

Perceptions of Partisan Ideologies: The Effect of Coalition Participation

David Fortunato University of California, Merced
Randolph T. Stevenson Rice University

Recent scholarship in comparative political behavior has begun to address how voters in coalitional systems manage the complexity of those environments. We contribute to this emerging literature by asking how voters update their perceptions of the policy positions of political parties that participate in coalition cabinets. In contrast to previous work on the sources of voter perceptions of party ideology in parliamentary systems, which has asked how voters respond to changes in party manifestos (i.e., promises), we argue that in updating their perceptions, voters will give more weight to observable actions than to promises. Further, coalition participation is an easily observed party action that voters use as a heuristic to infer the direction of policy change in the absence of detailed information about parties' legislative records. Specifically, we propose that all voters should perceive parties in coalition cabinets as more ideologically similar, but that this tendency will be muted for more politically interested voters (who have greater access to countervailing messages from parties). Using an individual-level data set constructed from 54 electoral surveys in 18 European countries, we find robust support for these propositions.

After several decades of empirical research, there can be little doubt that voters in modern democracies can (and do) form perceptions of the policy positions of political parties on one or more abstract ideological dimensions (e.g., Barnes 1971; Fuchs and Klingemann 1990; Huber and Inglehart 1995; Klingemann 1972). Further, voters clearly use their perceptions of these positions (relative to their own position) in their electoral choices (Adams et al. 2011; Ansolabehere et al. 2001; Canes-Wrone et al. 2002; Erikson 1971; Erikson and Wright 1980; Markus and Converse 1979; Page and Jones 1979; Whitten and Palmer 1996; Wright 1978; Wright and Berkman 1986). Consequently, the question of how voters form and update their perceptions of the ideological positions of parties is central to both positive and normative accounts of how democratic accountability does and should work (Downs 1957; Hinich and Munger 1994).

The importance of this question is further heightened in systems in which the policy-making process is complicated by formal power sharing among parties. Indeed, the literature on cabinet coalitions consistently highlights the tension cabinet partners face between maintaining distinct partisan ideological identities and participating in the kinds of policy compromises that are necessary to govern in coalition. For example, Martin and Vanberg tell us that

compromise obscures the relationship between the policies a party supports as a member of the government and its “pure” policy commitments. As a result, participation in coalition has the potential to undermine a party’s carefully established profile. . . . (2008, 503)

Despite its importance, however, there is surprisingly little research that has directly confronted the

David Fortunato is Assistant Professor, School of Social Sciences, Humanities, and Arts, University of California, Merced, 5200 North Lake Road Merced, CA 95340 (dfortunato@ucmerced.edu). Randolph T. Stevenson is Professor, Department of Political Science, Rice University, P.O. Box 1892, MS-24, Houston, TX 77251 (stevenson@rice.edu).

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question of how voters form and update their perceptions of the ideological positions of political parties, especially in systems in which coalition cabinets usually form. Further, the work that has been done is often contradictory. Most problematic is the disconnect between empirical work on European political parties, which tends to find only weak, conditional, or even non-existent relationships between changes in voters' perceptions of the ideological positions of parties and actual changes in those positions (Adams and Somer-Topcu 2009; Tavits 2007; Adams et al. 2011, respectively), and an emerging body of work on U.S. voters that appears to show the opposite (Ansolabehere and Jones 2010; Carson et al. 2010; Levendusky 2009; Woon and Pope 2008).¹

In this article, we provide a wealth of new evidence on the sources of voters' perceptions of the ideological positions of European parties and, in so doing, suggest a simple resolution to the apparent disagreement in the American and European literatures. Specifically, we point out that almost all of the relevant research on European parties has measured change in the ideological positions of parties using measures of policy promises (usually from election manifestos), while the new research on the American case has used various measures of policy actions (e.g., actual votes in the legislature). In our view, this suggests the simple hypothesis that voters (in both Europe and America) respond more to the observable actions of parties than to party promises. Consequently, if the analysis of the perceptions of European voters were to change its focus from party promises to party actions, we may well find a link between party positions and voter perceptions that is more robust than that found in the previous literature.

Here, we provide a start on this project by asking whether European voters' perceptions of the left-right positions of parties respond to observable party actions, as appears to be the case with American voters. The answer to this simple question is complicated, however, by the fact that the multiparty coalitional systems of Europe present a very different information environment to voters than does the American case. Party discipline is high, recorded legislative votes are rare, and most of the policy-

making action occurs in the (often opaque) process of cabinet negotiations between coalition partners. This suggests that a European take on the basic idea that voters are moved more by policy actions than policy promises must account for the centrality of coalition cabinets in policy production in these systems. Further, not even the most optimistic study of the American case suggests that most American voters actually pay attention to the day-to-day process of legislative policy making. Instead, all the work connecting policy actions to perceptions posits some set of heuristics that stand between policy and the voter, helping them to make sense of the otherwise complicated process by which policy gets made. Similarly, we do not suggest that the typical European voter can (or wants to) successfully peer inside the black box of cabinet decision making. Instead, we argue that most voters understand that coalition cabinets essentially formalize a relatively broad-ranging and stable (in the medium term) policy compromise between parties. Thus, coalition membership itself provides voters with an almost costless, but generally accurate, heuristic to use in updating their perceptions of the policy positions of parties that join (and do not join) cabinets. Thus, the main hypothesis of this article is simply that voters will perceive parties serving together in the cabinet as more ideologically similar than otherwise identical parties that are not serving in cabinet together. This simple idea has never been systematically explored in the empirical literature; but, using a new data set that combines individual-level survey data from 54 elections in 18 countries from 1994 to 2004, we find very strong and robust evidence that it is true.

In the rest of this article, we first discuss the literature that both motivates the study and provides its theoretical direction. Next, we explain our theory of how voters form and update their perceptions of the policy positions of political parties. Next, we present our data, methods, and results. Finally, we discuss the implications of our findings for the two literatures that motivated them.

Motivation

Our theory and empirical analysis are motivated by intersecting literatures in comparative and American politics. In comparative politics, our work responds to the empirical literature on the impact of changes in party ideology on both party support and voter perceptions of the ideological positions of parties. This literature, which has almost universally used campaign promises (election manifestos) to characterize party positions, has produced only weak evidence that changes in party manifesto positions affect aggregate electoral support. In addition,

¹A small but important line of related work on European parties shows that voters slowly adjust their perceptions of the policy content of party ideologies (as opposed to the policy positions of parties) in response to broad, long-term changes in the political environment. For example, scholars have explored the process by which voters seem to have incorporated "new politics" issues into their understanding of the ideological profiles of European parties following the emergence of Green parties (Bauer-Kaase 2001; Knutsen 1995; Middendorp 1992). In our view, the long-term nature of this change is consistent with the distinction we make below between the results of research based on party actions versus that based on party promises.

these effects, when found at all, are delayed (Adams and Somer-Topcu 2009; Somer-Topcu 2009) and conditional (Tavits 2007). In fact, the most recent, relatively comprehensive, examination of the data (Adams et al. 2011) finds no systematic relationship between changes in party manifesto positions and either voter perceptions of those positions or electoral support. Interestingly, however, that study does find a strong connection between changes in the aggregate perceptions of the left-right positions of parties and aggregate support—a finding that mirrors the long-established importance of the perceived policy positions of parties on vote choice at the individual level (Canes-Wrone et al. 2002; Erikson 1971; Erikson and Wright 1980; Whitten and Palmer 1996; Wright 1978; Wright and Berkman 1986).

Taken together, these results suggest that there is a “break” in the representational chain linking European parties and electorates. While electoral support seems to depend on voters’ perceptions of the policy positions of parties, these perceptions have only weak, conditional, or even no relationship to changes in the parties’ advertised policy positions. This is particularly puzzling since it seems to contrast with recent work in American politics that has found strong links between changes in party policy positions and American voters’ perceptions of these positions.

Our response to this work in comparative politics is to suggest a move away from party promises as the source of voter perceptions of party ideology and toward observable party behavior.² This suggestion builds from both an older and a more recent stream of work in American politics. The older work has always doubted the power of party promises to move voters. V. O. Key is perhaps the most notable proponent of this view, famously arguing that

Voters may reject what they have known; or they may approve what they have known. They are not likely to be attracted in great numbers by promises of the novel or unknown. (1966, 61)

Others have echoed Key’s idea. Fiorina’s (1981) theory of retrospective voting explicitly adopts this position, as does most of the large literature on retrospective voting in

²Again, it is important to emphasize that the party behaviors that are even potentially “observable” are different across most coalitional and non-coalitional systems. Thus, we are not advocating the adoption of the specific measures of party behavior used in the American politics literature (e.g., analyses of roll-call voting) but rather a more general shift in conceptual focus away from party promises toward party behavior.

general (although, perhaps, more implicitly).³ Of course, if voters heavily discount party promises and instead focus on partisan actions in formulating their impressions of parties’ policy positions, this may well be reflected in exactly the kind of weak and empirically unstable relationship between promises and perceptions that comparativists have so far found. Thus, if we can focus our empirical analysis on actions rather than promises, we may yet recover a robust relationship between party ideology, voter perceptions, and voting behavior and finally establish the widespread importance of this straightforward mechanism of democratic accountability.

But what sort of “actions” matter to voter perceptions of party ideology? Other, more recent work on American politics offers one possible answer (though one that must be adapted to the particular circumstances of coalition government). Specifically, this work in American politics has challenged the decades-old common wisdom that voters are too ill-informed to reliably connect parties to their records of legislative activity (e.g., Stokes and Miller 1962). On one front, recent formal models of congressional policy making show that, even when parties are highly decentralized and party discipline is weak, it can be individually rational for legislators to vote in a way that helps to build a party “brand name” that communicates ideological information about the party’s candidates to uncertain voters (Snyder and Ting 2002). Further, a growing body of work simply takes the proposition that voters reward and punish parties for their record of legislative accomplishment as a basic assumption (Cox and McCubbins 1993; Kiewiet and McCubbins 1991).

On the empirical front, Hetherington (2001) has shown that public perceptions of the distance between the ideological positions of the Republican and Democratic parties increase as the votes of the members of the House delegations become more polarized (as measured by mean NOMINATE scores). Further, Woon and Pope

³Given the well-known irrelevance of American party platforms as a guide to the ideological positions of presidential candidates or congressional delegations, it is no wonder that scholars of American politics have not been lured into using party manifestos as a convenient measure of party positions. One important exception is Erikson, MacKuen, and Stimson, who use party platforms but emphasize that election platforms only reflect parties’ actual positions when smoothed over time since “past promises reflect past realities because parties do more or less follow up on their promises” (2002, 258). In a later section of the article, we explain how this notion of delay is consistent with our point that voters focus on a party’s actions rather than promises, for exactly the reasons expressed in the quote: if parties eventually enact policy that they previously promised and perceptions reflect this action in some way, then previous promises will be predictive of current perceptions.

(2008) show that American voters seem to understand not just the positions of parties, but also the extent to which the parties are unified or divided—as indicated by their legislative records. Likewise, they show that the electoral support of parties responds systematically to these features of the legislative record of parties. Overall, Woon and Pope conclude,

it is the congressional parties who produce the information in party labels through their legislative activities. (2008, 823)

Levendusky goes even further, suggesting that, taken together, the literature shows that

voters absorb the aggregate roll call record of the parties and use this to make relatively informed judgments about the parties' issue positions. (2009, 16)

Even more recently, work by Ansolabehere and Jones (2010) has supported such claims.

It is important to emphasize, however, that none of these authors believe that the typical voter actually follows the day-to-day policy-making activities of the legislature or knows the details of parties' legislative records. Instead, party labels (or "brands") summarize this information, providing a less costly, but generally informative, substitute for it (Lau and Redlawsk 2001).

Thus, our own explanation of how voters form and update their perceptions of a party's political ideology in coalitional systems begins, like the literature just reviewed, from the proposition that these perceptions depend—via a set of heuristics—on parties' legislative activities. In contrast to the American case, however, party labels are not the only widely available heuristic that summarizes ideological information about the parties. Instead, we argue that a party's status as either a member of the governing coalition or a member of the opposition is a very cheap, but generally informative, guide to the ideological movements voters can expect from parties in coalitional systems.

Theory

Since our concern in this article is with voters' perceptions of the policy positions of political parties, it is natural to begin with the assumption that change in these views responds, in some way, to the actual policy-making behavior of parties. As we suggested above, however, the cost of monitoring the ongoing policy-making process is likely

to be prohibitive for most voters. Thus, in updating their views of the policy positions of the political parties, many voters are likely to rely on a set of heuristics that allow them to make sensible judgments about how the positions of political parties are changing, without the need to pay close attention to news about the policy-making activities of parties. We think cabinet membership is one of the most important of these heuristics.

There is an extensive literature on the use of heuristics in general (e.g., Chaiken 1980; Kahneman et al. 1982) and in politics specifically (e.g., Kuklinski and Hurley 1994; Mondak 1993). One of the most studied political heuristics are partisan cues, in which voters substitute party labels for detailed knowledge about the ideological positions of candidates (Conover and Feldman 1989; Lodge and Hamill 1986; Rahn 1993). Most relevant to our article is recent work in American politics that has focused on the institutional sources of the information that gives partisan heuristics meaning (and makes them useful). Woon and Pope (2008), for example, show that the informational content of partisan labels in the United States is linked closely to the aggregate record of partisan policy-making activity and argue that it is the media, partisan elites, and interest groups who actually do the work of infusing partisan labels with its ideological content. Thus, what most voters get from the news is not a detailed understanding of the policy-making activities of candidates (or even parties) but rather a general sense of what the "Democrats" or "Republicans" have been doing lately—information that they seem to use to make an appropriate inference about the ideological positions of particular candidates. Our story about the institutional sources of the information in the cabinet membership heuristic in parliamentary democracies is quite similar to Woon and Pope's story about the institutional sources of information in U.S. partisan labels. Indeed, while they say:

To the extent that voters care about the ideological or policy positions of individual representatives but face uncertainty about the specific views and stances of the individual candidates, then a party brand name provides a natural shortcut because it conveys information about the set of candidates that run under it. (2008, 825)

We could state our main theoretical claim as follows:

To the extent that voters care about the ideological or policy positions of individual [parties] but face uncertainty about the specific views and stances of the individual [parties], then

[cabinet membership] provides a natural short-cut because it conveys [ideological] information about the set of [parties] that [are in the cabinet].

While much of the work on heuristics in psychology focuses on heuristics that lead to biased judgments and poor decision making (Kahneman, Slovic, and Tversky 1982; Nisbett and Ross 1980), the vast majority of work in political science assumes that heuristics persist because they are both “fast and frugal” and (more or less) reliable guides to the information for which they substitute (Gigerenzer et al. 1999; Lau and Redlawsk 2001; Popkin 1991). There would be little point, for example, to using party labels as a heuristic for candidate positions if, on average, co-partisan candidates did not hold similar views across a variety of issues. In our case, this suggests that heuristics that inform voters’ judgments about how the policy positions of parties are changing should be both cheap to obtain (relative to the cost of learning about the actual policy-making activities of parties) and relatively accurate (at least on average). In the next two sections, we explain why we think cabinet membership meets these criteria when it is used as a substitute for detailed information about the way the policy positions of parties are changing. In the final section of our theoretical discussion, we consider the likely nature of heterogeneity in the use of this heuristic and suggest one empirical implication that follows from it.

Cabinet Membership: A Widely Available and Cheap Piece of Information

The idea that information about the membership of cabinets in parliamentary systems is pervasive hardly needs justification; however, it is only recently that political scientists have begun to document the extent to which this information actually penetrates into the consciousness of citizens. The data that do exist reveal (rather dramatically) that knowledge of cabinet membership in parliamentary systems is widespread. For example, in the 1998 New Zealand National Election Survey, 74% of respondents were able to correctly identify National and New Zealand First as the parties in the governing coalition. Likewise, Fortunato, Stevenson, and Spiegelman’s (2011) surveys of voters in the week before parliamentary elections in six different contexts (between 2001 and 2009) show that voters are remarkably well informed about which parties are likely to get into cabinet.

Perhaps the best evidence on the question that currently exists comes from the 1998 Dutch Parliamentary Election Study, which asked voters to classify each of the

13 parties running in the election as members of either the government or opposition. The average success rate across all respondents and parties was over 90%. Even more remarkably, this knowledge, while not unresponsive to levels of political interest (as one would suspect), was very high even for individuals at the lowest levels of political interest. Table 1 summarizes these data, showing that an impressive 75% of individuals at the *lowest level of political interest* identified the cabinet status of at least 11 of the 13 parties correctly.

Overall, there is an increasing body of evidence that most voters in parliamentary democracies know the membership of the cabinet—even those who pay relatively little attention to politics. This gives relatively direct evidence that this kind of information is so pervasive that it must be almost costless to obtain. Thus, it should be an ideal heuristic to substitute for more costly ideological information about parties—as long as it actually contains some relevant ideological information.

The Ideological Information in Cabinet Membership

Why do we think cabinet membership contains ideological information, distinct from party manifestos, which would be useful to voters? There are three sources of evidence that push us in this direction: empirical findings in the cabinet formation literature relating ideological moderation (in the direction of one’s partners or potential partners) to the probability of cabinet formation, theoretical and empirical work suggesting that the actual experience of governing together creates incentives for policy accommodation and compromise, and empirical work on the legislative behavior of cabinet parties suggesting that once government parties have entered government on the promise of making specific policy compromises, they actively work to realize these promises in legislation.

First, various lines of research in the coalition formation literature point to the idea that parties are more likely to participate in coalitions together once they have actually moved closer together. For example, Martin and Stevenson (2001) as well as many other empirical models of government formation that have adopted their empirical approach (e.g., Martin and Stevenson 2010; Martin and Vanberg 2003; Proksch and Slapin 2006; Warwick 1996) show that the cardinal ideological distance between parties—not merely the rank order of ideological positions as in previous work—is a strong predictor of cabinet partnerships. Further, this rings true with qualitative accounts of coalition decisions within parties, like the German FDP, that periodically switch internal

TABLE 1 Ability of Dutch Voters to Classify Parties as Either Government or Opposition According to Their Level of Political Interest

Number of Parties Correctly Classified	Political Interest Score				
	Low = 0	1	2	3	High = 4
0–6 Correct	15.7%	4.6%	1.2%	.6%	.8%
7–10 Correct	8.8%	7.5%	3.1%	2.4%	1.7%
11–13 Correct	75.4%	87.9%	95.7%	97.0%	97.5%
Total	100%	100%	100%	100%	100%

Note: The political interest score was built from answers to four questions probing whether the respondent “reads the national news,” “talks about national news,” “reads about foreign news,” and is “interested in politics.” Respondents were asked to identify if each of 13 parties were in the government or opposition. The collapsed categories were chosen because they represented natural breaks in the distribution of responses. Numbers are column percentages for 2,101 respondents.

Source: 1998 Dutch Parliamentary Election Survey.

leadership between ideologically disparate coalitions.⁴ In these cases, the outcomes of factional struggles between competing ideological camps within the party often precede the decision of whether to coalesce with the left or right. In such cases, the choice of coalition partner is a signal of “true” ideological change within the party.⁵

A second source of evidence that leads us to posit that cabinet membership is a generally useful heuristic for inferring the direction of change in the policy positions of cabinet parties are studies that have asked whether parties that govern in formal coalitions together are more open to policy compromise than parties that are otherwise similar but not in formal coalitions (e.g., Ganghof and Bräuninger 2006; Goodin 1996). Such studies ask: what are the differences in legislative behavior one can expect from two parties that are the same on all rele-

vant dimensions except that in one case they are in a formal governing coalition and in another case not? The expectation of most such studies is that this difference in coalitional status will be consequential because

Where there is a formal coalition, collective agency has been created, and all parties to it will be judged at least in part by its successes or failures. Where, there is merely coalition-like governing [*i.e., two parties working together without being in formal coalition*], there is no collective agency and no shared responsibility. (Goodin 1996, 33)

Consequently, even if there are ideological compromises between opposition parties and governing parties that make sense, the opposition parties will find it difficult to claim credit for these policy changes and so, for electoral reasons, avoid them (Huber 1999; Scharpf 1997). Ganghof and Bräuninger’s (2006) study of Danish, Austrian, Finnish, and Australian cabinets provides the most comprehensive empirical examination of this idea, concluding that the evidence from these cases supports the proposition that parties in formal coalitions are more accommodating than otherwise similar, but uncoalesced parties.

Third, there is a growing line of research that suggests that cabinet parties strive to enforce and maintain policy compromises over the course of the government—successfully overcoming their partners’ incentives to “highjack” policy in those domains in which they have informational and resource advantages (e.g., those policy domains in which they control ministries). Martin and Vanberg (2004, 2005) and Thies (2001) have found that coalition partners utilize legislative institutions, such as committees or junior ministers, to prevent their partners from “drifting” from policy bargains. This is

⁴For example, Conradt’s (1993) textbook account of coalitional change in Germany emphasizes that policy change within the FDP was an important driver of the party’s withdrawal from coalitions with the CDU (1957 and 1966), joining the SPD in 1969, and switching back to the CDU in 1982.

⁵Another mechanism that can create ideological congruence between parties that form cabinets together begins with the observation that the senior members of most parties are, at least to some degree, ideologically heterogeneous. Thus, in negotiating to join cabinets, party leaders have a choice about which senior politicians to appoint to different cabinet positions—a choice that can be consequential both for whether a proposed cabinet can form and what policies will ultimately be produced. Indeed, when this idea is combined with the general result that parties whose ministers are more ideologically compatible are more likely to form cabinets together (Laver and Shepsle 1990), it implies that, even among the set of potential cabinets with the same partisan composition (*i.e., all the different arrangements of specific ministerial appointments*), the ones whose actual ministers are more ideologically compatible will be more likely to form. Some evidence for this idea comes from Bräuninger and Debus (2005), who estimate the ideal points of party factional leaders in Germany (for 1987–90 and 1998–2002) in order to show that the allocation of ministerial portfolios was compatible with this story.

important to our argument about the ideological content of the cabinet membership heuristic because it suggests that parties (contrary to a view of cabinets as a collection of autonomous ministers) will not be able to promise compromise to get into cabinet and then successfully avoid policy compromise once in office.⁶ Thus, a cabinet membership heuristic that assumes policy moderation and accommodation among the partners is consistent with the most recent evidence about what parties actually do in cabinet.

Taking these three sources of evidence together, we think it is reasonable to assume that cabinet membership contains information about the ideological movements of parties (and so can be used as a useful heuristic) because parties that get into cabinets actually move toward their partners in order to make the bargain work in the first place, because they follow through on the commitments they made to get into the cabinet, and because the process of governing together gives them new incentives to compromise with their partners. This, along with our observation that voters can easily learn who is in the cabinet, leads to the main hypothesis we will test in this article:

Hypothesis 1: Coalition partners will be perceived as more ideologically similar than non-coalition partners, all else equal.⁷

Heterogeneity in the Use of the Cabinet Membership Heuristic

While our main concern in this article is evaluating Hypothesis 1, we would be remiss if we failed to recognize that not all voters need to rely equally on heuristics. While we think that almost all voters will find cabinet membership a useful guide to the direction of partisan ideological

change among parties, there are two kinds of voters we do not expect to rely on this heuristic as much as the typical voter.

Most obviously, this heuristic is not useful for individuals who do not even know which parties are in the cabinet. While the evidence presented above suggests that such individuals are a minority of the population of Western publics, they may still be a sizable percentage. Such voters, however, will only add “noise” to the kind of data we examine in this study and so will simply make it more difficult to recover evidence of our hypotheses. Indeed, in the extreme case that the typical voter really did not even know which parties were in cabinet (which, again, we think quite unlikely given the evidence), then we would certainly not expect to find voters’ perceptions (or behavior) to be strongly conditioned on parties’ records of cabinet participation.

The more interesting cases are not individuals with little political knowledge or interest, but those with a great deal. Certainly, there are a sizable number of voters who are interested in politics, pay attention to policy debates, and monitor the outcomes of the policy-making process. Such voters may need to rely less on a cabinet membership heuristic and may instead tune their perceptions more precisely to the circumstances of a specific cabinet at a specific time. Certainly, numerous experimental studies in psychology and marketing support the general position that as the cognitive engagement and interest of subjects in a task decrease, reliance on heuristics increases (e.g., Chaiken 1980; Petty et al. 1983). Why does this matter? The reason is that, even though we think that a cabinet membership heuristic is a good guide to how cabinet parties change their ideological positions on average (i.e., over many cabinets), we certainly do not think that the heuristic will be accurate for every cabinet. To quote an anonymous reviewer of an earlier version of our argument, “. . . parties go into coalition governments for lots of reasons, and lots of things happen when parties are in coalition governments.”

Thus, while we might expect highly informed individuals to perceive cabinet parties as closer together on average (because, on average, they are closer together), their views of any given cabinet may deviate from this average view due to the specific circumstances of that particular cabinet (of which the highly informed voter is more aware).

It is important to point out, however, that the argument above (that better-informed voters rely less on a cabinet membership heuristic) does not imply the direction in which the perceptions of the politically informed should deviate from those of the less informed—it only implies that a cabinet membership heuristic will be a

⁶In Laver and Shepsle’s (1996) influential work, the stability of the cabinet itself is predicated on the ability of ministers to dictate policy on dimensions they control. If they also tend to control those ministries most salient to their supporters, then it is entirely possible that cabinet membership would reinforce ideological distinctions between partners rather than blurring the ideological lines between them. The work cited above, however, is incompatible with this view.

⁷The relevant comparison in this hypothesis (which we try to capture directly in the empirical research design described below) is between any pair of parties serving in cabinet together and any other pair of parties in which one or both are not in cabinet. We expect voters to perceive a pair of cabinet parties as more ideologically similar to each other than a pair of parties in which one or both are not in cabinet, even when controlling for several ideological and historical features of the dyad — including, but not limited to, their relative manifesto positions and history of co-governance.

generally less reliable guide to the perceptions of these voters on average. There is another argument, however, that gives a more precise prediction—suggesting that the well informed will systematically perceive cabinet parties to be more ideologically disparate than will less-informed voters who rely solely on a cabinet membership heuristic.

Specifically, recent work by Martin and Vanberg (2008) implies that more highly informed voters (those Zaller [1992] would describe as having high levels of “habitual news reception”) will regularly receive messages about the legislative activities of the cabinet parties that emphasize the ideological distinctiveness of different cabinet parties. In contrast, less-informed voters will not receive (or not pay attention to) these kinds of messages (and so, in our view, are more likely to rely on a simple cabinet membership heuristic to form their perceptions of the ideological positions of the cabinet parties).

This implication comes from Martin and Vanberg’s (2008) extensive new data set on legislative debates in Germany and the Netherlands. They use these data to show that (as elections approach) coalition partners use debates in the legislature to try to differentiate themselves ideologically from their partners. Thus, to the extent that voters receive and pay attention to such messages, they will tend to work against a simple heuristic that leads voters to perceive cabinet partners as more ideologically similar. The key point, however, is that not all voters are likely to receive and pay attention to these kinds of political messages. Indeed, it is hard to imagine the average voter ever paying attention to reports of legislative debates—or even that the average political story in the media would give them much attention. Instead, it is much more likely that these kinds of messages are found in more specialized accounts of party politics that are consumed by the politically interested—including interested party partisans, who are likely the intended audience. Even if, as Martin and Vanberg argue, the content of legislative debates reflects a more general effort by the party to communicate a particular message to voters via other communication channels (its website, generating news stories, public speeches), it is clear that those most likely to receive these messages will be those most interested and attentive to politics (Zaller 1992). Thus, if all voters rely to some extent on a cabinet membership heuristic, it follows that it is the more attentive voters who will be most likely to receive messages from cabinet parties that attempt to distinguish themselves from their partners—despite the policy compromises they may have made.⁸

⁸Martin and Vanberg (2008) give a particularly helpful example of how this works in practice. In their example, the German coalition partners (the SPD and the CDU/CSU) had compromised on

Hypothesis 2: The propensity to perceive coalition partners as more similar than non-partners will be diminished by higher levels of political interest.

Data and Analysis

In order to capture variation in voters’ perceptions of the ideological positions of political parties, we use a data set compiled by Stevenson and Vonnahme (2010) that brings together 54 election surveys conducted in 18 advanced democracies. These surveys each asked voters the following question (in essentially the same wording):

“In politics people sometimes talk of left and right. Where would you place yourself on a scale from 0 to 10 where 0 means the left and 10 means the right?”

This question was followed by a series of additional questions asking voters to place parties on the same left-right scale:

“Now, using the same scale, where would you place [name of party]?”

Our theory suggests that voters’ perceptions of the similarity of any two parties’ policy positions will depend on whether those two parties are in cabinet together. The most direct measure to use as our dependent variable is simply the absolute value of each respondent’s perceived distance between each pair of parties. Thus, we transform our survey data from the respondent level (one line per respondent) to the respondent-dyad level (one line per party-dyad per respondent) and ask what factors cause a voter to perceive the policy positions of any two parties as more or less distant. This design makes it easy to examine our main hypothesis directly by comparing (in a properly controlled statistical model) dyads in which both parties are in the cabinet to those in which either one or both the parties in the dyad are not in cabinet.⁹

unemployment and welfare benefit reform. Each party, however, used legislative debates and speeches to try to justify to the (highly informed) party faithful why the compromise did not actually represent an abandoning of party principals—the Social Democrats claiming that the bill did not represent a departure from their commitment to the working class and the Christian Democrats espousing the benefits of self-reliance and the need for state efficiency. Such subtle messages are simply not going to be received by most voters.

⁹Since the same dyads appear in different surveys (at different times), these comparisons include (as with any time-series

Structure of the Data

In developing appropriate empirical models with which to test our hypotheses, it is important that we understand the relatively complex, hierarchical structure of the data. Specifically, we have data on respondents, answering questions about parties, which are converted into party dyads, in as many as 54 election surveys in 18 countries (where the sets of parties either completely or partially differ across surveys). The dependent variable in all our analyses records how far apart each respondent in a particular survey perceives the positions of the parties in each party dyad included in the survey to be. This means that each respondent enters the data m_j times, where m_j is the number of dyads in survey j . Likewise, each dyad within a survey, or “survey-dyad,” enters the data n_j times, where n_j is the number of respondents to survey j . Thus, in the language of hierarchical data structures, the dyads and respondents are “crossed.” In addition, dyads are crossed with surveys. That is, each party dyad may appear in multiple different surveys (e.g., the FDP-SDP dyad appears in all the German surveys) and each survey has multiple dyads. Finally, surveys are nested in countries, since each survey applies to one, and only one, country.

This data structure leads to six possible sources of both measured (fixed) and unmeasured (random) effects on the dependent variable:¹⁰

- (1) *Country*: effects that vary over countries but that are constant over surveys, dyads, and respondents within a country
- (2) *Survey*: effects that vary over surveys but that are constant over dyads and respondents within surveys

cross-sectional data) comparisons across different dyads at the same time, the same dyads at different times, and different dyads at different times. Unfortunately, there are simply not enough dyads in the data that span different surveys (and that change their cabinet partnership status) to pull out only these cases and do a meaningful analysis of the “within dyad” variation in the data. That said, one of the authors (Fortunato 2012) has used panel data tracking Dutch respondents over time to show that respondents do actually change their perceptions of the cabinet parties over the life of the cabinet—systematically identifying cabinet parties as more ideologically similar the longer they have served together. This result on the individual-level dynamics of the process gives us some assurance that the cross-sectional (or “between dyad”) comparisons, which constitute the main source of variation in our data, reflect (at least in part) an underlying dynamic process of perceptual change that occurs as parties serve together.

¹⁰One could imagine a party level in which parties are nested in dyads. However, any factor one could attribute to a party (e.g., its size) can be similarly attributed to a dyad and captured at the dyad level (e.g., a dyad in which one party is of a given size and the other is of a given size). Consequently, there is no need to complicate the model further by adding this level.

- (3) *Dyad*: effects that vary over dyads but that are constant over respondents evaluating a given dyad (even if these respondents are evaluating the dyad in different surveys)
- (4) *Respondent*: effects that vary over respondents but that are constant over all dyads evaluated by the same respondent
- (5) *Survey-Dyad*: effects that are constant over respondents evaluating a given dyad, but that vary from survey to survey for the same dyad
- (6) *Dyad-Respondent*: effects that vary from dyad to dyad for the same respondent (when this is unmeasured, it is the “residual” error)

While this characterization gives all the possible levels of variation in the data, our specific focus on the voter’s perceived distance between pairs of parties leads us to discount the importance of some of them. Specifically, we are not greatly concerned that there are measured or unmeasured factors associated with countries that would systematically cause all respondents to perceive all dyads in a country (over all surveys) as either closer or farther apart. Indeed, it is difficult to come up with even one example of a variable that would have such an effect. In contrast, we can think of features of the survey administration or design that may make the task of placing parties different for respondents in one survey than the other and so may systematically impact how all respondents in a single survey place parties. Below, we identify and measure one such factor and discuss how to control for other possible influences at the survey level that may have gone unmeasured.

Likewise, our hypotheses identify two factors (that we will measure) at the survey-dyad level that we think will impact the way all respondents in a particular survey will place the parties in a given dyad (cabinet membership and the “true” distance between the parties in the dyad), and we can imagine that there are other factors at this level that may be unmeasured.¹¹ Similarly, there may be measured and unmeasured factors at the dyad level that systematically impact how similarly the voters perceive the positions of the parties in the dyad (i.e., that apply to a pair of parties in all surveys in which the dyad appears). These would tend to be more enduring characteristics of dyads (for example, if two parties are in the same ideological family).

Finally, there may be measured and unmeasured characteristics of individuals that would cause him or

¹¹For example, perhaps a party dyad at the time of one of our surveys has entered into an electoral alliance with another. If this is not measured, it will create correlations across respondents who will systematically perceive this survey-dyad as closer together.

her to systematically perceive all dyads as more similar or more different. Indeed, with respect to measured factors, our hypotheses suggest that political information is one of these factors (at least in interaction with coalition participation). Other measured characteristics of respondents that we have examined are gender, income, and others. That said, unlike models in which the question is whether perceptions of policy positions are accurate (Stevenson and Vonnahme 2010), thinking of individual characteristics that should have a systematic impact on the perceived similarity of all dyads in a survey is a challenge. For example, while previous literature on political knowledge strongly suggests that gender is an important control for a study of the *accuracy* of perceptions of party positions (with females generally having less accurate perceptions), we have no clear intuition for how gender would impact the perceived *similarity* of party positions. Given this, while we have included various measures of individual-level characteristics as controls in our models for the sake of robustness (available from the authors), we have not made the inclusion of a long list of respondent characteristics in the model a priority. Likewise, while we think there may well be some unmeasured factors associated with individuals that systematically impact how far apart they perceive the positions of all pairs of parties to be, we are not overly concerned that our results will be sensitive to how we deal with these particular unobserved characteristics statistically.¹²

Measured Variables

Our hypotheses require that we measure the respondent's level of political interest and whether the parties in a dyad are in cabinet together. Cabinet membership was taken from Strøm et al. (2008). Political interest is measured by the standard question probing how interested the respondent is in politics. It was asked identically in each survey in which it was included and has four re-

¹²One exception to this expectation concerns individuals who systematically answer the survey questions about party positions by giving all (or many) parties the same position (usually at the center of the scale). To the extent that voters with given characteristics (measured or unmeasured) are more likely to do this, these characteristics will be systematically related to more similar ratings for dyads. We have investigated this possibility as well as the impact of such responders on our results. Only about 6% of our responders appear to answer in this way. One option is simply to drop them, and our results are all robust to this approach. However, it is possible that the response is genuine—that is, it expresses the respondent's belief (perhaps in an overly dramatic fashion) that all the parties are the same. Given this, and because it is inconsequential for the results reported below, we leave these individuals in the data.

sponse categories (with higher numbers indicating more interest).

In addition to these theoretical variables, we include a number of control variables. First, we include a measure of the ideological distance between parties in the dyad as indicated by their manifesto promises. Controlling for their “manifesto distance” is essential given our theoretical argument that cabinet composition provides ideological information that impacts voter perceptions independently of manifesto promises. We measure this variable using the absolute distance between left-right positions of the parties in the dyad, which we calculate with data from the Comparative Manifestos Project (CMP) according to Lowe et al. (2011), using the election manifesto most proximate to the survey and rescaling appropriately to match the 10-point left-right scale in our surveys.¹³

In addition to manifesto distance, we also include a measure intended to capture the longer-term “true” policy positions of the parties. The idea here is that parties develop, over time, a particular policy profile that should have an important impact on voter perceptions. Our measure is a 10-year rolling average of manifesto positions. That is, we take the average of the left-right position from each manifesto published over the 10-year period leading up to our survey. We then take the absolute distance of these averaged positions as above. This measure has the significant advantage of being available for nearly every party in our sample. However, it is reasonable to ask, given the previous literature that has shown the disconnect between changes in manifesto positions and changes in perceptions, whether it is sensible to use manifestos to measure the parties' “true” positions. We think that it is reasonable to do so because the critique in the previous literature is that manifestos do not predict change in policy perceptions about the *same* parties compared over time—not that they are not broadly predictive of the relative absolute positions of *different* parties compared at the same time. There is no question, for example, that manifestos generally order the parties correctly,¹⁴ though there are certainly fluctuations in individual elections that can create odd, one-off orderings that no doubt

¹³We select the most proximate, rather than the most recent, survey because we are trying to use manifestos to capture a snapshot of the evolving policy message of parties at a given time—a concept better captured by the most proximate manifesto than the last published one (usually at the time of the previous election), which can be several years old by the time our surveys are in the field. Finally, rescaling of this control variable is not essential, but it does facilitate substantive interpretation of the results.

¹⁴Benoit and Laver (2006) and Lowe et al. (2011) demonstrate that their manifesto-derived measurements correlate quite well to measurements of policy position taken from expert surveys.

contribute to the negative finding for perceptions of change reported above (e.g., the rank-order violation between the British Labour Party and the Liberals in the early 1960s, which is discussed by Benoit and Laver 2007).

A third control, which we call “familiarity,” is measured at the level of the survey-dyad (i.e., it will vary for each party dyad in each survey) and captures the (discounted) history of joint cabinet participation for the dyad at the time of the survey. The idea is that for all party dyads, a recent history of joint service in cabinet may (by our own argument) have resulted in voters previously perceiving the parties in the dyad as closer together—an effect that could, plausibly, linger. To measure “familiarity” we calculate, at the time of the survey, the proportion of all months (since 1945) that the two parties in the dyad served in the same cabinet. Then, we discount periods of service that occurred previous to the time of the survey, with more distant periods of service discounted more heavily. The measure was developed by Martin and Stevenson (2010) and is described in detail in that article.

A quick look at the distribution of this measure (figure given in the supporting information), however, suggests that simply including it, as is, in the additive specification of controls is a mistake. Specifically, a histogram of the variable reveals that about 85% of the cases of this variable take on a zero value (i.e., the parties in the dyad have never been in cabinet together—or at least not very recently). This suggests that there might be a discontinuity between the effects of a zero value for this variable and anything positive (i.e., the parties in the dyad have some recent history of joint cabinet service). We can allow for this kind of discontinuity by coding a dummy variable that takes a value of zero when familiarity equals zero and one when it is positive. Including this dummy variable in the equation along with familiarity means that the total effect of familiarity for any case in which familiarity is greater than zero is the value of familiarity multiplied by the linear coefficient, plus the coefficient on the dummy variable—thus, allowing for a jump at zero followed by a linear function of familiarity after that.¹⁵ As it turns out (see the results below), exactly this kind of discontinuity is evident in the data. Finally, we allow for the impact of familiarity to be different for dyads in which both parties are currently in cabinet, both in opposition, or where one party is in and one is out.

Next, we include two controls at the respondent level. While above we explained that our focus on the respondent’s perceived *distances between* parties leads us to dis-

count the importance of individual characteristics that may systematically influence the manner in which voters perceive individual parties (e.g., gender, income, etc.), there is one exception to this intuition: the voter’s placement of herself on the left-right scale. Specifically, there are two ways in which we think this could impact the respondent’s placements of the parties.¹⁶ First, Stevenson and Vonnahme (2010) show that when respondents locate themselves between the parties in a given dyad, they are much more likely to correctly identify the left-right ordering of the parties. In that case, the fact that the voters can “look” from their own position and “see” one of the parties to their left and one to the right helps them to order the parties more accurately. Similarly, we suspect that when a voter places herself between the two parties in a dyad, she will have a natural tendency to place the parties farther apart than she would if both parties were to her left or to her right. Thus, we include a control variable which is coded one when the respondent is located between the parties in the relevant dyad and zero otherwise.

The second control variable that we derive from the respondent’s left-right self-placement is her ideological extremity. The idea here is that voters who place themselves on the extreme left or the extreme right will tend to see most parties as very different from themselves. Likewise, these respondents are the ones most likely to conflate the parties’ ideological positions on the left-right dimension with alternative dimensions (e.g., radical or anti-system versus not). While we do not have a strong intuition about the expected direction of the overall effect of extremity on perceived distance (i.e., one can think of plausible arguments for both a systematic positive and negative effect), we do worry that such respondents are sufficiently atypical that we need to be sure this is not consequential for the main point of this article. Consequently, we include this control.

Our final control variables address measurable effects at the survey level. Our data are the products of three different instrument administration types: (a) surveys administered by a surveyor in person; (b) surveys administered by a surveyor over the telephone; and (c) surveys in which the respondent self-administered. Though we have little intuition of how these administration types may affect the relationships we are testing, the relevant literature suggests that administration effects may be substantively significant and should be accounted for (see Schwarz et al. 1991 for a detailed discussion on these three administration types). Thus, we include dummy variables for

¹⁵Note that, by construction, simply including the dummy variable accomplishes what we want, without the need for an interaction term.

¹⁶Notice that in all cases, the respondents place themselves on the scale first, so this is formally a “pretreatment” variable and so properly included as a control.

telephone surveys and self-administered surveys, utilizing in-person administration as our baseline.

Finally, we confront issues with missing data in our measured variables by examining a number of different specifications.¹⁷ Specifically, when we include the set of familiarity variables, we lose a significant number of observations. Thus, we provide results with and without these measures. The bigger missing data problem, however, comes from our measure of political interest. While the theory clearly calls for a measure of how attentive an individual is to politics, including this measure reduces the sample from 54 to 38 surveys (and from 18 to 15 countries). In order to combat this problem, we provide results with an alternative, but certainly more noisy, measure of attentiveness to politics that is asked in all our surveys: level of education. Given the different samples created by these different specifications, we provide descriptive statistics for all variables, for each of the four samples, in the supporting information.

Unmeasured Variables

In hierarchical data structures of the kind described above, the usual approach to dealing with unmeasured factors at each of the levels of the hierarchy is to estimate statistical models in which one assumes that the combined influence of all unmeasured factors at each level of the hierarchy is constant for that level (i.e., it impacts all observations at the level in the same way) and can be described as a realization of an appropriate random error. One then assumes a distribution governing the error terms at each level (usually a multivariate normal) and estimates the parameters of this distribution to characterize the aggregate features of the errors at each level.

Such models have been used extensively in many disciplines. However, estimating such a six-level “random effects” model (with two crossed levels) on our several hundred thousand observations is not practically possible. Thus, we have to compromise. In particular, we estimate a series of different models in which we at-

tempt to deal with the correlation of the error term in the regression—caused by unmeasured factors at each level—in different ways. Specifically, we first estimate a series of regressions in which we use robust standard errors clustered on the groups of observations defined by the various levels of the hierarchy. This is not ideal. Since one cannot cluster on all the levels at once, in accounting for correlation between observations stemming from one level, one ignores correlation stemming from another. As Stevenson and Vonnahme (2010) discuss, however, this exercise is useful in that it can help pinpoint those levels of variation in the hierarchy that are most important to the estimates. That is, if one clusters standard errors on a group of observations defined by a particular level of the hierarchy and it changes the standard errors of the estimates a great deal (relative to a baseline without clustered standard errors), then one can conclude that this is a level for which one should be concerned about correlation. Obviously, if this is true for all levels, it does not help much; but, if it is only true for one level then one is more confident in concentrating on modeling that level using the more computationally demanding (and more correct) techniques of multilevel random effects modeling.

Results

Table 2 provides the results of our estimations using the four specifications discussed above and the strategy of calculating robust standard errors, clustering on different levels in the data hierarchy. Before turning to what the results say about the substantive hypotheses, several general observations are in order. First, there is a great deal of stability in the coefficients across different specifications, despite fairly drastic changes in sample size when moving from one to another. Indeed, there are no changes in sign in any of the parameter estimates across specifications.

Second, there is a very strong pattern in the standard errors that is essentially identical across specifications. That is, that effects at the survey level (recall that surveys are nested within countries) are most important in generating correlated observations. This is to be expected as this level will capture not only idiosyncrasies in survey administration, but, more importantly, characteristics unique to the party systems of the different countries included in our sample. Further, effects at the dyad/survey-dyad level are quite salient as well, exactly the levels that we suggested should theoretically be important to explaining variation in perceptions of similarity between parties in a dyad (indeed, the survey-dyad level is the one at which most of our theoretical variables are measured). Importantly, respondent-level correlation seems to have relatively

¹⁷The best approach for handling missing data is to use multiple imputations. The problem in our case, however, is the multilevel structure of our data. It is not at all clear how one should impute data collected from multiple surveys in different contexts, especially when the focus of the analysis is the impact of a contextual variable (coalition membership) on individual behavior. Indeed, this is an open area of research in statistics. At the very least, we know that all the variables at both the individual level and the other higher levels would have to be included in the imputation model, but since some of the higher-level variables are missing as well, the existing literature provides no solutions. Efforts to address these issues, however, are underway (e.g., Gelman, King, and Liu 1999; Yucel 2008).

TABLE 2 Error Clustering Experiment

Variable Name	Coefficients (Standard Errors)			
	Model 1	Model 2	Model 3	Model 4
10-Year Average CMP	.333	.407	.448	.549
Distance	(.005, .080 ^s , .066 ^d , .071 ^{sd} , .007 ^f)	(.005, .076 ^s , .060 ^d , .064 ^{sd} , .007 ^f)	(.004, .080 ^s , .058 ^d , .063 ^{sd} , .006 ^f)	(.004, .088 ^s , .053 ^d , .059 ^{sd} , .006 ^f)
Most Proximate CMP	.336	.295	.273	.200
Distance	(.005, .119 ^s , .070 ^d , .064 ^{sd} , .008 ^f)	(.004, .123 ^s , .066 ^d , .061 ^{sd} , .008 ^f)	(.004, .102 ^s , .062 ^d , .059 ^{sd} , .007 ^f)	(.003, .105 ^s , .054 ^d , .053 ^{sd} , .006 ^f)
Coalition Partners	-.881	-.731	-.731	-.583
Distance	(.033, .196 ^s , .149 ^d , .146 ^{sd} , .036 ^f)	(.032, .183 ^s , .153 ^d , .153 ^{sd} , .035 ^f)	(.024, .141 ^s , .130 ^d , .130 ^{sd} , .028 ^f)	(.024, .136 ^s , .013 ^d , .129 ^{sd} , .027 ^f)
Opposition Partners	-.054	-.064	-.043	-.201
Distance	(.020, .141 ^s , .139 ^d , .140 ^{sd} , .025 ^f)	(.019, .114 ^s , .124 ^d , .124 ^{sd} , .023 ^f)	(.014, .136 ^s , .107 ^d , .112 ^{sd} , .017 ^f)	(.012, .110 ^s , .090 ^d , .096 ^{sd} , .016 ^f)
Political Interest	.006	.003		
Distance	(.005, .044 ^s , .021 ^d , .019 ^{sd} , .009 ^f)	(.004, .038 ^s , .019 ^d , .018 ^{sd} , .008 ^f)		
Political Interest *	.066	.055		
Coalition Partners	(.012, .058 ^s , .039 ^d , .040 ^{sd} , .014 ^f)	(.012, .055 ^s , .040 ^d , .041 ^{sd} , .014 ^f)		
Political Interest *	.001	-.018		
Opposition Partners	(.008, .038 ^s , .036 ^d , .035 ^{sd} , .010 ^f)	(.007, .034 ^s , .032 ^d , .031 ^{sd} , .009 ^f)		
Education			-.026	-.030
Distance			(.002, .012 ^s , .008 ^d , .009 ^{sd} , .002 ^f)	(.001, .012 ^s , .007 ^d , .009 ^{sd} , .002 ^f)
Education * Coalition Partners			.017	.015
Distance			(.004, .019 ^s , .016 ^d , .017 ^{sd} , .004 ^f)	(.004, .020 ^s , .017 ^d , .018 ^{sd} , .004 ^f)
Education * Opposition Partners			.018	.025
Distance			(.002, .015 ^s , .012 ^d , .013 ^{sd} , .003 ^f)	(.002, .014 ^s , .012 ^d , .012 ^{sd} , .003 ^f)

(Continued)

TABLE 2 (Continued)

Variable Name	Coefficients (Standard Errors)			
	Model 1	Model 2	Model 3	Model 4
Between Partners	2.766 (.007, .073 ^s , .054 ^d , .068 ^{s,d} , .012 ^r)	2.812 (.007, .070 ^s , .050 ^d , .061 ^{s,d} , .011 ^r)	2.670 (.006, .061 ^s , .042 ^d , .054 ^{s,d} , .008 ^r)	2.705 (.005, .056 ^s , .038 ^d , .050 ^{s,d} , .008 ^r)
Extremism	.362 (.002, .021 ^s , .016 ^d , .022 ^{s,d} , .004 ^r)	.368 (.002, .019 ^s , .015 ^d , .020 ^{s,d} , .004 ^r)	.347 (.002, .013 ^s , .012 ^d , .017 ^{s,d} , .003 ^r)	.358 (.001, .012 ^s , .012 ^d , .016 ^{s,d} , .003 ^r)
Familiarity	-.547 (.052, .408 ^s , .381 ^d , .386 ^{s,d} , .045 ^r)		-.873 (.039, .303 ^s , .312 ^d , .328 ^{s,d} , .034 ^r)	
Familiarity > 0	-.179 (.013, .109 ^s , .126 ^d , .133 ^{s,d} , .011 ^r)		-.034 (.010, .085 ^s , .094 ^d , .101 ^{s,d} , .009 ^r)	
Familiarity * Coalition Partners	2.122 (.065, .582 ^s , .463 ^d , .609 ^{s,d} , .055 ^r)		2.067 (.050, .460 ^s , .454 ^d , .530 ^{s,d} , .044 ^r)	
Familiarity * Opposition Partners	-.217 (.063, .464 ^s , .443 ^d , .435 ^{s,d} , .058 ^r)		-.553 (.044, .417 ^s , .405 ^d , .479 ^{s,d} , .042 ^r)	
Telephone Survey	-.165 (.007, .132 ^s , .080 ^d , .063 ^{s,d} , .012 ^r)	-.130 (.007, .140 ^s , .077 ^d , .061 ^{s,d} , .012 ^r)	-.163 (.005, .085 ^s , .060 ^d , .051 ^{s,d} , .009 ^r)	-.132 (.005, .084 ^s , .057 ^d , .048 ^{s,d} , .009 ^r)
Self-Administered Survey	.185 (.011, .186 ^s , .184 ^d , .174 ^{s,d} , .018 ^r)	.135 (.009, .139 ^s , .127 ^d , .122 ^{s,d} , .015 ^r)	.264 (.007, .163 ^s , .113 ^d , .110 ^{s,d} , .012 ^r)	.242 (.006, .108 ^s , .086 ^d , .082 ^{s,d} , .011 ^r)
Intercept	1.208 (.015, .193 ^s , .106 ^d , .109 ^{s,d} , .025 ^r)	1.142 (.014, .168 ^s , .093 ^d , .094 ^{s,d} , .023 ^r)	1.260 (.011, .117 ^s , .087 ^d , .099 ^{s,d} , .016 ^r)	1.220 (.009, .110 ^s , .075 ^d , .086 ^{s,d} , .015 ^r)
Number of Countries	15	15	18	18
Number of Surveys	38	38	54	54
Number of Dyads	525	594	726	844
Number of Survey-Dyads	295	353	360	439
Number of Respondents	30291	30316	56953	57074
Number of Survey-Dyad-Respondents	395909	478797	671749	832604

Note: Standard errors clustered, respectively, on the following: none, survey^(s), dyad^(d), survey-dyad^(sd), respondent^(r). Dependent variable is perceived dyad distance.

minimal impacts on standard errors, which contrasts with the Stevenson and Vonnahme (2010) analysis using the same data but in which the dependent variable was accuracy of perceived party positions (a variable we expect to be greatly impacted by individual respondent characteristics) instead of the similarity of perceived positions.

Third, all of our control variables for which we had strong expectations act as we expected. The 10-year rolling average of a dyad’s CMP distance, as well as its current CMP distance, are both strong predictors of how ideologically similar voters will perceive the dyad to be. Likewise, locating oneself between the parties in the dyad results in the perception that they are farther apart. While it is harder to see from the relatively complex set of coefficients that make up the familiarity effect, it too meets our expectations. For example, using the coefficients in Table 2, we can calculate that, at mean levels of familiarity for coalition members with positive scores (.26), the contribution of familiarity to perceived distance is only .23 for Model 1 and .28 for Model 3. These effects for opposition dyads with mean familiarity greater than zero (also .26) are $-.38$ and $-.40$ for Models 1 and 3, respectively. The fact that the effect is negative and relatively large for opposition party dyads, but not for current cabinet party dyads, makes sense—and gives us additional confidence that our specification is reasonable.

Looking at the signs (and significances) of the coalition membership variable across all specifications, we see that the effect is large relative to other variables and in all cases statistically significant, clearly providing strong support for our main theoretical proposition. Likewise, for both political interest and education, we find evidence that greater levels of political awareness moderate the impact of cabinet membership on the extent to which respondents perceive the parties in a dyad as similar. Again, this is consistent with our hypothesis.

Of course, we hesitate to go too far in interpreting these effects, given that the use of robust standard errors in these models is not an ideal way to deal with the correlation caused by unmeasured variables at various levels. Thus, we prefer to present results from a more properly specified hierarchical model. That said, we have already indicated that it is not practically possible to estimate the full six-level hierarchical model. However, the pattern of results for the standard errors in Table 2, as well as the theoretical arguments we made earlier, give us a clear indication that the troublesome levels (i.e., those in which there are probably unmeasured factors creating correlations between rows in our data) are the levels which involve surveys and dyads rather than respondents. Thus, the model reported below is an error components (or “random effects” hierarchical) model that allows for

TABLE 3 Hierarchical Regression Analysis

Variable	Coefficient	(Std. Err.)
10-Year Average CMP Distance	.447	(.066)
Most Proximate CMP Distance	.315	(.063)
Coalition Partners	-.588	(.138)
Opposition Partners	-.029	(.082)
Political Interest	-.006	(.005)
Political Interest * Coalition Partners	.036	(.012)
Political Interest * Opposition Partners	.003	(.007)
Between Partners	2.228	(.007)
Extremism	.328	(.002)
Familiarity	-.236	(.346)
Familiarity > 0	-.041	(.089)
Familiarity * Coalition Partners	.415	(.498)
Familiarity * Opposition Partners	.090	(.436)
Telephone Survey	-.152	(.144)
Self-Administered Survey	.079	(.263)
Intercept	1.361	(.130)
Random Effect: Survey (38)	.335	(.063)
Random Effect: Dyad (525)	.790	(.026)
Random Effect: Residual	1.749	(.002)
N		395909
Log Likelihood		-784461.860

two kinds of random intercepts: one for each survey and one for each dyad within a survey. This is the most general model that we could practically estimate, and while it is not perfect, it does simultaneously account for the two most problematic levels of grouping in the data.¹⁸ The results are given in Table 3 and are quite consistent with those in Table 2 for our covariates of interest. Coalition participation causes voters to perceive parties as more similar and this relationship is not only robust statistically, but the substantive magnitude is also large. For example, for the least informed voter (political interest level of 1), the estimated effect of coalition partnership

¹⁸There was no practical way to estimate a crossed model between dyads and surveys and so we treat dyads as nested within surveys. Substantively, this means we treat a dyad like the German FDP-SDP in one survey as if it were different from the FDP-SDP dyad in another. This is exactly the same thing that researchers using time-series cross-sectional data do when they specify a random intercept model in which years are nested within countries (as is the common practice) rather than estimating a crossed model.

on perceived dyad similarity from Table 3 is $-.588$. For the average coalition partner dyad (10-year rolling average CMP distance of $.69$ and proximate CMP of $.66$), this effect is larger in absolute magnitude than the effects of both CMP derived distances *combined* ($.537$). Again, this is substantively significant and statistically robust support for Hypothesis 1.

Examining Table 3 also reveals the same systematic relationship between political interest and perceived dyad similarity that we saw in Table 2. Specifically, higher levels of political interest have a mitigating effect on the impact of coalition partnership on perceived similarity. This supports our hypothesis that highly interested voters will be less reliant on the heuristic of cabinet membership and thus less likely to systematically place coalition partners closer together in the policy space. It is important, however, to understand that the effect, while not negligible, is nowhere near large enough to eliminate the negative impact of coalition membership on perceptions of dyad similarity, even for the most informed voters. A simple calculation using the coefficients in Table 3 shows that the impact of coalition membership for the most politically interested respondents in our sample is a decrease in perceived distance of $-.444$ (compared to the $-.588$ reported for the least interested above).¹⁹

Conclusion

Previous empirical studies of voters in coalitional systems have failed to find a strong link between changes in parties' policy positions and changes in voters' perceptions of these positions. At the same time, such links have been relatively easy to identify in the American case. This finding, with its implicit message that there must be something different about coalitional systems that inhibits the ability of voters to track changes in the policy positions of parties, is in keeping with a long line of research and discussion that questions the ability of voters in coalitional systems to navigate their complexities. Downs (1957), for example, famously suggested that voters in coalitional systems will, because of the difficulty in forming expectations about the likely outcomes of coalition formation, forgo prospective policy voting. Similarly, in a very influential line of work, Lewis-Beck (1988) and Powell and Whitten (1993) suggested that voters in coalitional systems will also retreat from retrospective performance voting (with its requirement that one sensibly attribute responsibility over coalition par-

ties) in favor of less informationally demanding voting strategies.

In this article, we suggest, however, that the negative results in the previous literature do not reflect a fundamental difference between voters in coalitional and non-coalitional systems, but only a difference in the way that researchers have examined this question in the American and coalitional cases. Specifically, while studies of coalitional voters have almost always used party promises (manifestos) as a proxy for party positions, studies of American voters have usually used measures of policy actions (summaries of party voting in Congress). Voters, we suggest, are more likely to update their perceptions based on observable actions than they are promises. In the coalitional case, however, it is not roll-call votes (or mediated summaries of these), but coalition membership itself that is the concrete action that voters use to update their perceptions of the ideological movements of cabinet parties. That is, voters understand (or, at least, act as if they understand) that coalition membership usually represents a wide-ranging policy compromise between the parties, as well as a set of institutional incentives for collaboration, that (again, usually) result in real ideological moderation of cabinet parties toward each other's positions. Further, the average voter in these systems knows which parties are in the cabinet because these are among the most pervasive and easily obtainable pieces of political information available in coalitional systems (with levels of knowledge similar, for example, to the percentage of Americans who know which party holds the presidency). Consequently, we argue, coalition membership is a useful heuristic that voters can use to infer how the policy positions of cabinet parties are changing or are likely to change over the life of a cabinet.

Using individual-level data from 54 electoral surveys in 18 countries from 1994 to 2004, our empirical analysis uncovered robust support for the idea that voters perceive cabinet parties to be more ideologically similar than parties that do not serve together in cabinet and that this impact is diminished (but not absent) for more well-informed voters (whom we would expect to rely less heavily on a cabinet membership heuristic).

This conclusion resonates with a growing body of work on voter responses to coalition government that asserts, in contrast to the decades-old common wisdom, that voters can (and do) use simple heuristics to negotiate the complexities of coalition cabinets. This includes using simple rules of thumb to form sensible expectations about which coalitions form (Armstrong and Duch 2010; Duch et al. 2010; Fortunato, Stevenson, and Spiegelman 2011), to decide how to attribute policy-making responsibility over cabinet parties (Duch

¹⁹This remains highly significant.

et al. 2011), and, given the above results, to form useful perceptions of the ideological movements of cabinet parties. All of this work on voter perceptions, expectations, and attributions in coalitional systems supports an even larger body of work (though almost as recent) that demonstrates voters use these expectations, perceptions, and attributions in their vote choices. Duch and Stevenson (2008), for example, have demonstrated that voters' expectations about which cabinets form, as well as their attributions of responsibility for policy making, systematically impact retrospective performance voting. Likewise, Kedar (2005), Blais et al. (2006), Bargsted and Kedar (2009), Bowler et al. (2010), Meffert and Gschwend (2010), and Carman and Johns (2010) all demonstrate voters use their expectations about which coalitions will form to cast policy-oriented "coalition-directed" votes—votes intended to help a preferred coalition form.

Overall, then, recent work on voters in coalitional systems is beginning to overturn the traditional view that the complexity of these systems encourages voters to forgo either prospective policy voting (e.g., Downs 1957) or retrospective performance voting (e.g., Powell and Whitten 1993) in favor of less informationally costly voting strategies. Our study identifies another heuristic that voters in coalitional systems can use to efficiently inform themselves about the fundamental aspects of political competition in their systems and so underscores the general message of this emerging, revisionist, view of the abilities and knowledge of the coalitional voter.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

- Figure S1:** Distribution of Familiarity
- Table S1:** Descriptive Statistics by Model
- Table S2:** Distribution of Data Over Country

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