turnout Senate election study for 1994, 1996 and 1998.

To sum up, the PSW method is the most accurate estimation scheme to get at the counterfactuals. With similar data limitation, the PSW was capable of producing on average (for the 7 election under study) an error a little over 1%. That is, by reweighting the responses of voters by the inverse of their probability of voting (using a logit function), it was possible to obtain a fairly accurate estimate of the overall sample statistics (for both voters and nonvoters). It is with confidence then, that I select the PSW technique to calculate the counterfactual in statewide analyses presented in the next section.

2.9. Statewide PSW Estimations of Turnout, 1994–2000

The PSW statewide measures of full turnout are estimated independently in each of the states where exit polls were conducted between 1994 and 2000.[12] I begin by estimating the statewide probability of voting in the CPS dataset with logistic regressions using the same set of independent variables. The variables were matched and coded similarly in the CPS and VNS statewide datasets. I use age (coded in seven categories), race (White, African American, Hispanic, other), education (grade school, high school, some college, college, graduate), and income (four categories). When available the analyses also incorporate variables indicating if the respondent is working, part of a labor union, or married.[13] The parameter vector obtained from the logistic equation in each state is then transposed to the exit poll data where each respondent who voted is "assigned" a probability of voting (even if in actuality they all voted).

Once the predicted voting probability is obtained for each respondent in the exit poll data set, I multiply this value by its inverse ($1/x$) to obtain the appropriate weight. The statewide dataset is then weighted by these values (and by the overall exit poll selection weight).[14] Thus, only the exit poll respondents are used to calculate the counterfactuals, all CPS respondents are dropped from the subsequent analyses. The difference between the observed statewide exit poll dataset and the re-weighted estimates theoretically represent the difference in preferences between the full and the actual level of turnout. In this study, I calculate this difference in all years for partisanship and ideology. I also look at presidential voting in 1996 and 2000. It is important to note that because I calculate the overall statewide participation weight with a

[12] 36 states in 1994; 50 states and DC in 1996; 42 states in 1998; 50 states and DC in 2000.

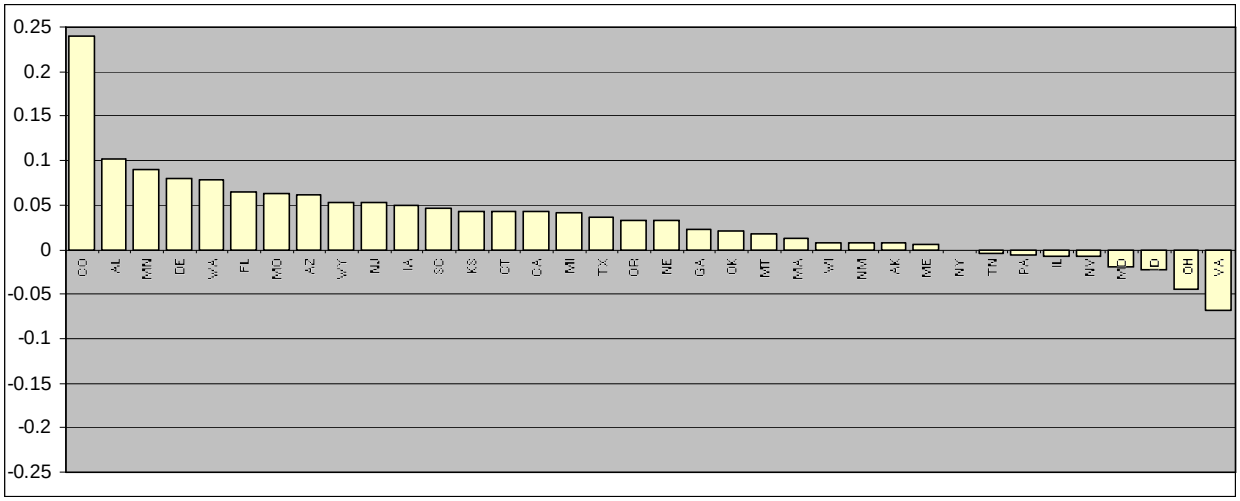
[13] Education is also missing in some state.

[14] We use the analytical weight in STATA which is the default weighting scheme in this case.

random sample of the whole population (CPS dataset), my estimates of the probability of voting give an accurate estimate of the distribution of preferences assuming full turnout. Remember that the statewide probability of voting is obtained from a random sample including both voters and nonvoters. So by weighting each respondent by the likelihood of voting and the likelihood of being included in the exit poll, I am capable of backtracking toward the "true" overall population statistics (see Appendix 2.B for more detailed example of procedure).

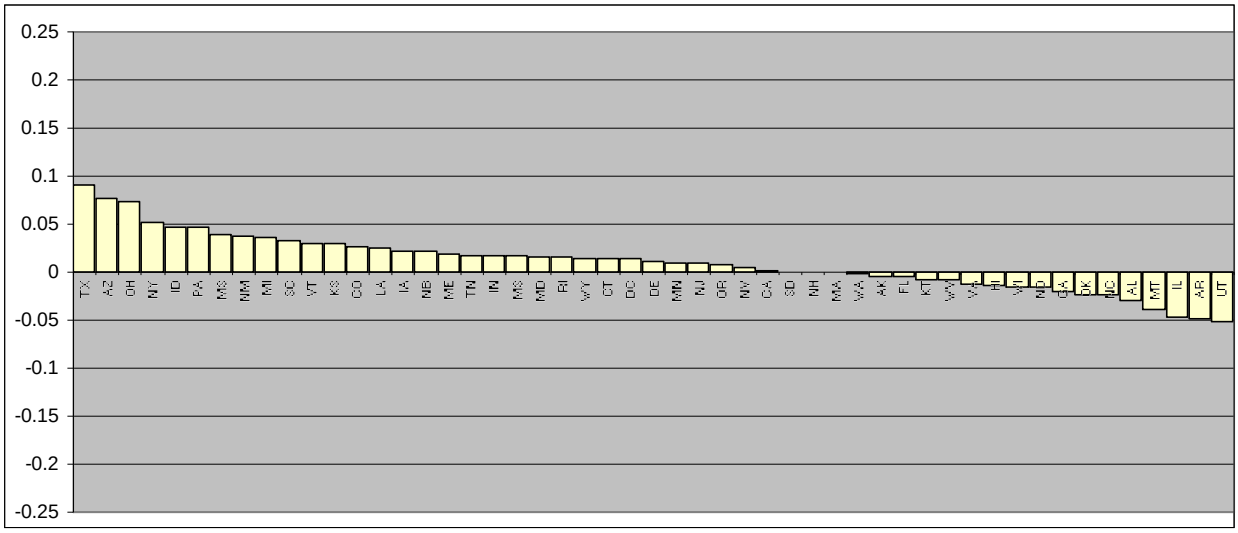
I have regrouped the main findings in 10 graphs (figures 2.9–2.18).

Figure 2.9. 1994 Party identification (Democrats minus Republicans).



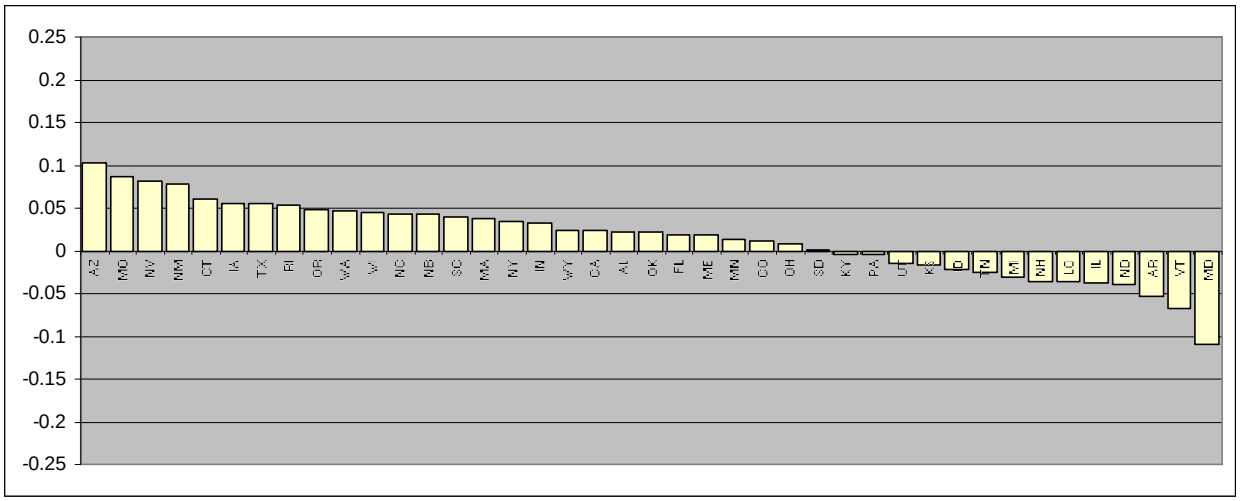
Note: differential = (full turnout dem minus observed dem) - (full turnout rep minus observed rep). Values are percentage of identifiers, third party vote not reported.

Figure 2.10. 1996 Party identification (Democrats minus Republicans).



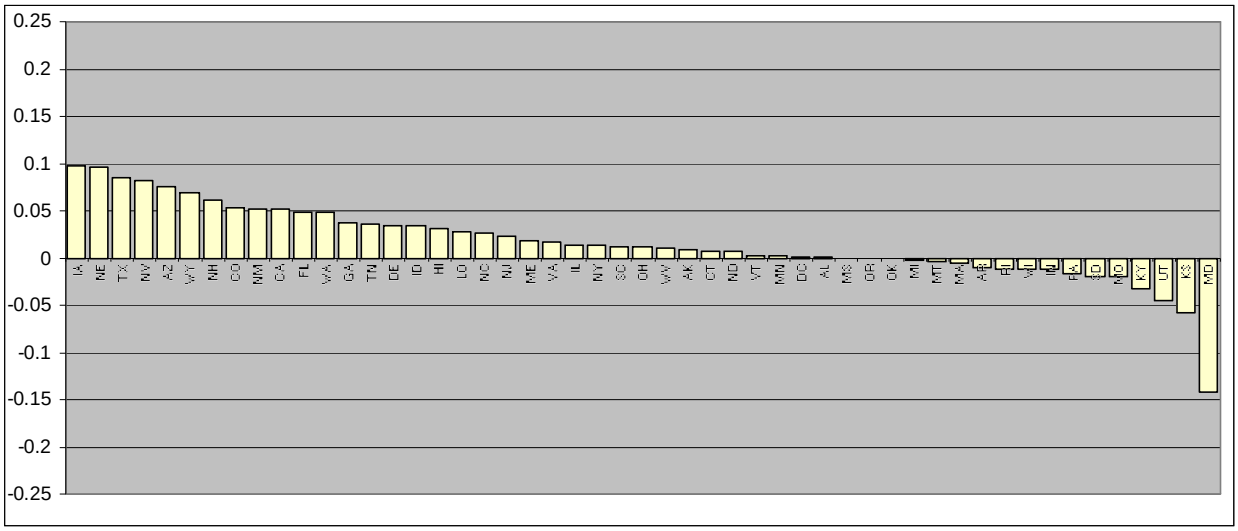
Note: differential = (full turnout dem minus observed dem) - (full turnout rep minus observed rep). Values are percentage of identifiers, third party vote not reported.

Figure 2.11. 1998 Party identification (Democrats minus Republicans).



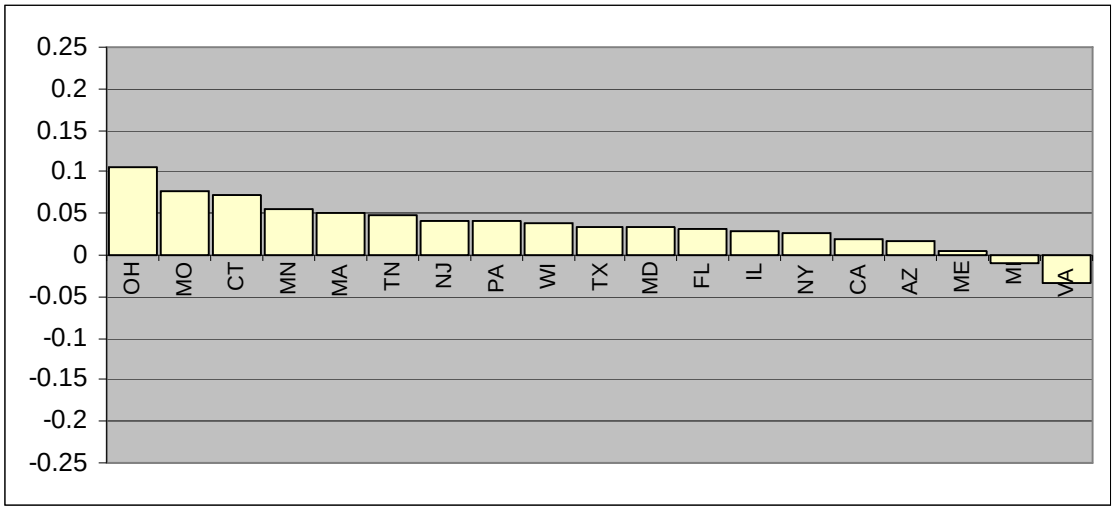
Note: differential = (full turnout dem minus observed dem) -(full turnout rep mimus observed rep). Values are percentage of identifiers, third party vote not reported.

Table 2.12. 2000 Party identification (Democrats minus Republicans).



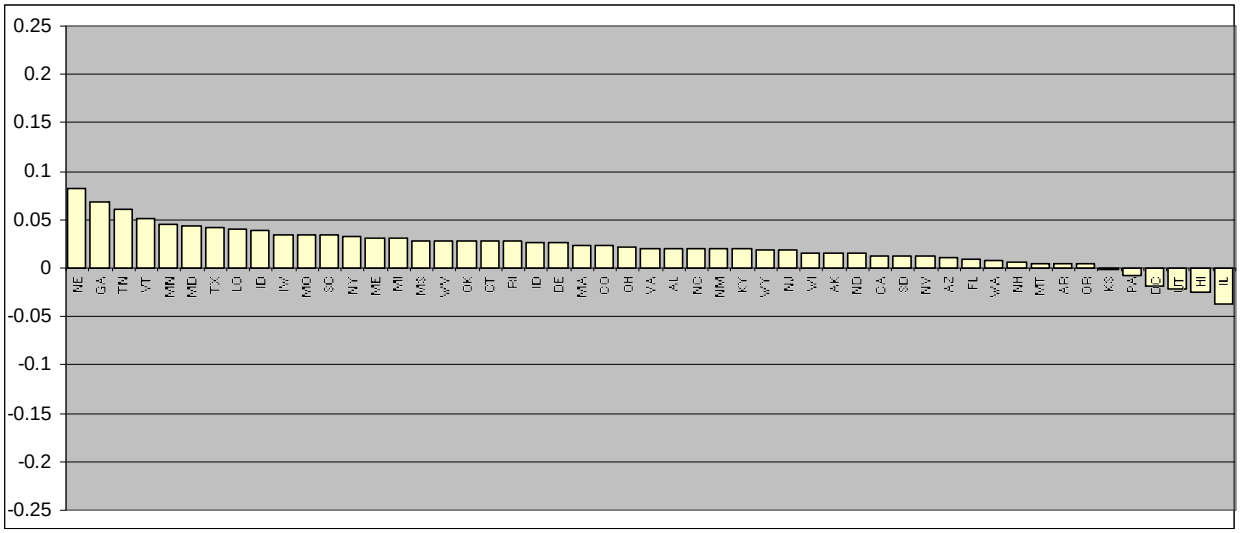
Note: differential = (full turnout dem minus observed dem) - (full turnout rep minus observed rep). Values are percentage of identifiers, third party vote not reported.

Figure 2.13. 1994 Ideology (Liberals minus Conservatives).



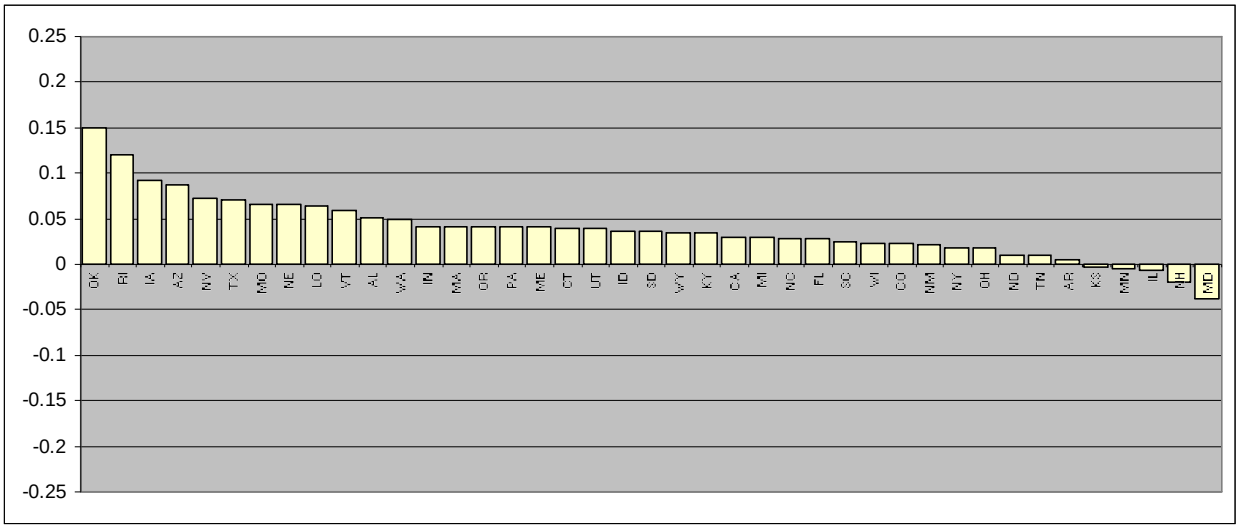
Note: differential = (full turnout lib minus observed lib) - (full turnout cons minus observed cons). Values are percentage of identifiers, middle of the road identifiers not reported.

Figure 2.14. 1996 Ideology (Liberals minus Conservatives).



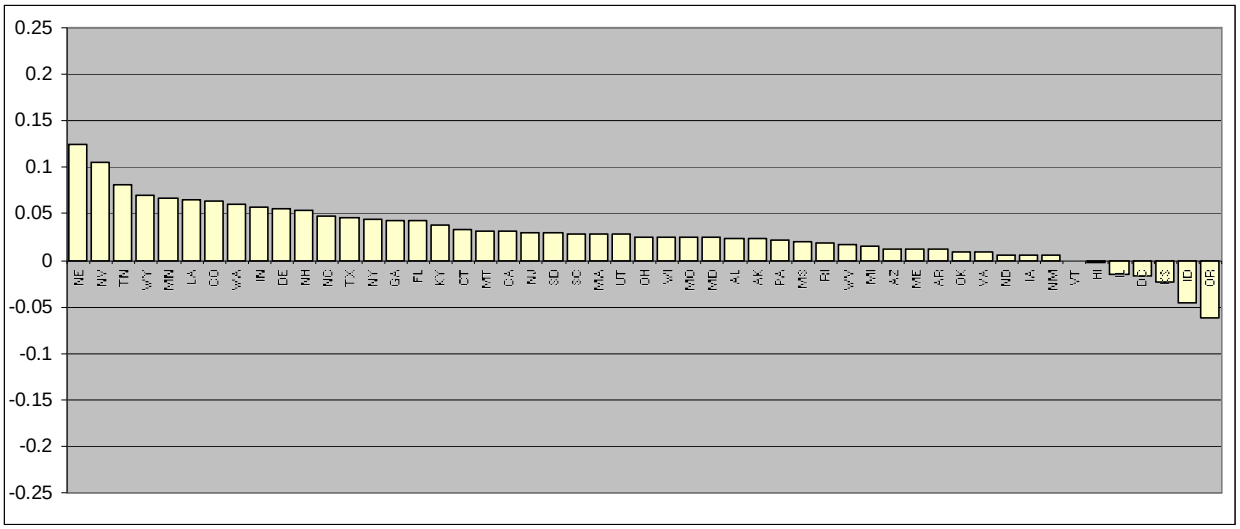
Note: differential = (full turnout lib minus observed lib) - (full turnout cons minus observed cons). Values are percentage of identifiers, middle of the road identifiers not reported.

Figure 2.15. 1998 Ideology (Liberals minus Conservatives).



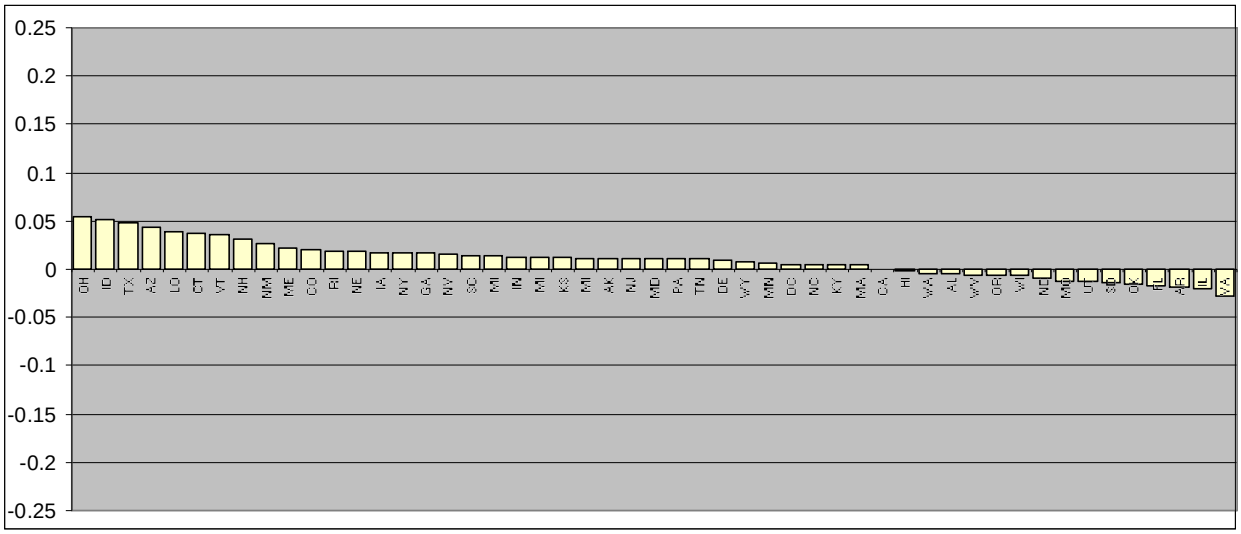
Note: differential = (full turnout lib minus observed lib) - (full turnout cons minus observed cons). Values are percentage of identifiers, middle of the road identifiers not reported.

Figure 2.16. 2000 Ideology (Liberals minus Conservatives).



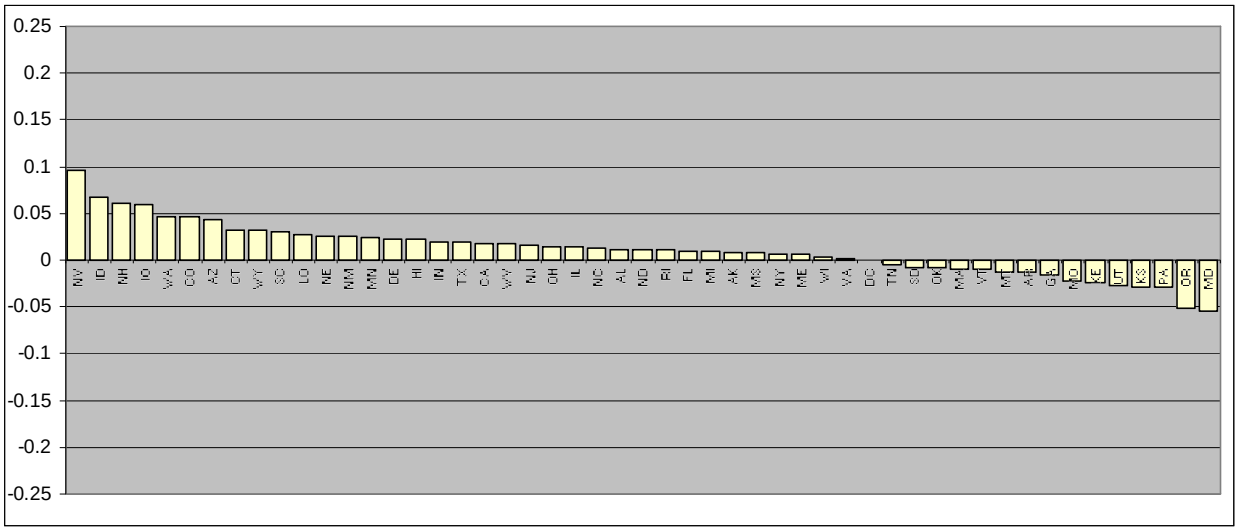
Note: differential = (full turnout lib minus observed lib) - (full turnout cons minus observed cons). Values are percentage of identifiers, middle of the road identifiers not reported.

Figure 2.17. 1996 Presidential vote (Democrats minus Republicans).



Note: differential = (full turnout vote dem minus observed vote dem) - (full turnout vote rep minus observed vote rep). Values are percentage of voters, third party vote not reported.

Figure 2.18. 2000 Presidential vote (Democrats minus Republicans).



Note: differential = (full turnout vote dem minus observed vote dem) - (full turnout vote rep minus observed vote rep). Values are percentage of voters, third party vote not reported.

The graphs compare the respondent party identification, ideology (the ideology question was not asked in all of the exit polls in 1994 and 1996), and presidential vote in 1996 and 2000. It is important to note that the differential measures simplify the presentation of the results. In effect, I also have estimates for political independent, moderate ideologues, and third party voters in each of the statewide samples (remember that the model only re-weights the exit poll statistics). However, the loading of a three way table for more than 180 cases over four elections is very hard to interpret and analyze. I have not included these results here, but they are available from the author upon request. For ease of presentation, I use a simple formula to tabulate the results into one easily readable statewide differential value. Each bar represents the difference between the estimated value means of full turnout for Democrats/liberals/Democratic-voters in the state minus the observed un-weighted values for these same estimates. I then subtract to this total the difference between the estimated value means of full turnout for Republicans/conservatives/-

Republican-voters in each state minus the observed un-weighted values for these same estimates. Formally:

(3) $$\left[\sum_{1}^{n_s}\frac{(y_{is}\times p_{is}^{-1})}{n_s}-\sum_{1}^{n_s}\frac{y_{is}}{n_s}\right]-\left[\sum_{1}^{n_s}\frac{(z_{is}\times p_{is}^{-1})}{n_s}-\sum_{1}^{n_s}\frac{z_{is}}{n_s}\right]=differential_s$$

(4) $$p_{is}=\left(\frac{1}{1+e^{-(\alpha+\beta_1 X_1+\beta_2 X_1+\beta_i X_i)}}\right)$$

Where p the probability of voting for individual i in state s, is the selection weight (inverse of the predicted logit coefficient), y and z are the partisan/ideology/voting statistics, and $\beta_i X_i$ represent the independent variables in the model (age, race, gender, income, education, union, working status, and married). Assuming a full turnout rate in a state, a positive score would entail that the differential favors the Democrats/liberals. A statewide differential value that approaches zero presupposes that an increase in turnout would not make a difference in the statewide distribution of preferences between the Republicans/conservative and Democrats/liberals—either because the increase in support is insignificant for both groups, or because this increase is of similar proportion in each groups (in this case the number of independent voters would shrink). Finally, a negative differential entails that the Republicans/conservatives would see their share of support increase assuming full turnout in the state.

I begin by looking at the party identification differential in figures 2.9, 2.10, 2.11, and 2.12. Of course, partisanship is not perfectly correlated with voting intentions. But it is a very strong predictor of the vote. And unlike presidential or senatorial elections votes, which are not available in all of the exit poll, partisanship is the most consistent measure found in these studies. One first notices that full turnout would generate an increase in the number of democratic voters

in the electorate in most of the states included in the analyses (75% of the states in 1994; 63% in 1996; 63% in 1998; 65% in 2000). That being said, the increase in support is somewhat small. It only reaches the 5% values in a couple of states in each election year. Assuming that the traditional margin of errors in public opinion survey is ± 3%, I feel pretty confident that any differential value greater than 4% has a very low probability of being caused by random sampling or measurement errors alone. Consequently, I will focus on those states where the registered differential surpasses the 4% threshold.

Incidentally, most of the substantive differential values are generally found on the democratic identifier side. These results also fit nicely with the Citrin, Schickler, and Sides (2003) thesis on statewide fluctuations in senatorial party support since full turnout does actually increase the number of republican voters in a few states. For example in 1994, assuming full turnout, Virginia and Ohio would have seen the number of Republican voters increase by a few points. Maryland is another good example, where support for the Republican Party would have substantially improved in 1998 and 2000. Maryland is by far one of the nation's most Democratic states, with an African American population of about 28% and its high proportion of federal employees. So one shouldn't be surprised to find that some Republican identifiers, part of the minority statewide constituency (constituents who do not support the incumbent), do not bother to turnout on Election Day. This result confirms the two effect theory developed by DeNardo (1980) which stipulates that higher turnout helps the party with the larger proportion of supporters in the state, but that this effect can also be countervailed by the higher defection rates of these peripheral voters. Consequently, in heavily Democrat districts, higher turnout should favor the Republican Party because of higher peripheral voter defection rates. But I also believe

that a third effect might also be at play. In effect, it is possible that abstention is a consequence of uncompetitiveness in the districts. I will return to this point in the discussion section and in chapter three.

Not only do I find statewide fluctuations in partisanship support in each election year, but I also notice statewide fluctuations in partisanship distribution across election years. Certain states, like Arizona, Colorado, and Texas, could always have counted on a substantive increase in Democratic Party identifiers in each election had all voters turned out.

However, other states, like Ohio and Illinois experience a complete reversal in their party differential support. For instance, in 1994 an increase in participation would have helped Ohio Republicans (-4.4%); but in 1996, the Democratic Party would have benefited from full turnout (+7.3%). This particular phenomenon may be explained by the Republican takeover of Congress in 1994, when the Democrats suffered a backlash at the polls. Assuming full turnout in 1994, Ohio would have seen a 3% decline in Democratic supporters, a 1% incline in Republican supporters, and a 2% increase in independents (not shown in the graph).

The party differential can also drop substantially from one election to the next, like in Colorado, where it went from .24 in 1994 to .03 in 1996. Which in a sense fits well with my thesis since turnout generally increases in presidential election years (turnout officially increased from 58% to 65% in Colorado between 1994 and 1996). Of course, this seven point increase cannot account for this 21 point shift in partisan support. Other factors must be at play. For instance, it is possible that there were a disproportionate number of Democratic voters who stayed home in Colorado in 1994. Also, Colorado's population grew robustly, up 14% from 1990–95, and most of the newcomers appeared to be family-oriented cultural conservatives. In

this state, Republican registration rose by 156,000 between the two elections while Democratic registration rose by about 38,000."[15] The changing composition of the population can certainly explain, at least in part, why this model of turnout predictions can behave heretically if one compares one election with the next.

Looking now at the ideology measure (2.13, 2.14, 2.15, and 2.16), one sees the presence of some very large fluctuations across states. Like in the case of partisanship, the differential value is equal to the difference between the estimated mean values of full turnout in each state minus the observed un-weighted values for these same estimates (liberals minus conservative). Once again, the differential seem to favor liberal voters (remember that "middle of the road" respondents are not reported here). But what is really interesting is that in all years, southern states would have seen the number of liberal voters increase under full turnout (which is consistent with Nagel & McNulty, 2000 results). Many factors can account for this trend, but probably the most convincing is the fact that turnout is generally lower in the South, and that individuals of lower socio-economic status (SES) tend to vote less and have more liberal policy preferences. Of course ideology, like partisan identification, is not a perfect predictor of the vote. Nonetheless, it gives a good indication of the statewide general policy preference over a broad set of policy issues (such as redistributive governmental programs).

One also finds that there are fluctuations in the ideological differential within election years. Once again, the Citrin, Schickler, and Sides (2003) thesis is validated, since states like Illinois or Kansas would have seen the number of conservative voters increase. This should come as no surprise in Illinois, where the southern part of the state is considered to be more culturally

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[15] http://nationaljournal.com.turing.library.northwestern.edu/pubs/almanac/1998/oh.htm. Retrieved May 17, 2007.

conservative than the north. And since low voter turnout is generally the rule in Southern Illinois, it is normal to find that an increase in participation adds to the number of conservative voters; incidentally this is what my model predicts.

The results also display some consistency in the distribution of preferences across election years. For instance, in Nebraska, the number of liberal voters would have increased quite substantially in all the elections under study. Nebraska is a very strong conservative state. Bush carried Nebraska with a large margin in 2000. He amassed more than 60% of the vote in 86 of the 93 counties and won all of the counties. It follows that the number of liberal voters is very small in this state (around 17% in 2000 according to the statewide exit poll). Since liberal Nebraskans are a minority in this largely conservative area, it is possible that this group might be more inclined to stay home on Election Day (because their probability of influencing the electoral outcome is very small). Hence full turnout may produce a modest increase in the number of liberal voters even though their overall weight is very small in the electorate. In short, the simulated calculations of the ideological differential demonstrate that in a majority of the states, full turnout increases the number of liberal voters (especially in the South). Given the fact that individuals with lower SES are less likely to vote and more likely to favor liberal redistributive policies, one should expect a surge in turnout to displace the median voter toward the left.

The main conclusion so far is that greater participation has the potential to change the distribution of preferences among political units. It appears that in some overwhelming partisan states, full turnout will actually increase the number of voters who identify with the minority party. The DeNardo thesis cannot account for this fact since the peripheral voter argument

predicts that a surge in participation will lead to an increase in the number of moderates and independent voters, which is not what I observe in the simulations.

The models also permit us, in two of four exit poll studies, to calculate the impact of full turnout on presidential vote in each state. Figures 2.17 and 2.18 present these estimates. Here again the differential between the weighted and un-weighted turnout values is reported (Democratic vote minus Republican vote). Both graphs show that the Democratic Party would have benefited from a modest increase in support had everyone voted. However, certain states like Utah or Maryland would have actually seen a surge in the support for the Republican presidential candidates in 1996 and 2000. Probably the most important finding in these two analyses is the relative low re-distribution of votes between the Democratic and Republican Party. In 1996, only a differential shift greater than 4% is found in Texas, Idaho and Ohio. The difference, however, is more important in 2000. In this election, the support for the Democratic Party in Nevada would have almost increased by 10%. Nevada has one of the lowest turnout rate in the United States (it ranked 47th in 2000, turnout was around 38% of the voting age population). This is probably related in part to the fact that 17% of adults in Las Vegas and Clark County had college degrees in the 1990s, one of the lowest numbers for any big metropolitan area in the United States (remember that education is a strong predictor of turnout). Furthermore, Nevada is one of the fastest growing states (the population increased more than 60% between 1990 and 2000) which probably help explain why registration—and consequently—turnout is so low. All these factors combined with the oft mentioned relationship between lower SES and the Democratic vote can help us understand this important differential value. The same can be said about Arizona, which is one of America's fastest growing states. Arizona went for Clinton in

1996, and under a full turnout scenario, the state would have seen its share of Democratic voters increase by almost 5%. The same trend is also observed in 2000. However, this time the state was carried by the Republicans. Under full turnout, Al Gore would have won. Many believe that the elderly voters, fearing for Medicare, and environmental issues helped Clinton win in 1996 (Barone & Ujifusa 1999). Also, the phenomenal growth rate of the state (the population grew from 3.6 millions in 1990 to 5.1 million in 2000) and the important proportion of residents with a high school degree only (81%) explains why an increase in turnout would favor the Democrats.

Of course, a statewide increase of 3 or 4% in support for the Democratic party could have a major impact when it is enough to actually shift the plurality (or majority) of the vote away from the majority party and change the electoral outcome in a state. Tables 2.3 and 2.4 present such an analysis for the 1996 and 2000 elections. The first four columns list the actual percentage and Electoral College vote that both major parties received; columns five and six reproduce the statewide exit poll results; and column seven and eight show the estimates of the simulated weighted vote for both the Republican and Democratic parties under full turnout.

Table 2.3

1996 Full Turnout Scenario Electoral Results

1996	Percentage of Votes		Electoral Votes		Exit Poll Results		Full Turnout Estimates		Switch	Electoral Vote	
State	D	R	D	R	D	R	D	R		D	R
Alabama	0.43	0.50	0	9	0.43	0.51	0.42	0.50		0	9
Alaska	0.33	0.51	0	3	0.39	0.48	0.39	0.47		0	3
Arizona	0.46	0.44	8	0	0.48	0.44	0.51	0.40		8	0
Arkansas	0.54	0.37	6	0	0.55	0.38	0.53	0.39		6	0
California	0.51	0.38	54	0	0.51	0.39	0.51	0.39		54	0
Colorado	**0.44**	**0.46**	**0**	**8**	**0.46**	**0.47**	**0.48**	**0.45**	**SWITCH**	**8**	**0**
Connecticut	0.52	0.35	8	0	0.53	0.36	0.56	0.33		8	0
Delaware	0.52	0.37	3	0	0.52	0.37	0.52	0.36		3	0
D.C.	0.85	0.09	3	0	0.88	0.09	0.88	0.09		3	0
Florida	0.48	0.42	25	0	0.48	0.42	0.46	0.43		25	0

Georgia	**0.46**	**0.47**	**0**	**13**	**0.46**	**0.47**	**0.46**	**0.44**	**SWITCH**	**13**	**0**
Hawaii	0.57	0.32	4	0	0.59	0.33	0.59	0.33		4	0
Idaho	0.34	0.52	0	4	0.34	0.53	0.39	0.49		0	4
Illinois	0.54	0.37	22	0	0.55	0.37	0.52	0.39		22	0
Indiana	0.42	0.47	0	12	0.42	0.48	0.42	0.46		0	12
Iowa	0.50	0.40	7	0	0.51	0.41	0.51	0.38		7	0
Kansas	0.36	0.54	0	6	0.36	0.55	0.37	0.53		0	6
Kentucky	0.46	0.45	8	0	0.46	0.45	0.46	0.44		8	0
Louisiana	0.52	0.40	9	0	0.53	0.40	0.56	0.36		9	0
Maine	0.52	0.31	4	0	0.53	0.32	0.55	0.30		4	0
Maryland	0.54	0.38	10	0	0.55	0.39	0.55	0.37		10	0
Massachusetts	0.61	0.28	12	0	0.62	0.29	0.63	0.28		12	0
Michigan	0.52	0.38	18	0	0.52	0.39	0.52	0.37		18	0
Minnesota	0.51	0.35	10	0	0.53	0.35	0.53	0.35		10	0
Mississippi	0.44	0.49	0	7	0.44	0.50	0.45	0.50		0	7
Missouri	0.47	0.41	11	0	0.48	0.43	0.49	0.41		11	0
Montana	0.41	0.44	0	3	0.42	0.45	0.40	0.46		0	3
Nebraska	0.35	0.54	0	5	0.35	0.54	0.37	0.52		0	5
Nevada	0.44	0.43	4	0	0.45	0.44	0.46	0.42		4	0
N.-Hampshire	0.49	0.39	4	0	0.50	0.40	0.53	0.37		4	0
New Jersey	0.54	0.36	15	0	0.54	0.37	0.54	0.36		15	0
New Mexico	0.49	0.42	5	0	0.51	0.43	0.53	0.40		5	0
New York	0.59	0.31	33	0	0.60	0.32	0.61	0.30		33	0
North Carolina	0.44	0.49	0	14	0.44	0.49	0.44	0.48		0	14
North Dakota	0.40	0.47	0	3	0.40	0.47	0.39	0.47		0	3
Ohio	0.47	0.40	21	0	0.48	0.42	0.51	0.36		21	0
Oklahoma	0.40	0.48	0	8	0.41	0.49	0.39	0.49		0	8
Oregon	0.47	0.39	7	0	0.50	0.41	0.48	0.41		7	0
Pennsylvania	0.49	0.40	23	0	0.50	0.41	0.50	0.39		23	0
Rhode Island	0.60	0.27	4	0	0.62	0.27	0.63	0.26		4	0
South Carolina	0.44	0.50	0	8	0.44	0.50	0.45	0.48		0	8
South Dakota	0.43	0.46	0	3	0.43	0.47	0.42	0.48		0	3
Tennessee	0.48	0.46	11	0	0.48	0.46	0.48	0.44		11	0
Texas	**0.44**	**0.49**	**0**	**32**	**0.44**	**0.50**	**0.49**	**0.46**	**SWITCH**	**32**	**0**
Utah	0.33	0.54	0	5	0.34	0.56	0.33	0.56		0	5
Vermont	0.53	0.31	3	0	0.56	0.32	0.56	0.28		3	0
Virginia	0.45	0.47	0	13	0.45	0.48	0.42	0.50		0	13
Washington	0.50	0.37	11	0	0.54	0.38	0.54	0.39		11	0
West Virginia	0.51	0.37	5	0	0.52	0.37	0.49	0.36		5	0
Wisconsin	0.49	0.39	11	0	0.50	0.39	0.48	0.39		11	0
Wyoming	0.37	0.50	0	3	0.37	0.50	0.37	0.49		0	3
Total	**0.49**	**0.41**	**379**	**159**	**0.49**	**0.42**	**0.49**	**0.41**		**432**	**106**

Note: Official election results. Values are rounded. Full turnout estimates are obtained using PSW method. Minor party vote is not shown.

Table 2.4

2000 Full Turnout Scenario Electoral Results

2000	Percentage of Votes		Electoral Votes		Exit Poll Results		Full Turnout Estimates		Switch	Electoral Vote	
State	D	R	D	R	D	R	D	R		D	R
Alabama	.42	.56		9	0.43	0.56	0.44	0.55			9
Alaska	.28	.59		3	0.31	0.59	0.31	0.57			3
Arizona	**.45**	**.51**		**8**	**0.46**	**0.51**	**0.50**	**0.46**	**SWITCH**	**8**	**0**
Arkansas	.46	.51		6	0.46	0.51	0.44	0.52			6
California	.53	.42	54		0.54	0.42	0.56	0.40		54	
Colorado	**.42**	**.51**		**8**	**0.43**	**0.51**	**0.47**	**0.47**	**SWITCH**	**8**	**0**
Connecticut	.56	.38	8		0.57	0.39	0.60	0.36		8	
Delaware	.55	.42	3		0.55	0.42	0.57	0.39		3	
D.C.	.85	.9	21		0.86	0.10	0.86	0.10		21	
Florida	**.49**	**.49**		**25**	**0.49**	**0.49**	**0.50**	**0.48**	**SWITCH**	**25**	**0**
Georgia	.43	.55		13	0.44	0.56	0.42	0.57			13
Hawaii	.56	.37	4		0.56	0.38	0.58	0.35		4	
Idaho	.28	.67		4	0.29	0.70	0.35	0.62			4
Illinois	.55	.43	22		0.54	0.43	0.56	0.42		22	
Indiana	.41	.57		12	0.42	0.58	0.44	0.56			12
Iowa	.49	.48	7		0.54	0.43	0.60	0.37		7	
Kansas	.37	.58		6	0.39	0.57	0.36	0.60			6
Kentucky	.41	.57		8	0.41	0.57	0.39	0.59			8
Louisiana	.45	.53		9	0.45	0.53	0.47	0.49			9
Maine	.49	.44	4		0.49	0.44	0.49	0.44		4	
Maryland	.56	.40	10		0.57	0.40	0.52	0.45		10	
Massachusetts	.60	.33	12		0.60	0.33	0.59	0.33		12	
Michigan	.51	.46	18		0.52	0.46	0.53	0.46		18	
Minnesota	.48	.46	10		0.48	0.46	0.50	0.43		10	
Mississippi	.41	.58		7	0.42	0.57	0.42	0.56			7
Missouri	.47	.50		11	0.47	0.51	0.44	0.52			11
Montana	.33	.58		3	0.34	0.59	0.33	0.60			3
Nebraska	.33	.62		5	0.33	0.63	0.36	0.60			5
Nevada	**.46**	**.50**		**4**	**0.47**	**0.50**	**0.56**	**0.41**	**SWITCH**	**4**	**0**
N.-Hampshire	**.47**	**.48**		**4**	**0.47**	**0.48**	**0.53**	**0.43**	**SWITCH**	**4**	**0**
New Jersey	.56	.40	15		0.56	0.41	0.58	0.39		15	
New Mexico	.48	.48	5		0.49	0.47	0.52	0.44		5	
New York	.60	.35	33		0.60	0.36	0.60	0.35		33	
North Carolina	.43	.56		14	0.43	0.56	0.45	0.55			14
North Dakota	.33	.61		3	0.33	0.61	0.34	0.59			3
Ohio	.46	.50		21	0.46	0.50	0.47	0.48			21
Oklahoma	.38	.60		8	0.39	0.61	0.38	0.61			8
Oregon	**.47**	**.47**	**7**		**0.47**	**0.47**	**0.43**	**0.52**	**SWITCH**	**0**	**7**
Pennsylvania	**.51**	**.46**	**23**		**0.51**	**0.47**	**0.48**	**0.49**	**SWITCH**	**0**	**23**
Rhode Island	.61	.32	4		0.61	0.32	0.61	0.30		4	
South Carolina	.41	.57		8	0.41	0.57	0.44	0.54			8
South Dakota	.38	.60		3	0.38	0.61	0.37	0.61			3
Tennessee	.47	.51		11	0.47	0.51	0.47	0.51			11
Texas	.38	.59		32	0.38	0.59	0.40	0.58			32
Utah	.26	.67		5	0.27	0.68	0.24	0.69			5

Vermont	.51	.41	3		0.51	0.41	0.50	0.42	3	
Virginia	.44	.52		13	0.45	0.53	0.45	0.53		13
Washington	.50	.45	11		0.51	0.45	0.55	0.40	11	
West Virginia	.46	.52		5	0.46	0.52	0.48	0.50		5
Wisconsin	.48	.48	11		0.48	0.48	0.48	0.48	11	
Wyoming	.28	.68		3	0.29	0.70	0.32	0.67		3
Total	**.48**	**.48**	**266**	**271**	0.47	0.50	0.48	0.49	304	252

Note: Official election results. Values are rounded. Full turnout estimates are obtained using PSW method. Minor party vote is not shown.

As seen in 1996 (Table 2.3), Colorado, Georgia, and Texas would have switched toward the Democratic Party. Under the full turnout scenario, Clinton would have won 432 Electoral College seats. This is consistent with the overall "Clinton effect," namely, the fact that Bill Clinton enjoyed a strong showing in the South. In fact, as McKee and Shaw (2003) explain, it is quite possible that the increase in the southern Democratic vote in 1996 was due to the politics, personality, and native son status of Bill Clinton. So it is not really surprising that an increase in turnout would have benefited him even more.

But the 2000 election (Table 2.4) is even more interesting. In this election, Arizona, Colorado, Nevada, New Hampshire and Florida would have all shifted their support for Gore, given him a 311 Electoral College victory. However, what is striking is that Pennsylvania and Oregon would have switched to the Republicans. This may appear somewhat surprising, but not if one considers the fact that Pennsylvania was carried by the Republicans in 1980, 1984 and 1988. Furthermore, Pennsylvania was a very competitive state in 2004; Bush actually visited the state 44 times during his first term. But regardless of these facts, the puzzle remains to explain how could the support for the Republican Party actually *increase* under full turnout? Unlike Maryland, where the Republican vote also surged in the simulations for the 2000 election, Pennsylvania is not a stronghold for the Democratic Party. One should have expected Republican

and Democrat voters alike to be highly motivated to participate on Election Day; the closeness of the race entailing both sides to the polls. And since higher turnout rate usually mean a greater number of lower SES voters, the number of democratic voters should have actually increased in 2000. This is exactly what happened in 2004, when turnout increased by 10% (from over 50% in 2000 to 60% in 2004) and the Democrats carried the state (.51 versus .49).

So what happened in 2000? One potential explanation is related to the fact that the base turnout for the Democrats may have been near full capacity in this election. In effect, there was a massive minority and labor turnout drive in 2000, where African-American turnout actually exceeded white turnout.[16] Hence, maybe this model accounts for this unusual surge by giving more weight to Republican voters who had somewhat a lower probability of voting. Another explanation relates to the DeNardo two effect theory. One can assume here that the increase in peripheral voters in 2000 may have actually produced a higher defection rate among potential Democratic supporters. One thing is certain: the mere fact that my model predicts a 1.47% difference between the votes for both parties should definitely limit the interpretations of these results.

As for the case of Oregon, this result can be explained by the fact that an increase in turnout would have been highly favorable to Ralph Nader (result not shown here), which reduced the level of support for the Democratic Party in the full turnout simulation. Nader had his best showing in this state in 1996 (4% of the vote) and won 5% of the vote in 2000. Here again, the

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[16] Mark Gersh, "The New Battleground," *Blueprint Magazine* (published by Democratic Leadership Council) (2004, May 7). Retrieved April 21, 2007, from http://www.dlc.org/print.cfm?contentid=252571.

fact that Gore won the electoral vote in 2000 by less than 7000 votes highlights the fact that these results should be carefully interpreted.

Overall, it is interesting to note that the national average distribution of votes between the Republican and Democratic Party is almost equal in 2000 and shifts only one percentage point under the full turnout scenario by looking at the average of state differences. However, full turnout hypothetically produces a 12.5% increase in the Electoral College vote for Gore in 2000. So this example actually demonstrates that federalism and the Electoral College system can favor the democrats under full turnout, even following modest gains at the poll.

To sum up, the simulated weighted analyses of statewide turnout indicate that there are fluctuations in partisan support within and across elections. One has to remember that there are exogenous and institutional forces at play which might influence the dynamic of political campaigns and election results. For starter, turnout will fluctuate between presidential and midterm election. This can account for some of the variations in support found across elections. In addition, it is likely that full turnout increases the number of peripheral voters, whose loyalties, as indicated by DeNardo (1980), tend to be more volatile. The combination of both of these forces makes it difficult to expect systematic and predictable patterns of partisan support across elections under universal voting. However, I find the presence of certain clear and specific trends. The general impression is that the Democratic Party is the big winner in a majority of the states, especially in the South. I also find confirmation of the DeNardo (1980) and Citrin, Schickler, and Sides (2003) theses, which stipulate that the Republican Party, at least in a few states, could actually benefit from higher electoral participation. These results may at first appear to contradict the theory of incumbency and turnout presented in the introduction. However, one

has to remember that an increase in turnout will not automatically correspond to an increase in the number of votes for the Democratic Party. As was indicated earlier, higher turnout means a greater number of non-partisan and lower socio-economic status voters whose allegiance to the Democratic Party is in no way guaranteed ex ante. In fact, although socio-economic status is a good predictor of political participation, it is not a perfect predictor of voting preferences. Certain electors from this segment of the voting population may indeed prefer the Republican Party, even if they would theoretically benefit from a higher level of redistribution. Nevertheless, in an overwhelming majority of the cases, an increase in turnout would favor the Democrats (82% of the states in 1996, 80% in 2000).

I have also determined that ad hoc political, demographic, and unobservable factors can help explain some of the inconsistencies found in certain results. Because I base the calculations of the probability of voting on socio-demographic variables, the counterfactuals are highly susceptible to abrupt shifts in the composition of the statewide population, as seen in the cases of Nevada, Colorado or Pennsylvania. Also, as was indicated, punctual political events, like the Clinton effect in 1996, can also be expected to affect the overall distribution of partisanship and voting preferences. Notwithstanding the impact of these exogenous forces on the models, I feel confident in both the external and internal validity of the analyses.

2.10. Discussion

Estimating election results under different turnout scenarios is a common task in political science. Previous studies, some quite convincing, have tried to determine which party would benefit from a sudden increase in political participation (e.g., Tucker, Vedlitz, DeNardo 1986;

Nagel & McNulty, 2000, 1996; Citrin, Schickler, & Sides, 2003; Brunell & DiNardo, 2004). Conventional wisdom suggests that the Democratic Party should be the big winner. But recent work by Citrin, Schickler, and Sides (2003) indicate that in certain states these gain would carry over to the Republican Party. The preceding analysis has confirmed the Citrin et al. thesis, as well as the arguments put forward by DeNardo (1980, 1987) and later re-confirmed by Nagel and McNulty (1996, 2000), which stipulate that an increase in turnout actually has varying consequences across states and even benefit the Republican Party in some cases.

I have confirmed these results by using a novel methodology developed by Brunell and DiNardo (2004) which was determined to be more precise than the Binder/Oaxaca methodology employed by Citrin et al. The existence of a certain number of disparities between voters and nonvoters were validated, at the national level, using an extended number of public opinion surveys spanning over more than thirty years. These differences were especially important when I compared the opinions of people with a high propensity to vote with people with a low propensity of voting. I also considered the possibility that the strength of the national comparison of voters and nonvoters policy preferences may be weakened by regional disparities. Under full turnout the national ideological trend would seem to tilt toward the liberal side. And as the results show, greater political participation may actually have some very important electoral consequences. In the 2000 election, I actually demonstrated that the Democratic Party could have won the Electoral College vote. Therefore, unlike Citrin, Schickler, and Sides, I believe that compulsory voting can actually have drastic electoral consequence and transform the political agenda, especially if one considers the fact that peripheral or independent voters will be drawn out to the polls. This will undoubtedly affect the composition and the orientation of the

distribution of preferences in the political unit. Clearly, at the national level one could assume that the growing participation of a conservative electorate is putting pressure on Republican and Democratic incumbents to support more conservative ideological principles in Congress. However, at the local level, one could also expect in heavily Democrat constituency for example, that incumbents would favor more extreme liberal policy positions since they would be expecting a higher turnout rate among lower socio-economic status voters.

Scholars cannot continue to downplay or ignore the potential legislative impact of low or even declining turnout rates in American politics. This chapter has demonstrated that the average ideology and preferences of voters and nonvoters shows a clear differential when opinions are disaggregated at the state level. This was found to be true in a majority of the states, where this differential was usually skewed in a more conservative direction. Furthermore, I also established that when comparing voters with high and low probabilities of voting at the national level, a distinctive divergence in policy preference emerged. This difference is not unlike the predicted gap between peripheral and core voters' preferences predicted by DeNardo's thesis more than 25 years ago. Hopefully, this chapter has put to rest the claim that abstention cannot have any clear legislative consequences since voters and nonvoters only differ at the margins (Highton & Wolfinger, 2001; Rosenstone & Hansen, 1993, Wolfinger & Rosenstone, 1980). However, at present, it remains impossible to conclude that turnout level systematically affects legislative voting and electoral outcomes.

A number of primary challenges remain. First, it is necessary to construct an adequate measure of turnout level in every congressional district. This analysis has shown that when we disaggregate national opinion at the state level, we find regional fluctuations in voters and

nonvoters preferences. Hence, it is also necessary to think of the possibility that disaggregate statewide opinion by congressional district for example, may unearth the same type of disparities within the state. In many of the more local races, where incumbents are basically guaranteed reelection, voters and nonvoters may have a radically different median position—especially if we take into account the fact that lower socio-economic status and non-partisan constituents are less likely to vote in closed-seat elections (for a similar argument on congressional casework, see Wagner, 2003).

Because the incumbent reelection rate is so high (99% of the House in the 2002 and 2004 elections, see Abramowitz, Alexander and Gunning, 2006), open seat elections are basically the only remaining competitive congressional contests today (Cox & Katz, 2002; Ansolabehere, Brady, & Fiorina, 1992). So if one considers that there are fewer competitive elections in the House, one should expect to find that independents or occasional voters will have a lower probability of turning out to vote on Election Day. The remainder of this dissertation sets out to determine the validity of these claims by means of a series of empirical tests that measure the influence of incumbency on political participation, and more broadly on legislative behavior in the U.S. Congress.

Appendix 2.A

1. Ideological Distribution in the Cumulative American National Election Study

Figure 2.A1. Distribution of ideology, 1972–2002.

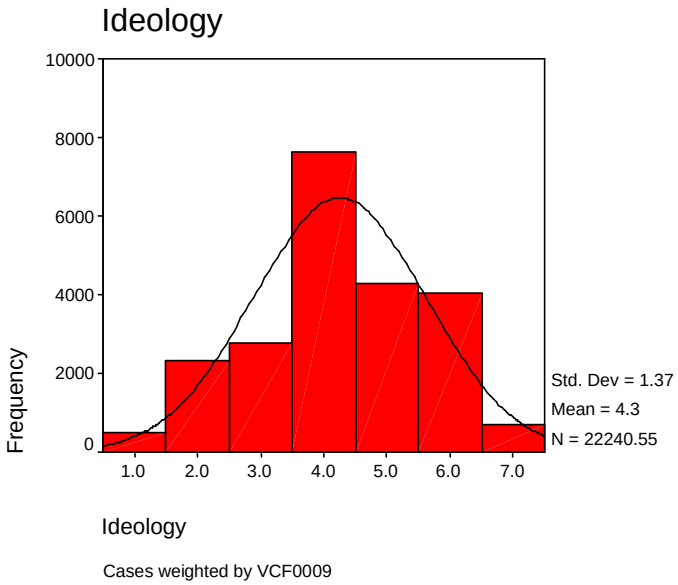


Figure 2.A2. Distribution of ideology of voters, 1972–2002.

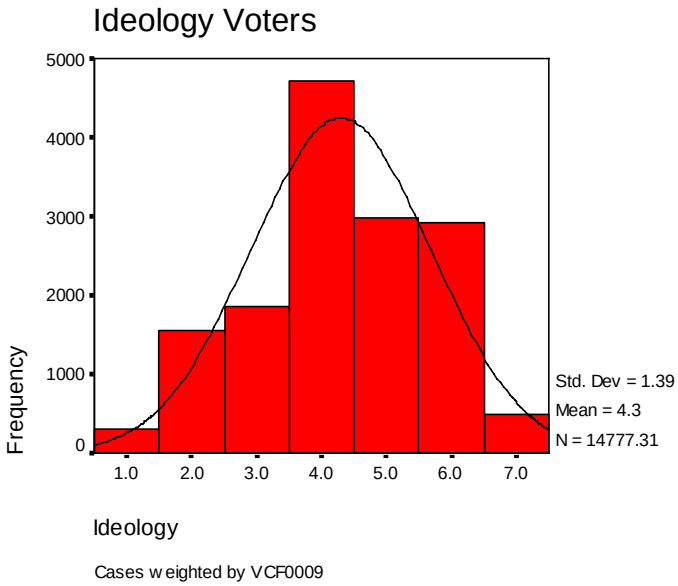


Figure 2.A3. Distribution of ideology of nonvoters, 1972–2002.

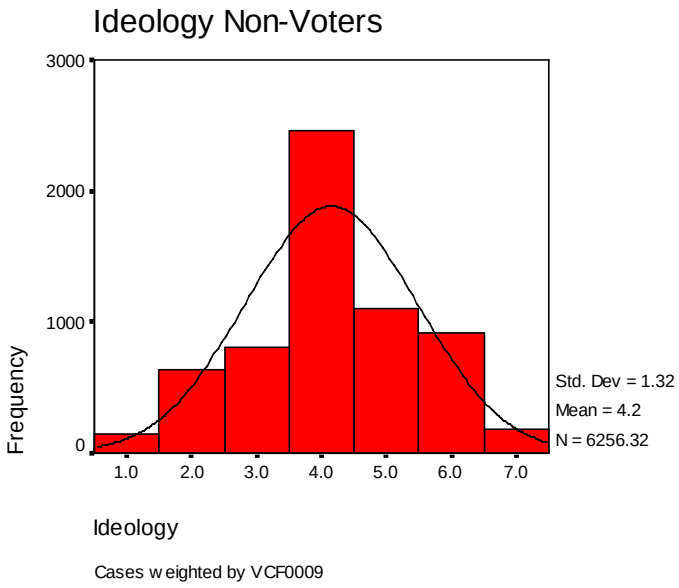


Figure 2.A4. Distribution of party identification, 1972–2002.

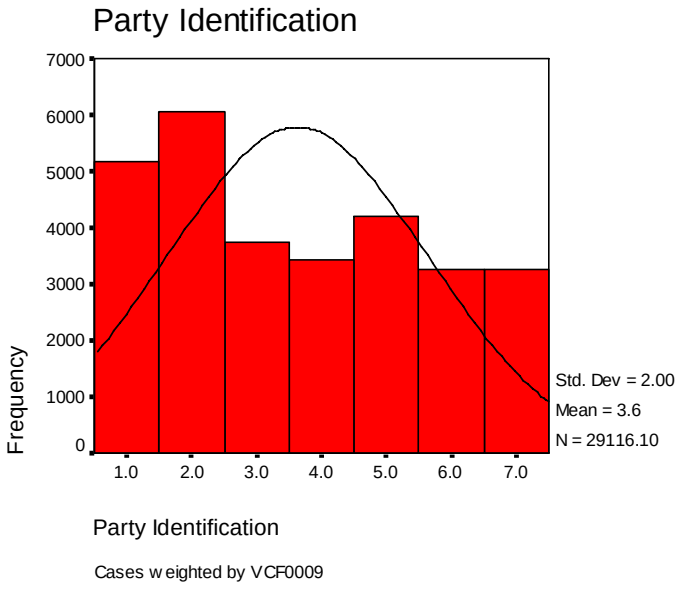


Figure 2.A5. Distribution of party identification for voters, 1972–2002.

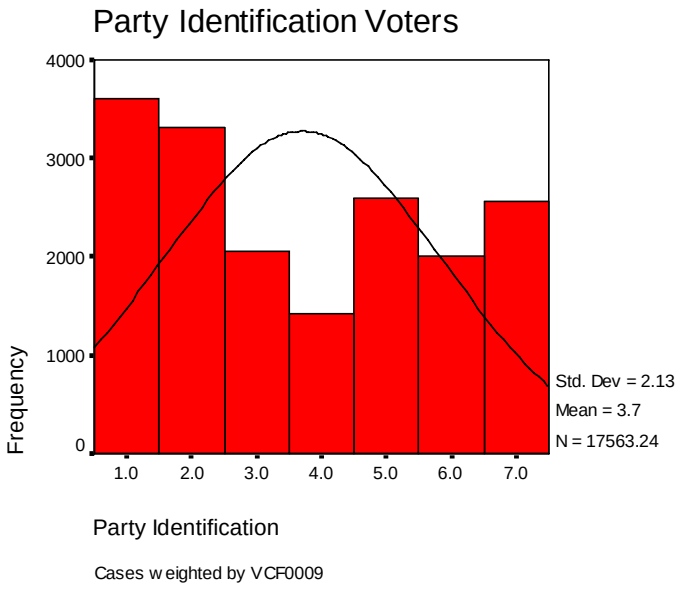
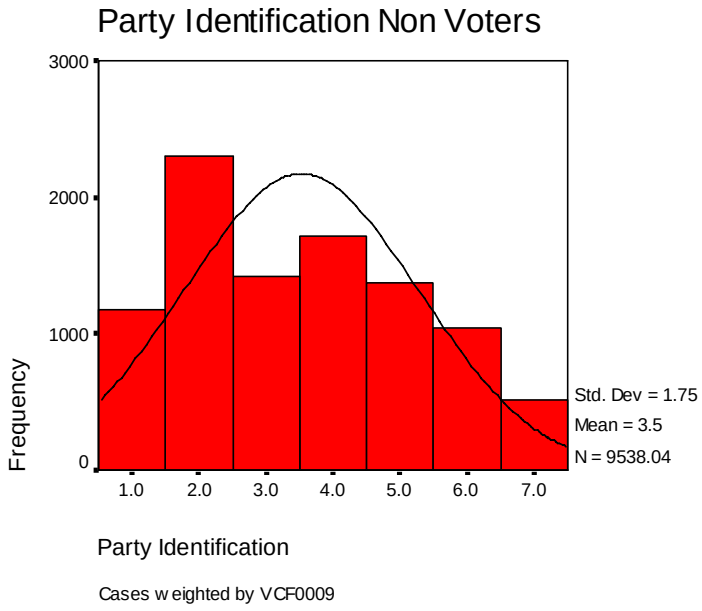


Figure 2.A6. Distribution of party identification for nonvoters, 1972–2002.



2. American National Election Studies, Question on Government Help

Figure 2.A7. Voters and nonvoters, 1972–2000.

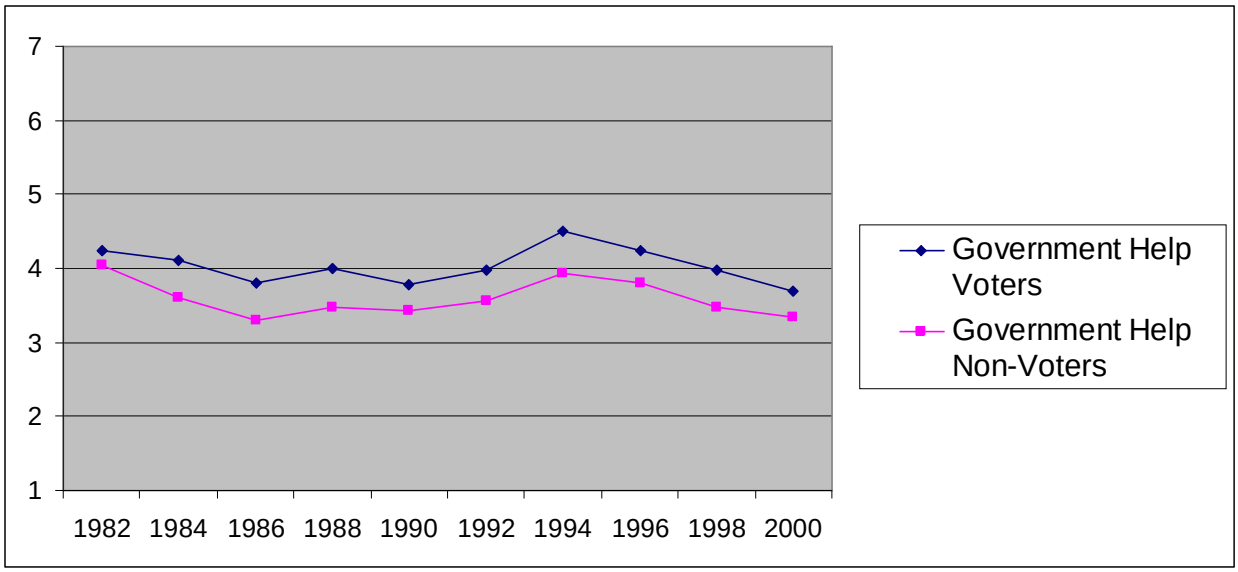


Figure 2.A8. Voters and nonvoters, verified, 1984–1990.

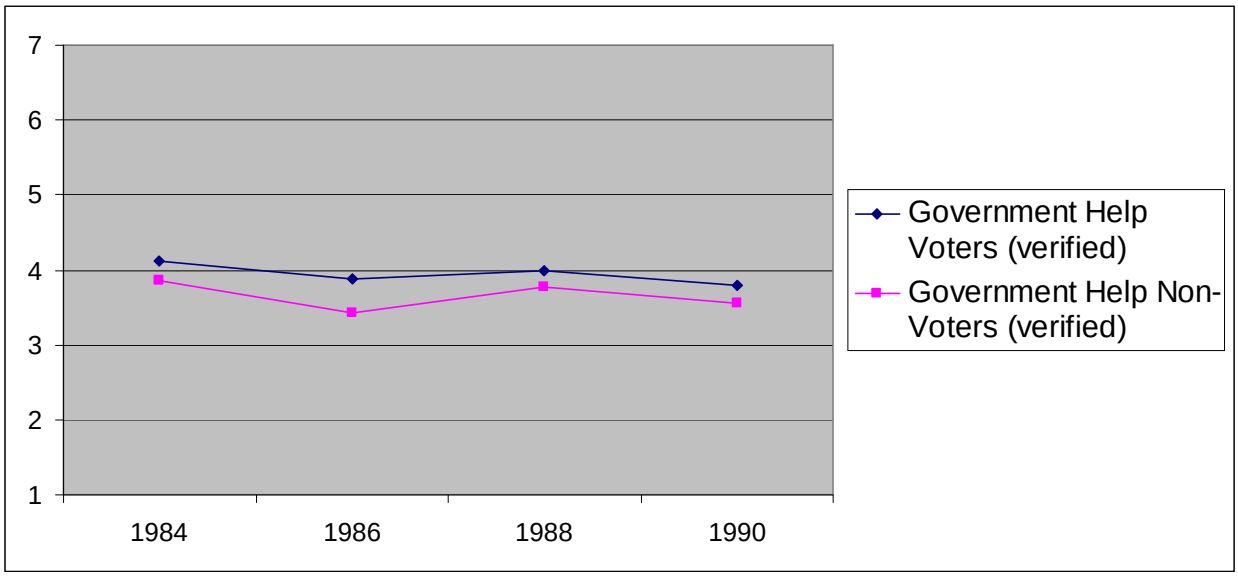


Figure 2.A9. Probability of voting 3 categories, 1972–2000.

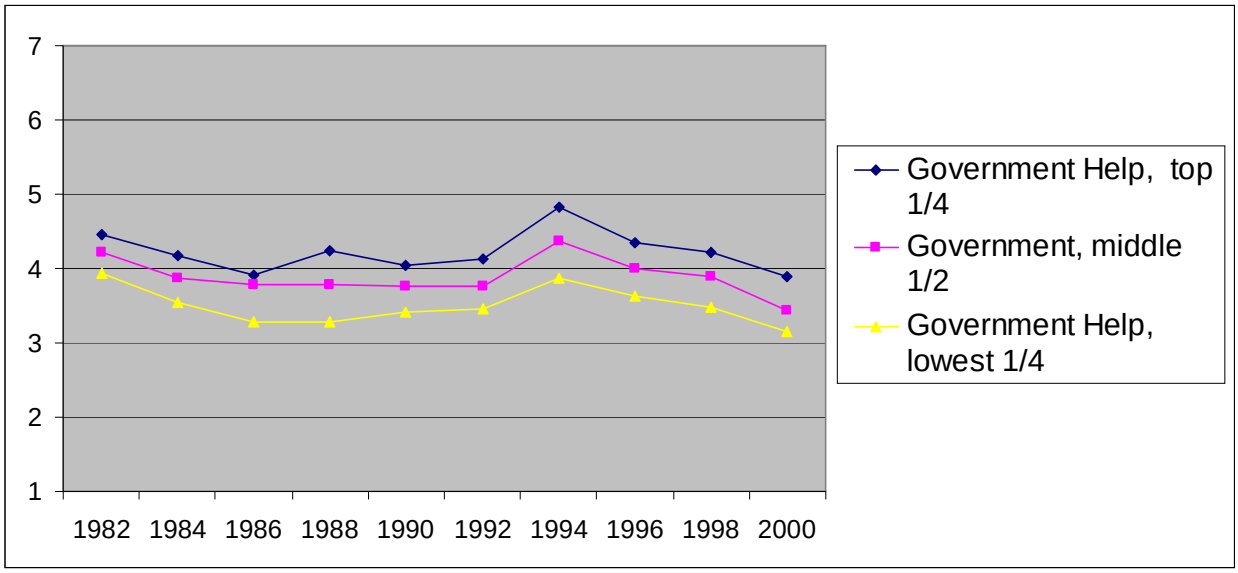
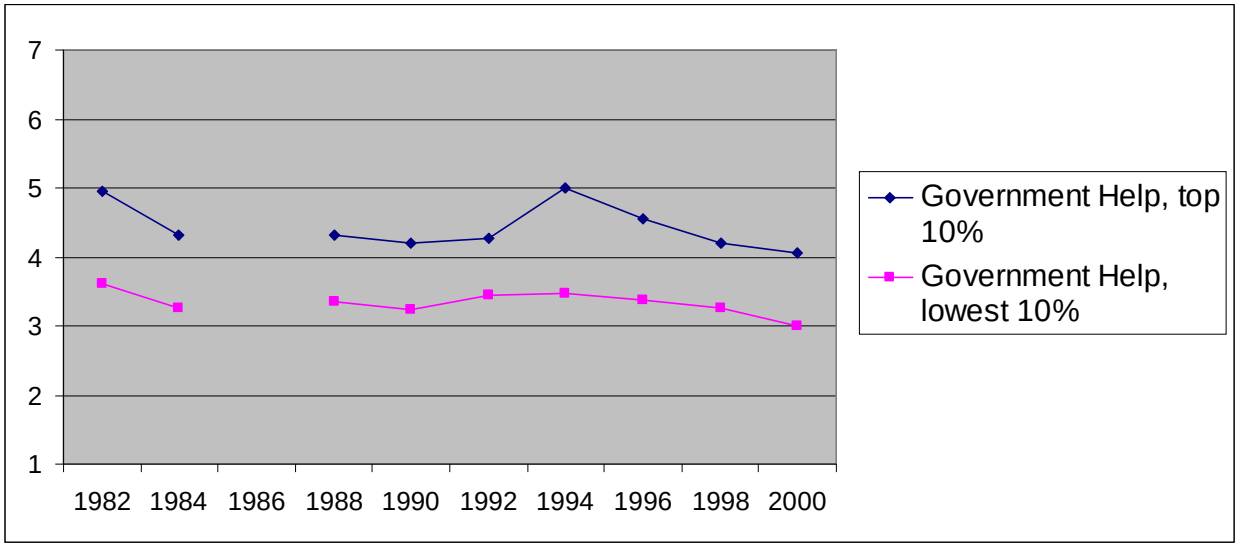


Figure 2.A10. Top 10% and Lowest 10% probability of voting, 1972–2000.



3. American National Election Studies, Question on Health Insurance

Figure 2.A11. Voters and nonvoters, 1976–2000.

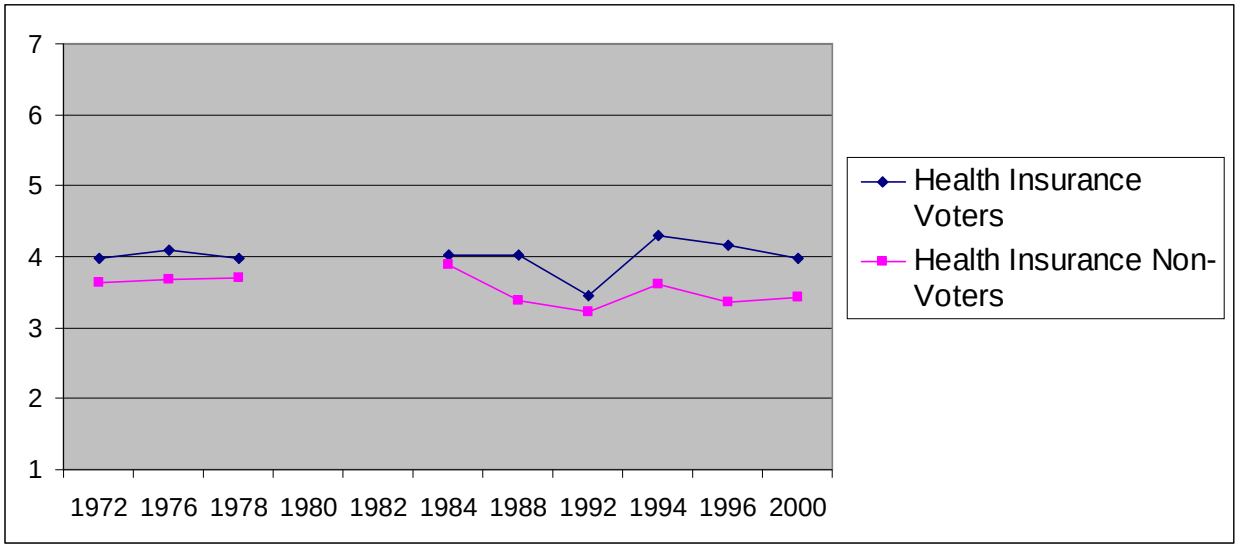


Figure 2.A12. Voters and nonvoters, verified, 1976–2000.

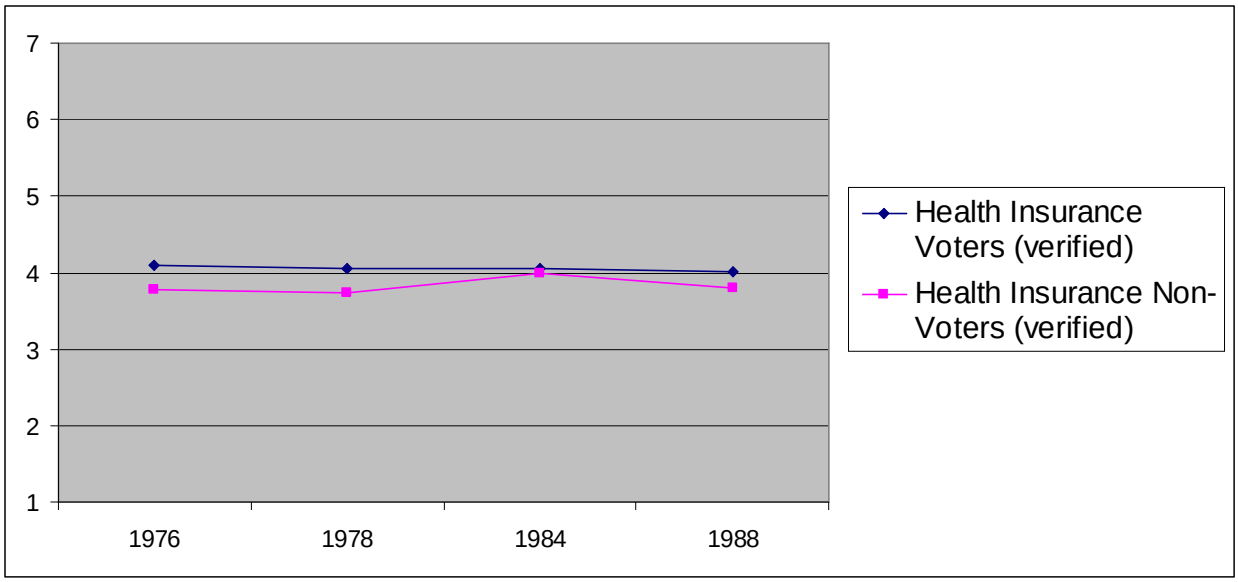


Figure 2.A13. Probability of voting 3 categories, 1976–2000.

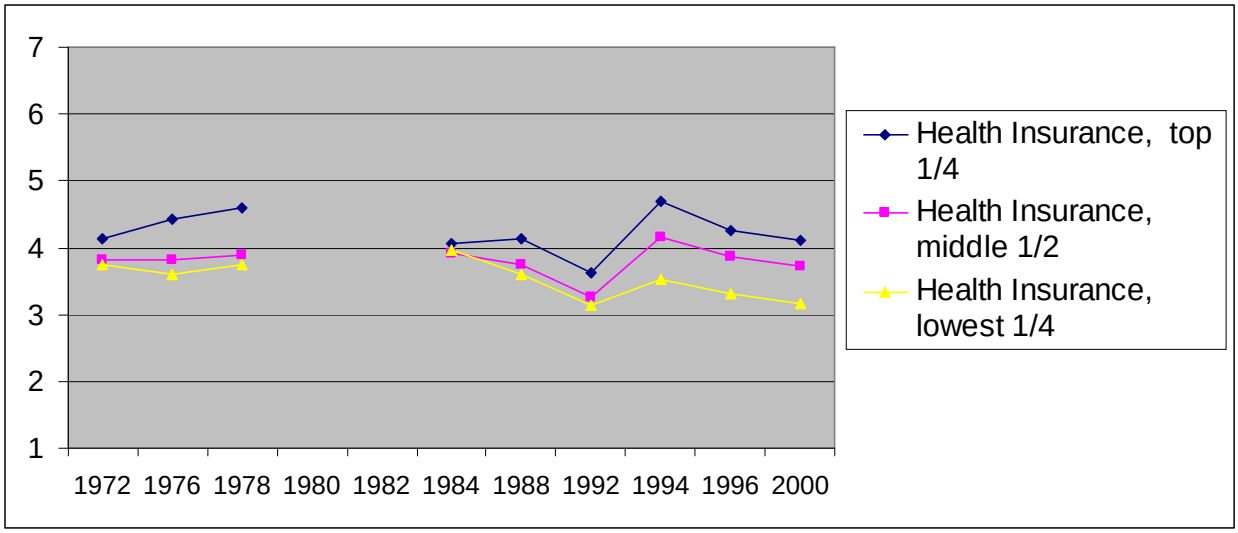
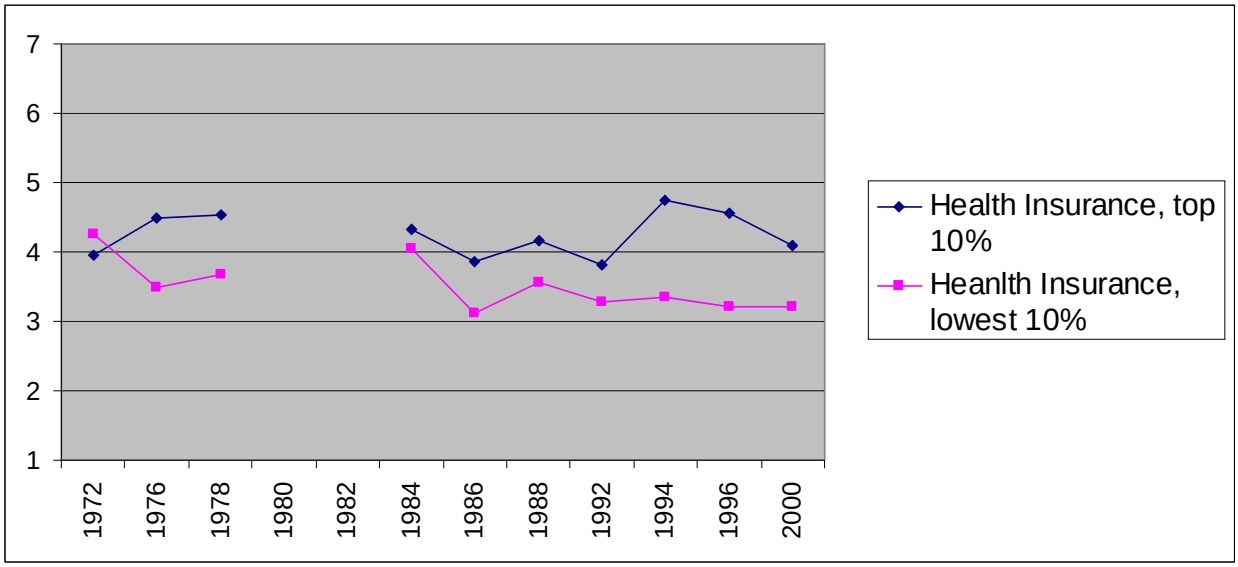


Figure 2.A14. Top 10% and lowest 10% probability of voting, 1976–2000.



4. American National Election Studies, Question on Jobs

Figure 2.A15. Voters and nonvoters, 1972–2002.

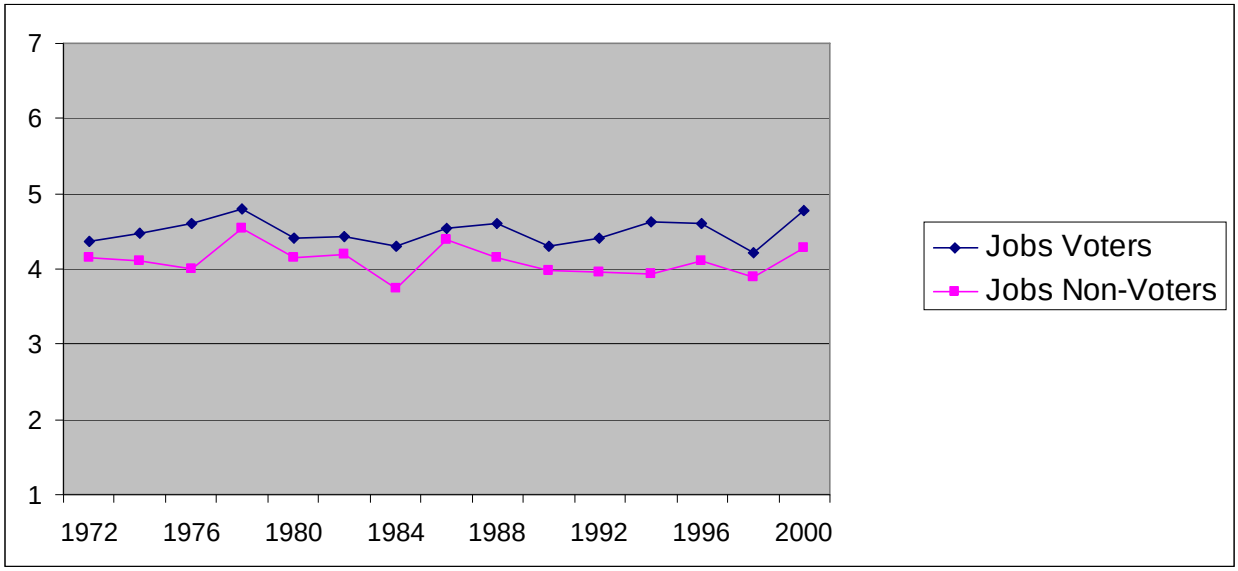


Figure 2.A16. Voters and nonvoters, verified, 1976–1990.

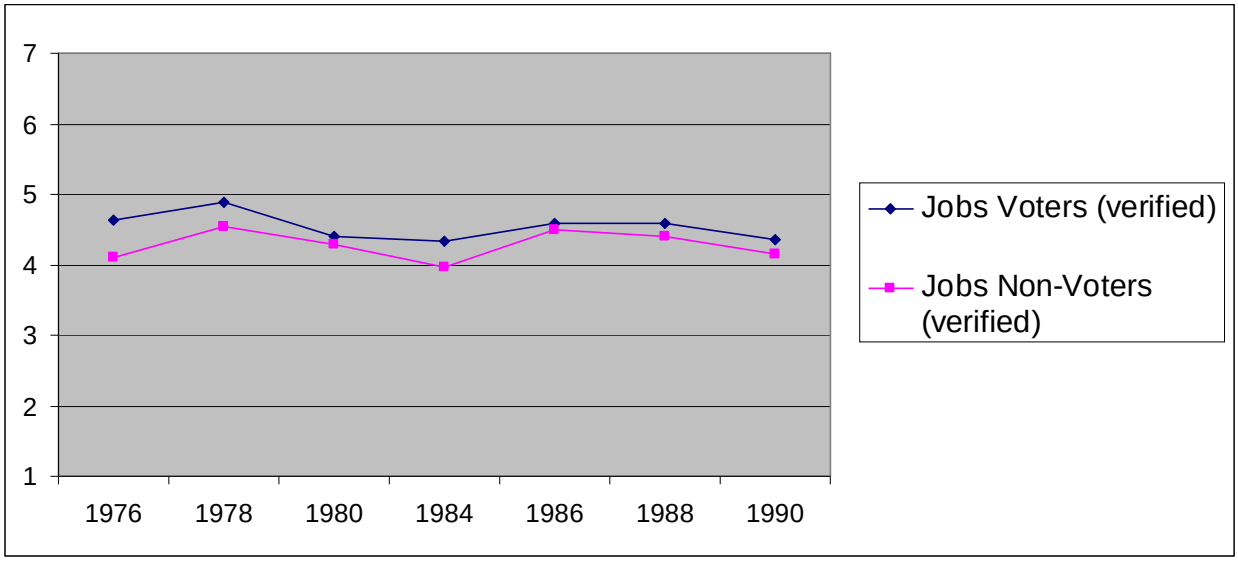


Figure 2.A17. Probability of voting 3 categories, 1972–2002.

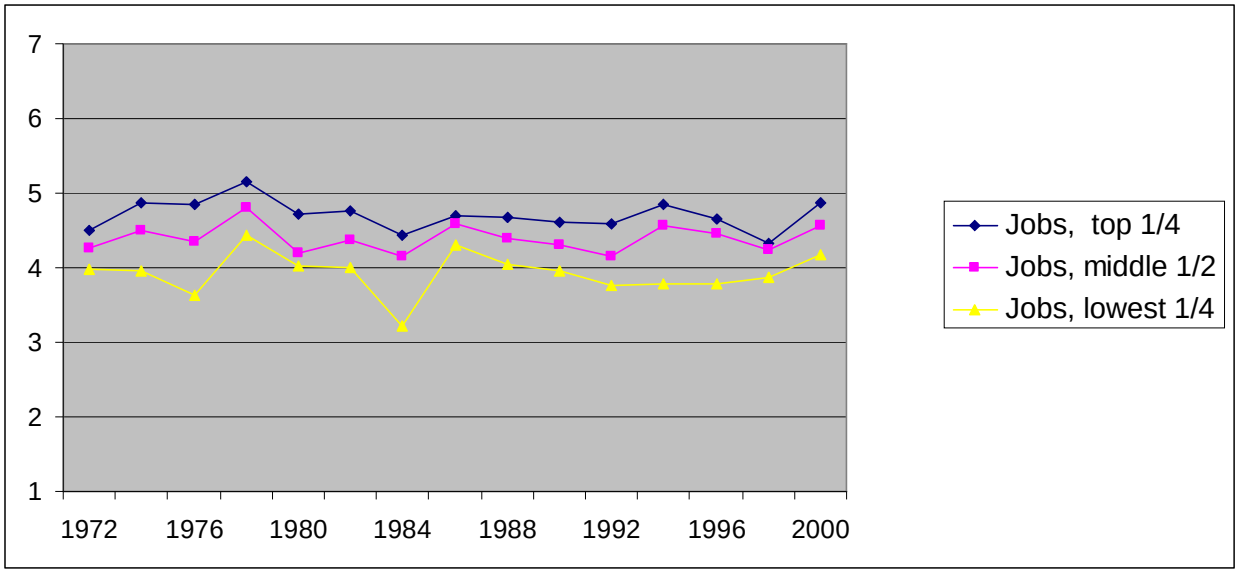
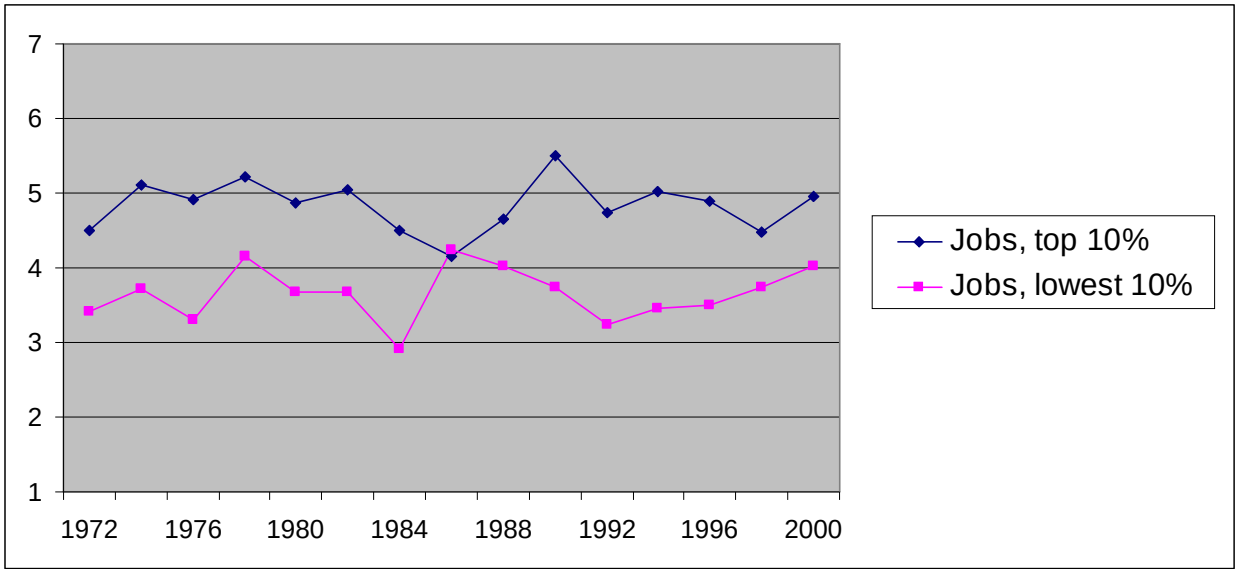


Figure 2.A18. Top 10% and lowest 10% probability of voting, 1976–2000.



5. American National Election Studies, Question on Different Issues

Figure 2.A19. Social security, 1984–2002.

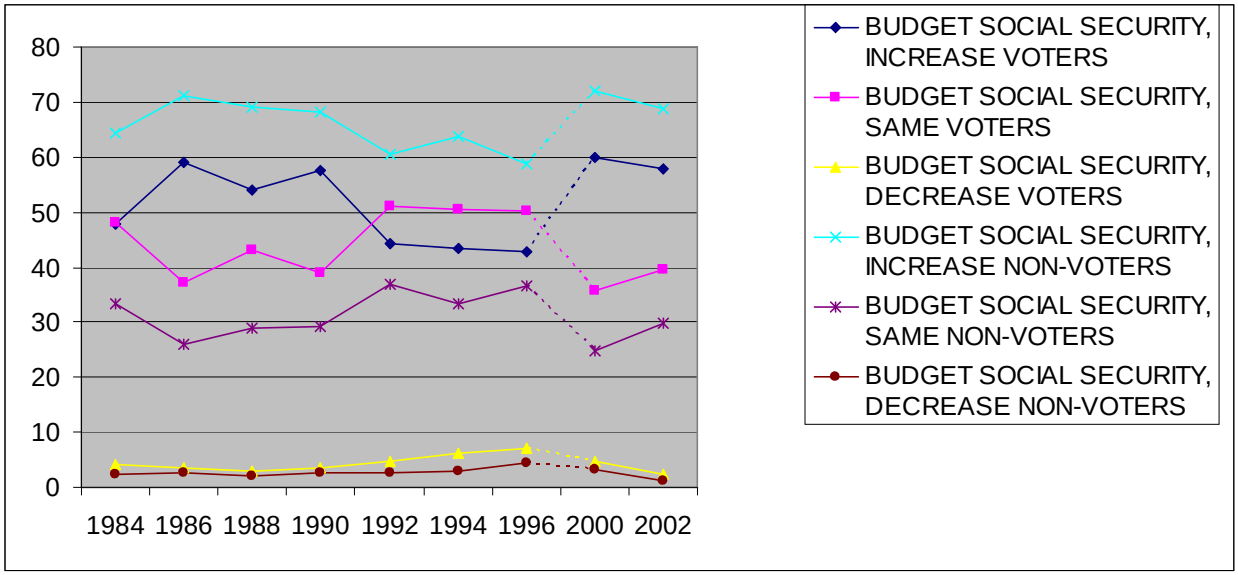


Figure 2.A20. Federal spending on children, 1988–2000.

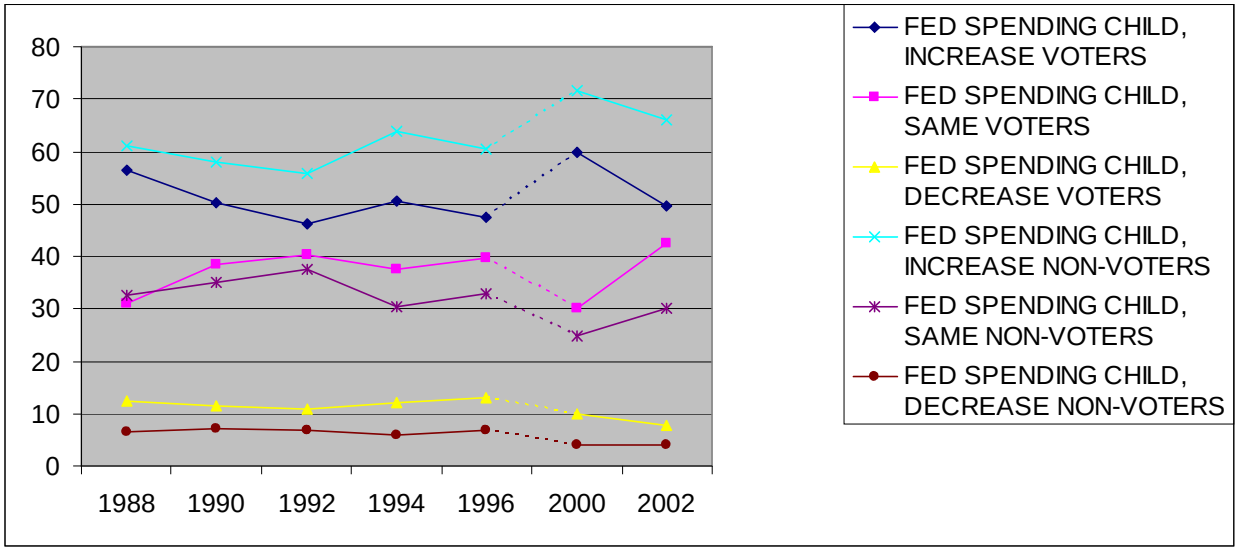


Figure 2.A21. Federal spending on crime prevention, 1984–2002.

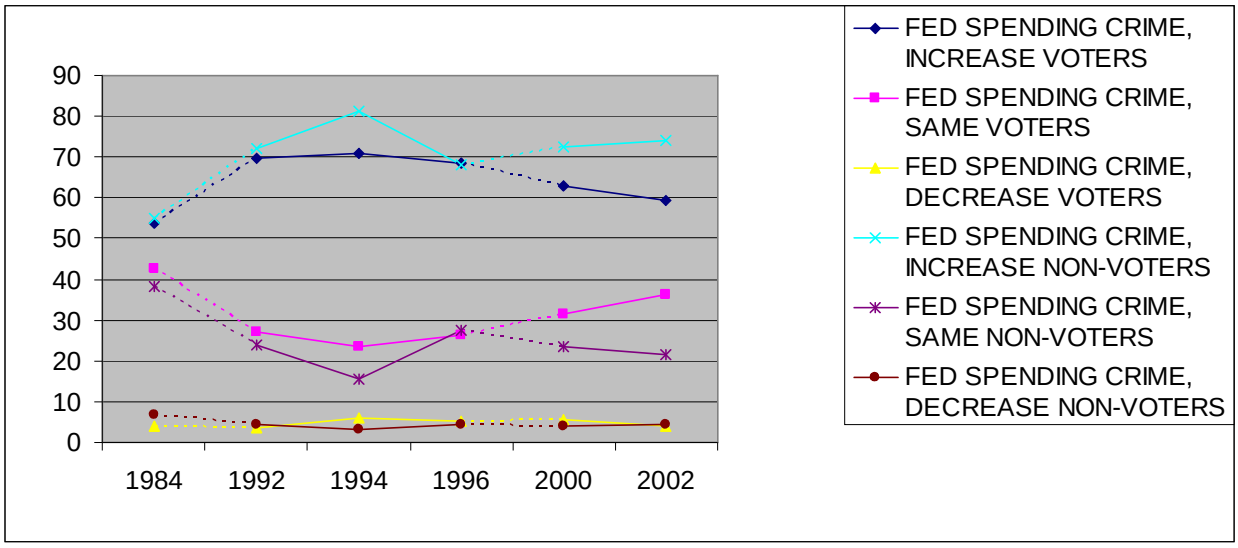


Figure 2.A22. Federal spending on the poor, 1992–2002.

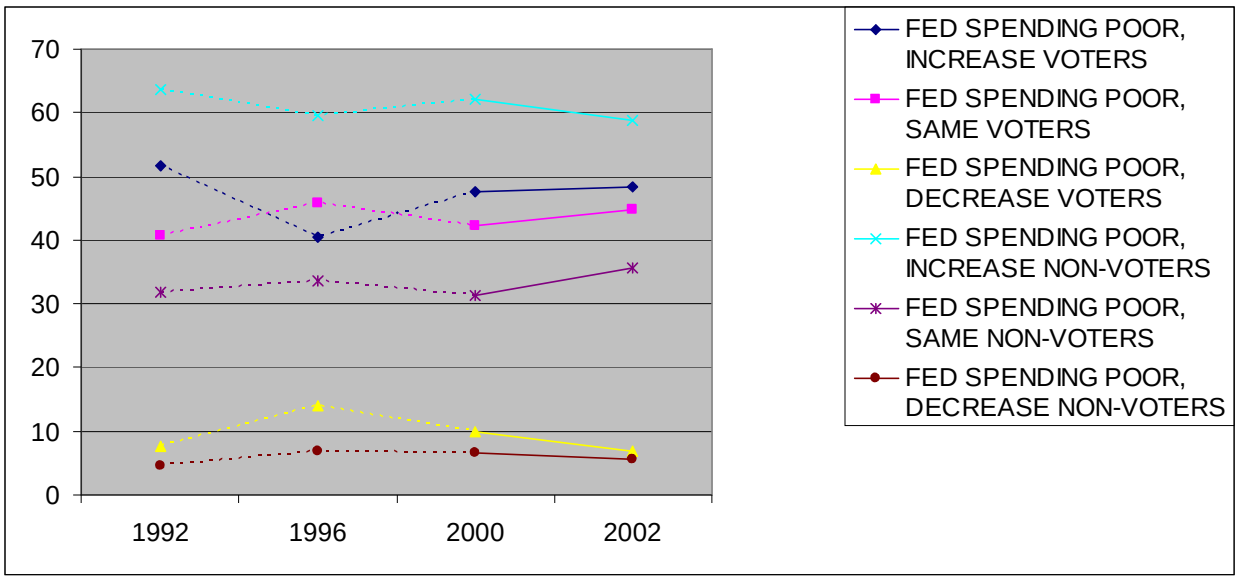


Figure 2.A23. Federal spending on schools, 1984–2002.

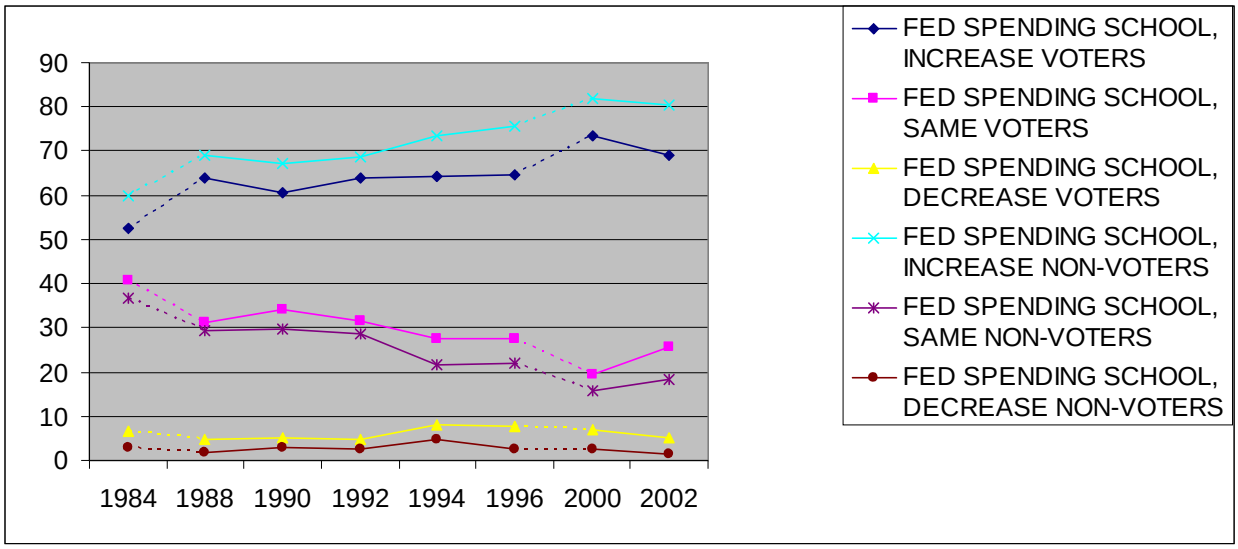


Figure 2.A24. Federal spending on food stamps, 1984–2000.

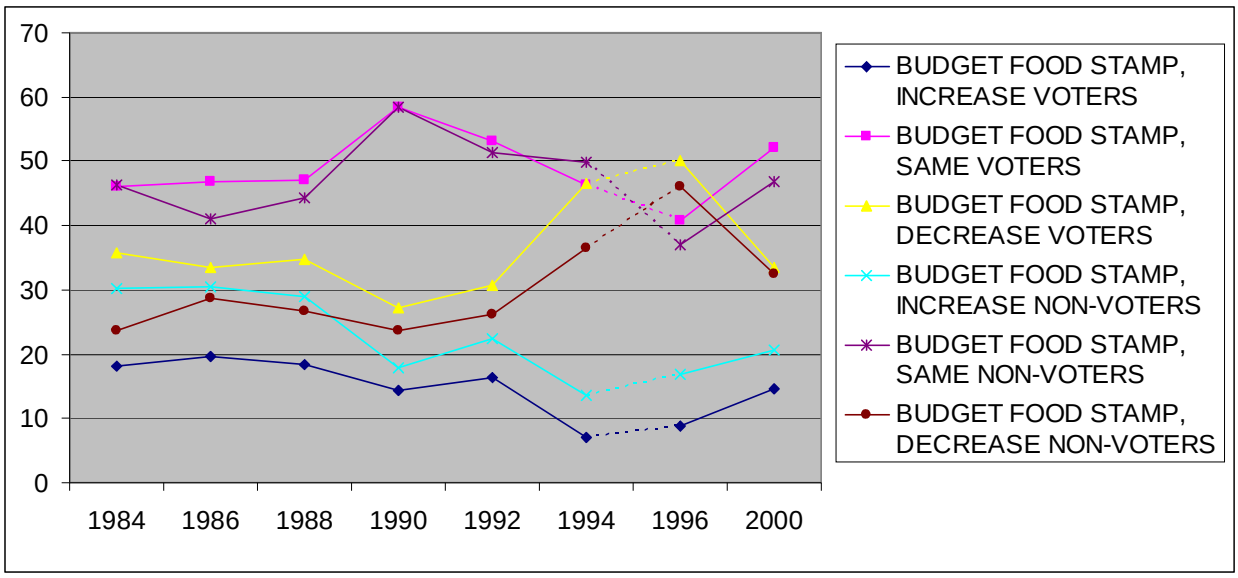


Figure 2.A25. Federal spending on welfare, 1972–2002.

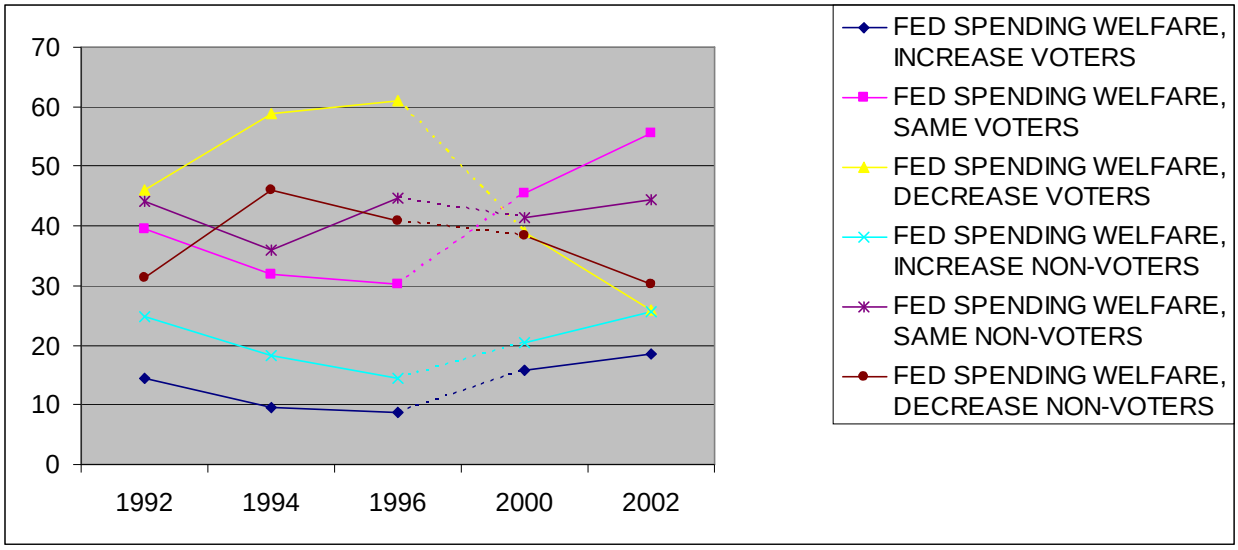


Figure 2.A26. Federal budget on the environment, 1984–2002.

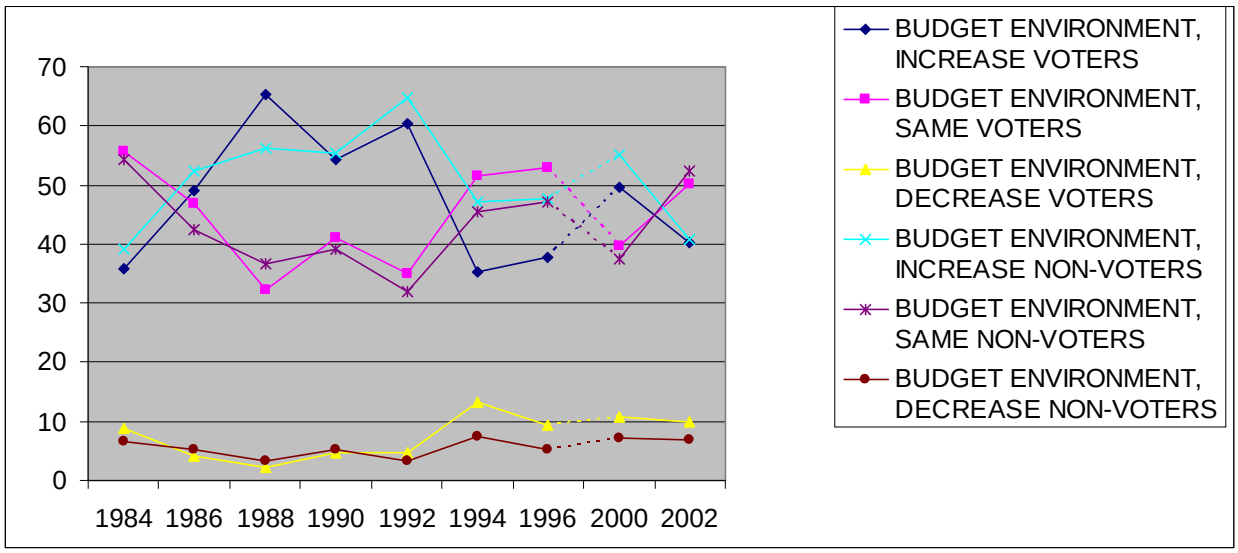
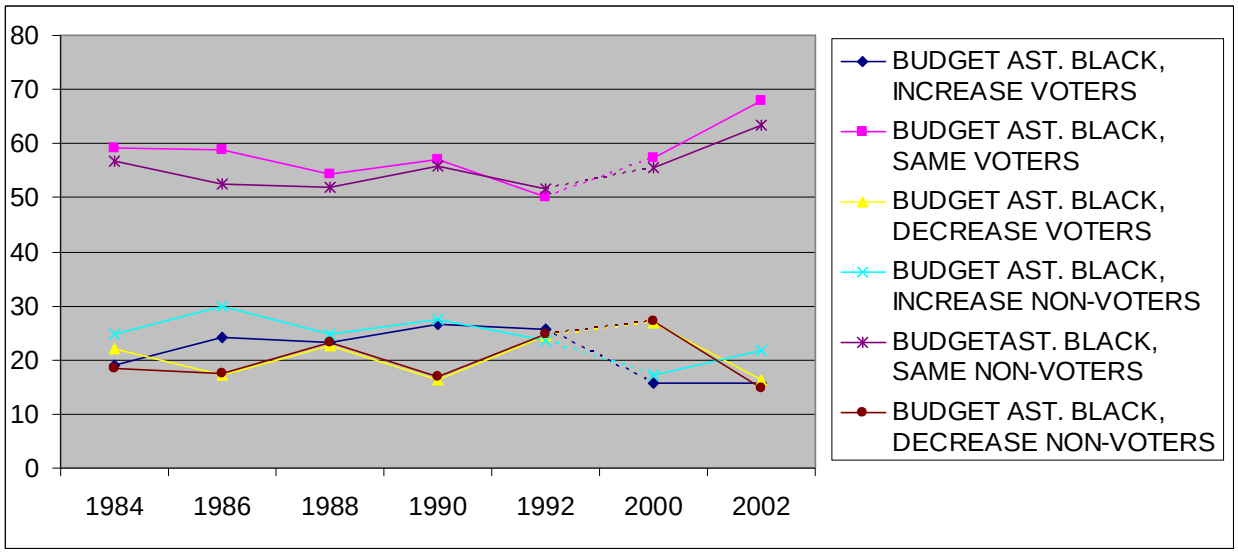


Figure 2.A27. Budget for assistance to African-Americans, 1984–2002.



Appendix 2.B

Notes on PSW Methodology

Let us assume the following distribution in an hypothetical population of say 200 inhabitants, where 55 percent are considered to be rich, and 45 percent are considered to be poor. Let us assume that there was an election where two candidates ran (one liberal and one conservative). Now suppose that we know the true distribution of preferences according to wealth, that is:

	Conservative (thousands)	Liberal (thousands)
Rich	80	10
Poor	20	90

But that the actual distribution of votes is:

	Conservative (thousands)	Liberal (thousands)
Rich	72	9
Poor	10	45

The probability of voting is .9 for the wealthiest segment and .5 for the lowest segment of the population. Now, if we sampled the whole population of voters in order to determine the true population preference, we would have to weight according to the inverse of the probability of selection (here 1.11 and 2). Let us assume that our sampling is not random, and that the probability of selection for the wealthiest segment is one, and the probability of selection for the poorest is .8, our sampled distribution of voters would look like this:

	Conservative (thousands)	Liberal (thousands)
Rich	72	9
Poor	8	36

Now in order to find out the true population preference we would have to weight this data by participation and selection probability.

	Conservative (thousands)	Liberal (thousands)
Rich	72*1.11	9*1.11
Poor	8*1.25*2	36*1.25*2

Without loss of generality, we need to assume in our study that the probability of being in the exit poll needs to be weighted by the probability of voting. Hence we need to multiply the inverse of the probability of being selected in the exit poll (different in each state) by the inverse of the probability of participating in the election. This double weighting scheme will allow to backtrack toward the *true* population preferences. This is the method we employ in the analysis.

CHAPTER 3

Effect of Incumbency, Challenger Quality, and Tenure Length on House Election Turnout

Representative democracy, as conceived of by Schumpeter (1942), rests on an electoral process in which legislators acquire power by means of a competitive struggle for the people's vote. The study of congressional careers is, therefore, central to our understanding of democracy and democratic institutions. In the United States, Congress scholars have generally focused their attention on factors associated with the election or reelection of legislators. But the recent reduction in turnover of elected officials that was identified by Polsby (1968) almost forty years ago together with the development of lawmaking as a career, has pushed political scientists to develop theories to explain legislative behavior.[1]

With the reelection rate of House incumbents having increased from 94% between 1952 and 1980 to 97% between 1982 and 2000, and finally to 99% in 2002 and 2004, it comes as no surprise that incumbency has been extensively investigated.[2] High reelection rates are also found in the Senate, where incumbents have won on average more than 80% of their bids between 1914 and 2002 (Gowrisankaran, Mitchell, & Moro, 2005). Abramowitz, Alexander and Gunning (2006) have calculated that in 2004, more than 172 of the winning candidates in the House had no major party opposition or had a margin of victory of at least 40%. Today, the risk of losing one's congressional seat in an election is very low. A congressional career offers more job

[1] See also Fiorina, Rohde, and Wissel (1975) for earlier identification of the reduction in turnover in Congress.

[2] Numbers are drawn from Abramowitz, Alexander and Gunning (2006). We only take into account incumbents who have made the decision to run for re-election.

security than large corporations can offer CEOs, or universities can offer faculty members through tenure (Ehrenberg, 2003).

In trying to identify the causes of the growing incumbency advantage, scholars have failed to analyze its effects, especially on political participation and legislative responsiveness. Since reelection rates are so high in the House and Senate, it is important to understand both the motivations that lead lawmakers to voluntarily exit from Congress and the strategies that are used by members of Congress (MCs) to guarantee high reelection rates. And because the 2002 and 2004 House elections accounted for only one percent of involuntary departures (there is virtually no variance), it seems important to orient studies toward the analysis of careers and the causes of the incumbency advantage rather than simply focusing on election results.[3]

Some scholars (e.g. Zaller in 1998) believe that the high retention rates are basically rewards for excellent performance. A more plausible and somewhat complementary explanation may be that incumbents have become increasingly insulated from electoral competition. Many factors have been proposed as explanations for the growing incumbency advantage in the House. Aside from the traditional institutional benefits of office, such as franking privilege, pork-barrel projects, and the increase in constituency services (Fenno, 1978; Fiorina, 1974; Mayhew, 1974), other factors, such as redistricting (Tufte, 1973; Lyons & Galderisi, 1995), partisan polarization (Black & Black, 2002), weakening partisan tides (Ferejohn, 1977), and campaign finance laws (Kazee, 1983; Abramowitz, 1991; Cox & Katz, 1996; Levitt & Wolfram, 1997; Campbell, 2003)

[3] See Diermeier, Keane, and Merlo (2005) for a detailed analysis of congressional careers.

have been proposed to explain the growth in incumbents' vote margins and reelection rates.[4] I will not summarize the extensive Congress-focused literature on incumbency; instead I will explore a few of the more unconventional accounts of why there has been a decrease in turnover in Congress.

Following Miller (1999) and Trounstine (2006), I posit that legislators have an incentive to behave like monopolistic firms. That is, in order to protect their seats lawmakers seek to construct barriers to entry in the political "market" of their district. This type of strategic behavior both increases the cost of voter participation and decreases the size of the electorates. Therefore, one would expect the presence of an incumbent to reduce turnout. Legislators have an incentive to keep the size of the reelection constituency low so as to maximize their chance of reelection while minimizing the costs of campaigning. By keeping the cost of participation high, an incumbent will reach a greater number of core supporters with his campaign dollars since the electorate will be smaller. One would also expect lower turnout rates to produce a greater level of electoral support for incumbents, especially if we consider that quality challengers tend to avoid costly electoral contests. Consequently, it makes sense to assume that lower participation rates in congressional elections are both a cause and an effect of the House incumbency advantage. Most incumbents have an interest in keeping the level of political participation low since it increases their electoral support (for similar argument see DeNardo, 1980, and Converse, 1966).

[4] See Jacobson (2005) for a review, and Ansolabehere, Snyder, and Stewart (2000); Cox and Katz (1996); Abramowitz, Alexander and Gumming (2006); Gelman and King (1994); and Levitt and Wolfram (1997) for estimations of the incumbency advantage.

If we accept that incumbency increases voter participation costs in elections, then we can understand how incumbency tends to reduce turnout. In an open-seat election, both candidates and parties have to spend more. This, of course, affects the campaign dynamic, increasing media coverage and publicity. It also raises the information level of the electorate and reduces the overall cost of political participation because of the need to mobilize voters. My theory assumes that there is an uneven distribution of the costs of voter participation in the electorate. This cost, and by extension turnout, is ultimately determined by the type of campaign (incumbent versus open-seat), the quality of the challenger, the level of campaign spending, and the tenure length of the incumbent.

Surprisingly, the preceding thesis has never been fully tested. Most research on turnout in congressional elections has focused on the effects of higher participation rates in presidential elections or on the relationship between partisan outcomes and higher voting rates.[5] The conventional wisdom has always been that an increase in turnout would favor the Democrats. But studies by DeNardo (1980), Nagel and McNulty (1996), Citrin, Schickler, and Sides (2003), and my own work in chapter 2, have demonstrated that Republicans can also benefit from a higher turnout in some congressional races. In the aggregate, these analyses show that an increase in participation can actually be detrimental to incumbent candidates since independents and sporadic voters have a higher likelihood to cross party lines and support the minority party on Election Day. Consequently, greater turnout rates could actually mean fewer votes for incumbents, whether Democratic or Republican. Likewise, a decrease in the level of participation may also translate into an increase in support for the incumbent.

[5] One of the most widely tested theory relates to the surge-and-decline hypothesis, which holds that the party of the president will lose support during the midterm election, e.g., Campbell (1985).

The bulk of the existing empirical work that looks at the relationship between incumbency and turnout demonstrates that incumbents are more likely to be elected when participation is low, regardless of party affiliation (Caldeira, Patterson, & Markko, 1985; Jackson, 1996; Carson, Finocchiaro, Leoni, & Rhode, 2001, Converse, 1966). Since we know that the number of terms served by incumbents affects the probability of reelection to the House (e.g., Diermeier, Keane, & Merlo, 2005; Kiewiet & Zeng, 1993; Groseclose & Krehbiel, 1994; Hall & Van Houweling, 1995; Groseclose & Milyo, 1999), we should also expect tenure length to affect turnout in congressional elections. We should find that the presence of an entrenched incumbent who has served many terms significantly increases the cost of entry for challengers, especially for quality challengers who have higher opportunity costs. This will ultimately affect the level of electoral competitiveness and participation in the district.

Previous studies of congressional elections have never directly tested whether incumbency and tenure length affects turnout and electoral support. In this chapter I aim to rectify this omission. This chapter is organized as follows. First, I present a brief review of the industrial organization (IO) literature on congressional elections. Second, I introduce a theory of incumbency, campaign spending, and turnout. Third, I test the turnout and incumbency theory. Finally, I draw some conclusions.

3.1. The Benefits of the IO Approach in the Study of Incumbency

Conceptualizing the whole electoral transaction in strictly economic terms has many advantages. By doing so, we can start thinking in terms of firm entry barriers, which are widely discussed in the IO literature. Entry barriers in IO are said to arise from the incumbent firms' past expenditure

on such things as advertising, research, reputation, and goodwill (Lott, 1986; Salop, 1979). These factors create barriers because they entail different costs of production for both the entrant and the monopolistic firm dominating the market. In this context, a startup firm faces a higher cost in entering the market, which can contribute to the existence of a monopoly.

In political markets, barriers to entry are associated with the so-called incumbency advantage of legislators. These political barriers to entry may take many forms. They can be associated with manipulations of electoral rules, such as redistricting, voting and registration requirements, and campaign finance laws (such as limits on donations); or they can refer to specific investments, such as war-chests, advertisement in the district, or constituency services. When the size of entry barriers to the electoral process is too great (e.g., the torture and execution of the opposition), the monopoly becomes permanent and the political institutions cease to be democratic (Mulligan & Tsui, 2005). Hence, the degree of political competitiveness, or uncompetitiveness, in the selection of political leaders becomes central in assessing the state of a democratic regime.

The IO literature holds that an incumbent firm can always gain competitive benefits from informational asymmetry. That is, an incumbent firm has an advantage in informational precision of the supply and demand conditions associated with the market. This informational advantage means that an entrant firm must surpass some nonzero threshold in terms of attaining a lower expected marginal cost before it will replace the incumbent firm in equilibrium (Harstad & Crew, 1999). In congressional elections, incumbent candidates have an informational advantage that stems from the fact that they have an existing reputation record in their constituency. Incumbents make themselves more visible through the media, but they are also in a position to

directly communicate with the voters through their control of government activities. For the challenger, the cost of disseminating information during a campaign is higher since most of the readily available political knowledge in the electorate is about the incumbent. In keeping the cost of entry high enough to cause negative utility for a challenger, an incumbent can successfully deter any serious competitor from entering the race, and maintain a legislative quasi monopoly (for a similar argument, see Goodliffe, 2004; Epstein & Zemsky, 1995; Banks & Kiewiet, 1989).

If we consider the utility of holding office to be a function of the probability of winning the election and the costs of campaigning, we should find that strong incumbents can deter entry for almost all but the weakest challengers. The preceding relationship holds because high quality challengers have greater outside opportunity costs; e.g., they may have to leave a lower elected office or a successful career in the private or public sector (Jacobson & Kernell, 1983; Bond, Covington, & Fleisher, 1985; Bond, Fleisher, & Talbert, 1997).

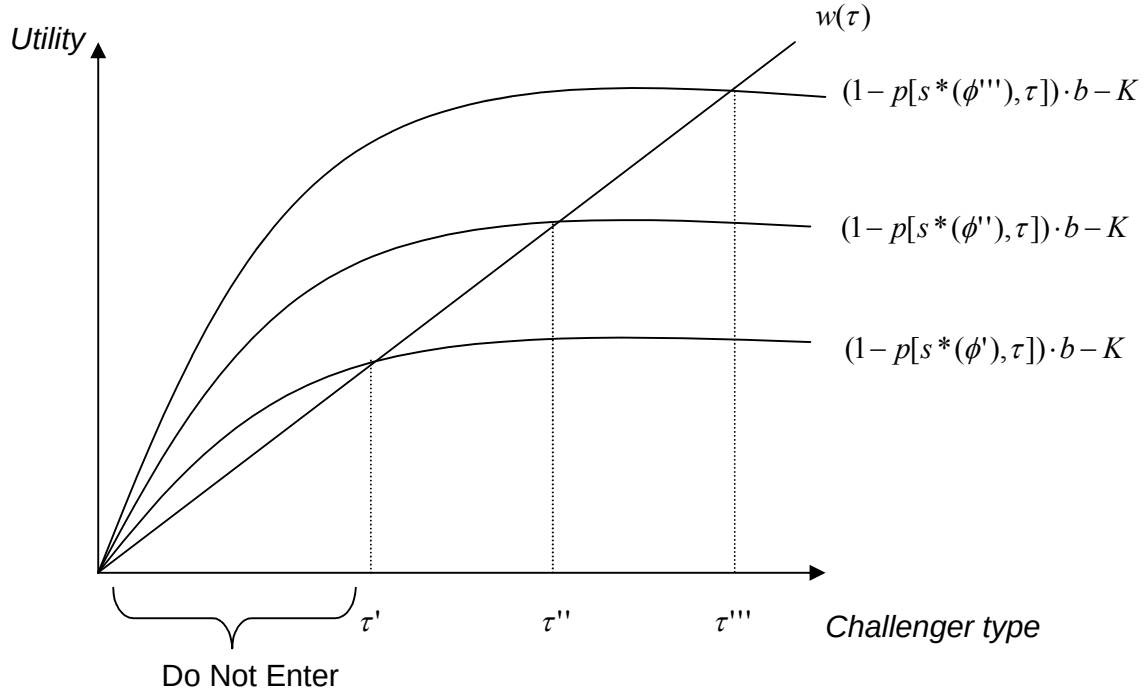
Since more than 90% of incumbents running for reelection between 1954 and 2004 won their races, we can safely assume that a fair number of quality challengers opt to stay out of uncompetitive contests.[6] The fact that some incumbents are not even challenged in the general congressional elections—presumably because the expected costs associated with campaigning entails negative utility for all potential entrants—demonstrates the usefulness of deterrence as a campaign strategy. More than 14% of the returning incumbents were unopposed in general elections between 1970 and 1998, and only 17% of all congressional races were contested by

[6] Data from Abramson, Aldrich, & Rhode (2006).

quality challengers during the same period.[7] In short, it appears that the growth in the scarcity of the supply of quality challengers has been one of the major contributing trends responsible for the increasing retention rates of incumbents in the House. In a world where all incumbents would have to face quality candidates, we should expect to find an overall greater level of political participation and a lower level of electoral support for the incumbent candidate.

The intuition for the interpretation of the relationship between candidate utility and the cost of entry can be seen in Figure 3.1.

Figure 3.1. Outside utility by challenger type.



[7] Compiled by the author and based on the data of Jacobson (1999) presented in Figure 9 of his *Political Science Quarterly* article, and on the data of Ornstein, Mann, and Malbin (2002) presented in Table 2.12 of their book.

This figure plots the different levels of utility for a given challenger as a function of her probability of winning. The *x* axis corresponds to a continuum of challenger types while the *y* axis measures the challenger's utility. As we move away from the origin, the challenger quality increases (i.e., $\tau' < \tau'' < \tau'''$). The vector $w(\tau)$ delimits a threshold that represents the outside opportunity costs for challengers. Whenever the expected utility of winning an election is greater than $w(\tau)$ the challenger will enter the race. In the plot, this utility function is represented by the probability function $[1 - \Pr(s(\phi), \tau) - K] \cdot b$, where b is the utility of holding office; τ is the challenger quality; $s(\phi)$ is the incumbent's level of spending in the campaign; and *K* is the cost of entry.

The figure demonstrates that as the probability of winning increases, or as a challenger faces a weaker incumbent (from ϕ' to ϕ''' in Figure 1), the quality of the entrant increases from τ' to τ''. Baskan and Godbout (2006) formally derive the preceding assumptions. The authors demonstrate that strong incumbents will face only weak challengers. Baskan and Godbout also show that strong challengers will only enter an electoral contest when the expected utility of winning is greater than the expected outside opportunity costs. Finally, the authors demonstrate in a basic coordination game that candidates in open seat elections will maximize their expected utility by both increasing spending. Essentially, their coordination game model predicts that a low anticipated level of campaign spending ($s(\phi)$) will encourage strong candidates to challenge weak incumbents (see Jacobson, 2004 for a similar argument). Since this parameter is tied to the probability of success of the incumbent candidate, we should find there to be higher spending in both highly contested and open seat elections.

The positive relationship between campaign spending and the closeness of a congressional race is well established in the literature. When looking at the distribution of campaign spending and electoral vulnerability between 1972 and 2002, Jacobson (2004) found, for example, that available funds have become increasingly concentrated in marginal districts and open seat elections. Unlike incumbents who generally engage in more campaign spending when they expect their vote share to be low, the likelihood of success for challengers is directly related to their ability to raise campaign money. Money raised by nonincumbents allows them to buy the attention and recognition necessary to wage a successful campaign and neutralize the incumbents' advantage. Without a well organized campaign, the chances of off-setting an incumbent remain very low.

In retrospect, the principal consequence of the decline in the number of incumbents being challenged by experienced candidates has been a reduction in the number of competitive House elections. [8] An additional consequence of this trend, which follows directly from the previous one, has been the reduction of campaign spending in congressional races where incumbents have been expected to win with a comfortable margin. Both trends have served to insulate a large number of incumbents from electoral competition. In return, this insulation has created an important number of congressional legislative quasi monopolies. Since uncompetitive electoral markets are most likely to affect the turnout rate of undecided, sporadic, and independent voters, we should expect both the lack of quality candidates and the lack of campaign spending to have a

[8] Between 1970 and 2000, 75% of incumbents running for reelection won with more than 60% of the vote. Of course, we can also think that part of the reason why incumbents are being challenged by quality candidates is explained by the reduction of competitive electoral districts. There is a question of reciprocal causation here.

depressive effect on electoral participation.[9] The previous conjectures form the core of my theory on incumbency and turnout. This theory links candidates' entry decisions to campaign spending, electoral support, and turnout.

To sum up, I maintain that the monopolistic nature of many congressional elections decreases electoral participation. My contention here is that incumbency and the lack of electoral competition raise the cost of participation for the electorate and reduces the overall level of electoral competitiveness and political participation.

3.2. A Theory of Legislative Elections

I start from the premise that politicians, like economic agents, are rational actors. I should find that whenever a candidate makes the decision to enter an electoral contest his or her expected utility gain from winning office is greater than the expected costs of campaigning. This should be true for both incumbents and challengers. If the utility gain from winning office is lower than a candidate's outside opportunity costs, he or she can be expected to abstain from entering the race.

Competition theory in the IO literature stipulates that when a firm in a monopolistic market raises prices above a certain level in order to reap monopoly profits, the consumers will buy less of the product, and less will be produced. So society as a whole will be worse off. In the political market, unless an incumbent goes unchallenged, the electors always have the option of voting against the incumbent legislator. They can also exit the political market by simply deciding not to participate in the election. In effect, the electorate may have to pay the costs of

[9] It is a well established fact in the political behavior literature that independents have a lower likelihood of voting (e.g. Rosenstone & Hansen, 1993; Green, Palmquist, & Schickler, 2002).

implementing different public policies, but it does not have to face the costs associated with the act of voting—especially if there is no serious competitive alternative to replace the incumbent candidate. Trounstine (2006) has a similar argument about municipal elections and turnout. She claims that "Like business firms operating in a highly competitive market, political organizations seek to create a monopoly [in municipal elections]" (p. 890). And likewise, she argues that this will reduce the level of voter participation in return.

Like many before me, I argue that voting is a costly activity, both in terms of opportunity and information. Of course, the cost of participation is not constant across the electorate (Brady, Scholzman, & Verba, 1999; Whielhouwer, 2003). Some voters face higher costs than others. For example, voters who are highly informed about the candidates, and who are already registered will have a lower cost of participation than uninformed and unregistered voters. The same is true for partisan or party supporters. This group usually faces a lower cost of participation than undecided or independent voters (Rosenstone & Hansen, 1993). From an incumbent's perspective, mobilizing partisan supporters makes more sense because that type of voter needs only to be reminded to vote on Election Day (partisans are unlikely to defect and support the challenger). Mobilizing a broader group of potential voters may prove to be a riskier strategy, especially if the expected rate of defection among undecided voters favors the opposition (DeNardo, 1980, Goldstein and Ridout, 2002, Holbrook and McClurg, 2005).[10]

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[10] This depends on the size of the incumbent's base and their turnout rate. If we assume for example that N is the total number of votes found in a congressional district for a specific election, then $N = E_I + E_C + E_U$ is the total Voting Age Population (VAP) in the district. Here, E_I is the sum of Incumbent supporters, E_C is the sum of Challenger supporters, and E_U is the sum of Undecided constituents. Each of these group should have a different turnout rate: $\rho_{I,C,U} = [0,1]$, where $\rho_U < \rho_{I,C}$. Turnout among each specific group of supporters will equal $E_I \cdot \rho_I = V_I$, where V_I is the votes for the

For a challenger, getting the message to voters will usually be the costlier activity. In order to have a chance of winning the election, challengers not only have to mobilize their base, they usually have to convince a sufficiently large proportion of undecided voters, or even voters who lean toward the incumbent, to support their candidacy. And because very few challengers have at their disposal the level of resources to wage this type of competitive campaign, most voters in a typical race will remain unaware of the positions, or even the existence, of the challenging candidate.

Considering that an increase in the intensity of campaigns has a much greater chance of influencing the decision to participate in the election, we should expect hard profiled and hard-fought races to reduce the costs of political participation and subsequently increase turnout (Holbrook & McClurg, 2005; Jackson, 2002; Jackson, 1996; Rosenstone & Hansen, 1993; Gilliam, 1985; Caldeira, Patterson, & Markko, 1985). If we consider that an incumbent's probability of reelection is partially determined by her quality and campaign spending, we should also expect to find that tenure length will have affected entry decisions of qualified challengers.

Just as in the case of a monopolistic firm that has been dominating a market for an extended period, we would expect that an entrenched incumbent who has served many terms to

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incumbent, $\mathrm{E}_C \cdot \rho_C = V_C$, V_C is the votes for the challenger, $\mathrm{E}_U \cdot \rho_U = V_U$, and V_U is the total number of votes cast by undecided voters. So as long as the incumbent expect the total vote share to be greater than the sum of the undecided and opposition voters (i.e., $[\mathrm{E}_I \cdot \rho_I] > [\mathrm{E}_C \cdot \rho_C] + [\mathrm{E}_U \cdot \rho_U]$) then he or she will only have to worry about mobilizing a sufficiently large number of core supporters for this inequality to hold. However, when the total number of undecided voters supporting the challenger (γ) is greater than $(1-\gamma)$, the incumbent will have to mobilize an additional group of undecided voters for this inequality to hold: $[\mathrm{E}_I \cdot \rho_I] + [\mathrm{E}_U \cdot \gamma \cdot \rho_U] > [\mathrm{E}_C \cdot \rho_C] + [\mathrm{E}_U \cdot (1-\gamma) \cdot \rho_U]$. Unless the size of the challenger supporters group is larger than the incumbent's base (which is highly unlikely), the winning incumbent will have to convince a sufficiently large proportion of undecided voters to turnout on Election Day, and to support their candidacy.

significantly increase the costs of entry for any type of challenger (Geroski, 1995). Jacobson (2004) points out that first-term incumbents unusually attract vigorous opposition. This is because their electoral hold on the district has yet to develop (see also Fenno, 1978). This also explains why recently elected incumbents work hard to expand their base and to increase the size of their electoral margins. One of the consequences of this increase in the probability of winning for the career incumbent will be to reduce the quality of the entering challengers. This will subsequently raise the cost of political participation in the district.

The distribution of incumbent career length confirms this trend. The data shows that a lot of incumbents only served a limited number of terms. The distribution is positively skewed; the median is at the third term (see figures 3.2 and 3.3).

Figure 3.2. Distribution of the number of terms the incumbents served between 1972 and 2000.

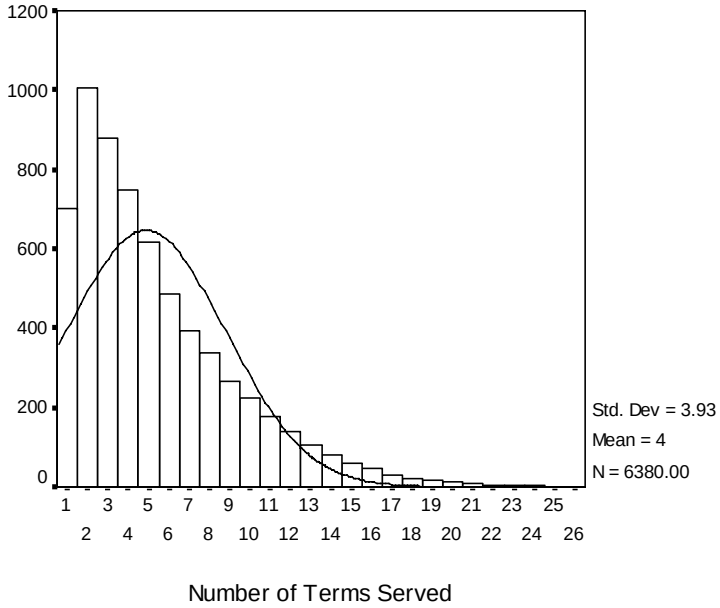
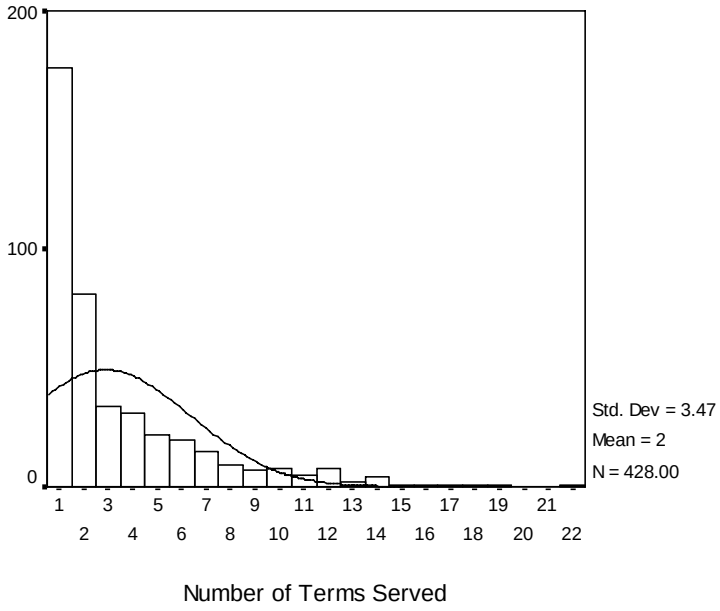


Figure 3.3. Distribution of incumbents who lost a reelection by number of terms served between 1972 and 2000.



These data imply that half the incumbents elected between 1970 and 2000 did not make it past the junior status in the House. Many factors explain this high level of attrition, the primary one being that vulnerable legislators are removed early from office (voluntarily or involuntarily). Consequently, the presence of quality challengers (and therefore turnout) is higher at the beginning of incumbents' careers—particularly because of the inexperienced pool of returning candidates.

Another significant characteristic of the distribution of seniority in the House relates to the proportion of incumbents who have ambition for higher office (such as the Senate). As Diermeier, Keane, and Merlo (2005) indicated in their study of congressional careers, ambitious legislators are more likely to exit the House within the first five terms of their tenure. For example, between 1972 and 1989, 28% of House members who exited Congress voluntarily

either ran for Senate or accepted a federal office or other type of government position.[11] This proportion is even higher today since twenty-eight senators have held prior elected office in the 107th Congress (Ornstein, Mann, & Malbin, 2002). It follows from this seniority distribution that incumbents who make it past their first ten years in the House are more likely to have made the decision to finish out their career in this chamber. That explains why we should observe a second surge in the number of quality challengers as the pool of incumbent career office-holders increases. This expectation is consistent with what Fenno (1978) calls the protectionist phase in an incumbent's career. In effect, after being in office for several consecutive terms, incumbents are said to enter a stage "during which they work to maintain the support they have built over the years but no longer attempt to add to it (Jacobson, 2004: 48)." It is in this period that senior legislators are often accused of being overly involved with the Washington establishment and out of touch with their congressional districts. In addition, a large proportion of these incumbents will also have reached some position of influence in the House, and working their districts will need to be balanced with their ambition in the legislature. Consequently, we should also expect to find that long legislative careers may have a negative influence on electoral support.

To summarize, my theory hypothesizes that the entry decision of quality challengers is a function of some baseline costs and the probability of incumbent reelection. This probability is determined by both the quality of the candidates and the fundraising ability of the incumbent. I also assume that the probability of reelection of an incumbent is affected by his or her tenure length. I believe that recently elected incumbents will have a lower probability of returning to office. The same is true for incumbents who have been in office for several consecutive terms

[11] From McKibbin's dataset (1992).

(i.e., more than 10 years). Since campaigning is a costly activity, we should only expect hard-profiled and hard-fought races to be competitive. This type of campaigning will reduce the costs of political participation, increase turnout, and reduce the incumbent's vote share. We will test the following conjectures in the remaining sections of this chapter.

3.3. Research Hypotheses

The following analysis focuses on estimating the different distributions of voter turnout and evaluating how these interact with the strategic campaign decisions of candidates. I look at elections in the House between 1972 and 2000. In this chapter, I test the following four hypotheses:

Hypothesis 1: *Turnout in congressional elections is lower when there is an incumbent.*
Hypothesis 2: *The longer an incumbent has served, the lower turnout will be.*
Hypothesis 3: *Greater levels of campaign spending and the presence of a quality challenger increase turnout.*
Hypothesis 4: *When turnout is high, the incumbent is less likely to win.*

The first hypothesis is that incumbency decreases turnout in congressional elections. I also hypothesize that this effect is greater when the number of terms served increases. The third hypothesis is that an increase in campaign spending and the presence of quality challenger raises the level of political participation in congressional elections. Finally, I hypothesize that higher turnout reduces the electoral support for incumbent candidates and decreases their probability of reelection.

Before proceeding with the empirical analysis, it is important to underline one of the most important problems facing a majority of the studies that analyze congressional elections. This limitation is specifically related to the reciprocal causal relationship between campaign spending and electoral success. Most electoral studies of Congress note that spending by challengers has a positive and significant effect on their vote share. On the other hand, the marginal effects of spending by the incumbent appear to be lower, or even negative. This counterintuitive finding is explained by the fact that incumbents generally increase spending only when they feel seriously threatened by a challenger. When the vote for the incumbent is expected to be high, less campaign money will be raised and spent (Jacobson, 1980, 2004; Erikson & Palfrey, 1998). Consequently, it remains difficult to disentangle the specific effect of campaign spending by challengers from the incumbent's reelection prospects, since the amount of money raised is also a function of the latter.

A similar type of reciprocal causation is to be expected in a study that looks at the relationship between turnout and incumbency. Higher turnout rates should help the challenger's electoral prospects since higher spending is more likely to positively influence participation. One could also imagine that weaker incumbents would attract stronger challengers with the ability to raise more campaign money. The question remains whether an increase in turnout is the consequence of higher spending or just a consequence of a more competitive race with a quality challenger.

Any study of the effects of incumbency, challenger quality, and turnout will be affected by this reciprocal causation. This one is no exception. I expect that more campaign spending will translate into fewer votes for the incumbent. However, I am not interested in determining

whether an increase in spending will affect the vote share of the incumbent or the challenger. Rather, this chapter aims to determine how incumbency, tenure length, and spending affect turnout. Like Coates (1998), I assume that campaign expenditure simply induces voters to go to the polls. In other words, campaign spending affects the participation decision. Money mobilizes voters. We should find this to be true for both incumbents and challengers. Hence, higher levels of campaign contributions will lead to higher turnout. The incumbent campaign spending effect will be positive in that way.

The same cannot be said about the relationship between turnout and the incumbent's vote margin. In effect, both turnout and vote share can be simultaneously influenced by the threat posed by a challenger and the quality of the incumbent. A higher level of turnout may indeed affect the probability of reelection for the incumbent. But it is likely that this probability will also depend inter alia on the level of turnout in the coming election. Thus, it is necessary to use a method to control for this simultaneous relationship. I proceed to do so in the next section by using two-stage least squares estimations.

3.4. Empirical Evidence

In order to test the preceding hypotheses, the following analysis uses a novel dataset for the years 1972–2000 that contains House election results, congressional tenure length, participation rates, as well as a series of socio-demographic control variables.[12] The dataset combines updated variables from Canes-Wrone, Brady, and Cogan (2002); Adler (2002); Jacobson and Snyder (for the challenger quality variable which was included in the Canes-Wrone et al. dataset). In order to

[12] We have excluded Louisiana from the analysis because of their special election process. See Jenkins, Crespin, and Carson (2005), and Rothenberg and Sanders (2000) for similar procedure.

get the necessary information about congressional careers, I have also used Keith Poole's DW-NOMINATE data file, McKibbin's Biographical Characteristics of members of the United States Congress (1789–1979), the Election Statistics from the United States Office of the Clerk (1970–2000), and the online Biographical Directory of the United States Congress.

Formally, I estimate four equations. For each congressional district *cd,* in election *t,* the turnout rate and incumbent vote share are said to be a function of the following equations:

(1)

$$Turnout_{cd,t} = \beta_0 + \beta_1 Open_{cd,t} + \beta_2 Freshman_{cd,t} + \beta_3 Quality_{cd,t} + \beta_4 Inc\ Spend_{cd,t} + \beta_5 Chal\ Spend_{cd,t} + \beta_6 Inc\ Dem_{cd,t} + \beta_7 Election\ (dummy)_{cd,t} + \beta_8 lagvote_{cd,t-1} + \beta_9 lagturnout_{cd,t-1} + \beta_x Sociodem_{cd,t} + \varepsilon_{cd,t}$$

The second equation estimates the effect of the number of terms served on turnout:

(2) $$Turnout_{cd,t} = \beta_0 + \beta_1 Nb\ Terms_{cd,t}\ (Nb\ Terms^2, Nb\ Terms^3) + \beta_2 Freshman_{cd,t} + \beta_3 Quality_{cd,t} + \beta_4 Inc\ Spend_{cd,t} + \beta_5 Chal\ Spend_{cd,t} + \beta_6 Inc\ Dem_{cd,t} + \beta_7 Election\ (dummy)_{cd,t} + \beta_8 lagvote_{cd,t-1} + \beta_9 lagturnout_{cd,t-1} + \beta_x Sociodem_{cd,t} + \varepsilon_{cd,t}$$

The third equation estimates the vote share for each incumbent who faces a major party challenger in election *t:*

(3) $$IncVote_{cd,t} = \beta_0 + \beta_1 Turnout_{cd,t} + \beta_2 Nb\ Terms_{cd,t}\ (Nb\ Terms^2, Nb\ Terms^3) + \beta_3 Freshman_{cd,t} + \beta_4 Quality_{cd,t} + \beta_5 Inc\ Spend_{cd,t} + \beta_6 Chal\ Spend_{cd,t} + \beta_7 Inc\ Dem_{cd,t} + \beta_8 Election\ (dummy)_{cd,t} + \beta_9 lagvote_{cd,t-1} + \beta_{10} lagturnout_{cd,t-1} + \beta_x Sociodem_{cd,t} + \varepsilon_{cd,t}$$

Finally, the fourth equation calculates the effect of turnout and tenure length on the probability of reelection:

(4) $$\Phi(Elected)_{cd,t} = \beta_0 + \beta_1 Turnout_{cd,t} + \beta_2 Nb\ Terms_{cd,t}\ (Nb\ Terms^2, Nb\ Terms^3) + \beta_3 Quality_{cd,t} + \beta_4 Inc\ Spend_{cd,t} + \beta_5 Chal\ Spend_{cd,t} + \beta_6 Inc\ Dem_{cd,t} + \beta_7 Election\ (dummy)_{cd,t} + \beta_8 lagvote_{cd,t-1} + \beta_9 lagturnout_{cd,t-1} + \varepsilon_{cd,t}$$

The measurement of each variable I use in the model is as follows:

Turnout: In order to obtain the turnout rate in each congressional district for every election between 1972 and 2000 I divided the total number of votes counted in an election (as listed by the U.S. Office of the Clerk) by the voting age population of the district. Estimating the voting age population in each congressional district is not an easy task. Especially if we consider that an important proportion of district boundaries change following each decennial redistricting (and also following mid-decade court ordered redistricting). The Bureau of the Census offers an estimate of the voting age population at the beginning of a decade in each congressional district. The voting age population data associated with the new census generally does not correspond exactly to the boundaries of the old congressional district. To account for this inconsistency, I have estimated the statewide growth rate of the voting age population between censuses, and have adjusted the voting age population of every congressional district for the five elections following the first measure of the voting age population in each census.[13] When possible, I have also re-estimated the voting age population for each congressional district in which a court-ordered redistricting occurred between censuses. In the period covered by this study, our mean turnout for all congressional elections is 44% of the voting age population (with the lowest observed turnout at 9% and the highest at 90%). The official average turnout rate in federal elections for the same period was

[13]We use linear interpolation. For example, we took the 1970 census measure of the voting age population (VAP) in a congressional district, estimated the growth rate of the statewide population over 18 between 1970 and 1980. Then we used this value to calculate the growth rate of the VAP for any congressional district in the state in the five elections following the census (1972, 1974, 1976, 1978, and 1980). We did the same for 1982–1990 and 1992–2000.

45% according to the Census Bureau. For presidential election years, I calculated the average turnout to be 50% (52.5% according to the Census Bureau); and in midterm elections, to be at 37% (37% according to the Census Bureau).[14]

Nb Terms: The number of terms variable is the total number of consecutive congresses in which the incumbent faces reelection, regardless of district geography. This was done in order to control for the creation or suppression of congressional districts. In other words, when counting the number of terms an incumbent served, I only verified that these terms were served in the same state (regardless of district numbers), and that the congressional service was consecutive (in the House). Members elected in special elections were counted as freshmen in the following general election. Terms served prior to 1972 were also counted. We control for any potential nonlinear effects on the dependent variables by taking the square and the cube of the number of terms served.

Inc Vote: The percentage of the two-party vote received by the incumbent (either Democrat or Republican) in election *t.* In open seat elections, the candidate who shares the party of the previous incumbent is used instead. Third party candidates were excluded.

Φ(Elected): The dependent variable in equation 4, coded 1 if incumbent was re-elected, 0 otherwise. In open seat elections, the incumbent party candidate is coded.

Open: The open seat variable determines whether an incumbent is running in the congressional election. This variable is coded 1 if the district is open, 0 otherwise. Elections where an

[14] I am in the process of computing the exact population shift between censuses in each congressional district according to the specific district boundaries. The numbers do not match exactly because of some missing cases following mid-decade redistricting, and of course variations across the growth rate in districts, which may not necessarily match the statewide growth estimates.

incumbent is re-districted into a new district are not considered to have open seat elections. This variable measures the incumbency effect, which can take many forms. We can think of constituency services, legislative behavior, the implementation of pork barrel projects, and name recognition as characteristics of incumbency which are not measured in our models. The open seat variable accounts for these missing characteristics.

Freshman: Dummy variable, equaling 1 if the House incumbent has just served one term, zero otherwise.

Quality: Challenger quality as reported by the updated Jacobson and Kernell (1983) data. The variable equals 1 if the challenger has held elected office, 0 otherwise.

Inc Spend and *Chal Spend:* These variables measure how much money each candidate spent in the congressional election (as reported by the FEC). These measures are only reported between 1978 and 2000. We have converted the total value of campaign spending to the value of the dollar in the year 2000 (for comparison purposes). In order to account for the fact that candidates are not required to report spending under $5,000, I have assumed, like Canes-Wrone, Brady and Cogan, that each candidate spent at least that much (I did not convert this $5000 value in 2000 dollars).

Inc Dem: Reports whether the previous winner of the election in the district was a Democrat. This variable is coded 1 if the incumbent candidate was a Democrat, 0 otherwise.

Election: Is a series of dummy variables representing election years: 1972 is the baseline in models where there is no campaign spending variable, 1978 when spending is included. We

also indirectly control for presidential campaign effects on turnout with this variable. This is done to account for the higher turnout rates found in presidential election years.

Lagturnout: Reports the turnout rate in the previous election (for the same congressional district). This variable was included to control for the time-series aspect of our dataset. Turnout is likely to be dependent on the previous turnout rate of the district. Consequently, by adding a lagged measure of participation, the model is capable of controlling for the different distributions of participation rates across congressional districts.

Lagvote: Represents the percentage of the two party vote that the incumbent party received in the last election. This measure is used to control for the level of competitiveness in the current election. The closer the vote was in the last election, the more competitive the election is likely to be. Uncontested elections were excluded. We use the previous vote in the old constituency boundary when the district was redrawn.

Sociodem: Corresponds to all the socio-demographic variables included in the model that I believe have a significant effect on turnout in congressional elections. I have selected these variables based on the work of Rosenstone and Hansen (1993) and on the availability of the measures. I include the proportion of the population in the district that is 65 years or older, the percentage of African Americans in the district, the percentage of union members in the state, the percentage of urban residents in the district, the percentage of unemployed workers in the district, the percentage of college educated people in the district, the percentage of homeowners in the district, the median family income in the district (converted in 2000

dollars), and a dummy variable indicating whether the district is in a Southern state.[15] The previous socio-economic data were collected from the Census Bureau; the CQ books on Congressional districts in the 1970s, 1980s, 1990s, and 2000s; and Adler's datasets. Consequently, these data were not updated for every election; unlike the voting age population calculated for each election in all of the congressional districts. I have only updated this data between decades covered by our study. This of course leads to imprecision in the effect of these variables, especially in the later part of the decade. Like Adler, I have accounted for court-ordered mid-decade redistricting whenever the data was available.[16]

3.5. Results on the Turnout and Electoral Margin Models

Table 3.1 describes the parameter estimates of our cofactors on the turnout rate in each congressional election covered by the study. We first analyze a reduced form of equation (1) for the election between 1972 and 2000.

[15] We have also included for the general model which range between 1978 and 2000, a dummy variable controlling for decade effect. The variable is coded 1 if the election was held between 1992–2000, 0 otherwise.

[16] Interested readers should consult Adler (2002) for a detailed description of the data.

Table 3.1

Impact of Incumbency on Turnout in House Congressional Elections 1972-2000: OLS Regressions

	Model 3.1.1	Model 3.1.2[a]	Model 3.1.3 (1978-000)[a]	Model 3.1.4 (1978-000)[a]
Open seat	.015 (.005)***	.017 (.003)***	.023 (.004)***	.024 (.003)***
Freshman	-	-.002 (.003)	-.005 (.003)	-.003 (.003)
Challenger quality	-	.015 (.003)***	.009 (.009)***	.008 (.003)***
Lagged vote	-	-.040 (.011)***	-.009 (.013)	-.009 (.013)
Lagged turnout	-	.760 (.011)***	.754 (.012)***	.607 (.013)***
Incumbent party Democrat	-	-.010 (.002)***	-.008 (.002)***	-.004 (.002)
Incumbent spending (× 1,000,000$)	-	-	.003 (.001)***	.003 (.001)***
Challenger spending (× 1,000,000$)	-	-	.019 (.001)***	.019 (.001)***
Age 65+	-	-	-	.111 (.030)***
African American	-	-	-	-.009 (.009)
Union	-	-	-	.034 (.018)
Urban	-	-	-	-.042 (.005)***
Unemployment	-	-	-	-.235 (.065)***
College	-	-	-	.050 (.014)***
Home owner	-	-	-	.000 (.000)
Median income (× 10,000$)	-	-	-	.008 (.002)***
South	-	-	-	-.035 (.004)***
Constant	.437 (.002)***	-.039 (.064)	-.023 (.012)	.035 (.015)
Number of observations	5708	4447	3711	3560
R-square	.002	.718	.717	.743

Notes: Significance are two tailed tests, *p<.10; **p<.05; ***p<.01. Dollar values are converted in year 2000 currency (baseline). Dependent variable is turnout rate in congressional district. Standard errors are in parentheses.

[a] Dummy variables for election years are not reported. Complete results are available on request.

We find that there is indeed a correlation between incumbency and turnout. In our model, the open seat variable is positive and significant (p=.003, two tailed). The simple bi-variate equation demonstrates that an open seat election increases turnout by 1.5%. This is true for our whole sample, which contains over 5,708 specific House elections. If we look at the three remaining

models, we also find a confirmation of the first hypothesis, which stipulates that incumbency has a negative effect on turnout. This result is robust across specifications, and remains significant, even after we account for numerous control variables, such as campaign spending, closeness of the race, lagged turnout, lagged incumbent support, challenger quality, and socio-demographic cofactors. On average, an open seat election increases political participation by a proportion ranging from 1.5 to 2.4 percentage points. Thus, in every election between 1972 and 2000 (and later 1978–2000 when we account for campaign spending), the presence of an incumbent puts a downward pressure on political participation, ceteris paribus. As these results suggest, the effect of the control variables is consistent with our expectations. Challenger quality and campaign spending all increase turnout in congressional elections. On the other hand, the lagged vote variable, which represents the proportion of vote that the incumbent party received in the last election, has a negative effect on turnout: the higher the winning margin is in the previous election, the lower the turnout will be in the next (but this variable is no longer significant in equations 3.1.3 and 3.1.4). Our model predicts that incumbency will have a negative effect on turnout regardless of district competitiveness, since this variable remains significant after we control for the previous margin of victory of the incumbent party. As Table 3.1 also shows, the lagged turnout variable has a strong positive effect on turnout.

Furthermore, my theory of legislative election stipulates that the presence of a quality challenger, and by extension an increase in campaign spending, would reduce the cost of participation and stimulate electoral turnout (hypothesis 3). The models estimate this specific effect by calculating the effect of campaign spending by both challenger and incumbent, on turnout (equation 3.1.3–3.1.4). If we assume that the challenger spends the mean value of our

sample for her campaign ($171,823 in 2000 dollars), we find that turnout increases by .003 (using the cofactors in model 3.1.4). So assuming that the challenger spends three times that much, turnout increases by roughly 1% (in the sample a little over 11% of all challengers spent that much). The effect of campaign spending for incumbents is still positive, but weaker. When incumbents spend the average amount in their campaign ($542,344 in 2000 dollars), turnout increases by only .002 (using the cofactors in equation 3.1.4). This result confirms that campaign spending has the effect of mobilizing voters for both the incumbent and the challenger. But this effect is stronger for challenger. Given the consistent and significant positive effect of open seat elections and campaign spending on turnout, we can next turn to the question of whether an increase in tenure length reduces political participation. Table 3.2 presents these results.

Table 3.2

Impact of Number of Terms the Incumbent has been in Office on Turnout in House Congressional Elections 1972-2000: OLS Regressions

	Model 3.2.1	Model 3.2.2	Model 3.2.3 (1978-2000)[a]	Model 3,2.4 (1978-2000)[a]
Total number of terms	-.002 (.000)***	-.004 (.001)***	-.005 (.001)***	-.005 (.001)***
Total number of terms squared ($\times 10^{-3}$)	-	.479 (.191)	.573 (.203)***	.563 (.200)***
Total number of terms cubed ($\times 10^{-4}$)	-	-.147 (.074)	-.184 (.077)**	-.184 (.074)***
Challenger quality	-	.013 (.003)***	.007 (.003)***	.006 (.003)
Lagged vote	-	-.033 (.011)***	-.001 (.013)	.000 (.013)
Lagged turnout	-	.763 (.011)***	.760 (.012)***	.611 (.014)***
Incumbent party Democrat	-	-.009 (.002)***	-.007 (.002)***	-.002 (.002)
Incumbent spending (× 1,000,000$)	-	-	.003 (.001)	.003 (.001)
Challenger spending (× 1,000,000$)	-	-	.017 (.003)***	.016 (.003)***
Age 65+	-	-	-	.111 (.030)***
African American	-	-	-	-.012 (.009)
Union	-	-	-	.034 (.018)
Urban	-	-	-	-.043 (.005)***
Unemployment	-	-	-	-.239 (.065)***
College	-	-	-	.051 (.014)***
Home owner	-	-	-	.000 (.000)
Median income (× 10,000$)	-	-	-	.008 (.001)***
South	-	-	-	-.034 (.004)***
Constant	.449 (.004)***	-.039 (.064)	-.020 (.012)	.038 (.015)
Number of observations	5682	4445	3712	3561
R-square	.006	.718	.716	.742

Notes: Significance are two tailed tests, *p<.10; **p<.05 ***; p<.01. Dollar values are converted in year 2000 currency (baseline). Dependent variable is turnout rate in congressional district. Standard errors are in parentheses.

[a] Dummy variables for election years are not reported. Complete results are available on request.

The reduced equation in model 3.2.1 demonstrates that each additional term served by the incumbent negatively affects turnout. This effect becomes increasingly significant as we add

control variables (especially the lagged turnout, lagged vote, and campaign spending cofactors). The previous results confirm our second hypothesis, namely, that turnout decreases as the number of terms served by the incumbent increases. It is interesting to note that the effect of seniority appears to be cubic. This nonlinear relationship suggests that there is an initial reduction in turnout following the election of a freshman incumbent, but that this effect is somewhat attenuated as the number of terms of service increases (because of the positive squared term in equation 3.2.1–3.2.4). On the other hand, the cubic cofactor in the equations leads us to expect that turnout will once again decrease when the number of years served by the incumbent reaches its apex. Overall, the effect of the number of terms on turnout appears to be relatively small but robust and significant—ranging between .05 and 2.5% points.

Table 3.3

Impact of Turnout and Number of Terms Elected on Incumbent Vote Share 1972–2000: OLS Regressions

	Model 3.3.1[a]	Model 3.3.2[a]	Model 3.3.3 (1978-2000)[a]
Turnout	-.183 (.012)***	-.236 (.022)***	-.171 (.023)***
Total Number of terms	.027 (.002)***	.021 (.002)***	.024 (.002)***
Total Number of terms squared ($\times 10^{-3}$)	-2.795 (.261)***	-2.532 (.257)***	-2.946 (.260)***
Total Number of terms cubed ($\times 10^{-4}$)	.807 (.010)***	.816 (.098)***	.947 (.098)***
Challenger quality	-	-.032 (.003)***	-.020 (.004)***
Lagged vote	-	.540 (.015)***	.481 (.016)***
Lagged turnout	-	.115 (.022)***	.066 (.023)***
Incumbent Party democrat	-	.001 (.002)	-.006 (.002)***
Incumbent spending ($\times$ 1,000,000$)	-	-	.000 (.000)
Challenger spending ($\times$ 1,000,000$)	-	-	-.056 (.004)***
Constant	.677 (.006)***	.283 (.015)***	.354 (.016)***
Number of Observations	5028	4059	3374
R-square	.107	.391	.451

Notes:

Significance are two tailed tests, *p<.10; **p<.05 ***; p<.01. Dollar values are converted in year 2000 currency (baseline). Dependent variable is incumbent vote share in congressional district. Standard errors are in parentheses. We do not control for socio-demographic variables in this model because, unlike in the case of turnout, our theory does not lead us to expect that one socio-demographic group will be more likely to support an incumbent candidate, regardless of his/her party affiliation.

[a] Dummy variables for election years are not reported. Complete results are available on request.

We turn now to the question of whether an increase in political participation and an increase in tenure length reduce the incumbent candidate's vote share. As Table 3.3 shows, the results strongly support the third hypothesis derived from our theory. For more than 3,374 elections, an increase in turnout appears to have a strong and significant negative effect on the vote share received by the incumbent candidates. Depending on whether we control for campaign spending (model 3.3.3) or not (model 3.3.2), the effect of the turnout variable ranges between -.24 and -.17. If we set the turnout variable at the mean of the sample (.44), we find that the voting share of the incumbent would be reduced by .11 (obtained from .44 × -.24). Tenure length also has a positive nonlinear effect on the incumbent's two party vote share. There appears to be an initial increase in the electoral support for a freshman incumbent following his or her reelection. However, this effect becomes negative as the seniority of the incumbent reaches five terms (because of the negative squared term in equation 3.3.1 and 3.3.2). The cubic cofactor in the model accounts for the increase in the incumbent support observed when the member has been in office for more than 15 terms.

As I pointed out earlier, it is very probable that the relationship between turnout and incumbent vote share is endogenous. In effect, both turnout and vote share are most likely simultaneously influenced by the threat posed by a quality challenger. We can suppose that turnout affects the probability of reelection. But we can also assume that this probability is

influenced by the level of turnout in the coming election. Furthermore, isolating the effect of competitiveness empirically is also difficult because of challenger quality. This is explained by the fact that the challenger's entry decision is itself endogenous to incumbent performance (Gordon & Huber 2007). Short of randomly assigning turnout rates, or vote margins in the districts, the results will likely have a simultaneity bias. It is still possible to control for this simultaneous relationship by using two-stage least squares estimations (e.g., Green and Krasno, 1988, Gerber 1998). When using this method, we first predict district turnout with cofactors that are in theory not directly related to the election outcome (see Kelejian, 1973; Hausman, 1983; Greene, 2000). The second stage uses the new turnout estimates, purged of the campaign specific effects, to predict the total vote share. Table 3.4 provides the results of our two-stage model.

Table 3.4

Impact of turnout and Number of Terms Elected on Incumbent Vote Share 1978-2000: Two Stage Least Square

	Model 3.4[a]
Turnout	-.090 (.016)***
Total number of terms	.027 (.002)***
Total number of terms squared ($\times 10^{-3}$)	- 3.188 (.287)***
Total number of terms cubed ($\times 10^{-4}$)	. 100 (.108)***
Challenger quality	-.029 (.004)***
Lagged vote	.226 (.012)***
Incumbent party Democrat	.002 (.003)
Incumbent spending ($\times$ 1,000,000$)	-.006 (.004)
Challenger spending ($\times$ 1,000,000$)	-.087 (.006)***
Constant	.513 (.012)***
Number of observations	3430
R-square	.336

Notes:

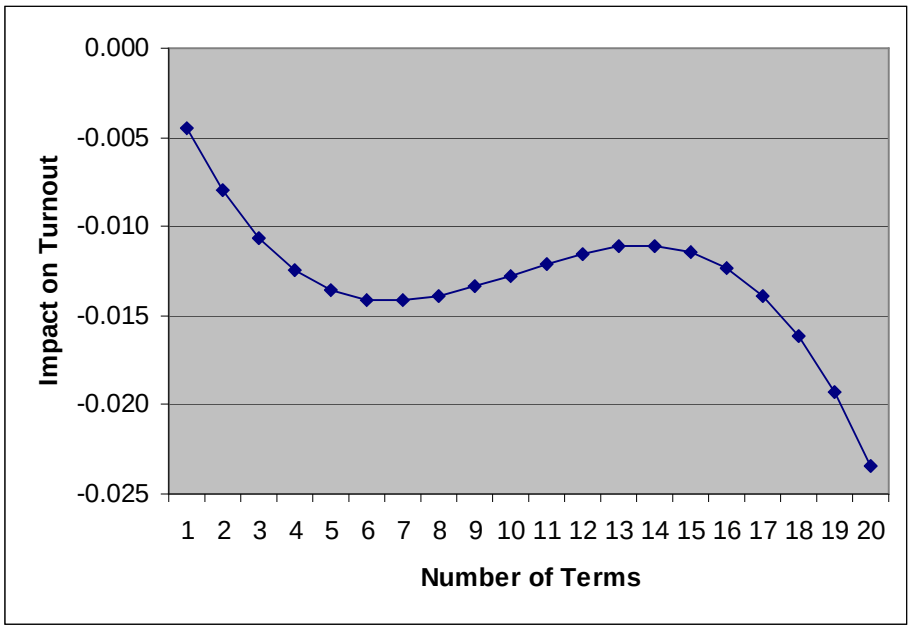
Significance are two tailed tests, *p<.10; **p<.05 ***; p<.01. Dollar values are converted in year 2000 currency (baseline). Dependent variable is incumbent vote share in congressional district. Standard errors are in parentheses. The variable turnout is instrumented with the following variables: lagged turnout, election year dummies, Age 65+, African American, Union, Urban, Unemployment, College, Home Owner, Median Income (× 10,000$), South.

The instruments used to calculate turnout are reported at the bottom of the table. The estimations of the two-stage least-squares model yields results similar to the simple OLS models included in Table 3.3. The major difference is in the reduction of the substantive effect of the turnout measure on the dependent variable. Its coefficient is about half the value of model 3.3.3 (-.171 vs. -.090). However, both effects remain negative. This implies that an increase in turnout, regardless of the competitiveness of the race, will still significantly reduce the incumbent's electoral vote share.[17]

A posteriori, the preceding analyses on the effect of the number of terms on turnout and on the two party vote share confirm our expectation that the likelihood of a quality candidate emerging to challenge incumbents would be nonlinear. Figures 3.4 and 3.5 demonstrate this relationship. We begin by looking at the effect of seniority on turnout (in Figure 3.4). This effect appears to be very low at the start of a legislative career. However, as the number of terms served increases, turnout gradually diminishes because weaker and marginal members are eliminated from the pool of incumbents, leaving more qualified incumbents or incumbents in safer districts. In other words, turnout gradually falls because fewer "hot" races can be contested by quality challengers.

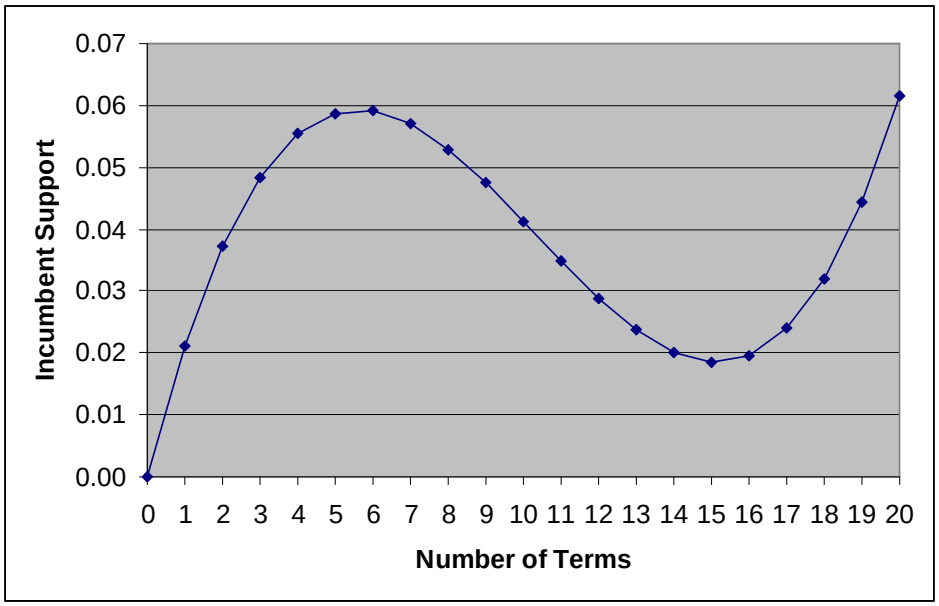
[17] This will be true to the extent that a valid instrument for turnout is found. The instrumental variables used in the Appendix are not completely independent of electoral competitiveness. However, since the socio-demographic variables used are measured only at each decade, they do provide a somewhat isolated measure of turnout in each election year.

Figure 3.4. Simulated impact of the number of terms on turnout for incumbent candidates (1978-2000).



Note: Included in this graphic is the predicted values obtained from model 3.2.4. The marginal effect of an increase in the number of terms (squared, and also cubed) on turnout is plotted in the graphic. Values are obtained simply by multiplying the terms coefficients with the number of term served in model 3.2.4.

Figure 3.5. Simulated effect of the number of terms on two-party vote share (1978–2000).



Note: Included in this graphic is the predicted values obtained from model 3.3.2. The marginal effect of an increase in the number of terms (cubed) on the incumbent two party vote share is plotted in the graphic. Values are obtained simply by multiplying the terms coefficients with the number of term served in model 3.3.2.

In the later part of congressional careers, the number of terms served starts to stimulate and increase turnout. I believe that this phenomenon corresponds to a resurgence in contested elections and a re-emergence of a pool of quality challengers. This dynamic can be explained once again by sample attrition. Since the most ambitious members of the House usually depart for higher office or for higher paying jobs by the time they have served their sixth term (Diermeier, Keane, & Merlo, 2005), the sample is left with senior incumbents who are more likely to have settled their careers in the House. Because the likelihood of scandals and of being labeled as "out of touch" with the district increases with tenure length, we should also expect the number of opportunities for quality challengers to campaign against weaker incumbents to increase, and turnout to be stimulated (Stewart, 1994). This is in fact what we observe in Figure 3.4 when we look at the second local maxima located at the thirteenth term served.

We find the same type of dynamic when we look at the effect of congressional tenure on the incumbent candidate's vote share in Figure 3.5. Initially, incumbent support increases during the first five terms served. This relationship makes sense if we consider that marginal lawmakers are more likely to be challenged by strong candidates at the beginning of their careers. So their removal from office leaves us with a sample of more qualified and "safer" incumbents who will generally have stronger electoral margins. After attaining a local maximum at the sixth term served, the incumbent support begins to decline steadily as the number of career politicians included in the distribution reaches its apex. This reduction in support can once again be

explained by the emergence of quality challengers looking to seize any opportunity to dislodge career incumbents who, as their seniority increases, are more likely to spend time away from their district (Fenno, 1978). Finally, the later part of the distribution corresponds to the very few successful incumbents who have managed to attain a high level of seniority in Congress and maintain a strong support for their candidacy in their home district.

Table 3.5

Impact of turnout on the Probability of Incumbent Reelection 1972-2000: Probit Regressions

	Model 3.5.1[a]	Model 3.5.2 [a]	Model 3.5.3 (1978-2000)[a]
Turnout	-.834 (.237)***	-2.631 (.546)***	-2.030 (.652)***
Number of terms	.428 (.036)***	.467 (.044)***	.574 (.056)***
Total number of terms squared	-.048 (.006)***	-.055 (.007)***	-.067 (.008)***
Total number of terms cubed	.002 (.000)***	.002 (.000)***	.002 (.000)***
Challenger quality	-	-.413 (.075)***	-.375 (.092)***
Lagged vote	-	2.102 (.317)***	1.274 (.370)***
Lagged turnout	-	1.703 (.522)***	1.195 (.613)
Incumbent party Democrat	-	-.033 (.066)	-.199 (.079)***
Incumbent spending (× 1,000,000$)	-	-	.285 (.097)***
Challenger spending (× 1,000,000$)	-	-	-.867 (.088)***
Constant	1.158 (.120)***	-.516 (.351)***	.496 (.412)***
Number of observations	5030	3732	3003
Pseudo R-square	.088	.351	.226

Notes: Significance are two tailed tests, *p<.10; **p<.05 ***; p<.01. Dollar values are converted in year 2000 currency (baseline). The dependent variable is coded 1 if the incumbent was re-elected, 0 otherwise. The numbers used in the table are probit index standard deviations; standard errors are in parentheses.

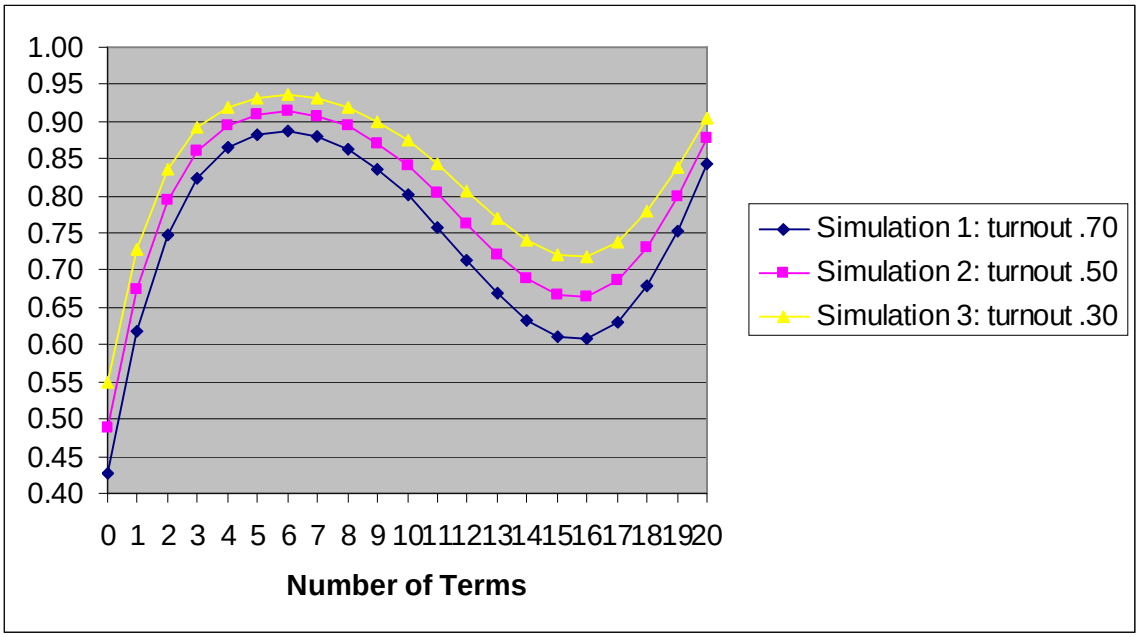
[a] Dummy variables for election years are not reported. Complete results are available on request. The 1998 and 2000 election years were excluded since the model predicts a 100% reelection rate in 1998 and 2000.

The previous analysis still leaves open the question of whether turnout and career length can affect the probability of winning reelection. To assess this possibility, I have estimated a probit regression model measuring the effect of turnout and tenure length on the probability of winning

an election for the incumbent candidate. Table 3.5 presents these results, which confirm our fourth hypothesis. In all three models, turnout has a significant and negative effect on the vote, and career length appears to have the same type of nonlinear relationship with the vote as in all the previous two party vote support models. The predictions indicate that in elections where turnout is high, the probability of being re-elected is reduced. Because we are interested in the specific effect of turnout and seniority on reelection probability, and in order to make sense of the probit parameters found in the models, I have simulated three hypothetical congressional races in which an incumbent was running for reelection. In all three simulations, I alter the meaning of the data by calculating the effect of three different turnout rates (.3; .5; .7) on the probability of reelection, given the number terms served (0 through 20).[18] The results and detailed descriptions of the simulations are presented in Figure 3.6.

[18] For the case of open seat elections (when the number of term served was set at 0), incumbent candidates had to share the party of the previous incumbent.

Figure 3.6. Simulated effect of turnout and number of terms on the probability of reelection of an incumbent candidate (1978–2000).



Note: Included in these graphics are simulation values obtained in the probit equation in model 4.2. The marginal effect of an increase in the number of term served on the probability of reelection for the incumbent is plotted in the graphic. The predicted values are obtained from three hypothetical congressional races.

The first congressional race, labeled Simulation 1 in the graphic, corresponds to a midterm election race in 1990 where an incumbent is running for reelection against a challenger of quality. The incumbent spent $500,000 in the campaign, and the challenger spent $200,000. The incumbent is a Democrat. Turnout was at .70 in all of the elections, and the incumbent candidate was previously elected in a close race where she/he won .51 of the two party vote in the previous election. Note that turnout was at .50 in this last election.

The second congressional race, labeled Simulation 2 in the graphic, corresponds to a midterm election race in 1990 where an incumbent is running for reelection against a challenger of quality. The incumbent spent $500,000 in the campaign, and the challenger spent $200,000. The incumbent is a Democrat. Turnout was at .50 in all of the elections, and the incumbent candidate was previously elected in a close race where she/he won .51 of the two party vote in the previous election. Note that turnout was at .50 in this last election.

The third congressional race, labeled Simulation 3 in the graphic, corresponds to a midterm election race in 1990 where an incumbent is running for reelection against a challenger of quality. The incumbent spent $500,000 in the campaign, and the challenger spent $200,000. The incumbent is a Democrat. Turnout was at .30 in all of the elections, and the incumbent candidate was previously elected in a close race where she/he won .51 of the two party vote in the previous election. Note that turnout was at .50 in this last election.

As in the result on vote share, the probit equations and the simulations actually re-enforce the notion put forward by my theory, which stipulates that an increase in turnout reduces the probability of winning a reelection bid. When turnout is low (set at .3), the probability of reelection for an incumbent remains higher than when turnout is significantly greater (set at .5 or .7). Furthermore, accumulated experience and seniority in the House (measured by the number of terms served) does increase the probability of winning a reelection bid for the incumbent at the start of his or her career. However, as in our analysis of the effect of tenure length on two party vote share in models 3.3.1–3.3.3, the effect of seniority is nonlinear, with an increase in the probability of reelection for beginners, and a later slump for career politicians.

To summarize the key findings of tables 3.1–3.5, we first found that open seat election, challenger quality, and campaign spending (especially challenger campaign spending) increase the level of political participation in House elections. We also found that an increase in turnout reduces the level of incumbent electoral support and the probability of winning, even after we controlled for previous turnout and margin of victory in the district. The results also demonstrated that the number of terms served by the incumbent simultaneously reduces turnout and the two party vote share. In both cases, this effect was nonlinear. The models predicted an increase in the vote share at the beginning of an incumbent's tenure, followed by a later "career" reduction in support. A complementary relationship was found when we looked at the specific effect of incumbent seniority on turnout. Turnout remains high when we consider the careers of incumbents who have served a limited number of terms. However, it tends to decline simultaneously with the reduction of competitive races and quality challengers.

3.7. Discussion

The previous results support our theory of legislative elections, which postulates that it is in the interest of incumbents to keep turnout low by maintaining higher costs of participation. To be sure, the cost of political participation is not distributed evenly among voters. Incumbent and challenger party supporters face lower costs since it is easier for them to obtain information about their own preferred candidates. On the other hand, it is safe to assume that independents and leaners face higher costs of participation. This explains why it is more expensive for both challenger and incumbents to get undecided voters at the polls. Not only is it more costly to encourage them to participate, but it also represents a riskier strategy for the incumbent candidate. As DeNardo (1980) explains, undecided voters are less reliable supporters of the incumbent because "peripheral voters do not have strong feelings of attachment to either of the political parties, and make decisions about voting largely in response to the level of excitement associated with a particular election" (409).

A quality challenger and higher campaign spending ultimately increases turnout. However, as our results demonstrate, the outcome does not necessarily translate directly into additional support for the incumbent candidate. Flooding the congressional district with campaign money will indeed get more voters to the polls. But it will in no way guarantee their ex-post facto support. This is especially true if we consider the negative effect of turnout on an incumbent's probability of reelection. I believe that it is in the interest of both Republican and Democratic incumbents to keep turnout low among undecided voters in order to maximize their probability of remaining in office. Unless the incumbent is facing a tough reelection battle or a

strong challenger, he or she should not invest campaign resources to increase turnout among undecided voters.[19]

The previous results also demonstrate that seniority ultimately affects the probability reelection. Because incumbents who are running for reelection can expect a backlash at the polls at the beginning and in the later stage of their careers, it becomes particularly important for them to discourage any potentially strong challengers from entering the race, and to keep turnout to a minimum. This can be done by setting up entry barriers, such as building impressive campaign war chests or cultivating a brand-name in the district. Of course, not every incumbent is successful in this task. But very few are not. Short of voluntarily exiting from Congress, the probability of being removed from office is extremely small. That is why higher turnout rates are especially important for challengers. Unfortunately for them, the campaign funds required to increase turnout are in very short supply.

The moral hazard associated with quasi-monopolistic behavior by legislators could be devastating. If lawmakers are effectively insulated from electoral competition, can they be accountable for their political decisions? Because incumbent reelection rates in contemporary congressional elections are so high, open seat elections are typically the only remaining competitive contests. If we consider that there are fewer competitive races in the House, we should find that independents and peripheral voters will have a lower probability of turning out on Election Day. We should also find that a reduction in turnout will reduce the number of voters with a lower socio-economic background.

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[19] When $[E_I \cdot \rho_I] > [E_C \cdot \rho_C] + [E_U \cdot \rho_U]$ or the proportion of the vote for the incumbent (excluding the support of undecided voters) is expected to be greater than the total vote for the challenger, then the incumbent is considered safe.

The analysis presented in Chapter 2 shows that disaggregate national opinion at the state level highlights regional fluctuations in the preferences of both voters and nonvoters. In many of the more local races, where incumbents are basically guaranteed reelection, voters and non-voters may have radically different policy preferences—especially if we consider the possibility that undecided voters are less likely to vote in closed-seat elections.

If we follow the conventional theory of the "electoral connection" research program, we should be able to compare the voting records of members of Congress and electoral turnout in order to determine the extent of the relationship between participation and legislative behavior. Greater turnout (and by extension the presence of quality challengers) should, according to this theory, increase the number of non-partisan voters and voters of lower socio-economic status. This phenomenon should theoretically encourage re-election minded incumbents to be more responsive to the different needs of these new electors. The next chapter will measure the extent to which political participation has an effect on legislative behavior. This will be gauged by studying the linkage between falling turnout rates, congressional voting, and extreme legislative behavior in the United States Congress.

CHAPTER 4

The Influence of Turnout and Competitiveness on House Legislative Behavior

The assumption that legislators are responsive to their constituents' interests is a truism of political science (e.g., Miller & Stokes, 1963; Mayhew, 1974; Fiorina, 1974; Achen, 1978). However, the links between congressional legislative behavior and the level of political participation has been understudied.[1] This can be explained by the fact that existing empirical and formal studies of voter turnout generally argue that abstention has no clear legislative consequences. For example, in public opinion analyses many scholars conclude that because voters and nonvoters express similar voting preferences, higher turnout will not affect election results (Highton & Wolfinger, 2001; Rosenstone & Hansen, 1993). Similarly, in formal models of elections, electoral abstention is expected to make little differences on the behavior of candidates since policy convergence almost always maximizes a candidate's probability of reelection (Hinich & Ordeshook, 1969; Ledyard, 1984).

Nevertheless, recent studies have increasingly challenged the notion that turnout plays an insignificant role in congressional election. For instance, Citrin, Shickler, and Sides (2003) show that the ideologies of voters and nonvoters differ when opinion is disaggregated at the state level (this finding is also confirmed in chapter 2). Moreover, formal models developed by Adams and Merrill (2003), Callander and Wilson (in press), Glaeser, Ponzetto, and Shapiro (2005), and the institutional theory of political participation proposed in chapter 3, all demonstrate that turnout can have an important influence on a candidate's entry decisions and reelection probability.

[1] Notable exceptions are Griffin and Newman (2005) and Martin (2003).

Given these recent findings, one should not simply assume that the level of political participation does not affect legislative behavior or electoral accountability.

One of the principal difficulty with this line of reasoning is that competitiveness, rather than turnout, could be the principal factor influencing legislative behavior. The idea that high levels of competition can cause lawmakers to be more responsive to public opinion has always been a central condition of representative democratic theory. For example, Mayhew (1974) argues that vulnerable incumbents have to be more responsive or attentive to the opinion of the district as a whole if they aim to maximize their chances of reelection. However, Fiorina (1974) maintains the opposite, arguing that politicians serve the interests of constituents who contribute most to their reelection chances. Hence, in districts where incumbents are elected by narrow margins, representatives have an incentive to focus on the segments of the electorate that can most significantly influence the election outcome. Concretely, this means that incumbents will be less responsive to average district opinion in competitive districts.

This chapter will add to this debate by proposing a novel approach to analyzing the relationship between voting and policy extremism. The aim is to determine whether varying levels of competitiveness, and by extension turnout, influence roll-call voting in the House of Representatives. The chapter will also study the effect that legislative behavior can have on political participation and on the incumbent's level of electoral support. I will look mainly at House elections from 1972 to 2000. So this chapter is not only the first study to measure the effects of political participation on House roll-call behavior, but it is also the first attempt to simultaneously consider how the electorate holds members of Congress accountable for their legislative actions and how these members subsequently represent the interests of their

constituents. The norm in the literature has been to analyze each of these dimensions independently.[2]

For instance, recent work by Griffin (2006) and Gulati (2004) study the effects of electoral competitiveness on congressional legislative behavior, but they both fail to integrate prior determinants of electoral marginality in their models. On the other hand, electoral accountability studies done by Canes-Wrone, Brady, and Cogan (2002) and Erikson and Wright (2000) center their analysis on the impact of past legislative behavior on incumbent electoral support, but they disregard any potential effects that electoral participation and incumbent vote share might have on future legislative behavior.

In this chapter I will argue that any clear understanding of electoral accountability and legislative responsiveness must be placed in a holistic framework. More specifically, the study will examine the relationship between participation and policy extremism in order to determine whether levels of turnout make a difference for legislative behavior (*the responsiveness hypothesis*), and whether legislative behavior in turn affects political participation and the incumbent's vote share (*the accountability hypothesis*). This approach leads to a solution to an empirical puzzle in the congressional election literature; namely, that competition and electoral marginality sometime lead lawmakers to be *less* responsive to public opinion.

The chapter begins with a review of the literature on congressional representation, accountability, and political participation. The second section evaluates some of the existing theoretical assumptions about the links between legislative behavior, turnout, and constituency preferences. The third section presents some data that validate the *accountability* and

[2] Erikson and Wright conducted a similar study (2000). However, they do not explicitly measure the effect of competitiveness on roll-call behavior.

responsiveness hypotheses. The last section draws some implications from the empirical analyses.

4.1. Accountability, Representation, and Legislative Behavior

Past research on congressional behavior has established that legislators consider constituent policy preferences when deciding upon roll-call votes (Canes-Wrone, Brady, & Cogan, 2002; Brady, Cogan, & Canes-Wrone, 2000; Arnold, 1990; Fiorina, 1974; Mayhew, 1974). This is explained by the fact that district-level competition inclines candidates to adapt their legislative behavior to the voters' ideological preferences (Ansolabehere, Snyder, & Stewart, 2001; Erikson & Wright, 2000). The bulk of these studies make the Downsian assumption that voters select candidates on the basis of ideology. They show that legislators who vote with the extreme of their party see a reduction in their electoral support (e.g., Canes-Wrone, Brady, & Cogan, 2002; Jacobson, 2004; Brady, Cogan, Gaines, & River, 1996). In contrast to this body of work, some scholars argue that legislators are increasingly nonresponsive to the preferences and concerns of their constituents (Schlozman, Page, Verba, & Fiorina, 2005; Jacobs & Shapiro, 2000; Lijphart, 1997). This decline in responsiveness is explained by many factors, most notably the increase of split-ticket voting, the growing importance of parties in Congress, heightened incumbent electoral security, partisan polarization, the lack of electoral competition, interest groups proliferation, and divisive interbranch relations (e.g., McCarty, Poole, & Rosenthal, 2006; Carson, Crespin, Finocchiaro, & Rhode, 2004; Roberts & Smith, 2003; Jacobson, 2003, 2005; Stonecash, Brewer, & Marianni, 2002). An additional explanation that is occasionally put forward is linked to the declining level of political participation.

In this last strand of studies, the focus has principally been on the relationship between turnout and electoral outcomes. Some analyses have tried to determine whether higher turnout favors the Democratic or Republican parties (e.g., Citrin, Schickler, & Sides, 2003; Highton & Wolfinger, 2001; Grofman, Owen, & Collet, 1999), while others have looked at the impact of institutions and electoral competition on political participation (Grofman, Collet & Griffin, 1998; Hanks & Grofman, 1998). To date only a handful of analyses have examined the crucial link between turnout and congressional voting behavior per se. This work on legislative responsiveness has either focused on the consequences of a reduction in the level of participation in congressional redistributive policies (Martin, 2003; Fleck, 1999; Martinez, 1997) or at the potential effect of unequal participation in electoral outcomes (Bok, 2001; Piven & Cloward, 2000; Shields & Goidel, 1997; Hill & Hinton-Anderson, 1995; Hill & Leighley, 1992; Leighley & Nagler, 1992). The essential argument of this literature is that turnout influences political rewards and political elites will react to political participation in deciding who will benefit from redistributive policies. An electorate that is disproportionately representative of citizens of high socioeconomic status is expected to be rewarded with favorable public policies.

This body of scholarship confines its focus to the redistributive aspects of legislative behavior and, thus, does not measure whether members of Congress change their voting behavior as their external or electoral demands change. A good example of this type of research has recently focused on municipal elections and finds that lower turnout leads to substantial reductions in the representation of minorities in city councils (Hajnal & Trounstine, 2005). This study basically shows that lower minority turnout has important consequences on the distribution

of public goods; it appears that minority groups receive a lower proportion of municipal funding when their level of political participation is low.

There exist only two clear studies that estimate the effect of electoral participation on legislative behavior. The first analysis, by Griffin and Newman (2005), presents convincing evidence that turnout influences the behavior of senators. The authors argue that legislators are more likely to represent the interests of those who turn up on Election Day. They postulate that senators respond disproportionably to the preferences of voters (as opposed to nonvoters) in their districts. This is explained by the fact that voters are inclined to select like-minded representatives and are more likely to communicate with their representatives. Griffin and Newman test these hypotheses by using data from the General Social Survey and by looking at senatorial roll-call behavior. Unfortunately, their research design does not control for statewide fluctuations of turnout rates, which could be an additional cause of legislative responsiveness. Furthermore, the study fails to account for the possibility that competitiveness, rather than turnout, could be what prompts lawmakers to modify their legislative behavior.

The second study which estimates the effect of electoral participation on legislative behavior was done by Ellis, Ura, and Ashley-Robinson (2006). The authors construct a model of government responsiveness to voter and nonvoter policy sentiment and conclude "[…] that policymakers do not functionally ignore the nonvoting public, either because nonvoters' preferences are largely indistinguishable from voters' or because they strategically respond to voters" (p.227). The evidence presented in chapter 2 indicates that their first explanation is simply wrong; individuals who have a higher propensity to vote are more affluent *and* conservative. Second, their aggregate study of dynamic representation suffers from the same

shortcoming as the works by Stimson, Mackuen, and Erikson (1995) and Erikson, MacKuen, and Stimson (2002): it depends heavily "upon extreme statistical aggregation that greatly restricts the domain and range of variation in the independent and dependent variables" (Page, 2002, p. 329). By looking only at aggregated national turnout rates and policy mood over the last thirty years, their work is incapable of adequately estimating how district level fluctuations in political participation and competitiveness might influence public policy.[3]

In this chapter I argue that it is impossible to understand the relationship between turnout and legislative behavior without considering how electoral competition affects both legislative responsiveness and electoral participation in the first place. Since the previous chapter has demonstrated that the level of electoral competition has a strong and positive influence on turnout, it is quite possible that electoral competitiveness can also have a moderating, or even confounding, effect on the relationship between participation and roll-call behavior. And as we will see, it is still unclear in the congressional election literature what the exact effect of competitiveness is on legislative behavior.

There exist two diametrically opposed theories that focus on determining whether elected officials are more responsive to the preferences of voters in their district when electoral competitiveness is high. To add to the confusion, both views are supported by empirical evidence. The first theory argues that the higher levels of competition incline candidates to adapt their legislative behavior to their constituents' ideological preferences (for similar arguments, see Ansolabehere, Snyder, & Stewart, 2001; Erikson & Wright, 2000; Burden, 2004). Starting with MacRae (1952), researchers have observed that incumbents who win elections with a narrow

[3] Also the authors exclude all midterm elections from their analysis.

margin are usually more responsive to the opinions of the constituency as a whole. However, as Fiorina (1973, 1974) later discovered, the opposite can also be true. The proponents of this second view argue that elected officials who win with a narrow electoral margin tend to be less responsive to the median constituent. This is explained by the fact that in marginal districts, lawmakers cannot hope to represent the interests of the whole district. Therefore, representatives must make a choice and decide whom to support, which may explain why we find more policy extremism in highly competitive districts since responsiveness is more likely to be focused on a partial segment of the constituency (see Huntington (1950) and Bartels (1991) for similar arguments). The preceding view has also been supported by empirical studies (Fiorina 1973, 1974). And the most recent evidence, presented by Gulati (2004), shows that senators who hold competitive seats are less responsive to the ideological center than those who hold safe seats.[4]

This brief overview of the literature on turnout and responsiveness has highlighted the importance of an existing empirical puzzle, namely, does electoral competition (and by extension higher turnout rates) create more or less responsive incumbents? One line of argument stipulates that competitiveness (or electoral demand) brings more responsiveness, while the other postulates the contrary. Both views are supported by empirical evidence. The question now is to determine which account is correct? In this regard, one very important and interesting additional puzzle to confront is the following: Does an increase or a decrease in the level of political

[4] It is important to note that Griffin (2006) finds contrary results at the House district level. Using House district data from the last thirty-five years, Griffin shows that incumbents elected in competitive districts tend to be more responsive to their constituents' preferences. However, some of these results are problematic since the assessment of competitiveness is not based on the incumbent's vote share, but rather on the presidential voting patterns within the constituency. Also Griffin's measure of political extremism is based on Poole's D-NOMINATE scores, which only vary monotonically across an incumbent's career.

participation, *regardless of the level of competitiveness in the district,* lead to a shift in the legislative behavior of lawmakers?

In the remainder of this chapter I argue that solving this conundrum requires an approach that takes into account the incumbent's former legislative behavior. The principal reason for this is that sample selection explains why higher competitiveness leads to more extreme legislative behavior (for similar arguments see Sullivan & Uslaner, 1978). Unless we are simply focusing on open-seat races, past legislative behavior is likely to affect voting preferences, and election results are likely to shape future legislative behavior. As Erikson and Wright (1980) put it, "Constituency electorates can contribute to representation by voting House candidates in and out of office on the basis of issues. And partly in response to such constituency pressure, elected House members and their challengers may take issue stands that reflect constituency views" (p.92). But surprisingly, studies focusing on estimating how the electorate responds to either candidates' positioning (Erikson & Wright, 1980; Ansolabehere, Snyder, & Stewart, 2001) or incumbents' legislative records (Erickson, 1971; Canes-Wrone, Brady, & Cogan, 2002) do not consider how candidates modify their congressional behavior to adjust to their constituents' shifting preferences.[5] Similarly, studies that focus on candidates' responsiveness to electoral competition (Gulati, 2004; Griffin, 2006) do not evaluate how this relationship can be affected by past legislative behavior. My study attempts to fill this important gap in the congressional literature. However, before proceeding, I want to introduce some theoretical considerations to

[5] Ansolabehere, Snyder, and Stewart (2001) look at the predictors of candidate positioning in the 1996 election, but they do not include previous electoral results as a control. Erikson and Wright (1980) also have a similar study with a limited sample.

provide a number of clarifications about the nature of the relationship between turnout, competitiveness, and legislative behavior.

4.2. Theoretical Considerations

In this section I provide a series of speculative propositions in an attempt to justify the inclusion of both turnout and competitiveness in the study of legislative responsiveness and electoral accountability. To begin with, if like rational choice theorists we assume that representative democracy links the spatial theory of voting to legislative behavior, we should expect politicians to represent their constituency's interests by converging toward the electoral median on a single-issue dimension space (e.g., Downs, 1957; Black, 1948; Davis, Hinich, & Ordeshook, 1970). In the spatial theoretical framework, policy preferences are signaled through the act of voting, and participation is seen as a mechanism to ensure that citizens' interests are represented by the government (for a similar argument, see Key, 1942; Schattschneider, 1942). This line of reasoning implies that elected representatives who are not responsive to the preferences of a majority of their constituents risk being removed from office. However, for this expectation to work, voters need to hold their representatives accountable for their past legislative records.[6] It follows that if a sufficiently large proportion of electors abstain, representation will be reduced because politicians will be accountable to a smaller sample of their constituency.

[6] Many studies have shown this to be true; for example, Canes-Wrone, Brady, and Cogan (2002); Erickson and Wright (2000); Ansolabehere, Snyder, and Stewart (2001); and Bovitz and Carson, (2006). In the traditional Downsian framework, presidential candidates (or parties) propose policy platforms on a single issue space. The analysis I present below assumes that the same logic applies to congressional districts where the platforms are represented by the incumbent's legislative record. The data does not consider nonincumbent policy positions unless otherwise indicated (for exhaustive work on this topic, see Ansolabehere, Snyder & Stewart, 2001; Erikson & Wright, 2000). It is important to also assume that a voter's assessment of an incumbent's ideology is not carried out without looking at how he or she votes on bills.

One of the main predictions of the spatial theory of voting is that competing candidates will converge on the district median in order to maximize their likelihood of election. This will be true for national and local elections.[7] An extension of the median voter theorem stipulates that convergence should be observed in any type of electoral district. In other words, in conservative constituencies, Democratic candidates will tend to adopt more moderate positions, while in liberal constituencies, Republican candidates will tend to adopt more moderate potions (Erikson & Wright, 1980, 2000). Achen (1978) calls this phenomenon responsiveness. This entails a candidate's position corresponding to the preferences of a majority of the voters in the district. The median voter theorem also predicts that legislators will benefit electorally from moderation, especially when representing marginal or unsafe districts (where the previous margin of victory was small). To remain in office, successful candidates can broaden their electoral appeal by avoiding controversial legislative positions in constituencies evenly divided between Republicans and Democrats. And this is most likely to be achieved through the adoption of a more moderate legislative agenda, where the roll-call votes of the incumbent tend to support an equal number of Republicans and Democratic bills.

Although somewhat appealing intuitively, the spatial model of electoral competition fails to be confirmed empirically. A large number of studies either predict or observe that Democratic and Republicans candidates do not converge toward the district median in congressional elections. Even Downs (1957) explains that in a two-party system, the candidates will converge on the ideological center only when faced with a unimodal distribution of voters, where there is a

[7] Since candidates rarely propose platforms at the district level, proxies of candidate positioning have generally been used, such as surveys (Ansolabehere, Snyder, & Stewart, 2001; Sullivan & Uslaner, 1978), interest groups scores (Canes-Wrone, Brady, & Cogan, 2002), or roll call voting records (Poole & Rosenthal, 1997; Heckman & Snyder, 1997).

low variance and most of the mass is clustered around the mode.[8] And since many studies have concluded that Democratic and Republicans candidates do not converge on the district median in general elections, it makes sense to assume that the level of competitiveness or even turnout rates could partially accentuate this phenomenon (for a similar argument, see Adams & Merrill, 2003).[9]

Nonconvergence can even prove to be a wise electoral strategy when considering primary electoral competitions. In such contests, candidates have to rely on the party base to obtain the maximum level of support. This is usually achieved by adopting a policy agenda closer to the average party member; as opposed to representing the interests of the constituency as a whole (Aranson & Ordeshook, 1972; Coleman, 1971, 1972; McGann, 2002; Merrill & Grofman, 1999). And because marginal districts are highly valuable for challengers, primary contests are more likely to be competitive and to select extreme challengers who may be located away from the district median voter (Fiorina, 1973; Sullivan & Ulsaner, 1978).

In addition, converging on the median of the constituency can prove to be electorally costly if the gap between the voters' median and the overall constituent median is large. This will be especially true if abstention is more prevalent among certain types of voters (concentrated around the political center, or located in the lower socio-economic strata of the district).[10] The

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[8] There are of course many more conditions for pure party convergence. For a review see Grofman (2004).

[9] Reasons given by formal theorists for nonconvergence are the presence of party activists (Aldrich, 1983a, 1983b), potential third party candidates (Palfrey, 1984), and the policy preferences of candidates (Calvert, 1985). Erickson and Wright (1993) and Ansolabehere, Snyder, and Stewart (2001) have also observed nonconvergence in a series of empirical studies.

[10] This assumption holds unless the distribution of constituent preferences is uniform in the district (which is very unlikely) or the decision to vote is assigned randomly within the electorate (which is not the case; see Chapter two).

analysis in chapter 2 and those in recent studies on turnout have demonstrated that voters are more likely than nonvoters to be party-identifiers and to come from higher socioeconomic strata (Griffin & Newman, 2005; Martin, 2003; Highton & Wolfinger, 2001; McDonald & Popkin, 2001; Aldrich, 1995). As Hinich and Ordeshook (1969) demonstrate, variations in turnout can bring a shift in the position of the median voter. In a homogenous ideological district where leaners and independents abstain more than party supporters, there is a greater likelihood of witnessing a displacement of the overall district median toward the margins. An existing ideological disparity between voters and nonvoters could theoretically produce nonsymmetric numerical distributions of preferences, such as bi-modal distributions or distributions skewed to the left or the right, with two modes and a smaller variance at the extremes. Under these circumstances, a member of Congress could choose to adopt more extreme legislative behavior, even if we find that, in the aggregate, the combined preferences of voters and nonvoters is distributed in a Gaussian framework (for similar argument, using the notion of proximity, see Griffin, 2006).[11] As a result, an additional exogenous force could explain shifts in the legislative behavior of a member of Congress, namely, the level of electoral participation in the legislator's constituency.

[11] One can also conceive of the relationship between turnout and representation as a very basic signaling game. As Arnold (1990) explains, the logic behind the fact that voting encourages responsiveness is that higher turnout rates signal that legislators are currently representing an attentive public (Martin, 2003). Verba and Nie (1972) argue the same case by stating that higher political participation communicates information about the preferences of citizens. So falling turnout rates would explain why members of Congress have an incentive to adopt legislative agendas that may not necessarily correspond to the overall ideological orientation of their districts. In this vein, since a smaller proportion of voters will be holding members accountable for their policy choices, legislators could maximize their own policy goals (or those of their partisans) by moving away from the median voter's policy preferences while simultaneously avoiding electoral punishment (for similar argument see Jacobs & Shapiro, 2000).

It is important to note that the preceding argument, which stipulates that turnout can influence legislative behavior, will only make sense if we consider how electoral competitiveness interacts with political participation. Because turnout is intrinsically related to the level of competition, we cannot assume that each of these cofactors has an independent influence on the behavior of incumbents. Unless we limit our attention to noncompetitive elections, it is necessary to consider the possibility that competitiveness, rather than turnout, might be the principal driving force behind changes in the level of responsiveness of elected officials. And as we saw earlier, it is still not clear how exactly electoral competition affects policy responsiveness in the first place. Thus, before we can even begin to understand how turnout interacts with legislative behavior, we need to determine how competitiveness affects congressional voting.

The next section of the chapter includes such an analysis. The first task will be to determine whether previous roll-call ideological extremity influences turnout and incumbent vote share (the study of electoral accountability). The subsequent task will be to investigate whether legislators respond to shifts in the level of participation and electoral support by altering their roll-call behavior (the study of legislative responsiveness).

4.3. Data and Models

The empirical analysis focuses on House elections from 1972 to 2000. The accountability and responsiveness hypotheses are modeled independently for each election year. The analysis also includes different pooled cross-section models (combining all elections from 1972 to 2000), and a model where incumbent and challenger policy positions are estimated simultaneously. These

different specifications are incorporated in order to control for some of the confounding cofactors that are known to influence congressional election results. The aim of this empirical study is to determine whether districts with higher levels of turnout and electoral competition tend to be represented by more moderate legislators, and whether extreme legislators are more likely to be punished at the polls. The analysis tests the following four interdistrict hypotheses:

Hypothesis 1 (accountability):
More extreme roll-call legislative behavior increases turnout in the next election.

Hypothesis 2 (accountability):
More extreme roll-call legislative behavior reduces the incumbent's level of electoral support.

The next two hypotheses measure the relationship between election results and subsequent legislative behavior.

Hypothesis 3 (responsiveness):
Districts with a low level of turnout are more likely to be represented by extreme legislators.

Hypothesis 4 (responsiveness):
Districts with a high level of incumbent support are more likely to be represented by extreme legislators.

In order to test these hypotheses, the study employs the same data set concerning House elections as presented in chapter 3. The data is updated to include a variable from Groseclose, Levitt, & Snyder (1999); Groseclose (2001); and Canes-Wrone, Brady, and Cogan (2002) that measures legislative extremism (the coding of the variables is described below).[12] All of the following analyses focus on races where an incumbent is running for reelection and facing a major-party candidate; unless otherwise indicated, open-seat races are excluded. To begin, we test the

[12] I use ADA scores from Groseclose's (2001) dataset.

accountability hypothesis in every election year from 1972 to 2000. We use a very simple specification in order to determine whether previous legislative behavior has an impact on the incumbent's electoral support.[13] From the standpoint of the electorate, the temporal outlook in the next two formulas is retrospective since the model incorporate previous legislative behavior and tests for the accountability hypothesis.

The model in the first equation is included to determine whether the level of turnout is affected by roll-call ideological extremism, district ideology (as measured by the presidential vote in the district), and district marginality.

(1) $$Turnout_{cd,t} = \beta_0 + \beta_1 Close_{cd,t-1} + \beta_2 pres\,Vote_{cd,t(p)} + \beta_9 Legislative\ Extremism_{cd,t-1} + \varepsilon_{cd,t}$$

The next model is intended to replicate some of the results of Canes-Wrone, Brady, and Cogan (2002).[14] Here, the incumbent's vote share in congressional district *cd* is said to be a function of legislative extremism, district ideology, and district marginality.

(2) $$IncVote_{cd,t} = \beta_0 + \beta_1 Close_{cd,t-1} + \beta_2 pres\,Vote_{cd,t(p)} + \beta_9 Legislative\ Extremism_{cd,t-1} + \varepsilon_{cd,t}$$

The final model tests for the two responsiveness hypotheses. The equation determines whether a particular level of turnout, district ideology, or incumbent support has an impact on roll-call ideological extremism. Formally, the model estimates for congressional district *cd* in election *t* the following equation:

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[13] Later we use a more complete set of control variables in a pooled cross-sectional model. I use a very simple model with a very limited amount of recoding in order to make it easier to understand the relationship between these variables. The simple modeling and codification scheme allows for a very straightforward interpretation of the parameters included in the models.

[14] I use a different model specification and a different coding scheme for certain variables.

(3) $Legislative\ Extremism_{cd,t+1} = \beta_0 + \beta_1 turnout_{cd,t} + \beta_3 pres\ Vote_{cd,t} + \beta_4 Inc\ Vote_{cd,t} + \varepsilon_{cd,t}$

The notation in the previous three equations, especially the temporal outlooks (*t, t*-1, and *t*+1) is very important. When the dependent variable is roll-call legislative extremism, the incumbent is assumed to be responsive to the most recent election results. However, the same legislative behavior is also assumed to affect subsequent election results. This is why the most recent measure of legislative behavior prior to the election is included as an exploratory variable in the accountability models (for equations 1 and 2). The measurements of each parameter used in the model are as follows:

Legislative Extremism: This variable represents the level of extremism of the roll call voting behavior of all elected representatives. The variable is based on the scores provided by the liberal interest group American for Democratic Action (ADA). This group counts the number of liberal positions each House member takes for a sample of roll-call votes in a particular Congressional session. These votes are usually selected because they are considered important for advancing the liberal legislative agenda, and so a high ADA score signifies a strong liberal record. ADA scores are widely used in the Congressional voting literature (e.g., Lee, Moretti, & Butler, 2004; Canes-Wrone, Brady, & Cogan, 2002; Erickson & Wright, 2000; Groseclose, Levitt, & Snyder, 1999). Unlike DW-NOMINATE scores, ADA scores can fluctuate nonmonotonically over the course of an incumbent's career. And since the following analysis aims to determine whether participation rates affect legislative behavior,

using the ADA measures makes intuitive sense.[15] To construct the legislative extremism variable, the study simply reproduces the Canes-Wrone, Brady, and Cogan (2002) measure, which uses the ADA value for democratic incumbents (which range from 0 to 100). However, for republican incumbents, legislative extremism was measured by taking the absolute value of the ADA score minus 100.[16] The transformation is done in order to obtain a conservative score on the liberal scale: a high absolute ADA score represents a more extreme roll-call legislative behavior while a midrange value represents a more moderate record, *regardless of party affiliation.* Many scholars (e.g., Ansolabehere, Snyder, & Stewart 2001) have also focused on estimating the electoral consequences of both incumbent and challenger ideological positions in a specific district-level election. But since the following analysis intends to measure the consequence of policy extremism in the U.S. Congress, the appropriate design is to simply focus on the incumbent candidates. It makes sense to assume that a challenger's ideological location in a race where the incumbent was re-elected will have very little impact on future legislative behavior. On the other hand, it may be possible that a challenger's position will influence an incumbent's vote share in the subsequent election. To control for this possibility, the empirical analysis presented below includes a series of models where the previous incumbent legislative record is integrated into the model. The study also contains a replication of the Ansolabehere, Snyder, and Stewart (2001) study where both challengers' and incumbents' positions were modeled simultaneously.

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[15] I also used Turbo ADA scores, which are adjusted for inflation. However, since the turbo ADA scores are not available for 2000, they are not included in the analysis. For 1980–1998, the results are similar if one uses ADA or adjusted turbo ADA (results available upon request to the author).

[16] For purpose of presentation, I divided the result by 100.

Turnout: In order to measure the turnout rate in each congressional district for the elections from 1972 to 2000, the analysis divides the total number of votes counted in an election (as listed by the U.S. Office of the Clerk) by the number of people of voting age in the district. It is the same measure of turnout that was used in chapter 3.

Pres Vote: This variable represents the presidential candidate's two-party vote share in the district. It is used as a proxy for district ideology and it is coded by party affiliation. The House incumbent receives the number of district votes that the presidential candidate of the same party obtained in the most recent presidential election.[17] Recent work by Levendusky, Pope, and Jackman (2005) has shown that district-level presidential vote is a reliable substitute for district ideology (also see Griffin, 2006; Canes-Wrone, Brady, & Cogan, 2002; Ansolabehere, Snyder, & Stewart, 2001; Erickson & Wright, 2000, 1980, for a similar argument).[18] The assumption here is that the presidential vote is not affected by the candidate's personal vote. It should therefore be a more precise measure of the district's ideological orientation. Since the data covers elections occurring after 1970, the presidential vote-share count is available by district (rather than by county).[19]

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[17] For example, in a 1996 election, the third model (responsiveness) reports the two-party presidential vote share of the incumbent in 1996. However, in the first and second models (accountability), this variable used the 1992 election results.

[18] Other studies use other ways to measure district level preferences. For example, Clinton (2006) uses the National Annenberg Election Survey, which contains over 100,000 respondents, to estimate House-district ideology. Snyder (1996) and Lewis (2001) use voting records in California ballots as a proxy for ideology. Finally, Levitt (1996) uses the voting behavior of House members to estimate the ideology of senators. The main advantage of using the presidential vote is that it is available over a longer period of time and is widely accepted by congressional scholars.

[19] I use Carson's presidential district two-party vote share measure.

Close: This variable is included in the first and second models of accountability to control for the previous level of electoral competitiveness in the district (at *t*-1). Incumbents who won with less than 53% of the vote were coded 1, 0 otherwise. The variable is used in lieu of previous incumbent two-party vote share, since this measure is strongly correlated with the dependent variable (incumbent support). Consequently, the inclusion of this cofactor (instead of a simple lagged measure of incumbent support) reduces multicollinearity while still permitting some control for the level of competitiveness found in the district. The cutoff measure of 53% was used to determine whether the incumbent represented a safe district.[20]

Inc Vote: Represents the percentage of the two-party vote received by the incumbent (either Democrat or Republican) in election *t.* Third-party candidates were excluded from the analysis. In the accountability model, this variable reports how much the incumbent candidate received in the election (which can theoretically be below 50%). However, for the responsiveness model, the two-party vote share will always be greater than 50% since the analysis focuses at the subsequent legislative behavior of *elected* incumbents. Elections where the incumbent candidate is not facing a major-party candidate are removed from the analysis (797 elections were removed from the sample of 5,706 elections).

The analysis I present below also includes results from instrumental, pooled cross-sectionals, and candidate-positioning models. The descriptions of these equations and their variables are presented in Appendixes A, B, and C of this chapter. The reader will be reminded

[20] Lee, Moretti, and Butler (2004) demonstrate that the winners in elections determined by less than a 2 percent vote share can be virtually conceived as randomly determined. So this explains why we select a 3 percent cutoff point, which includes a larger number of cases to control for district level competition.

of this fact whenever the analysis discusses any of those results. It is also important to note that congressional redistricting and reapportionment can alter the values of some of the cofactors presented above. Hence, in the models where turnout and incumbent vote-share are the dependent variables, it was necessary to remove all races where a statewide reapportionment occurred in 1972, 1982, or 1992.[21] This was done to account for the fact that the most recent measure of presidential vote for these years is only available for 1968, 1980, and 1988—all of which are elections under the old district boundaries.[22] Thus, the districts originating from the states where reapportionment occurred in 1972, 1982, and 1992 were removed from the sample, and subsequently reintegrated in the dataset once a presidential election was held under the new statewide congressional boundaries (for the responsiveness model, this only affects the 1982 election).[23]

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[21] States that lost or gained seats in 1970 were Alabama, Florida, Tennessee, Texas, West Virginia, New York, Pennsylvania, Illinois, Indiana, Michigan, Ohio, and South Dakota. In 1980: Florida, Tennessee, Texas, Kansas, Missouri, Massachusetts, New Jersey, New York, Pennsylvania, Illinois, Michigan, and Ohio. In 1990: Florida, Georgia, Louisiana, North Carolina, Texas, West Virginia, Massachusetts, New Jersey, New York, Pennsylvania, Illinois, Indiana, Michigan, Ohio, Iowa, and Kansas.

[22] 1984 was also removed since the most recent presidential vote measure was only available in 1980. Remember that the accountability models measure how previous legislative behavior affects the election results.

[23] Since 1972 and 1992 correspond to presidential election years. Keep in mind that the responsiveness model looks at how election results affect legislative behavior. This approach does not control for some of the redistricting or court-ordered redistricting that may have occurred in states where there was no reapportionment. However, the results do not change when we remove all of these cases from the sample.

4.4. Empirical Results

The empirical analysis begins by looking at the distribution of legislative extremism in House electoral districts from 1972 to 2000. Summary statistics for the variables of interests are reported in tables 4.1, 4.2, and 4.3.

Table 4.1

Legislative Extremism by Incumbent Two-Party Vote Share in the District for the First Congress Following the Election (t+1, 1972–2000)

Two-party vote share for incumbents at time t	*Mean legislative extremism(*t*+1)*
50%-59% (n=1552)	.783 (.213)
60%-65% (n=1169)	.773 (.226)
66%-71% (n=1215)	.773 (.229)
72%-100% (n=1327)	.778 (.235)

Note: Standard deviations in parentheses.

Table 4.2

Distribution of Mean Legislative Extremism by the Incumbent's Party Presidential Vote Share in the District for the first Congress Following the Election (t+1, 1972–2000)

Presidential vote share for in party, last presidential election	*Mean legislative extremism (*t*+1)*
0%-47% (n=1367)	.647 (.252)
48%-56% (n=1406)	.764 (.215)
57%-64% (n=1375)	.815 (.200)
65%-100% (n=1306)	.887 (.147)

Note: Standard deviations in parentheses.

Table 4.3

Distribution of Mean Legislative Extremism by Turnout Rates in the District for the First Congress following the election (t+1, 1972–2000)

Turnout at time t	*Mean legislative extremism (*t+*1)*
0%-37% (n=1348)	.763 (.245)
38%-45% (n=1342)	.776 (.231)
45%-54% (n=1208)	.791 (.219)
55%-100% (n=1434)	.790 (.193)

Note: Standard deviations in parentheses.

The tables display the mean legislative extremism score (the absolute ADA score coded by party affiliation) in relation with three election measures: the percentage of vote the incumbent received; the distribution of presidential support in the district; and the district turnout rate. The standard deviations are in parentheses. Each table contains four categories: dividing the 25th, 50th, 75th, and 100th percentiles of the distribution. The data is assumed to be normally distributed and the centiles are based on the sample mean plus *k* standard deviations, where *k* is determined from the normal standard distribution. For example, the lowest value of the third row of each table corresponds to the mean value of the election measures (i.e., the 50th centile of the distribution for those variables is equal to 66% for the two-party vote share, 57% presidential vote share, 45% for turnout).

The results indicate that incumbents who receive a higher level of electoral support subsequently adopt slightly more moderate legislative records (Table 4.1). It is worth noting that the difference is small, less than one point on the roll-call extremism scale (which is not enough to change a single vote on the transformed ADA scale). The most important difference is found in Table 4.2, where the level of presidential vote, which represents the ideological distribution in

the district, is strongly associated with roll-call behavior. For example, incumbents elected in constituencies where the presidential candidate of their own party received less than the average level of support (57%), adopted on average a more moderate legislative record (mean transformed ADA score is at 76 points on the 0–100 scale). Not surprisingly, incumbents elected in homogenous ideological districts displayed the highest level of legislative extremism (89 points on the same scale). Table 4.3 also indicates that higher levels of turnout are associated with more extreme legislative behavior. In fact, turnout seems to influence legislative extremism monotonically; by gradually increasing with the level of political participation in the district. At first glance, it seems that higher turnout and electoral competitiveness do not induce incumbent to moderate their roll-call behavior. However, before we can come to this conclusion, it is important to consider how lawmakers are held accountable by the electorate.

Table 4.4

Distribution of Turnout Rates by Legislative Extremism in the Previous Congress following the election (t, 1972–2000)

*Legislative extremism (*t-*1)*	*Mean turnout in election* t
0–60 pts (n=955)	.393 (.129)
60–76 pts (n=849)	.433 (.125)
76–92 pts (n=1564)	.439 (.125)
93–100 pts (n=1466)	.433 (.123)

Note: Standard deviation in parenthesis.

Table 4.5

Distribution of Incumbent Support by Legislative Extremism in the Previous Congress Following the Election (t, 1972–2000)

Legislative extremism (t-1)	*Mean incumbent support at election* t
0–60 pts (n=782)	.659 (.095)
60–76 pts (n=775)	.647 (.093)
76–92 pts (n=1407)	.659 (.099)
93–100 pts (n=1311)	.662 (.105)

Note: Standard deviation in parenthesis.

Tables 4.4 and 4.5 present a series of cross-tabulations linking the most recent measure of legislative extremism in the Congress preceding the election with the subsequent level of turnout and incumbent electoral support. First, Table 4.4 shows that turnout increases dramatically once we move beyond the 25th percentile of the distribution of legislative extremism. In other words, more moderate candidates tend to be re-elected in elections where turnout is low. On the other hand, Table 4.5 indicates that legislative extremism has a nonlinear, almost convex effect on the incumbent's electoral support. We see that elected House members who have previously adopted a moderate legislative record (below 60 points on the transformed ADA scale) win reelection with an average of 66% of the vote. This level of electoral support declines as we move along the scale; only to increase again when incumbents display a higher level of roll-call extremism (above 93 points on the same scale).

The previous analysis raises two very important questions. First, how can we explain the fact that higher turnout induces incumbents to adopt more extreme roll-call legislative behavior? And second, why does an increase in legislative extremism first reduce and then raise the level of incumbent support? The logic proposed earlier predicted a straightforward negative monotonic relationship between each sets of parameters. In the remainder of this chapter I will argue that in

order to answer these questions, one must take into account the reciprocal causal link that exists between legislative extremism and electoral outcomes.

By definition, any study of incumbent responsiveness must necessarily be based on a sample of *elected* candidates. This selection criterion creates a bias that alters the meaning of the data. By looking at successful incumbents only, the classical expectation of finding a positive correlation between high levels of electoral security and high levels of legislative extremism will be mistaken. It is possible that some "successful" incumbents create safe electoral districts by avoiding extreme roll-call behavior in the first place. Therefore, the relationship between electoral security and legislative behavior may simply be explained by the fact that incumbents who receive higher levels of support are actually more likely to have previously adopted moderate roll-call records. As Canes-Wrone, Brady, and Cogan (2002) indicate, "The vulnerability of the safe members occurs in part because moderate voting increases the probability of holding a safe seat or, in other words, because safety itself derives from a member's roll call positions" (p.138). It is also possible that unsafe districts produce extreme congressmen partially because previous extreme roll-call records may accelerate the making of unsafe districts (for similar argument see Erikson, 1971). Yet, we may still find that certain ideologically homogenous constituencies create secure districts for extreme incumbents, and that heterogeneous, or marginal, districts become safe havens for moderate House members. But if Canes-Wrone, Brady, and Cogan (2002) are correct in assuming that the electorate truly prefers to re-elect legislators who balance their roll-call records, we may still find that incumbents with extreme legislative records are more likely to be punished at the polls.

4.4.1. Electoral accountability models of legislative extremism

In order to disentangle between these simultaneous effects, any model that aims to measure the influence of candidates' vote shares on future legislative behavior will have to account for the fact that marginal incumbents may be electorally vulnerable because of their previous roll-call behavior. If past legislative behavior is any indication of future voting patterns (and the congressional literature seems to point in that direction; see, e.g., Poole and Rosenthal, 1997, and Poole, 2005), we should find this to be true. By extension, it is very likely that higher levels of turnout will also be associated with extreme roll-call behavior. This follows logically from the previous argument, which stated that political extremism causes more competitive elections. These predictions run counter to some of the most common arguments found in the responsiveness literature. That literature holds that electorally secure incumbents will adopt more extreme legislative behavior while marginal incumbents will moderate theirs (e.g., Bartels, 1991; Miller & Stokes, 1963). Still, the first series of regressions, presented in tables 4.6 and 4.7, demonstrate the accuracy of the theory of electoral accountability formulated above.

Table 4.6

Accountability Models of Legislative Extremism, Presidential Vote, and Closeness of the Race on Turnout by Election, OLS Regression 1974–2000

y=*Turnout at time* t	*Presidential vote (last presidential election)*	*Legislative extremism ADA (t-1)*	*Closeness of race (t-1)*	*R-square*
1974 (n=351)	.079 (.049)	.114 (.028)***	.064 (.021)***	.140
1976 (n=359)	-.013 (.045)	.103 (.024)***	.044 (.014)***	.084
1978 (n=333)	-.561 (.054)***	.143 (.017)***	.025 (.015)*	.332
1980 (n=346)	-.592 (.083)***	.136 (.020)***	.030 (.019)	.239
1982 (n=192)	-.061 (.054)	.137 (.025)***	.029 (.021)	.134
1984 (n=212)	-.123 (.058)**	.216 (.033)***	.053 (.020)***	.212
1986 (n=369)	-.046 (.041)	.087 (.025)***	.023 (.016)	.037
1988 (n=384)	-.016 (.042)	.092 (.031)***	.058 (.017)***	.042
1990 (n=376)	-.217 (.056)***	.154 (.027)***	.026 (.014)*	.085
1992 (n=149)	.007 (.105)	.035 (.043)	.069 (.021)***	.051
1994 (n=356)	-.347 (.043)***	.097 (.021)***	.025 (.013)*	.176
1996 (n=369)	-.333 (.045)***	.051 (.026)**	.024 (.015)*	.150
1998 (n=365)	-.265 (.036)***	.084 (.032)***	.022 (.012)*	.112
2000 (n=337)	-.351 (.048)	.015 (.036)	.019 (.031)	.155
1974–2000 (n=4498)	-.164 (.041)***	.125 (.007)***	.041 (.005)***	.345
1980–2000 (n=3327)	-.174 (.014)***	.034 (.008)***	.018 (.005)***	.539

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p <. 01. The cumulative sample also includes dummies for election years (1974 is the baseline; 1972 is excluded because 1968 election results are not included in the sample). In 1982, 1984, and 1992 the model excludes all the districts in states where there was a reapportionment. Uncompetitive races are removed from the sample. The last model (1980–2000) includes controls for challenger quality, incumbent and challenger spending, presidential years elections, Southern states, number of terms served by the incumbent, whether the incumbent is Democrat, and a lagged measure indicating whether turnout was high in the last election. The spending data is only available after 1980. See Appendix 4.A for detailed description of variables employed in the model.

Table 4.7

Accountability Models of Legislative Extremism, Presidential Vote, and Closeness of the Race on Two-party vote by Election, OLS Regressions 1974–2000

y=*Incumbent vote at time* t	*Presidential vote (last presidential election)*	*Legislative extremism ADA (t-1)*	*Closeness of race (t-1)*	*R-square*
1974 (n=306)	-.193 (.056)***	-.042 (.031)	-.052 (.024)**	.113
1976 (n=321)	.125 (.037)***	-.070 (.021)***	-.091 (.012)***	.165
1978 (n=306)	.616 (.048)***	-.062 (.020)***	-.072 (.019)***	.352
1980 (n=332)	.611 (.052)***	.007 (.018)	-.060 (.017)***	.316
1982 (n=173)	.263 (.069)***	-.119 (.039)***	-.085 (.018)***	.201
1984 (n=187)	.416 (.060)***	-.035 (.039)	-.095 (.015)***	.378
1986 (n=314)	.136 (.046)***	-.078 (.027)***	-.127 (.014)***	.210
1988 (n=322)	.239 (.036)***	-.045 (.025)***	-.081 (.017)***	.190
1990 (n=308)	.430 (.053)***	-.128 (.026)***	-.037 (.017)***	.230
1992 (n=144)	.280 (.096)***	-.089 (.043)**	-.067 (.022)***	.135
1994 (n=318)	.399 (.050)***	.053 (.026)**	-.057 (.015)***	.283
1996 (n=357)	.727 (.034)***	-.086 (.028)***	-.064 (.010)***	.623
1998 (n=299)	.594 (.032)***	-.113 (.024)***	-.050 (.011)***	.577
2000 (n=296)	.653 (.030)***	-.125 (.031)***	-.095 (.012)***	.631
1974–2000 (n=3983)	.301 (.014)***	-.067 (.007)***	-.081 (.004)***	.220
1980–2000 (n=3042)	.456 (.017)***	-.069 (.008)***	-.027 (.006)***	.578

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p <. 10; **p < .05; ***p < .01. The cumulative sample also includes dummies for election years (1974 is the baseline; 1972 is excluded because 1968 election results are not included in the sample). In 1982, 1984, and 1992 the model excludes all the districts in states where there was a reapportionment. Uncompetitive races are removed from the sample. The last model (1980–2000) includes controls for challenger quality, incumbent and challenger spending, presidential years elections, Southern states, number of terms served by the incumbent (the spending data is only available after 1980), whether the incumbent is a Democrat, whether the incumbent is a member of the president's party (coded as *in party*), a measure of presidential popularity (latest Gallup poll before the election; multiplied by -1 if incumbent is not in the president's party), and a midterm loss measure indicating whether the incumbent candidate shares the same party as the president in midterm elections only. See Appendix 4.A for detailed description of variables employed in the model.

Both tables report the results for the models testing the legislative accountability hypotheses; mainly to establish whether the electorate holds incumbents accountable for their past roll-call behavior. The results indicate the outcomes of ordinary least square regressions with robust standard errors. The regressions are conducted in each election years, and also in two different

pooled cross-sectional models (1972–2000) with numerous control variables (the description of the pooled, cross-sectional models and variables can be found in Appendix 4.A).[24]

In the first series of analyses, turnout is simply regressed on three cofactors: previous presidential vote share, a measure of incumbent electoral vulnerability (closeness of the previous race), and the scale of roll-call legislative extremism (absolute ADA score coded by party affiliation). The results demonstrate that the level of presidential support (conceptualized as the ideological distribution of the district) has a negative influence on turnout in almost all of the elections under study. In contrast, the closeness of the previous election race appears to increase turnout by 2 to 6 percentage points on average, depending on the election year. But the most important finding, included in Table 4.6, is related to the association between legislative extremism and turnout. In each election, this relationship is positive and almost always significant (except in 1992 and 2000). The results appear to be robust since the legislative extremism variable remains significant in the two pooled cross-sectional models which include numerous additional control variables (see Appendix 4.A for details). Thus we can accept as valid the first research hypothesis, which stipulates that *extreme roll-call legislative behavior increases turnout in the following election.*

Now, if we look at the impact of past legislative roll call extremism on the incumbent's vote share (Table 4.7), we find that closeness of the previous election has a negative influence on the level of incumbent support, reducing the vote by as much as 13% in 1986.[25] District ideology has the opposite effect, increasing the vote margin of all the incumbents, except in 1974. Clearly,

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[24] All models presented in the chapter have robust standard errors.

[25] A previous election results was considered to be close if the incumbent won less than 54% of the vote.

like many previous congressional election studies, my analysis confirms the existence of a strong and positive relationship between the incumbents' presidential party vote share (the proxy ideology measure) and the level of electoral security in the district. We also find partial confirmation of the results of Canes-Wrone, Brady, and Cogan (2002) and Erikson and Wright (2000) related to the effects of legislative roll-call extremism on an incumbent's electoral margin. This relationship is negative and significant in almost all the elections from 1974 to 2000. In other words, roll-call ideological extremism reduces the subsequent level of electoral support for the incumbent.[26] For example, using the pooled cross-section model presented in the last row (1980–2000) of Table 4.7, we find that incumbents with a perfect liberal record on the extremism roll-call scale (100 points) could have increased their electoral support by 3.5 percentage points simply by dividing their roll-call votes equally between conservative and liberal positions (a score of 50 points on the ADA scale). The previous results, in conjunction with the other analyses presented in Table 4.7, confirms the second research hypothesis which stipulated that *more extreme roll-call legislative behavior reduces an incumbent's level of electoral support.*[27]

Of course, it is the effect that representing the *party* extreme has on incumbency support that the models look at, not the effect of representing the extreme of the constituency. Sullivan

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[26] However, there are some notable exceptions, especially in 1994, when more extreme incumbents actually fared better at the polls ($p < .05$). This is probably explained by the conditions that led to the Republican takeover of Congress.

[27] I believe that the conclusions of Canes-Wrone, Brady, and Cogan (2002) and of Erikson and Wright (2000) about the effects of extremism on electoral security are correct. The reason we do not see this relationship as significant in all election years is most certainly related to differences in coding methodology, especially in the codification employed for the district ideology measure. As was pointed out earlier, for simplicity and clarification purposes I have adopted a very simple and straightforward codification scheme. I want the reader to have a clear understanding of how the variables included in the model interact with the dependent variables.

and Ulsaner (1978), Ansolabehere, Snyder, and Stewart (2001), and Erikson and Wright (1980) correctly explain that being moderate relative to fellow legislators does not logically imply moderation relative to one's district. Furthermore, Schmidt, Kenny, & Morton (1996) also show that the average party position in a state can have an impact on the legislative behavior of incumbent Senators. This is why all of these studies include both a measure of challenger and incumbent ideology in their analyses. Mainly because the authors want to assess whether opposing candidates converge on the electoral median, as the spatial theory of voting would predict. The problem with this type of analysis is of course that challenger ideological positioning is not easy to measure. Usually, scholars conduct opinion surveys of major party candidates directly, but more original measures have also been employed (Erikson & Wright, 1980; Sullivan & Ulsaner, 1978; Ansolabehere, Snyder, & Stewart, 2001).[28] For instance, the most exhaustive analysis of candidate positioning to date, that by Ansolabehere, Snyder and Stewart (2001), compares the legislative records of incumbents who face each other in specific elections but who may have served at different times in Congress. The authors conclude that there is an enduring pattern of candidate divergence in the U.S. House between 1874 and 1996. Ansolabehere, Snyder, and Stewart even specify that "the choices voters face locally mainly reflect national positions of the party" (p.152).

Nevertheless, in order to control for the possibility that extremism vis-à-vis one's constituency, rather than extremism vis-à-vis the incumbent's own party, could be the driving force behind the results presented above, the Appendix includes a replication of Ansolabehere,

[28] Some authors use more original methods to estimate challenger's ideological location. For example, Fowler (2005) compares past legislative records of challenging candidates of the House who move to the Senate.

Snyder, and Stewart's analysis. The analysis integrates some of their data containing the ideological location of both major party candidates in the 1996 election. The results (presented in Appendix 4.B) show that incumbent extremism increases the vote share of the challenger candidate; but the reverse is also true, the more extreme the incumbent (challenger), the greater the level of support for the challenger (incumbent).[29] Even by adding a measure of challenger ideology, the analysis demonstrates, at least for 1996, that ideological extremism—both in relationship to the party and to the challenger candidate—reduces electoral support for incumbent candidates.

4.4.2. Legislative responsiveness models of electoral turnout and competitiveness

We now turn to the responsiveness model of representation, which looks at the influence of electoral demands on roll-call legislative extremism. In the analyses presented in Table 4.8, the roll-call ideological extremism measure (the absolute ADA score coded by party affiliation) becomes the dependant variable. Most of the results indicate that turnout has a positive influence on legislative extremism (except in 1986 and 1998). The same relationship holds when looking at the district ideology measure for all election years under study (the second column of Table 4.8). It appears that districts with a higher than average turnout rate, or districts where the presidential incumbent party vote share is elevated, have a greater likelihood of being represented by extreme legislators. Conversely, we find that the level of incumbent support reduces the level of legislative extremism (except in 1994). This relationship demonstrates that

[29] In addition, when the differences between the roll-call ideological extremism measure of challengers and incumbents are taken into account simultaneously, the results demonstrate that challengers who are more extreme than incumbents will be more likely to obtain lower levels of electoral support (the reverse is also true for incumbents who are more extreme than challengers; see Appendix 4.B for details).

the greater the margin of victory, the more likely an incumbent is to adopt a moderate roll-call record. By extension, the result implies that incumbents elected with small margins of victory are actually more likely to display extreme roll-call behavior in the next legislative session. A priori, it seems that Fiorina (1973, 1974) and Gulati (2004) are correct in concluding that more extreme behavior in the House is associated with candidates who represent marginal districts.

Table 4.8

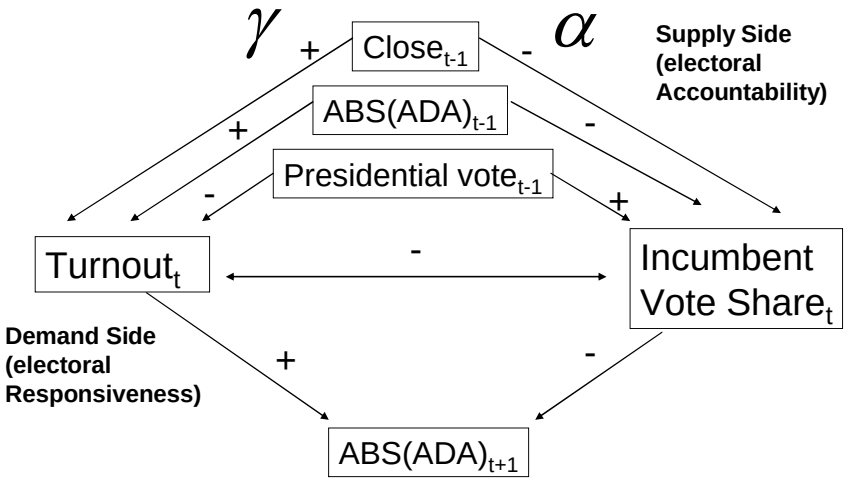
Responsiveness Models of Turnout, Two-party vote Share and Presidential Vote on Legislative Extremism by Election, OLS Regressions 1973–2001

y=*ABS(ADA) at time* t+*1*	*Presidential vote at time* t	*Two-party vote at time* t	*Turnout at time* t	*R-square*
1972 (n=371)	.984 (.061)***	-.461 (.124)***	.176 (.100)*	.402
1974 (n=368)	.751 (.073)***	-.094 (.129)***	.541 (.133)***	.260
1976 (n=375)	.907 (.193)***	-.572 (.191)***	.506 (.152)***	.083
1978 (n=338)	1.056 (.160)***	-.375 (.154)***	.848 (.133)***	.194
1980 (n=353)	.993 (.102)***	-.433 (.125)***	.280 (.094)***	.190
1982 (n=205)	.560 (.092)***	-.351 (.145)**	.374 (.149)**	.177
1984 (n=363)	.951 (.070)***	-.312 (.097)***	.205 (.097)**	.371
1986 (n=349)	.556 (.061)***	-.207 (.095)**	.127 (.097)	.189
1988 (n=347)	1.267 (.105)***	-.572 (.132)***	.284 (.094)***	.365
1990 (n=344)	1.412 (.085)***	-.676 (.129)***	.347 (.101)***	.459
1992 (n=384)	1.329 (.120)***	-.548 (.132)***	.379 (.096)***	.310
1994 (n=363)	.503 (.121)***	.014 (.111)	.318 (.109)***	.083
1996 (n=404)	.845 (.111)***	-.471 (.122)***	.236 (.086)***	.161
1998 (n=332)	1.027 (.125)***	-.626 (.137)***	-.003 (.096)	.286
2000 (n=307)	1.035 (.139)***	-.861 (.169)***	.139 (.065)**	.295
1972–2000 (n=5203)	.894 (.025)***	-.384 (.034)***	.328 (.029)***	.189
1980–2000 (n=3394)	.847 (.031)***	-.373 (.047)***	.050 (.032)	.328

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. The cumulative sample (1972–2000) includes dummies for election years (1972 is the baseline). The second cumulative sample (1980–2000) also includes dummies for election years in the last model with spending (1980 is the baseline). For the 1982 election, the model excludes all the districts in states where there was a reapportionment. Uncompetitive races are removed from the sample. The last two model (1980–2000) includes controls for challenger quality, incumbent and challenger spending, presidential years elections, Southern states, number of terms served by the incumbent, and whether the incumbent is a member of the Democratic Party (the spending data is only available after 1980). See Appendix 4.A for detailed description of variables employed in the model.

This counterintuitive result is best summarized in Figure 4.1, where the accountability model's variables (presidential vote, closeness of the race, and the measure of legislative extremism) are presented in a series of causal pathways linking turnout and incumbent vote share (the supply side of representation). Figure 4.1 also shows the subsequent relationship between turnout and incumbent vote share on the transformed ADA measure (the demand side of representation). The positive and negative signs between the cofactors in the figure correspond to the signs of the partial regression coefficients found in the pooled models of tables 4.6, 4.7, and 4.8.

Figure 4.1. Causal pathway between accountability and responsiveness.



The figure can help us determine whether lower levels of turnout reduce political extremism, or rather whether lower levels of political extremism reduce turnout. When looking at the relationship between legislative extremism and turnout in the upper section of the figure

(the supply side of the model), we see that the positive relationship between these variables takes on a different meaning when we consider the incumbent's previous levels of political extremism and turnout in the district. Second, the figure also explains why higher levels of incumbent support reduce political extremism, or why an increase in the level of political extremism decreases the level of incumbent support. The figure clearly demonstrates that it is the prior legislative behavior that is affecting the incumbent's vote share. In other words, the reason higher levels of support for the incumbent reduce legislative extremism is that political extremism tends to produce lower levels of electoral support (see α relationship); similarly, the fact that turnout increases extremism is explained by the positive association between higher levels of political extremism and higher levels of turnout (see γ relationship). So incumbents who obtain strong electoral support are more likely to have already adopted a moderate roll-call record. And districts where turnout is high tend to be represented by more extreme legislators.

4.4.3. Freshmen models of legislative responsiveness

Now for the previous explanation to make sense, we need to make sure that the direction of the causal relationships between these variables is correct. This requires the removal of any previous effects that legislative behavior may have had on turnout and incumbent vote share in the first place. This explains why I have added an analysis that pools all candidates who have been elected for the first time and are serving their first term (i.e., a sample of freshmen incumbents). In this model, the measures of turnout, presidential vote share, and incumbent two-party vote share are regressed against the transformed ADA measure of all newly elected House members. The results of this analysis are presented in Table 4.9.

Table 4.9

Responsiveness Models of Turnout, Two-party vote Share and Presidential Vote on Legislative Extremism for Freshmen Candidates Only, OLS Regressions 1972–2000

y=*ABS(ADA) (t+1)*	*Presidential vote at time* t	*Two-party vote at time* t	*Turnout at time* t	*Previous legislative record*	*R-square*
Model 1: Freshmen incumbents (n=825)	.761 (.069)***	-.343 (.113)***	.322 (.077)***	-	.183
Model 2: Incumbent party candidate win open seat election (n=324)	.311 (.106)***	.089 (.119)	.238 (.103)**	.568 (.044)***	.605
Model 3: Opposing candidate win open seat election (n=145)	.296 (.155)*	-.012 (.283)	.006 (.111)	-.221 (.067)***	.305
Model 4: Candidate defeat the incumbent (n=212)	.575 (.220)***	-.230 (.410)	.323 (.172)*	-.173 (.059)***	.164

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. Cumulative sample includes dummies for election years (1974 is the baseline). In the models which include the incumbent previous legislative record, the regressions exclude all the districts in states where there was a reapportionment in 1982, 1984 and 1992. Uncompetitive races are removed from the sample.

Just as in the previous responsiveness models, the results show that the relationship between two-party vote share and the level of roll-call extremism for freshmen incumbents is negative. The first equation in model 1 demonstrates that the higher the two-party vote for the freshman candidate, the more moderate the subsequent legislative record. This result raises some doubts about the validity of the explanation presented earlier in this section, since it appears that even freshmen legislators who won with a small margin adopt a more extreme roll-card record. However, before we can conclude that electoral marginality induces legislative extremism in this setting also, it is necessary to control for a potential confounding factor in the analysis. It is possible that the roll-call records of retiring representatives, especially successful ones, could

still influence the incoming candidate's behavior in the House. Stratmann (2000) has a similar argument about junior legislators who take cues from same-party colleagues. The argument made here is simply an extension of this, since incoming lawmakers can also be expected to take cues from the previous legislative record of exiting incumbents. In effect, it makes sense to assume that freshmen lawmakers will reproduce some of the behavior of their predecessors, especially if they replace a successful incumbent from the same party. On the other hand, it is possible that a freshman representative who has just defeated an incumbent, or its party's nominee (in the case of open seat elections), might chose to adopt a slightly different legislative agenda, simply to avoid a similar electoral fate. This is precisely what happens when challenger candidates use past roll-call votes in their electoral campaigns to criticize incumbent candidates (Jacobson, 2004).

Table 4.9 tests for this possibility by including various samples of freshmen elections. The first sample is limited to all freshmen candidates who share the same party as the exiting incumbent and won an open-seat election (model 2). The second sample includes freshmen candidates who have won an open-seat election and defeated the incumbent's party nominee (model 3). And the third sample is composed of all the freshmen House members who have defeated a returning incumbent candidate (model 4). Controlling for redistricting and reapportionment, Table 4.9 also includes a series of models in which the measure of roll-call behavior is regressed against the previous legislative record of the former incumbent in the district.

At first glance, one of the most important findings of this table is related to the negative partial regression coefficients in models 4 and 5. This negative sign implies that freshman

candidates who replace moderate incumbents tend to subsequently adopt a more extreme legislative record, whereas freshman candidates who replace extreme incumbents tend to subsequently adopt a more moderate legislative record ($p < .01$ in both models). Inversely, the strong positive sign of the coefficient in the third model, which includes all freshmen representative who replace members of their own party, indicate that new incumbents tend to replicate the behavior of their predecessors. It is also interesting to note that the two-party vote share variable does not reach the minimum significance level in any of the models under study (the effect is positive but not significant in the third model and negative in the fourth and fifth models). As these results suggest, extremism is not dictated by electoral marginality, at least for freshmen incumbents. Rather, it seems that moderation/extremism is not induced by electoral security/vulnerability; it is rather a combination of district ideology and former legislative behavior that is influencing lawmakers.

The preceding results are confirmed even when we follow the legislative careers of all freshmen candidates who entered the House by winning a new seat for their party. Table 4.10 report such an analysis by pooling all candidates who defeated incumbents or the incumbent party's nominees into a unique sample. Once again, turnout, presidential vote share, and incumbent two-party vote share are regressed on the transformed ADA measure. However, this sample is stratified by the number of terms served by the incumbents. The model also includes a measure of past legislative behavior, but only for representatives serving their first term (since incumbents will be influenced by their own behavior afterward). So the first row of the table represents all freshmen candidates who entered the House by defeating an incumbent (or the incumbent's party candidate). The second row follows the same group of representatives, but it

only includes those who have been re-elected and are serving their second term, and so forth. Here again, the negative partial regression coefficient (-.212) indicates that previous extreme legislative records have a moderating effect on the freshmen's roll-call votes (we find that the reverse is also true since previous moderation induces roll-call extremism). What is most striking is that the effect of the two-party vote share variable does not become significant until after the incumbent has served more than two terms. If indeed extreme legislators were more likely to represent marginal districts, the results would not indicate that challengers who defeat some of these extreme incumbents subsequently adopt a more moderate legislative record.

Table 4.10

Responsiveness Models of Turnout, Two-party vote Share and Presidential Vote on Legislative Extremism by Number of Terms Served. The Data Follows the Career of Candidates who defeated the Incumbent or the Incumbent Party Candidate in Open Seat Elections, OLS Regressions 1972–2000

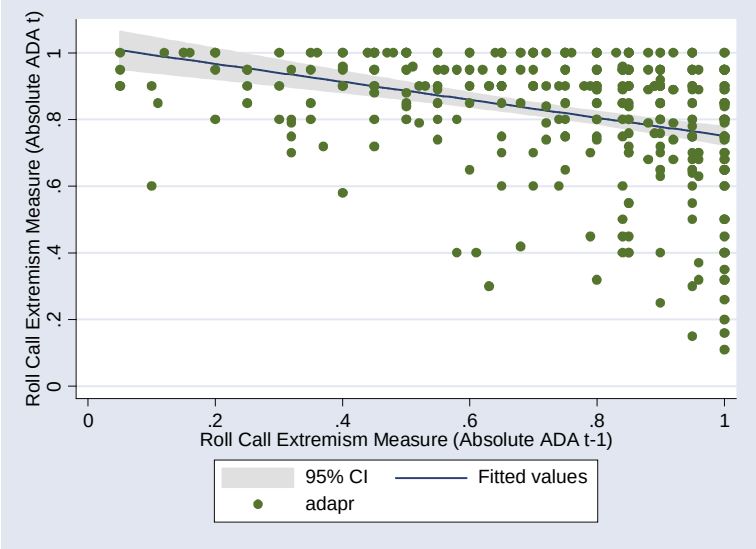
y=*ABS(ADA) (t+1)*	*Presidential Vote at time* t	*Two-party vote at time* t	*Turnout at time* t	*Previous Legislative Record*	*R-square*
Term 0 (n=357)	.459 (.134)***	-.122 (.229)	.167 (.110)***	-.212 (.038)***	.302
Term 1 (n=303)	.732 (.129)***	-.160 (.155)	.139 (.124)	-	.319
Term 2 (n=269)	.801 (.113)***	-.445 (.175)**	.111 (.118)	-	.299
Term 3 (n=185)	.906 (.147)***	-.328 (.197)*	.439 (.135)***	-	.171
Term 4 (n=123)	1.042 (.177)***	-.576 (.232)**	.209 (.192)	-	.178
Term >=5 (n=305)	.975 (.120)***	-.375 (.174)***	-.053 (.126)	-	.248

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. Cumulative sample includes dummies for election years (1974 is the baseline). In the models which include the incumbent's previous legislative record, the regressions exclude all the districts in states where there was a reapportionment in 1982, 1984, or 1992. Uncompetitive races are removed from the sample. This table follows the careers of incumbents who entered the House by defeating an incumbent or by beating the incumbent party in an open-seat election.

In order to make sense of the previous findings, figures 4.2 and 4.3 represent the simple correlation between the roll-call records of departing incumbents (at time *t*-1) and incoming

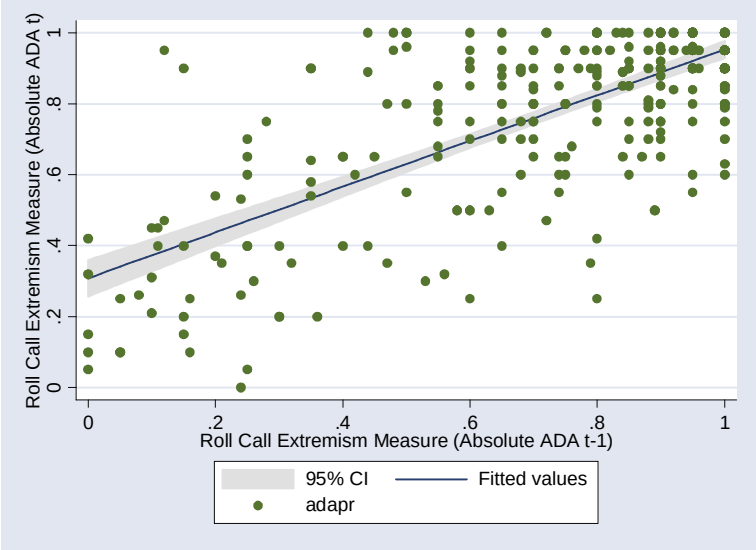
representatives (at time *t*). Figure 4.2 includes a sample of all freshmen who defeated an incumbent or the incumbent's party candidate. In contrast, the data in Figure 4.3 contains all the freshmen candidates who entered the House by replacing a departing member of their party. As we can see in Figure 4.2, the more extreme incumbents (situated at the end of the *x* axis) tend to be replaced by moderate representatives (closer to the origin of the *y* axis). Figure 4.3 demonstrates that newly elected candidates sharing the same party as the incumbents tend to follow their predecessors' legislative record.[30]

Figure 4.2. Correlation between former roll-call extremism and freshmen roll-call extremism when incumbent or incumbent party candidate lost (n=357).



[30] The partial regression coefficient of this equation (Figure 4.3) is equal to .644, which is a strong but not a perfect correlation. However, once we include the intercept (which is situated at .308), we see that replacing incumbents are more extreme than exiting incumbents from the same party until the scale reaches about 90 points.

Figure 4.3. Correlation between former roll-call extremism and freshmen roll-call extremism when incumbent or incumbent party won (*n* = 328).



To sum up, the preceding results presented in tables 4.9 and 4.10 run contrary to what Gulati (2004) wrote about Senate races which stipulated that "marginal legislators position themselves among their core constituencies rather than pursue an electoral strategy that rests on attracting moderate, independents, and other swing voters" (p. 510; also see Fiorina, 1973, 1974). Rather, the findings presented above demonstrate that previous roll-call records are more likely to be the cause of marginal districts in the first place. An important exception to this rule was found when the analysis focused on challengers who entered the House by defeating an incumbent (or the incumbent party's candidate). In these cases, it was shown that moderates were more likely to be replaced by extreme representatives, and that extreme incumbents were more likely to be replaced by moderate representatives.

I believe that the results presented above provide enough ground to reject the fourth research hypothesis, which stated that *districts with a high level of incumbent support are more*

likely to be represented by extreme legislators. It is true that the empirical analysis began by demonstrating that marginal districts were more likely to be represented by extreme legislators. However, it was subsequently shown that past legislative behavior was what caused marginal districts in the first place. Once the sample was limited to newly elected incumbents, the effect of two-party vote share basically vanished and the presidential vote measure and the district legislative records were identified to be the principal explanatory factors behind the orientation of subsequent roll-call votes.

4.4.4. Responsiveness models of turnout by party affiliation

It is important to note at this point that none of the previous results explain why the influence of turnout on legislative roll-call extremism remains positive in almost all of the models included in the analysis. In the earlier section of this chapter, it was first argued that an increase in turnout would reduce legislative extremism. However, the empirical evidence presented so far does not support this hypothesis, even when the data isolates previous legislative record from the equation and considers only freshmen incumbents. The results suggest that in all types of congressional elections, higher levels of participation increase roll-call extremism. But before we can conclude this, we need to consider the possibility that an endogenous relationship exists between turnout and electoral security; and that this relationship is affecting the results in turn (see the arrow connecting turnout and incumbent in Figure 4.1).

In chapter 3, it was demonstrated that higher turnout could actually decrease the level of electoral support for an incumbent candidate. It was also shown that this relationship is endogenous since highly competitive elections are generally associated with higher levels of turnout in the first place. So even if the analysis limits the sample to freshmen members of

Congress, there remains a distinct possibility that a small margin of victory might be associated with a higher level of turnout, and this would explain why higher participation rates actually increase legislative extremism.

The analysis tests for this possibility by conducting a two-stage least square regression which includes an instrumental variable for turnout. The results are reported in Appendix 4.C. At length, the outcome of this model demonstrates that a *cleaned* measure of turnout remains positively associated with post-electoral legislative behavior—meaning that increased levels of turnout add to legislative extremism, regardless of the level of district competitiveness.[31] It may well be that the instrumental measure is imperfect (with some cofactors still correlated with vote share and legislative behavior). But in the remainder of this chapter I will argue for an alternative explanation. This account will hopefully clarify why we find that higher participation rates induce incumbents to adopt more extreme roll-call behavior—even *after* we control for the endogenous relationship between these variables.

In order to understand the positive association between turnout and legislative extremism, it is necessary to return to some of the results presented in chapter 2. To restate the principal conclusions, we found that nonvoters usually hold slightly more liberal views than voters, when we looked at a series of public opinion surveys. The analysis also identified a gap between the party identification and the ideological preferences of both groups: nonvoters were more likely to be liberal and to identify with the Democratic Party. Concretely, the results in chapter 2 indicated that higher turnout could theoretically lead to an increase in the proportion of lower socio-

[31] This will be true to the extent that a valid instrument for turnout is found. The instrumental variables used in the Appendix are not completely independent of electoral competitiveness. However, since the socio-demographic variables used are measured only at each decade, they do provide a somewhat isolated measure of turnout in each election year (the same argument was made in the previous chapter).

economic status voters and non-partisan voters, which will have a different effect in Democratic and Republican districts. So in House districts controlled by Republicans, higher turnout could in fact dilute the conservative vote, whereas in Democratic constituencies, a higher level of turnout could increase the number of voters who prefer liberal policies even more.

In order to test for this possibility, Table 4.11 reproduces the responsiveness models of Table 4.8, where the dependant variable is the measure of roll-call ideological extremism and the independent variables are turnout and two-party vote share. The main difference in this model is that the sample is clustered by party affiliation.[32]

[32] I have also clustered the accountability models by party affiliation, but the results are not significantly different than what we find in tables 4.6 (turnout) and 4.7 (incumbent support).

Table 4.11

Responsiveness Models of Turnout, Two-party vote Share and Presidential Vote on Legislative Extremism by Election and by Party Affiliation, OLS Regressions 1972–2000

Party affiliation	y=*ABS(ADA) (t+1)*	*Presidential vote at time* t	*Two-party vote at time* t	*Turnout at time* t	*R-square*
Democrats	1972 (n=193)	1.418 (.152)***	-.725 (.204)***	.599 (.152)***	.484
	1974 (n=230)	1.416 (.144)***	-.436 (.173)**	.985 (.169)***	.456
	1976 (n=240)	.742 (.261)***	-.248 (.246)	.534 (.223)**	.043
	1978 (n=209)	1.457 (.192)***	-.382 (.201)*	1.116 (.176)***	.221
	1980 (n=187)	.999 (.190)***	-.450 (.254)*	.451 (.169)***	.106
	1982 (n=115)	.355 (.207)*	-.111 (.213)	.881 (.179)***	.199
	1984 (n=197)	.581 (.132)***	-.417 (.189)**	.581 (.132)***	.388
	1986 (n=194)	.899 (.111)***	-.463 (.156)***	.286 (.130)**	.256
	1988 (n=196)	1.401 (.160)***	-.555 (.219)**	.651 (.133)***	.427
	1990 (n=215)	1.427 (.119)***	-.565 (.183)***	.448 (.135)***	.397
	1992 (n=224)	1.721 (.130)***	-.650 (.142)***	.461 (.113)***	.516
	1994 (n=181)	1.601 (.188)***	-.760 (.188)***	.312 (.119)***	.467
	1996 (n=197)	1.660 (.174)***	-.885 (.175)***	.422 (.108)***	.466
	1998 (n=169)	1.075 (.201)***	-.610 (.210)***	.170 (.133)	.270
	2000 (n=158)	1.006 (.154)***	-.777 (.176)***	.179 (.094)*	.300
	1972–2000 (n=2905)	1.274 (.042)	-.542 (.052)***	.576 (.040)***	.382
	1980–2000 (n=1869)	1.034 (.048)***	-.613 (.065)***	.107 (.041)***	.489
Republicans	1972 (n=178)	1.571 (.297)***	-.552 (.187)***	-.088 (.162)	.256
	1974 (n=138)	1.274 (.399)***	-.700 (.252)***	-.443 (.254)*	.245
	1976 (n=135)	.960 (.250)***	-.323 (.222)	-.639 (.179)***	.134
	1978 (n=129)	.304 (.199)	-.264 (.153)*	.156 (.138)	.144
	1980 (n=166)	.887 (.167)***	-.125 (.116)	-.343 (.106)***	.141
	1982 (n=90)	1.094 (.222)***	-.667 (.259)**	-.511 (.165)***	.301
	1984 (n=166)	1.374 (.204)***	-.484 (.123)***	-.321 (.131)**	.324
	1986 (n=155)	1.389 (.310)***	-.491 (.158)***	.118 (.144)	.319
	1988 (n=180)	1.715 (.223)***	-.819 (.170)***	-.003 (.137)	.353
	1990 (n=129)	1.491 (.249)***	-1.003 (.227)***	.273 (.153)*	.481
	1992 (n=160)	.804 (.179)***	-.300 (.223)	-.220 (.135)	.169
	1994 (n=182)	.603 (.126)***	-.285 (.104)***	-.128 (.097)	.136
	1996 (n=207)	1.150 (.165)***	-.669 (.142)***	-.317 (.113)***	.300
	1998 (n=163)	1.499 (.215)***	-.638 (.173)***	-.314 (.121)**	.354
	2000 (n=149)	1.063 (.240)***	-.677 (.315)**	-.109 (.078)	.335
	1972–2000 (n=2298)	1.141 (.059)***	-.495 (.047)***	-.200 (.035)***	.261
	1980–2000 (n=1525)	1.138 (.070)***	-.565 (.076)***	-.093 (.046)**	.315

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. The cumulative sample (1972–2000) includes dummies for election years (1972 is the baseline). The second cumulative sample (1980–2000) also includes dummies for election years in the last model with spending (1980 is the baseline). For the 1982 election, the model excludes all the districts in states where there was a reapportionment. Uncompetitive races are removed from the sample. The last model (1980–2000) includes controls for challenger quality, incumbent and challenger spending, presidential years elections, Southern states, number of terms served by the incumbent, and whether the

incumbent is a member of the Democratic Party (the spending data is only available after 1980). See Appendix 4.A for detailed description of variables employed in the model.

Table 4.11 shows that for Republican and Democratic incumbents, district ideology increases roll-call extremism and that the level of electoral support has the opposite effect. This is basically the same result as in the general responsiveness model of Table 4.8. However, turnout has a very different influence on roll-call ideological extremism depending on the party affiliation of the incumbent.

For Democrats, the influence of turnout on legislative extremism remains positive and mostly significant in all the elections covered by the study (only 1998 is not significant). On the other hand, for Republican incumbents, higher turnout actually reduces the proportion of conservative roll-call votes. For example, in the 1974 election year, an increase of 10% in the turnout level would have resulted in a 6 point reduction in the extremism roll-call scale of a Republican incumbent, which is enough to change the outcome of one vote on the ADA scale.[33]

It is important to note that turnout does not have a negative influence on Republican roll-call extremism in all the elections covered by the study. The relationship is negative in 12 of the 15 models (7 are significant). If we control for the endogenous relationship that exists between turnout and competitiveness using the two-stage least square model presented earlier, we find that this same relationship is negative in 13 of the 14 models under study (9 are significant, see Appendix C, Table 4.C3). In contrast, all of the relationships between turnout and legislative behavior are positive for the Democratic elections, and 13 of the 14 models are significant. The fact that we confirm this relationship even after including an instrument for political participation

[33] Remember that the ADA scale is generally based on 20 votes of the House or the Senate.

adds support to the claim that roll-call extremism increases with turnout in Democratic districts and declines in Republicans districts under similar circumstances.

The observed difference between the pooled and the partisan clustered samples can best be explained by the strength of the relationship between turnout and legislative behavior in Democratic districts. As Table 4.11 demonstrates, the average value of the partial slope coefficient for turnout in the first series of regressions is at .54, which is much higher than the Republican values (-.19) and the original pooled sample of Table 4.8 (.32). As a consequence, when both Republican and Democratic districts were indiscriminately included in the same regression, the strength of the turnout coefficient for Democratic districts (which were more numerous than the Republican districts in most of the election years under study) diluted the negative relationship found between turnout and representation in Republican districts. This explains why we found positive partial regression coefficients in the pooled samples. In the latter analyses, I assumed that Republican and Democratic incumbents reacted similarly to an increase in political participation. But this assumption proves too hard to sustain since we know that an increase in the number of voters can send a different signal to Republican and Democratic lawmakers (see Lupia, Levine, Menning, & Gisela, 2006 for similar argument about how information affects public opinion).

The association between these variables validates the theoretical expectation outlined above, which stipulated that higher turnout would increase in the number of liberal voters and induce Republican incumbents to moderate their roll-call records. For Democratic incumbents, an increase in turnout actually means more extreme roll-call behavior since higher participation rates adds to the existing percentage of liberal voters in the district.

It is possible to illustrate this relationship with a simple model in which the average ideological position of a constituency is determined by the weighted sum of all conservative and liberal voters. Here, the weight actually represents the average individual propensity to vote for each group. So the mean district preference can be given by the following equation:

(4) Republican and Democratic district average ideological position = $$\frac{\alpha\left(\sum_{i=1} L_i\right) + \gamma\left(\sum_{i=1} C_i\right)}{N}$$

Without loss of generality, assume that conservatives and liberals have the following ideological preferences: L for liberals and C for conservatives located at 3 and 5 on the standard 7 points ideological scale (where 1 is extreme liberal and 7 is extreme conservative). In this equation, the weight α is the turnout rate among the liberal voters and γ is the turnout rate among conservative voters ($\alpha, \gamma = [0,1]$). If $\alpha < \gamma$ in all cases, $\sum_{i=1} C_i > \sum_{i=1} L_i$ in Republican districts, and $\sum_{i=1} C_i < \sum_{i=1} L_i$ in Democratic districts (since all incumbents have been re-elected), we should find that an increase in α will add to the proportion of liberal voters in both types of constituencies. However, the district average ideological position for Democrats will move away from the center and closer to the extreme left. And in Republican districts, an increase in α will add to the proportion of liberal voters, except that the average ideological position of the constituency will move away from the conservative mean and closer to the ideological center.[34]

[34] For instance, if the average position of conservative voters is at 5 and liberal voters at 3, the average will be at 4 on the 7 point scale assuming that both groups are of equal size in the electorate. Assuming that the district has 200,000 voters, and that only 50% of the liberals turn out to vote, the average district ideology will be at 4.3; however, if turnout increase to 75% for liberals, the mean district ideology will be at 4.14.

Of course, the principal assumption of this basic model is that $\alpha < \gamma$. But based on the research presented in the second chapter, it seems plausible that, on average, higher turnout rates increase the percentage of voters who favor more liberal policies. This overall trend provides an explanation for why we find that incumbent Democrats who represent districts with high turnout tend to have extreme legislative records, and why Republicans who represent districts with lower levels of turnout tend to have more extreme records. Thus we can partially confirm the third research hypothesis. For Republican incumbents it appears that *districts with a high level of turnout are more likely to be represented by moderate legislators.*

4.5. Discussion

This chapter represents the first attempt to measure the effect of turnout on legislative behavior in the U.S. House of representatives between 1972 and 2000. The analysis began by testing a basic theory of policy responsiveness that predicted that vote-maximizing lawmakers would adapt their roll-call voting to the preferences of a majority of their constituents. Since the location of the district median is a function of the number of constituents who turnout on Election Day, it was also anticipated that lower turnout rates would induce lawmakers to change their legislative behavior—principally because people who have a higher probability of voting hold more extreme policy preferences (for similar argument about party activists, see Aldrich, 1983a, 1983b, 1995). As a consequence, this chapter argued that higher turnout, and by extension electoral competitiveness, would push lawmakers to moderate their roll-call record in order to get re-elected.

Although the empirical evidence presented above did not entirely support all of the research hypotheses, it was possible to shed some light on some of the most interesting puzzles found in the congressional literature today. It was first demonstrated that districts where turnout was high tended to be represented by more extreme legislators. This relationship ran counter to the theoretical arguments presented above. But once the sample of House elections was divided by party affiliation, the hypothesis linking legislative behavior to turnout was partially confirmed, but only for Republican incumbents. Since people who have a lower propensity to vote usually hold slightly more liberal preferences (chapter 2), it was argued that in most conservative districts, higher turnout would be associated with an increase in the number of voters who prefer a higher level of redistribution. This surge in participation would in turn induce legislative moderation among Republican incumbents, mainly because the median is expected to be closer to the ideological center in those districts. By contrast, for Democratic incumbents the analysis showed that higher levels of political participation increased the proportion of liberal voters and encouraged more extreme legislative behavior.

Because turnout is so intrinsically related to electoral competition, the chapter also evaluated two different conceptions of the effect of congressional elections on legislative behavior. The first view held that district competition would incline candidates to moderate their roll-call record in order to represent their constituents' ideological preferences (e.g., Griffin, 2006; Ansolabehere, Snyder, & Stewart, 2001; Erikson & Wright, 2000). By contrast, the second view held that elected officials representing competitive seats would be less responsive to the ideological center (e.g., Huntington, 1950; Bartels, 1991; Fiorina, 1973, 1974; Gulati 2004). I have argued in this chapter that solving this debate requires an approach that integrates both the

incumbent's former and future legislative behavior. Since past legislative behavior is likely to affect the incumbent's future voting record, I maintained that it was necessary to consider the possibility that the relationship between electoral vulnerability and roll-call extremism is simply the consequence of electoral accountability. The results confirmed this expectation by demonstrating that legislative extremism will subsequently increase turnout and reduce the incumbent's vote share.

In short, this analysis established that the relationship between competitiveness and legislative behavior is actually an artifact of the reciprocal causal link existing between the two variables. Once newly elected candidates were isolated from the sample, the results showed that the level of incumbent electoral security had almost no effect on roll-call behavior in districts represented by newly elected candidates. In addition, when a measure of past legislative extremism was added to the analysis, we saw that defeated incumbents who exhibited more extreme legislative behavior tended to be replaced by more moderate candidates; while moderate incumbents were likely to be replaced by more extreme candidates. Finally, when the sample was limited to freshmen incumbents who were elected in districts formerly controlled by their own party, we found that the roll-call record of the former incumbent in the district was a better predictor of subsequent legislative behavior. The freshmen's level of two-party vote share had almost no effect on future roll-call record. This series of results led me to conclude that both of the theories outlined above were incorrect, and that the reason was that they failed to consider the accountability side of electoral representation.

At this stage, I believe it is necessary to formulate a coherent description of the dynamic process linking electoral accountability and legislative responsiveness in the U.S. Congress. This

description is based on my interpretation of the previous findings. To begin with, it appears that newly elected candidates who serve their first term in the House of Representative partially determine their future roll-call behavior by considering two indicators: the ideological composition of the constituency and the previous legislative record found in the district (of course we can also think that legislators are influenced by party leadership and by their own preferences). I believe that the level of electoral security may still play a role, but it is not as important as the previous indicators.[35] If newly elected candidates have just defeated a sitting incumbent (or the incumbent's party candidate), they are more likely to adopt a moderate roll-call record if their predecessors was extreme (in this case the reverse is also true, extreme candidates replace moderate incumbents). However, if freshmen candidates replace the party's outgoing congressmen, they are more likely to adopt a similar legislative record—whether extreme or moderate.[36]

Since we know that an extreme legislative record is likely to reduce electoral support, the question remains why would an incumbent choose to adopt this type of behavior in the first place? Is it because of electoral vulnerability, district ideology, or previous roll-call records? In the sample of House elections covered by the study, 63% of all candidates serving their first term actually adopted a more extreme legislative record than their predecessors.[37] The same trend is also observed when we only consider all the candidates who entered the House by defeating an

[35] Freshmen candidates elected with a very high electoral margin are most likely found in homogenous ideological districts, which would explain why district ideology is the prima facie effect.

[36] One could also argue, like Stratmann (2000), that freshmen candidates are simply following the party line until they form their own expectation about the district preferences, which would explain why more polarized parties produce more extreme freshmen incumbents.

[37] Ansolabehere, Snyder, and Stewart (2001) have also observed the same trend over the last 100 years of congressional elections.

incumbent (or the incumbent's party candidate). In these later cases, 58% of the newly elected challengers adopted a more extreme legislative record. These freshmen were also more likely to be associated with homogenous ideological districts (the mean presidential vote is at 52%), while the remaining 48% of freshmen representatives (the moderates) where more likely to be associated with the ideological minority in their districts (the mean presidential vote is at 46% for those districts). Now, since the average level of electoral support obtained by each of these group is the same (54%), it seems that in these cases at least, district ideology in combination with previous roll-call records, are really the principal forces driving the incumbent's roll-call behavior.[38]

The most fundamental lesson of this chapter is that recently elected incumbents run the risk of diluting their electoral support if they do not moderate their legislative behavior. The results clearly demonstrate that an increase in roll-call ideological extremism is associated with higher turnout and a diminution in the incumbent's vote share. Some challengers seem to enter the House because of an inconsistency between the ideological orientation of their district and the former legislative record of the incumbent. We can think of the 1994 Republican takeover of Congress as a good example. Most of the 54 newly elected members of the House adopted a more extreme legislative record, running the risk of being punished at the polls. And by not altering their roll-call records, the most extreme of these incumbents were eventually removed from office. Illinois' Representative Michael Flanagan (R), who was elected to the 104th Congress with 54% of the vote, but lost the next election to Rod Blagojevich who won with a 14 point margin, is a good example. Flanagan had a score of 85 on the extremism scale, while the

[38] The mean vote for both groups is at 54%. The difference was .0034 greater for more extreme candidates, two tailed t-test, p-value=.401.

district had a 61% Democratic presidential vote share in 1992 (meaning that Flanagan had a 39% conservative base). Flanagan had defeated eighteen-term Congressman and chairman of the Ways and Means Committee, Dan Rostenkowski, who was under indictment at the time of 1992 election. Still, by failing to adopt a moderate legislative record to adjust for the Democratic majority in his district, Flanagan was immediately removed from office in the next election.

Appendix 4.A

Pooled Cross-Sectional Models

Formally, six different equations are estimated (1972–2000 for the reduced forms, and 1980–2000 for full models with spending). To begin with, in the pooled cross-sectional models, turnout in an election at time *t* in congressional district *cd* is said to be a function of the following equation:

$$\begin{aligned} Turnout_{cd,t} &= \beta_0 + \beta_2 pres\,Vote_{cd,t(p)} + \beta_3 Legislative\ extremism_{cd,t-1} + \beta_4 Close_{cd,t-1} + \\ &\beta_5 Ch\ Quality_{cd,t} + \beta_6 Inc\ Spending_{cd,t} + \beta_7 Chal\ Spending_{cd,t} + \beta_8 Year\ president_{cd,t} + \\ &\beta_9 South_{cd,t} + \beta_{10} Nb\ of\ Term_{cd,t} + \beta_{11} Lagged\ Turnout_{cd,t-1} + \beta_{12} Inc\ Democrat_{cd,t} + \\ &\beta_{13} Election\ (dummy)_{cd,t} + \varepsilon_{cd,t} \end{aligned} \quad \textbf{(1)}$$

The incumbent two-party vote share is a function of the following:

$$\begin{aligned} Inc\,Vote_{cd,t} &= \beta_0 + \beta_2 pres\,Vote_{cd,t(p)} + \beta_3 Legislative\ extremism_{cd,t-1} + \beta_4 Close_{cd,t-1} + \beta_5 Ch\ Quality_{cd,t} + \\ &\beta_6 Inc\ Spending_{cd,t} + \beta_7 Chal\ Spending_{cd,t} + \beta_8 Year\ president_{cd,t} + \beta_9 South_{cd,t} + \\ &\beta_{10} Nb\ of\ term_{cd,t} + \beta_{10} Mid\ Loss_{cd,t} + \beta_{11} In\ Party_{cd,t} + \beta_{12} Popularity_{t} + \beta_{12} Inc\ Democrat_{cd,t} + \\ &\beta_{14} Election\ (dummy)_{cd,t} + \varepsilon_{cd,t} \end{aligned} \quad \textbf{(2)}$$

And legislative behavior, following an election (t+1) is a function of this equation:

$$\begin{aligned} Legislative\ extremism_{cd,t+1} &= \beta_0 + \beta_1 pres\,Vote_{cd,t} + \beta_2 Inc\,Vote_{cd,t} + \beta_3 turnout_{cd,t} + \\ &\beta_4 Ch\ Quality_{cd,t} + \beta_5 Inc\ Spending_{cd,t} + \beta_6 Chal\ Spending_{cd,t} + \beta_7 Year\ president_{cd,t} + \\ &\beta_8 South_{cd,t} + \beta_9 Nb\ of\ Term_{cd,t} + \beta_{10} Inc\ Democrat_{cd,t} + \beta_{11} Election\ (dummy)_{cd,t} + \varepsilon_{cd,t} \end{aligned} \quad \textbf{(3)}$$

The temporal outlook in equation 3 is prospective since the model tests for the responsiveness hypothesis. In equations 1 and 2, the temporal outlook is retrospective in order to determine whether incumbent support is influenced by previous legislative behavior.

The measurements of each variable included in the previous model that is not explained in the main section of this paper are as follows:

Challenger Quality: This variable measures challenger quality as reported by the updated Jacobson and Kernell (1983) data. The variable equals 1 if the challenger has previously held elected office, 0 otherwise.

Incumbent and Challenger Spending: This variable measures how much money each candidate spent in the congressional election (as reported by the FEC). Spending reports a measure that takes the natural logarithmic of the challenger spending minus the natural logarithmic of the incumbent candidate (this is the same measure used by Canes-Wrone, Brady, & Cogan, 2002; and Erikson & Wright, 1993). The data is also converted to the 1992 dollar value. In order to account for the fact that candidates are not required to report spending under $5,000, I have assumed, like Canes-Wrone, Brady and Cogan (2002), that each candidate spent at least that much (I did not convert this $5000 value in 1992 dollars).

Presidential Years: Coded 1 for presidential election years, 0 otherwise.

South: A dummy variable indicating whether the district is in a Southern state.

Number of Terms: The number of terms variable is the total number of consecutive congresses in which the incumbent faces reelection, regardless of district geography. This was done in order to control for the creation or suppression of congressional districts. In other words, when counting the number of terms an incumbent served, I only verified that these terms were served in the same state (regardless of district numbers), and that the congressional service was consecutive (in the House). Members elected in special elections were counted as freshmen in the following general election. Terms served prior to 1972 were also counted. Nonlinear effects are controlled for by taking the square and the cube of the number of terms served.

Lagged turnout: Represents the turnout rate in the previous election (for the same congressional district). This variable was included to control for the time-series aspect of our dataset. Turnout is likely to be dependent on the previous turnout rate of the district. Consequently, by adding a lagged measure of participation, the model is capable of controlling for the different distributions of participation rates across congressional districts.

Inc Dem: A variable indicating whether the incumbent is from the Democratic Party. This variable is not included in the fixed-effects models since very few incumbents switch party (it is almost always constant across careers).

Election: A series of dummy variables representing election years: 1980 is the baseline. Election also indirectly control for presidential campaign effects on turnout. This was done to account for the higher turnout rates found in presidential elections. The incumbent support models also include three additional control variables. These cofactors account for different national conditions that may have an impact on congressional election results:

In Party: Indicates whether the incumbent candidate is of the same party as the president.[39]

Mid Loss: Measures whether the incumbent candidate is of the same party as the president in midterm elections only.[40]

Popularity: Reports the most recent Gallup poll measure of the percentage of presidential support preceding an election. If the incumbent is of the same party as the president, this value remains unchanged. If the incumbent is from the out party, this value is multiplied by negative one.[41]

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[39] Same as Brady, Canes-Wrone, and Cogan (2002).

[40] Ibid.

[41] Ibid.

Appendix 4.B

Candidate Positioning Models for the 1996 Election

In order to test the validity of the results associated with the accountability hypothesis, we also include a model that controls for both the incumbent's and the candidate's ideological location. As was previously indicated, testing for challenger location on the incumbent's future legislative behavior does not make a lot of sense. Hence, this model is not used in the responsiveness model.

Knowing that political extremism vis-à-vis one's party reduces electoral support is different than knowing that extremism within the district reduces electoral support. So by demonstrating that incumbents (challengers) who are more extreme than challengers (incumbents) will be punished at the polls, additional support is found for the results of the previous analysis.

In order to accomplish this task, the same data is used that Ansolabehere, Snyder, and Stewart (2001) employed in their analysis of the ideological positioning of House candidates in 1996. The authors used data obtained from The National Political Awareness Survey (NPAT) sent to both major party candidates in the 1996 congressional election. Based on the questionnaire responses, Ansolabehere, Snyder, and Steward scaled over 200 policy questions using principal component analysis. The data was simply downloaded from James M. Snyder's website. The reader is invited to consult their "Candidate Positioning in the U.S. House Elections" paper to get a detailed description of the methodology employed.

The challenger and incumbent positions on the first dimension (which according to the authors is comparable to ADA scores) were simply matched with the existing dataset. The correlation between incumbent ADA score and the scales NPAT score is 63%. On the 0 to 1 NPAT scale, the average extremism score for incumbents is 73% and 74% for challengers (n=274).

In Table 4.B1, we report the results of the accountability model, with both the challenger and incumbent issue positions. The results show that incumbent and challenger issue positions do not significantly affect turnout. However, challenger extremism seems to be positively correlated with incumbent support (but the result is not significant). In addition, when the difference

between the extremism of challengers and incumbents is taken into account (by using the midpoint between both positions), both challengers and incumbents increase their vote share by converging on the middle.[42]

The midpoint results indicate that the incumbent will always fare better the closer he or she gets to the challenger, and the challenger will always do better the closer he or she gets to the incumbent (if the challenger is always on the left of the extremism scale when the incumbent is a Republican). Even tough the midpoint variable is not significant ($p < .125$), we still find that incumbent extremism reduces the vote share when it is included in a model similar to the one presented in the accountability analysis (Table 4.7).

Unfortunately, this brief analysis focuses only on the 1996 election cycle. However, by adding a measure of challenger ideology the models presented below provide a robustness check on some of the results presented in the chapter. First of all, it appears that ideological extremism (as measured by the NPAT survey) will reduce the electoral support of both incumbent and challenger candidates (for challenger candidates, this measure is not significant). And by looking at the difference between the incumbent and challenger candidates in a given district, the analysis demonstrates that ideological extremism will reduce the electoral support equally among candidates.

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[42] The midpoint is simply the sum of the incumbent score and the challenger score (minus 1). This difference is divided by two. So, for instance, if an incumbent had an extremism score of 80 and the challenger of 70, the midpoint would be 55 [80+(1–70)/2]. For instance, if an incumbent has a 90 points extremism measure, and a challenger has a 70 points measure, the midpoint is 60 points and incumbent support will be reduced by 3.96% (60 × -.066). Similarly, if the incumbent-extremism measure is 90 points, and the challenger's measure is 50 points, then the incumbent's support will decrease by 4.62% (70 × -.066). Finally, by moving closer to the challenger, say at 60 points, the incumbent's support would increase by 1% (55 × -.06).

Table 4.B1

Accountability Models of Candidate Issue Positions, Presidential Vote, and Closeness of the Race on Two-party vote and Turnout, OLS Regressions 1996

Controlling for presidential vote in the district, closeness of the race (not reported)	*Challenger issue Position*	*Incumbent issue position*	*Midpoint= [incumbent + (1— challenger position)]/2*	*R-square*
y=Turnout in 1996 (n=270)	-.026 (.037)	-.032 (.037)	-	.138
y=Turnout in 1996 (n=270)	-	-	.006 (.051)	.135
y=Turnout in 1996 (n=270)	-	-.022 (.037)	-	.136
y=Turnout in 1996 (n=270)	-.029 (.037)	-	-	.136
y=Incumbent vote in 1996 (n=269)	.005 (.029)	-.061 (.031)*	-	.574
y=Incumbent vote in 1996 (n=269)	-	-	-.066 (.043)	.571
y=Incumbent vote in 1996 (n=269)	-	-.061 (.031)**	-	.574
y=Incumbent vote in 1996 (n=269)	.013 (.028)	-	-	.567

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. Challenger and incumbent positions data is from Ansolabehere, Snyder, and Stewart (2001). Same coding as in the accountability model presented in Table 4.7.

Appendix 4.C

Instrumental Variable Model

This section presents a series of models included to control for the simultaneous relationship that exists between turnout and competitiveness in the general election. This was done in order to address the final empirical puzzle produced by the analysis, mainly that an increase in turnout actually produces more extreme legislative behavior, even when the analysis is limited to freshmen elections.

The analysis employs a two-state least square instrumental regression model, where turnout is instrumented by a series of variables that are expected to be related to electoral participation, but not to the actual legislative behavior of the incumbent.

Unfortunately, such variables are very difficult to identify. Nevertheless, the analysis employs an instrument constructed with the help of all the socio-demographic variables included in the model that are believed to have a significant effect on turnout in congressional elections. The variables were selected based on the work of Rosenstone and Hansen (1993). They are the proportion of the population in the district that is 65 years or older, the percentage of African Americans in the district, the percentage of union members in the state, the percentage of urban residents in the district, the percentage of unemployed workers in the district, the percentage of college educated people in the district, the percentage of homeowners in the district, the median family income in the district (converted to 1992 dollars), and a dummy variable indicating whether the district is in a Southern state.

The previous socioeconomic data were collected from the Census Bureau; the CQ books on congressional districts in the 1970s, 1980s, 1990s, and 2000s; and Adler's dataset (2002). Hence, the data were not updated for every election year; unlike the voting-age population, which is estimated for every election. The data are simply updated between decades covered by the study. This of course leads to imprecision in the impact of these variables, especially in the

later part of the decade. Like Adler, I have accounted for court-ordered mid-decade redistricting whenever the data was available.[43]

Table 4.C1 includes a model where turnout is regressed on all the instrumental variables included in the analysis (age 65+, household income, African American, union, urban, unemployed, college educated, homeowner, closeness of the previous race, and election years dummy variables). This analysis was included to report the validity of the variables used to construct the turnout instrument.

Table 4.C2 presents the results of the two-stage least square regressions for every election year, and also for the two, pooled cross-sectional models. The analysis shows similar results as the responsiveness models presented above, even when we remove the endogenous relationship between turnout and competitiveness: competitiveness reduces roll-call extremism and turnout has a positive influence the same variable.

Finally, Table 4.C3 includes a series of two-stage least square regressions where incumbents are clustered by party affiliation. This analysis was done in order to control for the endogenous relationship between turnout and two-party vote share in the party models of Table 4.11. In these models, again, the relationship between the variables of interests remains similar, even after we use a "clean" measure of turnout. Republican incumbents moderate their roll-call behavior when turnout increases, while Democrats tend to adopt a more extreme posture under similar circumstances.

Table 4.C1

Instrumental Variable Model of Turnout, OLS Regressions 1972–2000

y=*turnout in the district*	*Presidential vote (last presidential election)*
Presidential years	.108 (.008)***
Age 65 +	.196 (.034)***
Average income ($\times 10^4$)	.035 (.003)***
African American	-.098 (.010)***
Union	-.005 (.020)
Urban	-.143 (.006)***
Unemployed	-.321 (.076)***

[43] Interested readers should consult Adler (2002) for a detailed description of the data employed.

College	.105 (.014)***
Homeowner	-.001 (.000)
South	-.068 (.004)***
Closeness of race	.024 (.004)***
Constant	.404 (.014)***
R-square	.595
N	4347

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. Cumulative sample includes dummies for election years (1972 is the baseline). The regressions exclude all the districts in states where there was a reapportionment in 1982, 1984, and 1992.

Table 4.C2

Responsiveness Models of Turnout, Two-party vote Share and Presidential Vote on Legislative Extremism by Election, Two-Stage Least Squares 1972–2000

y=*ABS(ADA) at time* t+*1*	*Presidential vote at time* t	*Two-party vote at time* t	*Instrument: turnout at time* t	*R-square*
1972 (n=0)	-	-	-	-
1974 (n=304)	.687 (.082)***	-.038 (.152)	.513 (.227)**	.210
1976 (n=307)	.672 (.222)***	-.575 (.208)***	.438 (.254)*	.069
1978 (n=271)	.919 (.188)***	-.398 (.174)**	.659 (.247)***	.143
1980 (n=285)	.961 (.113)***	-.461 (.150)***	.377 (.167)**	.164
1982 (n=197)	.558 (.095)***	-.301 (.155)*	.569 (.244)**	.172
1984 (n=349)	.947 (.071)***	-.329 (.105)***	.227 (.164)	.367
1986 (n=344)	.527 (.059)***	-.217 (.109)**	.0499 (.195)	.172
1988 (n=342)	1.244 (.107)***	-.536 (.143)***	.364 (.185)**	.358
1990 (n=337)	1.409 (.088)***	-.645 (.151)***	.372 (.177)**	.461
1992 (n=180)	1.428 (.150)***	-.652 (.174)***	.512 (.206)**	.353
1994 (n=350)	.596 (.141)***	.028 (.113)	.639 (.226)***	.072
1996 (n= 396)	.913 (.123)***	-.496 (.125)***	.359 (.163)**	.168
1998 (n=324)	1.063 (.128)***	-.654 (.140)***	.040 (.164)	.294
2000 (n=271)	1.062 (.160)***	-.871 (.184)***	.177 (.113)	.289
1972–2000 (n=4257)	.856 (.029)***	-.340 (.040)***	.449 (.058)***	.265
1980–2000 (n=2924)	.921 (.033)***	-.362 (.051)***	.381 (.062)***	.290

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. Because we use a lagged measure, the cumulative sample (1974–2000) includes dummies for election years (1974 is the baseline). The second cumulative sample (1982–2000) also includes dummies for election years in the last model with spending (1982 is the baseline). The regressions exclude all the districts in states where there was a reapportionment in 1982 and 1992. Uncompetitive races are removed from the sample. The last model (1982–2000) includes controls for challenger quality, incumbent and challenger spending, presidential years elections, southern states, number of terms served by the incumbent, and whether the incumbent is a member of the Democratic party (the spending data is only available after 1980). See Appendix 4.A for detailed description of variables employed in the model.

Turnout is instrumented with the following variables (described above): Presidential years (for cumulative models only), age 65 +, average income, African American, union, urban, unemployed, college, homeowner, closeness of race, and Southern states.

Table 4.C3

Responsiveness Models of Turnout, Two-party vote Share and Presidential Vote on Legislative Extremism by Election and by Party Affiliation, Two-Stage Least Square 1973–2001

Party affiliation	y=*ABS(ADA) (t+1)*	*Presidential vote at time* t	*Two-party vote at time* t	*Instrument: turnout at time* t	*R-square*
Democrats	1972 (n=0)	-	-	-	-
	1974 (n=187)	1.406 (.191)***	-.303 (.215)	1.399 .219)***	.415
	1976 (n=187)	.791 (.301)***	-.250 (.264)	.944 (.367)**	.062
	1978 (n=162)	1.660 (.249)***	-.285 (.237)	1.738 (.410)***	.099
	1980 (n=146)	1.222 (.215)***	-.506 (.301)*	.931 (.321)***	.099
	1982 (n=110)	.545 (.209)***	-.033 (.222)	1.397 (.259)***	.161
	1984 (=190)	1.233 (.141)***	-.265 (.202)	1.036 (.216)***	.364
	1986 (n=191)	.933 (.125)***	-.261 (.167)	.990 (.289)***	.149
	1988 (n=194)	1.416 (.172)***	-.382 (.247)	1.233 (.221)***	.365
	1990 (n=210)	1.441 (.126)***	-.418 (.180)**	.737 (.205)***	.404
	1992 (n=106)	1.792 (.159)***	-.668 (.213)***	.317 (.153)**	.588
	1994 (n=172)	1.622 (.191)***	-.779 (.189)***	.271 (.161)*	.480
	1996 (n=194)	1.701 (.178)***	-.920 (.180)***	.413 (.149)***	.473
	1998 (n=165)	1.106 (.203)***	-.564 (.226)**	.383 (.191)**	.258
	2000 (n=139)	1.013 (.175)***	-.778 (.185)***	.183 (.117)	.294
	1972–2000 (n=2353)	1.324 (.050)***	-.384 (.063)***	1.164 (.078)***	.324
	1980–2000 (n=1681)	1.328 (.055)***	-.499 (.081)***	.937 (.083)***	.357
Republicans	1972 (n=0)	-	-	-	-
	1974 (n=117)	2.28 (.314)***	-.752 (.254)***	-.273 (.329)	.418
	1976 (n=120)	1.076 (.321)***	-.218 (.239)	-1.314 (.299)***	.048
	1978 (n= 109)	.344 (.275)	-.286 (.203)	-.299 (.268)	.000
	1980 (n=139)	.994 (.227)***	-.144 (.142)	-.826 (.253)***	.079
	1982 (n=87)	1.206 (.231)***	-.711 (.291)**	-1.153 (.280)***	.237
	1984 (n=159)	1.448 (.225)***	-.557 (.134)***	-.934 (.246)***	.264
	1986 (n=153)	1.446 (.308)***	-.540 (.166)***	-.544 (.225)**	.251
	1988 (n=148)	1.789 (.229)***	-.818 (.186)***	-.544 (.331)	.310
	1990 (n=127)	1.484 (.259)***	-.993 (.239)***	.531 (.341)	.464
	1992 (n=74)	.831 (.204)***	-.434 (.243)**	-.190 (.266)	.269
	1994 (n=178)	.597 (.128)***	-.278 (.105)***	-.294 (.147)**	.125
	1996 (n=202)	1.145 (.168)***	-.691 (.147)***	-.538 (.177)***	.289
	1998 (n=159)	1.519 (.212)***	-.666 (.176)***	-.602 (.202)***	.352
	2000 (n=132)	1.093 (.271)***	-.587 (.357)	-.280 (.150)*	.335
	1972–2000 (n=1904)	1.199 (.066)***	-.531 (.052)***	-.798 (.074)***	.229
	1980–2000 (n=1381)	1.267 (075)***	-.593 (.080)***	-.676 (.082)***	.278

Note: Robust standard errors are given in parentheses. Significance are two tailed tests, *p < .10; **p < .05; ***p < .01. Because we use a lagged measure, the cumulative sample (1974–2000) includes dummies for election years (1974 is the baseline). The second cumulative sample (1982–2000) also includes dummies for election years in the last model with spending (1982 is the baseline). The regressions exclude all the districts in states where there was a reapportionment in 1982 and 1992. Uncompetitive

races are removed from the sample. The last model (1982–2000) includes controls for challenger quality, incumbent and challenger spending, presidential years elections, southern states, number of terms served by the incumbent, and whether the incumbent is a member of the Democratic party (the spending data is only available after 1980). See Appendix 4.A for detailed description of variables employed in the model. Turnout is instrumented with the following variables (described above): Presidential years (for cumulative models only), age 65 +, average income, African American, union, urban, unemployed, college, homeowner, closeness of race, and Southern states.

CHAPTER 5

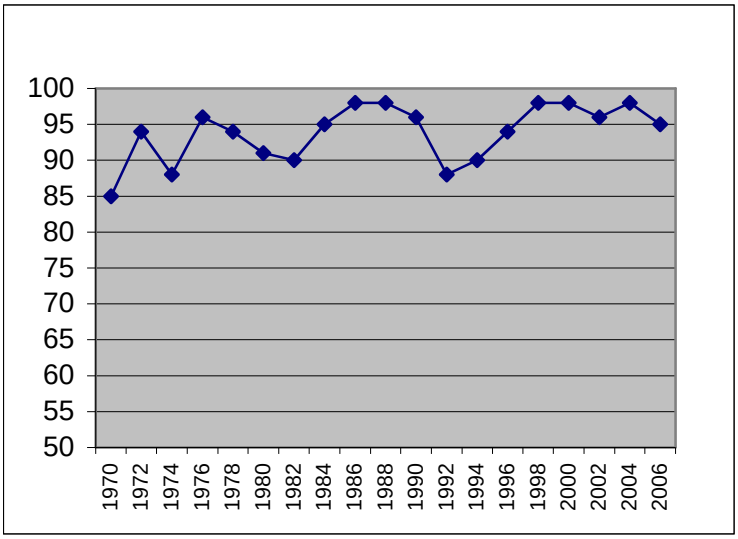
Conclusion

The Republican victories in the 2002 and 2004 elections have highlighted the importance of electioneering and information technologies in the modern political campaign. The conventional wisdom is that politicians are increasingly targeting specific types of messages to segments of the electorate. For instance, families with children might receive information about a candidate's stance on educational reforms, whereas wealthier households might receive information about tax reforms. This type of campaigning is not wholly novel. Ever since the early 1970s, the demise of the political machines and the advent of candidate-centered politics have forced lawmakers to devise alternative methods for enlisting potential supporters. What remains uncertain about this trend is the effect of campaigns on political participation and, in turn, on the orientation of public policies adopted by elected representatives. In this concluding chapter, I explain how this postwar trend of targeting has affected electoral accountability, participation, and legislative responsiveness in the U.S. Congress.

I began the dissertation by demonstrating that people who are most likely to vote have a greater likelihood to support the Republican Party and to prefer conservative public policies. Chapter 2 showed that higher levels of statewide electoral participation would produce, in most states, a modest increase in the percentage of liberal and Democrat voters in both midterm and presidential elections. We also saw in chapter 3 that one of the principal consequences of the changing political landscape in the postwar era has been the advent of the so-called institutional incumbency advantage in congressional elections. This phenomenon has led to a reduction in the

number of contested elections for the House of Representatives. Incumbent reelection rates have been consistently above 85% in recent years, reaching as high as 99% in the House for the 2002 and 2004 elections (see Figure 5.1).

Figure 5.1. Percentage of House incumbency reelection rates, 1970–2006.



Note: From author's dataset.

This dissertation has demonstrated that one of the chief electoral consequences of the incumbency advantage has been reduced turnout levels in congressional elections. We also found that this influence varied with the length of the congressional career: junior incumbents faced higher levels of turnout and more competitive elections, while senior incumbents were more likely to be re-elected in low salience, low turnout elections.

Since we found that incumbency affects the level of political participation, the next step in the analysis was to determine whether political participation and electoral competition influence the incumbent's behavior. The primary argument of chapter 4 was that varying levels of political participation (and by extension political competitiveness) in House elections could

ultimately affect the incumbent's roll-call legislative record. Instead of finding that higher turnout induced legislative moderation for all incumbents, I discovered that Republican districts, where voter turnout is high, tended to be represented by more moderate incumbents. By contrast, in Democratic districts with a high level of turnout, the incumbent was more likely to adopt an extreme legislative record. In both cases, the result was confirmed regardless of the level of electoral competition.

It is possible to explain this counterintuitive finding by considering the fact that people who are less likely to vote usually hold slightly more liberal preferences. Hence, in most conservative districts, higher turnout can be expected to be associated with an increase in the number of voters who prefer a government that does more, rather than less. This surge in participation can, further, induce legislative moderation among Republican incumbents, mainly because the district average voter is expected to be closer to the ideological center. Conversely, the analysis showed that for Democratic incumbents higher levels of political participation would increase the number of liberal voters and encourage more extreme legislative behavior. Both of these seemingly complementary findings led me to conclude that a surge in turnout would induce lawmakers to be more responsive to the needs of constituents of a lower socio-economic status. Now that I have restated my principal argument, let me enumerate what I see as the major weaknesses in the previous analysis.

5.1 Measurement Problems

To begin with, one could argue that because the level of political participation in congressional elections is not correctly measured and since turnout is not really decreasing in the

United States (McDonald & Popkin, 2001), any analysis of the relationship between political participation and legislative responsiveness will be flawed. It is true that the lowering of the minimum age of voting a quarter of a century ago from 21 to 18 has contributed to increase the number of eligible voters, but has also reduced the level of political participation (since younger voters participate less). And it is also true that the inclusion of felons and resident and illegal aliens in the voting age population by the Census Bureau can put an artificial downward pressure on the level of voter turnout (as Uggen & Manza, 2002 and McDonald & Popkin, 2001 claim). However, since we focused on the effects of fluctuating turnout rates on legislative behavior in each congressional district, the fact that there is a measurement error associated with voter turnout in the study makes little difference because the error is systematic across the entire sample. That there is a difference of 20 points in the level of political participation when we compare two districts should remain in the same range when we use a corrected measure of the voting age population.[1]

Even if we consider turnout to be incorrectly measured, we still find that by excluding non-citizens and felons, turnout is lower today than it was 40 years ago, and income inequality and ideological polarization in Congress is at a post-World War II high.[2] That there is a marked political divergence between high and low income citizens today suggests that elected officials are not responsive to the interests of a significant proportion of nonvoters. A recent study done by McCarthy, Poole, and Rosenthal (2006) concludes that "politicians have become more polarized, and that the rightward move and electoral success of the Republicans have moved the

[1] Of course, we need to assume that the distribution of non-citizens is similar across both districts, which is highly unlikely if we compare an urban district in Chicago with the only House district in Vermont for example.

[2] McDonald's reconciled voting age population data can be seen at http://polmeth.wustl.edu/polanalysis/vol/11/mcdonald.NES_Bias.htm, accessed May 9, 2007.

political system away from public policy that might alleviate income inequality" (p.165). The authors claim that this increasing polarization cannot be explained by a decline in political participation because the social-economic biases in turnout among citizens (as opposed to non-citizens) have remained relatively constant since 1972. McCarthy, Poole, and Rosenthal even argue that "while overall inequality has increased, the pressure for redistributive policies has been sharply mitigated by the fact that the income inequality of voters did not increase" (p.193).

These findings do not contradict in any way the results presented in this dissertation. The authors simply fail to recognize that a dichotomous view of participation (voters versus nonvoters) is too simplistic to fully account for the relationship between electoral accountability and legislative responsiveness. Moreover, the thrust of McCarthy, Poole, and Rosenthal's argument is not that far from my own. The authors claim that lower participation rates create more inequality and polarization in the United States. Whether one focuses on the income distribution of non-citizens or on the income distribution of citizens who rarely vote, the conclusion is the same: higher participation rates bring with them a modification of legislative behavior.

Another very important problem found in this dissertation has to do with the causal relationship between some of the variables included in the analysis, especially with regard to turnout and the vote share for the incumbent. As we saw in chapters 3 and 4, when turnout and vote are considered simultaneously in an equation, one cannot be sure about the independent effect of each of the variables. Basically, this relationship is endogenous. I have acknowledged this potential bias in the analysis, and I have tried to control for the endogeneity by using an instrumental variable for turnout. As I indicated before, this approach is far from ideal. The

identification phase in the two-stage least squares regression model is the most problematic, since it is very difficult to find a set of variables that correlate with turnout but not with the individual voting decision. For this reason, I believe that my statistical modeling provides a parsimonious description of the relationship between turnout, electoral competition, and legislative behavior.[3] It should be clear that it is not my claim that higher turnout *causes* legislative moderation among Republicans, or that incumbency *causes* a reduction in political participation. Rather, my analysis demonstrates that all of these variables are statistically associated. And since my dataset covers more than one election, it is very difficult to determine precisely what may have been the ultimate cause of legislative extremism in the first place. As we saw, even if we look at freshmen incumbents who have no prior record to influence their behavior, we can still find that they are influenced by the legislative record of their predecessors. Short of being able to randomly assign turnout across all the House districts in one particular election, it will remain virtually impossible to determine precisely what the impact of political participation is on the level of incumbent support, or on subsequent legislative behavior. Nevertheless, by using different types of models and a very straightforward coding methodology, it was my intention to provide for the reader an overview of the strength of the interaction between the variables included in the analysis.

5.2 Southern Realignment

A more serious criticism of the analysis presented in this dissertation bears on one of the primary claims presented in chapter 3. I argued there that an increase in turnout would be

[3] See Sobel (2005) for a discussion of causal inference in the social science.

detrimental to the incumbent candidate, *regardless of party affiliation.* This argument was confirmed by my results, which showed that the presence of a quality challenger and higher levels of campaign spending would ultimately be associated with an increase in turnout. The analysis also indicated that the electoral effect of a surge in participation would actually be detrimental to the incumbent candidate.

These findings directly contradict some of the major results presented in chapter 4, in which it was shown that a surge in turnout would be associated with an increase in the number of liberal voters in both Republican and Democratic districts. The earlier results make sense if we assume that an increase in the level of political participation will actually bring voters of a lower socio-economic status to the polls. Such an eventuality seems reasonable in the case of Democratic districts, because their elected officials are more likely to represent constituencies where this type of elector is found. But what about the undecided or Republican voters in those Democratic constituencies—should we not find that an increase in turnout also has them voting in greater numbers? Put differently, should we not find that higher levels of turnout in Democratic districts will be associated with moderate legislative behavior?

This may very well be what happens when a Democrat is representing a competitive district and facing a truly competitive election. But since the models presented in chapter 4 control for electoral vulnerability, the measure of political participation that is employed was theoretically "uncontaminated" by electoral competition. In this context, higher turnout for both Republican and Democrat incumbents implies a greater representation of constituents who have a lower probability of voting, *regardless of the incumbent's electoral security.* And as chapter 2 demonstrated, these voters are more likely to prefer liberal policies.

One could also argue that Democrats elected in districts where turnout is generally high, e.g., in Minnesota, or the rest of the Upper Midwest,[4] are just more likely to represent constituencies where there is a strong liberal contingent, as opposed to other Democratic districts where turnout is low and ideology is more evenly distributed. In addition, one could also claim that Southern Republicans, who are more extreme than their Northern counterparts, usually represent districts where turnout is low. And if we found that Northern Republicans are elected in districts where participation is high, then there is a distinct possibility that the relationship between political participation and moderation/extremism in the House would just be an artifact of some of the geographical characteristics found in the country.

I believe this is a valid concern. Upon closer examination of the data, we find that the average legislative roll-call extremism of Southern Republicans is 10 points higher than for their non-Southern counterparts (92 versus 82 points on the transformed ADA scale) while the average turnout is 10 points lower (39% vs. 49%). We also find higher levels of turnout in Democratic districts of the Upper Midwestern states (Minnesota, Iowa, South Dakota, and Wisconsin). In these districts, legislative behavior is also slightly more extreme, but the sample is too small to conclude that there is a systematic effect (82 points versus 78 points on the transformed ADA scale).[5]

The preceding results seem to indicate that in the South, at least, the relationship between turnout and legislative responsiveness might ultimately be the result of geographical characteristics. For instance, it is widely believed that the Southern realignment following the

[4] Eric J. Ostermeier and Lawrence Jacobs, "Minnesota Voters Turnout." Available on the authors' Website http://www.hhh.umn.edu/img/assets/23537/Minnesota%20Voters%20Turnout.pdf

[5] The mean turnout in these districts is 11 points higher in comparison to the remaining non-Southern Democratic constituency (54% vs. 43%). However, there are only 194 cases of Upper Midwest elections in the sample, which is too small to have an effect on the Democratic election results.

1964 election and the subsequent court-ordered minority/majority redistricting have increased the number of extreme Republicans who represent predominantly white districts (the same can be said about minority districts represented by extreme Democrats). But before we can conclude that the South is the reason why Republicans moderate their legislative behavior when turnout is high, we should note that McCarthy, Poole, and Rosenthal (2006) have established that the Southern realignment has had a very small effect on partisan polarization in Congress.

These authors explain that even if Republicans from the South have increased their number in recent years, they did not constitute a majority of Southern seats in Congress until after the 1994 election. In addition, McCarthy, Poole, and Rosenthal show that the total level of House polarization has actually exceeded non-southern polarization only after the mid-1990s. The authors also note that the predominantly white districts from the South have not significantly contributed to the increase in partisan polarization among House representatives. In chapter 4 I showed that in some of the earliest elections included in the sample, when the pool of Southern Republicans in the House was very small, higher turnout brought with it a moderation of incumbent Republican legislative behavior. Based on my analysis, and on the McCarthy, Poole, and Rosenthal (2006) study, I argue with confidence that this Southern exception argument is insufficient to counter the previous findings.

5.3 Voter Information

In *Democracy, Accountability, and Representation* (Przeworski, Stokes, & Manin, eds., 1999), the editors stipulate that a government is representative if people are sufficiently informed, elections are contested, and electoral participation is widespread. In this dissertation, I have

implicitly assumed that voters possess enough information about the voting records of their representatives to hold them accountable for their actions in Congress. Of course, this assumption is hard to sustain given the consistently low level of political information displayed by the average voter in America (e.g., Delli Carpini & Keeter, 1996). One could argue that since voters possess so little information about their representatives, any study of electoral accountability, especially in Congress, will be valid only for a small subsample of the electorate (the most informed). Therefore, any analysis focusing on the link between legislative records and electoral accountability will be biased.

This hypothetical critique runs counter to most of the existing studies of representation in the congressional election literature. In fact, scholars and campaign experts widely assume that past legislative behavior has an influence on electoral outcomes. As Jacobson (2004) explains, "Anyone who consistently votes contrary to the wishes of his or her constituents is likely to run into trouble" (p.220). And when we focus more specifically on studies of public opinion, we find that in the aggregate voters are expected to form coherent impressions of their representative's roll-call voting patterns (Page & Shapiro, 1992; Erikson, MacKuen, & Stimson, 2002).

As Zaller (1992) says, "although most people cannot *recall* their incumbent's name, about 80 percent can *recognize* it" (p.19, emphasis in original). In this respect, we can imagine that certain uninformed voters are taking cues from political elites, especially in competitive election campaigns, where higher spending can help decimate controversial roll-call votes in the constituency (Canes-Wrone, Brady, & Cogan, 2002, make a similar argument). With Chong (2000), I believe it reasonable to assume that a large proportion of voters are rational in that they try to identify their own interests by following some of their personal experiences. In that sense,

we can expect that the average elector, who possesses a limited amount of political information, will be capable of holding his or her representative accountable for past legislative actions, especially if the incumbent is persistently unresponsive to electoral demands.

5.4 A Calculus of Voting?

The dissertation also avoided the question of the individual voter's decision to participate in an election. Chapter 3 began by assuming that candidates were rational economic agents competing in elections in order to maximize their election probability. This type of assumption is well sustained when scholars focus on the behavior of political elites. However, it is has been said that there is a problem in trying to impute purely rational economic motives to the voter. In chapter 3, I simply assumed that the electorate faced a cost for participating in an election. And I did not discuss any of the flaws underlying the rational choice theory of voting.

In its most basic model, the rational choice approach implies that citizens always vote for their preferred candidate because individuals favor outcomes with higher utility and choose actions to receive the most benefits (Downs, 1957). The decision to vote is, in turn, determined by the costs associated with voting and the probability of seeing one's preferred candidate elected. If the cost of voting is higher than the expected benefits from participating in an election, then the voter abstains. As the cost of voting increases, turnout rates declines. Consequently, the pure rational choice model of voting predicts that voters will always abstain since the probability of breaking or making a tie is virtually zero in a large election, and the benefits of seeing one's preferred candidate win is outweighed by the costs of participation. To sum up, the central claim of this model is that it is rational to vote only if the net utility of voting is positive, but this is

almost never the case (Franklin, 2004). Yet it is widely acknowledged that this conclusion constitutes a paradox, as citizens still participate in elections, sometime at the cost of their own lives, even if their individual votes have virtually no chance of influencing the outcome.

I believe that the results presented in this dissertation can provide us with an alternative conception of the calculus of voting, which remains rational in the economic sense, but still manages to avoid the inclusion of exogenous parameters in the model. If we view turnout as an additional mechanism to influence public policy—even if a voter's preferred candidate is not elected—then the notion that voters only participate in order to make or break a tie becomes secondary in the model.

The results in chapter 4 demonstrated that turnout and competitiveness affect the incumbent's legislative behavior. This implies that from a voter standpoint, participating in an election, regardless of the outcome, can influence lawmakers—e.g., higher turnout means more moderate representatives in Republican districts. I am certainly not claiming that this is true in all cases, especially since it was shown that higher participation in Democratic districts is associated with more extreme legislative behavior, which by extension could encourage abstention among Republicans in those constituencies. But this point is beyond the scope of the present dissertation. I am only suggesting that future work on political participation should take into consideration the possibility that there is an additional incentive to vote, one that would be independent of the election outcome. This effect would exist outside of the realm of the more conventional expressive or non-instrumental benefits of participation, concepts generally put forward to salvage the rational choice model of voting.

5.5 Some Final Thoughts

Widespread political apathy and declining voter turnout have undermined legislative representation and accountability in more than one modern democratic country. Certain countries, aware of this problem, have imposed mandatory voting as a way to counter this form of legislative shirking. When turnout is low, legislators only represent a small fraction of the electorate. This reality falls short of the Jeffersonian conception of democracy, where representatives are expected to convey the interests of all citizens in their districts. As Madison explains in Federalist No. 39, "It is essential to such a government that it be derived from the great body of the society, not from an inconsiderable proportion, or a favored class of it" (1778–89, 2003, p.228). Because nonvoters are disproportionately located in the lowest socio-economic segment of the American population, representative democracy gives politicians few incentives to pay attention to the needs and concerns of this social group. Ironically, these citizens are more likely to be dependent on governmental programs like Medicare and Social Security.

The dissertation has shown that elected officials are more likely to ignore the preferences of the nonvoting public. When turnout is high in a congressional district, Republican incumbents tend to moderate their legislative behavior and Democratic incumbents tend to favor even more liberal policies. Like Canes-Wrone, Brady, and Cogan (2002), we also found that incumbents who are not representing the ideological preferences of their constituents are more likely to lose support at the polls. However, the fact that House members seem to be influenced more by the ideological composition of their district and their own previous legislative record (or that of their predecessors) than by their margin of victory, suggests that incumbents are not really responsive to electoral competition. If the institutional advantages conveyed to incumbents by their

positions in the legislature are the principal factors explaining why competitiveness is declining in congressional elections, then the only hope for the electorate to obtain like-minded officials is through electoral selection (for a similar argument, see Lee, Morretti, & Butler, 2004). However, as we saw in the previous chapters, the agency relationship between voters and elected officials is further weakened by lower levels of political participation. For this reason, I believe it is necessary to stimulate voter turnout to maintain a healthy democratic and accountable government in the United States.

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