

Seeking the Constituent Signal: Exit Poll Measures of Public Opinion and Dynamic Congressional Responsiveness

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Do American political institutions comport with accepted norms of democratic representation? The three arguably most important American political scientists of the 20th century—Harold Lasswell, V.O. Key, and Robert Dahl—each held that if policymaking does not generally respond to the policy attitudes of constituents, then democracy in America is a fiction. The institutional design of the House of Representatives, which emphasizes frequent elections and small constituencies, makes it a likely locus of responsiveness in our political system. However, the vast literature investigating House responsiveness has failed to provide unambiguous answers about the phenomenon's nature and extent. One reason for this is a data problem: measuring the constituency half of the constituent-representative dyad is difficult in the absence of large-*n*, geographically comprehensive social surveys of House district attitudes. Scholars have relied on survey disaggregation and proxy variables as substitutes, but these respectively proved unreliable and invalid. Multilevel regression and post-stratification (MRP), which calculates the attitudinal propensities of demographic groups in national surveys and adjusts these propensities by their demographic composition per jurisdiction, is a new and promising method for imputing district-level attitudes. A new generation of MRP responsiveness literature claims to, at last, precisely measure the attitudinal signals constituents are sending to legislators. I nonetheless take issue with three features of conventional MRP measures: they are cross-sectional (and assume public opinion is stable); they are derived from the general population (and not voters); and they are issue-focused (which poses problems when the issue agenda is unstable). In response, I generate longitudinal MRP measures of ideology using state and national exit polls from 2004 to 2010. Exit polls have

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larger biennial *ns* than widely-used social surveys like the Cooperative Congressional Election Study, and sample only voters. Employing my measures, I find evidence of heterogeneous ideological change and growing polarization in the districts. I also find that candidates and legislators do not respond to the preferences of median voters. Instead, and in violation of conventional democratic theory, they respond primarily to their party subconstituencies, particularly when district attitudes are widely dispersed. I offer directional voting as a formal explanation for this behavior.

Seeking the Constituent Signal: Exit Poll Measures of Public Opinion and Dynamic
Congressional Responsiveness

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Chapter 1. The Search for Representation

The Puzzle of Responsiveness

Representation is now a core concept to both democratic theorists and election researchers. Whereas the Western philosophical canon (for instance, Mill 1861) and some modern participatory democrats (Barber 1984) have held that representation works at cross-purposes with direct popular sovereignty, representation is now understood as an election-driven mechanism by which citizens govern themselves (Pitkin 1967; Dahl 1989). Even as no reasonable observer in the complex world of 21st century politics expects a democratic government to always and purely express the “general will,” growing interest in representation has tracked higher democratic expectations. Our accepted political norms now demand there be at least some mass-elite connection in American politics. To Harold Laswell (1941, 15), the “open interplay of opinion and policy is the distinguishing mark of popular rule.”

How to look for links between constituencies and representatives is not immediately obvious, because representation is definable in many ways. Hanna Pitkin (1967, 8) once observed that representation is not a “single, [albeit] highly complex concept,” but rather a set of distinct ideas that frequently receive the same classification. Nonetheless, the demands of testability and simplicity have led Political Scientists to converge on a single conceptualization of representation. This conceptualization is “responsiveness,” or the degree to which the behavior of legislators corresponds to the attitudes of their constituents. Responsiveness is the natural expectation in any political system that features free and fair elections, as voters are likely to elect representatives who share their political traits. In addition, the nature of the promissory voting contract, whereby constituents vote for a representative in exchange for a promise to obey their expressed desires, is likely to produce responsiveness (Mansbridge 2003). Constituents can sanction representatives for

shirking from the voting contract by withdrawing their support in the next election. Representatives, who prefer continuing their careers in office, will act in accordance with constituent demands even against their own will, and despite the fact that few democracies contain mechanisms to legally coerce elected officials to act responsively (Manin, Przeworski, and Stokes 1999). When public attitudes change, we therefore expect policy change to follow.

Formal models of electoral competition—that is, abstract, mathematical proofs drawn from microeconomics and that explain political phenomena—build on this familiar and intuitive set of predictions. The best known, the Median Voter Theorem, posits a Nash equilibrium that requires competing parties or candidates to present identical platforms that coincide with the constituent median (Downs 1957a; Black 1958). While this result requires conditions that are unlikely to obtain in reality—that is, an electorate where voters have only single-peaked, unidimensional issue preferences and never vote strategically—, it does explain why moderation does confer advantages to candidates (Gelman and Katz 2007), and why electoral competition seems to promote responsiveness (Griffin 2006). The median voter theorem is the accepted explanatory framework in many historical studies of responsiveness. If American institutions are representative, responsiveness scholarship goes, then the median voter of national, state, and House constituencies should exercise an enormous pull over legislation.

Responsiveness is thus the standard metric for thinking about the representativeness of American political institutions. Its assumptions are grounded both in common sense and abstract mathematical expressions. It requires no strong normative assumptions about what constitutes good policy, privileges all constituent preferences equally, and comports with most people’s pre-theoretical notions of legitimacy. As Robert Dahl (1971, 1) put it, “a key characteristic of

democracy is the continued responsiveness of the government to the preferences of its citizens, considered as equals.”

Political Science’s well-articulated expectation that responsiveness obtains in the United States has unfortunately led to increasing disappointment. Elections, it seems, are not enough to guarantee representation. A large body of comparative political research suggests that elected officials in democratic systems still enjoy broad independence from public opinion (Skocpol 1995). A possible explanation is that responsiveness is possible only if citizens possess the efficacy and sophistication to form political attitudes, as well as monitor representatives to detect and sanction shirking. It is questionable that they do. In the 1920s, Walter Lippmann (1922, 273) ridiculed traditional democratic theory as requiring an omniscient citizen who was “fitted to deal with all public affairs” and “consistently public-spirited and endowed with unflagging interest.” It is unlikely that voters have become substantially more sophisticated since then, even by rubrics more modest than “omniscience.” (Delli Carpini and Keeter 1997). Voters seem far removed from the rational utility-maximizers of formal models of representation. Indeed, the leitmotif of political behavior scholarship is that raw, emotive group identifications—not policy preferences or worldviews—drive voter decision-making (Achen and Bartels 2016); how is a congruence between representative and constituent views possible when the latter are, in the main, vague and inchoate? The strong expectation for responsiveness also runs up against the growing inequalities of power and resources in the United States and other democratic countries (Gilens 2005; Bartels 2008). Furthermore, an emerging body of subconstituency politics literature holds that constituent preferences carry unequal weight, as when groups actuated by strong identity beliefs or possessing unique access to power override the will of constituencies (Bishin 2009). In sum, the “electoral connection” that is the supposed guarantor of democracy appears broken to

many observers, who hold that “legislators are largely free from popular control and are influenced instead by interest group wishes, party loyalties, peer pressures, or their own judgments” (Page et al. 1984, 741).

One would hope that the mountain of responsiveness research that has arisen since the early 1960s would be able to address the critics of the conventional democratic model. After all, responsiveness is a popular conceptualization of representation because it is seemingly easy to test. To do so, it is enough to see whether the measured policy preferences of representative-constituency pairs, or “dyads,” covary when placed on the same scale. If an electorate grows more conservative, then so too should the voting preferences of its representative. There is no need for researchers to ask whether representatives are voting *well*, or even if enacted legislation is consistent with constituent preferences. (If [say] a Senator voted totally in accordance with her district’s desires, she may be outvoted by 99 other legislators and fail to deliver policy congruent with her representatives’ opinion).

In practice, measuring constituent opinion—the first step in a model of responsiveness—is a fraught exercise. To be sure, there is no shortage of public opinion polls, whose original *raison d’être* was to help the government assess and respond to constituent demand (Gallup 1938). There is plenty of academic and journalistic analysis of whether the presidency, the Senate, and even the Supreme Court react to the country’s changing policy mood that interminable commercial polls capture (Stimson 1999). A problem with standard survey instruments emerges when we interrogate the question of responsiveness more closely. For one thing, polls on most issues are quite nonspecific when it comes to gauging just what policies constituents want (Wlezien 1996). For another, there may be no single national policy mood (Best 1999), whereas the coverage frame of most polls gauging policy attitudes is national. When a President claims to represent hundreds of

millions of people in conditions of ineluctable pluralism, in what way can one meaningfully speak of constituency responsiveness? The same is likely true of many Senate and gubernatorial constituencies.

To look for policy responsiveness to constituencies of millions, tens of millions, or hundreds of millions of people is to look for it in the wrong place. If there is any evidence that national politicians respond to public opinion, we are likely to find it in the House of Representatives. House constituencies are smaller (with just over 700,000 residents, on average), more homogenous, and express their preferences more frequently than constituencies of any other federal office. The Framers thus assumed that the House would be the most direct organ of sovereignty, and that a House representative would more than any other elected official exercise accountability. *Federalist 52* predicts that Congress would have an “immediate dependence on, and an intimate sympathy with, the people”—or, in more social scientific vocabulary, be responsive to district attitudes. House responsiveness scholarship is therefore the best positioned to answer whether or not American political institutions comport with democratic expectations.

Unfortunately, House responsiveness scholarship has faced a historic Catch-22. The dispositions of Congressional districts, while more likely than those of states or the country at large to engender responsiveness, are also many times more difficult to measure. Political Science has yet to find a satisfactory approach to getting around this problem. As a result, responsiveness literature presents the scholar with a tangle of contradictory findings. In the heyday of the “textbook” Congress, where party polarization was relatively low, many studies found strong linkages between constituent attitudes and representative behavior (Miller and Stokes 1963). Congress scholarship in this era treated representatives as masters of appealing to district constituencies; it was supposed that they responded to public opinion even when so doing went

against partisan considerations (Fenno 1978), the institutional relevance of Congress (Fiorina 1989), and the goal of good policy (Mayhew 1974a). Some (Canes-Wrone et al. 2002; Carson et al. 2010) still agree with this view, and present evidence that constituents sanction representatives in the manner that dyadic representation supposes. Others hold that this is an illusion perpetuated by selective biases in candidate emergence (Burden 2007), and that (say) party considerations explain most of the variation in legislative activity. Whether dyadic representation is declining in an era of party polarization has been suggested (Jacobs and Shapiro 2000; Ansolabehere et al. 2001), but remains an open area of debate.

Signal Versus Noise

Dyadic representation obtains when supply (legislative behavior) meets demand (the changing attitudes of constituents). On the supply side, valid and reliable measures of legislative behavior exist. While legislative ideology is not directly observable, the fact that roll call votes are a matter of public record leaves open many avenues of estimation. Scholars have historically used scores that interest groups assign to various legislators. In more recent years, Poole and Rosenthal's (1997) DW-NOMINATE scores, generated by scaling most roll call votes taken since the 1st Congress, have become standard. The overwhelming consensus is that House representatives vote on a single spatial dimension, which is interpretable as member partisanship or ideology and which readily lends itself to studies of responsiveness.

On the demand side, Political Science has struggled to assign ideal points to House districts. Our basic notions of representation tell us that House representatives read constituent signals and vote accordingly. No survey instrument, however, adequately captures the aggregate attitudes of all Congressional districts, nor is one likely to ever exist. In the absence of good data,

how does one discern the constituent signal, and distinguish it from the noise of measurement error? For over half a century, Political Science has proposed a number of ways around the problem. Initially, the tack was disaggregating the low- N social science surveys that University of Michigan researchers began conducting in the late 1940s (Miller and Stokes 1963). Unfortunately, this approach failed to produce estimates for most Congressional districts, and also was plagued by obvious measurement error (Achen 1978). Later research shifted from the direct measurement of individual attitudes to district-level demographic proxies (for instance, race and rates of college education) that are available in the decennial Census, as well as presidential vote by district (LeoGrande and Jeydel 1997). Each imaginable proxy, however, proved to be a poor substitute for the constituent signal. Fenno (1978, xiii) once asked, “what does an elected representative see when he or she sees a constituency?” It is unlikely that a representative, when factoring public opinion into a voting decision, imagines her constituents as “an undifferentiated mass that is 40% college educated.” The inadequacy of any particular proxy led to later approaches that simulated district-level ideology using *many* proxies (Erikson 1978; Ardoin and Garand 2003). This still left research at an impasse, however. In a textbook case of the ecological fallacy, researchers selected proxies on the basis of their ability to predict ideology at the individual level, without evidence that they predicted the ideologies of whole constituencies.

Recently, a new statistical technology has emerged that promises to break the methodological impasse: MRP, or multi-level regression, imputation and post-stratification (Park, Gelman, and Bafumi 2004; 2006). MRP takes large- n national surveys, models the attitudinal propensities of demographic groups, and then weights these propensities by their demographic composition per district. In this manner, it incorporates both the individual-level approach of early behavioralists and the jurisdictional-level approach of later scholarship. While unassailable as a

disaggregation method, it is only as good as the data it operates on. When combined with the new, yearly Cooperative Congressional Election Study (CCES), which has polled online respondents in every district since 2006, MRP seemingly solves the missing data problem (Warshaw and Rodden 2012). A review of the responsiveness studies the method begat would tell us that MRP produces valid, reliable estimates of constituent preferences, correlating highly with existing district polls and corresponding electoral preferences.

In fact, MRP's promise to find the constituent signal remains unfulfilled. MRP requires large-*n* surveys to generate good estimates for every district. While the CCES is large enough to estimate each district's attitudes cross-sectionally, it is not so large that it can generate yearly estimates. Recall, however, that testing dyadic representation means testing whether changes in constituent attitudes produce changes in legislative action. Static measures are wholly unsuited to the task, particularly in the face of suggestive evidence that aggregate ideology evolves sub-nationally. Another problem is that the national polls query the entire general population, whereas it is *voters* who authorize representative control and hold them accountable for shirking from their preferences, and much available evidence suggests that these voters differ systematically from nonvoters. Including a "voted" item does not solve the problem, since rates of self-reported voting far outstrip actual voter turnout. In the CCES, it does so by as much as 30% (Ansolabehere and Hersh 2012).

The measurement problems call into question the veracity of most studies of House responsiveness undertaken since the early 1960s. A new approach is needed to generate valid estimates of district ideology that vary from year to year, and which capture the attitudes of voters.

Exit Polls: A New Place to Look

My dissertation proposes that exit polls are a promising solution to the problem of the constituent signal. Exit polls have existed since the 1940s, but became a well-financed and widely-reported phenomenon in the 1970s. Warren Mitofsky, head of the elections unit at CBS News, applied probabilistic sampling to select precincts across the country and sent pollsters to interview voters as they left the voting booth. Thus, CBS could generalize about the entire presidential electorate and make forecasts well before the official results. In the 1980s, CBS expanded its exit poll operation and began polling voters in states with interesting electoral contests. Only recently have scholars used historical exit poll data to study longitudinal trends with the intention of *explaining* rather than predicting election results (Best and Krueger 2012). No one has yet made systematic, social-scientific use of the state-level data.

Exit polls enter the discussion because they are a hitherto untapped resource and transcend the main limitations of CCES-derived MRP measures. Exit polls contain enough cases to produce longitudinal measures, rather than static measures that are the result of collapsing several years. Exit polls' vast biennial sample sizes allow for estimates that are sensitive to changes in public opinion and to the unique dynamics of election cycles. This is an important advantage, given strong intuitive and empirical reasons to suspect that public opinion is unstable. Another strength of exit polls there is no issue of teasing out voters from the survey sample: in exit polls, all the respondents *are* voters. If we adopt the sensible assumption that rational representatives will respond to political participants who can in principle remove them from office, we cannot underestimate this advantage. While past scholarship has tested whether constituents with "voting power" enjoy greater policy representation (Bartels 1998), no one to date has produced and made use of direct measurements of House district electorates. I hope to be the first to do so.

Lastly, most MRP studies have looked at responsiveness through the lens of individual issue positions, in the face of evidence that representation is a “global” phenomenon requiring constituent measures across the general contours of policy (Stimson, MacKuen, and Erikson 1995). In this case, the issue-specific properties of constituent attitudes are “noise,” and do not signal meaningful information to legislators. Departing from the corpus of MRP studies, I plan to use MRP to disaggregate the exit polls and produce biennial, district-level measures of symbolic ideology: that is, the extent to which districts identify as liberal, moderate, or conservative. I argue that symbolic ideology in this context furnishes superior measure of district attitudes because it embraces all issues on the Congressional agenda and remains stable over many years, whereas the lifespan of most issues is short.

Exit polls, which have unique properties suited to gauging the signals that constituents transmit to legislators, will enable me to unveil previously unknown insights about the behavior of district electorates. In the following chapters, I will describe the creation of my MRP exit poll measures, establish their validity and reliability, and detail how they overcome the shortcoming of previous survey-based opinion measures. I will then use them to study public opinion change and longitudinally explain House roll call voting in recent Congressional sessions. With the evidence in place, I will at last be able to precisely assess the extent to which the House of Representatives is beholden to public control, and address supporters and critics of the traditional model of popular representation in the United States.

Organization of This Manuscript

My task is to find the constituent signal in exit polls, and use this measure to take a fresh look at responsiveness that is unencumbered by old limitations. Towards that end, I divide my analysis into four main sections.

In Chapter 2, I flesh out the approaches to identifying the legislative signal in the context of dyadic representation. As noted above, these approaches include direct measurement, the use of indirect proxies, bottom-up simulations, top-down simulations, raw survey disaggregation, and finally MRP. I will discuss the body of research associated with each approach, keeping in mind the question of why social scientists—smart people fully cognizant of their limitations—nonetheless chose them. I will discuss the problems with each, and why they were ultimately modified or supplanted.

Chapter 3 will propose my alternative measures: district-level symbolic ideology imputed from the National Election Pool exit polls in the previous decade, and post-stratified using the Census Bureau's new annual American Community Survey. As with any methodological undertaking, generating these measures required a number of discretionary choices, each of which I will do my best to explain and defend. I find that my measures are reliable and exhibit cross-validity with district-level measures of ideology and issue attitudes derived from the CCES, as well as with presidential and House election results.

Chapter 4 proceeds from the insight that, in a country where up to 20% of households move annually, demographic change alone will likely cause district-level ideology to change from election to election. I select a test for identifying districts that, independently of national political trends, undergo aggregate attitudinal change between the 109th and 112th Congresses. Given that this time-span featured three notable wave elections (2006, 2008, and 2010), which one would expect to pull district electorates in strong national directions, this is a strong test. I find that, even in this short period, many districts exhibit monotonic change, and that the changes are bimodally clustered in the most ideologically extreme districts—in other words, districts are polarizing. I find

that changes in demographic, economic, and crime indicators at the district-level significantly explain aggregate ideological change.

Finally, Chapter 5 comprehensively tests House responsiveness using my measures. Within the accepted framework of spatial theories of voting, particularly the Median Voter Theorem, I operationalize the median district voter as the ideology measures derived in Chapter 3. I test whether both candidates and legislators change their positions in accordance with changes in median voter ideology. I also test whether legislators respond to partisan subconstituencies, which is the expectation of the Median Voter Theorem's cousin, the Directional Voting Model. Due to the overwhelming influence of partisanship on legislative behavior in contemporary American politics, I bifurcate my analysis by party. I find evidence that candidates and representatives do not respond to their geographic constituencies, and are instead sensitive to the attitudes of their party supporters. This is a confirmation of the party responsiveness hypothesis, which has a long history in Congress scholarship but has not received a systematic empirical treatment over multiple Congresses. I conclude by discussing partisan representations' troubling implications for normative theories of democratic representation.

Accounts of democratic legitimacy demand that public opinion has a role in the formation of policy. It must have this role not only on particular issues or votes, but generally. Good exit poll data, harnessed with the statistical power of MRP, will enable Political Science to determine House of Representatives is truly "the people's House," or an out-of-touch institution separated from voters by a wide democratic deficit.

Chapter 2. Measuring District-Level Ideology

The Problem

If the representation of public opinion is an important feature of any democracy (Key 1961), and if the House of Representatives is supposed to be the most representative branch of American government, then the development of valid and reliable measures of district attitudes should be a priority of Political Science. Ideally, a test of legislative responsiveness would have at its disposal a national survey that frequently interviews large- n samples in all 435 House districts (Aroin and Garand 2003). No such survey exists, nor—but in the unlikely event that House incumbents authorize the National Science Foundation funding to study their own performance—will it ever exist. Congress scholarship has therefore relied on small- N district samples that are disaggregated from national surveys, as well as other indirect stand-ins for the true and unobserved value of district-level opinion. Does this tack necessarily call the scholarship's veracity into question? The answer is that it likely does. Achen (1978) notes that such measures possess high rates of error, which harms econometric models of legislative responsiveness. Because measurement error in an individual predictor biases its regression coefficient to zero, a regression of legislative behavior on an imperfect measure of district-level ideology is likely to result in a finding of non-responsiveness. Significantly, a large portion of historical responsiveness research since the early 1970s consists of null findings (see, for instance, Bernstein 1989).

This chapter will review most of the ways in which scholars operationalized district-level ideology. I discuss, first, the approach of the early behavioralists who were the first to gauge the effects of constituency pressure. They derived measures of district policy attitudes from a University of Michigan study of the 1958 midterm election. Second, in light of the problems with these and similar measures, I catalogue the proxy measures (demographics, vote shares,

simulations, and others) that researchers have offered as alternatives. Proxy measures unfortunately diverge, both conceptually and empirically, too greatly from ideology to serve as adequate substitutes. Third, I discuss simulation approaches that impute district ideologies using many proxies. While constituting an advance, these suffer from many of the same problems as disaggregation and proxy methods they seek to refine. Finally, I discuss multilevel regression imputation and post-stratification (MRP), the state-of-the-art methodology for estimating sub-national public opinion. This recent scholarship applies a Bayesian approach to individual survey responses, calculates the attitudinal propensities of demographic groups, and then weights these propensities by their demographic composition per district. While accepted as valid and reliable, even MRP-derived estimates of district-level ideology, I argue, import contentious assumptions about public opinion.

Survey Disaggregation

Miller and Stokes (1963) founded the modern House responsiveness literature, and were the first to develop an empirical test of dyadic representation. They generated district-level measures of public opinion on social welfare, foreign affairs, and civil rights with survey disaggregation. Disaggregation entails taking one or several large-*n* national surveys and breaking down the sample to sub-national geographic levels, for instance states or House districts. The only data available to Miller and Stokes was the 1958 American National Election Study (ANES). This survey was the second wave of a four-year panel study that tried to identify the long-term determinants of voting behavior. Unfortunately, case attrition and budget constraints reduced this study's sample size to only 1,450, making the ANES a poor candidate for survey disaggregation. As a result, Miller and Stokes tried to correlate measures of representative behavior—based on

scores of roll call votes, as well as private interviews with legislators—with district aggregations of ANES responses with an average sample size of only 11. Moreover, Miller and Stokes could only measure attitudes in 116 districts. Erikson (1978) and Achen (1978) criticized Miller and Stokes' methodology on account of its obvious proneness to sampling error. The former found that error caused Miller and Stokes to overestimate the covariance of constituent and representative opinion, while the latter found that they *underestimated* it.

Is the problem with Miller and Stokes' groundbreaking work, however, merely a data problem? More modern studies of sub-national public opinion have disaggregated commercial and social-scientific polls with *ns* many times the sample size of the 1958 ANES. Erikson, Wright, and McIver (1993) pioneered the creation of measures of state-level ideology by merging and disaggregating over 100 CBS/New York Times surveys. At the district level, Clinton (2006) revived the Miller and Stokes approach by disaggregating the 1999/2000 National Annenberg Election Study (NAES), which includes *ten times* the number of cases as the 1958 ANES. The NAES also uses a random digit dialing sampling method as opposed to the cluster sampling of the ANES, and therefore has greater geographic coverage and representativeness than the latter. (For a discussion of the perils of cluster sampling for survey disaggregation, see Stollwerk 2012).

Increasing the sample size by even 1000% may not be enough, however. Erikson (1978) finds that even the largest national surveys contain too few cases to provide valid, reliable measures of district-level opinion. Clinton's paper reports that the average district sample size is only 41. To get around the *n* problem, scholars who employ disaggregation typically concatenate surveys administered over many years, which is problematic if aggregate public opinion is unstable. Erikson, Wright, and McIver—in *Statehouse Democracy* and follow-up studies—vigorously argue

that subnational public opinion has been glacially stable since World War II. This interpretation is questionable, a point to which I will return later.

Survey disaggregation also has limitations that the mere addition of cases does not fully resolve. Because disaggregation operates on surveys with a national coverage frame—that is, sampled and weighted to project the results to a national population—the disaggregated samples are unweighted by the population characteristics of the desired geographic area. This is particularly problematic for small states, where surveys have only a few PSUs (primary sampling units) that are disproportionately concentrated in population centers. The raw tabulation of responses are unlikely to be representative of the state populations and are thus rife with sampling error (Berk 1983; Hill and Hurley 1984). One can only imagine that the disaggregated estimates for House districts, with population sizes comparable to the least populated states, are even more biased. Adding to the bias is the fact that many surveys, including the NAES, oversample minorities. Once survey weighting is removed, it becomes impossible to interpret the results as random samples of national, state, or district populations.

Alternatives to Direct Measurement

In the years between Miller and Stokes' seminal study and Clinton's revival of survey disaggregation, most studies of House responsiveness relied on proxies for district ideology. The proxies are variables that do not gauge the actual disposition of House constituencies, but that bear a close enough relationship to district ideology to serve as surrogates. I will here analyze three sets of proxies that have appeared in responsiveness literature: demographics, presidential returns, and ballot initiative returns.

First, many scholars have used decennial Census figures for this purpose. These include district demographics like education, income, age, social class, occupational distribution, urbanization, and rates of home ownership (Pool, Abelson, and Popkin 1965; Sinclair-Deckard 1976; Weber and Shaffer 1972). If there is a systematic relationship between constituent demographics and policy preferences, this line of research reasons, then representatives will use demographics as approximations of said preferences in their voting behavior. If this is the case, then the influence of demographics on roll call voting is a “good enough” indicator of legislative responsiveness.

Are the assumptions of the demographic proxy line of scholarship true? Auguste Comte famously said that “demographics is destiny,” but this is sadly not the case in politics. Demographics empirically have weak, nonlinear, or uncertain correlations with district ideology (Bishin 2000). Conceptually, it is difficult to imagine how the Census could reliably signal to legislators how to vote on most issues:

Knowing, for instance, that a district has a high proportion of its citizens with a college education does not necessarily give a legislator clear, unambiguous signals about the policy preferences of constituents, since this demographic characteristic, like others, is not perfectly related to policy preferences (Ardoin and Garand 2003, 1169).

Still, even if a demographic could unambiguously signal constituent opinion to legislators, how would we know? There is no obviously best metric by which to select demographic predictors in a responsiveness model. Because the relationship between aggregate demographics and aggregate public opinion is unobservable for House districts, researchers choose those variables that predict ideology at the individual level. They thereby commit the ecological fallacy, as they cannot assume that individual-level predictors are reliable aggregate-level predictors.

Second, district-wide presidential returns have served as a proxy for district-level ideology (Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Cogan, and Brady 2002). This makes

sense, since presidential vote is an observed political variable. It is also frequently updated, such that it permits longitudinal analysis (Kernell 2009). This proxy is nonetheless problematic because campaign-specific factors govern the outcomes of presidential elections (Levundusky, Pope, and Jackman 2008), and the ideological intensity of some elections is stronger than others (for instance, the 1976 presidential election was comparatively unideological). Even more crucially, ideology is not the only structural determinant of voting: the leitmotif of political behavior ever since *The American Voter* (Campbell et al. 1960) is that partisanship and other deep-seated group affiliations explain vote choice better than issue attitudes. In light of this, it is hard to agree with the strong assumptions required to interpret presidential vote as a proxy for district attitudes, namely that “issue positions espoused by the two parties’ presidential candidates are perceived similarly by everyone and that all voters vote according to the spatial model” (Ansolabehere, Snyder, and Stewart 2001, 40). Attempts to refine the presidential vote proxy by removing short-term forces, moreover, are problematic. The resulting measures of district ideology can only be ranked ordinally (not cardinally), and even then only on the assumption of equal variance across districts (Kernell 2009).

Finally, a third proxy of constituent ideology has been referenda votes (McCrone and Kuklinski 1979). The advantage of ballot initiative returns as opposed to presidential returns is that they are clearly interpretable as indicators of public opinion. I might vote for a presidential candidate on the basis of non-attitudinal characteristics like looks and trustworthiness, but I am unlikely to vote for a referendum because its font on the ballot was aesthetically pleasing. Gerber and Lewis (2004) made excellent use of this proxy when they disaggregated election data in Los Angeles County, whose voters they subdivided into State Senate, state Assembly, and House districts. Voting on 1992 ballot initiatives served as proxies for district ideologies, to which they

found representatives responsive only in homogenous districts. Their data and measures, however, are limited to states and years with ballot initiatives that suitably track ideology. They foreclose any inquiry that looks at responsiveness nationally, and over multiple elections.

Simulations

Dissatisfaction with both survey disaggregation and proxies led researchers to see if they might have better luck by combining both approaches. Recall that survey disaggregation builds measures using individual survey responses, whereas proxies are aggregate measures. Would it not make sense to create a measure of district ideology that incorporates individual- *and* aggregate-level data? Erikson (1978) thought so, and used this insight to simulate district attitudes. First, using the ANES panel study, he regressed individual ideology on individual demographic characteristics. Then, he multiplied the unstandardized regression coefficients by district demographics. Erikson's measures are more strongly related to representative behavior than are Miller and Stokes' raw tabulations, thus establishing good face validity. This innovative approach nonetheless suffers from the limitations of previous approaches. Owing to their exclusive reliance on demographic variables, the simulated models' fits (R^2) are unfortunately low. Therefore, like Miller and Stokes' raw measures taken from the same dataset, simulated measures are prone to measurement error.

Ardoin and Garand (2003) refer to Erikson's simulation as "bottom-up," since it begins at the individual level and proceeds to higher levels of aggregation. Their alternative is a "top-down" simulation, whereby they first model predictions for state-level ideology (as operationalized by Erickson, Wright and McIver [1993]), and then substitute the aggregate district-level predictors for the aggregate state-level predictors. Estimating aggregate variables using a model from an even

higher level of aggregation seems counter-intuitive. It makes sense, however, since aggregate-level regressions tend to explain more variation than individual-level ones.

I nonetheless identify four major problems with top-down simulations. First, the state-level attitude measures used in the first aggregate-level regression must themselves be valid. As I discussed previously, Erikson, Wright, and McIver's measures are simply the unweighted state means of responses in national commercial surveys, and their measures for small and heterogeneous states are likely to suffer from bias. Second, given that Erikson, Wright, and McIver's numbers are collapsed over many years (for instance, one set of estimations for the 1980s, and another for the 1990s), the top-down model problematically assumes that ideology at the state level is constant, as are the effects of demographic variables on state-level ideology. Third, aggregate variables might have different effects at different levels of analysis (Gelman 2009). As the authors themselves acknowledge, a majority black district will be more liberal than a majority white district, but states with larger black populations tend to be more conservative. For this reason, Ardoin and Garand discard race from their simulation, despite the fact that racial composition is a strong covariate with district political ideology (Levendusky, Pope, and Jackman 2008). Finally, fourth, both top-down and bottom-up simulations produce unknown errors (Clinton 2006).

I have documented why methods that people have historically employed to estimate district-level public opinion are unworkable. Measures derived from survey disaggregation suffer from catastrophic sampling error. Demographic proxies fail to explain the majority of variance in political attitudes, save for rare niche issues that unusually correlate with a demographic or set of demographics (see, for instance, Bailey 2001). Election proxies fail because election outcomes are susceptible to the influence of factors other than ideology. The various simulation methods make strong measurement and parametric assumptions that are unamenable to testing. Ten years ago,

responsiveness literature was at an impasse. Unable to clearly measure constituent inputs in House representative behavior, the literature's biggest contribution was perhaps environmental: entire forests gave their lives in the service of methodological critiques.

The MRP Revolution

Luckily, research has since taken an unexpected and salutary turn. A new statistical technology has emerged to fulfill Miller and Stokes' promise of empirically testing responsiveness. This method, multilevel regression imputation and post-stratification (MRP), has rapidly become social science's state-of-the-art method for disaggregating survey data (Park, Gelman, and Bafumi 2006). MRP bears three major similarities with older simulation techniques. First, it proposes to directly measure public opinion, rather than rely exclusively on surrogates. Second, like simulations, MRP synthesizes the disaggregation and proxy approaches in its incorporation of both individual- and aggregate-level predictors of ideology. A third similarity is that, like simulations but unlike raw disaggregation, it does not strictly require national surveys with large *ns*.

Where MRP differs from older simulations is, first, in its use of random geographic effects to help model state- and district-level ideology. Aggregate fixed effects (such as, say, the district's African-American population) in turn help model geographic effects (Lax and Phillips 2009b). Second, MRP incorporates a powerful probability adjustment using individual-level Census data. The result is that MRP is exceptionally sensitive to between-district variation in public opinion, and discards the problematic assumption of "top-down" simulations that demographic indicators have the same regression coefficient at every level of aggregation.

MRP proceeds in two discrete steps. First, using national survey data, a multilevel model estimates a dependent individual-level political variable (for instance, ideology) using

demographic (for instance, age and income) and geographic (for instance, state and congressional district) predictors. The incorporation of geographic effects is a novelty that deserves brief expatiation. MRP assumes that each level of American politics (national, regional, state, and House district) contributes to political attitudes in some way. If (say) I am a respondent in a national survey, the model tries to predict my response using various character traits (white, male, aged 25-29, earning between \$18,000 and \$30,000, and residing in a rural area). My response (“liberal”) affects the prediction of everyone else in the model. But because I am nested within Maine’s First Congressional District, the multilevel model also applies the effect of that variation unexplained by demographics to other cases in my district, state, and region. MRP here differs starkly from the exclusively demographic approach, which might predict that Maine’s 95% white (but solidly Democratic) 1st District is conservative.

After the model generates a prediction for every possible demographic and geographic type (or “cell,” in the literature’s parlance), MRP requires a second step. This step performs a probability adjustment by weighting the cell predictions by the actual proportion of these cells in each district. In more technical language, the step requires the researcher to use an inverse logit function that, given the derived multi-level regression coefficients, assigns a predicted value (say, the likelihood of an individual self-reporting as liberal) for every case in an individual-level Census dataset. These predictions are then weighted and averaged at the state or district levels. The probability adjustment, crucially, produces reliable and unbiased aggregate estimates of public opinion at the district level, even from national surveys with relatively small sample sizes (Lax and Phillips 2009a). MRP outperforms all previous methods of estimating state- and district-level public opinion, including top-down simulations, irrespectively of sample size (Warshaw and Rodden 2012b).

We can see how MRP defeats raw disaggregation when we think about how the method corrects for data deficiencies. For Congressional districts with few cases in the national survey, the raw district average of ideology does not carry much information. Thus, in the first MRP step, the multi-level model endogenously pulls the predicted value of cases towards state, regional, and national averages, a phenomenon called “shrinkage towards the mean” (Pacheco 2009). In the second MRP step, post-stratification uses the nationwide regression coefficients of both individual- and aggregate- demographic covariates with ideology to produce an estimate. This adjustment corrects for non-representative samples of states and districts in the national survey, while preserving the prediction information of the individual-level model. In contrast, for districts with large *ns*, and in which the survey selected a reasonably representative sample of PSUs, MRP is unlikely to produce an estimate that meaningfully differs from the results of raw disaggregation.

How has MRP spoken to the question of legislative responsiveness thus far? Lax and Phillips (2009a) found that state policies towards eight gay rights policies were highly responsive to MRP-imputed public attitudes towards them, more-so even than ideology. However, in a wider study of 39 policy issues, the same authors found that state policies were congruent with majority opinion only half the time (Lax and Philips 2012), with troubling implications for statehouse democracy in the United States. Tausanovitch and Warshaw (2013) use MRP on a massive sample—275,000 cases, pooled from multiple sources—to impute constituency opinion at the state-legislative and city levels. They find a “strong link between public opinion and city taxation regimes.” In the Senate, Kestel, Lax and Philips (2010) find that state-level public opinion was a significant predictor of Supreme Court confirmation votes.

The path of state-level MRP is now well-trodden. In the still-unplowed lands of House research, Warshaw and Rodden (2012) use MRP on the Cooperative Congressional Election Study

(CCES), a large-*n* online survey designed by Harvard researchers and conducted by YouGov, to produce district-level profiles along seven different policy domains. These domains emphasize “moral” issues such as stem cell research and abortion. They find that representatives are unresponsive to district attitudes on moral issues, owing to partisan and activist pressure at the national level. Wichowsky (2009) uses MRP to determine whether a citizen agrees with his or her House representative across a wide set of issue domains. She finds that incumbents are sanctioned for their disagreements with constituents, especially where electoral competition is robust and varies by policy. Incumbents respond to constituent preferences even if citizens are not actively monitoring them, because challengers can appear at any time. Given that citizens know little about legislators’ roll call votes, elites (namely, electoral challengers) must publicize these disagreements to affect voting behavior at all. Therefore, according to Wichowsky, competition incentivizes responsive behavior, as it triggers sanctioning. Tausanovitch and Warshaw (2013) find that representatives respond to party pressures independent of district-level opinion, thus identifying a key mechanism for shirking. Lastly, Bishin and Smith (2013) use MRP of national polls to estimate district-level opinion for the Defense of Marriage Act (1996) vote. They find that subconstituency politics—the presence of intensely motivated groups, operationalized as the presence of LGBT or evangelical communities within each district—explain why some Democrats “shirked” majority public opinion to support minority rights. MRP is therefore at the forefront of responsiveness research, and has helped research amend Downs’ (1957a) parsimonious demand model of representation, wherein representatives respond exclusively to their median constituent.

The advantage of these newer studies is that they match direct measures of district-level ideology with appropriate counterparts at the elite level. It is plausible to interpret a relationship between (say) a House representative’s vote on the Affordable Care Act and her district’s average

MRP-imputed attitude towards healthcare reform as legislative responsiveness. In contrast, older approaches would have tested the relationship between the same vote and a proxy such as district-level poor or uninsured populations, when in fact it is not clear that representatives would interpret this information as constituency pressure for or against healthcare reform. Another advantage of MRP responsiveness literature over predecessor approaches that it can test a wider range of issue positions. Raw disaggregation requires the concatenation of hundreds of surveys to generate meaningful measures of subnational ideology, whereas MRP can do just as well with one tenth the number of cases (Lax and Phillips 2009b). As a result, even if just *one* national poll of an average size includes an item for an issue position, it may still be possible to assign dispositions towards the issue at the desired geographic level.

While MRP has ushered in a new generation of scholars who approach the study of legislative responsiveness with unprecedented confidence, current MRP research is not without problems. I will focus on three of them: the fact that they do not measure the attitudes of known voters, their assumption of attitude stability, and their inability to test responsiveness in general.

Bringing Voters Back In

It is naïve to think that MRP is a panacea for bad and missing data. In the words of one set of critics, “one should not presume that the properties of the MRP model are sufficient to produce the desired opinion estimates from conventional national survey samples” (Buttice and Highton 2013, 465). The following critiques are therefore not of the statistical technology of MRP-derived measures, but rather of the number and types of respondents from which the measures are imputed. Flaws in the data threaten to distort our understanding of constituents. This distortion, in turn, potentially frustrates the ability of Political Science to assess the question of representation.

The first flaw is that these studies disaggregate samples of the general voting-age population, and not of the electorate. Commercial polls, as well as social-scientific surveys like the CCES, fail to distinguish voters from nonvoters in their samples (Silver et al. 1986). They are administered days, weeks, or months before or after elections, and dramatically over-report voter turnout due to social desirability response bias (Traugott and Katosh 1979) and the fact that politically engaged individuals self-select as survey respondents (Burden 2000). While the latter individuals are more likely to vote than the disengaged, they are also more likely to misreport having voted (Ansolabehere and Hersh 2012). The CCES therefore reports voter turnout to be as high as 90% for some years. In the past, poll disaggregators' inelegant solution to this problem was simply to dispense with survey weighting, on the theory that unweighted tabulations more closely approximate voters than weighted ones (Erikson, Wright, and McIver 1993). Luckily, better solutions now exist. The administrators of the CCES, and formerly the those of the ANES, have thus undertaken the costly exercise of validating responses using county-level registration and turnout records. Is this satisfactory? Some have raised concerns that this matching process systematically misclassifies respondents due to poor recordkeeping and flawed matching technology (Berent, Krosnick, and Lupia 2011). Moreover, where matching software is inconclusive and for jurisdictions that do not keep records, voter validation lowers the number of usable cases in the survey. The CCES Voter Validation Study could not validate 20% of its sample, including every respondent in Virginia, from 2006 to 2010. Using MRP with known voters in the CCES is certain to produce less reliable estimates than with the entire CCES sample.

Should we care? In the context of responsiveness, the answer is yes. An inquiry into legislative responsiveness must have a theoretical expectation about whom legislators are responding to, and it is unsound to assume without proof that they would respond to everyone.

More likely, they respond only to voters: either all voters in the district, or perhaps to some subset of especially loyal voters, as Huntington (1950) and Fiorina's (1974) "subconstituency hypothesis" holds. The voters-only view is plausible because every obvious linkage of constituent preferences and roll call behavior involves political participation. According to Griffin and Newman (2005), these links include voters selecting likeminded representatives, voters communicating their opinions to policymakers, and representatives proactively inferring voter preferences. If this is true, we would expect that competitive elections increase the extent to which representatives respond to voters, and there is indeed evidence for this "marginality hypothesis" in the House (Griffin 2006).

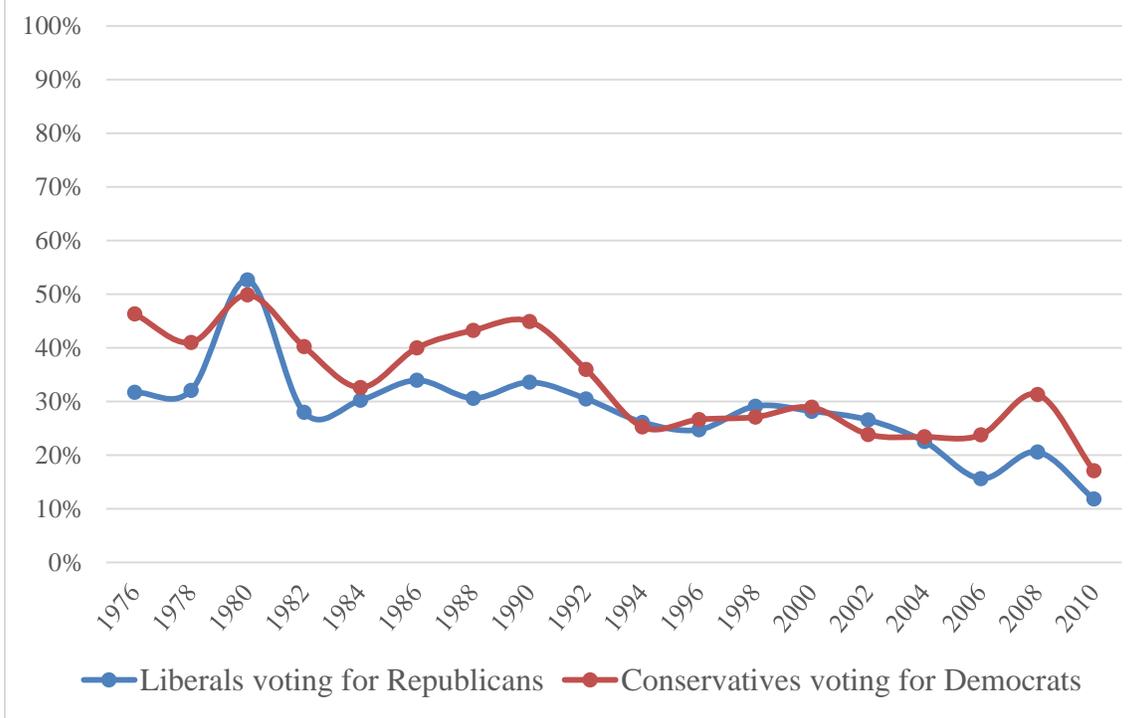
Taking a broader view, we see that Congress scholarship has long taught that the "electoral connection"—the transmission of attitudes from the public to lawmakers through voting—looms large as a determinant of floor votes. In Mayhew's (1974) classic model of distributional politics, representatives are single-minded reelection seekers who devote their resources (which include votes) to advertise themselves, claim credit for popular policies, and take positions to ingratiate themselves with constituents. Fenno (1978) similarly believes that even electorally safe representatives focus on expanding and maintaining their "reelection constituencies" (rather than trying to appeal to their "geographic constituencies," which may contain nonvoters and irreconcilable opponents who are politically useless) and emphasizes that they do so by connecting with voters through a "home style."

Fenno and Mayhew tell us that *voters* matter. But would they lead us to believe that *voter ideology* matters, at least to the extent that it deserves a privileged position vis-à-vis public opinion as a whole? After all, Mayhew models representatives as concerned with policy only inasmuch as policy allows them to distribute concentrated benefits to their districts, and Fenno holds that style

outranks substance; a representative's ability to credibly explain her votes to constituents can lead to reelection no less than robotically adhering to the voting public's preferences.

We have reason to think voter ideology does matter, however. The securement and advertisement of concentrated benefits in Congressional districts is one way to court voters, but it has never been the most important one. According to Stein and Bickers, the pork barrel is primarily the refuge of very vulnerable representatives, and “most members of the general public remain indifferent to alterations in the flow of new awards [to their districts]” (1994, 377). Pork has declined in importance in an era of mounting deficits, and after the 112th Congress' likely permanent elimination of earmarks. Moreover, the electoral conditions that prevailed in the days of Mayhew's electoral connection have radically changed. Fenno himself predicted that, as districts necessarily become larger and more complex due to national population growth, policy responsiveness would replace schmoozing as the best strategy to build and reinforce trust with constituents. His insights were prophetic, but size and complexity are not the only ways that district electorates have changed. The biggest change has been polarization. The partisan realignment of the South catalyzed a sorting process whereby liberals have increasingly identified as Democrats and conservatives increasingly identified as Republicans (Fiorina, Abrams, and Pope 2011). Polarization has undermined distributional explanations for representative behavior and has increased the importance of ideological connections to the mass electorate. The correlation between voter ideology and House vote has therefore dramatically increased (Jacobson 2013, 128), and this sea change in voting behavior has—more-so than institutional changes like (say) the phasing out of the old seniority- and consensus-driven “textbook” Congress—engendered a corresponding change in elite behavior (Abramowitz 2010a). Figure 2.1 highlights the divergent voting behavior of self-identified liberals and conservatives in House races.

**Figure 2.1: Ideology and Two-Party House Vote
National Exit Polls (1976-2010)**



In the wake of these changes, Congress is now unrecognizable to the scholarship of the sixties and seventies. No longer is the institution an irrelevancy compared to the modern presidency (Huntington 1965) or a corrupt establishment, “oblivious to the changing political sentiments of the country” (Fiorina 1989, 15), whose main purpose is chasing down constituents’ Social Security checks in exchange for votes. Members of a now-transformed Congress have deemphasized localized, distributional politics and have instead emphasized national *ideological* issues (Pietryka 2012). They now govern as members of responsible, polarized parties-in-government. Parties, not committee chairs, rule Congress (Cox and McCubbins 1993), and it is likely that changing voter behavior bears some responsibility for this change.

The growing correlation between ideology and House vote has arguably attenuated the ability of representatives to appeal to “shirk” from voter opinion. The same scholars who once worried about the “vanishing marginals”—viz., the proliferation of representatives who, election

after election, face no significant challenge irrespective of their voting records—have now revised their views, and find that representatives out-of-step with district-level policy attitudes face grave electoral risks. In an institution well-known for high reelection rates and low challenger quality, incumbent representatives of districts with opposite party majorities now incur defeat in over half of their elections (Schaffner 2015). Voters can and do hold representatives to account for their floor voting behavior (Ansolabehere and Jones 2010), and even when voters are largely ignorant of major votes, incumbents understand that challengers can at any time publicize votes that diverge from the policy preferences of district electorates (Wichowsky 2009). According to Bafumi and Herron (2010), representatives—cognizant of the need to strengthen their perceived identity with their closest supporters, or face defeat in primary or general elections—have recently grown more ideologically extreme than their constituents as a whole. In competitive races, ideologically-motivated voters may have outstripped the median constituent in importance. When member replacement occurs, a politician of one extreme replaces her mirror image on the other so as to “leapfrog” the median constituent. In his literature review of Congressional responsiveness, Shapiro (2013) speculates that representatives may in fact be *over*-responding to voters and elite political participants at the expense of non-voters.

What are such trends’ implications for MRP research? The main implication is that, because we have a strong theoretical expectation that policy responsiveness has supplanted distributional (Mayhew) and interpersonal (Fenno) reelection-maximizing strategies, it is now more necessary than ever to predict congressional behavior with measures of voter attitudes, not those of the general population. *Voter* ideology decides House elections, and voter ideology is what representatives are likely to respond to. The new line of responsiveness literature needs to operationalize constituent attitudes as voter attitudes. To date, it has not.

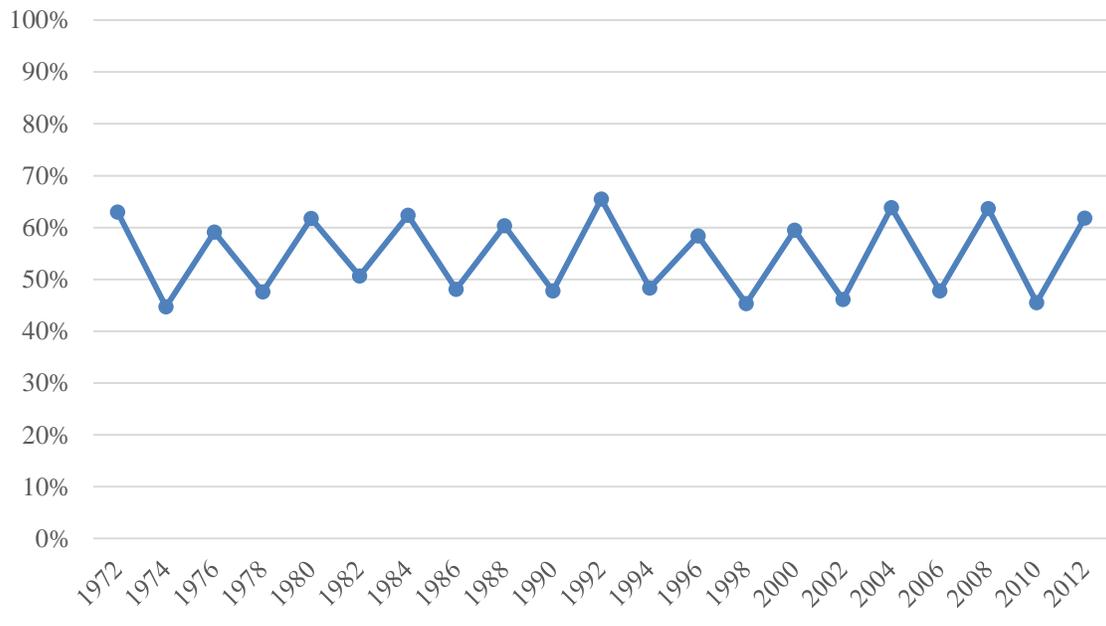
The above discussion would not distort MRP-derived measures if voters' were politically indistinguishable from those of the general population. This is not the case. A certainty of Political Science is that the American voter's attitudes systematically differ from that of nonvoters. Voters are older, more educated, and have higher socioeconomic statuses (SES) than nonvoters (Wolfinger and Rosenstone 1980), the latter of which correlates with the civic skills necessary for formal and informal modes of political participation (Brady, Verba and Schlozman 1995). Importantly from the standpoint of responsiveness, these voter characteristics map onto political attitudes. Griffin and Newman (2006, 1223) write of state-level electorates:

Voters are more conservative than nonvoters, and voters are better represented by elected officials ... Our results based on state-level differences between voters and nonvoters suggest that increases in turnout may lead to greater policy liberalism.

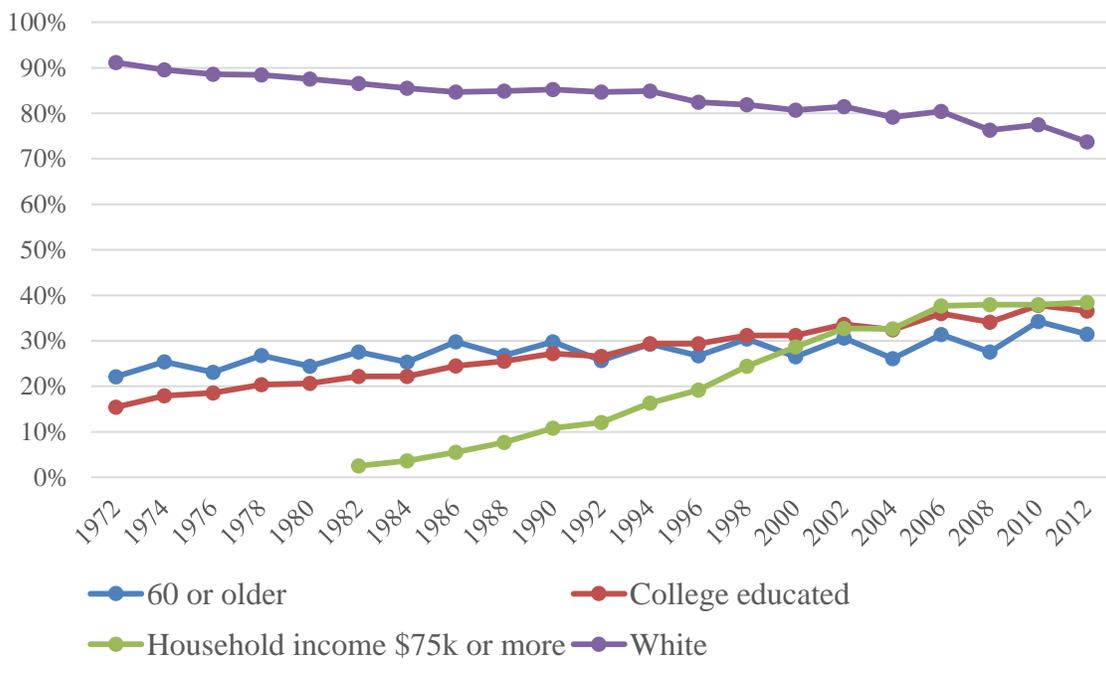
The differences between voters and the general population are likely to have grown as representatives' election constituencies are increasingly sorted into ideologically-divergent party blocs.

Moreover, the profile of the electorate varies from cycle to cycle in ways that the general population does not. According to Wattenberg and Briens (2002, 408), "Democrats are more likely to have demographic characteristics associated with low turnout and therefore are more inclined to drop off in a low stimulus election." Midterm elections are lower stimulus than presidential elections, and thus feature more smaller electorates (see Figure 2.2 for Census-derived turnout data, and Figure 2.3 for the demographic breakdown of voters in federal elections from year to year). These smaller electorates, consistent with Wattenberg and Briens' prediction, are more conservative; according to national exit polls from 1976 to 2010, midterm year voters are 4.5% more likely to self-identify as "conservative" than presidential year voters.

**Figure 2.2: Voter Turnout, Voting-Age Citizens
Current Population Survey Voter Supplement**



**Figure 2.3: Frequency of Voter Demographic Traits
Current Population Survey Voter Supplement**



Not only do previous studies employing MRP globally miscategorize the traits of constituents, but they also assume that the universe of constituents does not change from elections. This assumption requires testing: responsiveness, after all, is a dynamic process that centers on changing political conditions. Legislators, who are reelection-seekers more than they are popularity-seekers, could therefore be responding to short-term fluctuations in electoral preferences that are invisible in the public opinion polls used by conventional MRP approaches. My next chapter will explore how I might synthesize MRP with Lijphart's time-tested insight that "who votes, and who doesn't has important consequences for who gets elected and for the content of public politics" (1997, 4).

The Misplaced Assumption of Stability

A second limitation of extant MRP literature is that, in order to attain a sample size large enough to cover all 435 House districts, researchers have had to collapse survey data across several years. Attitudes towards specific policies, in particular, force researchers' hands; such items appear in fewer datasets, and in fewer years, than items like ideology and partisanship. In collapsing data, social scientists trade off precision for higher *ns*—that is, they must assume that the attitude or set of attitudes they are simulating are stable. They also arguably lose sight of what they are trying to study. Responsiveness is not a phenomenon amenable to static measures, either of constituents or legislators. It takes place over time, as representatives gradually make policy that takes constituent pressure into account. A cross-sectional analysis of legislation is therefore more likely to underestimate responsiveness than a longitudinal one (Lax and Phillips 2012, 157-8).

More importantly, the assumption that constituent attitudes are stable is too strong to safely make. Good but largely untested reasons exist to suppose that public opinion in subnational

geographic jurisdictions are unstable. One of them is that demographic compositions, which predict ideology in the aggregate, change. In a country where as much as twenty percent of residents move annually (Census 2012), population replacement alone could account for the instability of public opinion in Congressional districts. In this case, new district residents would replace older residents with different political attitudes. In this vein, one of the many notable hypotheses for polarization in the U.S. is “partisan sorting,” whereby individuals with similar characteristics and similar patterns of political behavior are choosing to live in the same areas; the net result are political homogenous congressional districts (Bishop 2008). Moving beyond migration, we might also expect local economic performance, which is often independent of national economic trends, to produce district-level ideological instability (Brace et al. 2004, 530).

It would be surprising if district preferences were *completely* static, and the assumption of stability runs up against mounting evidence that attitudes are unstable at the national (Stimson 1999) and state levels. In the latter research area, the once-dominant image of stable, glacially shifting state-level electorates—researchers used Elazar’s (1966) static “subcultures” as workable measures of state-level ideology for many years—has been overthrown. Berry et al. (1998; 2007) have found a wide temporal variation in the interest group scores of state House delegations and their electoral challengers, which they operationalize as state-level ideology. Brace et al. (2004) found temporal variation in the state-level ideologies of 20% of American states between 1960 and 1993 using a measure derived from the General Social Survey and American National Election Study. Pacheco (2014) finds that while state-level symbolic ideology is relatively stable, preferences towards certain policies (such as state government spending and the death penalty) exhibit both dynamism (change) and heterogeneity (having state-specific trends that differ from the changing national policy mood). Whether constituent ideology, let alone *voter* ideology, is

stable at the district level is unknown. Granting this, the fact that election-driven member replacement, and not changes in the behavior of House incumbents, is responsible for most of the historical change in House floor voting (Theriault 2006; Poole 2007) suggests that district preferences must change at least occasionally. And *if* district-level ideology is variable, then the results of all extant district-level MRP simulations (which generate only one estimate for each district, at one point in time) are quickly outdated.

The Case for Symbolic Ideology

Two competing conceptualizations of ideology exist in American public opinion literature. The first, *operational* ideology, refers to individuals' attitudes towards discrete policies or policy domains. Most MRP responsiveness research tests measures of operational ideology. Indeed, MRP's advantage is that it expands the universe of issue positions amenable to survey disaggregation.

However, it is important to not lose sight of the forest for the trees. My final critique of conventional MRP research is its excessively narrow focus on issue attitudes. This focus threatens to cloud our understanding of responsiveness in general and over multiple election cycles. The threat comes from the fact that the lifespan of most issues is short (Kingdon 1984; Baumgartner and Jones 1993). Even if issues do linger on the national agenda, their context and meaning may change. For example, the domain of gay rights has evolved from attitudes towards anti-sodomy laws, to laws against workplace and housing discrimination, to the legalization of civil unions, and finally to the recognition of gay marriage. Suppose we wanted to measure policy mood on gay rights over time. An MRP researcher would struggle to do so, since the issue, as well as the items that surveys choose to include on questionnaires, underwent several rapid transformations.

An alternative approach is the measure ideology more broadly, and rely on the symbolic conceptualization of ideology. Symbolic ideology refers to individuals' self-placed ideological beliefs. Behaviorists traditionally interpret symbolic ideology as a simplifying belief system that "constrains" individuals policy positions, such that one political attitude coheres with another (Converse 1964), but symbolic ideology has other roles. Media-driven issue framing is another antecedent of ideological self-identification, as are apolitical associations, as when individual identify as "conservative" because the word refers to moral rectitude in everyday speech. While some scholarship has held that symbolic ideology refers mainly to affective attachments to abstract symbols, and therefore not directly related to issue attitudes (Conover and Feldman 1981), recent work has shown that symbolic ideology is an important independent determinant of political behavior. This is particularly true among the educated, to whom the meaning of abstract ideological concepts are most readily accessible (Popp and Rudolph 2011). Ideological self-identification helps independently explain vote choice (Levitin and Miller 1979; Stimson 1975), issue attitudes (Jacoby 1991; Sears et al. 1980), and favorability towards government spending (Jacoby 1994; 2000; Rudolph and Evans 2005).

Some argue that symbolic ideology is less precise than its operational counterpart, but I counter that it is better adapted to historical analysis of responsiveness. Whereas the policy agenda unpredictably fluctuates, and it is impossible to know what America's policy agenda will be in advance, self-reported ideology measures the same thing over many decades. Attempts to measure operational ideology over long periods of time are of limited utility. For instance, the policy items in the GSS and ANES have not changed in a lifetime, and therefore may not accurately track policy mood on the most important, salient issues of the day. The domain of civil rights issues, in one example, declined dramatically in salience in the 1970s (Best 1999), perhaps rendering time-bound

Miller and Stokes' finding that representatives are especially responsive to constituent opinion on race. In contrast, while the ideological composition of the American electorate changes, the meanings of ideologies are stable despite the efforts of political elites to modify them—most famously when Republican strategists turned “liberal” into an epithet (Jacoby 1995). Only measurements of symbolic ideology offer the chance of studying attitude stability and instability independently of accidental, agenda-setting factors.

Symbolic ideology, being more constant and comprehensive than its ideological counterpart, make it a better conceptualization of district dispositions. Representatives cannot know in advance what their constituents think of particular issues that appear *de novo* on the national agenda. The proportion of liberals to conservatives in their districts, however, can predict issue opinion in the future. When they cast particular roll call votes, it thus makes sense for representatives to gauge constituent attitudes using this information. If MRP scholarship wants to speak to the question of House responsiveness in its entirety, and not merely to responsiveness on one vote in one Congress, it should take a longer view of the research question and test measures of symbolic ideology.

Moving Ahead

Having established itself in the most prestigious journals of American Political Science, MRP must now come of age. It behooves its practitioners to test the assumptions they have too readily assumed. I do not propose to join the faultfinders who claim that the absence of direct district-level survey data will prevent any successful attempt at observing district-level ideology and constituent influence in the House. I instead propose that MRP-generated exit poll estimates are a corrective to all of the weaknesses I identified.

Chapter 3. Disaggregating the Exit Polls

A New Approach

The previous chapter documents the need for a direct measurement of House district ideology. Multi-level regression imputation and post-stratification, or MRP, is a state-of-the-art survey disaggregation method that promises to meet this need. It is still only as good, though, as the data that goes into it. MRP responsiveness literature currently disaggregates commercial polls, as well as Harvard's Cooperative Congressional Election Study (CCES), and from them imputes district-level public opinion. I identified three data problems implicated in this approach. First, this data measures the dispositions of the district populations and not district electorates, when only the latter exercise constituent pressure on roll-call voting. Second, this data contains too few *ns* to generate longitudinal MRP estimates of district ideology. Researchers have collapsed surveys over many years to achieve satisfactory samples, and have made a problematic assumption that district ideology is stable. Finally, the new generation of scholarship studies measures constituent attitudes on only specific issues on the national agenda. This limits the breadth of our understanding of responsiveness.

I put forth exit polls as a promising alternative to the current, limited approaches. Uniquely among survey instruments, exit polls directly measure voter attitudes and behaviors. They do so at one point in time, and—in contrast to data concatenated from many sources collected at different time points—are interpretable as snapshots of public opinion on election day. Exit polls also collect questionnaires from far more individuals than commercial surveys or even the CCES. This eliminates the need to collapse surveys over time. These advantages mean that, when combined with MRP, exit polls can generate biennial district-level estimates of voter ideology.

I pursue the unexplored exit poll avenue in the subsequent pages. My plan for this chapter is thus as follows. First, I describe the exit poll phenomenon, and how I believe it will revitalize literature in legislative responsiveness. Second, I discuss the disadvantages of exit polls, and I how I propose to ameliorate them. Third, I specify, in detail, every step I take in generating my estimates of district-level ideology from the exit polls. Finally, fourth, I describe the results, which I hold to be valid and reliable.

Why Exit Polls Matter

Exit polls interview voters right as they leave the voting booth. They predict returns on election day even before every vote is cast, and they offer insights into the election immediately after. Journalists rely on exit polls to explain the most important issues on election day, how different groups voted, and how voters evaluated candidate traits (Best and Krueger 2012). The first sporadic exit polls were conducted in the 1940s, but Warren Mitofsky receives credit for popularizing them (Moore 2003). As director of CBS News's elections unit in the late 1960s, he developed a method for randomly selecting precincts in the exit poll's sampling frame. The purpose of the first systematic exit polls was to project election results on television, but soon arose the idea to ask a broader set of questions in order to *explain* the results. In the early years, this was impossible due to technical limitations, but by 1972 CBS was able to conduct a nationwide exit poll with a lengthy series of demographic questions. Journalistic and public interest in exit polls swelled. In addition, a media arms race emerged in the 1980s: CBS, ABC, NBC, and the Los Angeles Times all funded their own, separate exit polls in an attempt to be the first to call the election. Since 1990, news agencies have formed consortia to oversee exit polls and save costs. These organizations include the now-defunct Voter News Service (VNS) and the extant National

Election Pool (NEP). They do not themselves conduct the polls, a task they outsource to specialized polling firms.

The growing market for exit polls has led agencies to fund them even for off-year elections. Since 1982, CBS News and successor polls have administered state exit polls that are separate from, but compliment, their national ones. These exit polls administer questionnaires tailored to each state's gubernatorial, Senate, and (in small states) House elections as well as ballot initiatives to voters in all or most states.

The public is widely aware of exit polls, but their inner workings are inaccessible to most voters. Their most abstruse feature is their complex probability structure. In order to be generalizable to electorate, an exit poll must select respondents in such a way that every voter in the country (or, for state exit polls, the state) has an equal chance of receiving a questionnaire. It must also include a representative mix of demographic and geographic groups across the coverage frame. To achieve these goals, exit pollsters must A) randomly sample voting precincts and B) randomly sample voters exiting them. Exit pollsters categorize precincts in each state by two size strata, to ensure that small precincts have a chance of being selected. Within these strata, they classify precincts by proprietary sub-state regions to minimize coverage error. Lastly, they classify them by their vote percentage for one of the major political parties in a previous election, to ensure that political minorities achieve representation. Pollsters then randomly sample precincts from the above strata to get a representative set of precincts in both the state and national coverage frames. On election day, interviewers stand at a specified length away from the booths and are instructed to intercept and interview every tenth voter. While even in the best of circumstances there is the possibility of sampling error—not every voter has an equal chance of being approached for the survey, and not every voter approached has an equal chance of completing the questionnaire—,

this concern is often rendered harmless once responses are weighted to the actual election returns (Best and Krueger 2012, 12-13).

Exit polls naturally receive attention on election night, when news desks report the preliminary turnout rates and frequency distributions of vote choice in the exit poll. The polls are subsequently quick to fade from the public imagination. Only recently have scholars used them to study longitudinal trends, with the intention of historically *explaining* rather than predicting election results (Best and Krueger 2012). Even now, this intention has confined itself to studying the national polls. In the field of responsiveness, some researchers have used state exit poll distributions to validate their own MRP measures of state ideology, but in no instance have they used exit polls to generate the measures themselves.

Exit polls offer four major benefits for measuring state- and district-level public opinion. First, exit polls directly measure voter behavior and solve the overreporting of voting that plagues conventional polls. They allow researchers to dispense with complicated reclassification and voter validation schemes to prune nonvoters from the sample. If we adopt the sensible assumption that rational representatives will respond to political participants who can in principle remove them from office, then exit polls are uniquely suited for analysis of Congressional behavior. In contrast, the weighted frequency distributions of the voter-validated CCES are at wide variance with election returns.

A second advantage of exit polls is temporal. Their respondents are known voters who complete interviews immediately after casting a ballot, when they are unlikely to misremember their vote choice.¹ This means that exit poll results are interpretable as a snapshot of public opinion. In

¹ Additionally, to account for early and no-excuse absentee voting, exit pollsters have since 1996 conducted random-digit-dialed phone interviews, the sample size of which is based on the expected ratio of early to election day voting. The results of these are combined with election day interviews.

contrast, MRP studies have typically relied on concatenations of survey instruments (say, two years of the CCES) that are administered over a substantial time-frame. In that case, it hard to interpret *when* a House district was 40% liberal; it is possible that public opinion shifted from year to year. It would therefore be fraught to interpret such a measure as a precise constituent signal to representatives. In contrast, any relationship between an exit poll-derived measure and Congressional behavior captures responsiveness to voter ideology on the *exact day* that voters authorize incumbent representatives to continue their careers.

A third advantage is that exit polls are self-administered in person. Exit poll respondents read the questionnaire themselves and confidentially record their own answers, militating against the social desirability response bias that afflicts phone surveys. Moreover, live administration makes the exit polls immune to concerns about the increasingly common opt-in Internet survey mode, which the CCES employs. According to a recent taskforce that the American Association of Public Opinion Researchers commissioned, the proneness of Internet surveys to measurement error is not fully understood (AAPOR 2010).

Finally, a fourth advantage of the exit polls is their massive case size, which is far in excess of conventional telephone surveys and all other prominent academic surveys with a multi-decade time series. They also collect more questionnaires than the large-*n* CCES. Table 3.1 shows compares the *ns* of the combined state and national exit polls from 2006 to 2010 to the CCES. After the voter validation study eliminates CCES cases unmatched to voter rolls, we can see that exit polls have sample sizes that are 150% (2010) to 350% (2008) as large as those in the CCES. The massive *ns* permit me to run separate MRP models for each year, whereas researchers running MRP on the early CCES studies must merge them by year and so produce only one estimate of district ideology for their time series. Running models for each year offers two advantages. First,

previous MRP regression models in the House responsiveness genre treat “year” as a random slope, fitting an effect size for each case grouped by year. While such models account for between-year variation unexplained by demographics and geography, they undersell the systematic differences between midterm- and presidential-year electorates, and cannot account for (say) different effect sizes for “race” from year to year. (It is probable that the effect of race on presidential vote choice was more muted in the 1988 presidential election, where 25% of African Americans voted for George H.W. Bush, than in the 2008 election). Thus I can create better measures, which capture election-specific dynamics. Second, and more importantly, biennial measures of district ideology permit me to study responsiveness longitudinally. In contrast, much previous literature looks at House responsiveness only statically and cross-sectionally, on the problematic assumption that constituent ideology is stable in the aggregate.

Table 3.1: Ns in the Exit Polls and CCES, 2006 to 2010

Year	Exit Poll	CCES (all respondents)	CCES (known voters)
2006	53,742	36,119	26,084
2008	83,620	32,461	24,006
2010	55,027	54,857	37,583

The Disadvantages of Exit Polls

Most critiques of exit polls center on whether or not they “get it right” on election night. Notable gaffes in 2000 (when miscommunicated country-level election returns caused news networks to prematurely call Florida for George W. Bush), 2002 (when the results of the state-level exit polls were so unreliable that they were never released), and 2004 (when the exit polls overestimated John Kerry’s performance in battleground states) undermined journalistic and public trust in exit polling (Freeman 2004). These worries are not here relevant. The probability structure of exit polls

is as sound as any other national poll, and post-stratification often eliminates the response bias that caused the 2004 debacle (Edison Research 2004).

What *is* relevant for those who wish to disaggregate exit polls is the problem of missing data. State exit polls—designed to sample states electorates, naturally, and *not* district ones—do not begin to reliably include district-level identifiers until 2004. This limits the scope of their ability to study responsiveness. Fortuitously, the 2004 election coincides with the election of the 109th Congress, and the true end of the post-2000 redistricting cycle. (The last decade featured notorious out-of-cycle redistricting in Texas in 2002). The polls in 2004, 2006, 2008, and 2010 therefore include consistent geographic identifiers, and still afford us a good opportunity to examine the evolving policy mood of Congressional districts, and the responsiveness of their representatives.

More troublingly, state exit polls are also not geographically comprehensive. While a national exit poll is always held biennially, poll administrators do not fund polls in every state during off-year (and, in 2012, even presidential) elections to save costs. In 2006 and 2010, the NEP thus restricted its state-level efforts to states holding Senate and gubernatorial elections. Moreover, even the state polls they do conduct are not fully comprehensive of the Congressional districts in their coverage frame. As I discussed above, the probability structure of exit polls aims at geographic comprehensiveness by sampling precincts from a state subregional strata. Even so, these state subregions (usually 3 to 5 per state) do not track congressional districts and are typically less numerous than the latter. Whether the pollsters collect questionnaires from precincts in every Congressional district is accidental, and it is extremely unlikely in populous states such as California. The exit poll's recent incorporation of a supplemental telephone survey in eighteen

states increases the effective number of congressional districts in the biennial sample, but is far from solving the problem.

It is true that MRP has a built-in mechanism for correcting for missing data: in districts without a single case, the individual level model without a random slope for “district” generates the estimate (Kastellec 2012). This raises the question, however, of why to bother using exit polls as opposed to surveys with greater geographic comprehensiveness. My solution to this problem is to merge the national exit poll with the state exit poll, dramatically increasing the size and coverage (the percentage of districts in which there is at least one respondent) of our sample. While such a merge is unacceptable for normal purposes—the sample of precincts in the national exit polls is representative only of the national population, such that the weighting between the state and national exit polls are inconsistent—MRP uses its own probability adjustment in lieu of proprietary survey weighting.

I therefore merge the state exit polls with the national polls by year (taking care not to double count respondents who were administered the national questionnaire, some of whom appear in the state exit poll datasets). Table 3.2 shows the expanded jurisdictional coverage that the merge accomplishes.

Table 3.2: Cases in the district-level Exit Poll MRP*

Year	Cases	House Districts**	Coverage	Average Cases per District	Median Cases per District
2004	65,193	379	87%	172	103
2006	51,139	377	86%	136	77
2008	77,031	428	98%	180	129
2010	56,962	404	93%	141	82
2004 - 2010	250,325	435	100%	144	100

* Includes cases from both the state and national exit polls, where the variables ideology, race, gender, income, and population density are not missing. These variables are necessary to create individual-level predictions of ideology that can then be post-stratified using Census data.

** Includes at-large districts and Washington, D.C.

Having achieved the sample size necessary for my purposes, I must assess whether the combined state and national exit polls meet the subtler data requirements for MRP to produce valid, reliable estimates. Butticce and Highton (2013), who are skeptical about whether the method is as panacean as its votaries claim, identify some. The most important requirement, in their estimation, is that the ratio of the dependent variable's variation across geographic units is high relative to its variation within the units. Lax and Phillips (2009b) and Warshaw and Rodden (2012) found that MRP produces reliable estimates of opinion on moral issues at the state and district levels respectively, and this is largely because there is a greater level of district homogeneity for moral attitudes than for other issues domains (Butticce and Highton 2013, 463). Likewise, gerrymandering and the residential sorting hypothesis (Bishop 2009), as well as the fact that most at-large districts (with the possible exception of Delaware) are ideologically heterogeneous compared to most other states, lead me to expect concentrations of liberals and conservatives within districts.

The Model

With the above considerations in mind, I have chosen to use MRP on a merge of state and national exit poll data. As I discussed in the previous chapter, the survey item I impute and operationalize as constituent opinion is self-reported ideology.

MRP begins with an individual-level model of the dependent variable. The goal is to generate numeric probabilities of different demographic and geographic groups to hold certain beliefs, in our case whether they self-identify as "liberal" or "conservative." My model of ideology must include only those predictors with corresponding variables in a post-stratification dataset, and must therefore exclude political predictors such as party identification, interest, efficacy, and

so on. I run two multilevel, mixed-effects binomial regressions to predict ideology for each year: first, I predict whether or not a respondent is liberal; then, I predict whether or not a respondent is conservative. Ideology is a function of an individual's demographics, Congressional district, state, and region.

It is important to note that the ideology model need not be theoretically useful, nor very predictive, to work for my purposes. I simply choose my demographic predictors—race, sex, income, age, and income—because many of these indicators have served as proxies for ideology in previous literature (for instance Kalt and Zupan 1984; Peltzman 1984), and because MRP literature has considered them “standard” beginning with the earliest studies (Park et al. 2004, 2006). I also use interaction terms given that the intersection of race and gender variables, as well as those of age and income, may produce systematic changes in political behavior. Education is also a standard factor in explaining ideology, but as this item inexplicably does not appear on every state questionnaire in the exit poll from 2004 to 2010, I substitute the dummy variable “rural” (whether or not the respondent lives in a Census-designated Metropolitan Statistical Area, or MSA) in its stead. Theoretically, rural should predict conservatism at the individual level. Table 3.3 shows the result of a simple and none-too-adequate logistic regression of liberal using my demographic predictors, or dummy recodes thereof. The results validate the common wisdom that demographics are real, though not very powerful, predictors of individual ideology.

Table 3.3: Binomial Logistic Regression, Liberal on Demographic Predictors, State Exit Polls (2004-2010)

Fixed-Effect Predictor	(1) Liberal
Affluent	-0.297*** -0.259*** (-20.95)
African-American	0.177*** 0.232*** (13.68)
Female	0.312*** 0.253*** (22.60)
Latino	0.077*** 0.150*** (5.91)
Rural	-0.350*** -0.303*** (-24.41)
Under 30	0.407*** 0.581*** (33.60)
<i>N</i>	265,640
Pseudo R ²	0.02

Standardized beta coefficients; unstandardized point estimates; robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

I now formally specify the model. For respondent i in a given election year—with indices j, k, l, m, n , and o for race, age, income, an interaction term of sex and race, an interaction term of income and age, and district, respectively—the probability of an individual responding “liberal” is:

$$\Pr(y_i = 1) = \text{logit}^{-1} (\beta^0 + \alpha_{j|i}^{\text{race}} + \alpha_{k|i}^{\text{age}} + \alpha_{l|i}^{\text{income}} + \alpha_{m|i}^{\text{sex_by_race}} + \alpha_{n|i}^{\text{income_by_age}} + \alpha_{o|i}^{\text{district}} + \beta^{\text{sex}} \cdot \text{sex}_i + \beta^{\text{rural}} \cdot \text{rural}_i)$$

where:

$$\begin{aligned}
\alpha_{j[i]}^{race} &\sim N(0, \sigma_{race}^2), \text{ for } j = 1, \dots, 5 \\
\alpha_{k[i]}^{age} &\sim N(0, \sigma_{age}^2), \text{ for } k = 1, \dots, 8 \\
\alpha_{l[i]}^{income} &\sim N(0, \sigma_{income}^2), \text{ for } l = 1, \dots, 6 \\
\alpha_{m[i]}^{sex_by_race} &\sim N(0, \sigma_{sex_by_race}^2), \text{ for } m = 1, \dots, 10 \\
\alpha_{n[i]}^{income_by_age} &\sim N(0, \sigma_{income_by_age}^2), \text{ for } n = 1, \dots, 48
\end{aligned}$$

The above are random effects. The ideology of each respondent is assumed to vary randomly between the categories of each variable, so a slope is fitted to each category. Each effect is modeled as drawn from a normal distribution with a mean of zero, plus some estimated variance. Rural and sex are individual-level, linear fixed effects. The district effects are modeled as a function of the state in which the district resides, plus district-level aggregate effects held constant for every respondent in the district (percent of district residents in the bottom quintile of national household income; percent of residents with a graduate-level education; percent of residents who are military veterans; percent of residents who are African-American; and percent of residents who speak English as a second language):

$$\alpha_{o[i]}^{district} \sim (\alpha_{p[o]}^{state} + \beta_{poor} \cdot poor_o + \beta_{grad_educ} \cdot grad_educ_o + \beta_{veteran} \cdot veteran_o + \beta_{black} \cdot black_o + \beta_{retired} \cdot retired_o + \beta_{est} \cdot esl_o, \sigma_{district}^2), \text{ for } 1, \dots, 436$$

Aggregate effects are necessary ingredients in MRP models. The size and significance of area-level covariates can decisively increase the performance of imputed estimates (Buttice and Highton 2013). Table 3.4 shows the correlations between these fixed effects and unadjusted district ideology in my exit poll data. These correlations are robust, and in the expected directions. I made the conscious decision not to include a lagged ideology variable among the district effects. Lagging the dependent variable would mask year-to-year variation in my results, defeating my purpose of

studying longitudinal trends in public opinion. It would also, in all likelihood, so strongly over-predict ideology as to crowd all other effects to insignificance (Achen 2000).

Table 3.4: Product-Moment Correlations, Raw Ideology Disaggregation and District-Level Covariates (Source: State and National Exit Polls, 2004-2010)*

Correlation	% Black	% ESL	% Graduate Educated	% Veteran	% Poor	% Retired
Ideology	-0.19	-0.31	-0.49	0.41	0.19	0.24

*District ideology is coded from -1 (most liberal) to 1 (most conservative). I generated the values using a raw, unweighted disaggregation

The state effects are in turn modeled as the subregion (New England, Mid-Atlantic, Midwest, Appalachia, Southeast, Southwest, Plains, Mountains, Pacific Coast, and Washington, D.C.) in which the state resides:

$$\alpha_p^{state} \sim (\alpha_{q[p]}^{region}, \sigma_{state}^2), \text{ for } 1, \dots, 51$$

To reiterate, MRP assumes that every level of geography makes a random (“random,” because drawn from a larger population of geographic categories) contribution to individual ideology. Standard, therefore, in the most cited MRP literature is the inclusion of region, typically the four Census-designated regions, in the individual model. Pacheco (2011, 435) holds that this is unnecessary and even harmful. Region tends to slightly homogenize the aggregate estimates of attitudes, because the model assigns the same random slope to every jurisdiction in a region. Nonetheless, because we are working on the assumption that sub-national ideology changes from year to year, and because scholars have hypothesized recent American ideological change to be regional in nature (Bishop 2009), I still wish to include a regional variable in the model. I propose a middle ground, whereby I include *subregion*. These subregions are chosen for their geographic and ideological affinities, forestalling the inclusion of ideologically opposite states like (say) Washington, D.C. and Alabama in the same area. Table 3.5 shows the breakdown of subregions,

and the weighted frequencies of liberals by subregion in the 2008 CCES. The latter illustrate that subregions differ in ideological composition in intuitively expected ways.

Region	States	% Liberal
New England	Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont	34.87%
Mid-Atlantic	Delaware, Maryland, New Jersey, New York, Pennsylvania	30.92%
Midwest	Illinois, Indiana, Michigan, Ohio, Wisconsin	27.67%
Appalachia	Kentucky, North Carolina, Tennessee, Virginia, West Virginia	27.08%
Southeast	Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, South Carolina	21.42%
Southwest	Arizona, New Mexico, Oklahoma, Texas	23.33%
Plains	Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota	26.00%
Mountain	Alaska, Colorado, Idaho, Montana, Nevada, Utah, Wyoming	24.17%
Pacific Coast	California, Hawaii, Oregon, Washington	34.34%
Washington, D.C.	Washington, D.C.	64.97%

In sum, because my pre-empirical intuition is that ideological polarization in the US is occurring by region (New England and the West Coast grow more liberal as the South grows more conservative), I include region as the last modeled effect:

$$\alpha_{q|p}^{region} \sim (0, \sigma_{region}^2), \text{ for } 1, \dots, 10$$

This model, first, will predict whether exit poll respondents in a given year are liberal. Second, it will predict the probability of non-liberals responding as conservative. To arrive at a prediction of ideology for each group, or “cell” (c), of demographic and geographic variables, I subtract the liberal prediction (derived from the inverse logit function, given the predictors and their regression coefficients) from the conservative prediction thus:

$$ideology_c = \Pr(conservative_c = 1) \cdot (1 - \Pr(liberal_c = 1)) - \Pr(liberal_c = 1)$$

The conservative probability is weighted by the inverse of the liberal probability, to account for the fact that the conservative model excludes all liberal respondents. This multistep process, whereby I use MRP to estimate a variable with three or more possible categories, is known as nested multinomial MRP (Kastellec et al. 2014). The direction of the estimates (-1 being most liberal; 1 being most conservative) permits their visualization on a number line (the further to the right a cell estimate is, the more likely it is to “right-wing”). My estimates will also correspond to the scale of Poole and Rosenthal’s NOMINATE scores, permitting us to more easily infer the correlation between district ideology and representative behavior. If I wish to make my estimates more interpretable, and more comparable to such measures in the policy mood literature as macro-partisanship (Erikson, MacKuen, and Stimson 2002), I can compute cell ideology thus:

$$macro_ideology_c = \frac{\Pr(liberal_c = 1)}{\Pr(liberal_c = 1) + \Pr(conservative_c = 1) \cdot (1 - \Pr(liberal_c = 1))}$$

The above estimates ideology as a function of the probability of being liberal out of the combined probability of being liberal or conservative, much as macropartisanship is the proportion of Democrats out of all partisan identifiers. If we aggregate estimated macroideology by district and year, and observe that macroideology in Vermont’s at-large district has increased by 0.05 over a decade, then I can instantly infer that the proportion of liberals to conservatives in the electorate has increased by 5%.

Once I have generated an estimate of ideology for every possible combination of demographic and geographic variables in the combined state and national exit polls, the most crucial step in MRP—the probability adjustment—remains. This adjustment reweights the ideology estimates by the proportions of each cell in each Congressional District. The American Community Survey (ACS) will serve as my post-stratification dataset, as well as my source for

district-level fixed effects in the model. The ACS is a monthly household survey, the largest the Census Bureau conducts outside of the decennial Census, sent randomly to about 3.5 million addresses each year.

Unfortunately, the full ACS is publicly unavailable. While the Census makes the full aggregate results—say, pre-tabulated summaries of demographic information by state, Congressional district, and county—available to the public on its FactFinder website, it limits the release of individual-level results due to concerns about privacy and liability. The power of MRP, however, consists in how it applies individual level predictions of a dependent variable to individuals in a post-stratification dataset; the tabulations themselves do me no good. Warshaw and Rodden (2012), who also reweight the MRP results by ACS data, did manage to simulate cases in each Congressional district using the tabulations. This is an inelegant solution because it forbids them from using certain key predictors for which cross tabulations are unavailable. Among these unavailable predictors is age.

My solution for reworking the ACS is more ambitious. I begin not with the cross-tabulations on the Census website, but rather with the limited individual-level ACS data (“microdata”) the Census makes public. The Census imposes two serious limitations on its microdata. First, the Census releases only 5% yearly samples of the ACS called Public Use Microdata Samples (PUMS). Second, and what is worse news for my cause, the PUMS do not identify cases by Congressional district owing the privacy concerns. The only geographic identifiers they include are state and Public Use Microdata Area (PUMA). PUMAs are arbitrary statistical geographic areas containing at least 100,000 residents. To adjust my estimates by PUMS data, I need a way to assign individual a district to ACS respondents using only the respondents’ PUMA. Doing so is possible, as I am moving from a smaller geographic unit to a larger one

(Congressional districts have an average population of about 700,000), but hazardous. What I plan to do is assign all cases a probability for residing in each of the congressional districts in the country.

To convert PUMAs into districts, I use the University of Missouri's MABLE/Geocorr2K correspondence engine, available online at <http://mcdc2.missouri.edu/websas/geocorr2k.html>. This web application produces a spreadsheet with columns for PUMA, state, congressional district number (in my case, for the 109th Congress), and the percentage of the PUMA's population that resides in the district per the 2000 Census. I use this spreadsheet to generate a Stata-Do file that inserts probabilistic district identifiers in the ACS. If an ACS respondent resides in a PUMA that is allocated (say) 70% to Maine's First District and 30% to Maine's Second District, the Do-file clones the respondent with the `expand` command. One clone's Congressional district variable is set to First District and its weight is 0.7; the other clone, in all other respects identical to the other, has its district variable set to the Second District and its weight is 0.3. This creates a fuzzy approximation of district-level geographic information—fuzzy because since most respondents in the ACS have a chance of residing in more than one district, and because the accuracy of MABLE/Geocorr2K's generated correlations of PUMA and district populations decreases as I move away from the 2000 Census. Another source of inexactitude is the fact that, prior to 2005, the PUMS did include any sub-state geographic identifier, as the ACS was only in trial stage at this time. I have no choice but to post-stratify the 2004 Exit Poll census-level estimates with the 2005 PUMS.

Once I merge the ACS from 2005 to 2010 and add the district probabilities, more work remains. Since I am modeling ideology with voter data, I want the probability adjustment to, as much as possible, reflect the electorate as opposed to the general population. I will first drop

ineligible voters according to the age and citizen variables. There is no turnout data, unfortunately. A logical solution is to weight the cases, incorporating a dataset that *does* include turnout and using the ACS's demographic and geographic variables, by their likelihood to vote. To do so, I model voter turnout in the Census's Current Population Survey (CPS)—a monthly survey, geared towards employment data, that polls 50,000 American households—which, on November of each election year, includes a Voter Supplement with registration and turnout questions. The voting model, naturally, will use predictors in both the ACS and CPS. For each year of CPS data (2004, 2006, 2008, and 2010), I run a mixed-effects logistic regression, with accepted covariates with turnout (socioeconomic status and social capital items) and geographic variables as my predictors: state; income; age; race; Education (5 categories); sex; veteran status; marital status; whether or not residing in a Metropolitan Statistical Area (MSA); whether or not respondent is in a white-collar occupation; whether or not respondent is in a blue-collar occupation; whether or not respondent is a naturalized, as opposed to born, citizen; whether or not respondent has moved in the last year; and, lastly, relevant interaction times (sex by age, and education by age).

To create a turnout weight in the ACS, I then use the inverse logit function given the estimated CPS regression coefficients and their respective predictor variables in the ACS. The final weight will be the CPS-derived turnout weight multiplied by the district weighting. While the ACS includes its own weight, I decline to use it for two reasons. First, even the 5% sample of the ACS is large enough to, unweighted, serve as a proxy of the actual district populations. Second, since the weighting is designed to inflate the importance of groups that are systematically less likely to receive or respond to questionnaires, and since we have intuitive reasons to think that such groups are also systematically less likely to vote, the ACS weight does us no good in reconstructing the electorate.

Once I generate ideology estimates for each demographic-geographic cell, estimating the means of ideology for each district is straightforward. Where Y^{MRP} is simulated aggregate ideology, o is the district index, N_c is the frequency of each cell in the ACS, θ_c is the predicted ideology value for the cell, and w_i is the CPS-derived turnout weight for each individual in the ACS:

$$Y_o^{MRP} = \frac{\sum_{c \in o} N_c \theta_c w_i}{\sum_{c \in o} N_c w_i}$$

As I run a separate model for each election, every Congressional district in the US will receive four estimates, one for each election between 2004 to 2010.

The Results

The sheer number of estimates ($436 \cdot 4 = 1,744$) that the MRP imputation creates means that I cannot display them all in a readable published table. The unwieldiness of the output led me to create a web application that filters and outputs raw tabulations and summaries of my district-level ideology data. The application is available at the following link: http://cliffordvickrey.com/diss_files/php/mrp.php. In the following section, I subordinate my choice of tables to the need for assessing the concept validity and reliability of my exit-poll-derived imputed estimates.

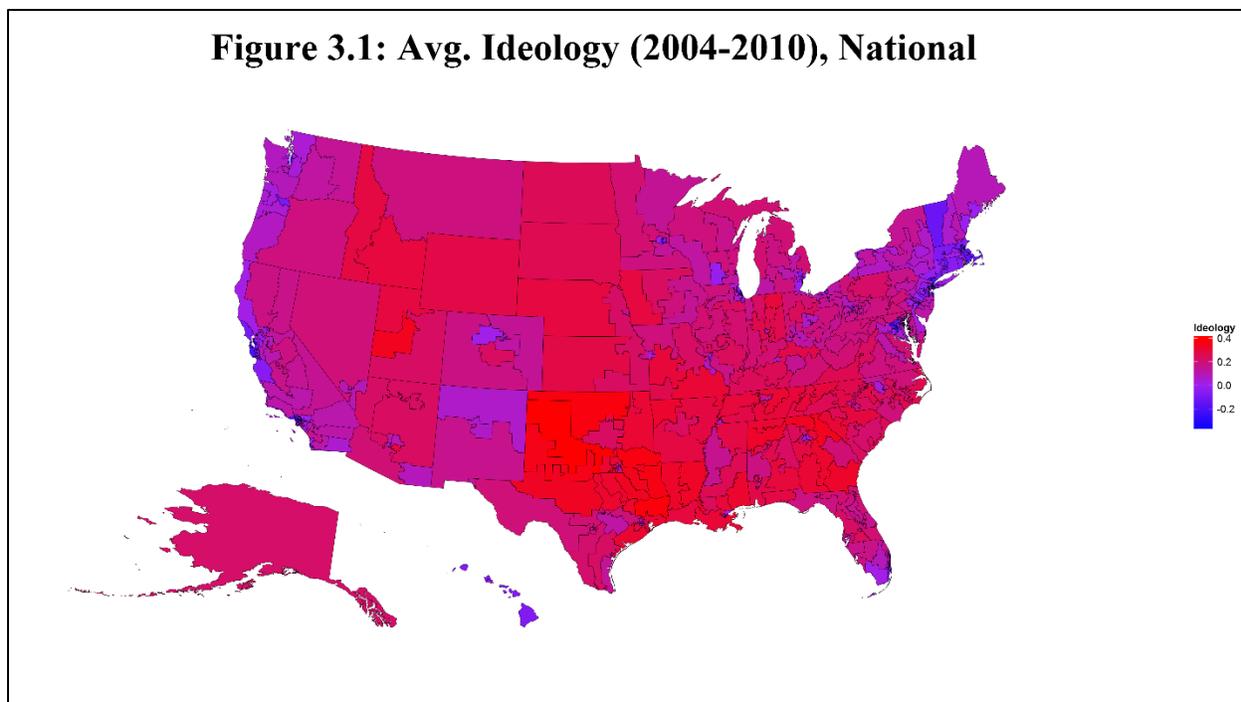
Table 3.6 shows summary data by year. The table includes the mean aggregate ideology by year, the mean ideology in the National Election Pool's (NEP) national exit poll by year (for validation), information on the median district, the minimum (most liberal) district, the maximum (most conservative) district, standard deviation, and the number of observations per year and district. As expected, district voting populations grow more liberal in presidential years and more

conservative in midterm ones, tracking the national trend evidences in the NEP average. The most liberal districts are Massachusetts 8 (covering Cambridge, one of the most liberal cities in America [Alderman 2005]), New York 8 (covering minority-majority precincts in Manhattan) New York 12 (covering minority-majority precincts in Brooklyn and Queens), and New York 16 (the South Bronx, among the poorest urban areas in the country [Sisk 2010]). The most conservative districts are Alabama 4 (majority white counties in the north-central part of the state), Georgia 9 (rural northeastern Georgia), Texas 8 (engineered by state Republican leader Tom Delay to be overwhelmingly Republican), Texas 13 (the most Republican district in the country [Wasserman 2013]). The relative ideological positioning of such districts comports fully with expectations.

Year	Mean	NEP Mean	Median	Median Dist.	Min	Min Dist.	Max	Max Dist.	St. Dev.	Obs.	Mean Dist. Obs
2004	0.11	0.13	0.13	NM 2	-0.36	MA 8	0.38	GA 9	0.15	65,193	150
2006	0.09	0.12	0.11	TX 30	-0.47	NY 12	0.41	TX 13	0.16	51,139	117
2008	0.09	0.12	0.11	NJ 4	-0.49	NY 8	0.39	AL 4	0.16	77,031	177
2010	0.15	0.21	0.20	IA 3	-0.59	NY 16	0.49	TX 13	0.19	56,962	131
Pres. Years	0.10	0.12	0.12	TX 18	-0.40	MA 8	0.37	TX 8	0.15	142,224	163
Midterm Years	0.12	0.17	0.16	NJ 3	-0.39	MA 8	0.45	TX 13	0.17	108,101	124
All Years	0.11	0.14	0.14	OH 2	-0.40	MA 8	0.40	TX 13	0.16	250,325	144

Figure 3.1 shows a map of district ideology in the entire United States, averaged from 2004 to 2010. The more liberal a district, the bluer it is; the more conservative, the redder. Aggregate liberalism is concentrated in New England, urban districts, the Black Belt, the Río Grande Valley, and the coasts—again, perfectly consonant with popular and academic understandings of the distribution of voters in American political geography.

Figure 3.1: Avg. Ideology (2004-2010), National



To test concept validity, I correlate my yearly measures with both alternate MRP estimations of ideology, as well as measures bearing close conceptual resemblance with symbolic ideology. The correlations appear in Table 3.7. First, I correlate my measures with the results of raw disaggregation using the exit poll. My expectation is that they will be highly correlated—as the two measures are drawn from the same data—especially in the years containing the most survey respondents. When *ns* are smaller, MRP’s post-stratification step more powerfully adjusts survey data. As expected, the correlations are very high except in 2010, where the NEP’s national coverage was poorer than in previous years. Second, I ran my MRP model using the CCES—substituting the random effect of education for my “rural” variable, which lacks a counterpart in the CCES—for the years 2006 to 2010. The correlations with my measures are rather high, owing to the fact that both MRP procedures used the same post-stratification dataset. Third, I correlate my measures with those of previous responsiveness scholars. For this purpose, I average Warsaw and Rodden’s (2012) estimates of district-level opinion various policy domains; this average

constitutes an estimate of operational ideology. The high correlations with my symbolic ideological measure suggest that the distinction between symbolic and operational ideology is more conceptual than empirical. Fourth, I ran my same MRP model, only substituting party identification for ideology as my dependent variable. My expectation is that both measures are correlated, given the ideological sorting of voters into American political parties since the 1970s (Fiorina et al. 2011), and this is indeed the case. The correlation is the highest in the ideologically-charged 2010 midterm elections. Finally, fifth, I correlate my measures with election results, which have served as proxies of district-level ideology in the past. The correlations suggest that, while ideology is highly related to voting behavior, non-ideological factors do influence vote choice.

Table 3.7: Correlation of District-Level Ideology Estimates with Related Aggregate Measures, 2004-2010

Variable	Exit Poll Ideology*				
	All Years	2004	2006	2008	2010
Raw Exit Poll Tabulations	0.81	0.91	0.90	0.88	0.67
CCES Ideology*	0.89		0.83	0.91	0.91
Warshaw-Rodden Ideology**	0.85		0.77	0.79	
Exit Poll Party ID*	0.81	0.75	0.87	0.89	0.86
Dem Pres. Vote†	-0.76	-0.77	-0.77	-0.78	-0.82
Dem. House Vote††	-0.62	-0.56	-0.55	-0.56	-0.74

* Imputed using MRP.

** An average of MRP-imputed estimates of district attitudes towards abortion, environmental policy, gay marriage, the minimum wage, Social Security, and stem cell research.

† Percentage of two-party vote in the most recent presidential election.

† † Percentage of two-party House vote. General elections only.

Uncontested districts codes as 100% (Democratic) or 0% (Republican).

Votes for Bernie Sanders (I-VT at-large) coded as Democratic.

In sum, my measures are valid. The correlations are highest (.8 to .9) with other MRP imputations of symbolic ideology, which directly measure the same intended characteristic as my measure, only with a different population (the general population, not exit poll voters). They are somewhat lower (.7 to .8) for imputations of policy mood, which is conceptually distinct from but empirically closely related to symbolic ideology. Finally, the correlations are lowest (.5 to .7) with

election returns, which are at a great conceptual remove from ideology. All of this comports with what we would expect from a valid measurement.

Concept reliability refers to the replicability of a measurement over repeated samples. I must establish reliability before I can say with confidence that my measures *consistently* capture district attitudes. Measuring the reliability of aggregate measurements poses a special difficulty, as statistics of individual-level consistency such as Cronbach’s Alpha are inappropriate (Jones and Norrander 1996, 296). An estimate of reliability must account for both variation between different individuals nested in each district, as well as that of aggregate means between different districts. O’Brien (1990) conceptualizes aggregate reliability as the ratio of universe score variance—the estimated mean of several aggregate samples of ideology—to the expected observed score variance—which includes ideological variation both between and among groups. Where x is ideology, i is district, and j is individual survey respondent, I can mathematically express aggregate reliability for each year as:

$$E\rho^2 = \frac{\sum_i(x_i^2 - \bar{x})^2 - \sum_i \sum_j(x_{ij}^2 - \bar{x}_i)^2}{\sum_i(x_i^2 - \bar{x})^2}$$

In the parlance of ANOVA, or analysis of variance, I divide the difference between the *between sum of squares* and the *within sum of squares* by the *between sum of squares*. Table 3.8 displays the yearly reliability of my measures. Reliabilities for each year are very robust ($E\rho^2 \geq 0.7$). The high coefficients suggest that individual responses within districts are relatively homogenous, and that differences *between* districts perhaps largely explains the phenomenon of ideological polarization. As MRP works best when there is less response variability in districts, this is cause for still greater confidence in my measures.

Table 3.8: Reliability
Coefficients, Exit Poll
MRP Measures*

Year	Coefficient
2004	0.89
2006	0.88
2008	0.89
2010	0.87

* See O'Brien (1990) for a discussion of the relevant ANOVA-based formula for deriving these aggregate measures.

A Methodological Advance

Lack of constituency data has plagued House responsiveness literature since the 1960s. Only recently, with advent of MRP, have political scientists had direct, district-level measurements of public opinion at their disposal. The MRP Revolution, I have argued, has nonetheless been incomplete. Exit Polls expand our knowledge of district-level ideology because they overcome two limitations of previous research: its inability to precisely model voter opinion, and its implausible assumption that ideology is stable in the aggregate. I have produced valid, reliable longitudinal estimates of voting constituency ideology at the level of the House district from 2004 to 2010. This is in contrast to other MRP estimates that are pooled over several years, which likely smooth over short-term variations public opinion that produce dramatic political change (Erikson, Wright, and McIver 2006). Moreover, longitudinal estimates potentially allow us to track whether changes in constituent attitudes produce changes in representative behavior, as in a first-difference model. The time period that my measures cover is apt for such an endeavor, as the geographic boundaries of each jurisdiction are constant during the years my dataset includes.

Imputed exit poll measures constitute a methodological advance. The following chapters will prove whether or not they can provide Congress scholarship with *substantive* advances as

well. In other words, do they help us answer important questions in American politics? I will address two: the question of whether or not constituent attitudes are stable, and whether or not they influence representative floor voting.

Chapter 4: District Ideology from a Temporal Perspective

Do Constituencies Change Their Minds?

The main weakness of extant MRP literature is its reliance on district-level measures that are collapsed over many years. The main strength of the exit poll approach is that it furnishes enough cases to produce measures by election cycle, thus accounting for instability in sub-national public opinion. So far, however, the discussion of instability has remained purely in the realm of concept. Exit poll measures would lose some of their luster if district ideology did not vary between redistricting cycles. This chapter, I hope, will show that my methodological efforts are not wasted. Even in the short span of six years, districts underwent significant change to which static measures of public opinion are deaf.

In this chapter, I, first, discuss the small corpus of literature relevant to district-level ideological change. This literature provides few definitive answers as to whether public opinion is unstable at the state level, let alone whether it is so at the district level. Second, I discuss the stability of my MRP measure. Descriptive statistics applied to these measures, I find, would incline reasonable observers to believe that there is great temporal variation in district ideology. Moreover, strictly controlling for cross-sectional and national trends, 110 districts show linear ideological change. Finally, third, I attempt to explain ideological change. Majority-minority status, as well as population growth, economic stress, and demographic changes help explain district-level change.

Theoretical Expectations

What should one expect about changes in district-level ideology? The literature gives scarcely any clues. Old responsiveness literature that looked at more than one Congress still relied mostly on static measures. For instance, Glazer and Robbins (1985) used the natural experiment of decennial

redistricting to assess the whether Congresspeople, when they found their constituency suddenly changed, suffered reversals in their electoral fortunes. Fleisher and Bond (2004) created a longitudinal measure of district ideology by standardizing district presidential vote relative to the national vote (ostensibly because an unstandardized measure is more susceptible to short-term political factors), and found a growing correlation between aggregate district ideology and House vote, supporting what Jacobson (2013) found at the individual level. While they can tell us that (say) a conservative representative will have an increasingly harder time getting elected in a liberal district, the authors do not address the question of whether liberal districts are getting more liberal. Even more sophisticated is Levendusky, Pope, and Jackman's (2008) use of a latent factor analysis of vote shares to generate partisanship scores in every Congressional district. However, because latent factors other than ideology drive voting, and because their latent estimate is constant by decade, they consciously resist the temptation to measure district-level changes in public opinion.

The paucity of studies tracking public opinion in the district behooves us to look elsewhere for our expectations. We might look at the Congress literature, whose boilerplate thesis is that the policy attitudes of members, when they enter Congress, are static (Asher and Weisberg 1978; Stone 1980; Poole and Rosenthal 1997; Poole 2007). Even the roll call behavior in post-war Congress, noted for passing the first Civil Rights legislation since Reconstruction and widely perceived as undergoing revolutionary change, was remarkably stable. If any change does occur, it seems, it is due to member replacement, thus implicating the changing minds of constituents—not those of Congressmen—in the process (Therriault 2006). We therefore have a sketchy and *prima facie* reason to suspect ideological change in the districts.

More might also profitably interrogate the literature on state public opinion. As I discussed in Chapter 2, scholarship is divided as to whether state ideology is stable. The dominant thesis is

that symbolic ideology in the past half-century is remarkably stable, and with a handful of exceptions varies only nationally (Erikson Wright and McIver 1993; 2001; 2006; Brace et al. 2004; Pacheco 2014). That is, the proportion of liberals to conservatives in Maine may have increased in (say) 2008, but only because it increased everywhere. Meaningful change occurs, these authors tell us, only when the rank ordering of states by ideology shifts, or when the most liberal and conservative states diverge. The stability thesis thus casts state ideology as a static “thermostat” whose role in influencing policy outcomes is passive, as when it corrects state policy that becomes too liberal or too conservative (Wleizen 1995). Researchers at Florida State University (Berry et al. 2007) counter that, by relying on alternative measures of ideology derived from the interest group scores of legislative candidates and by relaxing the assumption that all change must be wholly independent of national trends, ideological variation is more temporal (between time points) than cross-sectional (between states). Indeed, they estimate that three-fourths of states experience significant change in years that Erikson, Wright, and McIver cover. The Florida State school’s findings are consonant with the policy mood literature, which suggests that changes in state opinion is cyclical and reacts to recurring policies as well as the party of the president (Stimson 1999), and other studies which show that party identification, issue positions, and evaluations of the president exhibit substantial sub-national dynamism (Erikson, Wright and McIver 2006; Pacheco 2014).

The exit polls shed some light as to whether state ideology moves. Table 4.1 shows the average election-to-election change in state-level MRP measures of symbolic ideology from 1982 to 2010. The imputation process for states is more or less identical for that of the district-level MRP, differing mainly by using the Current Population Survey (CPS) instead of the American Community Survey as post-stratification data. Applying Brace et al.’s (2004, 533) stringent test of

stability—where state ideology for each state and year is regressed on dummies for each state but one and each year but one; if the model’s error terms are significantly related to time, then there is change that is independent from national trends and from cross-sectional variation between states—we find that trends in 21 states are significant. Figure 4.1 maps ideological change, casting into sharp relief that ideological change is a regional phenomenon. States with unambiguously liberal trends in public opinion are concentrated on the West Coast and to a lesser extent the Northeastern and Mid-Atlantic states. The Southeast, Florida excepted, grew more conservative. In sum, imputed exit poll measures of state ideology exhibit more movement than do measures derived from the disaggregation of commercial polls, as well as measures derived from the American National Election Study (ANES) and General Social Survey (GSS). There is prima facie evidence of regional divergences, lending mild support to Abramowitz’s (2010a) and Jacobson’s (2013) thesis that public opinion—not merely “partisan sorting,” or the viciousness of American political leaders and intellectuals (Fiorina et al. 2011)—drives polarization. We might attribute ideological instability in the exit polls to the fact that, from election to election, the political profile of electorates varies to a far greater extent than does that of all voting-age citizens.

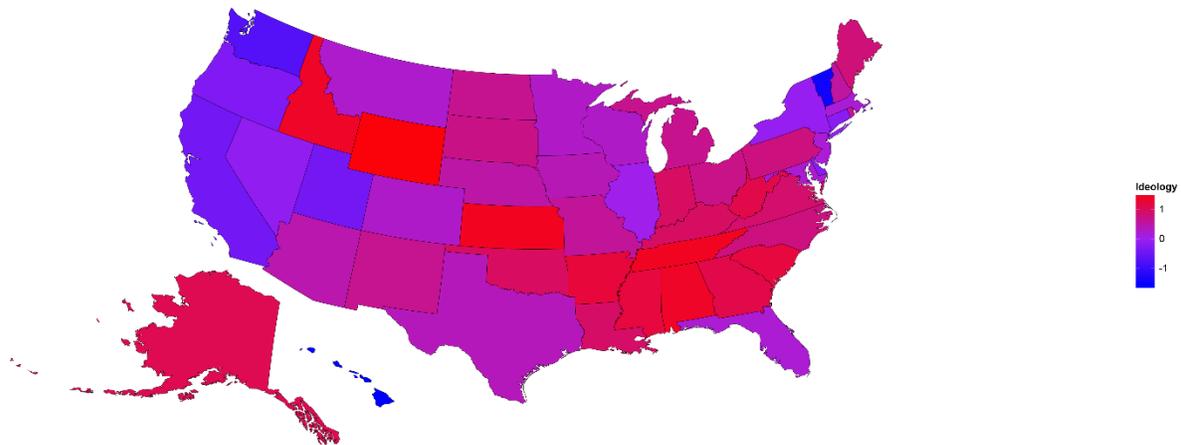
Nonetheless, the measures do not permit us to say, as Berry et al.’s Florida State contingent does, that most states are characterized by large swings in ideology since 1960; only a handful of states, too, change at a remarkable rate, and there seems to be no case of a state’s rank order traversing from one extreme to another. The exit polls agree with Brace et al.’s contention that the Berry et al. measure “is capturing something different from mass ideology” (2004, 537).

Table 4.1. State-level ideological change in the exit polls, 1982 - 2010.

State	Avg. Election-to-Election Change	<i>p</i>	State	Avg. Election-to-Election Change	<i>p</i>
Alabama	0.006	0.023*	Montana	-0.001	0.837
Alaska	-0.004	0.561	Nebraska	0.008	0.450
Arizona	0.003	0.571	Nevada	-0.005	0.070
Arkansas	0.012	0.017*	New Hampshire	0.004	0.572
California	-0.010	0.003**	New Jersey	-0.008	0.055
Colorado	-0.008	0.933	New Mexico	-0.005	0.162
Connecticut	-0.003	0.074	New York	-0.009	0.023*
Delaware	-0.003	0.111	North Carolina	0.002	0.771
DC	-0.021	0.188	North Dakota	0.009	0.060
Florida	-0.004	0.091	Ohio	0.001	0.560
Georgia	0.007	0.006**	Oklahoma	0.005	0.115
Hawaii	-0.022	0.025*	Oregon	-0.005	0.049*
Idaho	0.012	0.049*	Pennsylvania	0.003	0.921
Illinois	-0.009	0.186	Rhode Island	0.000	0.101
Indiana	0.006	0.017*	South Carolina	0.009	0.088
Iowa	0.005	0.120	South Dakota	0.013	0.061
Kansas	0.017	0.004**	Tennessee	0.001	0.016*
Kentucky	0.005	0.186	Texas†	0.002	0.048*
Louisiana	0.011	0.053*	Utah	-0.002	0.235
Maine	0.009	0.113	Vermont	-0.015	0.004**
Maryland	-0.012	0.100	Virginia	0.002	0.953
Massachusetts	-0.003	0.016*	Washington††	-0.010	0.008*
Michigan	-0.003	0.792	West Virginia	0.005	0.224
Minnesota	-0.002	0.081	Wisconsin	-0.005	0.721
Mississippi	0.014	0.460	Wyoming	0.011	0.085
Missouri	0.002	0.112			

Table excludes 2002. H_0 = state exhibits no linear change independently of national public opinion trends.
 * $p \leq 0.05$; ** $p \leq 0.01$
 † Curvilinear change
 †† Cyclical change

Figure 4.1. Avg. Change in Ideology (1982-2010), National



Nonetheless, caution is advisable when interpreting the above. Exit poll coverage of states in the 1980s was spotty, and the 1992 and 1994 state exit polls inexplicably omitted an ideology item on their questionnaires in many states. Cases from the national exit poll—which codes for state beginning in 1982, but which lacks representative state sample—were imported and used to impute measures for most states during this period. The measures for 1982 are especially problematic, since the national exit poll did not yet publish the states of respondents (meaning that measures in 25 states are purely a function of regional and demographic predictors, and lack a random slope for each state), and because the MRP model lacks an important aggregate fixed effect (percentage of the state electorate in a labor union) due to its omission in the CPS. Even so, the fact that the early CBS News and Voter News Service exit polls are post-stratified gives us more reason to trust these numbers than we have to trust the measures of ideological change in Brace et al.’s meta-analysis of state ideology. When my state measures are adjusted to exclude self-identified moderates, their product-moment correlation with Pacheco’s (2011) measures of state

liberalism using commercial polls from 1982 to 2006 is 0.67, suggesting some face validity. Because the purpose of the state-level MRP is merely to provide expectations about district-level change, I will not devote further discussion to the generation, reliability, and validity of my state ideology measures.

If decades of previous research cannot establish whether state public opinion is stable, woe unto anyone who asks the same question of districts. Suppose that we are agnostic on the debate between Brace et al. and Berry et al.; the exit poll data, after all, suggests detectable but hardly dramatic longitudinal changes in aggregate public opinion. What, then, might our theoretical expectations of district-level change be? In all likelihood, since district-to-district migration flows are logically greater than state-to-state migration flows (the 2014 American Community survey suggests that 15% of households move each year, but only 25% of these moves are out-of-state), districts will exhibit greater demographic change than states. This residential mobility could produce ideology change in the aggregate (Tiebout 1956). Does this mean that Congressional districts will grow more homogenous, as when people with similar traits and attitudes cluster together (Bishop 2008)? Might transplants, who initially differ ideologically from their new neighbors, gradually conform to their new milieu (Brown 1981, 1989), or might their early social learning prove strong enough to resist this change in their core values (Finifter and Finifter 1989)? Good questions all, but no answers abound. Against the importance of demographics, Brace et al. (2005) identify only three states—California, Oregon, and West Virginia—that have undergone substantial and independent ideological change since the mid-seventies, and only the first of these has experienced any notable change in population characteristics. Warshaw and Rodden (2009) are among the few to address the homogeneity question at the level of House districts. They contend that neither residential sorting nor gerrymandering is a factor in ideological polarization in the

United States, identifying wide intra-district heterogeneity in public opinion on seven issue domains. If right, their findings would militate against the role of demography in district-level attitudes. Economic factors might also affect subnational opinion, especially in our federalist system where state economies are more independent of national trends than in the past (Brace 1993). The economic upheaval of 2008 could plausibly have produced immediate effects on district-level public opinion.

Confounding the utility of previous scholarship even further is the fact that they relied on general public opinion measures, not those of voters. The changes I document at the state-level convince me that the voter-derived character of my measures is critical. The systematic differences between midterm and presidential electorates, as well as the fact that measures of elite public opinion such as Berry et. al's scores seem more labile than measures disaggregated from public opinion polls, give me cause to think that districts will show marked instability between 2004 and 2010. Voters, after all, fall somewhere in between "general population" and "policy elites."

An Empirical Model of Change

I first ask: are districts more different from district to district, or are they more different from year to year? To address this question, I ran a one-way analysis of variance (ANOVA) with a repeated measures design using aggregate district-level ideology from 2004 to 2010. Table 4.2 reports the results, as well as the results for district-level partisanship, and for state-level ideology and partisanship. Temporal variability equates to the mean sum of squares for between-groups (MS_b), whereas cross-sectional variability between districts equates to the mean sum of squares for within groups (MS_a). To give a picture of how much these districts move independently of national trends, I correlated each district measure with every other district measure in the election; for the entire

timespan, this meant correlating 284,490 pairs. The “average inter-item correlation of changes in public opinion” column reports the result. A value of 1 indicates that there is no sub-national heterogeneity in how public opinion moves; a value of 0, on the other hand, indicates that district-level change is random and that the ideological measure is totally unreliable.

Table 4.2. Analysis of Variance, Sub-National Public Opinion Across Time

	Mean	Mean election-to-election change	Variance	Standard deviation	% of variance explained	Avg. inter-item correlation of changes in public opinion
CD ideology						
2004 - 2010 (<i>n</i> = 1,744)						
Overall	0.110	0.014	0.028	0.167		0.168
Across districts			0.098	0.312	20%	
Across time			0.396	0.630	80%	
CD partisanship						
2004 - 2010 (<i>n</i> = 1,744)						
Overall	-0.081	-0.010	0.048	0.219		0.159
Across districts			0.171	0.414	24%	
Across time			0.540	0.735	76%	
State ideology						
1982 - 2010 (<i>n</i> = 765)						
Overall	0.145	0.005	0.013	0.114		0.400
Across states			0.128	0.358	61%	
Across time			0.083	0.288	39%	
State partisanship						
1982 - 2010 (<i>n</i> = 765)						
Overall	0.064	0.008	0.021	0.146		0.222
Across states			0.198	0.445	71%	
Across time			0.081	0.285	29%	

The results indicate that district ideological variation in the years 2004 to 2010 is more temporal than cross-sectional. Historically, time explains a much smaller proportion of variation in states. Moreover, while the tendency in districts was towards greater conservatism (partly because of the 2010 wave election, which featured the most conservative electorate in the exit poll era), ideological change from election to election is quite small. On average, the proportion of conservatives to liberals in a district increases between elections by 0.01%. The biennial changes indicate that short-term and nationwide election dynamics are at work: in 2006, the average district electorate had 7.4% more liberals than in the previous election; in 2008, it had 3.24% more liberals;

and in 2010 it had 10.95% fewer liberals. Nationally, ideology does not change as much as it seems to regress to the mean. However, the comparatively low correlation of district-level changes with changes in every other district tells us that changes in district electorate are heterogeneous, with public opinion moving in different directions and at different rates.

Pacheco (2014) divides subnational change into four types: first, gradual, homogenous change; second, rapid, homogenous change; third, gradual, heterogeneous change; and fourth, rapid, homogenous change. Rapid change (types 2 and 4) is most likely a response to a sudden political or economic stimulus (Erikson et al, 2002)—as when the equilibrium of public policy is “punctuated” (Baumgartner and Jones 1993)—whereas gradual change (types 1 and 3) are most likely the result of subtler demographic shifts. My exit poll measures do not give us much leverage in addressing the rapidity-to-gradualness axis, given our limited timespan and the fact that the substantial district-level change from election to election (in absolute terms, the proportion of liberals to conservatives at an average of about 10% between elections) is consistent with the fact that exit polls interview wholly different populations from election to election. (Chapter 2 discusses the comparative wealth, whiteness, education, and conservatism of midterm electorates versus presidential ones). With much greater confidence can we say that the changes in district ideology are heterogeneous, which means that local jurisdictions are either diverging demographically (for instance, Democratic-leaning minorities relocating from one district to another), or responding differently to national political stimuli. Table 4.3 shows more evidence of heterogeneity. Nationally, the electorate grows more liberal from 2004 to 2006, more liberal from 2006 to 2008, and more conservative from 2008 to 2010; fewer than 20% of congressional districts follow the national trend, and there is a relatively wide distribution of districts in all eight of the possible directional trends. Comparatively few jurisdictions were more liberal in the 2010 election

than in the 2008 election. The heterogeneity is surprising given that our time series features three wave elections, or elections where strong national political currents alter the balance of power in Congress (2006, 2008, and 2010). In 2006, discontent with the Iraq War allowed the Democrats to gain 31 seats in the House of Representatives, and in 2008 an economic near-collapse allowed them to gain still 20 more as well as the White House (Jacobson 2010). 2010, on the other hand, featured political conditions nearly identical to 1994—an unpopular first term Democratic president, and a conservative grassroots insurgency mobilized by the healthcare reform debate (Abramowitz 2010b)—and a nearly identical result: the total erasure of Democratic gains in Congress. The presence of the three out of four wave elections since 1974 in our dataset should nationalize district-level ideological change; it does not.

Table 4.3. Trends in disrict-level ideology, 2004 - 2010		
Change type	count	%
Monotonic change		
monotonic more liberal	17	3.90%
monotonic more conservative	32	7.34%
Subtotal	49	11.24%
Oscillatory change		
more liberal in 2006, more liberal in 2008, more conservative in 2010*	87	19.95%
more liberal in 2006, more conservative in 2008, more conservative in 2010	114	26.15%
more liberal in 2006, more conservative in 2008, more liberal in 2010	39	8.94%
more conservative in 2006, more conservative in 2008, more liberal in 2010	7	1.61%
more conservative in 2006, more liberal in 2008, more liberal in 2010	25	5.73%
more conservative in 2006, more liberal in 2008, more conservative in 2010	115	26.38%
Subtotal	387	88.76%
Total	436	100.00%

*Follows national trend, per national exit polls

Which districts appear to be moving? We might (say) ask if districts are polarizing between redistricting cycles, in which case dispersion would increase from year to year. Alternatively, it could be the case that redistricting “packs” partisans in electorally uncompetitive districts, but that migration blunts the ideological extremity of these districts and that variance decreases by the end

of the cycle. Figure 4.2 shows the densities of our ideology measure by election. We find that in every year the distribution of districts has a profound left skew—that is, the most liberal districts are several more standard deviations away from the average district than are the most conservative ones. While we might take this as evidence of recent polarization, this skew appears in Democratic voting in each of the last six decades, and was perhaps even more acute historically (Levendusky, Pope, and Jackman 2008). As Peress (2013) and Warshaw and Rodden (2009) find, and as my plots show, the distribution of district ideology is not bimodal. Redistricting, rather than geographic clustering or the growing fractiousness of American politics, is the likely explanation for the left skew. An irresistible suspicion is that the liberal outliers are majority-minority jurisdictions, and this is indeed the case: whites are a minority in New York 8 (Manhattan), New York 15 (Harlem), Washington D.C., California 8 (Oakland), and New York 16 (South Bronx). Interestingly, while the central tendency of aggregate district ideology moves with the electoral context, the most liberal districts exhibit almost no movement and leave the left skew unaltered. The largely minority-composed districts, then, are the ones that exhibit the most independent trends. Table 4.4 breaks down my ANOVA analysis by quartile, finding that the most liberal districts in 2004 largely resist the slight national trend towards conservatism. A near-zero correlation between ideological change in these districts and ideological change in all districts, as well as their primarily cross-sectional variance, starkly differentiate these districts from the rest.

Figure 4.2: Density of District Ideology by Election

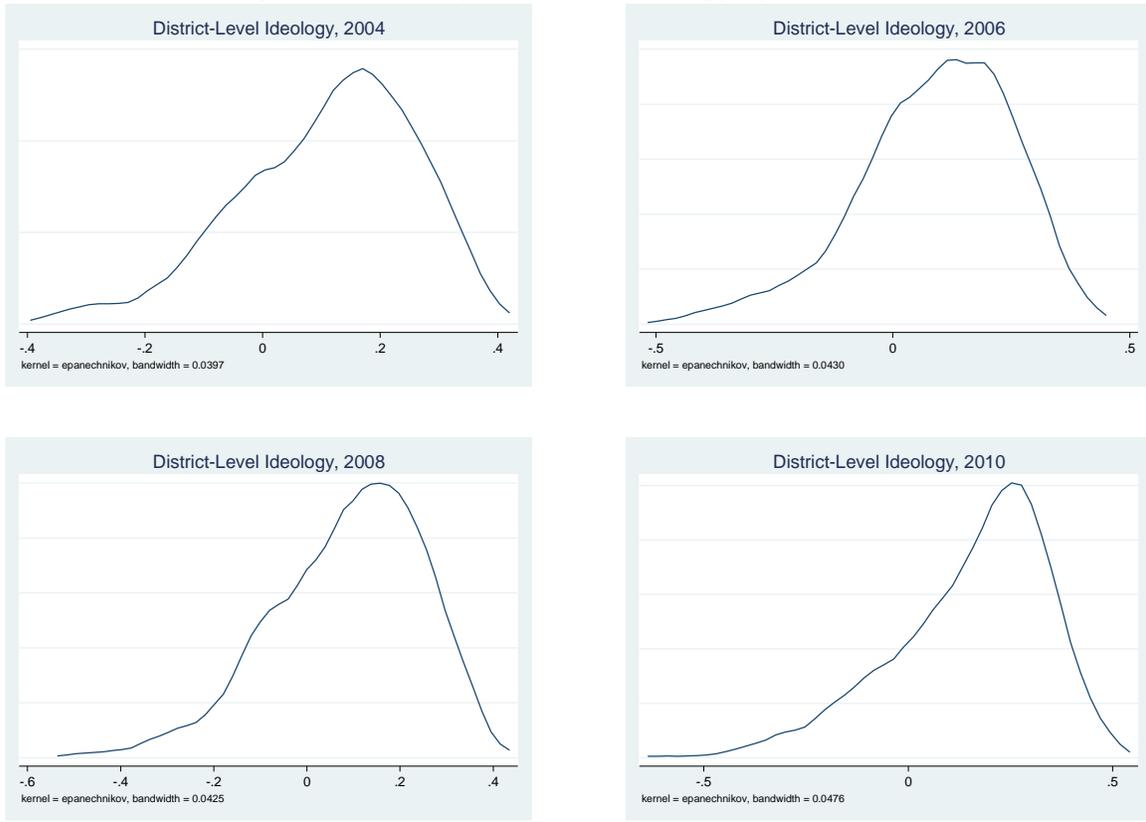


Table 4.4. Analysis of Variance, Sub-National Public Opinion Across Time						
	Mean	Mean election- to-election change	Variance	Standard deviation	% of variance explained	Avg. inter-item correlation of changes in public opinion
Most liberal to 109th most liberal district in 2004 2004 - 2010 (<i>n</i> = 436)						
Overall	-0.102	0.003	0.015	0.123		0.058
Across districts			0.043	0.208	58%	
Across time			0.022	0.149	42%	
110th liberal to 218th most liberal districts in 2004 2004 - 2010 (<i>n</i> = 436)						
Overall	0.081	0.018	0.007	0.082		0.178
Across districts			0.012	0.110	24%	
Across time			0.128	0.357	76%	
219th liberal to 327th most liberal districts in 2004 2004 - 2010 (<i>n</i> = 436)						
Overall	0.184	0.023	0.006	0.076		0.254
Across districts			0.009	0.096	18%	
Across time			0.191	0.437	82%	
328th to least liberal district in 2004 2004 - 2010 (<i>n</i> = 436)						
Overall	0.276	0.013	0.005	0.072		0.242
Across districts			0.010	0.101	23%	
Across time			0.114	0.337	77%	

Moving away from descriptive statistics, we need an empirical test that, for each district, distinguishes real change from noise. In my discussion of states, I alluded to Brace et al.'s significance test, whereby they employ a two-way fixed effects model controlling for inter-state differences as well as nationwide longitudinal variances. If a state's residuals are correlated with time, then there is significant state-level ideological change. Might one apply this test to district measures? Unquestionably, this is a conservative test that hardly appears in every study of sub-national public opinion trends. For one thing, it cancels out national trends, when in fact such trends—when reflected at the level of jurisdiction—might have important policy consequences, even if the rank ordering of districts is unchanged (Berry et al. 2007, 116). For another, a model with aggregate variables in a limited time span necessarily lacks the degrees of freedom of an

individual-level model, making it harder for any predictor to significantly explain variance. If the yearly estimates of ideology are even modestly reliable, the test is predisposed to type II error, or the false conclusion that there is stability at the jurisdictional level (118). However, the high temporal variance of my measures—which, I believe, owes to the fact that voter profiles differ from midterm and presidential electorates, as well as the unusual abundance of wave elections in the late 2000s—inclines me to minimize type I error and choose the most stringent possible test. If there is change, then this test may not detect it; but if the test detects change, then there likely really is change.

To model ideological change, I first regress ideology on district fixed effects and election year fixed effects:

$$P_{ij} = \beta_0 + \sum_{i=1}^n \beta_i D_i + \sum_{j=1}^n \beta_j EY_j + \varepsilon_{ij}$$

where:

- P_{ij} = district ideology for each district i and each election year j
- D_i = dummy variables for each district i , sans Wyoming's at-large district
- EY_j = dummy variable for each election year j , sans 2010
- ε_{ij} = error term for each state i and election year j

If district-level ideology is stable, then the error term should be randomly distributed within each district. If the error term has a systematic longitudinal component, then temporal variation in the district's ideology is more than noise.

How do we identify this systematic component? Brace et al. propose using the Lagrange Multiplier Test, more commonly known as a score test. The test quantifies the expected improvement of fit when an excluded predictor is added to a “restricted” model, which lacks the predictor. For each district, we therefore first regress the error term on nothing but the intercept:

$$\varepsilon_j = \beta_0 + v_j$$

where:

ε_j = error term from the fixed-effects model for each year j within each district
 v_j = remaining variation in error

The unrestricted model includes an interval-level time series variable:

$$\varepsilon_j = \beta_0 + \beta_1 T + v_j$$

where:

ε_j = error term from the fixed-effects model for each year j within each district
 T = the time series [1 = 2004, 2 = 2006, 3 = 2008, 4 = 2010]
 v_j = remaining variation in error

If the test statistic (“score”) of the improvement of fit is significant for a district, then its variation is a linear function of time. Brace et al. also test whether nonlinear relationship exists between time and district-level ideology with quadratic equations of the second degree (testing for curvilinear change) and third degree (testing for cyclical change). Our data, unfortunately, lacks the time points necessary to model ideology as square or cubic functions of time.

Table 4.5 shows the rank order, magnitude of change, and a description of districts that exhibit independent change at 90% confidence. The relative paucity of degrees of freedom in the fixed effects model, which predicts a mere 1,744 cases with 438 factors, makes it hard for T to explain variance unexplained by the many district and election year dummies. Only 14 out of 435 districts¹ show change from 2004 to 2010 at 95% confidence, but this is hardly strange; only a small minority of states using measures that span *generations* show such significant longitudinal trends in the contemplation of Brace et al. When α is increased to 0.10—a reasonable response to

¹ Georgia 9th is significant at 95%, but is excluded from this tally for reasons discussed below.

the limitations of the exit poll time series—then we find that 111 out of 435 districts exhibit independent change.

Table 4.5. Districts Exhibiting Independent Ideological Change				
District	Rank (1 = most liberal)	<i>p</i>	Avg. % change in ratio of liberals to conservatives	Notes
Massachusetts 8	1	0.10*	0.29%	"People's Republic of Cambridge." High % graduate-educated
New York 8	2	0.06*	-0.79%	West side of Manhattan. Mostly white
California 9	6	0.05*	1.40%	Eastern San Francisco Bay Area
California 8	8	0.09*	0.40%	San Francisco
New York 11	12	0.07*	5.33%	Brooklyn. Majority black
Massachusetts 7	17	0.06*	-6.10%	Eastern Massachusetts. Ed Markey's old district
New York 18	18	0.06*	2.73%	Westchester County. Affluent
Illinois 4	23	0.05*	-1.28%	Affluent Chicago neighborhoods. Notoriously non-contiguous
New York 9	25	0.06*	-0.05%	White ethnic neighborhoods in Southern Brooklyn and Queens
Illinois 7	26	0.06*	-0.73%	Majority black Chicago neighborhoods
Virginia 8	29	0.07*	-5.19%	Arlington, Alexandria, and surrounding areas. Quite Democratic
California 36	33	0.06*	-2.25%	Riverside County
Maryland 7	37	0.05* *	3.80%	Most of Baltimore. Historically a racial gerrymander
Minnesota 4	40	0.05* *	2.76%	St. Paul and surrounding areas
New Jersey 12	45	0.08*	-8.00%	Central New Jersey. Increasingly trends Democratic
California 13	47	0.10*	8.42%	Oakland. Majority-minority
Maryland 4	49	0.09*	5.21%	Democratic, majority-minority gerrymander
Pennsylvania 1	53	0.09*	5.70%	Central and South Philadelphia. Majority minority
New York 6	54	0.06*	0.65%	Queens. Majority black
Massachusetts 1	55	0.05*	-13.69%	Rural Western Massachusetts
Connecticut 3	57	0.08*	2.09%	Central Connecticut. Includes New Haven
California 35	65	0.08*	5.64%	Inglewood. Majority-minority
Rhode Island 2	67	0.05* *	-5.83%	Southwestern Rhode Island
Connecticut 2	71	0.08*	-7.11%	Eastern Connecticut

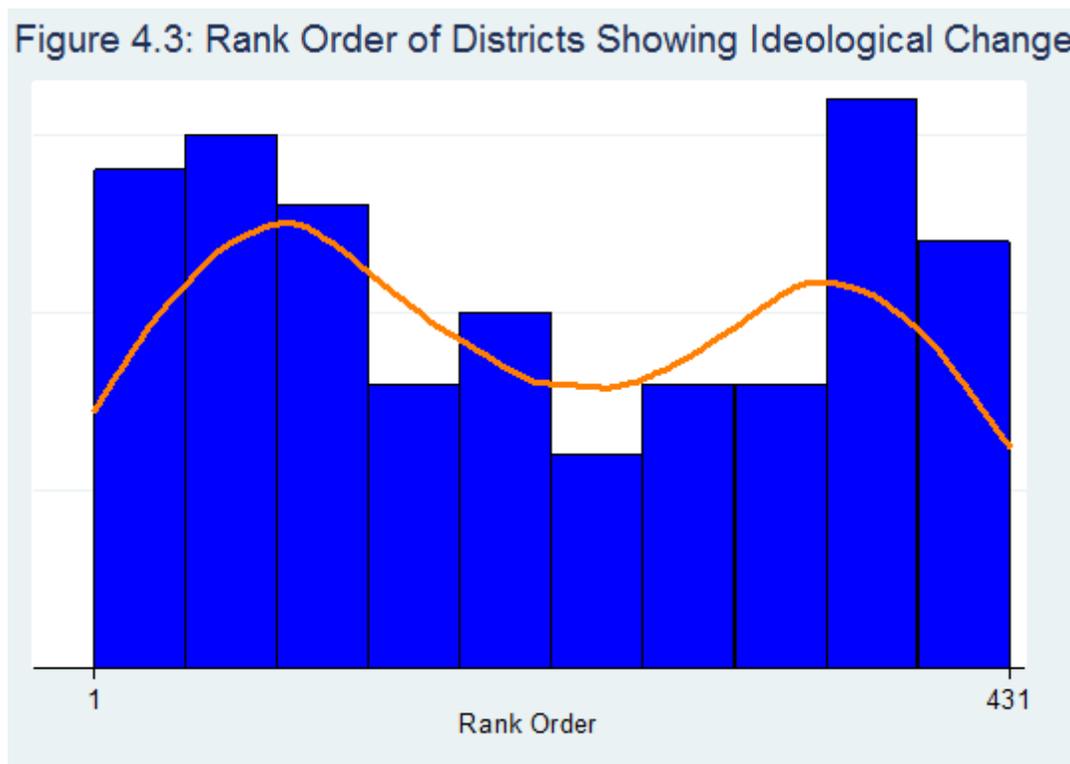
Florida 18	72	0.06*	4.94%	Most of Miami. Heavily Latino
California 32	75	0.07*	2.41%	Eastern Los Angeles County. Heavily Latino
Florida 21	81	0.07*	-2.30%	Central Miami-Dade County. Leaned Republican
New Jersey 8	83	0.09*	-7.39%	Jersey City, Hoboken, Elizabeth, East Newark. Heavily Latino
New York 19	85	0.05*	-5.45%	Putnam County
Georgia 5	92	0.07*	5.72%	Atlanta. Includes heavily black Dekalb suburbs
New Jersey 7	95	0.06*	-7.15%	Northern New Jersey. Competitive House races
Maine 1	96	0.10*	-10.79%	Includes Portland, Augusta, and all of York County
Connecticut 5	100	0.05* *	-6.10%	Waterbury and surrounding areas
Rhode Island 1	102	0.05* *	-7.72%	Providence, Newport, and Bristol. Growing Latino population
California 50	104	0.09*	-1.87%	San Diego suburbs. Primarily white
Louisiana 2	105	0.10*	6.37%	New Orleans. Majority black
California 38	108	0.10*	-0.20%	Los Angeles. Gerrymandered to have Latino majority
California 39	117	0.10*	0.55%	Southeastern Los Angeles. Heavily Latino
Missouri 5	121	0.10*	5.18%	Kansas City metro area
Oregon 1	124	0.08*	6.01%	Includes downtown Portland
Minnesota 3	125	0.06*	-10.45%	Mixed-income suburbs of Minneapolis
North Carolina 4	127	0.09*	-1.34%	Chapel Hill, Durham and surrounding areas. Democratic stronghold
New York 25	140	0.06*	-13.64%	Syracuse and Rochester suburbs. Electorally competitive
New Mexico 3	147	0.06*	-3.88%	Northern Half of New Mexico. 1/3rd Latino
Illinois 13	148	0.07*	-7.73%	Chicago suburbs. Solidly Republican, but did vote for Obama in 2008
Ohio 15	149	0.07*	-16.64%	Southern Columbus and Union and Madison counties
Tennessee 9	150	0.05* *	12.21%	Memphis. Majority black
California 40	151	0.07*	-3.40%	Orange County. Primarily white
California 46	154	0.05* *	3.03%	Orange County, Port of Los Angeles, Long Beach
Ohio 12	165	0.06*	-4.68%	Black neighborhoods of Columbus
Washington 9	173	0.06*	8.13%	Puget Sound Area
Florida 23	176	0.05* *	1.62%	Broward and Palm Beach Counties. Liberal retirees
Washington 3	179	0.05*	5.09%	Southwest central Washington. Swing district in presidential elections
Maryland 2	180	0.07*	5.48%	Counties surrounding Baltimore, and parts of East Side Baltimore
Nevada 3	183	0.10*	-5.63%	Clark County South of Las Vegas. Primarily white
Ohio 9	184	0.05*	-10.97%	Northern border with Lake Eerie. Democratic gerrymander.
Ohio 13	192	0.07*	-15.31%	Lorain, Akron suburbs
California 52	195	0.06*	-11.31%	San Diego suburbs. Primarily white
Washington 4	214	0.06*	1.76%	Rural areas surrounding Yakima. Leans Republican

New York 26	215	0.05*	-17.01%	Buffalo and Niagara Falls
Ohio 17	233	0.07*	-9.77%	Northeast Ohio
California 25	238	0.07*	1.93%	Northern Los Angeles County. Relatively conservative
Tennessee 5	243	0.10*	7.45%	Nashville. Democratic stronghold
New Mexico 2	248	0.06*	-5.95%	Southern New Mexico. 1/3rd Latino
Ohio 16	249	0.05*	-4.41%	Area surrounding Canton
Florida 13	254	0.07*	-3.51%	Sarasota and counties further south
Alabama 7	268	0.09*	5.82%	Majority-minority (includes Birmingham)
California 19	269	0.05*	-10.81%	San Jose
Florida 14	271	0.07*	-10.11%	Southwestern Florida, including Naples
Michigan 7	275	0.05* *	-10.87%	Southwestern Michigan
Ohio 18	284	0.09*	-20.70%	East central Ohio. Swing district
West Virginia 1	296	0.05* *	-10.01%	Northern portion of the state. Solidly Republican in pres. Elections
Pennsylvania 17	299	0.05*	-9.36%	Harrisburg and surrounding areas
Georgia 13	301	0.07*	10.67%	Greater Atlanta area. Majority black
Ohio 5	309	0.06*	-11.85%	Bowling Green and northwestern Ohio
Ohio 8	311	0.07*	-9.30%	Western Ohio. John Boehner's district
Florida 15	312	0.09*	2.28%	Central eastern coast, including Melbourne
Michigan 2	315	0.07*	-7.08%	Western Michigan
Indiana 9	319	0.06*	10.37%	Southeastern Indiana
Texas 29	322	0.08*	6.33%	Eastern Houston. Majority Latino
Alaska 1	326	0.05*	-39.47%	At-large district
Missouri 9	333	0.09*	9.00%	Rural northeastern Missouri and Columbia
South Carolina 2	345	0.10*	-7.08%	Columbia and southern portions of the state
North Carolina 8	347	0.09*	7.58%	Southern North Carolina
North Carolina 2	348	0.07*	10.49%	East Central North Carolina. Blacks gerrymandered out of district
Illinois 19	349	0.07*	-5.13%	Rural southern Illinois
Ohio 4	350	0.05*	-13.10%	West central Ohio
Louisiana 6	351	0.05* *	3.67%	Baton Rouge. District lost black voters after 2010 redistricting
Kentucky 1	353	0.08*	-11.50%	Western Kentucky. Most voters are conservative Democrats
Texas 24	354	0.07*	-3.41%	Dallas and Fort Worth suburbs
Texas 3	362	0.09*	1.88%	Northeast Dallas suburbs
North Carolina 3	368	0.09*	0.61%	Atlantic coast and Outer Banks. 22% black
North Carolina 6	374	0.06*	10.45%	Central North Carolina
North Carolina 10	375	0.06*	7.77%	West Central North Carolina. Heavily Republican

Wyoming 1	377	0.07*	-17.53%	At-large district
Tennessee 7	378	0.10*	5.57%	Western Tennessee and Nashville suburbs
Kansas 1	379	0.05*	-13.59%	Massive rural section of Kansas. Highly conservative
Oklahoma 1	387	0.05*	-20.23%	North Central Oklahoma. Solidly Republican
Arkansas 1	388	0.05* *	-13.96%	Mississippi Delta. Increasingly votes Republican in pres. races
Iowa 5	389	0.06*	-10.38%	Western Iowa. Solidly Republican
Louisiana 3	405	0.06*	-12.05%	Eastern Louisiana Gulf Coast
South Carolina 3	407	0.09*	-23.60%	Rural northwestern South Carolina. Very conservative
Texas 2	410	0.06*	-2.71%	Houston Suburbs and Southeast Texas. 2002 Republican gerrymander
Alabama 5	412	0.05* *	-8.08%	Northern Alabama
Texas 14	414	0.05* *	-4.46%	Greater Houston area and Galveston. Ron Paul's district
Texas 12	416	0.08*	-15.06%	Fort Worth suburbs
Arkansas 3	417	0.08*	-5.30%	Northwestern Arkansas. Solidly Republican
Alabama 4	423	0.08*	-16.56%	Northern Alabama
Texas 11	425	0.08*	-6.10%	Central Texas and Waco. Solidly Republican
Georgia 9	426	0.05* *	6.84%	District was transplanted across the state in 2006. Ignore
Texas 19	431	0.08*	-4.66%	Lubbock and surrounding areas. 2002 Republican gerrymander

Is there a pattern to district-level change? First, it is interesting to note that the one district that was redistricted in this timespan—Georgia 9, which in the 110th Congress moved from the northeastern to northwestern part of the state—showed among the strongest independent variation in the dataset. This natural experiment is perhaps an indication that my measures are able to capture actual change in the profile of Congressional districts, though I would be well-advised not to oversell this. As for the remaining districts, the median rank order of districts that are unstable at 90% confidence is 183.5 out of 436, indicating that the most liberal districts disproportionately show independent change. Consistently with Figure 4.2, the “left tail” of liberal districts proved resistant to the conservative trends of the late 2000s. And, as Levendusky, Jackman, and Pope (2008) suggest, a large number of these districts are majority-minority—either as the natural result of their urban constituencies, or as the result of the conscious racial gerrymandering (for instance,

in California 38th, Maryland 4th, Maryland 7th) that burgeoned beginning with the 1990 redistricting cycle (Tate [2003], for example, defends this practice as a means of enhancing descriptive representation). Maryland 7th, in particular, features the unusually low compactness typical of gerrymandering (Barone et al. 2013). Figure 4.3 is a histogram of the rank order of the 111 unstable districts, and visualizes both the left skewness and bimodal distribution of aggregate ideological change. It would appear that the one-way ANOVA of districts by quartile did not capture what the stability test shows: clear evidence of ideological polarization. Most districts do not lie on ideological extremes; however, the most extreme districts are pulling in opposite directions.



Explaining Change: A First-Difference Approach

What accounts for the changes? In answering, I will proceed in two steps. First, consistently with Pacheco (2014), I will select a sample of interesting districts and attempt to tell a “story” therein. This story will try to show the extent to which the demographic profile of the electorate tracks

ideological change. The second step will be to model ideological change using those demographic changes that we suspect are implicated.

Table 4.6 shows a sample of the districts that exhibit the most unambiguous ideological change, together with their demographic profile. The striking feature is that districts that exhibit liberal change (or that, at least, fail to become more conservative) are, again, their disproportionately African-American or Latino populations. Note that district-level demographics operate in much the same way as individual-level predictors, as when low income or minority status predicts liberalism; this is hardly the case for state-level demographics, which operate in more counterintuitive directions (Gelman 2009). Other features of liberal-moving districts, then, include a high population density, and comparatively high rates of poverty, unemployment, and violent crime. Unsurprisingly, districts that exhibit conservative change tend to be rural and homogeneously white. It could be that the homogenization imposed by gerrymandering is a predictor of nationally-independent changes in public opinion.

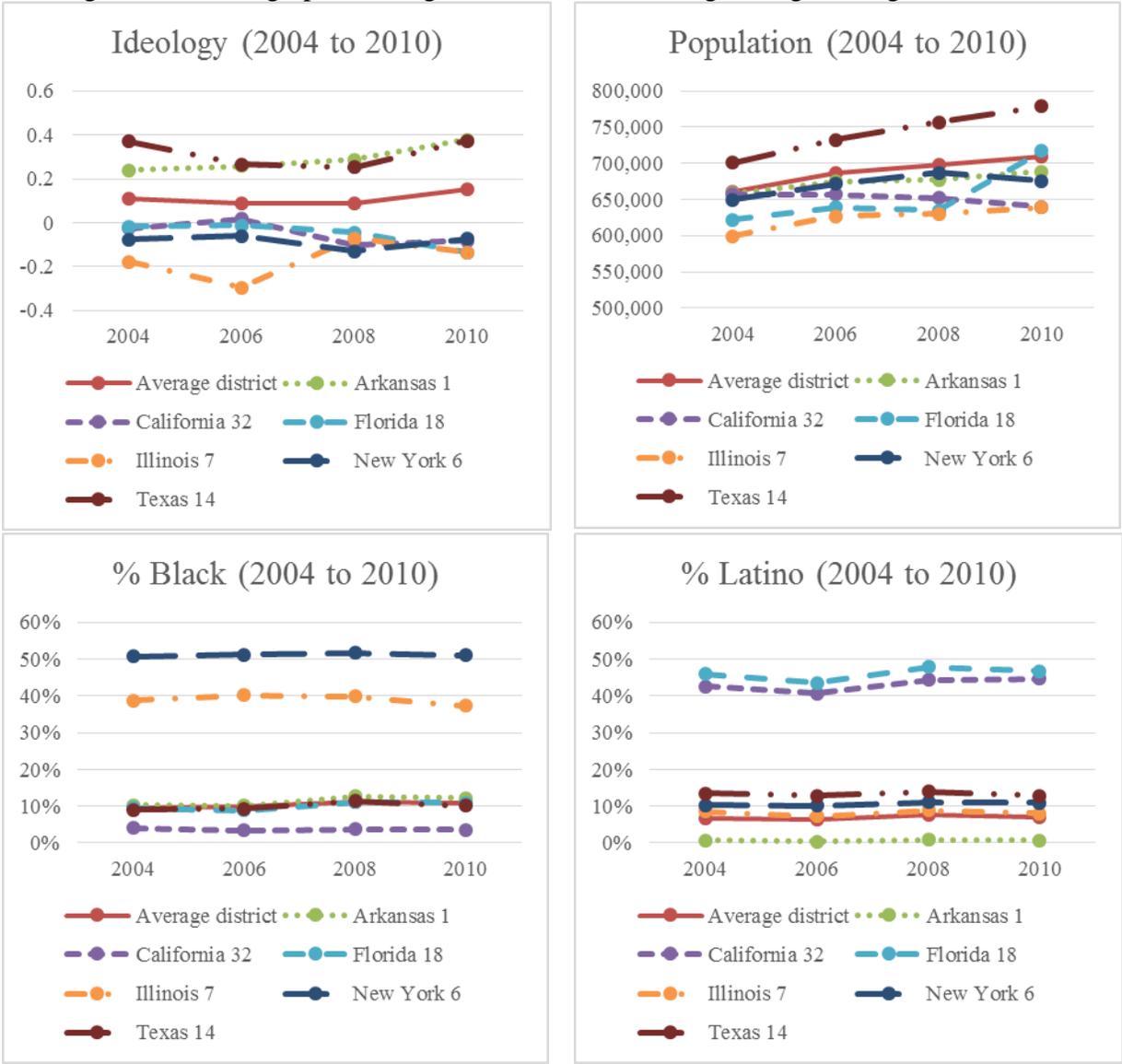
District	Trend	Affluent	Black	Latino	College Educated	Foodstamps	Retired	Rural	Unemployed	Murders per 100k people*
Alabama 5	More conservative	21.9%	13.3%	1.0%	33.5%	4.1%	19.2%	8.5%	4.9%	1.9
Arkansas 1	More conservative	10.4%	11.4%	0.7%	18.4%	9.4%	15.4%	70.5%	5.6%	1.9
Louisiana 6	More conservative	22.1%	28.6%	1.4%	31.8%	10.1%	14.8%	12.4%	4.7%	3.2
Minnesota 3	More conservative	37.1%	2.9%	1.0%	46.5%	1.8%	11.3%	0.0%	4.1%	3.6
Texas 14	More conservative	25.8%	9.9%	13.3%	29.8%	4.6%	14.7%	19.4%	4.5%	1.6
West Virginia 1	More conservative	12.5%	1.7%	0.6%	24.2%	6.7%	19.6%	62.4%	4.6%	0.0
Illinois 7	Constant	28.9%	39.0%	8.2%	45.6%	12.6%	10.4%	0.0%	8.0%	11.8
Massachusetts 8	Constant	31.7%	16.7%	5.6%	54.5%	7.1%	9.5%	0.0%	5.8%	7.7
New York 6	Constant	29.4%	51.2%	10.6%	31.0%	9.9%	16.0%	0.0%	7.1%	4.6
California 9	More liberal	36.1%	21.1%	8.2%	54.9%	2.9%	14.0%	0.0%	6.2%	9.6
California 32	More liberal	26.7%	3.7%	43.0%	26.7%	3.7%	11.6%	0.0%	6.8%	9.2
Florida 18	More liberal	28.3%	10.1%	46.0%	43.1%	11.1%	10.6%	14.8%	5.0%	3.3
Florida 23	More liberal	22.5%	18.1%	8.2%	35.0%	6.3%	15.5%	1.1%	6.4%	4.1
Louisiana 6	More liberal	22.1%	28.6%	1.4%	31.8%	10.1%	14.8%	12.4%	4.7%	3.2
Maryland 7	More liberal	34.6%	37.0%	1.6%	44.8%	7.0%	15.2%	0.0%	5.0%	16.8
Minnesota 4	More liberal	29.1%	4.1%	2.3%	43.9%	3.6%	13.7%	0.0%	4.9%	2.9
New York 11	More liberal	28.1%	49.4%	7.4%	42.3%	13.1%	10.4%	0.0%	6.8%	9.0
New York 18	More liberal	51.9%	11.3%	6.5%	57.1%	2.5%	14.9%	0.0%	4.6%	2.4
Tennessee 9	More liberal	20.0%	48.9%	1.0%	34.0%	13.5%	13.5%	0.0%	7.7%	16.4
<i>Average district</i>	<i>More conservative</i>	<i>24.4%</i>	<i>10.3%</i>	<i>6.9%</i>	<i>34.3%</i>	<i>5.6%</i>	<i>15.1%</i>	<i>21.1%</i>	<i>5.3%</i>	<i>3.8</i>

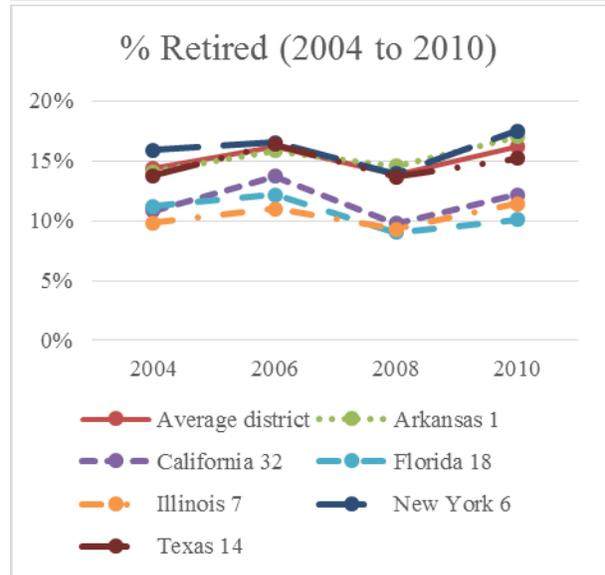
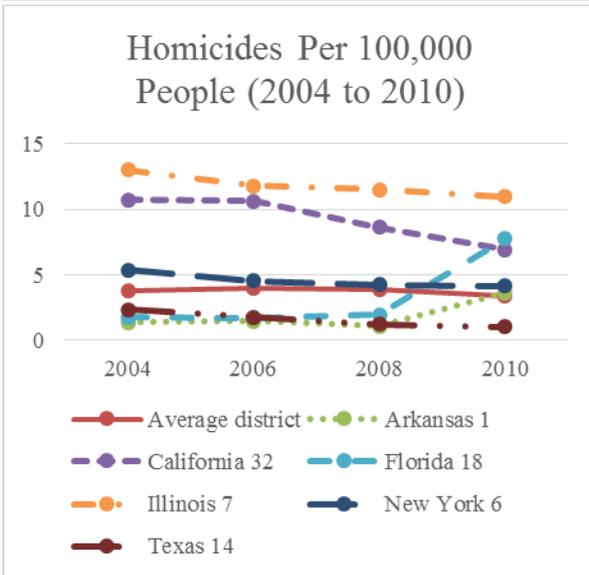
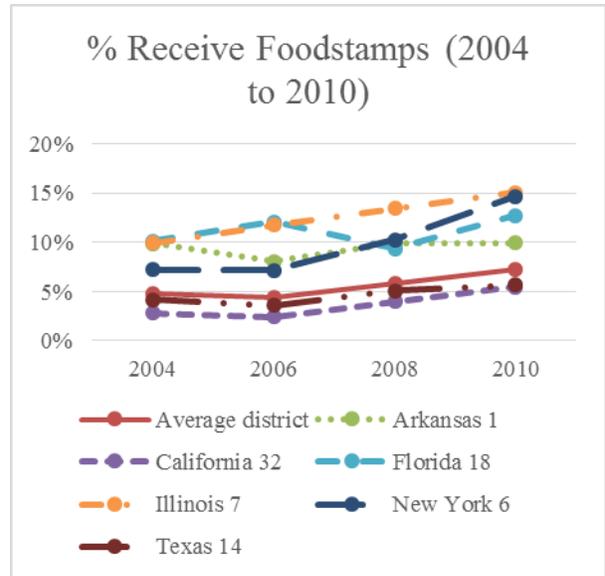
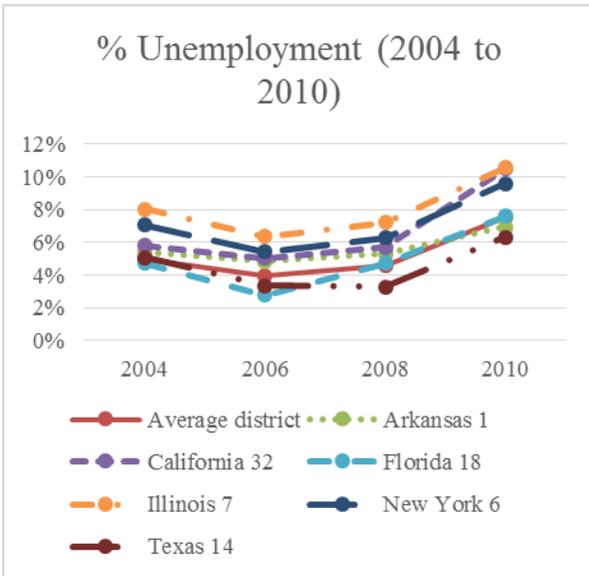
* Homicide rates are taken from CDC's WONDER (Wide-ranging OnLine Data for Epidemiologic Research) utility at <http://wonder.cdc.gov>. These are approximate in the extreme. For one thing, WONDER only yields county-level results, meaning that these had to be reweighted by county likelihood of being in a Congressional district. For another, the CDC does not publish homicide numbers for counties with fewer than 10 murders in a specified timespan. To get around this, I coded counties having suppressed murder rates as having no murders. This means that I underestimate murder rates in rural jurisdictions. Nonetheless, my numbers are the best district estimates of homicide rates that anyone can possibly generate. The remaining demographics are taken from my American Community Survey post-stratification data (see previous chapter).

Is there a change in district-level demographics that might explain the unusual behavior of districts that are ideologically unstable at or near 95% confidence? Recall that migration and the changing demographic profiles of districts are the likely explanations for gradual attitudinal change, whereas dramatic economic and political are likely behind sudden change. Figure 4.4 graphs the social and economic changes in a sample of districts that exhibit the strongest trends, largely using the same post-stratification dataset with which I performed the probability adjustment in the previous chapter. The racial composition of these districts, including the majority-minority ones (for instance, the districts in Chicago, Los Angeles, Miami, and New York), remain largely unchanged. The plot of retirees shows that inner-city districts are, in the aggregate, aging at slower rates than the national population, which may furnish a partial explanation for why they have failed to become more conservative. Miami and the Dallas/Fort Worth suburbs show substantial population growth since decennial redistricting; the rest, either

stagnation or decline. Migration itself tells us little about ideological change, though it is possible that (say) growing affluent, conservative suburbs such as the Greater DFW Area either attract conservatives or induce newcomers to conform. The most central relationship that the graphs capture is nonetheless that between economic stress and ideology. Namely, unemployment and the percentage of the population requiring economic assistance grew more sharply in districts that became more liberal, or at least failed to become more conservative in the 2010 wave election, than elsewhere.

Figure 4.3. Demographic Change in Districts Exhibiting Strong Ideological Variation





It goes without saying that the above furnishes only an impressionistic portrait of how and why districts move ideologically. It behooves me to specify a regression model on this portrait's basis. A first difference model that regresses ideological change on changes in district-level fixed effects makes the most sense. This treats my data like a panel study, with district as the panel variable and election year as the time series variable. Theoretically, this approach is superior to a model with just cross-sectional measures because ideological change, rather than ideological composition, is what I seek to explain. Moreover, first difference models obviate the effects that

omitted district-level variables may have on my ideology estimates (Liker, Augustyniak, and Duncan 1985), in addition to reducing the correlations between predictors and thus multicollinearity (Gujarati 2004, 367); in the latter, we might imagine that (say) unemployment and levels of people seeking government assistance are highly correlated, but that the year-to-year change scores for both measures are much less correlated.

Nonetheless, it seems logical, from the graphs in Figure 4.4, for the model to incorporate the proportion of African-Americans and Latinos rather than their change scores because—at least within the short time-span of the late 2000s—few districts undergo appreciable changes in their racial composition. I include the race variables on the theory that majority-minority districts are systematically more likely to become more liberal, and in particular to resist the strong conservative tide of the 2010 wave election. The remaining predictors are changes in district demographic social and economic characteristics, a dummy variable for “South” (which is standard in Congressional responsiveness literature), a national change score to control for national ideological trends, and finally voter turnout (hypothesized to predict change in the liberal direction). I specify the model as follows:

$$\Delta ideology_{ij} = \beta_0 + \beta_1 south_i + \beta_2 black_{ij} + \beta_3 latino_{ij} + \beta_4 \Delta national_ideology_j + \beta_5 \Delta turnout_{ij} + \beta_6 \Delta population_{ij} + \beta_7 \Delta affluent_{ij} + \beta_8 \Delta college_educated_{ij} + \beta_9 \Delta esl_{ij} + \beta_{10} \Delta graduate_educated_{ij} + \beta_{11} \Delta retired_{ij} + \beta_{12} \Delta under_30_{ij} + \beta_{13} \Delta unemployed_{ij} + \beta_{14} \Delta homicide_rate_{ij} + \varepsilon_{ij}$$

where:

$\Delta ideology_{ij}$ = Ideology change score in district i and election year j from previous election year

$south_i$ = Whether district i is in the South ($south = 1$) or not ($south = 0$)

$\Delta national_ideology_j$ = Nationwide ideology change score in election year j from the previous election year, per the national exit polls

ε_{ij} = Error term for district i and year j

Table 4.7 shows the results, with standardized regression coefficients and standard errors. Unsurprisingly, the national ideology variable is significant—but not overwhelmingly so, as it explains only 6% of the variance in district-level change scores. The heterogeneity of ideological change is again on display. Also with little surprise do we see that districts in the south became disproportionately more conservative. Strikingly, district minority composition strongly predicts liberal trends; this is not the case for the change scores of the black and Latino variables, which are excluded from the model. Majority-minority districts exhibit different behavior than majority white districts, and this difference is unaccounted for by demographic change between redistricting cycles. Racial gerrymandering, as well as partisan gerrymandering that packs minorities into Congressional districts, thus do much to explain the observed heterogeneity of ideological change. Also interesting is that voter turnout is insignificant, despite the voluminous scholarship and punditry that links high voter turnout to Democratic electoral success (Hansford and Gomez 2010). Population increase also weakly predicts liberal change.

Table 4.7. First-Difference Model of District-Level Ideological Change

	(1) First-Differenced Ideology
South	0.049** 0.00975** (0.00357)
% Black	-0.153*** -0.112*** (0.0155)
% Latino	-0.072*** -0.0628*** (0.0173)
National Ideology†	0.704*** 0.949*** (0.107)
% Turnout††	0.058 0.0307 (0.0427)

Population†	-0.064* -0.000000261* (0.000000117)
% Affluent†	0.037 0.202 (0.193)
% College Educated†	0.230** 0.813** (0.297)
% ESL†	-0.048 -0.358 (0.234)
% Grad Educated†	-0.300*** -1.579*** (0.361)
% Retired†	-0.095 -0.378 (0.277)
% Under 30†	0.300** 0.592** (0.227)
% Unemployed†	-0.158** -0.782** (0.298)
% Veteran†	0.217*** 1.184*** (0.338)
Murder rate†	0.059* 0.00469* (0.00200)
<hr/> <i>N</i>	1308
<hr/> Overall <i>R</i> ²	0.238

Standardized beta coefficients; unstandardized point estimates; robust standard errors in parentheses

Panel regression with random effects estimators.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

† Predictor is first-differenced.

†† First-differenced percentage of the voting-age population (VAP) that voted in a House election. For Washington D.C., House vote is coded as the percentage of the voters who voted for the highest office (taken from McDonald 2016). For districts with uncontested House elections, voter turnout was imputed from state turnout levels.

Of the other demographic covariates of ideology, aggregate college education predicts conservative change with 99% confidence, while graduate education predicts liberal change at 99.9% confidence. Veteran, percentage of young people, unemployment, and (weakly) homicide

rate are also significant. Notably, percent under 30 moves ideology in an unexpected direction (in the individual-level model used to generate district estimates, age is negatively correlated with liberalism), and changes in unemployment strongly predict district-level liberalism (perhaps voters in depressed districts respond by favoring government intervention). Affluent, retired, and English as a second language are insignificant.

A possible counter-consideration is that demographic covariates are endogenous to my ideology measures, and that therefore any regression of my measures on demographic change is “rigged” to show a significant result. They are endogenous, one might argue, because MRP first uses individual- and aggregate-level demographics to model survey responses, and because these responses are weighted to the distribution of demographics in congressional districts. The results do not support this conclusion, however. My MRP model eschewed Latino, percent under 30, unemployment, college education, and population size as district-level fixed effects—and these are among the most significant factors of ideological change. Likewise, retired and ESL *are* used to estimate ideology, and these factors are insignificant. The first-difference design of my model, as well as the inability of any combination of non-political variables to explain most of the attitudinal variance in Congressional districts, attenuates concerns about endogeneity.

Conclusion

The foregoing highlights the weakness of static measures of district-level ideology. Districts show independent change between decennial redistricting cycles. This change is heterogeneous, even in the face of three consecutive wave elections that should have exerted a strong homogenous pull on district-level attitudes. Ideological change is bimodally distributed—the most liberal and conservative districts exhibit the strongest trends—but is most pronounced in majority-minority districts, which grew in relative liberalism from 2004 to 2010. Aggregate changes in population,

age, education, and crime also help to explain ideological change. Demographic change plays a more unambiguous role in district-level change than in state-level change, where states that exhibit ideological instability share few obvious commonalities.

Having established that there is ideological change in the districts, I am now well-placed to answer the question whether legislators respond to it. This will be the focus of the next chapter.

Chapter 5. The Elusive Median Voter

Full Circle

I have thus far used recently-developed statistical technology as well as large- n state and national exit polls to compute longitudinal measures of House district voter ideology. I have shown that they are reliable, and correlate highly with existing proxies of district ideology (such as presidential vote) as well as estimates of district-level issue attitudes. I have also shown that district ideology changes between decennial censuses, and that this change is nationally heterogeneous rather than nationally uniform. I have *not* yet shown, however, whether my measures give us any leverage in understanding the phenomenon of representation. I have come full circle, and return my discussion to the substantive questions that necessitate public opinion measures in the first place.

The purpose of this chapter is to fill this final gap and see whether members of Congress (MCs) respond to voter attitudes, and, if so, *which* voter attitudes. I will, first, briefly discuss the meaning of responsiveness; second, I will discuss two competing models of spatial voting—the Median Voter Theorem and Directional Voting Hypothesis—that lead scholars to opposing predictions about responsiveness; third, operationalizing exit-poll imputed ideology estimates as constituency opinion, I will test whether candidate and legislator positioning correspond to the expectations of the Median Voter and Directional Voting models; and, finally, fourth, I will interpret the meaning of my results.

Responsiveness: A Primer

One of the oldest questions in Political Science is whether Congress responds to its constituents, and therefore is legitimate as a popularly accountable institution. Since the 1960s, an expansive literature on House representation has emerged. Therein, “responsiveness” emerged as a simple

and testable operationalization of the broader concept of substantive representation. Whereas many politico-theoretical definitions of representation emphasize the autonomy of representatives vis-à-vis the represented—for instance Pitkin’s (1967) suggestion that legislators ought to advance their constituents’ objective interests, as opposed to their mere demands—responsiveness starts from the assumption that representatives will act as agents of the will of voters. A legislator is responsive to the extent that her activity in Congress, most notably her roll call voting, covaries with the policy preferences of her geographic district: if her district’s electorate becomes more liberal, so do her roll call votes. Thus, responsiveness treats the constituency and the representative as single pair, or dyad.

It is important to here note what responsiveness is *not*, and what realities this chapter will not look for. Responsiveness is distinct from the related concept of congruence, which measures the extent to which a district majority actually agrees with the enacted preferences of its elected representative (Eulau and Karps 1977). Districts who elect responsive legislators are still not certain to get what they want out of government, particularly in a political system with permanently divided sovereignty. The state policy literature, which generally finds that state policy is consonant with state public opinion (Erikson, Wright, and McIver 1993; Berry et al. 1998; Lax and Phillips 2009a), is thus of limited utility for understanding House responsiveness. Another limitation is that dyadic representation captures only a portion of legislative activity, namely *voting*. Representation can also take the form of policy specialization and bill sponsorship, an insight that has spawned a parallel representation literature (see, for instance, Hall 1996) that I must here elide. A final limitation is that conventional studies of dyadic representation consider only one-way causation, where the theoretical possibility of reciprocal causation (voters educate MCs about what

they want, but MCs also modify constituent demand by educating voters) exists on a subset of policy domains (Hill and Hurley 1999).

As previous chapters discuss, Miller and Stokes (1963) inaugurated the responsiveness literature that is central to our purposes. They were of the qualified view that legislators respond to public opinion. Using the same low-*n* American National Election Study data as Miller and Stokes, Stone (1982) found that the relationship between district public opinion and legislative behavior was substantial and increased with time. This and a large number of subsequent publications supported this view (Mayhew 1974a; Erikson 1978; Fenno 1978; Bartels 1991; Bianco, Spence, and Wilkerson 1996). A dissenting body of literature appeared in response to the sunny responsiveness orthodoxy of Miller and Stokes. Achen (1978) was among the first to question the new consensus, finding that incumbents are actually less responsive than challengers in House races. Dozens of other studies have discovered no or limited evidence of Congressional responsiveness (Bernstein and Anthony 1974; Bernstein 1989; Fiorina 1974; Page et al. 1984; Kau and Rubin 1993; Lindsay 1990; Wilkerson 1990; Cohen and Noll 1991; Levitt, 1996; Hutchings 1998). Given the discipline's bias against publishing null findings, it is unknown how many papers in the non-responsiveness school remain unpublished (Gerber and Malhotra 2006; Bishin 2009, 9).

Why revisit the question of House responsiveness? I offer three answers. First, Political Science cannot yet wax triumphalist about its understanding of the topic. No literature review can illuminate a clear consensus about if, when, and how representatives translate public opinion into legislation. Typically, studies that focus on a narrow range of salient issues find that the effect of public opinion on policy is “awesome” (Erikson, Wright, and McIver 1993, 80), whereas those looking at the entire corpus of legislation tend to find non-responsiveness (Burstein 2006). This

tells us that responsiveness likely varies across policy domains (Wlezien 2004). Because I am looking at responsiveness *in general*, any finding of responsiveness on my part has a good chance of making a contribution to the literature. Second, Congressional scholars have typically relied on static measurements of voter ideology to test responsiveness, when in fact representation is a dynamic phenomenon that cross-sectional statistical models are unlikely to capture (Lax and Philips 2012). Finally, most theories of responsiveness hold that representatives will only respond to the preferences of voters (Griffin and Newman 2006), whereas most extant studies have not utilized measures of voter attitudes. My exit poll measures correct for these limitations and make possible a fresh look at the representation phenomenon.

To what hypotheses does the responsiveness question lend itself? To find my theoretical expectations, I now turn away from contradictory empirical studies and towards the conceptual literature. Below, I detail the contributions of spatial theorists of vote choice, and how I might leverage them into usable hypotheses for Congressional responsiveness during the time series of my measures. I identify two major conceptualizations of voting behavior, with opposing predictions for House legislative behavior. I propose to test these predictions.

Geographic Or Party Constituencies? A Tale of Two Models

Spatial models of politics derived from economics interpret elections as simple methods of aggregating voter preferences. The most popular of these is the Median Voter Theorem (Black 1958; Downs 1957a), or MVT. The MVT is a simple and intuitive framework for mass-elite linkage in American politics, and a logical starting point for anyone wishing to understand this linkage. The MVT predicts a “median” result in any election where the spatial analogy of politics

obtains and preferences are normally distributed.¹ In other words, when voters consider candidates on the basis of their proximity to themselves on a unidimensional policy continuum, the median voter decides every election. Candidates will then position themselves at the center of the continuum to capture the median vote. The MVT also predicts strong responsiveness, since successful candidates—as single-minded, entrepreneurial election-seekers who must win in order to realize any of their goals—must respond to the preference profile of the entire electorate, rather than subsets thereof (for instance, party constituents). Candidates must also try to enact their stated preferences once in office, as rational voters might use retrospective reasoning to place candidates spatially, thereby punishing “shirkers” (Downs 1957a, 72). Rational representatives, in turn, monitor public opinion even in the absence of electoral opposition, since challengers and other elites might always emerge to highlight constituent disagreements with their representatives (Key 1961). Some evidence shows that legislators can respond even without intervening elections, so as to prevent potential electoral coalitions from emerging (Arnold 1990; Bartels 1991; Stimson, MacKuen, and Erikson 1995; Wichowsky 2009). Even so, electoral competitiveness is central to spatial voting, as it predicts that challengers will force the incumbent to assume the median vote’s position. Against Bartels’ (1991, 436) claim that “representatives who win with 100% of the vote appear to be as responsive to constituency opinions those who win with 51% of the vote,” Griffin (2006) finds that electoral marginality is strongly linked with responsive behavior.

The MVT allows that symbolic ideology is crucial to responsiveness. Critics of public choice who associate economic accounts of voting with affectless super-rationality overlook the later discussions in *An Economic Theory of Democracy*. In Downs’s model, individuals are not issue voters, as issue voting imposes high information costs on an unformed mass electorate.

¹ This means that the MVT is not likely helpful where the distribution of public opinion is multi-modal, which is highly possible in proportional representation systems, as well as extremely polarized societies.

Voters instead offset these costs by relying on emotionally-charged ideological labels, which tend to make politics intelligible by narrowing an otherwise multi-dimensional policy space (Enelow and Hinich 1984). Representatives then use constituent ideology as a signal for how to behave responsibly, which would be much harder without the form and coherence that ideology imposes on public demands (Sartori 1969). Hinich and Munger (1994) expand still further on the role of ideology in public choice. Rather than functioning mainly as an instrumental heuristic, as in Downs, ideology in later public choice accounts is *substantive*. Candidates must present their ideas in terms of the broad analogies, potentialities, and symbolisms that cohere in a mass belief system—a system that often takes the form of a centralized party platform—and commit to making policy in terms of these analogies. (This attenuates the MVT, however. If a candidate “flip flops” in order to adopt the median preference, the utility of voting for her drops; voters can no longer approximate her ideal point. The need for ideological credibility partly explains why candidates in a given election do not adopt identical platforms). They predict that ideology will influence representative behavior independently of constituent issue preferences.

My measures are therefore uniquely positioned to test the MVT, as they quantify the belief systems of district median voters as they select representatives. If my data can show that representative behavior responds to biennial changes in these measures, then I have gone a long way in showing that Congress is a democratically responsive institution.

There are nonetheless reasons to question whether MVT will obtain. Non-constituent factors like party discipline (Cox and McCubbins 1993; Lee 2009), the imperative of preserving parties’ national “brand” (Aldrich 1995; Jones 2010), the need to appeal to interest groups for vital financial and political resources (Schofield and Miller 2007), and legislators’ desire to enact good policy as they understand it (Fenno 1973) are common explanations for why representatives

“shirk” from constituent demand. Many of these factors are outside the scope of the MVT’s parsimonious spatial model, and neither Anthony Downs nor Duncan Black would have ever quibbled with their importance. More fundamental critiques of the MVT’s internal logic, however, have abounded for as long as it has existed. Adherents of the Michigan Model, holding that emotive sources of identity and not candidate positioning determine vote choice, express skepticism about whether the model’s neat assumptions could ever obtain in the disorder of American politics. Stokes (1963) doubted that all voters evaluate candidates using the same issues and the same scale, that all political questions exist on an ordered dimension (nobody is “pro-corruption”), and that voters reason using the same set of cognitive partisan and ideological cues. Ian Shapiro (2009) argues that history has passed an unfavorable verdict on the MVT. If the median income in a modern capitalist state is always lower than the average income (Pareto 1897), then would not the MVT predict that electorates will always enact radically redistributive policies? This populist result, a nightmare for the large number of public choice theorists of the classical liberal persuasion, has not come to pass in advanced democracies, which Shapiro takes as a falsification of the theory.² Gelman and Katz (2007), more focused on the United States Congress, find that ideological moderation confers only a 2% electoral advantage in a given House race—hardly enough to make the median constituent’s preference profile the only one worth considering. In this vein, Fiorina (1999) argues that partisan polarization is proof that House candidates do not generally try to appeal to median preferences.

Dissatisfaction with the MVT has led positive political theorists to rethink the microfoundations of voting. Attempts to bring spatial models in line with the facts of real-world

² Downs, who denies that control over economic policy is equally distributed in any society, would vigorously dispute this interpretation. Chapter 8 of *An Economic Theory of Democracy* contains a pithy discussion of the distinction between mere voters and “influencers,” who are disproportionately attentive to and disproportionately benefit from policy.

political life have produced an alternative to the MVT known as the Directional Voting Model, or DVM (Rabinowitz and MacDonald 1989). The DVM tells us that, while voters sanction candidates for their ideological views as in the MVT, they care less about the proximity of candidate ideology to themselves than they do about said ideology's direction and intensity.³ This is particularly the case with unsophisticated voters, who are better able to identify candidates' broad ideological leanings than they are equipped to estimate candidates' proximity to themselves. Thus, in the conditions of an uninformed electorate, and in an institutional setting with two polarized parties, a rational candidate will assume the risk of alienating moderates and continually emphasize that she is on the same team as ideological compatriots. Extreme positioning is especially likely among candidates appealing to ideologically heterogeneous electorates, where the median preference does not clearly describe the central tendency. As Gerald Wright puts it, "partisans are more homogenous, probably more communicative, and hence easier to represent than the full constituency" (1989, 469). Consequently and paradoxically, a moderate district where attitudes are normally distributed is likely in the directional model to produce an extreme candidate relatively unresponsive to the median voter (for a confirmation of this hypothesis in the Senate, see Harden and Carsey 2012). Directionality may explain why representatives are far more ideologically extreme than the district median voter (Powell 1982; Adams et al. 2004), and why member replacement usually takes the form of extreme candidates replacing less extreme ones (Bafumi and Herron 2010). Adams and Bishin (2004), in an individual- and aggregate-level

³ Yet another formal conceptualization that predicts extreme election outcomes is the discounting hypothesis. "Discounting" voters evaluate candidates not on their ideological proximity, but rather the expected position of government taking into account the separation of powers, for instance the party they expect to control the legislature. They will discount "extreme" issue positions when they deem them unlikely to influence policy outputs. In legislative elections where the winner is unlikely to dramatically influence policy, the discounting motive is naturally greater (Lacy and Paolino, 1998). The short time span of the exit poll data unfortunately makes an empirical test of discounting prohibitively difficult. If the time series were longer, we might test whether historical changes in the configuration of divided government has led to more extreme candidate positioning relative to district ideology.

analysis of Senate races between 1988 and 1992, find strong evidence for the DVM. In their empirical test, “the estimated coefficient for the proximity variable is in fact negative, which is consistent with the proposition that ideological centrism depresses candidate support” (363).

An upshot of the DVM is that, in some elections at least, not all voter preferences are equal when it comes to influencing policy. The DVM predicts that legislators will appeal to present or potential ideological supporters at the expense of others. A new and growing body of literature, embedded in the idea of subconstituencies, agrees with this insight. Subconstituency theory holds that identity groups, smaller than entire legislative constituencies and harboring intensely-held ideological preferences, will exercise a disproportionate effect on legislative outcomes (Bishin 2009; see also Devine’s [1970] distinction between attentive and inattentive issue publics, which presages subconstituency theory). To take one subconstituency study, an examination of the Defense of Marriage Act’s passage details how the district-wide presence of LGBT subconstituencies caused some Democrats to “shirk” from a public opinion then largely hostile to gay rights (Bishin and Smith 2013). Validating one of the DVM’s predictions, Bishin and others other have found that subconstituencies are particularly powerful in heterogeneous districts.

Given our purpose of looking at representation in general, the question of which district subconstituency to look for arises. In conditions of partisan polarization, measures of *party* subconstituencies are necessary to understand legislative behavior on important roll call votes (Kastellec et al. 2015). Clinton (2006) finds suggestive but time-bound evidence that representatives might be especially attentive to the preferences of “partisan publics,” as when a Republican legislator only responds to the median Republican voter in her district. This provides recent evidence for Fenno’s (1978) first-hand observation that representatives devote personal attention to their primary and general-electoral constituencies, both of which are likely to resemble

partisan subconstituencies. Testing the significance of party subconstituencies is thus a good point of departure for an analysis of directional voting.

A comparison of the MVT and DVM is not simply an academic exercise. If House elections adhere to the median result—that is, candidates position themselves as proximately as possible to the median voter, and vote accordingly once in office—then Congress meets accepted requirements for representativeness. If the DVM obtains, then *Federalist 57*'s worry that legislatures will violate popular sovereignty and “favor the elevation of the few on the ruins of the many” may have proven prophetic. Instead of representing all constituents, House members might exercise “segmented representation,” whereby different segments of the electorate—in particular, the most intensely supportive constituents—receive more policy benefits than others (Druckman and Jacobs 2012). The DVM, which predicts relatively extreme and unrepresentative candidate positioning, also recommends itself as a suspect for growing levels of partisan polarization and disaffection with Congressional elites (Fiorina, Pope, and Abrams 2011; Abramowitz 2010a).

Despite the stakes involved, Political Science has been unable to adjudicate the question of whether House elections conform to the logic of either the MVT or DVM. For one thing, constructing a test of both spatial models is difficult. Empirical literature is therefore divided on whether voters favor candidates using proximity or direction, with results that say less about mass and elite behavior and more about authors' methodological choices (Lewis and King 1999). The problems of deriving measures of district-level ideology are multiplied when measures of within-district groups are needed. Research under the aegis of subconstituency theory has thus mainly confined itself to Senate responsiveness on discrete policy matters (Shapiro et al. 1990; Bishin 2009; Kastellec et al. 2015). The main research question of whether House representation conforms to the logic of median or directional voting remains unexplored.

Research Questions and Hypotheses

The above review leads me to identify three separate questions on the subject of responsiveness that 1) have important consequences for the popular legitimacy of “the People’s House;” 2) are not resolved to anyone’s satisfaction; and 3) are amenable to answer using exit poll measures.⁴ I adumbrate them below.

1. *Do median district preferences or median party subconstituent preferences better predict candidate positioning in U.S. House races?* The MVT predicts that candidates will locate their campaign positions as closely as possible to the largest number of voters. Major party candidate platforms will therefore converge on the median district preferences, though they may not do so completely owing to nominees’ need to maintain credibility with voters and eschew “flip-flopping.” The DVM says that voters reason using the direction and intensity of candidate preferences. Particularly in districts where public opinion is heterogeneous, candidates will receive an electoral bonus for appealing to their own partisan constituencies. A good way to test this proposition is to see if district-level partisan median preferences, operationalized in a manner similar to Clinton’s (2006) methodology, explains more variation in candidate position-taking than does the district-wide median.
2. *Do changes in median district preferences or changes in subconstituent preferences better predict changes in House legislative behavior for each district?* The MVT predicts that

⁴ I might also have asked, “are *voters* better represented than mere constituents?” After all, I consciously chose to disaggregate exit polls the assumption that the answer is, indeed, “yes.” Unfortunately, as Chapter 2 makes plain, there is not yet a longitudinal, district-level estimation of general population ideology in the timespan of my dissertation. To compare the effects of my exit poll measures on elite behavior with the effects of competing MRP measures in the House responsiveness literature (which do not go back to 2004, and are not suitable as biennial point estimates owing to small *ns*) would be to compare apples and oranges.

reelection-minded representatives will position themselves closely to the district median after entering office. Policy therefore meets constituent demand. A new subconstituency literature questions the “demand” model of representation, and holds that legislators make minority-preferred voting decisions in order to appeal to intensely mobilized district minorities, for instance partisan publics. While Bishin (2009) only mentions the DVM in passing, it is still a possible framework to explain the “tyranny of the minority” in this nascent Congressional literature. Which explanation has the most leverage in understanding House responsiveness? It is necessary to approach this question dynamically, as responsiveness is a dynamic phenomenon that seeks to understand changes in policymaking. Thus, if changes in the district median preference predict changes in Congressional ideology, then we have good evidence of constituent control over representatives. If only changes in district subconstituency preferences predict changes in Congressional ideology, then “the representativeness of the system and the legitimacy of resulting outcomes may be lacking” (Clinton 2006, 397)—or, in the blunter language of V.O. Key, “all the talk about democracy is nonsense” (1961, 7). Answering Question 2 does *not* tell us, however, if district ideological change owes itself to incumbents changing their minds or to member replacement. For this, I turn to my final question.

3. *Do changes in median district preferences or changes in subconstituent preferences better predict changes in House legislative behavior for each legislator?* If I answer Question 2 in the affirmative, I will need to identify a mechanism for changing roll call behavior in the House. Spatial political theorists—as well as Congressional scholarship purporting to show that representatives actively monitor public opinion—offer one such explanation: that incumbents monitor public opinion in order to foreclose the possibility of electoral

risk. Is this the case? Even with my biennial exit poll measures, this is a harder question to answer affirmatively than Question 2, because House representatives do not change their behavior very much when in office (Poole 2007), and member replacement probably explains most of the longitudinal variance in Congressional roll call ideology (Theriault 2006). I will nonetheless attempt an answer of whether incumbents respond to the changing attitudes of the district median voter, the attitudes of their core supporters, or neither.

On the basis of studies that show members of Congress to be more extreme than their district median voter (Ansolabehere, Snyder, and Stewart 2001), and that candidates make appeals primarily to constituents of their own party rather than converge on the median voter (Clausen 1973; Fenno 1978; Clinton 2006), I set my hypotheses thus:

1. H_1 : Candidates will spatially position themselves closer to partisan subconstituent ideology, particularly in heterogeneous districts, than to the median ideology of their geographic constituency.
2. H_2 : Changes in subconstituent ideology, not changes in the median voter ideology, will better predict changes in congressional roll call behavior in a given district, particularly if the district is heterogeneous.
3. H_3 : Individual legislators will respond mainly to the changing attitudes partisan subconstituents, particularly in heterogeneous districts.

In other words, I expect directional voting to more accurately describe the phenomenon of representation in the House of Representatives.

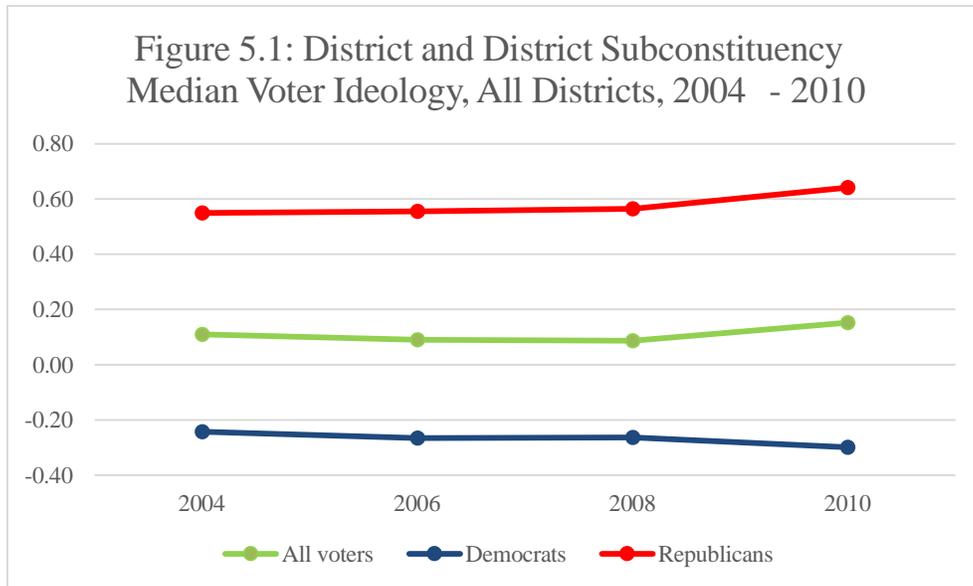
Data and Measures

This study will test the effect of median district voter and district subconstituency policy preferences on candidate and legislator ideology from 2004 to 2010. I will use panel data regressions with random effects estimators to predict legislator and candidate ideology using my district ideology measures. Owing to the overwhelming and growing correlation between partisanship and legislator behavior (Poole and Rosenthal 2011), and previous findings that responsiveness behavior varies by party (Clinton 2006), I will bifurcate my tests by party and run separate models on Democratic and Republican elite behavior. The results of these tests will tell us whether House representatives respond to constituent attitudes and, if so, *which* constituent attitudes.

Measuring constituent ideology. District-level ideology will serve as my most important predictor of candidate and legislator ideology. In the convention of traditional responsiveness literature, which interpreted the average survey response in a district as the median voter preference, I operationalize the median voter ideology using the exit poll measures I developed in Chapter 3. Testing the directional hypothesis requires measures of partisan subconstituencies. To generate these, I run an extension of my original MRP procedure. First, I run district MRP on the “Democrat” response in the exit polls; second, the individual-level estimates of “Democrat” (that is, the inverse logit of the individual’s demographic characteristics and their respective regression coefficients) are used to reweight the post-stratification data, capturing a Census respondent’s likelihood of being a Democrat; finally, the normal MRP model is run on ideology, using only Democrats in the individual-level model, and post-stratifying the model using the Census data reweighted. I repeat the process for Republicans.

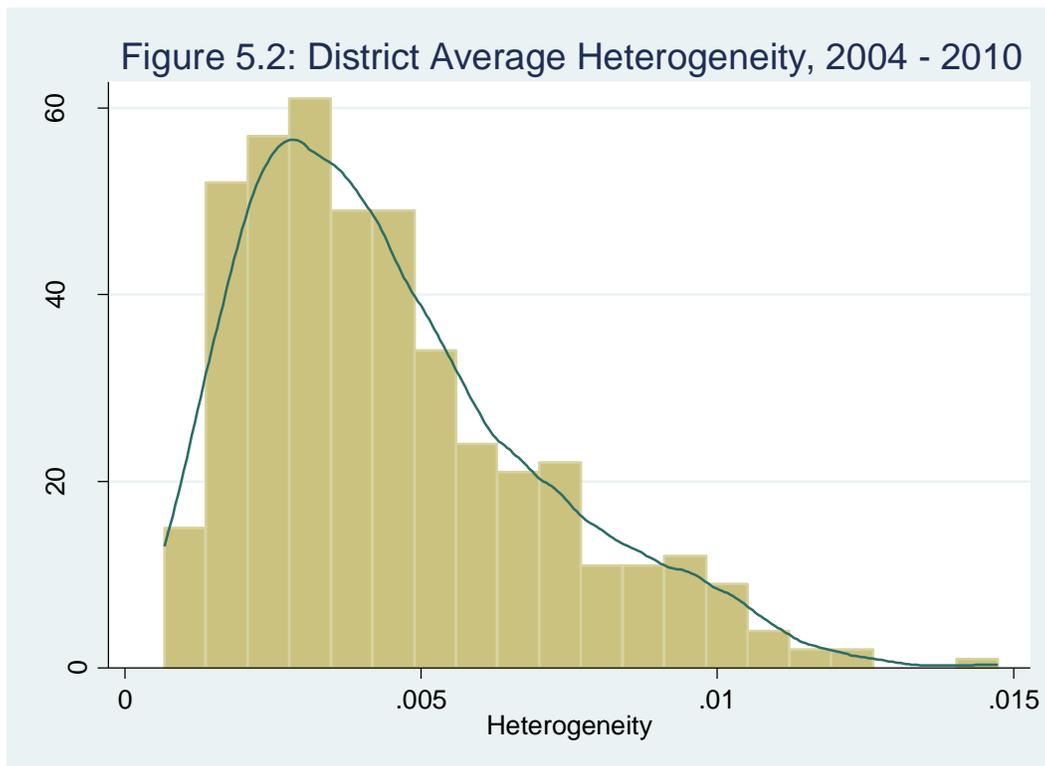
Figure 5.1 graphs the average measures of district Democratic voter ideology, Republican voter ideology, and median voter ideology by year from 2004 to 2010. Democratic ideology and Republican ideology are weakly correlated ($R = 0.25$), and differ from each other and from the district median in the expected ways. The measures appear to be valid on their face. Of course, we cannot lose sight of the fact that these measures are less reliable than the district-wide measures. While number of exit poll cases used to model subconstituency opinion is still massive (97,637 self-reported Democrats and 82,542 Republicans filled out questionnaires in my time series), the median number of modelled cases per district drops from 277 (all constituents) to 108 (Democrats) and 99 (Republicans). The sampling errors of my subconstituency measures are unknown, though certainly greater than that of my district-wide measure. The lack of research into the role of subconstituency politics in House districts nonetheless compels me to use the measures. Even a qualified conclusion in this area promises social-scientific utility.⁵

⁵ It is tempting to put the exit poll data in dialogue with the burgeoning literature on inequality in Congress. For example, Gilens (2005) and Bartels (2008) have found that Congress responds mainly to the attitudes of the rich when these attitudes diverge from those of lower income groups. While it is possible to generate district-level measures of low- and high-income voters using the extension of MRP I discuss in this section, it seems to be the case that the political attitudes of these groups within districts do not meaningfully vary from each other. It could be that, while the ideological composition of different income groups is similar, the meanings of ideological symbols vary between low- and high-income groups, with (say) higher-income liberals emphasizing the social as opposed to economic connotations of “liberal.” Alternatively, it could simply be that public opinion does not differ across income groups on most issues (Soroka and Wlezien 2008). Anyone interested in the MRP-by-income measures can download them at <http://www.cliffordvickrey.com/diss_files/php/mrp.php>.



District heterogeneity. A long-held suspicion of Congressional scholarship is that districts with more heterogeneous public opinion profiles elect more extreme candidates than more homogenous counterparts (Fiorina 1974). As with any district-level measure of voter attitudes, measurement of this important concept has eluded Political Science. Luckily, as with my subconstituency measures, Chapter 3 offers a readymade way to operationalize district heterogeneity. Recall that MRP’s probability adjustment applies individual-level predictions of ideology to individuals in a post-stratification dataset (in our case, the American Community Survey). To determine heterogeneity, I simply take the variance of the inverse logit of the ideology model (the predictions of ideology for each demographic and geographic cell in the post-stratification data) for each year and district. Districts with a low variance are homogenous and those with a high variance are heterogeneous. My measure of heterogeneity is correlated with conservatism ($R = 0.4$), apparently because the most homogenous districts are majority-minority and are in the “left tail” of ideological distribution that I identified in the last chapter; this accounts for why, in the 2012 House elections, Democrats won the majority of votes but Republicans won the majority of seats. Districts with cross-pressured representatives (for instance, Mississippi’s 4th,

represented by future party-switcher Gene Taylor), and Republican districts in the South with sizable black minorities, are the most ideologically heterogeneous. This comports with expectations. Also comports with expectations is the positive skewness of heterogeneity's distribution, which I show in Figure 5.2. Carson et al. (2007) speculate that gerrymandering (in particular, "packing" ideologically similar voters in districts in order to protect incumbents) will lead to a high concentration of homogenous districts. This appears to be the case. Interestingly, the most homogenous districts are found in large states with many districts, perhaps because it is easier for state legislators to gerrymander district lines when there are more of them to draw.



Because the literature hypothesizes that heterogeneity will induce candidates to appeal to relatively extreme party subconstituents, I will include an interaction term that multiplies heterogeneity by party subconstituent ideology. If the DVM's predictions are accurate, then this term will positively covary with candidate and legislative ideal points.

Measuring elite political behavior. Candidate and legislator ideology is not directly observable. While Congress scholarship has had an easier time measuring the representative side of the constituent-representative relationship, I still need to make discretionary choices about how to operationalize my dependent variables. To operationalize candidate ideology in elections, I rely on Bonica's (2014) Campaign Finance (CF) scores, the best and most comprehensive historical estimates and candidate ideological positioning available. He assigns these scores on the basis of giving patterns of campaign finance donors. Donors receive ideal points using the ratio of money they give to conservative causes to liberal ones; candidates across all federal offices are then assigned ideal points using the ideal points of their contributors. CF scores range from about -5 (most liberal) to 5 (most conservative). While CF scores are a godsend to Congressional scholarship, which has limited data regarding House challengers, they nonetheless possess limitations. For one thing, CF scores are static measures that do not vary between Congresses. For another, anyone who has worked with the Federal Election Commission's (FEC) detailed records of itemized transactions knows their limitations: namely, their poor documentation and non-comprehensiveness. Because campaigns are required to itemize only those individual donations of \$200—and because not all candidates, not even all primary winners, meet the fundraising filing threshold—CF scores are available only for the most viable candidates. Nonetheless, regressing CF scores on the district median preference still offers a good-enough test of whether candidate campaign behavior exhibits responsiveness. Median voter ideology and party subconstituency ideology should covary with these scores.

Where legislative ideology is my dependent variable, I operationalize legislator ideal points using Nokken-Poole (NP) scores (Nokken and Poole 2004). These measures are available up to the 113rd Congress. They are similar to the widely-used DW-NOMINATE scores, which are two-

dimensional scales of legislator roll call voting given revealed preferences, with the difference that NP scores are allowed to vary from one Congress to another.⁶ They are generated by, first, running the DW-NOMINATE model and, second, generating estimates for each Congress using the same item parameters as the DW-NOMINATE model. This approach generates ideal points (-1 is the most liberal; 1 is the most conservative) that exhibit both inter-temporal variance and inter-temporal comparability. While the measures' original purpose was to study party defection, they have become standard in studies that study longitudinal changes in roll call behavior. The reason for their appeal is their generality. Older approaches to measuring roll call behavior, for instance interest group scores, relied only on a few votes deemed important by (say) board members of the Americans for Democratic Action. NOMINATE scores, in contrast, are generated using every roll vote on which more than 2.5% of legislators disagreed, and so capture the entirety of a given legislators' voting activity. This has the salutary effect of smoothing out legislator idiosyncrasies across policy domains.

Nokken and Poole nonetheless have critics. For one thing, it seems conceptually bizarre to measure changing ideal points using a fixed ideal point model (Warshaw 2015). This problem becomes apparent when we think about constituent responsiveness. Take Rep. Charlie Rangel (D – NY 13). His constituency in 1970 was Adam Clayton Powell's old district in Harlem, then nearly-exclusively African American. As of the 114th Congress, Rangel represents an upper Manhattan constituency that is more Latino than black. Any relationship between my measures of district ideology and Rangel's NP score might be suppressed by the fact that Rangel's roll call

⁶ For the sake of brevity, I refer only to the first dimension when discussing both NOMINATE and NP scores. DW-NOMINATE spatially places legislators on two dimensions. Since the middle of the 20th century at the latest, Congressional voting is structured unidimensionally along the first dimension. The meaning of the second dimension in recent years is less certain, with guesses ranging from racial attitudes to economic populism.

votes in the 1970s, when he represented a totally different set of voters, will influence his more recent ideal points.

While, in the main, NOMINATE scores (widely accepted as valid; see Bishin 2003) and NP scores exhibit a strong linear relationship, outliers exist. These outliers' NP scores are uniformly more conservative than their most recently-computed NOMINATE scores, and appear to be concentrated among the 84 Republican freshmen in the 112th Congress. This attests to the importance of the fixed ideal points in Nokken and Poole's procedure. Because these individuals entered Congress when it had a conservative legislative agenda, and because I am comparing their NP scores with a more recent iteration of their NOMINATE scores (which includes the 114th Congress), their NP scores are greater than their most recently updated NOMINATE scores by an average of 0.1. This is roughly the ideological space between a mainstream Republican like John Boehner (R – OH 8) and gaffe-prone social conservative Todd Akin (R – MO 2). Again, the outliers highlight that Congresses outside the scope of my time series affect NP scores. While the measures are the best available ones for my purposes, a finding that they do not covary with district ideology might raise suspicions of type II error. In other words, a finding of biennial responsiveness in the Nokken-Poole scores is a hard test.

Controls. Because non-constituent factors influence legislative behavior, I include a number of institutional controls that typically appear in studies explaining Congressional behavior. One control is *seniority*, as legislators that entered Congress before the accelerated polarization of recent decades will likely be more moderate than younger counterparts. Seniority might also predict shirking, since more well-known representatives (such as senior legislators) might be freer to enact their preferred policy preferences than relative unknowns (Burden 2004).

The distributional Congress literature posits that committee chairs are “preference outliers,” and systematically exhibit different roll call behavior than members of the floor (Shepsle and Weingast 1987). Cox and McCubbins (1993) hold that institutionalized party preferences will override personal preferences on key votes. This is particularly true if the member is in the majority party, or if the representative belongs to the party of the president: the House-Speaker-as-party-leader or the White House may be effective in moderating representative voting behavior in the service of a national legislative agenda. To account for these effects, *ranking member of a committee* (sourced from Stewart and Woon 2011), *member of the majority House party*, and *member of the President’s party* factor as control variables.

I also include *fate in the next Congress*, or a dummy variable indicating whether or not the member of Congress is about to retire. Fate is theoretically important, since speculation exists that retiring members, who are no longer the single-minded reelection seekers of Mayhewian demonology, will be free from constituent sanctioning and indulge their personal preferences (Bender and Lott 1996; Jones 2003; Poole and Romer 1993).

Because Congressional literature has traditionally held that electoral competitiveness increase responsiveness (Griffin 2006), and that uncompetitive House elections are a threat to democratic representation (Mayhew 1974b), I code for a number of election controls. One control is *lagged election margin*, or the incumbent legislator’s share of the two-party House vote in the election to the previous Congress. Lagged electoral margin is significant on the theory that legislators use a retrospective instead of a prospective calculus in their roll call voting behavior. Ladewig (2010, 509) finds that historical election margins explain Congressional behavior instead of the MVT’s “future-oriented focus of mirroring their median constituent’s preferences and ideology.” In this case, the lagged election margin would override constituency effects and predict

shirking behavior on the part of electorally safe incumbents, who feel free enough from constituent control to advance their own notions of what constitutes good policy. I also include Jacobson's (1990) measure of *challenger quality*, coded 1 if the House challenger ever held elected office and 0 if not. Challenger quality is theoretically significant, as some scholars have found that viable election challengers predict less extreme roll call voting (Ansolabehere, Snyder, and Stewart 2001; Burden 2004). Sulkin (2005) offers a possible explanation for this: challengers signal areas of constituent-legislator disagreement to incumbents, who accordingly shore up their weaknesses through altered position-taking. The last election control I use is a dummy variable for *primary competition*, coded 1 if the candidate's margin of victory in the House primary was under 30%, and 0 if otherwise.⁷ Congressional scholarship has long suspected that primary competition drives polarization, since it pulls candidate positioning away from the median voter and towards ideologically extreme primary constituencies (Powell 1982; Gerber and Morton 1998). In this case, primary competition should predict roll call liberalism for Democratic legislators and conservatism for Republican ones.⁸

Finally, to ensure that any finding of responsiveness is not merely a function of geography, I include a dummy control indicating whether or not the Congressional district is in the South per Interuniversity Consortium for Political and Social Research (ICPSR) region codes. Southern districts systematically produce more conservative legislators controlling for party, so we expect it to positively predict NOMINATE scores. To further ensure that candidate and legislative

⁷ This is, of course, a very generous interpretation of what counts as a competitive primary race. The high threshold is necessary because most primaries are uncompetitive. According to my own data, fewer than 19% of major party House nominees between 1978 and 2014 were nominated in primaries where the margin of victory was 30% less. That figure drops to 11% if one considers only nominees successful in the general election.

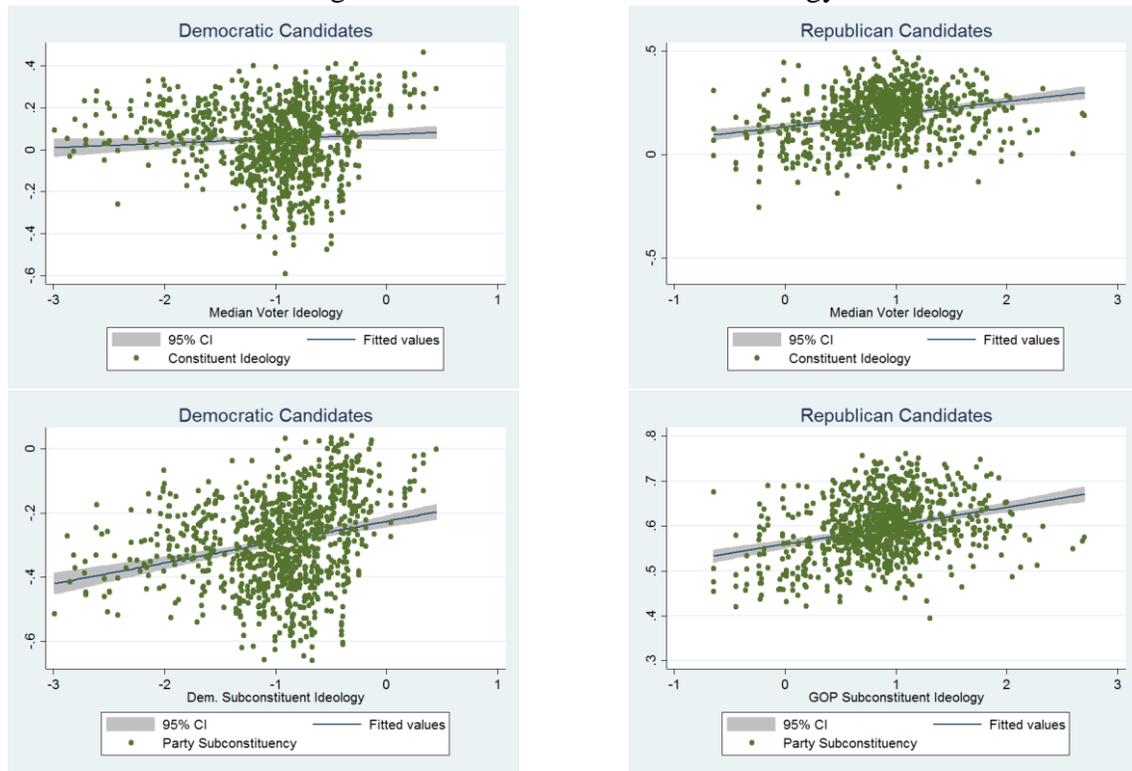
⁸ Election returns were taken from Federal Communication Commission (FEC) reports. Candidates in states where the mode of nomination is convention, such as Connecticut, Utah, and (usually) Virginia, are coded as 0. For states (such as California and Louisiana) with blanket primaries, where voters nominate candidates in a single nonpartisan primary rather than in multiple party primaries, I separated the returns by party to simulate major-party primaries.

positioning is not merely a function of majority-minority districts—whose median ideology is often several standard deviations apart from the national average, as Chapter 4 shows—I control for the proportion of black residents to district populations, with figures derived from The American Community Survey.

A Model of Candidate Positioning

The MVT, voters choose candidates proximately, and so candidates will try to assume the preference of the median voters. The MVT's basic prediction is that, when placed on the same scale, median constituent ideal points will predict candidate ideal points, perhaps to the point where candidates in the same election offer identical platforms. Figure 5.3 shows the linear relationship between the median district preference imputed using the exit polls and CF scores, a donor-based measure of House candidate ideology. Descriptively, we see that constituent ideal points do not explain most of the variation in candidate ideal points, particularly those of Democratic candidates. The CF scores of Democratic nominees and Republican nominees are correlated, as the MVT would predict, but not overwhelmingly so ($R = 0.34$). The *prima facie* evidence of the MVT from the 2004 to 2010 is therefore weak. The subconstituent scatterplots (also in Figure 5.3), in stark contrast, show a very strong linear relationship between CF scores and party subconstituent ideology. This would be the expectation of directional voting, though competing explanations (for instance, that primary competition produces more extreme candidates) must be ruled out.

Figure 5.3. CF Scores and Voter Ideology



To better tease out the real relationships between median and subconstituent ideology and candidate positioning, I first model candidate ideal points as a function of district median preferences thus:

$$CF_score_{ijk} = \beta_{0k} + \beta_{1k} \text{constituent_ideology}_{ij} + \beta_{2k} \text{south}_{ij} + \beta_{3k} \text{black}_{ij} + \beta_{4k} \text{primary_competition}_{ijk} + \beta_{5k} \text{incumbent}_{ij} + \beta_{6k} \text{candidate_quality}_{ijk}$$

where:

$$CF_score_{ijk} = \text{CF score of party } k\text{'s (1 = Democrat; 2 = Republican) nominee in district } i \text{ and election } j$$

Naturally, the MVT expects that district median preferences will exercise a strong and independent effect on candidate positioning. To test directional voting, I simply run the same model with three added predictors. These are *subconstituent ideology*, *heterogeneity*, and *subconstituent ideology* *

heterogeneity for each district i and election j . Respectively, these capture the preference of the median voter in the candidate's party, the variance of the symbolic ideology in a given year and district, and an interaction between subconstituent ideology and heterogeneity. The DVM expects both subconstituent ideology and the interaction term to covary with candidate ideology.

Because my dependent variable is comparable across years and districts, I estimate my dependent variable using a time-series cross-sectional random effects regression with robust standard errors clustered on jurisdiction. This corrects for serial correlation between CF scores in the same congressional district, and immunizes the model to heteroscedasticity.

Table 5.1 displays the results. Columns 1 and 2 show the standardized beta coefficients and standard errors of the restricted model, with only the median voter effect; Columns 3 and 4 display the entire model, with both median voter and subconstituency effects. As expected, Democrats in the South are more systematically more conservative than their non-Southern counterparts. Higher-quality candidates—that is, House incumbents or challengers who have previously held elective office—are more ideologically moderate than less viable candidates. *Primary competition* is insignificant for Republicans but weakly predicts Democratic conservatism. This agrees with recent research that refutes the intuitive but empirically unsupported thesis that House primaries exercise a centrifugal pull on nominee candidate positioning in a given election (Peress 2013). District racial composition, against expectations, fails to predict candidate positioning once South is controlled for.

Table 5.1. Candidate Positioning in U.S. House Elections, 2004 to 2010

	(1)	(2)	(3)	(4)
	Democrats	Republicans	Democrats	Republicans
Constituent Ideology	-0.021 -0.0648 (0.0945)	0.114*** 0.469*** (0.137)	-0.122** -0.384** (0.119)	0.036 0.146 (0.178)
Party Subconstituency			0.091* 0.351 (0.150)	0.220*** 1.625*** (0.320)
Heterogeneity			0.100* 19.65* (8.936)	0.609** 101.5** (33.05)
Party Constituent Ideology * Heterogeneity			0.095* 44.40* (19.78)	-0.691** -169.9** (52.13)
South	0.234*** 0.303*** (0.0612)	0.039 0.0392 (0.0576)	0.213*** 0.276** (0.0584)	0.054 0.0545 (0.0559)
District % Black	0.068 0.208 (0.110)	0.053 0.313 (0.319)	0.022 0.0682 (0.108)	0.040 0.237 (0.306)
Competitive Primary	0.083** 0.147** (0.0460)	-0.041 -0.0545 (0.0470)	0.081** 0.143** (0.0455)	-0.044 -0.0587 (0.0454)
Incumbent	0.416*** 0.547*** (0.0559)	-0.379*** -0.452*** (0.0545)	0.426*** 0.561*** (0.0561)	-0.380*** -0.454*** (0.0527)
Candidate Quality	0.073 0.133 (0.0853)	-0.083* -0.165* (0.0822)	0.077 0.140 (0.0846)	-0.068 -0.136 (0.0810)
Observations	1040	907	1040	907
Overall R2	0.416	0.273	0.454	0.309

Standardized beta coefficients; unstandardized point estimates; standard errors in parentheses.

Standard errors are robust and clustered by jurisdiction.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Most importantly, the results indicate that candidates tend to ignore the preferences of district median voters once the preferences of party subconstituencies are held constant: for Democrats, the median voter coefficient is significant in the wrong direction; for Republicans, it is insignificant. As the DVM predicts, Democratic candidates in heterogeneous districts, where the

entire jurisdiction is theoretically harder to represent, apparently make greater appeals to partisan subconstituencies. Republican nominees seem especially responsive to partisan subconstituencies during this time period. They are also more conservative in heterogeneous districts, though the relative extremism of party subconstituencies does not mediate this effect in the expected direction. The regression results indicate that, contrary to expectations of the MVT, candidates do not necessarily position themselves spatially to capture the largest number of voters. Given the importance of party identification in vote choice, and also “the long term exposure of most candidates to the people and views of a single party” (Clausen 1973, 128), candidates make appeals mainly to partisan supporters. I therefore validate my first hypotheses (H_1).

Still, caution is advisable in interpreting the regression results as a wholesale falsification of the MVT for Democrats. Attenuating the conclusion that candidate ideal points do not track district-level ideology is the fact that the CF measure, while an innovative and well-regarded contribution to Political Science, suffers from a few limitations. For one thing, Warshaw and Tausanovitch (2016) find that CF scores do a poor job of predicting the policy preferences of candidates once they take office. This is particularly true of non-incumbents who win their first election. They offer a possible explanation: donors are limited by political geography and the pool of viable candidates in a given district, such that their giving is not the most direct indicator of their real attitudes. Moreover, CF scores—not unlike older measures of district-level ideology—are static, raising the possibility that my model is not detecting changes in candidate positioning from election to election.

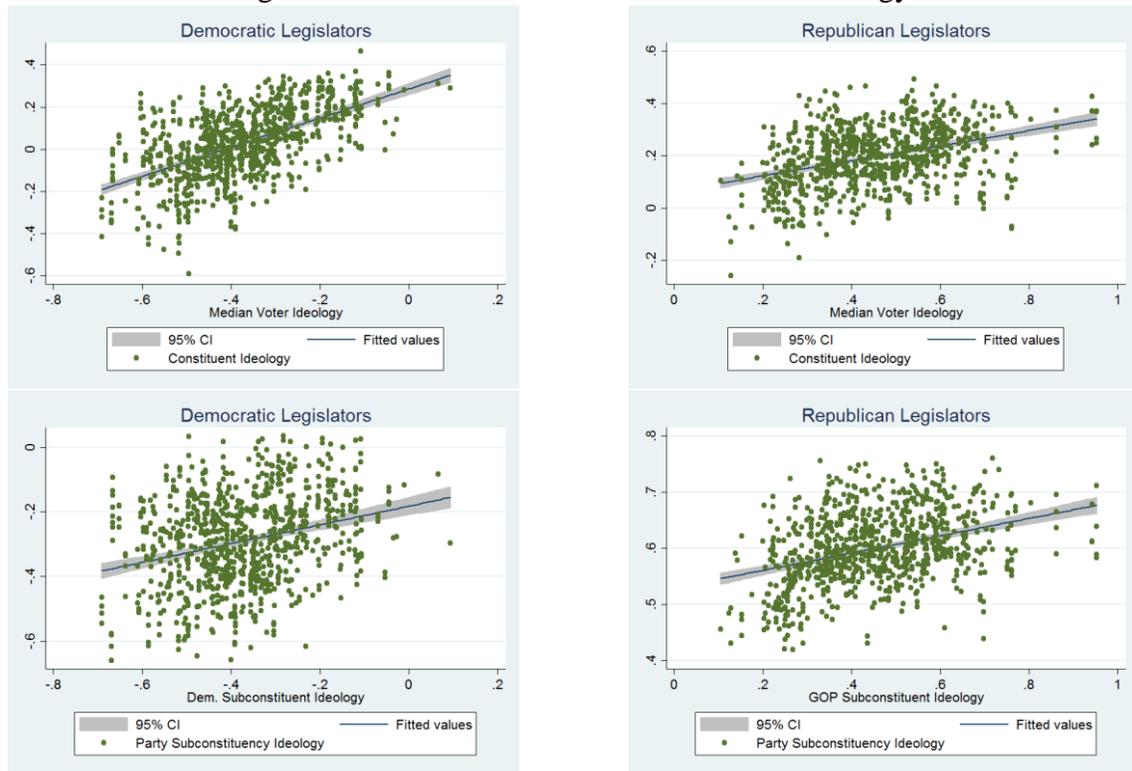
Despite these reservations, we have reason to suspect that the MVT is not an accurate depiction of House campaigns. Since the literature identifies campaigns as the mechanism whereby election-minded elites adopt the policy stances of their constituents, we may doubt whether the

MVT's strong expectation of legislative responsiveness will hold. To further clarify the question of whether representatives respond only to their partisan supporters, I will turn to my second research question, which takes us beyond campaign behavior and towards actual policymaking.

Explaining Changes in Roll Call Ideology

Even if candidates position themselves closely to the preferences of the constituency or subconstituency medians in their respective districts, there is no guarantee that their activity in Congress will match their campaign appeals (Sulkin 2009). To answer whether the MVT is a good explanation of House voting, I start (again) with descriptive statistics. Do my constituency ideology measures predict roll call voting of elected House members? The answer is “yes.” The correlation between DW-NOMINATE scores and my median voter scores is 0.62 for all legislators, 0.57 for Democrats, and 0.38 for Republicans. Constituent ideology compares in performance to CF scores, as well as recent measures of candidate positions based on voter perceptions (Aldrich and McKelvey 1977) and Twitter followings (Barberá 2015), in explaining variation in House member voting behavior. Figure 5.4 displays the relationships for legislators of both parties. The bivariate relationship between subconstituent ideology and NOMINATE scores (also in Figure 5.4) is comparable to that of the district median for Republicans, but somewhat weaker ($R = 0.36$) for Democrats.

Figure 5.3. Nokken-Poole Scores and Voter Ideology



The bivariate relationship between these constituent effects and NOMINATE scores, while a good case for the concept validity of my exit poll measures, is not entirely helpful. We need to know how important median district ideology is relative to other factors in House roll call voting, most importantly partisanship. Member party alone explains most roll call votes in Congress: in the 109th to 112th Congresses, it accounts for 92% of the variation in NP scores—which, as I previously discussed, are NOMINATE scores that are permitted to vary from Congress to Congress. Once this overwhelming party effect is taken into account, is there anything left to explain?

Because NP scores, unlike CF scores, vary from Congress to Congress, they present a unique analytical opportunity for the researcher. If longitudinal measures of public opinion exist, it is possible to determine the sources of ideological *change* in the House. To construct a measure of ideological change, I take the first difference of the jurisdiction's NP score at time j and $j - 1$,

where j is (say) the 110th Congress. To test the MVT, I can therefore model district-level ideological change as a function of evolving public opinion, as well as relevant institutional changes, election variables, and member traits:

$$\begin{aligned} \Delta NP_score_{ij} = & \beta_{0k} + \Delta\beta_{1k}constituent_ideology_{ij} + \Delta\beta_{2k}majority_party_{ij} + \beta_{3k}seniority_{ij} + \\ & \Delta\beta_{4k}majority_party_{ij} + \beta_{5k}seniority_{ij} + \beta_{6k}ranking_member_{ij} + \beta_{7k}retired_{ij} + \Delta\beta_{7k}majority_party_{ij} + \\ & \Delta\beta_{8k}party_of_president_{ij} + \beta_{9k}democratic_house_vote_{ij-1} + \beta_{10k}seniority_{ij} + \\ & \beta_{11k}challenger_quality_{ij} + \beta_{12k}competitive_primary_{ij} \end{aligned}$$

where:

ΔNP_score_{ij} = Change in the NP score of district i 's representative in Congress j from Congress j - 1.

As in the previous section, I also run an extended model with the same predictors, but with the three constituency variables (*first differenced party subconstituent ideology*, *first differenced district heterogeneity*, and an *interaction term*) that the DVM holds to be theoretically significant.

Subconstituency ideology as a first-differenced predictor is superior to the static measure because the latter is likely correlated with the preferences of national, issue-based interest groups. A relationship between the static measure and roll call ideology could simply mean that legislators are reliant on the support of extreme interest groups, in which case all I would have proven is the “resource hypothesis” (candidates shirk voter demands because they pander to extreme interest groups in exchange for political advertising money). A finding that district-specific subconstituent ideological change predicts changes in roll call behavior is less interpretable as a mere vindication of the resource hypothesis.

In the case that the district switches parties in an election, the subconstituent ideology variable is first-differenced with a legislator panel. That is, if Democrat wins a seat formerly held

be a Republican at time j , subconstituency ideology is the difference between district Democratic subconstituency ideology at j and $j - 1$. Because my dependent variable is comparable across years and districts, I estimate my dependent variable using a time-series random effects regression with robust standard errors. In contrast with my CF score regression, my first difference model does not require clustered standard errors to correct for serial correlation.

Table 5.2 shows the results, and a side-by-side comparison of the MVT and DVM conceptualizations. The DVM, in no uncertain terms, outperforms the MVT in terms of validating theoretical expectations; the inclusion of the DVM terms raises the overall variance explained by 20%. Democratic roll call ideology is unresponsive to the changing attitudes of district median voters, whereas it responds highly to Democratic partisan ideology in heterogeneous districts. Republican roll call ideology is weakly responsive to the constituent median, but highly responsive to partisan preferences in heterogeneous districts. Historical district partisanship and electoral safety do not account for these differences. Representatives' desire to exercise influence in the chamber, for instance moderating voting activity in order to better cooperate with the President and House Speaker or retain a committee chair, appears to predict shirking behavior. Republicans in their last term in office during this period also demonstrated a greater personal component in their roll call behavior.

Table 5.2. Legislator Roll Call Ideological Change, 109th to 112th Congresses, District Panel

	(1)	(2)	(3)	(4)
	Democrats	Republicans	Democrats	Republicans
First Difference: Constituent Ideology	0.014 0.0354 (0.0684)	0.193*** 0.702*** (0.150)	-0.025 -0.0624 (0.0888)	0.138** 0.502** (0.157)
First Difference: Party Constituency Ideology			-0.196*** -0.619*** (0.118)	-0.057 -0.313 (0.200)
First Difference: District Heterogeneity			0.313*** 28.12*** (5.381)	-0.598*** -74.23*** (15.45)
First Difference: Party Constituent Ideology * Heterogeneity			0.666*** 106.1*** (9.531)	0.783*** 104.0*** (21.65)
South	-0.019 -0.0109 (0.0197)	0.094** 0.0592** (0.0196)	-0.003 -0.00191 (0.0166)	-0.013 -0.00811 (0.0165)
First Difference: Majority Party	-0.030 -0.00886 (0.00719)	-0.214*** -0.0832*** (0.0113)	0.005 0.00149 (0.00728)	-0.122*** -0.0475*** (0.0127)
Senior Member of Congress in 2004	0.031 0.0193 (0.0156)	-0.033 -0.0308 (0.0178)	0.015 0.00919 (0.0115)	-0.029* -0.0270* (0.0134)
Ranking Member of Committee	0.013 0.00988 (0.0150)	-0.061** -0.0589** (0.0199)	0.002 0.00180 (0.0125)	-0.064*** -0.0624*** (0.0163)
Retired	0.030 0.0340 (0.0221)	-0.056** -0.0679** (0.0228)	0.011 0.0121 (0.0170)	-0.048*** -0.0590*** (0.0165)
First Difference: Party of President	0.245*** 0.104*** (0.0152)	-0.177*** -0.109*** (0.0214)	0.184*** 0.0781*** (0.0173)	-0.147** -0.0911** (0.0284)
Lagged Democratic House Vote	0.455*** 0.00611*** (0.000583)	0.393*** 0.00737*** (0.000840)	0.253*** 0.00339*** (0.000452)	0.149*** 0.00280*** (0.000806)
Challenger Quality	-0.074 -0.0360 (0.0220)	0.089* 0.0448* (0.0200)	-0.042 -0.0203 (0.0182)	0.079** 0.0395** (0.0148)
Competitive Primary	-0.144** -0.119** (0.0414)	0.237*** 0.199*** (0.0401)	-0.121** -0.100** (0.0344)	0.073 0.0609 (0.0348)
Observations	684	620	684	620
Overall R2	0.413	0.489	0.632	0.664

Standardized beta coefficients; unstandardized point estimates; robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In sum, when party is constant, constituency factors do explain a sizable portion of ideological change in Congress. This is good evidence of responsiveness. Directional voting explains much more of this change than does the MVT, which means that I can validate H₂. The model in this section includes cases where there is member turnover, meaning that H₂ does not tell us whether member replacement (a Republican House member losing her seat to a Democratic, or alternatively a Republican House member retiring to be replaced by another Republican) or member change (an individual representative grows either more liberal or conservative) drives House responsiveness to party subconstituencies. The next section will explore these very possibilities.

Do Legislators Change Their Minds?

Studies of Congress, on the basis of not only public opinion data but also in-depth interviews with MCs, claim that legislators routinely monitor and respond to public opinion (Kingdon 1989; Arnold 1990). Despite the fact that reelection rates are high, as well as the fact that most voters do not pay attention to most of the happenings in Congress, observers hypothesize legislators are aware of the fact that challengers can raise past instances of shirking in future campaigns (Key 1961; Sulkin 2005). Is this truly the case? Or, alternatively, are legislators stubborn and fixed in their revealed preferences once they enter office (Poole 2007)? Responsiveness demands that representatives listen to constituents. I will now judge Congress by this metric.

To test whether individual MCs respond to their constituents, I run the same model as in the previous section, with one difference: instead of first-differencing NP score with a district panel, I instead do so with a legislator panel. Legislators in the dataset are identified by their Poole-

corrected ICPSR number. Representatives who changed parties (in my time series, Rodney Alexander [LA - 5] and Parker Griffith [AL - 5] changed from the Democratic to the Republican parties) are treated as separate individuals, such that their roll call ideology for their older party does not contaminate their roll call ideology difference score after they switch. Because legislators have to have served in a least prior one term (or, in the cases of special elections, to have cast enough votes in a partial term) to receive a first differenced NP score, the legislator panel model exclude first-year legislators and works with a smaller universe of cases. How much smaller is the sample? According to my own hand-coded data, 217 MCs exited Congress during this period (85 lost reelection races, 57 retired, 48 ran for higher office, 9 lost re-nomination races, 9 resigned, and 9 died). This exclusion will make it harder for my main constituent predictors to achieve significance. However, most MCs (83.7%) remained in office from Congress to Congress, making the legislator panel model more than feasible.

Table 5.3 displays the results. As is obvious, explaining most of the within-legislator variance in legislative ideology is a hard task at which the model is not quite successful (the full model explains 3% of the ideological change for Democrats and 8% for Republicans). With near total certainty ($p = 0.99$), changes in median constituent ideology do not predict changes in individual legislator ideology for Democrats, and the median change score does not approach statistical significance for Republicans. The DVT model, on the other hand, has significantly more explanatory power. More than any other hypothesized effect, the interaction between heterogeneity and subconstituent attitude change predicts changed patterns in voting behavior. Thus, once again, we find that representatives of diverse geographic constituents maximize their reelection chances by appealing to extreme voters. For Republicans, none of the constituent variables achieve

significance, but party subconstituent change comes the closest ($p = 0.1$). I can therefore validate H_3 .

Table 5.3. Legislator Roll Call Ideological Change, 109th to 112th Congresses, Legislator Panel

	(1)	(2)	(3)	(4)
	Democrats	Republicans	Democrats	Republicans
First Difference: Constituent Ideology	0.001 0.000405 (0.0297)	0.069 0.0802 (0.0710)	-0.117 -0.0749 (0.0444)	0.091 0.106 (0.0852)
First Difference: Party Constituency Ideology			0.070 0.0584 (0.0574)	-0.151 -0.251 (0.158)
First Difference: District Heterogeneity			0.358*** 8.264*** (2.404)	-0.161 -6.056 (14.22)
First Difference: Party Constituent Ideology * Heterogeneity			0.276** 14.59** (5.590)	0.201 11.65 (22.71)
South	0.024 0.00361 (0.00495)	0.024 0.00467 (0.00799)	0.034 0.00516 (0.00496)	0.026 0.00492 (0.00804)
First Difference: Majority Party	0.008 0.000628 (0.00369)	-0.216*** -0.0244*** (0.00605)	0.004 0.000338 (0.00414)	-0.170** -0.0192** (0.00686)
Senior Member of Congress in 2004	0.025 0.00382 (0.00556)	0.038 0.0102 (0.00871)	0.034 0.00525 (0.00538)	0.033 0.00880 (0.00886)
Ranking Member of Committee	-0.022 -0.00413 (0.00625)	-0.121** -0.0327** (0.0102)	-0.032 -0.00600 (0.00624)	-0.120** -0.0325** (0.0102)
Retired	0.008 0.00216 (0.0110)	-0.114** -0.0384** (0.0121)	0.008 0.00228 (0.0104)	-0.113** -0.0383** (0.0122)
First Difference: Party of President	0.040 0.00508 (0.00613)	0.005 0.00109 (0.00959)	-0.094 -0.0118 (0.00745)	0.036 0.00722 (0.0153)
Lagged Two-Party House Vote	0.042 0.00977 (0.00982)	-0.030 -0.0131 (0.0215)	0.031 0.00711 (0.0100)	-0.031 -0.0137 (0.0214)
Challenger Quality	0.049 0.00901 (0.0114)	0.086 0.0217 (0.0120)	0.050 0.00911 (0.0115)	0.084 0.0212 (0.0120)
Competitive Primary	-0.005 -0.00167 (0.0188)	0.034 0.0166 (0.0232)	-0.009 -0.00280 (0.0191)	0.029 0.0142 (0.0240)
Observations	594	496	594	496
Overall R2	0.00614	0.0682	0.0342	0.0766

Standardized beta coefficients; unstandardized point estimates; standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

I validate the hypothesis, of course, with the caveat that first-differenced NP scores are stubbornly hard to predict when legislator is the panel variable. I identified a number of idiosyncrasies with NP scores that pose problems for inference when they are used outside of their original purpose (to explain party switching). To reiterate, NP scores first measures one-Congress-at-a-time preferences only *after* first modeling legislator preferences on a fixed ideal point model (viz., NOMINATE scores). Not surprisingly, NP scores are almost identical to NOMINATE scores and exhibit modest Congress-to-Congress variation for each single legislator. The fixed component of the NP estimation means that, for senior legislators, NP scores might capture constituent effects in long ago legislative sessions. Contemplating this possibility, the appendix to this chapter includes the legislator panel regression with an alternative measure of legislator ideology: adjusted ADA (Americans for Democratic Action) scores. ADA scores are a popular interest group rating, based on a scorecard of twenty yearly votes that the group's Legislative Review Board deems salient over a wide range of liberal causes. The deflationary adjustment described by Groseclose, Levitt, and Snyder (1999) minimizes the effects of shift and stretch between Congresses and places the numbers on the same scale. Because ADA scores are reverse-coded in relationship to my constituent effects, we expect the latter's regression coefficients to be negative. Table 5.4 displays the results, and validates H_3 for Democrats, though shows that Republicans exhibit greater median than subconstituent responsiveness behavior. Constituent effects may explain more variation in the ADA scores than in the NP scores owing to greater House responsiveness on salient, high-visibility votes, but I do not here explore this possibility. My aim is to explain responsiveness on all votes, which is why I prefer Nokken and Poole's scaling estimation to interest group scores.

Discussion

The Median Voter Theorem has long been the standard explanatory framework for democratic representation. However, scholarship has long struggled to find evidence that House Representatives respond to the median voter in their constituencies. The thrust of my dissertation is that all previous research has failed to generate longitudinal estimates of the ideal points of district voters, calling their findings of either responsiveness or non-responsiveness into question. Exit-poll-imputed measures of public opinion, on the other hand, are valid, reliable, biennial estimates of known voter attitudes.

This chapter has presented a snapshot of House responsiveness unencumbered by the usual methodological qualifications. Generally speaking, candidates and legislators in the last decade did not greatly respond to the district median voter. How does one square this finding with the longstanding consensus that MCs are beholden to public wants (Mayhew 1974a)? The answer is that, against the traditional spatial analogy of politics, not all wants are equal. Academic and non-academic treatments of Congress have long suspected that representatives take only the attitudes of certain voters, particularly their ideological supporters, into account. A competing spatial model of elections—directional voting—explains why this is the case: there is an electoral incentive to respond to extreme opinions, particularly when the geographic constituency is diverse and problematic to represent. (Primary challenges, an alternative explanation for candidate extremism, did not greatly predict roll call voting relative to constituent effects). Previous research has not found much leverage in testing this prediction, confining itself to cross-sectional studies of the House (Clinton 2006) or Senate (Kastellec et al. 2015). Fortunately, an extension of MRP allows me to estimate partisan subconstituencies in each year and district. My use of the subconstituency

measures shows that, vis-à-vis the MVT, directional voting better captures the constituent component of representative behavior.

Therefore, even controlling for electoral safety (the “vanishing marginals,” or proliferation of uncompetitive elections that may allow MCs to get away with extreme voting behavior) and primary competition (yet another explanation for party polarization is the centrifugal pull of primary elections), representatives are especially keen to respond to partisan supporters. The “electoral connection”—the transmission of mass attitudes to those of policy elites—is skewed in favor of partisans and not district majorities. Changes in extreme subconstituent attitudes predict corresponding changes in legislative ideology, such that any policy the House enacts is likely to be more extreme than that of the national policy mood. Per the expectations of directional voting, candidates appear the most likely to respond to party subconstituencies in heterogeneous districts. This means that smaller, rural, and generally Republican states with few or at-large Congressional districts—heterogeneous because they are less likely “packed” with co-partisans like gerrymandered urban jurisdictions—will elect the most extreme legislators. They will do so even controlling for constituent public opinion in these districts.⁹ Homogenous districts, on the other hand, tend to be concentrated in larger states where state legislators have a freer hand in gerrymandering district lines simply because there are more of them to draw. In such districts, candidates and legislators less likely to respond to partisan subconstituencies vis-à-vis the median voter. (In these districts, of course, the median voter is at a close spatial distance from the median partisan voter, meaning that the extra representation here afforded to median voters is not great).

Such findings pose normative challenges to students of the House of Representatives. The institutional design of the Constitution presupposes that the House is the locus of popular

⁹ As an aside, I can thus hypothesize Senators who represent inherently more diverse constituencies than House members will exhibit greater shirking behavior from the median voter than do the latter.

sovereignty. Based on *Federalist* 10's theory that majorities if unconstrained will tyrannically expropriate the rich, the Madisonian design of the Constitution constrains the power of electoral majorities in the House of Representatives.¹⁰ Any good high school civics student knows about such institutional remedies as divided sovereignty and enumerated powers. New institutionalist theories of political power, however, predict largely the opposite of Madison's whiggish predictions: namely, that small, geographically-concentrated, and intensely passionate minority groups will mobilize the anti-majoritarian biases of American politics for their own undemocratic ends (Olson 1965; Schaatsneider 1967). Likewise, Bishin (2009) has shown that identity groups who are emotionally invested in and highly informed about a particular policy outcome, for instance the Cuban-American population in South Florida that sustained a commercial embargo on Cuba for many decades, will subvert the preferences of the majority of non-invested and uninformed median voters. As Downs (1957a, 257) put it in a forgotten passage that predates subconstituency theory, "this inequality of information always results in a corresponding inequality of influence over government policy formation." I have empirically extended this insight into the House of Representatives, and to the entire corpus of American lawmaking. Concentrated partisan subconstituencies appear to influence roll call voting *generally*. Thus, in general, and not merely on a specific and time-bound policy issue, Congress fails to meet Miller and Stokes' straightforward expectations of democratic representation.

Finally, my findings speak to the party polarization debate. This debate, naturally, tries to explain why since about 1980 the voting behavior of Democratic and Republican House delegations have polarized, such that party overwhelmingly predicts a given legislator's roll call

¹⁰ As Robert Dahl consistently reminded us, the rhetoric of the *Federalist* is far from representative of James Madison's true beliefs, which were more sympathetic to popular sovereignty than those of many early supporters of the Constitution.

activity. Institutional explanations for the trend, such as rule changes in the 1970s that moved the center of gravity of policymaking away from committee chairs and towards party leaders (Aldrich and Rhode 2009), compete with public opinion explanations. Broadly speaking, the two dominant public opinion explanations are that polarization is mainly due to a polarized public electing extreme candidates (Jacobson 2010; Abramowitz 2010a) or mainly due to a polarized political class subverting the will of a silent majority (Fiorina, Abrams, and Pope 2006; Fiorina and Abrams 2009). While it is undeniable that American public opinion has grown more divided—indeed, Chapter 4 showed that the distribution of ideology change in the Congressional districts is bimodal, with extreme districts pulling in opposite directions between election cycles—the results of this chapter absolve district median voters from responsibility for Congressional ideological change. Instead, partisan district minorities are the most important constituent input into the political system. Moreover, inasmuch as there is any evidence that incumbent legislators monitor and react to public opinion, it appears they monitor the preferences only of their supporters. (For the most part, however, constituents express themselves via the replacement of incumbents in elections).

My conclusion will develop the implications of my findings, as well as suggest caveats for my findings and directions for future research.

Chapter 6. Representation in an Age of Partisanship

What I Have Done

My dissertation began with a simple question: do members of House of Representatives respond to their constituents' preferences? A quality representative, the conventional logic of American democracy goes, considers the views of all constituents in her geographic constituency when casting at least a portion of their roll call votes. Congress scholarship has historically offered few clues as to whether this straightforward expectation holds true. In contrast to more academically specialized inquiries—for instance, are ranking members of House committees “preference outliers” vis-à-vis the floor?—, the responsiveness puzzle has received comparatively less direct examination in the literature, with scholars preferring to study related but distinct cousins of responsiveness like “policy congruence” (Hayes, Hibbing, and Sulkin 2010, 91). Policy congruence may tell us if voters elect candidates who share their traits (Burden 2007). Only responsiveness can tell us, however, if the House conforms to the norms of democratic representation that theorists have set for it, and consequently whether citizens can effectively participate in the decision-making that affects their lives (Dahl 1971).

A data problem has hamstrung the discipline's capacity and willingness to tackle responsiveness. In Chapter 2, I reviewed the House representation literature as it has unfolded since the Behavioral Revolution of the 1960s. The dominant conceptualization of representation in this literature is dyadic responsiveness, which lends itself to a simple empirical test: do the views of House district constituents and House legislators covary when placed on the same scale? Unfortunately, responsiveness scholars have been unable to test the phenomenon on their own terms. Measuring the constituency half of the constituency-legislator dyad has proven prohibitively difficult. The various proposed ways around the problem—survey disaggregation,

the use of demographic or electoral proxies to stand in for district ideology, and simulations to combine these approaches—suffer from disabling limitations. For most of its history, House responsiveness literature has thus been forced to rely on static, unreliable, and conceptually invalid measures of house district ideology. Whether this caused the discipline’s seminal classics, for instance Miller and Stokes (1963) and successor pieces, to overestimate or underestimate House responsiveness is a matter of debate. Their overall veracity, however, is questionable.

In the last ten years, multi-level regression and post-stratification (MRP) has become the accepted method for imputing district-level public opinion. MRP combines survey disaggregation, Bayesian inference, and demography to impute the average survey response in a given jurisdiction. Study after study has shown that MRP outperforms every other known estimation of state and district public opinion (Lax and Phillips 2009b; Warshaw and Rodden 2012). Should this be the end of the story? Not content to let Political Science rest on its laurels, I answer in the negative. MRP-imputed measures of House district ideology that have surfaced in recent years suffer from three limitations. First, they are static and cross-sectional, and assume that district public opinion does not change between election cycles in the face of suggestive if inconclusive evidence that it does. Second, they aggregate the attitudes of individuals and not *voters*, when responsiveness literature agrees that voting is the normal mechanism by which attitudes translate themselves into representative behavior. Finally, third, MRP studies have narrowly focused on individual issue attitudes rather than public opinion as whole, which means they do not tell us much about responsiveness in general, and in a context of agenda instability.

Chapter 3 offers my alternative approach. I run MRP on symbolic ideology—a superior variable, I argue, than issue attitudes from the standpoint of responsiveness research—using concatenations of state and national exit polls from 2004 to 2010. The massive sample sizes of the

exit polls—averaging to roughly 60,000 cases per election year—mean that I can impute longitudinal measures of district public opinion that capture ideological change. The fact that exit polls interview individuals as they leave the booth on election days mean that there is no ambiguity about whether they are voters, and that they are a clear snapshot of public opinion: there is no question of collapsing survey responses over many months or years. Finally, the strong probability structure exit polls makes is a superior choice over the Cooperative Congressional Election Study (CCES), which relies on an online survey mode whose limitations are not yet fully understood (AAPOR 2010).

To compute district ideology, I calculate the attitudinal propensities of various demographic and geographic groups in the exit poll. I then weight these propensities by their distribution in a given congressional jurisdiction per the new American Community Survey. Did I succeed in the effort of producing a valid measure? To answer, I adopted the methodological commonplace that no measures are valid unless they tell us what we already know with certainty. Thus, I tested the cross-validity of exit poll ideology, finding them highly correlated with accepted measures of district-level policy mood and partisanship, as well as with House and presidential vote. Districts with the most extremely conservative and liberal estimates correspond to pre-theoretical expectations, for instance in that they are solidly partisan districts known to be products of gerrymandering. I then tested the reliability of the exit poll measures by comparing the within-district variation of exit poll respondents with between-district variation. I found that individual responses with districts are relatively homogenous, which O'Brien (1990) would take as an indication that my aggregate measures are reliable.

Chapter 4 examines the phenomenon of ideological change in the districts. This is a topic worth interrogating for two reasons. First, the literature contains no clear theoretical

expectations—let alone empirical conclusions—about whether the attitude profiles of districts change between redistricting cycles. Second, if district-level ideology is variable, then the recent tendency of MRP scholarship to impute a single measure of district ideology using surveys conducted over many years gravely requires revision. How do I test for jurisdictional ideological stability? This methodological question has inspired a spirited debate in the literature, with Brace et al. (2004) advocating a hard test that controls for national public opinion and Berry et al. (2007) favoring an easier test. I opted to use the harder test, which entails regressing ideology on year and district dummy variables, then using a scores test to see if the error term in each district has a longitudinal component. If this the case, then the aggregate ideology of the district has undergone change independently of the rest of the country.

The results of Chapter 4 show that there was heterogeneous ideological change in the districts in the 2000s, meaning that district attitudes did not track national swings in public opinion. At 90% confidence, over 100 districts exhibit change. Change was bimodally concentrated in extreme and homogenous districts, particularly majority-minority districts (which became more liberal) and rural conservative ones (which became more conservative). The finding that many districts exhibit this behavior is surprising, given that time series features three wave elections that should pull the districts in a uniform direction, and is a boon to observers who blame public opinion for polarization in Congress. What explains this phenomenon? This is an even harder question to answer than *whether* there is change; the state policy literature, for instance, has found that the only states that exhibit unambiguous historical ideological change since World War II share few commonalities. I found that demographic and socioeconomic change—particularly changes in unemployment, education levels, and crime—appears to predict ideological change in House districts. A longer time series is necessary to make a certain and generalizable determination about

the source of district-level change. Nonetheless, I offer a first attempt at solving the puzzle of subnational ideological change. Future research will doubtlessly build on my approach in this chapter.

Finally, Chapter 5 speaks to the MRP measures' *raison d'être*: the question of House responsiveness. If any American political institution responds to the will of voters, I argue, it should be the House of Representatives: House legislators represent small, usually homogenous electorates who express their wills at frequent intervals. Despite this, Congress scholarship has been unable to reach any consensus about the nature and extent of constituent control in the House. Findings of democratic representation are typically modest and qualified (Bishin 2009, 9), particularly when entire corpus of House legislation is under examination (Burstein 2006). To formulate a test of responsiveness, I turn away from empirical findings and towards formal economic models of voting. This first of these is the venerated but oft misunderstood Median Voter Theorem (MVT), which predicts that the median district preference will govern the outcome of every election if all voters select candidates on a single spatial dimension (for instance, symbolic ideology). Candidates will then spatially locate themselves near the median voter, thus capturing the votes of the largest numbers of voters and behaving responsively. The MVT offers rosy prospects for the democratic legitimacy of elected institutions. The newer Direction Voting Model (DVM), on other hand, paints an arguably darker picture of civic participation. This model supposes that voters do not care about proximity of candidates to themselves, but rather the extent to which candidates are credibly and intensely on the same side of the ideological spectrum. If this is the case, then candidates (and subsequently legislators) should respond to their most extreme supporters, particularly partisan subconstituencies, at the expense of electoral majorities. This should be doubly true in ideologically heterogeneous districts, where the median voter's preference

does not describe the central tendency of district ideology. This is precisely the expectation of many classics of Congressional scholarship (Fiorina 1973), as well as an emergent literature embedded in subconstituency theory (Bishin 2009; Bishin and Smith 2013).

In Chapter 5, I regress accepted measures of candidate positioning and House roll call ideology on MRP-imputed measures of district median voter ideology and party subconstituency ideology. I find that subconstituency effects, in particular the interaction of subconstituent ideology and district ideological variance, better predict changes in elite behavior than do the effects of geographic constituencies. Democrats appear to respond to subconstituent pressure to an even greater extent than do Republicans, the inverse of previous findings in House responsiveness literature (Clinton 2006). The short time series nonetheless cautions me against making sweeping inferences about between-party differences in representation. What is clear is that member replacement in elections is the primary mechanism by which Congress uptakes the preferences of party subconstituents, an insight consistent with what we already know about the sources of polarization in the House (Theriault 2006). However, the legislator panel regression showed that—inasmuch as MCs do change their minds—changes in party subconstituent preferences predict changes in roll call behavior to a greater extent than most institutional variables. This finding contributes to the body of evidence that members of Congress monitor public opinion between elections in order to forestall potential electoral challenges (Arnold 1990; Bartels 1991).

Should we trust these findings? I argue yes, for two reasons. Because I am looking at representation across all roll call votes, where the literature has found constituent effects to be less detectable than in studies of (say) state policy mood, and because I operationalize legislator ideology with measures that either do not vary over the course of the legislator's career (Adam Bonica's Campaign Finance [CF] Scores) or vary little (Timothy Nokken and Keith Poole's one-

Congress-at-a-time NOMINATE scores, or NP scores), I am confident that I have executed difficult tests of responsiveness. That is, by design, my dependent variables are only partly sensitive to constituent effects, minimizing the risk of type I error. When I validated my results by regressing constituent effects on inflation-adjusted Americans for Democratic Action [ADA] scores, a more variable but less comprehensive estimation of legislator preferences, I found similar constituent-elite relationships with higher levels of statistical significance.

Having established confidence in my substantive conclusions, I turn now to their implications for 1) democratic theory and 2) the empirical measurement of constituent and elite attitudes. I conclude with a discussion of the directions my project offers to future scholarship.

Implications for Democratic Representation

A cornerstone of traditional approaches to representation is that MCs respond to the preferences of constituents equally, and logically attend to majority instead of minority wants. In Tocqueville's account (2000 [1833], 103), majority public opinion—stubborn, inflexible, and potentially despotic—reigned supreme in early America, to the point where it governed not only legislative behavior but also the manners and mores of ordinary people:

The idea that the majority possesses the right, by its enlightenment, to govern the society was brought to the soil of the United states by its first inhabitants. The idea, which was alone sufficient for creating a free people, has today passed into the mores, and is visible even in the smallest habits of daily life.¹

Dahl (1956), while critical of the pure majoritarianism that fascinated and worried Tocqueville, believed that if representatives disproportionately responded to certain interest groups, then

¹ Tocqueville provided few specific examples of legislative responsiveness at either the state or national level. He mainly pointed to the penal code, for instance the harshness of penalties towards sexual offenses. This lacuna is forgivable because public opinion polling did not yet exist: he wrote in 1840 that the only thing harder than measuring changes in public opinion was for stubborn Americans to change their minds in the first place. In other words, he anticipated the data problem afflicting studies of House responsiveness by more than a century.

minority tyranny would replace majority tyranny as the central problem of American institutions. Unequal access to the legislative bodying governing state power threatens every normative theory of representation, however conceptualized (Mansbridge 2003).²

Steeped in Tocquevillian idealism, Political Science was once optimistic that equality and majoritarianism characterized constituent influence in the House. Miller and Stokes (1963) ambitiously set out to test—in Hanna Pitkin’s words—whether representatives “act in the interests of the represented in a manner responsive to them” (Pitkin 1967, 209). Both Pitkin and empirical researchers agreed that such substantive representation was the most important metric of a legislature’s legitimacy. Everyone, after all, wants to be governed by representatives, and all social movements claim to be representative in some way (Pitkin 1967, 2). Miller and Stokes confirmed their own strong expectation that member recruitment (constituents elect representatives like themselves) and the electoral connection (members fear the loss of their careers, the punishment for shirking) would easily guarantee responsiveness.

Unfortunately, a half-century of null findings have curdled the optimism of both political theorists and political researchers. Dahl, famous for his insistence that American institutions were democratically responsive to a plurality of interests, later concluded that only radically participatory democracy could address growing concentrations of unaccountable wealth and power (Dahl 1990). On the empirical side, as methodologies outstripped in sophistication Miller and Stokes’ early effort, researchers found that representatives’ personal traits and institutional

² It is a simplification to equate democratic theory with a demand for total responsiveness. With the exception of Jeremy Bentham, no serious political theorist held that legitimacy means the mechanistic enactment of every majority preference into law (Sabl 2015). History abounds with examples where procedural and moral considerations other than popular sovereignty defeated or ought to have defeated majority desires. Be that as it may, it cannot be the case that a legitimate representative government pays *no* attention to majority wants and attends only to various subconstituencies. While strength in numbers does not confer an indefeasible moral authority on a political coalition, it does confer *some* claim (Muirhead 2006, 719), and in many instances the majority should carry the day. In other words, while responsiveness is not a sufficient condition for democratic legitimacy, common sense dictates that *some* level of responsiveness is a necessary one.

variables like party more powerfully explain elite behavior than do constituent attitudes, however measured. Past work has intimidated one of the reasons why this is the case: namely, that the assumption of equality is misplaced. Representatives appeal to only the most visible and attentive segments of the electorate (Miler 2007), for instance partisan subconstituencies (Clausen 1973; Fiorina 1973; Fenno 1978). Despite the partisan representation hypothesis' long history, few studies of the House have systematically tested it, Clinton's (2006) groundbreaking work with National Annenberg Election Survey (ANES) data excepted.

Here is where I enter the discussion. My findings are perhaps the first longitudinal (over several Congresses) and most precise (I have direct measurements of median party *voter* ideology) validation of the partisan representation hypothesis in the House of Representatives: the only constituent effects that influence elite behavior are those of co-partisans. Why is this? The simplest explanation is that individual voters prefer candidates on account of ideological direction and intensity, not proximity (Rabinowitz and MacDonald 1989). Partisans are doubly likely to discount proximity (Jessee 2009; 2010). Reacting to these conditions, candidates rationally adopt extreme views to increase the turnout of potentially enthusiastic supporters, at a minimal cost to the support of moderates (Adams and Merrill 2003; Peress 2011). The MVT's prediction that party platforms will converge does not hold as a result. While Downs allowed that "parties ... must create enough product differentiation to make their output distinguishable from that of their rivals, so as to entice voters to the polls" (1957b, 152), this cannot account for strong and growing substantive polarization at the level of House and Senate candidates (Cameron, Kastellec, and Park 2013). Once in office, reelection-minded MCs "filter" their responsiveness behavior by partisan identification (Kastellec et al. 2015). Polarization at the site of mass-elite linkage reverberates in policy. Since MCs are responding to the party median voter and not to the geographic median

voter, entire Congressional party delegations are likely to exhibit extreme party voting, and even narrow party majorities in the chamber will cause gridlock in conditions of divided government. District majorities, on the other hand, are denied influence over legislation: their votes do not shape or constrain the policies that affect their lives.

The partisan representation I identify violates the most important tenet of Robert Dahl's polyarchy model: that all groups have more or less equal chances of achieving control of policymaking. More recent conceptualizations of political legitimacy in the deliberative democracy school also work at cross-purposes with my dissertation. Deliberative democracy aims to make political outcomes more justifiable by channeling them through discussion characterized by equality, reciprocity, reasonableness, freedom from power, accountability, and a focus on consensus (Mansbridge 1999, 221-227). In contrast, parties are hierarchical organizations that have winning as their goal, make particularistic appeals that alienate large segments of the population, and inspire affective loyalties that render individuals unwilling to hear the other side or to reach a consensus about truth. It is the votaries of these nondeliberative parties who appear to govern what policy elites say and do.

My research points to a yawning gap expectation and fact. How might normative political theorists bridge it? I identify three possibilities: resignation, resistance, and restructuring.

Resignation. One longstanding reaction to the finding that policymakers are unresponsive is to give up on the expectation that representatives will attend to their constituents' wants equally. We might call this school "democratic realism." This school holds that expectations must be grounded in empirical certainties about the enduring nature of citizens. Because citizens are largely ignorant about policymaking, and because members of the House of Representatives—whom Tocqueville famously termed a "sea of mediocrity"—are equally ignorant about what the public

wants, genuine policy responsiveness is an almost impossible prospect (Wahlke 1971). Interest groups and ideologically extreme partisans are better suited to mobilize the elitist bias of American institutions and achieve their goals than are median voters of districts and states. Taken to an extreme, this line of reasoning would cast the once-vaunted concept of representation as utopian and as firmly belonging in the dustbin of history. In the words of Achen and Bartels (2016, 268), “abandoning the [conventional] theory of democracy is a prerequisite to both greater intellectual clarity and real political change.” Idealist-turned-realist Walter Lippmann took things even further and spent the 1920s arguing that scholars abandon the idea of public opinion *itself*, and not merely the idea that government ought to respect it..

Resigners leave us with few tools for evaluating democratic institutions and outcomes. What is Congress’s role, if not to meet the demands of the public? Maass (1983, 5), anticipating the deliberative democrats, held that Congress could not justify itself as a body that “simply aggregates and reconciles narrow group or individual interests,” and must instead through consensus formation make policy consistent with “broader community interests.” Unfortunately, the “common good” is not a helpful criterion in Political Science. Maass humbly declines to offer a determinative definition of the “common good,” instead claiming that Congress itself defines and advances it through organic legislative processes. More ambitious than modern observers was John Stuart Mill (1861), no skeptic when it came to the best way to live and govern. He held that the task of representative government should not be to make or implement law, but rather to act as an intelligence gathering arm of the Executive. Its tasks would include disinterestedly researching policy alternatives, overseeing Executive decisions, and enlarging the moral capacities of the citizenry by showcasing debates at a high intellectual level. Mill’s account of representation may remain unsurpassed in its scope and novelty, but the institutional purpose he would set for the

Legislative is incompatible with both the Constitution's investment of popular sovereignty in Congress and the basic tenets of American political culture.³ Less original and intelligent critiques might take the supposed impossibility of responsiveness as a case for insulating government from public opinion, and of placing well-informed elites in charge of policy. (Estlund [2009] uses the word "epistocracy" to reject this view). As Dahl's (1956) criticism of the *Federalist* notes, the primary effect of this move is to leave undisturbed hierarchies of wealth and power in society. In sum, the conceptual confusion left in the wake of renouncing responsiveness makes it an unattractive option.

Resistance. My findings, deflationary as they are in relation to traditional democratic theory, lend themselves to a totally different interpretation. We might resist the temptation to rethink the concept of representation. Sticking to this view, we simply acknowledge that American institutions require radical restructuring if they are to approach both theoretical and popular expectations for democracy. Nonpartisan redistricting, open "top-two" primaries like those recently enacted in California, and centralized party organizations able to discipline copartisan extremists are oft-proposed reforms to empower centrist electoral majorities vis-à-vis hyper-partisans (for an excellent overview over these "good government" remedies, see Persily 2015). Civic education that enables majority constituencies to monitor their representatives just as competently as intense subconstituencies, as well as higher voter turnout (via, say, automatic voter registration) that dilutes the latter's electoral influence, are other possibilities. Putnam (2000) thinks that "bridging" social capital at the community level will (through the nostrum of problem-

³ One might argue that the emergence of the so-called "Imperial Presidency" (which relegated Congress to simply administering the president's program for much of the 20th century), as well as the experience of post-communist nations (where the center of political gravity shifted away from parliaments and towards sprawling administrations overseeing admission to the European Union), proved Mill correct about the general incapacity of legislatures to govern directly. This is the subject for another dissertation.

oriented, face-to-face discussion) narrow the ideological space between partisan subconstituencies and the rest, and engender greater responsiveness at every level of government. While I have clarified the problem of non-responsiveness, evaluating these and other solutions the problem is a task beyond my academic and personal competence. Psephologists and sociologists can devote themselves to identifying the best and most practical ways to incentivize responsive, moderate candidate position-taking.

Restructuring. Partisan polarization, and by extension the phenomenon of partisan representation I found, have existed for at least two generations and have deep roots. Neatly reversing the trend and the distorting effects it has wrought on representation is not a likely prospect in the near future. Without surrendering the demand for responsiveness, democratic theory could still take steps to reconcile itself with today's partisan realities. By undertaking restructuring, it can close the gap between theory and fact, and make principled but realistic demands of legitimacy on extant institutions.

What might a chastened theory of democratic representation look like? For starters, it could take a more indulgent attitude towards partisanship in lieu of its typical blanket condemnation. Whereas observers usually interpret the disproportionate influence of “extreme” partisans as the dark side of American civic engagement (Fiorina 1999), we might allow that the sources of partisan influence—their high levels of engagement and sophistication relative to political independents (Delli Carpini and Keeter 1997)—are not signs of personal pathology, but rather virtues. Having done so, we see that “there is no *necessary* reason why the median voter-preferred policy is more ‘desirable’ than that advocated by same-party constituents” (Clinton 2006, 407, emphasis added).

Nancy Rosenblum's (2008) highly original attempt to reconcile partisan identification and democratic citizenship opens a new avenue for Political Theory. Instead of depicting party

platforms as violating the norms of public reason that alone confers legitimacy in a pluralistic society, Rosenblum acknowledges that they offer partial conceptions of justice that take the interests of many groups within parties' vast coalitions into account. If we follow her tack, the project of political representation changes. It absolves itself from the futile task of ensuring that policies conform to the individual choices of rational voters free of party affiliation, and embraces the challenge of working *with* (and not *against*) party constituencies so that they might reconcile their ideological intensity with norms of consensus-seeking. The hope is that partisans can discard the stereotype of the "true believer" and embrace a more reflexively-critical ethic of partisanship: passion tempered by deliberation. In this way, the preferences of the median voter and median party voter could be brought closer, enhancing democratic responsiveness in the House and Senate.

Rosenblum's defense of deliberative partisanship is not only consistent with my findings about the importance of party to voting and representation, but also has the advantage of moving pluralism—an ineluctable feature of every well-ordered society (Rawls 1993)—from the margins to the center of theorizing. This seldom-explored theoretical direction takes us on the road to a workable concept of representation in the age of contentious pluralism, and it is a road that in my view is the most promising of the three I delineate.

Shortcomings of Research

I turn now from the culture of theorizing to the science of empirical measurement. What does my dissertation say about attempt to quantify constituent processes in Congress? Recall that dyadic representation is interpretable as the relationship between demand—what constituents *want*—and supply—what elites *do* to meet demand. In this section, I will thus discuss the implications of my findings to measures on both sides of the equation.

The demand side. My findings tell us three things about received measures of public opinion: the need for longitudinal data, the need for more informative measures of public opinion than national policy mood, and (finally) the need for further efforts to solve the *n* problem.

First, the finding that district electorates are labile between elections militate against a near-universally accepted practice of MRP scholarship. Namely, it calls into question the use of static measures in studies of dyadic representation. Scholars have previously relied on static measures of district ideology to predict House voting behavior, or alternatively relied on redistricting-induced constituency change to incorporate dynamic variables into their analysis (Glazer and Robbins 1985; Leveaux-Sharpe 2001). The assumption of attitude stability is untenable. House electorates undergo changes in public opinion between redistricting cycles, particularly in the many districts in which gerrymandering “packs” partisan co-identifiers. MRP can therefore only definitively add to our understanding of representation if it can harness large enough *ns* to estimate district ideology for one Congress at a time.

Second, the finding the district ideology is not only labile, but also changes independently of national trends, problematizes popular measures of national public opinion. The heterogeneity of ideological change makes it hard to confidently speak of a single national policy mood. To be sure, Stimson (1999) would allow that there is wide geographic variance in public opinion, and that policy mood is simply a global measure that in effect averages state- and district-level opinion. However, I found that one of the key expectations of the mood literature was unfulfilled. Whereas the policy mood is supposed to act as a “powerful undercurrent propelling Americans’ views across a range of policy issues” (Best 1999, 721), this current did not propel most Congressional districts to undergo the same rate and direction of ideological change, even during three national wave elections and two majority party changeovers in the House. The district-specific dynamics

of ideology raise the specter of non-responsiveness in an era where, in order to strengthen their position vis-à-vis the Executive, MCs have appealed to national constituencies, emphasized non-local issues, and raised out-of-district campaign funds (Pietryka 2012). Future research should consider the possibility that the nationalization of campaigns has produced a democratic deficit in the House.

Finally, third, the thrust of my dissertation is that MRP literature has an *n* problem. Recall that previous scholarship typically imputes district-level measures using the CCES. Exit polls are superior because of their larger sample sizes, and because they are immeasurably more representative of electorates. Table 6.1, which compares the weighted frequency tabulations of presidential vote in Maine in the exit polls and CCES, illustrates an example of this advance. The exit poll sample is a reasonable approximation of the statewide electorate. In contrast, the CCES over-reports Republican presidential voters by 3%. Furthermore, its estimates fall within a vastly wide confidence interval. Despite the CCES’s goal of district comprehensiveness, measurement error is even higher at the district level—so high that the raw tabulations convey little useful information, which necessitates MRP in the first place.

Table 6.1. 2008 Two-Party Presidential Vote in Maine, Exit Polls and CCES			
	Obama (D)	McCain (R)	Total
Election Results	58.83%	41.17%	100%
Statewide Exit Poll	59.51% n = 969 Margin of error = ±4.79%	40.49% n = 553 Margin of error = ±6.35%	100% n = 1,522
CCES	55.94% n = 102 Margin of error = ±15.01%	44.06% n = 84 Margin of error = ±16.56%	100% n = 186

What does this mean for MRP imputation? It means that CCES-derived measures will, at the multi-level modeling stage, fit imprecise random slopes for states and districts given the

differences between the estimated electorate and the true electorate. The low *ns* also mean that the effects of MRP's probability adjustment, which incorporates jurisdictional demographics, will be great. In that case, the measures employed by the newest generation of MRP responsiveness literature bear similarity to the demographic operationalizations of district ideology that formerly dominated the literature; without larger *ns*, MRP represents only a partial advance over older approaches. Exacerbating the *n* problem is my substantive finding: the constituents that "matter" are subsets of the voting public, and estimating *their* attitudes place even greater requirements on the data than do district-level constituencies.

However, even the exit polls do not solve the *n* problem. Particularly during midterm elections and in large states like California and New York, concatenations of state and national exit polls still did not achieve adequate coverage in every congressional district. Some districts contained only a handful of cases. If we take as a given that no adequately large-*n*, comprehensive panel study of Congressional districts is ever to exist, where do we go from here?

Tausanovitch and Warshaw (2013) have put together the American Ideology Project, which builds on the renewed academic interest in sub-national public opinion. Their approach is to concatenate *many* datasets, for instance the ANES and CCES, and to generate ideology scores using Item Response Theory (IRT). Their ambition is at once greater than my research project (their interest is studying responsiveness at the municipal level) and less: they only generate measures by decennial districting cycle. While I trust that their data is valid and reliable, we have already seen that voter preferences systemically differ from the participants in such studies, and that constituent measures require year-to-year precision to capture dynamic responsiveness.

One solution may be to concatenate the exit polls with other datasets, but also add a twist that addresses the voter problem. Much as the NEP weights exit poll cases by actual election

returns, would it not be possible to fit models using CCES and ANES responses—and those in exit poll datasets as well—in a way that makes them representative of House district electorates? Model-fitting functions available for *R* contain options for prior weighting: that is, each respondent can be assigned integer weights to correct for under- and over-representation. This forces *R* to converge on a model that treats (say) an under-represented Maine Democratic respondent in the 2008 CCES (see Table 6.1) as ten cases, and an over-represented Republican as nine cases. In this way, where exit polls are lacking in geographic coverage or survey items (for instance, the exit polls do a poor job of including biennial measures of attitudes other than ideology), we might make phone and internet surveys shares the virtues of the exit polls.

The *n* problem, which has plagued Congress scholarship since the era of *The American Voter*, still persists. Even after fifty years, researchers have still not exhausted every avenue around the case deficit. The discussion above, I hope, illuminates some of those ways.

The supply side. My dissertation has explored the “demand” side of the responsiveness equation. Chapter 5 nonetheless intimated that measures on the supply side—that is, point estimates of what legislators *do* in response to demand—require more scholarly interrogation.

For background, my main dependent variable is derived from an extremely popular two-dimensional scale of roll call votes called DW-NOMINATE. Recent scholars have argued against using such general measures in tests of legislative responsiveness, arguing that they are incomparable with any conceivable measure of district public opinion (Bafumi and Herron 2010). It is better, they say, to compare individual roll call votes with individual constituent preferences. However, I have argued such criticisms overlook that issues usually have short lifespans. Issues that do persist still create difficulties for public opinion research over time. Moreover, it stretches credulity to think that MCs, who can neither predict changes in the inherently unpredictable issue

agenda nor poll their own constituents' issue attitudes, will know what specific policies their constituents want at all times. The best, generalizable test of responsiveness is to see if *many* roll call votes covary with the sum of district attitudes.

To perform this test, there is a need for measures of roll call voting that is at once comprehensive across issue domains, vary between Congresses, and are comparable across time. Interest group scores vary from Congress to Congress, but are not comparable over time (Groseclose, Levitt, and Snyder 1999). DW-NOMINATE estimates are comparable across all 114 Congresses, but are static for each legislator. NP scores aspire to solve this problem, and incorporate both variability and comparability. They nonetheless have flaws. The most glaring of these is that they capture roll call voting throughout legislators' entire careers: in technical terms, Nokken and Poole estimated one-Congress-at-a-time ideology on the assumption that fixed DW-NOMINATE scores—computed by scaling roll call votes on two dimensions all the way back to the First Congress and revealing binary preferences—explained the outcomes.

Three problems emerge when we start engaging in thought experiments. First, if a member entered Congress in the 1970s, then his NP scores are likely tainted by decades-old constituent effects. Even if public opinion is static, the frequent redrawing of districts means that senior legislators are responding to different sets of issue attitudes over the course of their careers. This calls into question whether NP scores are comparable to longitudinal measures of district ideology, which are far less stable than House roll call scores. Second, rules in recent Congresses have undergone changes, which have tended to increase party leaders' control over committee assignment and policy agenda. Long-serving legislators whose terms of office predate these changes once casted roll call votes in a remarkably different institutional context. This affects not only how they voted, but also what they voted on in the first place. The fixed component of NP

measures thus introduces measurement error, because not every roll call score is subject to the same institutional effects. Finally, third, there is the potential for agenda-setting bias. Take the issue domain of race as an example. A notable feature of Congress up until the middle of the last century was its systematic prevention of civil rights legislation from the reaching floor votes. The finding that Southern Democrats and Northern Republicans exhibited more liberal voting behavior than today's conservative Republicans, and hence that Congress has polarized in the last four decades, is perhaps in part an artifact of the fact that Southern Democrats like Senators James Eastland and John Stennis seldom had to vote against commitments to civil rights. The agenda problem is particularly problematic, since the imperative to avoid agenda-setting effects is one of the main reasons to prefer NOMINATE scores over less comprehensive measures of legislator policy preferences.

Perhaps a solution is to compute the NP scores using only roll call votes in (say) the past decade. Legislators could receive ideology scores that are comprehensive over all votes during a redistricting cycle. In this way, responsiveness research receives the best of both worlds: comprehensive measures of legislator ideology that span all policy domains and that are computed in the same institutional, public opinion, and policy agenda context.⁴ These measures would be a closer elite counterpart of my constituent ideological measures than currently available NP scores. Purged of extraneous roll call votes that antedate my time series, they would likely show greater sensitivity to changes in public opinion. Future research could test this expectation.

⁴ Keith Poole was generous enough to extend his help to my dissertation committee, but the quite unreasonable complexity and specificity of our request (for one-Congress-at-a-time NOMINATE scores that scale only those roll call votes from the 109th to the 112th Congresses) led to a communication breakdown.

Future Directions

In addition to improving the measures employed in the later chapters of this dissertation, how else might researchers build on my work? I identify three: clarifying the partisan electoral connection, broadening and deepening our understanding of exit poll datasets, and using MRP to build an integrated model of representation.

Clarifying the partisan electoral connection. I found that 1) representatives respond mainly to copartisans and 2) directional voting is a likely reason for their so doing. Future work is necessary to specify the precise conditions that give rise to partisan representation. I speculate that in the context of growing ideological constraint among partisan identifiers (Layman and Carsey 2002), a growing relationship between ideology and House vote (Jacobson 2013), and partisan redistricting that increases between-district and within-district polarization (Carson et al. 2007), there are ever-greater electoral incentives for legislators to respond to copartisans. Another possibility is that budget deficits and recent rules changes in the House—most notably, Congress’s elimination of earmarks and downsizing of the Appropriations Committee’s role—have undermined the ability of House incumbents to maximize their reelection chances via the securement of particularized material benefits to their districts. We might now wonder if symbolic ideological appeals to partisan subconstituencies have replaced the “pork barrel” as a the predominant incumbency-maximizing strategy: to paraphrase one Congressional staffer, MCs now offer constituents pure interest group scorecards instead of highway overpasses (Linkins 2014). If this is the case, then the decline of cross-pressured legislators (say, Democrats who represent majority Republican jurisdictions) is hardly mysterious. A task of Political Science will be to see just how well distributional politics describes or fails to describe the organization of Congress in the 21st century.

Primaries are an another popular explanation for polarization at the campaign and institutional levels. Projecting the travesty of the 2016 presidential nomination race onto another branch of government, political journalists have implicated a supposedly rising number primary challenges in the growing partisan divisiveness and gridlock of recent years (Rauch 2016). Primary challenges, a common strand of reasoning goes, force incumbents to appeal to tiny and ideologically extreme electorates before pivoting back to more moderate position-taking in the general election. Successful ideologically-motivated challenges, or the inability of incumbents to successfully “run to the center” after publicly committing themselves to a number of extreme positions in the primary, import extremism into roll call voting. My findings cast doubt on this common wisdom. Primary competition, operationalized as the nominee’s margin of victory in the primary, exercised no independent effect on candidate and legislator ideal points after I incorporated constituent effects in the models. Similarly, other scholars have failed to find a link between primaries and elite behavior (Hirano et al. 2010), or find in the historical record any evidence that House primaries are becoming more frequent and salient (Boatright 2013). Future research that incorporates MRP point estimates—does the interaction of MRP-imputed party ideology and primary competition affect candidate ideology?—may definitively expose what hand (if any) House primaries have in negative political phenomena.

Extending and broadening exit poll data. Even I have made the first systematic attempt to place exit polls in dialogue with Congressional literature, I concede that I have not made the fullest use of the data towards this end. For one thing, while state and national exit poll datasets with the *ns* necessary for biennial MRP estimation are available all the way back to 1984, they sadly do not contain usable district identifiers. Could it still be possible to extend the time series backwards? I think the answer is yes. As the datasets contain identifiers for precincts (randomly coded to

maximize anonymity) and state sub-region, someone with a surplus of computational talent and patience could compare the weighted precinct- and subregion-level frequencies of election results with published election returns. They could then approximately recreate congressional district identifiers. By expanding the public opinion data's scope, a researcher could speak to the responsiveness question with more generalizable, less time-bound answers than I now offer.

I also only used the general election exit poll datasets. I have therefore been limited to testing formal theories of voting in the general elections, a relatedly well-trodden path in the literature. Tantalizingly, historical primary exit poll datasets exist, and have received even less academic treatment than the November ones. These may offer a rare chance to directly measure the attitudes of primary electorates, which (as I previously remarked) many have held to exercise a distortive effect on campaigns and government. Are primary electorates systematically different than district and party median voters in the generals? Are these—and not the party subconstituencies operationalized in my dissertation—the real loci of candidate and legislator attention?

What of future election years? Unfortunately, when I say that exit polls are rich in research possibilities, honesty compels to add that they face sour prospects. New modes of early voting are becoming popular, meaning that growing numbers of voters are beyond the reach of exit poll interviewers. Because large numbers of people will have already voted before election day (Sproul 2008), and because the pool of undecided swing voters is shrinking (Berg 2012), the samples of phone polls conducted in the last week of the election cycle will increasingly resemble the electorate. Exit polls run the risk of becoming expensive redundancies. Highlighting this risk, the National Election Pool drastically reduced the sampling frame of the 2012 presidential election poll at the eleventh hour (Cohen and Clement 2012). Moreover, if and when Internet voting

becomes available, the entire survey mode will instantly become obsolete. Almost in anticipation of this phenomenon, experts see in new Internet panel studies and analyses of social media networks the future of public opinion research. Warren Mitofsky's high-tech and cutting-edge approach to reporting elections in the late sixties now strikes many people as stodgily old-fashioned.

The prospects of electoral research using the exit polls sound grim, but I still believe my project to be a beginning and not an end. Though designed for journalists, exit polls possess advantages to the social scientist that no other survey, including the CCES, possesses: large and frequent historical samples, selected using a robust probability structure, of known voters interviewed in person. Political Science has an interest in their survival and accessibility. My research has shown that, when combined with the power of MRP, exit polls yield important insights about democracy in the House of Representatives. Whether we learn all that this peculiar store of voter information has to teach depends on our own efforts.

An integrated model of representation. As I remarked at various points, the purview of responsiveness literature is limited. It only analyses a portion of what members of Congress do: how they vote. MCs make other decisions as well, including how active to be, and what issues to focus on (Hall 1996; Sulkin 2005). A complete theory of House representation must therefore embrace more than legislative ideology. Responsiveness is also an imperfect indicator of democracy, because it is possible to imagine perfectly responsive legislators who—in a political system where sovereignty is divided—enact policy at odds with what most people want (Eulau and Karps 1977). Future MRP research might impute the priorities of district electorates and ascertain whether legislator policy attentiveness in the form of bill sponsorships and committee assignments, and not merely their revealed preferences, responds to constituent preferences.

Scholars must also look at the output of Congress as a whole and examine, as past research has done at the level of statehouse politics, whether it is congruent with the demands of geographic and partisan voting constituencies. The definitive book on House representation in our still-young century remains to be written.

Appendix to Chapter 5

Table 5.4. Adjusted ADA Score Change, 109th to 112th Congresses, Legislator Panel

	(1)	(2)	(3)	(4)
	Democrats	Republicans	Democrats	Republicans
First Difference: Constituent Ideology	-0.043 -4.518 (4.383)	-0.137** -18.87** (7.079)	0.012 1.245 (7.057)	-0.298*** -41.15*** (9.064)
First Difference: Party Constituency Ideology			0.026 3.510 (9.108)	-0.166 -32.70 (18.29)
First Difference: District Heterogeneity			-0.411*** -1556.3*** (461.2)	-0.489 -2216.3 (1580.9)
First Difference: Party Constituent Ideology * Heterogeneity			-0.300** -2609.4** (898.8)	0.825* 5737.4* (2282.3)
South	-0.061* -1.513* (0.706)	-0.051 -1.151 (0.693)	-0.066* -1.643* (0.752)	-0.056 -1.273 (0.740)
First Difference: Majority Party	0.006 0.0773 (0.599)	0.096 1.292 (0.706)	-0.026 -0.324 (0.664)	0.081 1.089 (0.935)
Senior Member of Congress in 2004	0.043 1.084 (0.703)	-0.012 -0.381 (0.646)	0.037 0.917 (0.729)	-0.010 -0.320 (0.646)
Ranking Member of Committee	-0.009 -0.276 (0.752)	-0.008 -0.253 (0.590)	-0.003 -0.105 (0.770)	-0.015 -0.475 (0.649)
Retired	-0.122** -5.497** (1.924)	0.045 1.804 (1.717)	-0.123** -5.534** (1.919)	0.044 1.776 (1.698)
First Difference: Party of President	0.279*** 5.752*** (0.940)	-0.019 -0.441 (1.343)	0.411*** 8.473*** (1.304)	0.153 3.622 (2.218)
Lagged Two-Party House Vote	0.044 1.671 (1.466)	-0.010 -0.518 (2.102)	0.058 2.179 (1.486)	-0.013 -0.692 (2.117)
Challenger Quality	-0.030 -0.883 (1.315)	-0.015 -0.473 (1.128)	-0.028 -0.825 (1.300)	-0.015 -0.497 (1.128)
Competitive Primary	0.036 1.856 (2.625)	-0.033 -1.941 (1.898)	0.031 1.600 (2.747)	-0.041 -2.371 (1.657)
Observations	582	486	582	486

Overall R2	0.106	0.0228	0.133	0.0592
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Standardized beta coefficients; unstandardized point estimates; robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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