DO VOTERS AFFECT OR ELECT POLICIES? EVIDENCE FROM THE U. S. HOUSE*

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There are two fundamentally different views of the role of elections in policy formation. In one view, voters can *affect* candidates' policy choices: competition for votes induces politicians to move toward the center. In this view, elections have the effect of bringing about some degree of policy compromise. In the alternative view, voters merely *elect* policies: politicians cannot make credible promises to moderate their policies, and elections are merely a means to decide which one of two opposing policy views will be implemented. We assess which of these contrasting perspectives is more empirically relevant for the U. S. House. Focusing on elections decided by a narrow margin allows us to generate quasi-experimental estimates of the impact of a "randomized" change in electoral strength on subsequent representatives' roll-call voting records. We find that voters merely *elect* policies: the degree of electoral strength has no effect on a legislator's voting behavior. For example, a large *exogenous* increase in electoral strength for the Democratic party in a district does not result in shifting both parties' nominees to the left. Politicians' inability to credibly commit to a compromise appears to dominate any competition-induced convergence in policy.

I. Introduction

How do voters influence government policies? An economist's answer is that they do so by compelling politicians to adopt "middle ground" platforms. Competition for votes can force even the most partisan Republicans and Democrats to moderate their policy choices. In the extreme case, competition may be so strong that it leads to "full policy convergence": opposing parties are forced to adopt identical policies [Downs 1957].[1] More realisti-

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1. Empirical studies indicate that Republican and Democratic legislators vote very differently, even when they share the same constituency. For example,

cally, competition leads to "partial policy convergence": candidates do pursue more moderate policies, even if they are not forced to adopt identical platforms [Wittman 1983; Calvert 1985]. This less rigid and arguably more realistic understanding of Downs' insight has become central to how economists think about political competition. Indeed, the so-called "Downsian paradigm" has remained the backbone of many models in political economy.

There is, however, a growing recognition of a serious shortcoming of this paradigm. In a recent survey of the literature, Besley and Case [2003] emphasize that the assumptions about politicians' commitment and motivation in the Downsian paradigm "are unreasonable and outcomes are highly unrobust to deviations from them." Downsian convergence depends on the assumption that elected politicians always implement the policies that they promised as candidates. But Alesina [1988] shows that when partisan politicians cannot credibly promise to implement more moderate policies, the result can be full policy *divergence:* the winning candidate, after obtaining office, simply pursues his most-preferred policy. In this case, voters fail to compel candidates to reach any kind of policy compromise.

What emerges, then, are two fundamentally different views of the role of elections in a representative democracy. On the one hand, when electoral promises are credible—as in a Downsian partial convergence—candidates seek middle ground policies, and general elections bring about some degree of policy "compromise." On the other hand, when promises to enact moderate policies are not credible—as in full policy divergence—general elections are merely a means to decide which candidate's preferred policy will be implemented. Which of these two competing views is empirically more relevant? This paper assesses the relative importance of the two contrasting perspectives in explaining how Representatives vote in the U. S. House.

As is apparent from Alesina's [1988] analysis of the role of credibility, the two broad views have sharply different predictions for how a politician's electoral strength influences her policy choices. When politicians have incentives to moderate their platforms—as in partial policy convergence—the relative electoral

Poole and Rosenthal [1984] show that senators from the same state but from different political parties have different voting records. This is inconsistent with Downs' original model, in which candidates adopt identical positions—"complete policy convergence." See also Snyder and Groseclose [2000] and Levitt [1996].

strength of the two parties matters. More specifically, when electoral support is high, a candidate can afford to vote in a relatively more partisan way if he is elected; a weaker candidate would be forced to choose a more moderate policy. An increase in electoral strength for the Democratic party in a district, for example, would cause both parties' nominees to shift to the "left." On the other hand, when voters do not believe promises of policy compromises—as in full policy divergence—the relative electoral strength of the two candidates is irrelevant, as politicians simply pursue their own personal policy views. That is, an increase in electoral strength for the Democratic party in a district leaves legislators' actions unchanged.

Therefore, an assessment of the relative importance of the two views requires estimating the effect of a candidate's electoral strength on subsequent roll-call voting records. To do so, we consider electoral races where a Democrat holds the seat—and hence an electoral advantage—and measure the roll-call voting records of the winners of these elections. We measure the extent to which they are more liberal than the voting records of winners of elections where the Republican had held the seat; i.e., where the Democrat was relatively weaker.[2] Of course, which of the two parties holds a district seat—and hence the electoral advantage—is clearly endogenously determined, influenced by the political leanings of the voters, the quality of candidates, resources available to the campaigns, and other unmeasured characteristics of the district and the candidates. A naive comparison that does not account for these differences between Democratic and Republican districts is likely to yield biased estimates. What is needed is an *exogenous* variation in who holds the seat—and hence greater electoral strength—in order to measure how politicians' actions respond to the odds of winning an election.

To isolate such exogenous variation, we exploit a quasi experiment embedded in the Congressional electoral system that generates essentially "random assignment" of which party holds a seat—and therefore which party holds the electoral advantage. In particular, we focus our analysis on the set of electoral races in

2. Our empirical strategy obviously accounts for the fact that Democrats are more liberal than Republican. That is, the roll-call behavior of a winner of an electoral race where a Democrat held the seat will tend to be more liberal simply because—due to the advantage of incumbency—the winner will more likely be a Democrat. This fact in itself would cause a difference in voting records, even if Representatives ignored electoral pressures and simply voted their own ideological position. It is easy to account for this factor—as we will show in Section II.

which the incumbent party had *barely* won the *previous* election (say by 0.01 percent of the vote). The key identifying assumption is that districts where the Democrats barely won are comparable—in all other ways—to districts where the Republicans barely won. We present empirical evidence that strongly supports this assumption: Democratic and Republican districts are in general very different, but among close elections, they are similar in every characteristic that we examine, including various demographic characteristics of the population, racial composition, size of the district, income levels, and geographical location. Our quasi experiment, then, addresses the endogeneity problem by isolating arguably independent and exogenous variation in candidates' electoral strength across Congressional districts.

Using this regression-discontinuity design and voting record data from the U. S. House (1946–1995), we find that the degree of electoral strength has no effect on a legislator's voting behavior.[3] Candidates with weak electoral support do not adopt more moderate positions than do stronger candidates, holding other factors constant. For example, a large exogenous increase in electoral strength for the Democratic party in a district does not result in shifting both parties' nominees to the left. This suggests that voters seem not to *affect* politicians' choices during general elections; instead, they appear to merely *elect* policies through choosing a legislator. That is, they do not *influence* policy through their Representatives' choices as much as they are implicitly *presented with* policy choices by different candidates.[4]

Our findings are consistent with the inability of opposing candidates to credibly commit to a policy compromise. It appears that the central prediction of the Downsian paradigm—that individual politicians' policy choices are constrained by voters' sentiments—has little empirical support, at least in the context of U. S. House general elections. Our findings provide some empirical justification for the notion that candidates confront a credi-

3. The voting score data include (1) the actual roll-call data from ICPSR Study 4 "United States Congressional Roll Call Voting Records," (2) Groseclose, Levitt, and Snyder's [1999] Inflation-Adjusted ADA Scores [Groseclose 2002], (3) McCarty, Poole, and Rosenthal's [1997] DW-NOMINATE scores [Poole 2002a], (4) Poole and Daniels' [1985] Interest Group Ratings [Poole 2002b], and (5) Poole's [1999] rank order data [Poole 2002c].

4. This leaves open the question of how candidates are selected. There are several models where candidates are endogenous. (See, for example, Persson and Tabellini [2000] for an introduction to this literature.) In this paper we take the candidates' ideologies as exogenous. We return to this point below.

bility problem. This notion has been explicitly adopted in recent theoretical analyses [Besley and Coate 1997, 1998].

It is important to recognize that our findings say little about whether members of the U. S. House generally represent their "constituencies." Instead, our analysis focuses on the role of *general elections* in inducing candidates with different policy stances to move toward the center. Although we find a small effect of the pressures of a general election on candidates, this does not imply that election outcomes do not "represent" the desires of the electorate. First, and most obviously, voters still do choose between the two available policy platforms. Second, "representativeness" does not necessarily occur only through general elections. Pre-election channels (primary elections, for example) may also be important in inducing representativeness. Indeed, within each district, the Republican and Democratic nominees may, respectively, represent the "median" Republican and "median" Democratic voter.

The paper is organized as follows. Sections II and III provide background and motivation for our analysis, and describe our empirical strategy, first informally, and then within a formal conceptual framework. Section IV describes the context and the data, and Section V presents our empirical results. We relate our findings to the existing literature in Section VI. Section VII concludes.

II. Background and Conceptual Framework

II.A. Role of Credibility in Political Competition

Voters can influence policy in two distinct ways. Competing political candidates have incentives to adopt positions that reflect the preferences of the electorate because doing so raises the chances they will win the election. That is, voters can *affect* the policy choices of politicians. Alternatively, voters always impact policy outcomes by selecting a leader among several candidates, who each may have already decided on a particular policy based on other reasons. In this way, voters may simply *elect* policies. Whether voters affect or elect policies depends on whether or not candidates are able to make credible promises to implement moderate policies.

A large class of models of political competition assumes that they can. The most well-known example is the simple "median

voter" model of political competition [Downs 1957]. Two candidates, who care only about winning office, compete for votes by taking a stance in a single dimensional policy space. Voters cast their vote based on these positions, and the equilibrium result is that the politicians carry out identical policies—the one most preferred by the "median voter." In this extreme example, voters have a powerful *effect* on politicians' choices, to the point where it is irrelevant *which* of the two candidates is ultimately elected.

A similar outcome results when opposing candidates care not only about winning the election, but also about the implemented policies themselves. Opposing parties may not choose *identical* positions, but in general electoral competition will compel them to choose policies more moderate than their most preferred choices [Wittman 1983; Calvert 1985]. The basic insight that voters *affect* candidates' positions by inducing spatial competition is robust to various generalizations of the simple model utilized by Downs [Osborne 1995].

But it is much less robust to the assumption that candidates can commit to policy pronouncements, as emphasized in Besley and Case [2003]. When politicians have ideological preferences over policy outcomes, credibility becomes an issue. Specifically, Alesina [1988] points out that Downs' equilibrium may fall apart if parties care about policies and there is no way to make binding precommitments to announced policies. After winning the election, what incentive does a legislator have to keep a promise of a more moderate policy? In a one-shot game, the only time-consistent equilibrium is that candidates carry out their ex post most-preferred policy. Electoral pressures do not at all compel opposing candidates to moderate their positions. Voters' only role in affecting policy outcomes is to *elect* a politician, whose policy position is unaffected by electoral pressures.

In a repeated election framework, both policy convergence and divergence are possible, as politicians can establish credibility through building reputations. If voters and opposing parties believe that there are sufficiently high costs to deviating from moderate promises, it is possible to achieve some degree of *policy convergence* [Alesina 1988]. Voters *affect* policies because of candidates' incentives to maintain a reputation. But if both parties and voters do not expect any compromise, the *fully divergent* outcome occurs in every election. Candi-

dates do not deviate from their ex post most-preferred policy, and voters only *elect* policies.[5]

The goal of this paper is to examine which phenomenon is more empirically relevant for describing roll-call voting patterns of U. S. House Representatives. Does the expectation of how voters will cast their ballot *affect* how legislators vote, or do voters simply *elect* a legislator among candidates with fixed policy positions? The answer to this question has important implications for understanding and modeling policy formation in a representative democracy.

If voters primarily affect politicians' decisions, then "centripetal" political forces generated by the broader voting population would largely outweigh any "centrifugal" forces that pull candidates' positions apart (e.g., party discipline, special interest groups). It would also imply that candidates are able to convince voters that they will compromise on policy, through the building of reputations or other mechanisms. The Downsian paradigm would then seem to be a reasonable, first-order description of policy formation as it relates to U. S. House elections.

On the other hand, if voters primarily elect policies, then "centrifugal" forces largely would dominate any Downsian convergence. It would then become more important, for example, to understand how a nominee, and the policies that she supports, is chosen by the party: primary elections could be more influential than general elections for policy formation. It would also provide an empirical basis for assuming that candidates face a serious credibility problem in their policy pronouncements. There is a growing recognition of the inadequacy of the Downsian paradigm on this point [Besley and Case 2003].

Existing studies have established that, controlling for constituency characteristics, Democratic representatives possess more liberal voting records than Republican members of Congress.[6] This constitutes strong evidence against the extreme case of *complete* policy convergence (e.g., the median voter theorem), but is too stringent a test of the more general notion of Downsian electoral competition. Therefore, to measure the relative impor-

5. It is also true that even if discount rates are sufficiently low, the fully divergent outcome still remains a subgame perfect equilibrium of the repeated election game.

6. The full convergence hypothesis has been tested, and rejected by many authors. For example, Poole and Rosenthal [1984] show that senators from the same state but from different political parties have different voting records. For a discussion of empirical regularities in the literature, see Snyder and Ting [2001a].

tance of competition-induced convergence, it is necessary to empirically distinguish between *partial convergence,* where voters *affect* politicians' policy choices—despite the undeniable party effect—and *complete divergence,* where voters merely *elect* policies. This is the goal of our study.

II.B. Identification Strategy

We now describe the main difficulties of addressing this question, and how we confront them with our identification strategy. Here we will intentionally be less formal, in order to provide the intuition of our approach. A more rigorous exposition of our conceptual and econometric framework is presented in the next section. Throughout the discussion we assume a two-party political system.

The most straightforward way to determine whether voters primarily affect or elect policy choices is to simply compare candidates' most-preferred policies (hereinafter "bliss points") and the policies they would actually choose. If the voting records were more moderate than their bliss points, this would indicate that the expected voting behavior of the electorate factored into the candidates' decisions. If there were no difference between their choices and their bliss points, this would imply that voters merely influence the relative odds of which of the two candidates' policies is "elected." Unfortunately, such a comparison is impossible, since there are no reliable measures of candidates' bliss points.

In this paper we utilize a simple empirical test of whether voters primarily affect or elect policy choices, based on how Representatives' roll-call voting behavior is affected by exogenous changes in their electoral strength. The test is based on the predictions of Alesina's [1988] model of electoral competition. In the next section we formally develop the idea, but the intuition is very simple. If candidates are constrained by their constituents' preferences, we should observe that exogenous changes in their electoral strength have an impact on how they intend to vote if elected to Congress. On the other hand, if promises to adopt moderate policies are noncredible, then the electoral strength of a candidate should be irrelevant to how (s)he intends to vote.

Throughout the paper we use the following notation for the timing of elections. t and $t + 1$ represent separate electoral cycles. For example, when $t = 1992$, it includes the 1992 campaign, the November 1992 election, and the 1993–1994 Congressional session. Similarly, $t + 1$ would include the 1994 campaign,

the November 1994 election, and the 1995–1996 Congressional session.

Our strategy is based on the following thought experiment. Imagine that we could decide the outcome of Congressional electoral races in, say, 1992 with the flip of a coin (but we allow all subsequent elections to be determined in the usual way). This initial randomization guarantees that the group of districts where the Democrat won would be, in all other respects, similar to the newly Republican districts. For example, the two groups of districts would be similar in the ideological positions of the voters and candidates, the demographic characteristics, the resources that were available to the candidates, and so forth.

Because incumbents are known to possess an electoral advantage, the outcome of the 1992 race would impact what happens in the 1994 election. Democrats are likely to be in a relatively stronger electoral position where they are incumbents, and similarly for Republicans. The key point is that the random assignment of who wins in 1992 essentially generates random assignment in which party's nominee has greater electoral strength for the 1994 election. We could use this change in electoral strength to test the hypothesis of complete divergence against the alternative of partial convergence.

Specifically, we could examine the 1995–1996 voting "scores" of the winners of the 1994 elections where the Democrats had held the seat during the 1994 campaign, and compare them with the scores of winners of elections where a Republican held the seat. This difference would represent a valid causal effect of who holds the seat during the 1994 electoral races on 1995–1996 voting records. We call this the "overall effect," and it is the sum of two components.

The first component would reflect that the 1995–1996 voting scores of the winners where a Democrat held the seat during the 1994 electoral race will tend to be more liberal simply because—due to the electoral advantage of holding the seat—the winner will more likely be a Democrat. And as we know, Democrats have more liberal voting scores. This first component reflects how voters *elect* policies: how they impact policy by simply altering the relative odds of which party's nominee is chosen. As we show more formally in the next section, this component can be directly estimated by answering the questions how much more likely is the winner to be a Democrat if the seat is already held by a

Democrat and what is the expected difference between how Republicans and Democrats vote, other things constant.

The remaining, second component would reflect how candidates might respond to an exogenous increase or decrease in the probability of winning the election in 1994. If legislators are pressured to keep their election promises, then a Democrat who is challenging an incumbent Republican in 1994 would be expected to have less liberal voting records in 1995–1996 (if elected) compared with an incumbent Democrat. After all, the challenger would be in a much weaker electoral position than the incumbent. This second component reflects how expected voting behavior *affects* the policy choices of candidates. It is computed by subtracting the first component from the overall effect.

The relative magnitudes of the two components indicate which equilibrium—full divergence or partial convergence—is relatively more important. If the "elect" component is dominant, it suggests full policy divergence: politicians simply vote their own policy views, unaffected by electoral pressures. If the "affect" component is important, it suggests partial policy convergence: policy choices are constrained by electoral pressure imparted by voters.

What allows us to perform this decomposition into the two components? The initial "random assignment" of who wins the 1992 election does. Without the random assignment, it would be difficult to distinguish between any of these effects and differences due to spurious reasons. After all, in the real world, the party that holds a district seat—and the electoral advantage—is clearly endogenously determined, influenced by the ideologies of the voters and candidates, and other unmeasured characteristics of the districts. A naive comparison that does not account for all these unobservable differences between Democratic and Republican districts is likely to yield biased estimates.

For example, Democratic legislators will have more liberal voting scores than Republicans (for simplicity, consider the period of the 1990s). But Democrats are also more likely to be elected in places like Massachusetts and than in places like Alabama. So it is not clear how much of this voting gap reflects the typical difference between Republican and Democratic nominees and how much of the gap reflects the typical difference between Representatives from Massachusetts and Alabama.

How do we generate the initial "flip of the coin" decision of who wins the 1992 election? We use a quasi experiment that is

embedded in the Congressional electoral system. Specifically, our empirical strategy focuses on elections that were decided by a very narrow margin in 1992, as revealed by the final vote tally. For example, we begin by examining elections that were decided by less than a 2 percent vote share. We argue that among these elections, it is virtually random which of the two parties won the seat [Lee 2003]. For the sake of exposition, we defer to a later section the discussion of why we believe this to be true, and the description of the empirical evidence that strongly supports this assumption. We have used 1992 and 1994 in this explanation of our empirical strategy. In practice, in our empirical analysis we use data for the period 1946–1995.

III. Theoretical and Econometric Framework

In this section we 1) formally define what it means to ask the question of whether voters primarily affect or elect policies, and 2) explain how our empirical strategy is able to distinguish between these two phenomena.

III.A. Model

We utilize the repeated election framework of Alesina [1988], adopting that study's modeling conventions and notation. Consider two parties, D (Democrats) and R (Republicans), in a particular Congressional district. The policy space is unidimensional, where party D's and R's per-period policy preferences are represented by quadratic loss functions, $u(l) = -(1/2)(l - c)^2$ and $v(l) = -(1/2)l^2$, respectively, where l is the policy variable and $c(>0)$, and 0, are their respective bliss points. As in Alesina, the analysis makes no distinction between the "party" and an individual nominee, so that the "electoral strength" of the party in a district is equivalent to the "electoral strength" of the party's nominee in that district, during the election. Also, candidates'/parties' bliss points are assumed to be exogenously determined.[7]

The timing of elections is as follows. Before election t, voters

7. This framework has little to say on the question of how candidates are selected. Alternative frameworks are possible and may generate different predictions. For example, the models proposed by Bernhardt and Ingberman [1985] and Banks and Kiewiet [1989] are quite different in spirit from the model used here. In those models, the challenger is at disadvantage because she cannot adopt the incumbent's position and is therefore forced to take a more extreme position. In equilibrium the low probability of defeating incumbent members of Congress deters potentially strong rivals from challenging them [Banks and Kiewiet 1989].

form expectations of the parties' policies, denoted x^e and y^e. At this point, the outcome of the election is uncertain to all agents in the model, with the probability of party D winning being P, which is "common knowledge." $P(x^e,y^e)$ is a function of x^e and y^e, and by assumption, when $x^e > y^e$, then $\partial P/\partial x^e$, $\partial P/\partial y^e < 0$; that is, more votes can be gained by moderating the policy position. If party D wins the election, x is implemented, and if party R wins, y is implemented. A rational expectations equilibrium is assumed throughout; $x = x^e$, and $y = y^e$. The game then repeats for period $t + 1$. Note that period t includes both the election and the subsequent Congressional session, and similarly for $t + 1$. For example, if $t = 1992$, t refers to the November 1992 election and the roll-call votes RC_t in the 1993–1994 Congressional session; $t + 1$ refers to the November 1994 election and the roll-call votes RC_{t+1} in the 1995–1996 session.

Alesina [1988] shows that the efficient frontier is given by $x^* = y^* = \lambda c$, where $\lambda \in (0,1)$. Because of the concavity preferences, both parties prefer a moderate policy with certainty to a fair bet. Three Nash equilibria are possible.

(a) Complete Convergence: $x^* = y^* = \lambda^* c$.

In this equilibrium, opposing parties agree to a moderate policy, by Nash bargaining on the efficient frontier. The "Folk Theorem" equilibrium is one where both parties "announce" the same, moderate policy, and the voters expect the moderate outcome, but as soon as a party deviates from the announced position, reputation is lost, and the game reverts to the uncooperative outcome, $y^* = 0$, $x^* = c$. As long as discount rates are sufficiently low, promises to adopt policy compromises are credible.

For our purposes, the key result is that $dx^*/dP^* = dy^*/dP^* = (d\lambda^*/dP^*)c > 0$, where P^* represents the underlying "popularity" of party D: the probability that party D would win at fixed policy positions, $x^e = c$ and $y^e = 0$.[8] An increase in P^* represents an *exogenous* increase in the popularity of party D, which would boost party D's "bargaining power" so that the equilibrium moves closer to her bliss point. This exogenous increase comes about from a "helicopter drop" of Democrats in the district, or campaign resources, or the advantage that comes from being the incumbent in the district. In this equilibrium, policy choices are implicitly

8. λ is used to characterize the entire efficient frontier. λ^*, on the other hand, denotes the Nash bargaining equilibrium.

constrained by voters. Thus, when dx^*/dP^*, $dy^*/dP^* > 0$, we say that voters *affect* candidates' policy choices.

Indeed, in this equilibrium—similar to Downs' original "median voter" model—voters exclusively affect policy choices, and do not elect policies at all: it is irrelevant for policy which party is actually elected.

(b) Partial Convergence: $0 \leq y^* \leq x^* \leq c$.

Is the result that voters *affect* policies—dx^*/dP^*, $dy^*/dP^* > 0$—robust to minor deviations from the complete convergence equilibrium? We show that it is. This agrees with our intuition that voters can induce policy compromise, even if they cannot force them to adopt identical positions. It also agrees with our intuition that a rejection of complete convergence says little about the relative degree to which voters affect or elect policies. Rejecting complete convergence simply implies that $y^* < x^*$, but nothing about whether $0 < y^*$ or $x^* < c$.

It is possible to extend Alesina's model to allow for parties to care about winning the seat, per se, in addition to caring about the policy outcome.[9] The result is that in general, $0 \leq y^* \leq x^* \leq c$, because there are values where $x = y$ is not Pareto efficient. Both parties can be made better off by one party moving closer to its bliss point, because there is an explicit benefit to obtaining office. A detailed proof is available on request.

The important point, for our purposes, is that the comparative static dx^*/dP^*, $dy^*/dP^* > 0$ is robust to this logical extension to the model. With an exogenously higher P^*, party D has a better "bargaining position" and therefore can compel the parties to agree on a position closer to party D's bliss point.

(c) Complete Divergence: $x^* = c$, $y^* = 0$.

In this equilibrium, voters expect nothing else than the parties to carry out their bliss points if elected, and the parties do just this. This can arise if promises to implement policy compromises are not credible. In this case, an increase in P^* now does *nothing* to the equilibrium: $dx^*/dP^* = dy^*/dP^* = 0$. This is a "corner solution," whereby an exogenous shock to P^* has no effect on candidates positions. Here, voters merely *elect* politicians' fixed policies.

9. Our extension should not be confused with that of Alesina and Spear [1988], in which parties agree to split the benefits of office. In our extension, they cannot split the benefits of office. This case should also not be confused with the partial convergent equilibria that can arise if discount rates are too low to support fully convergent equilibria. Alesina [1988] proves existence of these equilibria.

Among the above three equilibria, the full convergence equilibrium is not very realistic, and has already been empirically rejected by several authors. But a rejection of full convergence says little about whether politicians' behaviors are better characterized by partial convergence (voters can affect policy outcomes) or complete policy divergence (voters only elect policies). Distinguishing between these two equilibria is our goal. For this purpose, the key result of the theoretical framework is that differentiating between partial and complete divergence is equivalent to assessing whether dx^*/dP^*, $dy^*/dP^* > 0$ or dx^*/dP^*, $dy^*/dP^* = 0$.

We assume that voters are forward-looking and have rational expectations. This implies that voting records RC_{t+1}—roll-call votes *after* the election—are on average equal to voters' expectations. It is important to note that this is *not* the same as assuming that candidates can make binding precommitments. Politicians always have the option of not carrying out their preelection policy pronouncements. But in Alesina's repeated game equilibrium, candidates do carry out their "announced" policies because of the need to maintain a reputation.[10]

III.B. Estimating Framework

The above framework directly leads to our empirical strategy. Note first that the roll-call voting record RC_t of the representative in the district following the election t can be written as

$$RC_t = (1 - D_t)y_t + D_t x_t, \tag{1}$$

where D_t is the indicator variable for whether the Democrat won election t. A similar equation applies for RC_{t+1}. Simply put, only the winning candidate's intended policy is ultimately observable. In Appendix 1 we provide conditions under which the above expression can be transformed into

$$RC_t = \text{constant} + \pi_0 P_t^* + \pi_1 D_t + \varepsilon_t \tag{2}$$

$$RC_{t+1} = \text{constant} + \pi_0 P_{t+1}^* + \pi_1 D_{t+1} + \varepsilon_{t+1}, \tag{3}$$

where P^* is the measure of the electoral strength of party D—the probability of a party D victory at fixed platforms c and 0—and ε reflects heterogeneity in bliss points across districts. This equa-

10. Of course, the equilibrium depends on candidates not discounting the future too much.

tion simply parameterizes the derivatives dx^*/dP^*, dy^*/dP^* as π_0. It also allows an independent effect of party, π_1, which is reasonable given the existing evidence that party affiliation is an important determinant of roll-call voting records. In this equation, partial convergence (voters *affect* policy choices) implies that $\pi_0 > 0$. Full divergence (voters only *elect* policies) implies that $\pi_0 = 0$.

In general, we cannot observe P^*, so equation (2) cannot be directly estimated by OLS. But suppose that one could randomize D_t. Then D_t would be independent of ε_t and P_t^*. Also, if bliss points are exogenous—and hence are not influenced by who won the previous election—then D_t will have no impact on ε_{t+1}. It follows that

$$(4)\quad E[RC_{t+1}|D_t = 1] - E[RC_{t+1}|D_t = 0] = \pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}] + \pi_1[P^D_{t+1} - P^R_{t+1}] = \gamma$$

$$(5)\quad E[RC_t|D_t = 1] - E[RC_t|D_t = 0] = \pi_1$$

$$(6)\quad E[D_{t+1}|D_t = 1] - E[D_{t+1}|D_t = 0] = P^D_{t+1} - P^R_{t+1},$$

where D and R superscripts denote which party held the seat—and hence held the electoral advantage. For example, P^D_{t+1} denotes the *equilibrium* probability of a Democrat victory in $t + 1$ given that a Democrat held the seat during the campaign of $t + 1$; P^{*R}_{t+1} represents the "electoral strength" of the Democrat during the campaign of $t + 1$, given that a Republican held the seat. Note that while we cannot estimate P^{*D}_{t+1} and P^{*R}_{t+1}, we can estimate the P^D_{t+1} and P^R_{t+1} from the data.[11]

These three equations form the basis of our empirical analysis. Equation (4) shows that the total effect γ of a Democratic victory in t on voting records RC_{t+1} is the sum of two components, $\pi_1[P^D_{t+1} - P^R_{t+1}]$, and the remainder, $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$. The first term is the "elect" component. The second term is the "affect" component. The equation shows that the overall effect γ can be estimated by the simple difference in voting scores RC_{t+1} between districts won by Democrats and Republicans in t.

The next two equations show how to estimate the "elect"

11. It is important to distinguish between P^* and P. P^* is a measure of the underlying "popularity" of a party, the probability that party D will win if parties D and R are expected to choose c and 0, respectively. A change in P^* represents an exogenous change in popularity. On the other hand, P is the probability that party D will win, at whatever policies the parties are expected to choose.

component, which is the product of π_1 and $[P^D_{t+1} - P^R_{t+1}]$. π_1 is estimated by the difference in voting records RC_t.[12] $P^D_{t+1} - P^R_{t+1}$ is estimated by the difference in the fraction of districts won by Democrats in $t + 1$.

The "affect" component, $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$, can be estimated by $\gamma - \pi_1[P^D_{t+1} - P^R_{t+1}]$. If voters merely "elect" policies (complete divergence), we should observe little change in the candidates' intended policies following an exogenous increase in the probability of victory; that is, $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$ should be small. If voters not only choose politicians, but also affect their policy choices (partial convergence), candidates should move toward their bliss points in response to an exogenously higher probability of winning; that is, $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$ should be relatively large. This simple decomposition allows us to make quantitative statements about the relative importance of the "affect" and "elect" phenomena. We can compute what fraction of the total effect γ is explained by the "elect component" $\pi_1[P^D_{t+1} - P^R_{t+1}]$, and what fraction by the "affect component" $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$.

Note that the initial "random assignment" of D_t is crucial here. Without this, the estimated differences above would in general be biased for the quantities γ, and π_1.[13] As an example, without this random assignment, the simple difference in how Republicans and Democrats vote after election t would reflect both π_1, and that candidates are likely to have more liberal "bliss points" where Democrats hold the seat.

We argue that the examination of suitably "close" elections in period t isolates "as good as random assignment" in D_t. As would be expected from a valid regression-discontinuity design, among

12. As will be evident below, in principle, one could obtain an alternative estimate of π_1, by examining the difference in records RC_{t+1} among close elections in time $t + 1$. In practice, however, this makes little difference because we are pooling data from many years (i.e., the difference between estimating π_1 from data 1946–1994 and estimating it using data from 1948–1994).

13. More formally, without random assignment of D_t, the three expressions would become

$$\begin{aligned} E[RC_{t+1}|D_t = 1] - E[RC_{t+1}|D_t = 0] &= \gamma + E[\varepsilon_{t+1}|D_t = 1] - E[\varepsilon_{t+1}|D_t = 0] \\ E[RC_t|D_t = 1] - E[RC_t|D_t = 0] &= \pi_0(E[P^*_t|D_t = 1] - E[P^*_t|D_t = 0]) \\ &\quad + \pi_1 + E[\varepsilon_t|D_t = 1] - E[\varepsilon_t|D_t = 0] \\ E[D_{t+1}|D_t = 1] - E[D_{t+1}|D_t = 0] &= P^D_{t+1} - P^R_{t+1}. \end{aligned}$$

It is clear, from the expressions above, that without random assignment, the parameter estimates γ and π_1 would be biased.

elections decided by a very narrow margin, as long as there is some unpredictable component of the ultimate vote tally, who wins the election will be mostly determined by pure chance (e.g., unpredictable components of voter turnout on election day). This is shown more formally in Appendix 1.

By now it should be clear why it is necessary to examine the impact of who wins in t on RC_{t+1}, roll-call votes in period $t + 1$. The impact of who wins in t on RC_t, roll-call votes in t, only yields π_1. By estimating π_1 alone, it is only possible to test complete convergence $\pi_1 = 0$. This extreme hypothesis has already been tested by several studies.[14] But π_1 alone is not sufficient to say anything about the size of the "elect" phenomenon relative to the "affect" phenomenon. It is not sufficient for testing full divergence against partial convergence, and hence it is not sufficient for evaluating the Downsian perspective versus the alternative view that politicians face difficulties in credibly committing to policy compromises.

IV. Roll-Call Voting Records in the U. S. House

IV.A. Context

There are several reasons why the U. S. House of Representatives provides an ideal setting in which to empirically assess whether voters primarily affect or elect policies. First, the U. S. federal legislative body is virtually a two-party system, and policy convergence is frequently modeled in a two-party context. When there are more than two candidates, the basic insight of Downs' [1957] approach to policy convergence arguments becomes more complicated (see Osborne [1995]).

Second, it is well-known that Democrats and Republicans have different (and often directly opposing) policy positions. It is meaningful to ask whether electoral competition compels opposing parties' nominees to moderate their positions in the face of strong incentives to vote along party lines. If the U. S. House were a relatively nonpartisan environment (with "bliss points" relatively close together), the distinction between voters affecting or electing policies would be less important, and a test to distinguish between them less useful.

14. See, for example, Poole and Rosenthal [1984], Levitt [1996], and Snyder and Groseclose [2000].

Third, the U. S. House is arguably the most likely setting in which to observe policy convergence, if establishing reputations is important. U. S. House elections are held every two years, and there are no term limits (as opposed to gubernatorial and presidential elections), meaning that political careers can consist of several terms in office. Furthermore, political tenure in the House is often a stepping-stone to participating in electoral races for higher offices. For these reasons, it is plausible that candidates for the U. S. House have high discount factors, which would allow reputation to support convergent equilibria.

Finally, our empirical analysis focuses on Representatives' voting records. These votes are directly observable, and are part of the public record. In principle, voters can compare a legislator's record with their platforms and promises as candidates (and opponents can advertise any deviations during election campaigns). Convergent equilibria of the kind described in Alesina [1988] require that policy positions are perfectly observable by voters and that it can be determined whether politicians deviate from policy pronouncements.

IV.B. Data Description

We now discuss the choice of the dependent variable.[15] There are several alternative ways to measure Representatives' voting on legislation. A widely used measure is a voting score provided by the liberal political organization, Americans for Democratic Action (ADA). For each Congress, the ADA chooses about twenty high-profile roll-call votes, and creates an index that varies between 0 and 100 for each Representative of the House. Higher scores correspond to a more "liberal" voting record. Throughout the paper our preferred voting record index is the ADA score. Later, we show that our results are robust to many alternative interest groups scores and other voting record indices.

We utilize data on ADA scores for all Representatives in the U. S. House from 1946–1995, linked to election returns data during that period.[16] There is considerable variation in ADA scores *within* each party. For example, the distribution of ADA scores for Democrat and Republican Representatives in the three

15. All the data and the programs used in this paper are available at http://www.econ.ucla.edu/moretti/papers.html

16. To make the comparison across Congresses possible, we follow the literature and use "adjusted" ADA scores throughout the paper. This adjustment to the nominal ADA score, was devised by Groseclose, Levitt, and Snyder [1999].

most recent Congresses shows significant overlapping between the parties. It is not uncommon for Democrat representatives to vote more conservatively than Republican candidates, and vice versa.

One advantage of using ADA scores is that it is a widely used index in the literature. However, one limitation is that it includes only twenty votes per Congress, and the choice of what issues to include and what weight to assign to each issue is necessarily arbitrary. To assess how robust our results are to alternative measures of "liberalness" of roll-call votes, we have reestimated all our models using three alternative sets of voting record measures.

First, we use the DW-NOMINATE scores constructed by McCarty, Poole, and Rosenthal [1997]. Poole and Rosenthal [1985] developed the NOMINATE procedure to estimate a low-dimensional measure of political ideology in a complex multidimensional political world. NOMINATE is an attempt to estimate the *underlying* ideology that drives observed roll-call behavior by assigning legislators the ideological points that maximize the number of correctly predicted roll-call votes. The NOMINATE data have the advantage of including all roll-call votes, not an arbitrary subset of votes. It also ignores the Representative's political party and the legislative issue in question, so it is arguably more exogenous than the ADA scores.[17]

Second, for each member and each Congress, we construct our own measure of loyalty to the party leadership using the individual vote tallies on every issue voted on in the House. For

17. Poole and Rosenthal [1997] note that a single dimension would be unlikely to capture the division between Northern and Southern Democrats during the Civil Rights Era. Therefore, the NOMINATE procedure estimates a two-dimensional measure of ideology where the first dimension captures party loyalty and can be thought of as a liberal to conservative scale, and the second dimension captures the issues of race that divided the Democrats until the mid-1970s. To remain consistent with our discussion of a single ideological dimension, we restrict our analysis to the first dimension during the period where the second dimension had little predictive power. Specifically, we restrict our DW-NOMINATE analysis to 1975 and beyond. However, we have reestimated our models including DW-NOMINATE data for the entire 1946–1995 period and obtained very similar results. For completeness, we have also reestimated our ADA models including only data for the 1975–1995 period and obtained very similar results. We use the DW-NOMINATE scores as opposed to the Poole and Rosenthal's [1991] earlier D-NOMINATE scores because the DW data cover up through the 106th Congress while the D-NOMINATE data ends with the 99th Congress. McCarty, Poole, and Rosenthal [1997] note that the D-NOMINATE and DW-NOMINATE scores are highly correlated where both scores are available. See Poole and Rosenthal [1997] for a description of the NOMINATE procedure. Poole's [1999] rank order data yield similar results.

this measure, we calculate the percent of a representative's votes that agree with the Democrat party leader.[18]

Third, we use ratings from interest groups other than the ADA. We include both liberal and conservative ratings from groups such as the American Civil Liberties Union, the League of Women Voters, the League of Conservation Voters, the American Federation of Government Employees, the American Federation of State, County, Municipal Employees, the American Federation of Teachers, the AFL-CIO Building and Construction, the United Auto Workers, the Conservative Coalition, the U. S. Chamber of Commerce, the American Conservative Union, the Christian Voters Victory Fund, the Christian Voice, Lower Federal Spending, and Taxation with Representation. Not all the ratings are available in all years, so sample sizes vary when using these alternative ratings.

As we show below, our results are remarkably stable across alternative measures of roll-call votes. This finding lends some credibility to the conclusion that our estimates are not driven by the unique characteristics of one particular measure. See the Data Appendix for a detailed discussion of our samples and data sources.

V. Empirical Results

In this section we present our empirical results. Subsection V.A presents our main results with a simple graphical analysis that illustrates that changes in electoral strength appears to affect future voting records entirely because it alters the relative odds of which party's nominee will be elected to the House. That is, candidates do not seem to change their intended policies in response to large exogenous shocks to electoral strength. This is followed by more formal estimates of the key parameters of interest: γ, $\pi_1[P^D_{t+1} - P^R_{t+1}]$, and $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$. Subsection V.B provides evidence supporting our main identifying assumption—that among elections that turn out to be close, who wins is "as good as randomly assigned." In subsection V.C, we show that our results do not change substantially when we utilize a number of alternative voting record indices. Finally, in subsection V.D, we examine the sensitivity of our results to a functional form as-

18. The results are nearly identical if one uses the party whip instead of the party leader.

sumption utilized in our base model. The analysis within this more general framework confirms our findings.

V.A. Main Empirical Results

Graphical Analysis. As discussed above, the way to distinguish between full divergence and partial divergence is to analyze the effects on roll-call votes of an exogenous change in the probability of winning the election. The total effect of such an exogenous change on roll-call behavior (γ) can be split into two components: the elect component ($\pi_1[P^D_{t+1} - P^R_{t+1}]$) and the affect component ($\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$). If voters merely "elect" policies (complete divergence), we should observe little change in the candidates' intended policies following an exogenous increase in the probability of victory: $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$ should be small. If voters not only choose politicians, but also affect their policy choices (partial convergence), candidates should move toward their bliss points in response to an exogenously higher probability of winning: $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$ should be relatively important.

We begin with a simple graphical analysis of ADA scores. Figure I plots ADA scores at time $t + 1$ against the Democrat vote share at time t. As an example, we are relating the ADA scores for the representative from, say, the first California district during the 1995–1996 Congressional session to the Democratic vote share observed in the 1992 election in that district. In practice, we use all pairs of adjacent years from 1946 to 1995, except for the pairs where we cannot link districts due to redistricting (pairs with years ending with "0" and "2").

Throughout the paper the unit of observation is the district in a given year. But to give an overall picture of the data, each point in Figure I is an average of the ADA score in period $t + 1$ within 0.01-wide intervals of the vote share at time t. The vertical line marks 50 percent of the two-party vote share. Districts to the right of the vertical line are districts won by Democrats in election t, districts to the left are districts won by Republicans in election t. The continuous line is the predicted ADA_{t+1} score from a regression that includes a fourth-order polynomial in vote share and a dummy for observations above the 50 percent threshold, and an interaction of the dummy and the polynomial. The dotted lines represent pointwise 95 percent confidence intervals of this approximation.

A striking feature of the figure is that ADA scores appear to

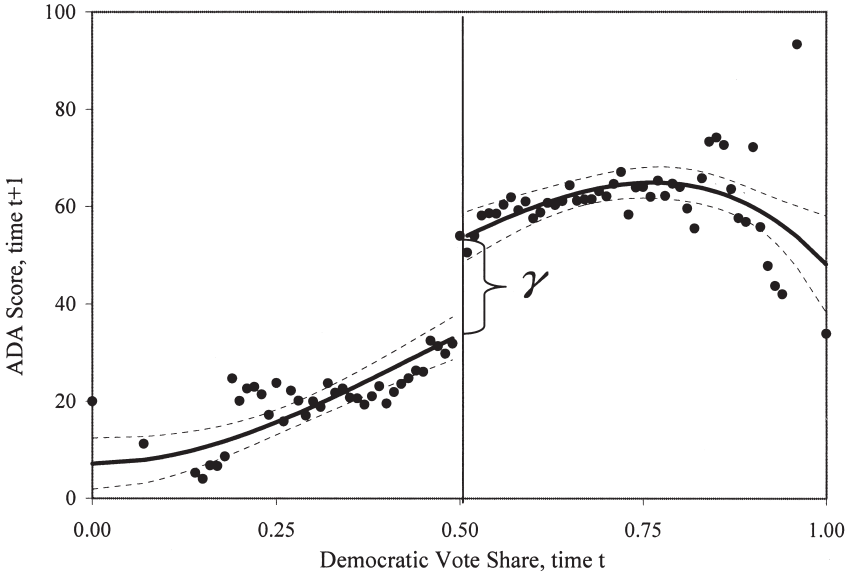

FIGURE I

Total Effect of Initial Win on Future ADA Scores: γ

This figure plots ADA scores after the election at time $t + 1$ against the Democrat vote share, time t. Each circle is the average ADA score within 0.01 intervals of the Democrat vote share. Solid lines are fitted values from fourth-order polynomial regressions on either side of the discontinuity. Dotted lines are pointwise 95 percent confidence intervals. The discontinuity gap estimates

$$\gamma = \underbrace{\pi_0(P^{*D}_{t+1} - P^{*R}_{t+1})}_{\text{“Affect”}} + \underbrace{\pi_1(P^{*D}_{t+1} - P^{*R}_{t+1})}_{\text{“Elect”}}.$$

be a continuous and smooth function of vote shares everywhere, except at the threshold that determines party membership. There is a large discontinuous jump in ADA scores at the 50 percent threshold. Compare districts where the Democrat candidate barely lost in period t (for example, vote share is 49.5 percent), with districts where the Democrat candidate barely won (for example, vote share is 50.5 percent). If the regression discontinuity design is valid, the two groups of districts should appear ex ante similar in every respect—on average. The difference will be that in one group, the Democrats will be the incumbent for the next election ($t + 1$), and in the other it will be the Republicans. Districts where the Democrats are the incumbent party for election $t + 1$ elect representatives who have much higher ADA scores, compared with districts where the Republican candidate

barely won and became the incumbent—on average. The size of the jump appears to be fairly large, at around twenty ADA points.

What does this discontinuity mean? Formally, the gap is a credible estimate of the parameter γ in equation (4). Intuitively, it is unsurprising to observe some discontinuity. We know that party affiliation is an important determinant of roll-call behavior. We also know that if a Democrat (Republican) is elected in period t, a Democrat (Republican) is more likely to be elected in period $t + 1$ in the same district, due to the incumbency advantage. The party effect, together with the electoral advantage of incumbency, suggests that we should expect to find a gap in Figure I. It is not surprising to observe that, for example, the 1995–1996 voting records are more liberal in the districts that were won by Democrats in 1992. The 1995–1996 representative, after all, is more likely be a Democrat. In Sections II and III we called this particular mechanism the "elect component," and denoted it $\pi_1[P^D_{t+1} - P^R_{t+1}]$.

There is a second component that contributes to γ. If candidates are constrained by expected voters' behavior, then a Democrat who is challenging an incumbent Republican (left side of the graph) would be expected to moderate his intended policies more, compared with an incumbent Democrat (right side of the graph). After all, the incumbent would be in a much stronger electoral position compared with the challenger. This is the other reason why voting scores should be more liberal where the Democrat is the incumbent, and hence why there should be a gap in Figure I. In Sections II and III we labeled this phenomenon "voters affecting policies," and denoted it $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$.

The discontinuity γ illustrated in Figure I is equal to $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}] + \pi_1[P^D_{t+1} - P^R_{t+1}]$. While it is not surprising to find that $\gamma > 0$, the real question is whether $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$ or $\pi_1[P^D_{t+1} - P^R_{t+1}]$ dominates. Our empirical strategy is simple. We can directly estimate $\pi_1[P^D_{t+1} - P^R_{t+1}]$, by separately estimating π_1, the expected difference in voting between the two parties, as well as $[P^D_{t+1} - P^R_{t+1}]$, the electoral advantage to incumbency. We can subtract this from the total effect γ to determine the magnitude of the "affect component" $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$.

In Figure II we illustrate the two elements that make up the elect component, π_1 and $[P^D_{t+1} - P^R_{t+1}]$. The top panel in Figure II plots ADA scores at time t against the Democrat vote share at time t. As in Figure I, average ADA scores appear to be a continuous and smooth function of vote shares everywhere, except at

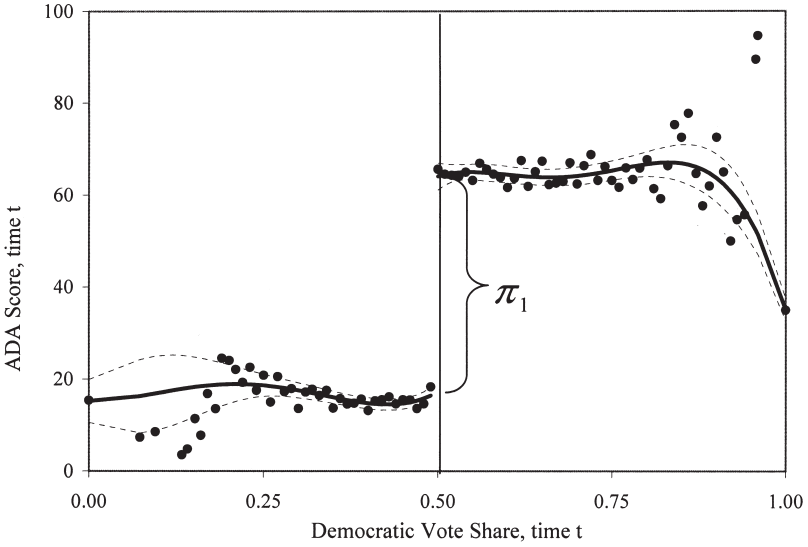

FIGURE IIa
Effect of Party Affiliation: π_1

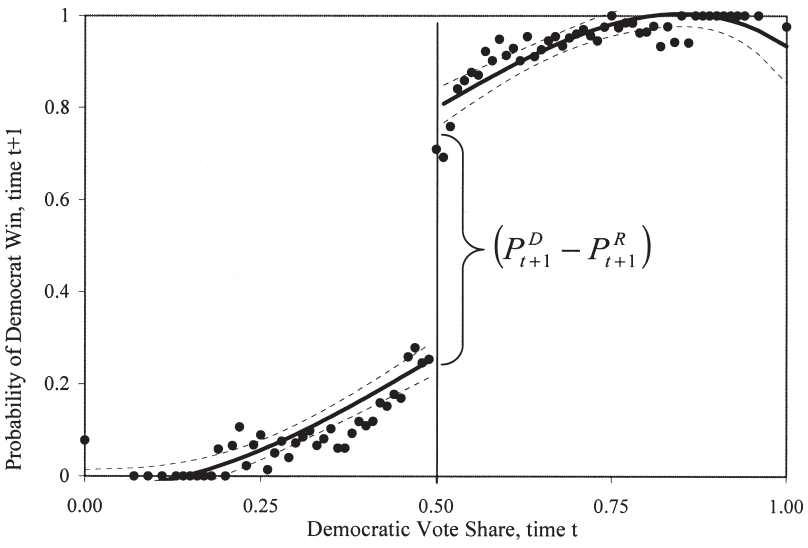

FIGURE IIb
Effect of Initial Win on Winning Next Election: $(P^D_{t+1} - P^R_{t+1})$

Top panel plots ADA scores after the election at time t against the Democrat vote share, time t. Bottom panel plots probability of Democrat victory at $t + 1$ against Democrat vote share, time t. See caption of Figure III for more details.

the threshold that determines party membership. The jump is a credible estimate of π_1 in equation (5).

Compare a district where the Democrat candidate barely lost at time t (for example, vote share of 49.5 percent), with a district where the Democrat candidate barely won at time t (for example, vote share of 50.5 percent). Again, if the regression discontinuity design is valid and has generated random assignment of who wins in t, then the average voting records of Democrats who are *barely* elected will credibly represent, on average, how Democrats *would have* voted in the districts that were in actuality, barely won by Republicans (and vice versa). The observed difference in voting scores represents a credible estimate of the average policy differences between the two parties across districts—the direct influence of party affiliation on voting scores. The difference at the 50 percent threshold appears quite large, with a gap of about 45 points.

Finally, the bottom panel of Figure II plots estimates of the probability that the Democrat will win election $t + 1$ for a given Democratic vote share at t. As in the previous cases, the figure shows a smooth function of vote shares everywhere, except at the threshold that determines which party won t. The size of the jump estimates $[P^D_{t+1} - P^R_{t+1}]$ in equation (6).[19]

The discontinuity around the 50 percent threshold indicates that, for example, districts which barely elected a Democrat in t are more likely to elect a Democrat in $t + 1$, consistent with a causal incumbency advantage. This is consistent with these districts experiencing exogenous increases in the probability of electing a Democrat (Republican) in 1994.

The total effect γ, given by the discontinuity in Figure I, appears to be about twenty ADA points. Figure IIa shows that the estimate of π_1 is about 45 points, and Figure IIb shows that $[P^D_{t+1} - P^R_{t+1}]$ is around 0.5. The "elect component" $\pi_1[P^D_{t+1} - P^R_{t+1}]$ is thus approximately $45 \times 0.5 = 22.5$. The small difference (20–22.5) implies that the *entire* effect of an exogenous change in electoral strength on future ADA scores is not operating through how candidates' policy choices respond to changes in the probability of winning. Instead, the effect is operating through simply changing the relative odds that a party will retain control over the seat. That is, this graphical analysis indicates that voters

19. This regression-discontinuity estimate of the incumbency advantage is documented in Lee [2001, 2003].

TABLE I
RESULTS BASED ON ADA SCORES—CLOSE ELECTIONS SAMPLE

	Total effect			Elect component	Affect component
	γ	π_1	$(P^D_{t+1} - P^R_{t+1})$	$\pi_1[(P^D_{t+1} - P^R_{t+1})]$	$\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$
Variable	ADA_{t+1}	ADA_t	DEM_{t+1}	(col. (2)*(col. (3))	(col. (1)) − (col. (4))
	(1)	(2)	(3)	(4)	(5)
Estimated gap	21.2	47.6	0.48		
	(1.9)	(1.3)	(0.02)		
				22.84	−1.64
				(2.2)	(2.0)

Standard errors are in parentheses. The unit of observation is a district-congressional session. The sample includes only observations where the Democrat vote share at time t is strictly between 48 percent and 52 percent. The estimated gap is the difference in the average of the relevant variable for observations for which the Democrat vote share at time t is strictly between 50 percent and 52 percent and observations for which the Democrat vote share at time t is strictly between 48 percent and 50 percent. Time t and $t + 1$ refer to congressional sessions. ADA_t is the adjusted ADA voting score. Higher ADA scores correspond to more liberal roll-call voting records. Sample size is 915.

primarily elect policies (full divergence) rather than affect policies (partial convergence).

Here we quantify our estimates more precisely. In the analysis that follows, we restrict our attention to "close elections"—where the Democrat vote share in time t is strictly between 48 and 52 percent. As Figures I and II show, the difference between barely elected Democrat and Republican districts among these elections will provide a reasonable approximation to the discontinuity gaps. There are 915 observations, where each observation is a district-year.[20]

Table I, column (1), reports the estimated total effect γ, the size of the jump in Figure I. Specifically, column (1) shows the difference in the average ADA_{t+1} for districts for which the Democrat vote share at time t is strictly between 50 percent and 52 percent and districts for which the Democrat vote share at time t is strictly between 48 percent and 50 percent. The estimated difference is 21.2.

In column (2) we estimate the coefficient π_1, which is equal to the size of the jump in Figure IIa. The estimate is the difference in the average ADA_t for districts for which the Democrat vote

20. In 68 percent of cases, the representative in period $t + 1$ is the same as the representative in period t. The distribution of close elections is fairly uniform across the years. In a typical year there are about 40 close elections. The year with the smallest number is 1988, with twelve close elections. The year with the largest number is 1966, with 92 close elections.

share at time t is strictly between 50 percent and 52 percent and districts for which the Democrat vote share at time t is strictly between 48 percent and 50 percent. The estimated difference is 47.6.

In column (3) we estimate the quantity $[P^D_{t+1} - P^R_{t+1}]$, which is equal to the size of the discontinuity documented in Figure IIb. The estimated jump is 0.48. This indicates that if the Democrat (Republican) candidate wins a close election in a given district in, say, 1992, the Democrat (Republican) candidate in the same district has a 0.48 higher probability of winning in 1994. This is indeed consistent with the notion that the party that already holds a seat holds a substantial electoral advantage.

In column (4) we multiply the estimates in columns (2) and (3) to obtain an estimate of the elect component, $\pi_1[P^D_{t+1} - P^R_{t+1}]$. The product is 22.84, which is not statistically different from the estimate of γ in column (1). Because estimates of γ and $\pi_1[P^D_{t+1} - P^R_{t+1}]$ are quite similar, we conclude that the "affect component" is quite small. In column (5) we subtract the estimate in column (4) from the estimate in column (1) to yield $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$. The difference is virtually zero.

What is the relative importance of the "elect component" $\pi_1[P^D_{t+1} - P^R_{t+1}]$, and the "affect component" $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$? Our results indicate that $\pi_1[P^D_{t+1} - P^R_{t+1}]$ overwhelmingly dominates $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$. Indeed, it entirely explains the overall effect γ. Below, we show that this is true not only on average, but also for every decade taken separately.

V.B. Tests for Quasi-Random Assignment

It is important to note that our empirical test crucially relies on the assumption of random assignment of the winner in close elections in t. Specifically, the key identifying assumption in our analysis is that as one compares closer and closer elections, all *predetermined* characteristics of Republican and Democratic districts (including the district-specific bliss points) become more and more similar. If this assumption does not hold, our estimates are likely to be biased.

Intuitively, this assumption seems to make sense. While Republican and Democrat districts are likely to be very different in general, the difference should decline as we examine elections whereby "pure luck" is a more important determinant of who wins—in other words, elections that turn out to be won by a tiny

margin. In the Appendix we provide a formal discussion of this assumption. Here, we provide two pieces of empirical evidence to support this assumption.

First, if examining close elections truly provides random assignment, characteristics determined before time t should be the same on both sides of the 50 percent threshold—on average.[21] We find that as we compare closer and closer elections, Republican and Democrat districts do have similar observable characteristics. Consider, for example, geographical location. There are sizable geographical differences in the entire sample. Averaging over the entire time period, Democrats are significantly more likely to be elected in the South than in the North and the West. However, as we start restricting the sample to closer and closer elections, the geographical differences decrease. For elections that are within only two percentage points from the threshold, the differences are not statistically significant.

This is shown graphically in Figures III and IV, which plot average district characteristics against Democratic vote share. Other than geographical location, we consider the following predetermined characteristics: real income, percentage with high school degree, percentage black, percentage eligible to vote, and size of the voting population. Generally, the figures indicate that the difference at the 50 percent threshold is small and statistically insignificant.

Table II illustrates the same point by quantifying the difference between Democrat and Republican districts for a larger set of characteristics. In particular, we examine all the characteristics shown in Figures III and IV, as well as the fraction of open seats, percent urban, percent manufacturing employment, and percent eligible to vote.[22] Column (1) includes the entire sample. Columns (2) to (5) include only districts with Democrat vote share between 25 percent and 75 percent, 40 percent and 60 percent, 45 percent and 55 percent, and 48 percent and 52 percent, respectively. The model in column (6) is equivalent to Figures III and IV, since it includes a fourth-order polynomial in Democrat vote

21. See Lee [2003] for the conditions under which RD designs can generate variation in the treatment that is as good as randomized.

22. Data on districts' characteristics in each election year are from the last available Census of Population. Because the census takes place every ten years, standard errors allow for clustering at the district-decade level.

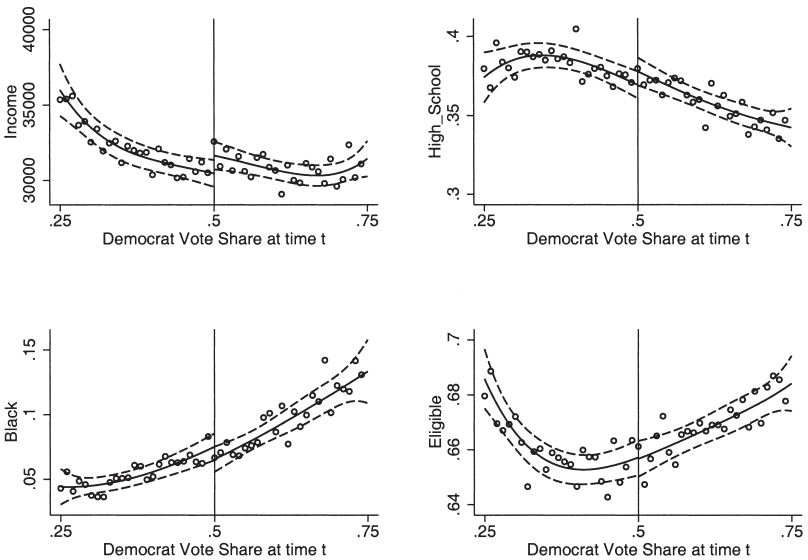

FIGURE III
Similarity of Constituents' Characteristics in Bare Democrat and Republican Districts–Part 1

Panels refer to (from top left to bottom right) the following district characteristics: real income, percentage with high-school degree, percentage black, percentage eligible to vote. Circles represent the average characteristic within intervals of 0.01 in Democrat vote share. The continuous line represents the predicted values from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line represents the 95 percent confidence interval.

share. The coefficient reported in column (6) is the predicted difference at 50 percent. The table confirms that, for many observable characteristics, there is no significant difference in a close neighborhood of 50 percent. One important exception is the percentage black, for which the magnitude of the discontinuity is statistically significant.[23]

As a consequence, estimates of the coefficients in Table I from regressions that include these covariates would be expected to produce similar results—as in a randomized experiment—since

23. This is due to few outliers in the outer part of the vote share range. When the polynomial is estimated including only districts with vote share between 25 percent and 75 percent, the coefficients becomes insignificant. The gap for percent urban and open seats, while not statistically significant at the 5 percent level, is significant at the 10 percent level.

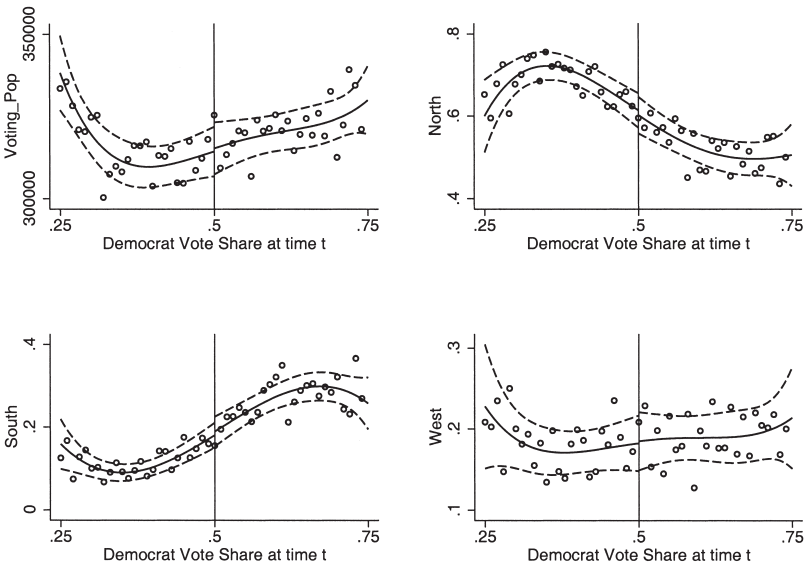

FIGURE IV

Similarity of Constituents' Characteristics in Bare Democrat and Republican Districts—Part 2

Panels refer to (from top left to bottom right) the following district characteristics: voting population, North, South, West. Circles represent the average characteristic within intervals of 0.01 in Democrat vote share. The continuous line represents the predicted values from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line represents the 95 percent confidence interval.

all predetermined characteristics appear to be orthogonal to D_t. We have reestimated all the models in Table I conditioning on all of the district characteristics in Table II, and found estimates that are virtually identical to the ones in Table I.

As a similar empirical test of our identifying assumption, in Figure V we plot the ADA scores from the Congressional sessions that *preceded* the determination of the Democratic two-party vote share in election t. Since these past scores have already been determined by the time of the election, it is yet another predetermined characteristic (just like demographic composition, income levels, etc.). If the RD design is valid, then we should observe no discontinuity in these lagged ADA scores—just as we would expect, in a randomized experiment, to see no systematic differences in any variables determined prior to the experiment. The

TABLE II
DIFFERENCE IN DISTRICT CHARACTERISTICS BETWEEN DEMOCRAT AND REPUBLICAN DISTRICTS, BY DISTANCE FROM 50 PERCENT

	All	+/− 25	+/− 10	+/− 5	+/− 2	Polynomial
	(1)	(2)	(3)	(4)	(5)	(6)
North	−0.211	−0.156	−0.096	−0.054	−0.059	−0.041
	(0.018)	(0.019)	(0.021)	(0.024)	(0.036)	(0.045)
South	0.250	0.145	0.093	0.053	0.009	0.015
	(0.015)	(0.014)	(0.016)	(0.019)	(0.028)	(0.036)
West	−0.031	−0.012	−0.036	−0.003	0.001	0.001
	(0.013)	(0.015)	(0.020)	(0.017)	(0.020)	(0.036)
Log income	−0.086	−0.036	0.014	0.026	0.030	0.052
	(0.013)	(0.012)	(0.014)	(0.017)	(0.026)	(0.033)
Percentage high-school grad.	−0.035	−0.024	−0.008	−0.001	0.001	0.008
	(0.003)	(0.003)	(0.004)	(0.004)	(0.007)	(0.008)
Percentage urban	0.070	0.065	0.053	0.053	0.056	0.053
	(0.011)	(0.011)	(0.012)	(0.014)	(0.023)	(0.028)
Percentage black	0.082	0.042	0.013	0.003	−0.003	−0.053
	(0.005)	(0.004)	(0.004)	(0.005)	(0.009)	(0.013)
Manufacturing employment	−0.002	0.000	0.004	0.004	0.005	0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)	(0.005)
Total population	−1817.9	3019.2	4961.5	3211.4	8640.4	2007.5
	(3517.3)	(3723.0)	(4562.4)	(5524.2)	(8427.9)	(10483.0)
Percentage eligible to vote	0.005	0.010	0.007	0.006	−0.003	−0.003
	(0.002)	(0.002)	(0.003)	(0.004)	(0.006)	(0.007)
Open seats	0.070	0.065	0.053	0.053	0.056	0.053
	(0.011)	(0.011)	(0.012)	(0.014)	(0.023)	(0.028)
Number of observations	13413	10229	4174	2072	910	13413

Standard errors are in parentheses. The unit of observation is a district-congressional session. Columns (1) to (5) report the difference in average district characteristics between Democrat and Republican districts. Column (1) includes the entire sample. Columns (2) to (5) include only districts with Democrat vote share between 25 percent and 75 percent, 40 percent and 60 percent, 45 percent and 55 percent, and 48 percent and 52 percent, respectively. The model in column (6) includes a fourth-order polynomial in Democrat vote share that enters separately for vote share above and below 50 percent. The coefficient reported in column (6) is the predicted difference at 50 percent. All standard errors account for district-decade clustering.

lack of discontinuity in the figure lends further credibility to our identifying assumption.[24]

Overall, the evidence strongly supports a valid regression discontinuity design. And as a consequence, it appears that among close elections, who wins appears virtually randomly assigned, which is the identifying assumption of our empirical strategy.

24. The estimated gap is 3.5 (5.6).

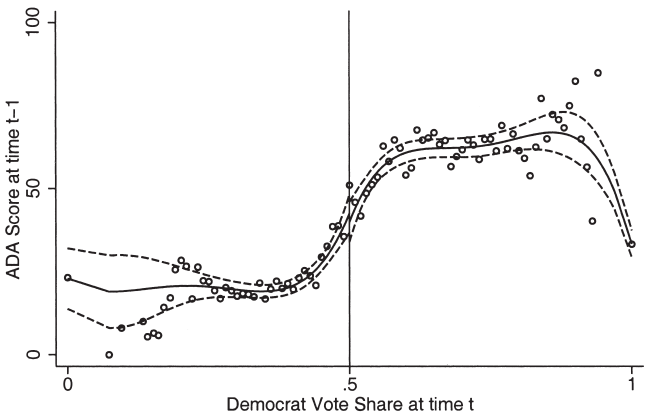

FIGURE V

Specification Test: Similarity of Historical Voting Patterns between Bare Democrat and Republican Districts

The panel plots one time lagged ADA scores against the Democrat vote share. Time t and $t - 1$ refer to congressional sessions. Each point is the average lagged ADA score within intervals of 0.01 in Democrat vote share. The continuous line is from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line is the 95 percent confidence interval.

V.C. Sensitivity to Alternative Measures of Voting Records

Our results so far are based on a particular voting index, the ADA score. In this section we investigate whether our results generalize to other voting scores. We find that the findings do not change when we use alternative interest groups scores, or other summary measures of representatives' voting records.

Table III is analogous to Table I, but instead of using ADA scores, it is based on two alternative measures of roll-call voting. The top panel is based on McCarty, Poole, and Rosenthal's DW-NOMINATE scores. The bottom panel is based on the percent of individual roll-call votes cast that are in agreement with the Democrat party leader. All the qualitative results obtained using ADA scores (Table I) hold up using these measures. When we use the DW-NOMINATE scores, γ is -0.36, remarkably close to the corresponding estimate of $\pi_1[P^D_{t+1} - P^R_{t+1}]$ in column (4), which is -0.34. The estimates are negative here because, unlike ADA scores, higher Nominate scores correspond to a more conservative voting record. When we use the measure "percent voting with the Democrat leader," γ is 0.13, almost indistinguishable from the

TABLE III
RESULTS BASED ON NOMINATE SCORES AND ON PERCENT VOTED LIKE DEMOCRAT LEADERSHIP—CLOSE ELECTIONS SAMPLE

Variable	Total effect γ Z_{t+1} (1)	π_1 Z_t (2)	$(P^D_{t+1} - P^R_{t+1})$ DEM_{t+1} (3)	Elect component $\pi_1[(P^D_{t+1} - P^R_{t+1})]$ (col. (2)*(col. (3)) (4)	Affect component $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$ (col. (1)) − (col. (4)) (5)
(a) Results based on Nominate scores					
Estimated gap	−0.36	−0.58	0.62		
	(0.03)	(0.02)	(0.04)		
				−0.34	−0.02
				(0.04)	(0.04)
(b) Results based on percent voted like Democrat leadership					
Estimated gap	0.13	0.29	0.46		
	(0.01)	(0.006)	(0.02)		
				0.13	0.00
				(0.02)	(0.02)

Standard errors are in parentheses. The unit of observation is a district-congressional session. The sample includes only observations where the Democrat vote share at time t is strictly between 48 percent and 52 percent. The estimated gap is the difference in the relevant variable for observations for which the Democrat vote share at time t is strictly between 50 percent and 52 percent and observations for which the Democrat vote share at time t is strictly between 48 percent and 50 percent. Time t and $t + 1$ refer to congressional sessions. The top panel uses the DW-NOMINATE score constructed by McCarty, Poole, and Rosenthal. Higher Nominate scores correspond to more conservative roll-call voting records. The bottom panel uses the percent of a representative's votes that agree with the Democrat party leader. Sample size is 276 in top panel and 1010 in bottom panel.

estimate $\pi_1[P^D_{t+1} - P^R_{t+1}]$ in column (4), which is 0.13. We show the graphical analysis for the estimate of π_1 in Figure VI.

Our empirical findings are also not sensitive to the use of ratings from various liberal and conservative interest groups. Liberal interest groups include the American Civil Liberties Union, the League of Women Voters, the League of Conservation Voters, the American Federation of Government Employees, the American Federation of State, County, and Municipal Employees, the American Federation of Teachers, the AFL-CIO Building and Construction, and the United Auto Workers. Conservative groups include the Conservative Coalition, the U. S. Chamber of Commerce, the American Conservative Union, and the Christian Voice. All the ratings range from 0 to 100. For liberal groups, low ratings correspond to conservative roll-call votes, and high ratings correspond to liberal roll-call votes. For conservative groups the opposite is true.

These alternative ratings yield results that are qualitatively similar to our findings in Table I and III. Instead of presenting these results in a table format as we did in Table I and III, we present the main results in graphical form. We summarize our

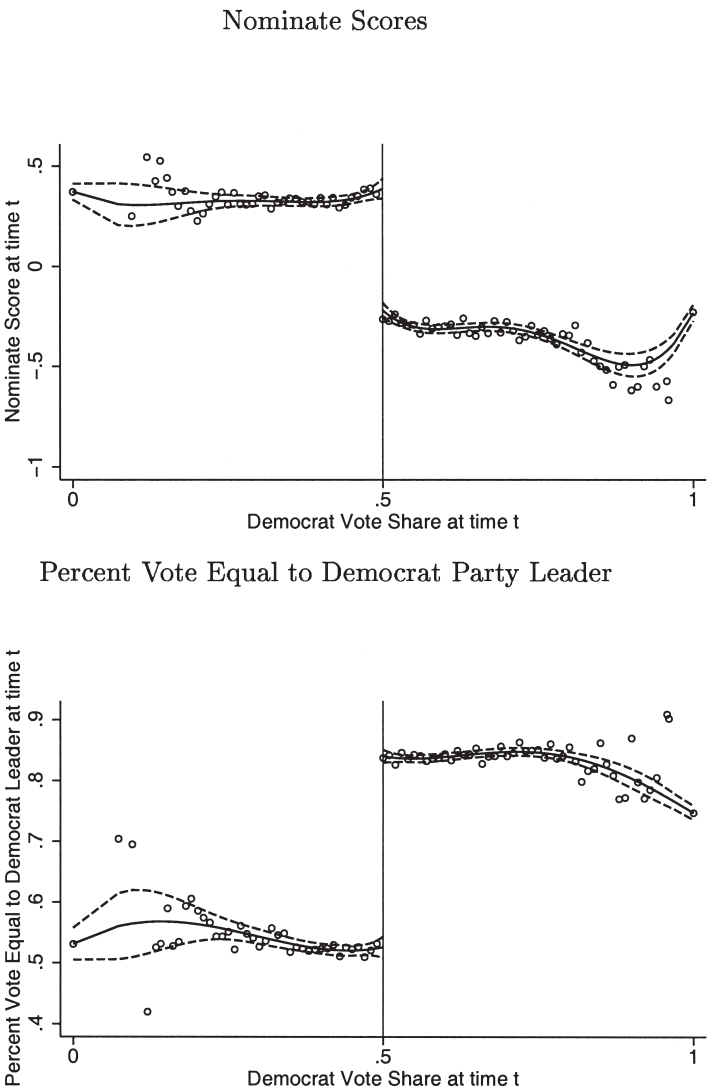

FIGURE VI

Nominate Scores, by Democrat Vote Share; and Percent Voted with Democrat Leader, by Democrat Vote Share

The top panel plots DW-Nominate scores at time t against the Democrat vote share at time t. Circles represent the average Nominate score within intervals of 0.01 in Democrat vote share. The bottom panel plots the fraction of a Representative's votes that agree with the Democrat party leader at time t against the Democrat vote share at time t. Circles represent the percent voted with Democrat leader within intervals of 0.01 in Democrat vote share. The continuous line is from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line is the 95 percent confidence interval.

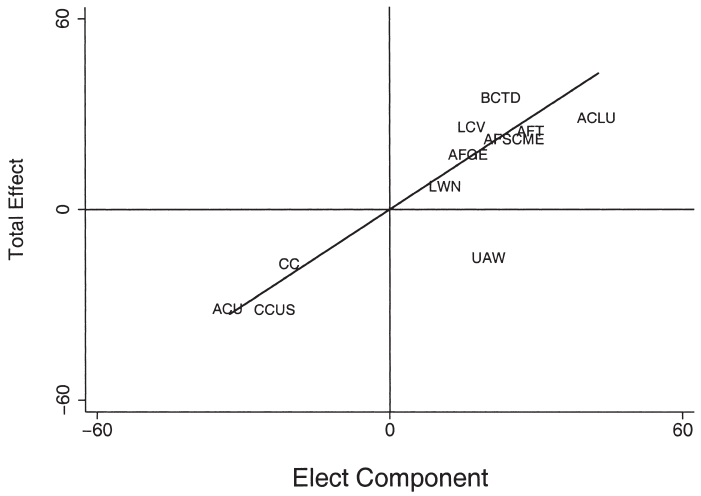

FIGURE VII
Total Effect [γ] Versus "Elect Policy" Component [$\pi_1(P^D_{t+1} - P^R_{t+1})$], for Alternative Interest Group Ratings

The figure plots estimates of the Total Effect [γ] on the y-axis against estimates of the "Elect Policy" component [$\pi_1(P^D_{t+1} - P^R_{t+1})$] on the x-axis for eleven interest groups ratings. The line is the 45° line. The closer the total effect is to the "elect component" effect for an interest group, the closer that group symbol is to the 45° line. All ratings are between 0 and 100. Higher ratings of liberal interest groups correspond to more liberal roll-call voting records. Higher ratings of conservative interest groups correspond to more conservative roll-call voting records.

ACLU is America Civil Liberties Union; LWV is League of Women Voters; LCV is League of Conservation Voters; AFGE is American Federal Government Employees; AFSCME is American Federation State, County, Municipal Employees; AFT is American Federation of Teachers; BCTD is AFL-CIO Building and Construction; UAW is United Auto Workers; CC is Conservative Coalition; CCUS is U. S. Chamber of Commerce; ACU is American Conservative Union.

results in Figure VII, where we plot our estimate of γ against our estimate of $\pi_1[P^D_{t+1} - P^R_{t+1}]$ for each of these alternative interest group ratings. The diagonal is the 45° degree line. Most estimates are on the line or close to the line, indicating again that across a variety of different interest groups scores, the results are highly consistent with the full policy divergence hypothesis.[25]

Our qualitative findings seem insensitive to the choice of voting score. Representatives' policy positions, on a wide array of

25. Figures VIII to XI show that the relationship between ratings, and the democrat vote share shares the same general features as the relationships for ADA, DW-NOMINATE, and "Percent vote with Democrat Leader." In all cases, we find a large discontinuity.

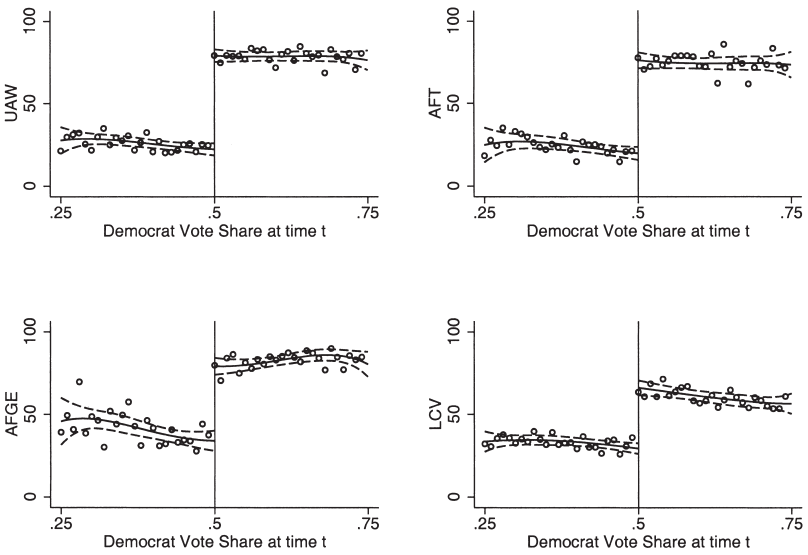

FIGURE VIII

Liberal Interest Groups Ratings, by Democrat Vote Share—Part 1

The top panel on the left refers to ratings from United Auto Workers. The top panel on the right refers to ratings from American Federation of Teachers. The bottom panel on the left refers to ratings from American Federation of Government Employees. The bottom panel on the right refers to ratings from League of Conservative Voters. Circles represent average ratings within intervals of 0.01 in Democrat vote share. The continuous line is from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line is the 95 percent confidence interval.

issues, do not seem to respond to exogenous changes in electoral strength. Voters appear to elect, rather than affect, candidates' platforms.

V.D. Heterogeneity

We now turn to the important issue of heterogeneity. Candidates' bliss points can be very different across districts or over time. For example, in any given year, a Democrat from Alabama is likely to have a bliss point that is quite different from a Democrat from Massachusetts. Our main results in subsection V.A are based on a model where candidates' positions can vary across districts and years. But one implicit functional form assumption that we adopted was that the *difference* in policy positions between Democrat and Republican candidates is constant across districts and over time. This assumption is violated if, for

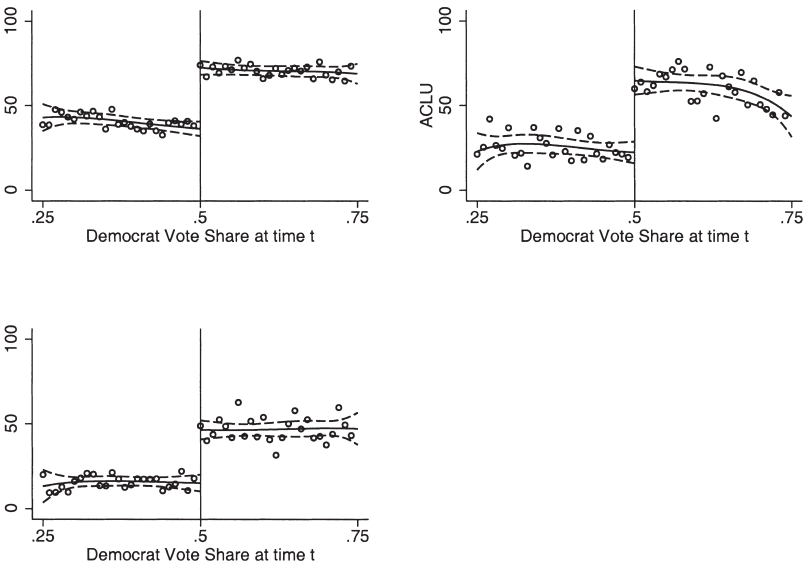

FIGURE IX

Liberal Interest Groups Ratings, by Democrat Vote Share—Part 2

The top panel on the left refers to ratings from League of Women Voters. The top panel on the right refers to ratings from American Civil Liberties Union. The bottom panel on the left refers to ratings from Taxation with Representation. Circles represent average ratings within intervals of 0.01 in Democrat vote share. The continuous line is from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line is the 95 percent confidence interval.

example, the gap in intended policies between Democrats and Republicans from Alabama is different from the gap between Democrats and Republicans from Massachusetts.

We show, however, that our findings are robust to a more general framework that allows for virtually unrestricted heterogeneity in the gap between opposing candidates' policies across time and districts. The details can be found in Appendix 3.

Intuitively, we know that the total effect γ is partially driven by the impact of who wins election t on the composition of Democrats and Republicans in office after election $t + 1$. In the Appendix we show that it is possible, ex post, to identify the "marginal" districts that switched from Republican to Democrat in $t + 1$, *because* the Democrat won in t. By deleting these districts from the sample, we can examine the impact of an increase in electoral strength on policy positions, without the confounding effects of the compositional change (i.e., the "elect component").

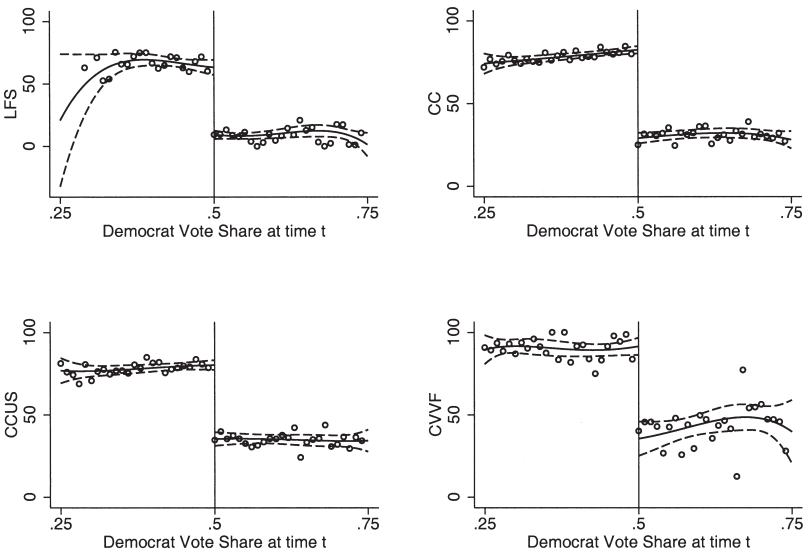

FIGURE X

Conservative Interest Groups Ratings, by Democrat Vote Share—Part 1

The top panel on the left refers to ratings from the Lower Federal Spending Support Score. The top panel on the right refers to ratings from the Conservative Coalition. The bottom panel on the left refers to ratings from the Chamber of Commerce. The bottom panel on the right refers to ratings from the Christian Voters Victory Fund. Circles represent average ratings within intervals of 0.01 in Democrat vote share. The continuous line is from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line is the 95 percent confidence interval.

Our findings using this more general framework indicate small estimates of $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$. This implies that our finding that voters primarily elect policies is not an artifact of a functional form assumption of our basic specification.

We conclude this section considering a different type of heterogeneity: heterogeneity over time. In Table IV we replicate the estimates in Table I, presenting separate estimates by decade.[26] Column (2) shows that the discontinuity estimated by pooling all the years (Table II, column (2)) masks some variation in the discontinuity gap across states and years. This is not surprising, as the political science literature, for example, has noted that in the South, Democrats and Republicans are ideologically closer

26. Because there are few observations in 1940 and 1990, we merge 1940 with 1950, and 1980 with 1990.

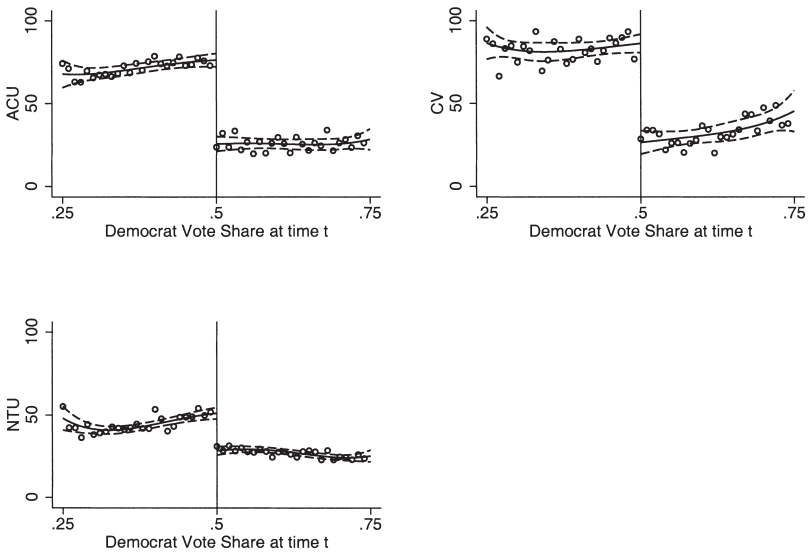

FIGURE XI

Conservative Interest Groups Ratings, by Democrat Vote Share—Part 2

The top panel on the left refers to ratings from the American Conservative Union. The top panel on the right refers to ratings from the Christian Voice. The bottom panel on the left refers to ratings from the National Taxpayers' Union. Circles represent average ratings within intervals of 0.01 in Democrat vote share. The continuous line is from a fourth-order polynomial in vote share fitted separately for points above and below the 50 percent threshold. The dotted line is the 95 percent confidence interval.

than they are in the North. The estimated discontinuity is relatively smaller during the 1970s, and relatively larger during the 1990s. When we stratify by region and decade (not shown), the discontinuity is relatively smaller in the South in the 1950s and 1970s. Consistent with previous evidence, column (3) shows that the incumbency advantage has increased over time. The key findings of the table are in column (5). Notably, the relative importance of the "elect component" and the "affect component" remains fairly stable over time. Entries in column (5) indicate that our findings in Table I hold in each decade from 1940 to 1990.[27]

27. We have also estimated separate models for presidential election years and midterm election years. Our results are similar to the ones in Table I. For example, estimates similar to column (5) for presidential election years and midterm election years are, respectively, −0.1 (2.8) and −2.5 (3.0).

TABLE IV
RESULTS BASED ON ADA SCORES, BY DECADE—CLOSE ELECTIONS SAMPLE

Variable	(1) Total effect γ ADA_{t+1}	(2) π_1 ADA_t	(3) $(P^D_{t+1} - P^R_{t+1})$ DEM_{t+1}	(4) Elect component $\pi_1[(P^D_{t+1} - P^R_{t+1})]$ (col. (2)*(col. (3))	(5) Affect component $\pi_0[P^{*D}_{t+1} - P^{*R}_{t+1}]$ (col. (1)) − (col. (4))
1946–1958	14.2	41.7	0.41		
	(3.2)	(2.3)	(0.05)		
				17.0	−2.8
				(4.8)	(4.0)
1960–1968	23.5	49.5	0.51		
	(3.5)	(2.7)	(0.05)		
				25.2	−1.7
				(4.9)	(4.1)
1970–1978	11.5	46.6	0.40		
	(4.7)	(3.1)	(0.06)		
				18.6	−7.1
				(5.1)	(5.1)
1980–1996	46.8	56.6	0.76		
	(3.7)	(2.8)	(0.05)		
				43.0	3.8
				(4.9)	(4.5)

Standard errors are in parentheses. The unit of observation is a district-congressional session. The sample includes only observations where the Democrat vote share at time t is strictly between 48 percent and 52 percent. The estimated gap is the difference in the average of the relevant variable for observations for which the Democrat vote share at time t is strictly between 50 percent and 52 percent and observations for which the Democrat vote share at time t is strictly between 48 percent and 50 percent. Time t and $t + 1$ refer to congressional sessions. ADA_t is the adjusted ADA voting score. Higher ADA scores correspond to more liberal roll-call voting records. Sample sizes are 322 in 1946–1958; 245 in 1960–1968; 183 in 1970–1978; 164 in 1980–1996.

VI. RELATION TO PREVIOUS EMPIRICAL LITERATURE

A number of empirical studies have directly or indirectly examined the policy convergence issue.[28] Typically, the studies examine whether party affiliation matters for the observed voting records of the legislator. Most studies find evidence of this, which is strictly inconsistent with the *complete* policy convergence result. For example, Poole and Rosenthal [1984] show that senators from the same state belonging to different parties have significantly different voting records.

28. An example of early empirical work in this area is Miller and Stokes [1963]. The literature is too large to be summarized here. Other examples include, but are not limited to, Snyder and Ting [2001a], Fiorina [1999], Poole and Rosenthal [2001], Snyder and Ting [2001b], Lott and Davis [1992], Canes-Wrone, Brady, and Cogan [2002], Krehbiel [2000], Bender [1991], McArthur and Marks [1988], and McCarty, Poole, and Rosenthal [2000].

More recently, Snyder and Groseclose [2000] estimate the effect of party affiliation on roll-call votes using an identification strategy based on the assumption that on votes with 65 percent or more legislators on one side, legislators are not subject to party influence. Their main concern is that the correlation between party affiliation and voting may be spurious, possibly reflecting legislators' or constituency preferences. They conclude that in the majority of cases, party affiliation is a significant determinant of roll calls.[29]

We have argued here, however, that evidence against the (arguably unrealistic) outcome of *complete* convergence says little about the relative importance of the broader Downsian notion that electoral competition pressures politicians to moderate their positions. An appropriate assessment of this phenomenon rests on being able to empirically differentiate between *partial* convergence and complete policy *divergence*.

Other studies have noted a correlation between the electorate's and legislator's ideologies across constituencies and over time [Levitt 1996; Snyder and Ting 2001a]. This observation does *not* constitute evidence that candidates are being forced to deviate from their bliss points (i.e., partial convergence). Since bliss points are likely to vary across districts within the United States, this correlation could simply reflect that candidates' ideologies may be correlated with those of his fellow citizens in the district. These correlations suggest that the U. S. Congress may achieve a modest degree of "representation" across constituencies, in voting on legislation. Our findings suggest that whatever degree of representation that may arise, it appears not to occur through Downsian competition during a general election.

Our findings are generally consistent with existing evidence that candidates exhibit ideological rigidity. Many studies have shown that representatives do not change their positions during their careers, even when their constituency changes. For example, redistricting does not seem to make much of a difference.[30]

VII. Conclusion

The arguably central notion of the Downsian paradigm is that the pressure of electoral competition constrains candidates'

29. See Krehbiel [2000] and McCarty, Poole, and Rosenthal [2000] for a different interpretation of the Snyder and Groseclose results.

30. See Poole and Rosenthal [1997] for an overview.

policies. This paper finds that voters do not *affect* policies in this way, but rather primarily *elect* policies. The empirical analysis finds that there is little evidence that members of the U. S. House alter their positions (in either direction) in response to a large exogenous change in the probability of winning the election. This result is unlikely to be spurious or a mere statistical artifact, because it is derived using a quasi experiment embedded in the Congressional electoral system that isolates arguably exogenous variation in the probability of winning the election.

The finding that voters primarily elect policies does not necessarily constitute evidence regarding the overall "representativeness" of members of the U. S. Congress. For example, one can define "representativeness" as the degree to which a politician represents the preferences of his/her core supporters. Our analysis would say nothing about this degree of representativeness. In loose terms, we have no way of knowing whether the Democrat's (Republican's) preferred policy exactly coincides with the most-preferred policy choice of the "median" Democratic (Republican) voter. It is possible that electoral competition in the U. S. primary system can work to force each party's nominee to represent the median voter *within* each party. Instead, our analysis takes candidates' preferred policies—representative or not—as given, and asks whether electoral competition in *general elections* compels politicians to deviate from those bliss points.

A leading explanation for our findings is that the difficulty of establishing credible commitments to moderate policies is an important real-world phenomenon, and dominates any Downsian convergence effect. This complete divergence hypothesis yields a very strong prediction—that exogenous changes in the relative popularity of the candidates has *no impact* on the candidates' positions. Our quasi-experimental evidence is consistent with this bold prediction. Thus, at a minimum, there appears to be empirical support for the assumption that politicians are unable to overcome this credibility problem—an assumption that has been adopted in recent theoretical analyses of representative democracy (e.g., Besley and Coate [1997, 1998]).

Indeed, if one accepts this interpretation, it suggests the need for a more nuanced view than the Downsian perspective of how the American political system translates voters' preferences into actual policy. When voters merely "elect" instead of "affect" policies, voters do not *influence* policy choices as much as they are *presented with* choices. Investigating how citizens can affect

which candidates, and hence which policies appear "on the ballot" seems to be a useful direction for future empirical research.

Appendix 1

1. Derivation of Equation (2)

By definition, we have $RC_t = y^*(k,\lambda^*,c) + (x^*(k,\lambda^*,c) - y^*(k,\lambda^*,c))D_t$. Both x^* and y^* depend on k, the Nash bargaining point $\lambda^* \in (0,1)$ and the bliss point c. Symmetry of the P function yields a symmetric efficient frontier (around the line $y^* = c - x^*$). Consider that P_t^* can take on only two values, P_t^{*D} and P_t^{*R} (with $P_t^{*D} > P_t^{*R}$), corresponding to which party was the incumbent party in election t. Assuming symmetry of the incumbency advantage, this implies $P_t^{*D} = 1 - P_t^{*R}$. Given the above symmetries, it follows that the Nash bargains are such that $\lambda^*(P_t^{*D}) = 1 - \lambda^*(P_t^{*R})$. Symmetry then implies that $x^*(k, \lambda^*(P_t^{*D}), c) - y^*(k, \lambda^*(P_t^{*D}), c)$ will be equal to $x^*(k, \lambda^*(P_t^{*R}), c) - y^*(k, \lambda^*(P_t^{*R}), c)$, and this gap we can denote, π_1. Defining $\pi_0 \equiv [y^*(k, \lambda^*(P_t^{*D}), c) - y^*(k, \lambda^*(P_t^{*R}), c)]/[P_t^{*D} - P_t^{*R}]$, we obtain

$$RC_t = \text{constant} + \pi_0 P_t^* + \pi_1 D_t + \varepsilon_t,$$

where we have added a translational shift ε of the entire setup, to reflect heterogeneity across districts around the "average" district (the bliss points would be $(\varepsilon_t, c + \varepsilon_t)$).

2. Random Assignment from Close Elections

Consider the vote share production function for all districts:

$$VS_t = vs(x^*(P_t^*,\varepsilon_t), y^*(P_t^*,\varepsilon_t), \varepsilon_t, e_t) \tag{7}$$

$$= \widehat{vs}(P_t^*,\varepsilon_t) + \hat{e}_t, \tag{8}$$

where ε_t and P_t^* vary across districts, and ε_t represents translational shifts in the parties' bliss points. e_t is an unpredictable component of the vote share. $\widehat{vs}_t$ is the forecast of the vote share, which depends on information at the time of the election, and $\hat{e}_t$ is the forecast error.

By Bayes Rule, we have

$$\phi(P_t^*, \varepsilon_t | VS_t = v_0) = \frac{f(v_0|P_t^*,\varepsilon_t) \cdot g(P_t^*,\varepsilon_t)}{h(v_0)}, \tag{9}$$

where $\phi(\cdot \mid \cdot)$ is the joint density of P_t^*, ε_t conditional on VS_t. $f(\cdot \mid \cdot)$ is the density of VS_t conditional on P_t^* and ε_t, $g(\cdot)$ is the marginal joint density of P_t^*, ε_t and $h(\cdot)$ is the marginal density of VS_t.

If $\hat{e}_t$ has continuous density conditional on $\widehat{vs}$, then for a fixed P_t^*, ε_t, $f(v_0|P_t^*,\varepsilon_t)$ is continuous in v_0. $h(v_0)$ is also continuous in v_0, which implies that $\phi(P_t^*,\varepsilon_t|VS_t = v_0)$ is continuous in v_0. This implies that the distribution of P_t^*, ε_t is very nearly the same between $VS_t = \frac{1}{2} + \Delta$ and $VS_t = \frac{1}{2} - \Delta$, for Δ small.[31]

Appendix 2: Data

The election data used in our analysis come from ICPSR Study 6311, "Elections to the United States House of Representatives, 1898–1992," compiled by Gary King [1995]. ICPSR 6311 reports the state, district, incumbency status, and the total votes received by the Republican and Democrat candidates. ICPSR 6311 has the advantage that King distinguishes between "normal" elections that can be characterized as a Republican running against a Democrat, with no important independent candidates, and all other elections labeled as "exceptions." We drop the 304 "exceptions" between 1946 and 1994 leaving 10,138 observations—roughly 422 observations per congressional session from 1946 to 1992. The 1994 election data were all obtained from the U. S. House of Representatives' Office of the Clerk's Web site.

The Americans for Democratic Action interest group ratings were taken from Timothy J. Groseclose's Web site [2002]. As mentioned in the text, we use "adjusted" ADA scores to be able to compare ratings across sessions of congress. See Groseclose, Levitt, and Snyder [1999] for a description of the procedure used to calculate the "adjusted" ADA scores. We replicated the analysis in the paper with the "nominal" ADA scores and found no qualitative difference in our findings.

Congressional District Data for 1962–1970 were taken from ICPSR Study 10, "United States Congressional District Data Books." Congressional District Data for 1972–1980 were taken from ICPSR Study 11, "United States Congressional District Data Book for the Ninety-Third Congress." Congressional District

31. See DiNardo and Lee [2002] for a similar proof when P_{92}^* and ε have discrete support, and Lee [2003] for conditions under which an RD design can generate random assignment of treatment.

Data for 1982–1990 were taken from ICPSR Study 8710, "Census of Population and Housing, 1980 [United States]: STF3D Congressional District-level Extract." Congressional District Data for 1992 were taken from the 1990 Congressional District level extract. All districts during a given decade were assigned the most recent census value. For example, 1974 congressional districts were assigned the 1970 census values.

Keith T. Poole's [2002] Web site provided much of the data used in this paper. The non-ADA interest group ratings came from Poole and Daniels' [1985] interest group rating data which contain 59 interest group ratings scores, each covering some or all of the years between 1959 and 1981. McCarty, Poole, and Rosenthal's DW-NOMINATE scores and Poole's rank ordering data were also found at Poole's Web site. For a description of the NOMINATE procedure, see Poole and Rosenthal [1997], and for a specific description of the DW-NOMINATE scores see McCarty, Poole, and Rosenthal [1997] and for a description of the rank order procedure see Poole [1999].

James Snyder was kind enough to provide us with the Presidential Election returns by Congressional District from 1952 to 1996 for a previous draft of this paper.

Appendix 3: Additional Estimates under Unrestricted Heterogeneity

To see how it is possible to introduce heterogeneity in the difference between parties' positions across districts and legislatures, we focus, as before, on districts that experience close elections at time t and assume that the winner in close elections is random.

Imagine that after election $t + 1$, *the econometrician* can divide this set of districts into three groups. The first group (which we call the *top group*) includes districts that were going to be Democrat in time $t + 1$, irrespective of the outcome at time t. This group includes the districts that were (exogenously) assigned a Republican representative at time t but become Democrat at time $t + 1$. But it also includes districts that were (exogenously) assigned a Democratic representative at time t, remained Democrat at time $t + 1$, *and* would have been Democrat at time $t + 1$ even if they had been assigned a Republican winner at time t. In the terminology of Angrist, Imbens, and Rubin [1996], these districts are "always takers," because irre-

spective of the quasi-random assignment of DEM_t, they are Democrat at time $t + 1$.

It is crucial to remember that even though *the econometrician* can identify the districts in this particular group, we maintain the assumption that all agents in the model *cannot.* If any party knew with certainty that it was going to win in $t + 1$, irrespective of the outcome of election t, there would be little incentive to moderate positions. We emphasize that we are not introducing a new assumption at this point. The assumption that electoral outcomes are not completely certain (particularly among close elections in t) provides the basis for Alesina's theoretical framework, and the analysis throughout this paper.

In a parallel fashion, we can identify a "bottom group," which includes districts that, irrespective of the outcome at time t, become Republican at time $t + 1$. This group includes the districts that were (exogenously) assigned a Democrat representative at time t but are Republican at time $t + 1$. It also includes districts that were (exogenously) assigned a Republican representative at time t, remained Republican after time $t + 1$, and *would* have been Republican at time $t + 1$ even if they had been assigned to be Democrat at time t. These districts could be called "never takers" [Angrist, Imbens, and Rubin 1996], because irrespective of the "random assignment" of DEM_t, they are Republican at time $t + 1$. Again, we can only identify these districts ex post, *from the point of view of the econometrician.*

The remaining districts make up the third and final group, which we call the *middle group.* In the terminology of Angrist, Imbens, and Rubin [1996] these districts are the "compliers."

We can think of the parameter γ that we estimated in our main result section as a weighted average of each group specific γ:

$$\gamma = Pr_1\gamma_1 + Pr_2\gamma_2 + Pr_3\gamma_3, \tag{10}$$

where γ_1 is the γ for the top group, γ_2 is for γ for the middle group, and γ_3 is the γ for the bottom group; and Pr is the relative size of each group.

If we can identify the three groups, we can provide a test of complete divergence against partial convergence based on the size of the group specific γs. In particular, we have sharp predictions on the size of γ_1 and γ_3 depending on whether complete divergence or partial convergence is true.

Under complete divergence, we expect the ADA scores of representatives from both the top and bottom group to be unaffected by who wins the election at time t. This is because, despite the underlying change in electoral strength, candidates are unable to credibly promise anything but their extreme bliss points. So it should not matter whether they had a strong or moderate chance of winning the election in $t + 1$.

By contrast, under partial convergence, a Democrat victory in t should cause ADA scores to rise for the top and bottom groups. A Democrat victory in t causes an increase in electoral strength for the Democrats for election $t + 1$. With greater "bargaining power" the compromise would move toward the Democrats' bliss points.

Essentially, by dividing the districts into these three groups, we have eliminated the "elect component" when focusing only on the top and bottom groups. Among the top and bottom groups, the change in electoral strength will not change the composition of Democrats and Republicans in office after $t + 1$, because we have deleted the marginal districts (the middle group). Thus, both γ_1 and γ_3 are direct estimates of the "affect component" $\pi_0(P^{*D}_{t+1} - P^{*R}_{t+1})$.

How do we identify these three groups in practice? In most contexts, it would be impossible to identify these groups of districts. However, in our context, it is possible because we actually observe the Democratic vote share, which is the underlying index that perfectly determines who wins in $t + 1$.

To see how this is done, consider elections t and $t + 1$. The "vote production function" is $VS_{t+1} = vs(x^*_{t+1}(P^*_{t+1}), y^*_{t+1}(P^*_{t+1}), e_{t+1})$, where VS_{t+1} is the vote share for the Democrats. e is an unpredictable component of the vote share, which must be present if electoral outcomes are uncertain. Taking a linear approximation to this function yields (normalizing all coefficients to 1) $VS_{t+1} = P^*_{t+1} + e_{t+1}$.

Consider the districts where the Democrat defeated the incumbent Republican party in $t + 1$. These are the districts in which $VS_{t+1} = P^{*R}_{t+1} + e_{t+1} > \frac{1}{2}$. Next, consider the Democratic incumbents who succeeded in $t + 1$ who have vote share $VS_{t+1} = P^{*D}_{t+1} + e_{t+1} > \frac{1}{2}$. To make this group comparable, we must select a subset of these districts. That is, we must choose $VS_{t+1} > \frac{1}{2} + \bar{\theta}$, where $\bar{\theta} = (P^{*D}_{t+1} - P^{*R}_{t+1})$. Doing this allows us to focus

on those districts such that $P^{*R}_{t+1} + e_{t+1} > \frac{1}{2}$. Intuitively, some of the districts where the Democratic incumbents won in $t + 1$ would *not* have won if the district had been won by a Republican in t. This selection procedure eliminates these "marginal" districts.

Since D_t is essentially randomly assigned (among close elections), we have that $E[RC_{t+1}|D_0 = 1, VS_{t+1} > \frac{1}{2} + \bar{\theta}] = E[x^*_{t+1}(P^{*D}_{t+1})|P^{*R}_{t+1} + e_{t+1} > \frac{1}{2}]$ and $E[RC_{t+1}|D_0 = 0, VS_{t+1} > \frac{1}{2}] = E[x^*_{t+1}(P^{*R}_{t+1})|P^{*R}_{t+1} + e_{t+1} > \frac{1}{2}]$. Therefore, the difference in these quantities is $E[x^*_{t+1}(P^{*D}_{t+1}) - x^*_{t+1}(P^{*R}_{t+1})|D_t = 0, D_{t+1} = 1]$. This is the response of Democrats' policy choice to differing probabilities of winning $t + 1$, P^{*D}_{t+1} and P^{*R}_{t+1}.

How do we practically choose this higher cutoff $\bar{\theta}$? We observe the proportion of the $D_t = 0$ group of districts that have $P^{*R}_{t+1} + e_{t+1} > \frac{1}{2}$. This proportion is $\Lambda \equiv \Pr[D_{t+1} = 1|D_t = 0]$. Now consider the $D_t = 1$ group of districts. If D_t is randomly assigned, then we must choose $\bar{\theta}$ so that $Pr[VS_{t+1} > \frac{1}{2} + \bar{\theta}|D_t = 1] = \Lambda$. This can be done since VS_{t+1} is perfectly observable.

Empirical Results with Heterogeneity. We now describe how to empirically implement the theoretical framework described above. Our empirical strategy is similar to the one used to obtain our main results in subsection V.A. As with our basic results, we include in our analysis only districts that experienced close elections at time t. In these districts we assume again that the winner party at time t is virtually random. The main difference with our previous empirical strategy is that we now allow for a more unrestricted form of heterogeneity across districts and divide our sample of close elections in three groups.

We begin by showing in Panel A in Table V that in the sample of close elections, among districts that were won by Democrat in period t, 72.6 percent remain Democrat in $t + 1$, and 27.4 percent are won by Republicans. Among districts that were won by Republican in period t, 75.9 percent remain Republican in $t + 1$, and 24.1 percent are won by Democrat.

If challengers, who are electorally weaker, are responding to electoral pressures by moderating their positions, we should see that a successful Democrat challenger's ADA score is less liberal than its comparison group, and that a successful Republican challengers' ADA score is more liberal than its comparison group.

Take, for example, the top left corner of panel A. It represents

TABLE V
ADDITIONAL ESTIMATES USING ADA SCORES—CLOSE ELECTIONS SAMPLE

Panel A	Districts, where $DEM_t = 0$ (1)	Districts, where $DEM_t = 1$ (2)	(3)	(4)
Percent $DEM_{t+1} = 1$	24.1	72.6		
Percent $DEM_{t+1} = 0$	75.9	27.4		
Total	100	100		

Panel B	Average ADA_{t+1} in districts, where $DEM_t = 0$ (1)	Average ADA_{t+1} in districts, where $DEM_t = 1$ (2)	Difference in average ADA_{t+1} (col. (2)) − (col. (1)) (3)	Obs. (4)
Districts in top group	67.6 (3.0)	65.9 (2.9)	−1.7 (3.0)	224
Districts in middle group	18.6 (1.7)	66.1 (1.8)	47.4 (1.8)	441
Districts in bottom group	21.1 (2.2)	16.5 (2.1)	−4.6 (2.3)	250

Standard errors are in parentheses. The unit of observation is a district-congressional session. The sample includes only observations where the Democrat vote share at time t is strictly between 48 percent and 52 percent. Column (1) refers to observations for which the Democrat vote share at time t is strictly between 48 percent and 50 percent. Column (2) refers to observations for which the Democrat vote share at time t is strictly between 50 percent and 52 percent. Time t and $t + 1$ refer to congressional sessions. Higher ADA scores correspond to more liberal roll-call voting records.

Democrats who were able to win the district even though it was held by a Republican. We would like to compare the voting score of those districts with the voting score of *otherwise comparable districts* where the Democrat party is the incumbent party, and the candidate from the Democrat party was elected at time t.

The empirical challenge is to identify the correct counterfactual. The methodology described above provides a solution and leads us to split the districts into three groups, as shown in Panel B. We empirically identify the three groups as follows. As before, we consider only the 915 observations that experience close elections at time t. We include in the top group of districts where a Republican (barely) won at time t but a Democrat regained the district at time $t + 1$ as well as the districts with the largest Democrat vote shares at time $t + 1$, among the districts where a Democrat won at time t. Specifically, we included the top 24.1

percent districts with the largest Democrat vote share among the districts where a Democrat won at time t. There are 224 such districts.

Similarly, we include in the bottom group districts where a Democrat (barely) won at time t but a Republican regained the district at time $t + 1$, as well as districts with the largest Republican vote shares at time $t + 1$, among the districts where a Republican won at time t. Specifically, we included the 27.4 percent districts with the largest Republican vote share, since among all the districts where the Republican lost at time t, the Republican candidate won at time $t + 1$ in 27.4 percent of the cases. There are 250 such districts. Finally, we include in the middle group the remaining districts. There are 441 such districts.

Because of the way we constructed the three groups, none of the top group districts in column (1) has a representative in period $t + 1$ who is the same representative in period t. On the contrary, 91 percent of the top group districts in column (2) has a representative in period $t + 1$ who is the same representative in period t. The corresponding figures for the bottom group are, respectively, 0 and 96 percent. The corresponding figures for the middle group are 90 percent and 89 percent.

Panel B shows the average voting scores, for each of the three groups, by the winning party at time t. In column (1) we report the average ADA at time $t + 1$ for districts that were Republican at time t for each of the three groups. In column (2) we report the average ADA score at time $t + 1$ for districts that were Democrat at time t. In column (3) we report the difference.

Consider the top and the bottom group. Consistent with the hypothesis of complete divergence, there is virtually no difference in the voting scores of the two sets of districts. Consider the middle group. As expected, there is a large difference in the voting scores of the two sets of districts.

Overall, the results in Table V lead us to conclude that our earlier results are robust to a more general framework that allows for unrestricted heterogeneity in the *differences* in policy positions across districts and over time. Finally, we have estimated similar models using alternative measures of roll-call behavior (DW-Nominate scores and percentage of vote consistent with Democrat leadership). In results available

from the authors, we find similar results: virtually no response of voting records to changes in electoral strength.

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