

Context, Heuristics, and Political Knowledge: Explaining Cross-National Variation in Citizens' Left-Right Knowledge

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Abstract

We present a theory that links variation in aggregate levels of political knowledge across countries and over time to corresponding differences in the political context in which voters become (or do not become) informed. Focusing on knowledge about the relative left-right positions of parties, we argue (via a theory about the contextual use of heuristics) that such knowledge will be more prevalent in political contexts where it helps voters to infer (1) the relative positions of parties on a variety of more narrow policy issues and (2) the likelihood of different policy-making coalitions. Combining data on the relative importance of these two functions across countries with survey-based data on of the aggregate distribution of left-right knowledge in 55 different contexts (in 18 European countries), our analysis reveals that voters understand the relative left-right positioning of parties to a much greater degree when these positions are important predictors of the formation of coalition cabinets.

Word count \sim 11,719

Surveys of British voters reveal that only about 56% of respondents can place the Labour Party to the left of the Conservative Party (Americans do about as well for the Republican and Democratic parties). In contrast, 86% of respondents to similar surveys in Denmark can place the Social Democratic Party to the left of the Conservative Party, 87% can place it to the left of the Liberals, and 82% can place it to the right of the Socialist People’s Party. Indeed, almost half of Danish respondents can correctly order all 15 major party pairs. Likewise, surveys gauging Brits’ and Danes’ interest in politics show that the typical Dane is 27% more interested in politics than the average Brit. Further, such differences are apparent across the Western democracies and have a dramatic impact on political participation and ultimately the quality of representative democracy (e.g., Brady et al. 1995, Lazarsfeld et al. 1948, Milbrath and Goel 1977, Verba et al. 1995).

In this paper, we seek to map and explain this kind of variation in knowledge about the relative left-right positions of political parties in the western democracies (we call this “partisan left-right knowledge”). We first describe a general theory that links variation in aggregate levels of partisan left-right knowledge to differences in the political context in which individuals become (or do not become) informed. With this theoretical compass, we next describe an empirical project that allows us to construct a map of contextual variation in partisan left-right knowledge across a large number of countries and a over long period of time (55 cases drawn from 18 countries from 1992 to 2004). Finally, we use this map of partisan left-right knowledge and corresponding measures of political context to test empirical hypotheses that flow directly from our theory.¹

¹Our purpose in studying political knowledge differs from many previous studies. While most have focused on explaining variation in knowledge among different individuals in the same political context (e.g, Delli Carpini and Keeter 1993), our focus is explaining differences in the whole distribution of political knowledge over individuals, across contexts (a notable exception is Gordon and Segura 1997).

Our theory begins with the idea that the level of partisan left-right knowledge in a given context ultimately depends on how useful the left-right metaphor is for organizing, simplifying, or otherwise facilitating voters' understanding of partisan politics. Where it is more useful for these purposes, elites will more often frame partisan politics in left-right language and voters, both actively and passively, will come to have greater levels of partisan left-right knowledge. As Benoit and Laver tell us (2013; 198):

“... political discourse is rather like a giant feral factor analysis. The concepts that emerge — liberal versus conservative, left versus right — emerge because people over the years have found them simple and effective ways to communicate their perceptions of similarity and difference.”

Thus, in very broad strokes (to be made finer in the sections that follow), our explanation of contextual differences in left-right knowledge isolates features of the political context that make the left-right metaphor (as applied to parties) a more or less effective way to communicate relevant similarities and differences about the parties. For the sake of clarity, we present our argument in two stages: First, we discuss the main functions of the left-right metaphor (as applied to individual's understanding of partisan politics) that have been identified in the literature and review various arguments for the salience of these different functions.

With a clear view of these functions, we then look to the recent, multi-disciplinary work on heuristics (most notably social-psychology, economics, and computer science) to give these functions a more concrete theoretical expression. Specifically, we propose that partisan left-right knowledge is the primary input to two particular heuristics that voters use to accomplish two otherwise prohibitively difficult cognitive tasks, each of which may

be necessary for meaningful political participation: (1) to understand the relative policy position of the parties on a large number of salient political issues, and (2) to form expectations about which parties are likely to form policy-making coalitions. While, as we will see, these two heuristics correspond closely to the two main functions of the left-right metaphor that have been often-discussed in the literature, expressing them as formal heuristics allows us to borrow tools from the literature on heuristics (specifically, the emerging literature on the “ecological rationality” of heuristics) to identify the specific features of the political context that should impact the usefulness of these heuristics — and ultimately the incentives of political elites to supply, and voters to collect, partisan left-right knowledge. We start, then, by identifying the functions of partisan left-right knowledge for simplifying partisan politics.

The functions of partisan left-right knowledge

Our survey of the large literature on the left-right metaphor in politics identified three main functions that the metaphor might accomplish in simplifying or organizing partisan politics for voters. We discuss each in the three sections below, commenting specifically on what the literature tells us about the relative salience of each function and how this salience might vary across different contexts.

Summarizing policy positions

By far the most pervasively discussed function of the left-right metaphor in partisan politics is to summarize a plethora of narrow policy positions into a more manageable, aggregated policy dimension. For example, Todosijevic (2004: 411), in his study of the usefulness of the left-right in Hungarian politics, typifies the way many applied researchers emphasize

this function when invoking the left-right metaphor: “Thanks to its absorptive nature, [the left-right construct] is able to represent a party’s stands on various issues simultaneously.” Likewise, Knutsen (1995, p. 63) suggests that “the use of the [left-right] schema is an efficient way to summarize the programs of political parties and groups, and to label important political issues of a given era.” And, of course, Downs (1957, p. 132) famously asserted that “each party takes stands on many issues, and each stand can be assigned a position on our left-right scale” (see also the many studies cited in Dalton and Wattenberg 1993 and Fuchs and Klingemann 1990).

Overall, the vast majority of work that invokes the left-right metaphor in studies of partisan politics uses it for this aggregative function. That said, the relevance of this function for understanding individual political behavior in the real world has not gone unchallenged. Most damning is the empirical case that voters do not actually use the left-right metaphor for this purpose. The American Voter (Campbel et. al. 1960) and Converse’s (1964) influential essay were only the first round in a persistent attack on the idea that voters use the left-right (or, in this view, any similar metaphor) to understand the policy preferences of the parties or to orient their own preferences to these (see also Klingemann 1979). And, while the specific empirical evidence behind these conclusions has often been challenged (e.g., Nie, Verba and Petrocik 1976), the general picture of a public “with little comprehension of [the] ideological meaning [of the left-right]” persists, “even though 70 percent or more of the citizens in these mass electorates may use them to describe political parties” (Levitin and Miller 1979, p. 751, see also Bartel’s 2012 review of the evidence for and against this idea).

While it is not our purpose to adjudicate this debate, the empirical results that we report later will bear on it. For now, however, we simply emphasize that despite tendency

of many scholars to treat the summarizing function of the left-right metaphor as essentially definitional (i.e., it is what the left-right dimension is), not everyone agrees that this is its most important (or even an important) function of the left-right construct in every context.

Structuring partisan coalitions

Politics is the art of compromise, and in many political systems parties can only make policy if they enter into policy-making coalitions with other parties. As such, one function of the left-right metaphor (though far less often invoked in the literature than the one discussed above) is in structuring voters' beliefs about which policy-making coalitions are likely to form, as well as their orientations toward different coalition possibilities. This function can either build on the aggregative function discussed above, or be independent of it. In the first view, if the left-right metaphor is an adequate summary of the parties' relative policy positions, and inter-party coalitions form (or not) according to the congruence of policy positions between the partners, then the left-right metaphor will be a useful guide to which coalitions are likely and which are not. Likewise, voters can use their left-right knowledge about parties to create (or perhaps discover) their preferences over coalitions. Certainly, this is the underlying view of many students of coalition formation in parliamentary democracies. For example, policy driven theories of coalition formation almost always predict ideologically compact coalitions (e.g., Axelrod 1970, de Swaan 1973, Laver and Shepsle 1990, Indridason 2011).² Likewise, many recent studies of coalition-directed voting (e.g., Duch et al., 2010) implicitly assume voters form preferences over coalitions by considering the left-right positions of parties.

A second view of how the left-right metaphor functions to guide voters' expectations about (and orientations toward) likely partisan coalitions dispenses with the need for the

²An exception is Luebbert (1986).

left-right to first function as an adequate summary of policy differences between the parties. For example, Arian and Shamir (1983: 140) argue that “for most people, left and right labels...do not denote ideology and surely do not reflect ideological conceptualization and thinking.” Instead, the left-right metaphor is “used to label and to identify the good or the bad, the right and the wrong, the desirable and the despicable” (142). In other words, quite apart from any specific policy content, when a party is on the “left” or “right”, this tells the voter which other parties should be considered allies and which enemies. Taken one step further, it can allow the voter to know if a given party is in her “in” or “out” group; with all the attendant emotional responses and perceptual and cognitive biases that give those categories force (Tajfel 1970, and see Aronson et al. 2010 for a recent review).³ Given this, the relative spatial positions of parties on a left-right scale can tell voters which partisan coalitions “make sense,” or, to which should they respond favorably or negatively (given their own preferences) and which should they expect?⁴

While partisan coalitions are certainly present in all modern democracies, they are most salient to the typical voter in those parliamentary democracies in which coalition cabinets usually form.⁵ Further, the existence of identifiable cabinet coalitions provides a clear

³We do not mean to imply here that these categories are strictly discrete. We can certainly think of a degree of “in-ness” or “out-ness” that could be related to the usual spatial distances that the left-right metaphor invokes.

⁴In support of the primacy of this function of the left-right metaphor, Arian and Shamir (1983) show that its use in Israel grew over time not as a function of the emergence of any underlying ideological structure to partisan policy positions but because its usefulness in identifying the poles of political competition (quite independent of policy content) grew.

⁵Indeed, a large empirical literature on “coalition-directed” voting shows that voters in these systems often cast votes consistent with an intention to influence coalition policy outcomes — an intention that requires expectations about the probabilities with which different coalitions form (e.g., Duch et al. 2010, Gschwend and Meffert 2010, and Kedar 2005).

set of coalitions on which to focus the part of our empirical investigation that requires characterization of the role that left-right plays in coalition formation across systems. Consequently, while we think the ideas developed so far and in the rest of the paper are likely useful for thinking about variation in partisan left-right knowledge in all democracies, in the rest of the paper, we will focus on parliamentary democracies where, even if not presently (or even usually) governed by coalition cabinet, the formation of coalition cabinets is at least possible.

Guiding the distribution of political support

To complete what we think is a more or less exhaustive list of the partisan functions of the left-right metaphor (and thus the possible reasons it could or could not be an “effective” tool in a given context), we can look to the very large literature that invokes the concept as a useful guide for how a voter should distribute her political support over parties.⁶ This is certainly one of the two functions (in addition to the policy aggregation function above) to which Downs ascribes the left-right metaphor.⁷ Further, on this point Adrian and Shamir (1983) agree with Downs that voters use left-right cues (but, in their case, absent any policy connections) to decide which parties they should or should not support. Indeed, a careful consideration of this third function of the left-right metaphor makes it clear that it can only be a reliable guide to the voter’s distribution of political support over parties if it works *through one or both of the other two functions described above*, and, as such, is not really a distinct function. If the left-right metaphor aggregates policy dimensions as Down’s suggests, then it provides an effective way to allocate support to parties (e.g.,

⁶We use the phrase “distribute her political support” to mean all the ways, from conversations to contributions, to voting, that individuals express degrees of support for different parties.

⁷Generalizing Downs somewhat, we could think of his main assumption about voters as: they support parties in proportion to how close they are to the party on a left-right dimension.

proportional to the extent one shares a party's policy preferences). Likewise, if the left-right metaphor signals the extent to which a party is in one's "in" group (again, absent policy considerations), then it can also provide an effective way to allocate support (e.g., in proportion to the extent to which the party is in one's "in" group). Further, it is difficult to imagine a mechanism by which a voter might allocate her support as a function of the left-right position of parties that does not fit into one (or both) of these two categories.

Thus, in our view, this third function of the left-right metaphor is subsumed in the first two and so, in what follows, we focus our attention on the salience of left-right as a policy aggregator and as a predictor of cabinet formation.

Partisan left-right knowledge, heuristics, and "ecological rationality"

We define a heuristic as a rule that maps a relatively small amount of information to more complex cognitions. There are three components to a heuristic: the informational inputs, the rule, and the target cognition (output). An example of a simple (and well-studied) heuristic is the *recognition heuristic*. This heuristic is often used when an individual must make a choice among different objects (e.g., she is asked which of two tennis players is likely to win an impending tennis match). The *recognition heuristic* takes a small information set as input (does the individual recognize one, both, or neither of the players) and a simple rule mapping that information to the cognition ("If I recognize one player and not the other, expect the recognized player to win, otherwise I don't know") to produce the target cognition (an expectation or prediction about the winner of the match). As Gigerenzer and Goldstein (2011) have demonstrated, under a variety of conditions, such heuristics can do a better job of producing accurate cognitions than much more complicated strategies that rely on more information (e.g., a multiple regression utilizing a database of statistics

about each player, see Scheibehenne and Broder 2011).

Examples of heuristics that have been studied in political science include a variety of different *partisanship heuristics*. These heuristics use the partisanship of some political actor (the informational input) to produce predictions or expectations about the actors' likely beliefs, attitudes, or behaviors (i.e., various target cognitions). Fortunato and Stevenson (2013b) have used a *partisanship heuristic* to study how Americans form beliefs about how their U.S. Senators have voted on different roll call votes. In this case, the heuristic takes information on one's Senator's party affiliation (as well as information about the preferences of parties over policies) and applies the simple rule "Senators vote with their party in the legislature" to produce the target cognitions: beliefs about senatorial votes on individual roll calls. They show that voters who use this heuristic will generate inferences that are correct approximately 90% of the time on average.

Returning to our example of the *recognition heuristic*, it is clear that the usefulness of the heuristic derives from the fact that in most media environments, individuals are more likely to have heard of better tennis players. Thus, differences in recognition should correlate closely to differences in win probabilities (on average). Likewise, in our American politics example, the *partisanship heuristic* is useful because there is a very high correlation between senatorial partisanship and legislative voting. An important claim in recent work on heuristics builds on this observation to suggest that individuals are able to intuit these correlations and so subconsciously use heuristics that are likely to "work" in a given context while avoiding those that are not.⁸ That is, individuals are "ecologically rational"

⁸Identifying the mechanisms through which individuals manage this is an important ongoing project in the literature on heuristics (Rieskamp and Otto 2011), but as yet these are not completely understood. It is likely that there are a variety of such mechanisms (e.g., direct inference of correlations from observations

— intuitively using appropriate heuristics in appropriate contexts.⁹ Likewise, a heuristic itself is said to be “ecologically rational” in a given context if it is efficient to use it in that context.

The claim that individuals are ecologically rational has been investigated for a number of common heuristics including the *recognition heuristic* described above. For example, Gigerenzer and Goldstein (2011) showed that individuals reach for the *recognition heuristic* for tasks in which recognition correlated highly with the target criterion (e.g., which tennis player will win, or which of two cities is largest), but that they do not use this heuristic when it is not (e.g., which of two cities is closest to a third city). Likewise, in the paper mentioned above, Fortunato and Stevenson (2013b) demonstrated that the *partisanship heuristic* was used much more often by the constituents of Senators who were party loyalists than by constituents of “maverick senators” (i.e., the heuristic was used more often in environments that had a strong correlation between a Senator’s legislative voting and his or her partisanship than environments where this correlation was weak).

While it has not been a theme of the multi-disciplinary literature on ecological rationality, we emphasize that differences in the empirical regularities that drive differences in of co-occurrence in the real world, learning from media and authority, etc.) and that the relevant ones vary depending on the nature of the target cognition (e.g., can its accuracy against the real world be judged easily? Are there enough instances of the phenomenon for individuals to actually infer long-term correlations?).

⁹There are considerations other than accuracy of these correlations that impact whether individuals will use a given heuristic in a given context — namely their simplicity and informational cost relative to other heuristics or decision-making strategies. See Fortunato and Stevenson (2013a, 2013b) for more detailed account. Since the argument in this paper, however, is built around contextual differences in the accuracy of such correlations, we will not focus on these other possibilities here.

ecological rationality across contexts (e.g., differences in the long-term correlations between variables that get incorporated into reliable heuristics) likely stem from corresponding differences in relevant institutions. Thus, the idea of ecological rationality fits quite naturally in the institutionalist agenda in political science. For example, in the case of the *partisanship heuristic*, various institutions encourage or discourage partisan loyalty in legislative votes and these institutions will drive long-term correlations between partisanship and legislative behavior (e.g., we would expect partisanship to be an excellent heuristic for predicting legislative votes in most parliamentary democracies, but would expect more variation across presidential democracies where legislators are elected from open lists). Another example comes from Fortunato and Stevenson (2013a) who provide evidence that voters in coalitional systems are ecologically rational in their use of heuristics for generating expectations about which party will become the Prime Minister. They find that voters use a *largest-party heuristic* (the party winning the most votes or seats will provide the prime minister) to predict which party will provide the prime minister, and that voters in Germany, New Zealand, and the Netherlands, where the largest party almost always controls the prime ministry, use the heuristic substantially more often than voters in Norway, where the largest party is far less likely to provide the prime minister.

As Stevenson (1997) and Glasgow et al. (2011) have argued, however, these correlations stem directly from the institutions that guide cabinet formation.¹⁰ That said, it is clear that the first (and most important) step in determining the heuristics that it will be “ecologically rational” to use in a given context (for a given purpose) is to identify the relevant empirical regularities, regardless of why they arise. Identifying the institutional

¹⁰For example, the institutions governing the possibility of minority cabinets (such as investiture rules and pre-electoral coalitions) and the institutions governing how formateurs are chosen influence the regularity with which non-largest party prime ministers get selected.

prerequisites of these regularities, while certainly an important and useful task, is a secondary mission. Consequently, while we briefly speculate on the institutional sources of the empirical regularities we discuss in this paper that is not our focus.¹¹

With this background, we can now turn back to the question at hand: what drives contextual variation in partisan left-right knowledge? Our answer: the factors that make one or both of two critical heuristics “ecologically rational” in a given context. Specifically, we argue that partisan left-right knowledge is a key informational input into at least two of the most important heuristics that voters use to navigate the complexities of their political systems. The first is a left-right heuristic for inferring partisan policy positions, which we will call the *LR-policy heuristic*. This heuristic allows voters to use their knowledge of the general left-right positions of parties to infer party positions on more specific issues. The second is a left-right heuristic for inferring patterns of partisan coalition formation, which we will call the *LR-coalition heuristic*. This heuristic allows voters to use their knowledge of the general left-right positions of parties to infer which policy-making coalitions are most likely to form.¹² We argue that ecological rationality of the *LR-policy heuristic*

¹¹Previous versions of this work focused almost entirely on these institutional connections, but it has become clear that that approach, in a manner of speaking, puts the cart before the horse.

¹²We do not think of partisan left-right knowledge, or indeed even the whole concept of the left-right, as itself being a heuristic, although it is often discussed (somewhat loosely) in these terms. Instead, we confine the term heuristic to the definition given above: simple rules that map a relatively small amount of information to more complex cognitions. The left-right, as used in politics, is not such a rule, but a conceptual metaphor (in the formal sense — see Lakoff and Johnson 1980) that maps the abstract (and physically inaccessible) concepts of relative policy stances and/or non-policy relationships to a more tangible, familiar, and useful domain (Laver and Benoit 2013 also identify the left-right metaphor in these terms). That is, we adopt spatial language — left-right, near-far — as a conceptual shorthand for understanding more complex, intangible concepts. This is not unlike our natural, spatial understanding of personal relationships: we use the adjective “close” to describe relationships that are more affectionate,

and *LR-coalition heuristic* vary according to the relevant features of the political context. Most importantly how accurately they predict relative policy positions and policymaking coalitions, respectively. Thus, by mapping the empirical regularities that determine the accuracy of the *LR-policy heuristic* and *LR-coalition heuristic*, we can predict where (and when) partisan left-right knowledge will be widespread.

In what follows, we first explain the details of the *LR-policy heuristic* and *LR-coalition heuristic*. Clearly, these correspond closely to the two main partisan functions we ascribed to the left-right metaphor and so we do not rehash the arguments for their relative salience across contexts reviewed above. However, we are now in a position to ask what features of the political context are likely to create differences in the ecological rationality of these two heuristics and so we spend some time on that question. Next, we explain how variation in these features of the political context (and therefore the ecological rationality of our heuristics) can drive variation in the costs and benefits of the information they take as an input (i.e., knowledge of the left-right positions of parties). This leads to several hypotheses that link variation in measurable features of the political context across countries (and over time) to variation in partisan left-right knowledge in these contexts. Finally, we test these hypotheses using a data spanning 18 countries from 1992-2004.

The *LR-policy heuristic*

The *LR-policy heuristic* allows voters to infer party stances on specific political issues by leveraging a more general understanding of party locations in the ideological space. That is, voters can use the informational input of a party's general left-right position, in conjunction with the simple rule, "specific policy positions correlate highly to general positions,"

devoted, friendly, intimate, loving, etc.

to infer where a party stands on specific policies. Thus, by knowing where a party stands in general (i.e., far left, far right, etc.), voters can infer a party's preferences on more specific issues such as social welfare (more generous programs or less generous programs), environmental protection (meticulous protection or loose oversight), or financial regulation (strict regulation or liberal deregulation).¹³

While this heuristic is just a different way to talk about the first function of the left-right metaphor discussed above, this small amount of formalism allows us to articulate a critical question: what are the empirical regularities that condition the accuracy of this heuristic across contexts and so might drive variations in its ecological rationality across those contexts? Clearly, the answer is the degree to which the parties' general left-right positions correlate with, or predict, their specific policy positions, across all salient policy issues. We can think of this as the policy "scope" or "reach" of left-right ideology in a given context and it corresponds fairly directly to how well variance in policy positions over parties in a given context can be explained by a single left-right dimension.¹⁴ For example, if knowing party A and party B's relative positions on the left-right dimension predicts their relative positions on all (or most) salient issues (and this holds for most party pairs) in the system, then we may say that this system has a fairly unidimensional policy space (and that the dimension is the left-right dimension) and the *LR-policy heuristic* will be

¹³Of course, voters would also have to understand how general left and right map onto each policy-specific dimension. For example, to infer that left party will favor higher taxes than a right party, requires that voters can map high/low tax preferences to the left-right dimension. Across a large number of issues, this is a substantial informational cost, that may potentially limit the usefulness of this heuristic. We will return to this point in the discussion of the empirical results.

¹⁴This is not the same as asking if the context is multi-dimensional. Rather, it is how much of the variance in specific policy positions is explained by a single left-right dimension. The question of how many dimensions it takes to explain the remaining variance is a different question.

accurate (and therefore useful) there. If, on the other hand, relative party positions on the left-right dimension are a poor predictor of positions on other issues in a given context, then we may conclude that this system is less unidimensional and the *LR-policy heuristic* will be less useful there.

The *LR-coalition heuristic*

In parliamentary systems in which coalitions cabinets usually form, understanding which coalitions are likely and which are not is critical to political participation because voters who wish to use their votes to influence future policy must have expectations about which policy-making coalitions will form and which will not.¹⁵

Traditionally, political scientists have been skeptical that voters can form sensible expectations about which coalition cabinets form (e.g., Downs 1957). However, as we noted earlier, a now large empirical literature on “coalition-directed” voting provides indirect evidence that they do. Further, Fortunato and Stevenson (2013a) have provided more direct evidence that voters in coalitional systems can and do form such expectations. Most importantly for this paper, they show that voters manage this feat by using two simple heuristics. The first facilitates beliefs about which party will provide the new prime minister (the *largest party heuristic* mentioned above) and the second is the *LR-coalition heuristic*. This heuristic takes knowledge of parties’ left-right positions as an informational input, applies the simple rule, “ideologically similar parties are more likely to coalesce,” to derive the target cognition: expectations for which parties are likely to form governing coalitions.

¹⁵More generally, to participate in satisfying political discussion in these systems one must make such assessments (so our argument is not tied in any way to a particular theory of voting behavior).

The empirical regularities that condition the accuracy of the *LR-coalition heuristic* across contexts concern the importance of parties' relative left-right positions in the formation of coalition cabinets. Where left-right positions are most critical to cabinet formation (as in Denmark or Norway) the *LR-coalition heuristic* will be an accurate tool for predicting which coalitions form and we should expect (via the ecological rationality framework) that citizens will use it more frequently in these contexts than in others. In contrast, where relative left-right positions of parties play a less prominent role in shaping real cabinet compositions, (as in Belgium or the Netherlands), the *LR-coalition heuristic* will be a less accurate guide to actual coalition outcomes and we expect the citizens to be less likely (again, probably subconsciously) to use it in forming their expectations.

Two sets of special cases are those parliamentary democracies in which all likely cabinets are usually one party-majority cabinets and those in which all likely cabinets are usually minority (coalition or one-party) cabinets. In the case of single-party majority cabinets, the cabinet composition is determined directly by the election and so relative policy positions play no direct role in choosing the cabinet. In such cases, the *LR-coalition heuristic* would certainly not be useful for a voter's understanding of who gets in the cabinet and so, *ceteris paribus*, we would expect lower levels of partisan left-right knowledge in such systems. Likewise, where only minority cabinets are likely to form, the mapping between the cabinet coalitions and policy-making coalitions is muddied significantly. While perhaps voters manage to work out what the real policy-making coalitions are likely to be in these situations, this task is (if not impossible) clearly more difficult than the alternative case (where the existence of majority cabinet coalitions provide a ready guide to the most likely policy-making coalitions). Here again, then, we would expect, *ceteris paribus*, lower

levels of partisan left-right knowledge in such systems.¹⁶

From contextual variation in heuristic use to contextual variation in left-right knowledge

If one accepts that the ecological rationality of the *LR-policy heuristic* and the *LR-coalition heuristic* may vary across contexts as a function of their accuracy, this suggests a mechanism by which we may explain the kinds of contextual differences in aggregate levels of left-right knowledge that were noted in the introduction. Specifically, we argue that differences in the ecological rationality of the *LR-policy heuristic* and the *LR-coalition heuristic* alter the costs and benefits of collecting left-right information: where empirical regularities exist that make these heuristics accurate, these empirical regularities also encourage the widespread use of left-right language by political elites, the media, and voters themselves. In other words, where the relative left-right positions of parties reliably predict more narrow policy positions and/or policy-making coalitions, those engaged in political discourse will be more likely to employ left-right language. Consequently, such language will be pervasive and easily available to voters.¹⁷

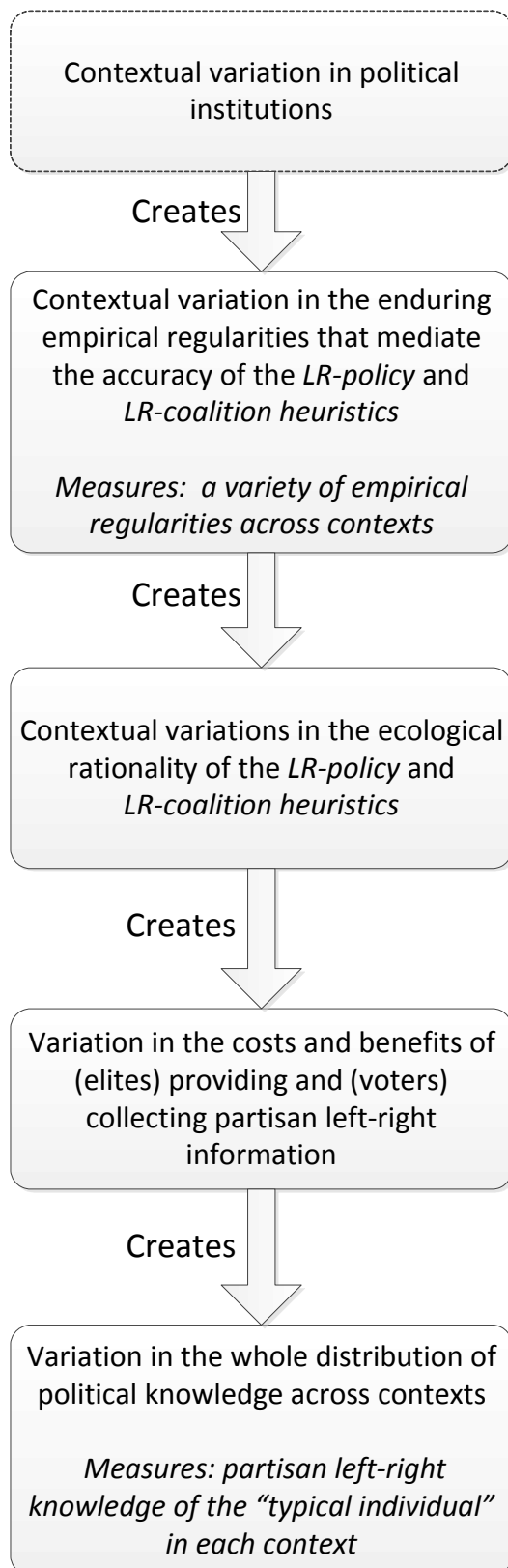
¹⁶There is a very important distinction to keep in mind here. We single out systems in which all of the likely cabinets are minority ones (so all likely cabinets will be forced to make policy-making coalitions that go beyond the cabinet — e.g., Canada) and systems in which minority cabinet sometimes form (or even often form) but there are alternative majority coalitions available (and relatively likely). In the latter case, the *LR-coalition heuristic* may well remain useful to voters.

¹⁷It is important to emphasize that we are not arguing that voters consciously take stock of the accuracy of the *LR-policy heuristic* and the *LR-coalition heuristic* in a given context and then choose whether or not to invest in information on the left-right positions of parties. Rather, we maintain that the usefulness of these heuristics will condition political discourse, which, in turn, creates information environments that facilitate the dissemination of left-right information to the mass public.

In addition, the lowered costs of collecting left-right information in contexts in which the *LR-policy heuristic* and the *LR-coalition heuristic* are ecologically rational (because of the existence of empirical regularities that make them so) may create in a virtuous circle in which the lower cost of using these heuristics further enhances their ecological rationality, which in turn further lowers these costs.¹⁸ So, returning to the quote by Benoit and Laver (2013) with which we began our argument — that the left-right metaphor will become salient when it has proven “simple and effective” at communicating (partisan) differences - we can now add the idea that this effectiveness happens (or does not happen) when either the *LR-policy heuristic* and the *LR-coalition heuristic* (or both) are “ecologically rational” in a given context.¹⁹

¹⁸While we did not emphasize it in the discussion above, recall that the ecological rationality of a heuristic depends on more than its relative accuracy. It also depends on the relative cost (compared to alternative heuristics or decision-making strategies) of its inputs and the simplicity or complexity of the rule (i.e., the relative cognitive costs of applying it).

¹⁹This general framework is not limited to left-right knowledge, but should apply to any piece (or set) of information that is used as an input to one or more salient heuristics. For example, where largest party status determines who will provide the prime minister, as in Germany or New Zealand, expectations over which parties are in contention for a plurality become critical to forecasting which constellations of parties have a chance at forming a government, and thus, the election narrative should reflect the primacy of this information and revolve around this question, resulting in “horserace” coverage and a high level of knowledge in the electorate about which party will win a plurality. Where plurality status is less salient, as in Norway or Sweden, then we should observe less of this kind of horserace discussion and more diffuse expectations amongst the electorate for which party is likely to win a plurality. This expectation is supported by the empirical findings of Fortunato and Stevenson (2013a).



**Figure 1:
Summary of the
Theoretical Argument
and Empirical Analysis**

Empirical Analysis:
Explores this connection for the *LR-policy* and *LR-coalition heuristics* - as well as gauging the relative importance of each

Figure 1, summarizes the argument and previews the empirical project described in the next section. Specifically, we will examine the two most important empirical implications that come out of the theory: If inferring specific policy positions (the *LR-policy heuristic*) is an important task for participating meaningfully in politics, then differences in partisan left-right knowledge across contexts should be explained (in part) by corresponding differences in the reach or scope of the left-right dimension in those contexts. Likewise, in a world where understanding policy-making coalitions (the *LR-coalition heuristic*) is important to meaningful political participation, differences in partisan left-right knowledge across contexts should be driven by corresponding differences in how well the left-right positions of parties predict which coalitions form. Importantly, these are not mutually exclusive hypotheses – we may find support for one, both, or neither in the coming analysis. Further, if we do find differential support, then this bears not only on the question of what drives difference in partisan left-right knowledge, but also the relative salience of the *LR-policy heuristic* vs. the *LR-coalition heuristic* generally.

- *LR-policy heuristic* hypothesis: The better (and more frequently) the relative general left-right positions of parties predict the relative positions of parties on more specific (and salient) policies in a given context, the greater the level of partisan left-right knowledge among the voters in that context.
- *LR-coalition heuristic* hypothesis: The more accurately the relative left-right positions of parties predict the composition of coalition cabinets in a given context, the greater the level of partisan left-right knowledge among the voters in that context.

Data analysis

We begin with our dependent variable. To test the hypotheses described in the last section, we examine data on citizens' knowledge of the ideological positioning of political parties as reflected in 55 election surveys conducted in 18 developed democracies from 1992-2004.²⁰ More specifically, we develop a measure of voters' knowledge about the left-right positions of parties that is comparable across voters, elections, and countries. Since this is one of the first times these kinds of data have been compared across a large number of countries and over a long time period, we spend some time discussing the various measurement decisions that we made and describing the extent and nature of the variation in our measures of partisan left-right knowledge. Thus, the first section below sketches how we measured partisan left-right knowledge, the next section presents some evidence justifying our different measurement decisions, and the third section provides a map of the variation in aggregate partisan left-right knowledge across countries and over time.

Mapping differences in partisan left-right knowledge across countries and over time

Our method of mapping variation in voters' partisan left-right knowledge proceeds in three steps. First, we identified 55 election surveys in 18 countries that asked voters the following questions (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?”

²⁰In some of the analyses that follow, we treat Belgium as three separate cases, corresponding to whether a given survey targeted Flanders, Wallonia, or did not differentiate. Thus our 18 “countries” is only 16 if we consolidate all the Belgian cases. A complete list of all the countries and party-dyads used in the estimations is provided in Table A2 in Appendix A.

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]?”

Second, we assigned a “correct” left-right position to each party (to which we can compare voters’ responses). We did this in several different ways and compared the results across the samples of cases in which more than one of these methods could be used. These different methods are discussed in the next section. However, in no case did the method used to assign “correct” party positions change the conclusions of the paper or, to any great degree, the specific empirical results.

Third, we transformed our respondent/party data into “respondent/party-dyad” data (i.e., if m is the number of parties, each respondent enters the data $\frac{m(m-1)}{2}$ times corresponding to every possible unordered pair of parties) and recorded, for each party-dyad, whether a respondent placed those two parties in the correct left-right order, the incorrect order, or said “Don’t Know” for one or both parties. This three-category variable is the main dependent variable in our analyses of the individual surveys. It is important to emphasize that this dependent variable focuses all of our analysis on the question of whether respondents can place parties in the correct ideological ordering, rather than whether they can place parties in the right place in some absolute sense, as in, for example, Gordon and Segura (1997).

There are a number of compelling reasons to focus on the ordinal placement of parties, rather than the absolute placement of individual parties. First, our theory speaks to variations in the voter’s need to understand the relative ideological positions of parties rather than their absolute positions. Second, a focus on the relative positions of parties

rather than their absolute positions drastically increases the extent to which different measures of the parties’ “true” ideological positions agree with one another (as we show in the next section). This largely insulates our conclusions from an otherwise important source of measurement error — error that is reflected in the (sometimes substantial) differences in absolute ideological placements of parties when ideology is measured in different ways (e.g., McDonald, Medes, and Kim, 2007). Finally, our focus on the ordinal positioning of parties means the cardinality of the ideology scales we use does not matter for most of our analysis — thus minimizing potential problems in comparing cases across contexts in which respondents may have systematically different definitions of what, for example, an “eight” on a left-right scale means.

The above sketch of our measurement procedure glosses over a number of thorny measurement issues that deserve more discussion than we can provide here. These issues are: which parties to include, what to do with “Don’t Know” responses, and what to do with “tied” responses (where voters give both parties the same placement). We provide detailed discussion for interested readers in section A1 of the on-line appendix, but the short answer to these questions are that we include “important” parties (i.e., excluding single-issue parties, regional parties, very small parties, and parties that do not compete on the traditional left/right dimension of politics — most importantly, this excludes Green parties), we model “Don’t Know” responses explicitly, and we count tied responses as incorrect. Importantly, however, we have explored the robustness of our results to changing all of these decisions in various ways (e.g., choosing instead the most important two parties, or all parties holding legislative seats; omitting “Don’t Know” responses from the analysis; or counting ties as correct). In no case do any of these decisions change the substantive results of our analysis.

The final issue we contend with is assigning “correct” orderings to the parties. We explore four possible approaches to determining “true” party positions: the expert codings from the Comparative Study of Electoral Systems (CSES) survey modules, the Laver and Hunt (1990) and Benoit and Laver (2006) expert survey codings, estimates from the Comparative Manifestos Project, and the final method simply takes the mean ideological placement of each party, over all the respondents in a given election survey, as the estimate of the “correct” position of the party at that time. We choose the final approach as it gives us a correct ordering for 100% of all party pairs in our data (the other methods vary from 40% to 97% coverage) and because there is almost no variation in the rank orderings of party pairs across these four different methods. We explore these data and our decision in great detail in section A2 of the on-line appendix.

An initial map of partisan left-right across countries and over time

With the above measures in hand, we can now turn to characterizing variation in partisan left-right knowledge across countries and over time. Before turning to the broad map, however, it will be useful to examine one of our countries more closely. Figure 2 provides a histogram of the number of mistakes that respondents to the 1998 Danish Election Survey made in ordering their six major parties on the left-right scale. There were 15 party-dyads and so 15 possible mistakes. Almost 24% of respondents placed all six parties in the correct order. The median number of mistakes is one, and most of these single mistakes are misplacing the Center Party and the Social Democrats, which are adjacent ideologically and less than half a unit apart in our measure of “true distance” (which is quite close given the observed range of the left-right placements for this case). If we put these “near misses” together with those who made no mistakes, we approach 50% of the sample that placed six parties in the correct ideological order.

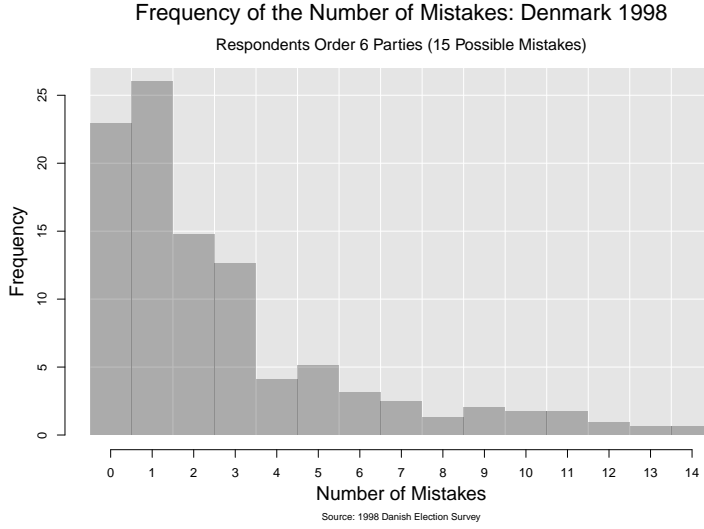


Figure 2: Frequency of the number of mistakes made by respondents in ordering 6 parties (15 possible mistakes); Danish 1998 Election Study

More revealingly, we can drill down a bit deeper in the examination of this case and see how well voters did in comparing particular pairs of parties. Table 1 presents the percentage of Danish respondents who correctly ordered the various dyads that included the Social Democratic Party. The results of this exercise leave little doubt that the remarkable aggregate distribution of knowledge cited in Figure 2 results from equally remarkable dyad-by-dyad knowledge. Indeed, when we go beyond the comparisons of the big parties that are on opposite sides of the left-right divide (e.g., the Social Democrats and Conservatives), we find that the Danes also understand differences in party ideological placement within the same ideological family. For example, the small Socialist People’s Party acts as the “conscience” of the left and Danish voters understand that fact - with 82% of them (which obviously includes a substantial number of rightist voters) reporting that the SPP is to the left of the SD.

A final thing to notice about Table 1 is that the percentage of “Don’t Know” responses seems to have an inverse relationship with the percentage of respondents (who answered at all) who correctly place the parties. This suggests that, while the two responses may not provide exactly the same information, they both reflect underlying levels of partisan left-right knowledge.²¹

Table 1: Accuracy in ordering the Social Democrats (0.36) with other major parties in 1998 Denmark. Proportion of correct responses among all respondents, those who chose to answer, and the proportion of those that did not answer. Share of legislative seats for each party given in parentheses.

Comparison	Correct	Correct	Don't Know
	All Respondents	Answering	
Liberals (0.24)	0.87	0.91	0.05
Conservative People's Party (0.09)	0.86	0.92	0.06
Socialist People's Party (0.07)	0.82	0.88	0.07
Danish People's Party (0.04)	0.84	0.91	0.08
Centre Democrats (0.02)	0.59	0.66	0.12

This detailed example helps establish the notion that differences in partisan left-right knowledge can be higher than has generally been appreciated in the literature and, as our earlier examples of low-levels of knowledge suggest, can also vary substantially across contexts. However, we also want to examine the broader variation in partisan left-right knowledge across all of our cases, with an eye toward establishing whether this variation

²¹This observation holds up in comparisons of the full sample of surveys.

tends to be concentrated within countries, across countries, or even over-time (i.e., in waves affecting all countries at the same time). There are many ways to do this, but the simplest and most appropriate at this stage of the analysis (before we introduce any statistical models) is to plot the average percentage of correct dyad placements in each of the surveys in our sample, organized by country and survey year.²² Since these are uncontrolled comparisons, we include only the percentage of respondents who correctly order the leading left and the leading right party — thus maximizing the comparability of the dyads being compared across countries. Figure 3 provides the data.

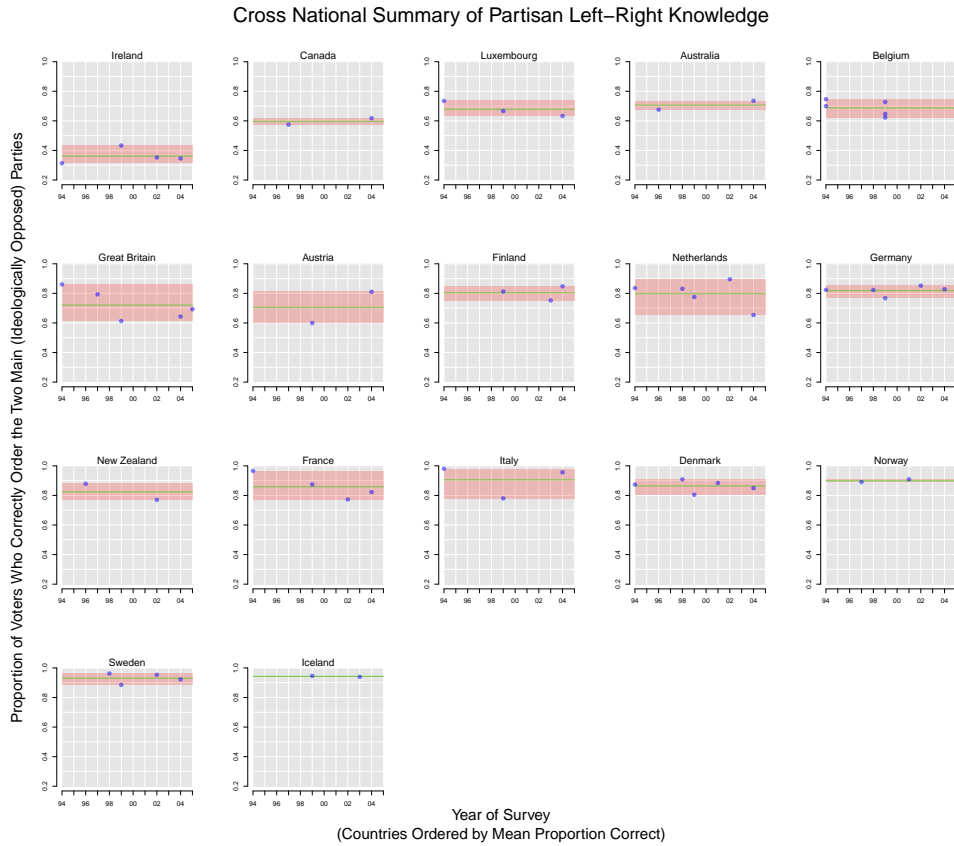
The dots are the estimated percentages, the line is the mean across surveys for the country, and the shaded area highlights the range between the maximum and minimum percentages across surveys in each country.²³ Cases are ordered by the mean chance that a given dyad will be correctly ordered and range from a low of less than a 40% (Ireland) to a high of almost 95% (Iceland).²⁴ The main point of providing maps of contextual variation like this one is to visually assess the extent of variation that exists and whether this

²²This chart includes 58 cases, while the analysis reported below includes only 56 cases. This is due to some exclusions (detailed in those analyses) due to missing data.

²³The mean line is the mean when collapsing the data over all respondents in all surveys in a country, (rather than the mean of the survey means after collapsing each survey over the respondents). This was done to maintain compatibility between these estimates and those displayed in Figure 4 below.

²⁴The absolute size of these estimates across all countries is determined to some extent by the specific task that is being graphed. In this case, it is among these easiest that we measured — ordering only the two largest parties on the left and right, respectively (also, we ignore “Don’t Know” responses here, further inflating the percent correct). If we were to examine other measures like percent that ordered the two or three most important parties correctly, a larger selection of parties correctly, or even all the parties correctly (or if we included “Don’t Know” responses as incorrect in the graph), the level of correct answers would go down for all countries. The relative order of countries, as it turns out, would not change much (with the exception that when we start including more parties, then in an uncontrolled look at the data — like this is — we get differences induced by difference in the number of parties in the system).

Figure 3: Map of partisan left-right knowledge across the western parliamentary democracies.



variation seems to be concentrated within countries, across countries, or both. As Duch and Stevenson (2008) have emphasized, this kind of parsing of variance at the beginning of an empirical study of comparative political behavior (economic voting in their analysis and partisan left-right knowledge in ours) provides an essential clue about the kinds of causal factors (both measured or unmeasured) that could possibly explain the observed variance. If there is variance in political knowledge overall, but this variance only occurs across countries and not within countries (so if the means in Figure 3 differ across graphs,

but knowledge within any one graph is constant) then causal variables that change from election survey to election survey (e.g. which parties are in cabinet) simply cannot explain the observed variance in the figure. Likewise, if most the variance is from survey to survey within a country and the means across countries differ little, then we should not expect explanations (like the one we have provided above) that emphasize enduring empirical regularities (and corresponding institutional characteristics) of countries to have much explanatory power.

A visual inspection of the graph reveals that the shaded areas (which give an indication of the extent of within-country variance) are small relative to the area spanned by the mean lines across charts. For countries in which we have two or more surveys, the average difference between the maximum and minimum survey is 11.6%, while, excluding Ireland, the difference between the maximum and minimum country mean is 35% (this is almost 60% including Ireland). This suggests both that there is substantial overall variation in the data and that between-country variation is dominant.

We can also formalize our parsing of the within versus between country variance by estimating a multilevel model (with no covariates) in which we nest surveys within countries and estimate the cross-country variance separately from the within-country variance.²⁵ Doing so reveals that over 66% of the total variance apparent in Figure 4 is attributable to factors (measured or unmeasured) that vary across countries but are constant within countries. Likewise, 34% of the variance is attributable to factors that vary within countries.²⁶

²⁵We estimate a normal-linear hierarchical model. We did this both for the dependent variable in Figure 4 and a transformation that mapped this bounded variable to an unbounded interval (and more approximately normal distribution). The results were qualitatively the same in both cases.

²⁶As we demonstrate below, this is not the whole story since we have collapsed the data over individuals and we are only showing information for a single dyad for each survey. These other levels will contribute

This suggests strongly that our search for explanations of this variation should focus first on contextual characteristics that are relatively unchanging over time but that vary across countries; the importance of parties' relative left-right positions in the process of coalition formation and the scope and importance of the left-right dimension as a policy aggregator are likely two such characteristics. That said, there is substantial within-country variation as well (especially in the Britain, the Netherlands, Italy, France, and, perhaps, Austria). Given the incremental pace of change in patterns of coalition formation and the dimensionality of political discourse, our theory does not provide a great deal of guidance on what might drive this within-country variance. Thus, we will want to tap into the rich literature that suggests other possibilities. This will not only provide a source of relevant controls that will help us make the main inference at which the empirical analysis is aimed but also the raw empirical material for fruitful extensions of the theory and tests of other theoretical perspectives.

Testing the hypotheses

The data in Figure 3 make it clear that there is significant variation in citizens' partisan left-right knowledge across countries. Can this variation be explained by corresponding variation in the empirical regularities that we hypothesize drive the usefulness of the *LR-policy heuristic* and *LR-coalition heuristic*?

To test the first of these hypotheses, we need a measure of how well the left-right positions of parties in a country (over some period of time) predict their positions on other, narrower policy domains. There are several existing measures that get at aspects of this and, since none is perfect, we examine our hypothesis using five different measures. The

their own variation to the overall data.

first simply measures the “importance” of the left-right dimension in the overall scope of party competition. This can be measured in two ways: objective measures of party behavior (votes, statements, manifestos) and subjective assessments by experts. We use a measure from each general strategy. Our first measure of the importance of the left-right uses the average expert judgement of the relative importance (over the parties in a system) of the main left-right policy dimension (“tax and welfare” dimension) as given by Laver and Hunt (1992) and Benoit and Laver (2006). For each survey, we use the scores (for each party) from the edition of the expert survey closest to the date of the election survey.

Our second measure turns to more “objective” estimates of the importance of the left-right. Specifically, we calculated the percentage of each party’s election manifesto that was devoted to left-right topics (using the categorization of left-right topics adopted by Budge and Laver 1992 and used by scores of subsequent studies) for all parties in each election most proximate to one of our surveys. Next, we averaged of this score over the parties included in a given survey (so for one election in one country).

While measures of “importance” like these get at aspects of our theoretical concept, we can do better. Specifically, the concept we have defined is about how well the left-right positions of parties predict their positions on other (more narrow) policy dimensions. If the left-right does so successfully for a large number of salient dimensions, then it will be a useful summary of the ideological landscape.

To get at this concept more directly, we calculated the salience-weighted average Spearman’s rank correlation between the left-right positions of parties and each of a large number of different policy dimensions. To do this, we used data from Lowe et al. (2011), who use the Comparative Manifestos Project data to define one left-right policy domain and 14

more narrow policy domains in which specific party positions are calculated (these are about foreign alliances, militarism, internationalism, the EU, constitutionalism, decentralization, protectionism, Keynesianism, nationalism, traditional morality, multiculturalism, labor policy, welfare policy, and education spending). The details of this measure (and all the others) are in Appendix B, but the key idea is that it is higher when the Spearman correlations between the left-right positions of parties and the 14 narrow policy dimensions are greater. We calculated the measure for all the parties in the data for each country, for the period spanning our survey data, 1992-2004. Finally, we weighted the measure so that correlations on salient dimensions mattered more than correlations on less salient dimensions.

While we think Spearman’s rank correlation nicely captures our emphasis on the relative positioning of parties, we also calculated a similar score in which we used the regression coefficients from a multiple regression of party left-right positions (using the same data as the Spearman measure) on all 14 narrow policy positions (so this also differs from the last measure in that for each dimension it conditions on the 13 other dimensions as all 14 are included in the model).

Finally, we also used a measure that makes no particular judgment about the content of any super-dimensions that might predict policy positions in more narrow domains. While this departs from the concept in our argument, which is explicitly made in terms of the left right, it is useful since it would certainly be possible to reformulate our argument to simply refer to a generic underlying aggregative dimension.²⁷ To get at this we simply use Laver

²⁷We did not go that direction here because we are skeptical that voters will be able to learn much about “super-dimensions” of policy that do not conform to the ready-made labels that are already available in political discourse (and, we suspect, it will take quite a long time for new labels with broad coverage — i.e.,

and Benoit’s (2006) estimates of the total variance explained by the first dimension in their principal components factor analysis of their expert survey data (reported in their Table 5.7a). This included nine issue areas (tax and spending policy, social policy, environmental policy, decentralization, EU peacekeeping, immigration, EU accountability, EU authority, and deregulation) and attempted to identify the extent to which expert placements of parties in these nine areas could be explained by a small number of factors. In focusing on the first factor, regardless of its substantive content, this measure — unlike all the others used here, which all focus explicitly on a substantive left-right dimension — simply asks how well voters would have done using any (or the best) underlying aggregative dimension (however defined, though in most cases in their results the first dimension looks like the left-right) to order more narrow policy dimensions.

In the analysis that follows, we examine models using each of these different measures. However, since all the measures give similar results and the Spearman measure is closest to what we want conceptually, we report substantive results only for it (the estimates for the others are in Appendix C).

To test our second hypothesis, we need to measure, for each of our countries, the extent to which the left-right placements of the parties are important to determining which cabinets form. In keeping with the nature of our proposed *LR-coalition heuristic*, which subsuming many policies to emerge. Thus, when a factor analysis tells us that a strong first dimension is apparent, but that it does not include the usual left-right issues, we hesitate to conclude that voters could actually use this (usually difficult to label) dimension to infer more narrow policy positions on a large number of issues because it is likely that voters and elites lack the shared language to use such a dimension for that purpose. Consequently, we really do think of the problem in the way we have portrayed it in this paper: as the left-right being more or less useful as an aggregative dimension — where if it is not very useful, there may be no real alternatives for performing this aggregative task for voters.

is about the left-right positioning of potential cabinet partners vis-à-vis the PM, we focus on estimating the importance of the left-right distance between potential partner parties and a potential PM in determining which cabinet forms. We have done this in two ways. First, we re-estimated Martin and Stevenson's (2001) conditional logistic model in which they estimate, among other things, the impact that ideological distance between potential cabinet partners and the PM has on the chances that any particular potential cabinet will form (out of the set of all possible cabinets containing the PM). We simply modify their model by interacting this distance variable with country-specific dummy variables.²⁸ This gives us country-specific estimates of how important ideological distance from the PM is in determining which potential cabinet forms.²⁹

A second method that we used to construct this measure was to re-estimate Warwick's (1995) logistic model of the selection of cabinet partners (out of the set of parties in the legislature). He includes each party's ideological distance from the prime minister as a covariate and so we are able, as above, to include country specific interactions with this variable to generate a country specific measure of the importance of relative ideology in the selection of cabinet partners.³⁰ Below, we report results from the first measure, but results were robust to the alternative.

²⁸More specifically, we rely on Martin and Stevenson's Model 9.

²⁹The coefficients can be used directly as this measure (which is what we do here) but one can also calculate changes in predicted probabilities and use those instead. We have done that, and it makes little difference to the results. In addition, of course, we would like to incorporate the uncertainty in these estimates into the statistical model. Given the complexity of the other statistical problems that we address in this paper (and the resulting complexity of the statistical models), we have not done so as yet, and so correct standard errors should be somewhat more conservative than those we report.

³⁰In general, Martin and Stevenson's model is to be preferred since Warwick's model does not account for the dependencies in the data (i.e., the logistic model assumes that probabilities of cabinet partnership are independent among potential partners).

Statistical models

The data that we will use to test the hypothesis is, as described above, based on 55 election surveys.³¹ In developing appropriate empirical models, it is important that we first understand the relatively complex structure of the data. Table 2 illustrates this structure. In each survey, each respondent placed each of the parties in the election on a left-right scale or chose “Don’t Know.” We then turned this data in to dyads, so that our dependent variable records whether each voter correctly or incorrectly ordered each dyad (or said they did not know for at least one of the parties). 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 and each survey has multiple dyads. Finally, surveys are nested in countries, since each survey applies to one, and only one, country.³²

³¹The 58 surveys pictured in Figure 3, less the two New Zealand surveys and one of the Luxembourg surveys which were omitted due to missing data.

³²The following description of the data structure is adapted from Fortunato and Stevenson 2013.

Table 2: Structure of the data

Countries	Surveys	Dyads	Respondents
Austria	EE 1994	SPO-OVP	A
Austria	EE 1994	SPO-FPO	A
Austria	EE 1994	SPO-OVP	B
Austria	EE 1994	SPO-FPO	B
Austria	AES 1999	SPO-OVP	C
Austria	AES 1999	SPO-FPO	C
Austria	AES 1999	SPO-OVP	D
Austria	AES 1999	SPO-FPO	D
Germany	CSES 2004	SDP-CDU	E
Germany	CSES 2004	CDU-FDP	E
Germany	CSES 2004	SDP-CDU	F
Germany	CSES 2004	CDU-FDP	F

Dyads CROSSED with Surveys	
Surveys NESTED within Countries	Respondents CROSSED with Dyads

This data structure leads to six possible sources of both measured (fixed) and unmeasured (random) effects on the probability of our dependent variable obtaining one of its three possible values:³³

1. Country: effects that vary over countries but that are constant over surveys, dyads,

³³One 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.

and respondents within a country

2. Survey: effects that vary over surveys but that are constant over dyads and respondents within surveys
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. Survey-dyad: effects that are constant over respondents evaluating a given dyad, but that vary from survey to survey for the same dyad
5. Respondent: effects that vary over respondents but that are constant over all dyads evaluated by the same respondent
6. Dyad-respondent: effects vary from dyad to dyad for the same respondent (when this is unmeasured, it is the “residual” error)

Given this, our first goal is to collect any relevant measurable factors at each level and then try to deal statistically with any remaining unmeasured factors. It is important not to ignore the possibility of such unmeasured factors since, at each level, these unmeasured effects are constant across some “rows” of the data — thus, necessarily creating correlation among our observations at each level.

Measured variables

We have data on a wide variety of control variables. The details of the variables that are used in the models reported here are described in Appendix B. However, the listing below provides a more general overview of the various concepts we attempted to measure and the level of data hierarchy where their influences can be felt. Some of these variables were not ultimately included in the specific empirical specifications reported here, because they

proved to be unhelpful in the specific sample of dyads (i.e., the “important” parties) for which we report results (e.g., whether anti-system parties were included in the dyad is important for a sample containing all parties, but not for the sample of important parties we report). However, in no case does the exclusion or inclusion of such variables alter the main conclusions of this manuscript. All the various results are available from the authors.

Concepts measured at each level

- Country

Long-term correlations between relative party positions on the general left-right dimension and many more narrow policy dimensions

Long-term correlations between relative party positions on the general left-right dimension and coalition composition

- Survey

Number of parties/dyads in survey or effective number of parties

Attributes of survey implementation (telephone vs. in person interview)

- Dyad

Party names (e.g., does the word “left” appear in one name and/or “right” in the other)

- Survey-dyad

Ideological distance between parties in dyad

Sizes of parties in dyad

Current and/or historical cabinet participation of parties in dyad

Anti-system profile of parties in dyad

Ages of parties

- Respondent

Income

Education

Age

Political interest

Gender

- Dyad-respondent

Respondent's ideological relationship to parties in the dyad

Accounting for unmeasured factors

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 — perhaps restricted to be independent) 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 and versions appropriate for an unordered categorical dependent variable are relatively well understood (e.g., the

mixed logit model). However, despite our ability to write down the statistical model most appropriate for our application, its complex, six-level structure (with several crossed-levels) is far too complicated to estimate directly.³⁴ Thus, a more creative strategy is necessary.

Our estimation strategy builds on recent work that has argued for a “two-stage” methodology when using multiple surveys to study the impact of context on political behavior (see, especially, the 2005 special issue of *Political Analysis*, which was devoted to the topic). Specifically, instead of stacking all the data from our 55 surveys, we use the following procedure:

1. Estimate individual multinomial logit models, with appropriate individual level controls, for each party-dyad in each survey (a total of 394 separate estimations).³⁵
2. Use the estimated coefficients from these models to calculate the predicted probability that a typical voter in the survey correctly, incorrectly, or does not order the dyad.
3. These predicted probabilities, which sum to one over the three possible outcomes, become the dependent variables in a linear, compositional, hierarchical model in

³⁴The mixed logit requires that we integrate the random terms out of the likelihood function and so we must approximate a high dimensional integral at each iteration of the maximization process. With every nested level included in the model, we increase the dimensionality of this integral by one and with every crossed level we add as many dimensions as there are categories in the smallest of the crossed variables (the number of surveys and dyads in our case). Clearly, the computational burden becomes insurmountable with very large problems and large data sets (and we have a large data set indeed).

³⁵In each individual level model we included measures of education, income, political interest, gender, age (and its square), and the ideological position of the respondent vis-à-vis the two parties in the dyad. We were missing political interest and/or income for some surveys (and age for one survey), so these were not included in those surveys. While we do not have the space to provide complete information on all these 55 surveys in this paper, all the estimates and specifications are available from the authors. In addition, some of the results for the individual level variables are given in Table 4 in the text.

which the independent variables are measured at the level of survey-dyads, dyads, surveys, and countries.

There are a number of compelling advantages to this approach over alternatives. First, like an approach in which one stacks the data, one gets estimates of the impact of any measured individual level variables on the probability of each of our three outcomes. However, unlike stacking approaches, one gets separate estimates for each survey-dyad (the equivalent of interacting all individual level estimates with survey-dyad dummy variables in a stacked model). These estimates can be presented directly (in graphs that show their variation across survey dyads) or can be aggregated to characterize the general impact of individual factors on knowledge. Since most of our surveys have over 1000 observations (and some many more), there is little to gain in making inferences about the impact of individual-level variables by stacking the data. More importantly, in the separate estimations, any characteristics of dyads (or of survey-dyads) that might cause respondents to systematically order correctly, order incorrectly, or not order the dyad are reflected in the estimates of intercepts in each separate model (and so are included in predicted values produced for each dyad). Thus, when we complete the first stage estimations, we have 394 three-element vectors of probabilities (that sum to one). For example, our estimate of these probabilities for the Socialist Left Party-Progress Party dyad in the 1997 Norwegian survey was 84% Correct 11% Incorrect, and 5% Don't Know. These vectors of probabilities then become the dependent variables in a second-stage, compositional model.

A compositional model is simply one in which the dependent variable is a vector of shares that sum to one. In our case the “shares” are the estimated probabilities of each outcome for the average voter. Such models are now common in political science (e.g., Katz and King 1999) and are particularly useful in this setting, since (after an appropriate

transformation of the dependent variable vector) they can be estimated using linear-normal statistical specifications, with which it is much easier to account for the remaining multi-level structure of the data.³⁶

To be clear, though we started with six levels of variation in the data, our separate estimation of multinomial choice models for each party-dyad is equivalent to estimating a party-dyad level “random effects” model with random intercepts and random coefficients for all measured variables.³⁷ Thus, the predicted values that come out of these models already account for the respondent and respondent-dyad levels of variation and the new data set based on these predicted values has only four remaining levels of variation: surveys crossed with dyads (so survey-dyad and dyad levels of variation) and surveys nested in countries. Since this eliminates two of the levels of the hierarchy in the data this becomes a much easier estimation problem in the second stage.

The price we pay for all these advantages is giving up whatever inferential power we would have gained from stacking all the rows of respondent-dyad data together (so, for example, instead of estimating $2 \times 394 = 788$ different coefficients for the impact of income on the probability of one of the responses — with each pair of estimates based on about 1500

³⁶Specifically, one can take log-ratios of the vector of probabilities (choosing an arbitrary baseline category). This leaves a two-element dependent variable that can now be modeled using a linear (multivariate) normal distribution (i.e., a “seemingly unrelated regression”). Further, one can easily extend the model to account for dependencies across observations using linear hierarchical methods. A final benefit is that, once we make this into a normal-linear problem, we no longer need to approximate any integrals, which greatly eases the computational burden.

³⁷Practitioners often fail to appreciate that the least restrictive hierarchical model is simply group-by-group separate estimations. Of course, this gives up any inferential benefits from pooling across surveys, but with plenty of respondents per survey, these benefits are minimal (Shively and Kedar 2005, Achen 2005).

observations — we could have estimated only two coefficients to capture this relationship, based on about 370000 observations).³⁸ However, since the only inferential cost comes from estimates on individual level variables, which are not our chief concern and for which we still have over a thousand observations for each constituent model, this is not a critical concern.

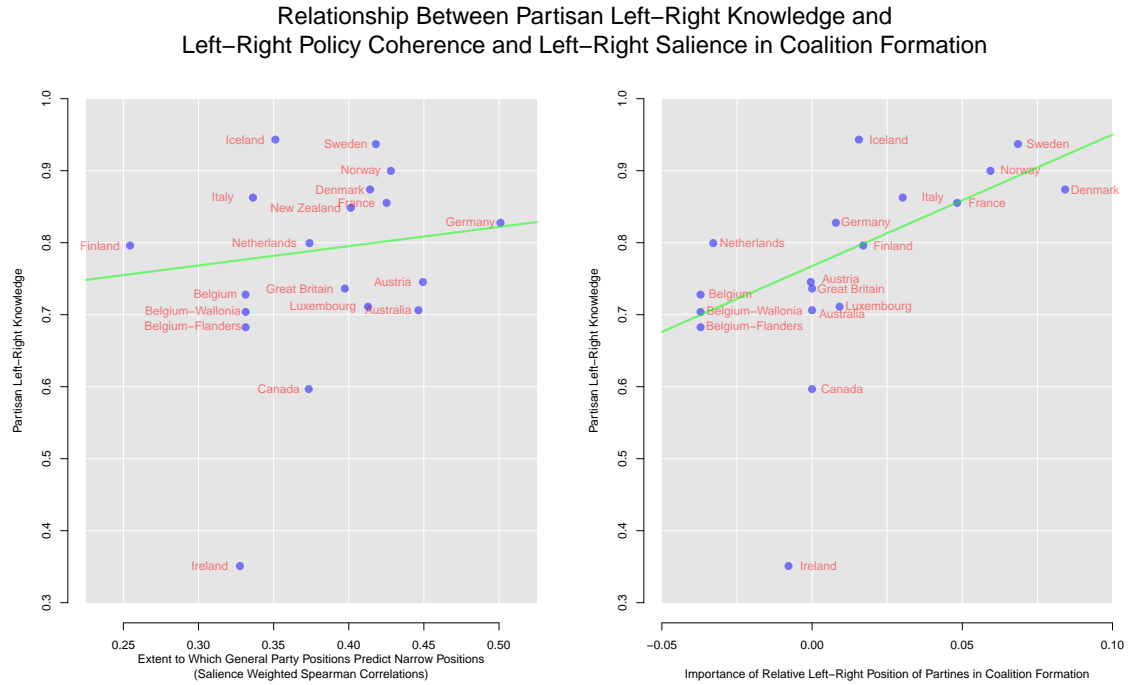
Results

In this section, we present some of the results from the estimation strategies detailed above.³⁹ Before we turn to these results, however, we first present, in Figure 4, a simple graph of the data relevant to our two main hypotheses.

³⁸An alternative modeling strategy, which we reject, is to simply stack the data and estimate a multinomial choice model that does not account for the multi-level structure of the data in the specification of the likelihood function itself; but, instead attempts deal with some of the dependence between observations that is created by the multi-level structure by estimating robust standard errors (grouped on one or another of the levels of the data). This approach essentially trades inconsistency of the parameter estimates (stemming from the unmodeled dependencies in the data) for the extra inferential power gained by stacking the data. As we have suggested in the text, however, this is not a good tradeoff to make when one has plenty of observations in each survey. Further, if the inconsistencies in the coefficient estimates are severe, then robust standard errors give us “correct” estimates of uncertainly around incorrect estimates, so it is unclear what their value really is.

³⁹Given the many alternative specifications discussed above, the results reported here are necessarily selections from these results. As we have repeatedly reported, however, our results are very robust to these many changes in specification. All the various estimates, however, are available from the authors.

Figure 4: Map of Partisan Left-Right Knowledge Across the Western Parliamentary Democracies



The variable on the y-axis of both graphs is a measure of the extent of partisan left-right knowledge in a given electoral context. The x-axis on the left is the measure of the scope of the left-right dimension that uses saliency weighted Spearman’s rank correlations (described above and in Appendix B). The x-axis on the right is the country-specific coefficient on “distance from the PM” in our interactive estimation of Martin and Stevenson’s (2001) model of cabinet partner selection. It has been multiplied by negative one so that as it gets larger (more positive) it indicates a larger role for the left-right positions of parties in cabinet partner selection. We have set the value of this measure to zero for cases in which the only likely cabinets are single party majority cabinets or minority cabinets. This

is an appropriate value for countries in which relative ideology of parties can play no role in selection of the executive (which is completely determined by the election result) or where policy-making coalitions will often shift and be hard to identify.⁴⁰ To make the plots easier to read, we collapse the data in this figure to country means and omit the greatest outlier, Ireland, from the fitted lines.

If both our hypotheses are correct, we should see a positive relationship between each measure and the percentage of voters ordering parties correctly. Clearly, however, there is only strong support for the *LR-coalition heuristic* hypothesis in this graph. While the relationship in the left-hand graph is positive, it is only weakly so, especially compared to the strong positive linear trend on the right. But even more than that, Figure 4 establishes the underlying evidence (and lack of evidence) that drives the estimates in the statistical models that follow. Indeed, one can think of all the elaborate modeling that we present below (as well as the many specifications described but not presented) as an attempt to see if we can do anything (sensible) to the specification of the models to change the relationship that is so obviously apparent on the right side of Figure 4 and so obviously absent on the left. To preview: we cannot. The results below, as well as all our other results (available from the authors) using various sets of control variables, alternative measurements of some variables, alternative treatments of “tied placements,” and different samples of parties all tell the same tale: the estimated effects of our measures capturing the *LR-coalition heuristic* hypothesis are substantively large, never in the wrong direction, and always statistically significant, while the estimated effects of our measures capturing the *LR-policy heuristic*

⁴⁰In the estimation of the country-specific interactive version of Martin and Stevenson’s model, we chose the “excluded country” (Austria) so that the coefficient on the ideological division variable for the excluded country would be essentially zero. Thus, the assignment of zero to the non-coalitional cases makes sense. We note that eliminating these countries from the main analysis does not change our findings.

hypothesis are substantively small, usually statistically insignificant, often of the wrong sign, and change in magnitude and direction with differences in model specification.

These estimated effects (for one model) are presented in Table 3. This table presents the estimated change in the probability of ordering a “typical” dyad correctly, incorrectly, or saying “Don’t Know” based on estimates from the “second-stage” model outlined above. We relegate all the estimated coefficients from this second stage model to Table C1 in Appendix C, since coefficient estimates are not particularly informative about the substantive effects in the multi-equation, compositional models we are using. Instead, we report how the probability of each category changes when each variable moves between its 20th and 80th percentiles (Dummies were changed from 0 to 1). All these estimated changes in probability are calculated for a case in which each dummy variable is zero and other variables are at mean levels. Confidence intervals are simulated in the usual way (King, Tomz, and Wittenberg, 2000).

Table 3: Cell entries are changes in probability when corresponding variable changes from its 20th to 80th percentile (0 to 1 for dummy variables). Numbers in parentheses under the point estimates are 95% confidence intervals. The estimated parameters, as well as estimates of the random effects are reported in Appendix C. We only have 18 countries and 55 surveys included here because two cases from New Zealand were dropped because we lacked any estimate of our main variable of interest for this case. Also, one case from Luxembourg lacked data on several variables.

Variable	Change in probability of response:		
	Correct	Don't know	Incorrect
Importance of left-right to selection of coalition cabinet partners	0.11 (0.04, 0.18)	-0.02 (-0.07, 0.03)	-0.09 (-0.13, -0.05)
Accuracy of left-right in predicting party position on narrow policy dimensions	-0.03 (-0.08, 0.03)	-0.01 (-0.05, 0.03)	0.04 (0.00, 0.08)
Ideological difference between parties in the dyad	0.08 (0.07, 0.09)	0.01 (0.01, 0.02)	-0.09 (-0.10, -0.09)
Number of dyads (parties) included in the survey	0.05 (-0.04, 0.05)	-0.03 (-0.06, 0.00)	0.03 (0.00, 0.06)
Telephone survey (base category is in-person interview)	-0.02 (-0.07, 0.02)	-0.01 (-0.05, 0.03)	0.03 (-0.01, 0.06)
Self-administered survey (base category is in-person interview)	-0.02 (-0.11, 0.08)	0.03 (-0.04, 0.12)	-0.01 (-0.05, 0.04)
Average time parties in dyad have been PM	0.01 (-0.01, 0.02)	0.00 (-0.01, 0.01)	-0.01 (-0.02, 0.01)
Average time parties in dyad were in cabinet (not as PM)	0.02 (0.00, 0.04)	-0.01 (-0.02, 0.00)	-0.01 (-0.03, 0.00)
Party names that might mislead respondent in ordering dyad	-0.01 (-0.06, 0.04)	0.00 (-0.02, 0.03)	0.01 (-0.03, 0.04)
Party names that might help respondent in ordering dyad	0.03 (-0.02, 0.07)	0.01 (-0.01, 0.04)	-0.04 (-0.07, -0.01)
Average size of parties in dyad	0.02 (0.00, 0.04)	-0.01 (-0.02, 0.00)	-0.01 (-0.02, 0.00)
Number of Countries		18	
Number of Surveys		55	
Number of Unique Dyads		187	
Number of Survey-Dyads		394	

Clearly, the estimated impact of the variable measuring differences (across contexts) in the importance of the left-right to coalition formation is large and strongly significant, while the effect of differences in the scope of the left-right dimension is not. Further, the former effect is substantively larger than all the other variables included in the model, including the ideological distance between the parties (and both of these effects are much larger than the third largest effect). The latter variable is, of course, strongly significant and positive (we would be very concerned with our specification if it were not) indicating that respondents are better at ordering very ideologically different parties than parties that are ideologically similar. In addition, it is encouraging that all the other control variables have estimated effects that are what one would expect (though many effects are not statistically significant). For example, dyads containing larger parties are easier to order than dyads with smaller parties.⁴¹ Again, Table C1 reports estimates from specifications with different measures of the main variables of interest and all of these estimates paint the same substantive picture.

While our main interest is on the contextual variables that impacts partisan left-right knowledge, our results for some of the individual-level variables are also of interest. Table 4 aggregates the results of our 433 separate estimates of individual level variables for each survey.⁴²

⁴¹The results for the coefficients from stacking the data (available from the authors) are largely compatible with the results in Table 3. Of course, the standard errors from this method differ dramatically (and are much too small) since they do not model the hierarchical dependencies in the data. This can be partially alleviated by using robust standard errors clustered on the survey-dyad or respondents, but even in this case, the specification is wrong and the standard errors appear too large.

⁴²These are the average change in probability (over all the separate estimates) for each category of the dependent variable when the relevant indicator variable changes from 0 to 1. The confidence intervals were simulated for each of the separate models and then averaged for presentation in Table 4.

Table 4: Substantive effects for the individual-level variables from the first-stage of the two-stage model: Cell entries are changes in probability when corresponding variable changes from 0 to 1. Numbers in parentheses under the point estimates are 95% confidence intervals. All results are averages of 433 separate estimations. There are more separate estimates here than survey-dyads in Table 3 (433 vs. 394) because of the exclusion of the surveys mentioned in the note to that table (each of which eliminated a number of dyads).

Variable	Change in probability of response:		
	Correct	“Don’t know”	Incorrect
Attended College (dummy)	0.12 (0.05, 0.19)	-0.06 (-0.11, -0.02)	-0.06 (-0.12, 0.00)
Female	-0.07 (-0.14, -0.01)	0.08 (0.02, 0.13)	-0.01 (-0.06, 0.05)
Does respondent themselves between the parties in the dyad? (dummy)	0.19 (0.12, 0.27)	-0.06 (-0.12, -0.01)	-0.13 (-0.20, -0.07)

We include the first result simply to reassure readers that our estimates of the individual level models were sensible. The models tell us clearly that the effect of education (in this case a dummy variable for college attendance) has a strong effect on individual level differences in political knowledge, as it should. The two other results are included because they provide some new information that should be of value to the literature on individual differences in political knowledge. The first is that our results strongly confirm the gender bias found in other studies of political knowledge and, for the first time, generalizes it to a

wide set of modern democracies. Further, not only is the effect itself confirmed, but there is also very strong evidence for the mechanism that has been suggested to explain this effect (that women are more willing to say “Don’t Know” than men). Specifically, nearly the entire shift in probability due to being female (a quite large 8%) moves between “Correct” and “Don’t Know.” Thus, in these data, women are less likely to be correct than men, but are no more likely to be wrong. Instead, they are much more likely to say “Don’t Know” — just as claimed by Mondak and Anderson (2004).

The second interesting finding here is that individuals who locate themselves ideologically between the two parties in the dyad are substantially more likely to correctly order the dyad than those who place themselves to the left or right of both parties. Indeed, the effect is about twice as large as having attended college (which the second largest of the individual effects).

Discussion and Conclusions

In this paper we argue that knowledge of the left-right positions of parties should vary over populations in different contexts because the usefulness of the left-right metaphor varies over those same contexts. We fleshed out this simple idea by identifying two heuristics that depend on partisan left-right knowledge as informational inputs: a *LR-policy heuristic* and a *LR-coalition heuristic*. We argued that where these heuristics were salient (voters needed their outputs to effectively participate in politics) and accurate, they would be used, and so incentives for elites to provide, and voters to collect, partisan left-right knowledge would be high. We confirmed this hypothesis for the *LR-coalition heuristic* but not for the *LR-policy heuristic*. Given the nature of each hypothesis what can we conclude? Clearly, we can say that our data analysis is consistent with the idea that thinking about policy-making coali-

tions (or at least governing coalitions) is a salient task for voters in many parliamentary democracies and that when they have an accurate heuristic that lets them leverage partisan left-right knowledge into expectations about which coalitions will form, they seem more likely to invest in (or otherwise acquire) this knowledge. In contrast, our negative result suggest either that (1) policy aggregation is not an important function of the left-right in many systems and/or (2) voters' use of this heuristic either (a) does not vary over contexts with its accuracy (or scope) or (b) does not incentivize the dissemination and collection of left-right knowledge in the way our theory suggests.

In our view, option (1) is by far the most likely of these possibilities to be true. First, the previous literature raises significant doubts about the extent to which voters actually infer policy positions from a general left-right understanding of the placements of parties. Second, as we discussed in an earlier footnote, using the left-right to infer more narrow policy positions requires a great deal more knowledge than simply partisan left-right knowledge. Specifically, it requires the voter to understand the mapping between the left-right positions of parties and each narrower policy dimension. Thus, we interpret our results as contributing to the case that the main function of the left-right in parliamentary politics is not its supposed aggregative policy function, but rather helping voters understand partisan political completion as it plays out in the making and breaking of policy-making coalitions.

Our theoretical arguments and empirical findings make at least one important contribution to the study of heuristics in general and several others to the study of comparative politics and political behavior. To our reading of the multidisciplinary research on heuristics (spanning computer science, economics, political science, psychology, sociology, and more), this is one of the first studies to draw a robust empirical connection between the usefulness (or ecological rationality) of a heuristic and the extent to which individuals have

its informational inputs.⁴³ As such it suggests that work on the ecological rationality of heuristics may have a greater explanatory reach than has been previously appreciated.

For comparative politics and political behavior more generally, this study also presents several important advances. First, this is, to our knowledge, the largest cross-national investigation of political knowledge to be based on a measure of political knowledge that can be reasonably compared across countries and over time.⁴⁴ That is, while several studies have attempted to measure variation in political knowledge, these studies have typically found themselves at the mercy of knowledge measures that are unsuitable for cross-national comparison. Questions, for example, that ask respondents to match photographs to names or names to cabinet posts are, for many reasons, not readily comparable across contexts and time. Second, this is not only one of the first studies to advance a theory of cross-national variation in political knowledge, but one of the first to advance any theory of the institutional foundations of political knowledge acquisition. Finally, both the theory and our empirical results give support to the general idea that voters can use simple heuristics to manage the complexity of their political systems and that the extent to which this is true varies with the enduring empirical regularities (and likely corresponding institutions) that characterize different systems.

⁴³There has been, however, research on active information search.

⁴⁴Gordon and Segura (1997) come closest to being an exception. They measure political knowledge by comparing cardinal distances in the perceived positions of parties and their “true” positions.

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Appendix A1

Which parties to include?

The first measurement issue that we had to contend with was deciding which parties to include. We would like to make the task of ordering parties as comparable as possible across contexts and we can do this both by managing the sample (for example, excluding party-dyads that include regional parties or protest parties) and by controlling for dyad characteristics in our statistical models (for example, including a measure of whether a dyad included regional or protest parties). As such, our statistical models will include controls for many different characteristics of party-dyads that we think impact how well individuals are able to order them ideologically. However, these kinds of controls can never capture everything that might be relevant to making one dyad more difficult to correctly order than another. Thus, we also examined results for five different sub-samples of our data that are increasingly restrictive in terms of which parties the respondents ultimately compare. The idea is that in the more restrictive samples, we can be increasingly certain that the difficulty of the party-placement task is the same across different political contexts. The five samples are:

1. All available party-dyads: Between 2 and 11 parties per survey — so between 1 and 55 party-dyads per survey
2. Parliamentary party-dyads: excludes all parties that have failed to win at least one seat in parliament in the last election before the survey was administered (or, in the case of pre-election surveys, the election following the survey)
3. Only dyads in which both members are “important” parties: Excludes single-issue parties, regional parties, very small parties, and parties that do not compete on the traditional left/right dimension of politics (most importantly, excludes Green parties)
4. Two or Three leading parties: The largest party on the left and right and the Christian center Parties in the Benelux Countries
5. Two leading parties on the Left and Right: The largest party on the left and right, respectively — excluding the Christian center in the Benelux Countries

While we applied the statistical models explained in the main text to each of these sets, the results were strikingly robust to differences in the sample used — a result we attribute to the extensive set of controls variables about the characteristics of parties and party-dyads that we collected and included in our statistical models. Consequently, we only report statistical results for the third sample (of “important” parties) in the main text.

What to do with “Don’t Know” responses?

There is a large literature in social science on the meaning and proper treatment of the “Don’t Know” response and a smaller literature that explores the question in the specific context of knowledge surveys in political science (e.g., Mondak and Anderson 2004). While such responses have often been excluded from the analyzed sample, these literatures make it clear that “Don’t Know” responses are unlikely to be randomly distributed in the sample and their exclusion could lead to severe biases in resulting inferences. For example, Mondak and Anderson point out that women are far more likely to give a “Don’t Know” response in answer to a factual question than men, even when they are otherwise equally informed. These kinds of findings not only make us skeptical about deleting “Don’t Know” responses from the sample, but should also raise questions about another seemingly reasonable strategy for dealing with these responses. This strategy, which is by far the most common one used in studies of political knowledge, simply counts “Don’t Know” responses as incorrect. However, if biases like the gender bias identified by Mondak and Anderson apply to our data (and our analysis below shows they do), counting “Don’t Know” responses as incorrect will cause us to mistakenly assign incorrect party placements to women more often than to men — even if the two groups have the same underlying level of knowledge. To see this, consider an unknowledgeable man who is unwilling to admit his ignorance and so guesses about the placement of the parties in a given dyad. His probability of recording an incorrect response is approximately 50%.⁴⁵ However, the same unknowledgeable woman, who admits her ignorance and gives a “Don’t Know” response, has, under this coding scheme, a 100% probability of recording an incorrect response. As Mondak and Anderson have pointed out, if “Don’t Know” responders had instead answered the questions by guessing randomly, they would have made fewer mistakes. Consequently, he argues, a more accurate representation of the “Don’t Know” voters’ level of knowledge is the probability they would have made a mistake if they had simply guessed. The easiest way to simulate this kind of random guessing is simply to assign each “Don’t Know” response a random value (from a uniform distribution on the left right scale) before making our dyadic comparisons. We have done this, but do not rely on it as our primary solution to the problem (again, this choice is not consequential for the substantive inferences we make — though, compared to the method described below, it limits how much information we can extract from the data).

Rather than exclude “Don’t Know” responses, count them as incorrect, or randomize responses for them, we have instead treated “Don’t Know” as a legitimate response in its own right and attempted to model this response along with correct and incorrect responses in an appropriate multinomial statistical model. This allows us to model the covariates associated with both knowledge and the willingness to admit lack of knowledge, and completely circumvents the kinds of problems illustrated by the gender differences discussed

⁴⁵This is just an illustration so we don’t worry about calculating the exact probability accounting for ties.

above.

What to do with “tied” responses?

In some cases the respondent will say that both parties have the same position, when in fact one party is to the right or left of the other. We can count these responses as wrong (they do not get the parties in the right order) or we can count these as right (they don’t actually get them in the wrong order) and the choice of rule seems fairly arbitrary. Consequently, we have produced results for both rules and can show that they are extremely robust to this choice. In what follows, we report the results that count ties as incorrect.

Appendix A2

How to measure the parties’ “true” positions?

To decide whether a respondent has ordered a given pair of parties correctly, we need to know the “true” ordering of the parties. We explored four different ways to assign parties “true” left-right placements (and so “true” orderings). One method gave parties the score that was assigned to them by the experts responsible for the implementation of the CSES survey module in each country. These assignments were made on the same scale as the one asked in the CSES surveys. Unfortunately, however, this could only be done directly for the surveys that were part of the CSES survey project (so only 40% of our sample surveys).

A second method used the expert judgments of party positions that are available from Laver and Hunt’s 1990 study (supplemented by Laver and Benoit’s 2006 update of these data).⁴⁶ These assignments were rescaled to match the 0 to 10 scale used in most of the voter surveys and provided coverage of most parties in all of the surveys included in the sample.

A third method of assigning ideological positions to parties uses the left-right positions scaled from the Comparative Manifestos Project data to place parties at each election (we use the scores from the manifesto published nearest in time to a given survey), again rescaled to match the 0-10 scale voters used. This method provides estimates for 97% of the sample.

The final method of assigning parties left-right positions simply takes the mean ideological placement of each party, over all the respondents in a given election survey, as the estimate of the “correct” position of the party at that time.⁴⁷ This method has the

⁴⁶Specifically, for any given survey, we used the version of the Laver/Hunt/Benoit survey that was closest to the time the survey was given.

⁴⁷This method is perhaps the most easily criticized because it results in a measure of citizen knowledge that compares individuals’ opinions to the opinion of the mean individual — thus creating the possibility of endogeneity. It is important then to emphasize that we report this measure only because of its better coverage. None of the results change when using other measures, because, as it turns out, the average voter and the experts agree on the relative placements of parties.

advantage that it is calculable for 100% of the parties in our sample.

In choosing which of these methods to use (or whether to report results for all of them), we were very much aware of the large literature in comparative politics that has examined the consistency of different methods for measuring the left-right positions of parties (e.g., expert survey based, manifesto based, election survey based). While some in this literature emphasize the broad compatibility of different measures, a more usual theme has been that there are substantial differences in positions produced by different methods. Given this, we were initially surprised by the fact that none of our substantive results depend at all on which method we use for placing parties (other than the drastic loss of sample if we rely on the first method). While this is an encouraging result, it is sufficiently odd to warrant skepticism. Why would our results be immune to the (often large) differences in party placements across methods that have been demonstrated in the literature?

The obvious place to start in looking for an answer is the unlikely possibility that previous analyses have simply been wrong and overemphasized the disparities in these measures. While a complete analysis of this question is neither the purpose of this paper, nor, as we will see, necessary, we can quickly get a sense of the extent of compatibility between our different measures of parties' left-right positions by examining how closely they correlate. Table A1, provides these correlations.

Table A1: Correlations between different measures of the left-right placement of parties for all the parties in the sample

	LHB expert placements	Average survey placement
Average survey placement	0.67	
Manifesto scores	0.53	0.74

Some of these correlations are relatively high; nevertheless, they are, in our view, consistent with the skeptical conclusions about the compatibility of different measures of left-right ideology that is reflected in much of the literature. Surely a correlation of .53, or even .74, is not enough to support the almost complete immunity of our results to the choice of how to measure the true positions of parties. Thinking more carefully about how we use the data in this study, however, resolves the question. Specifically, while the correlations in Table A1 use the cardinal information in the measurements, our design relies only on the ordinal information. We are not concerned with absolute placements of parties, but with relative placements. Thus, we should not examine how closely correlated these different measures are, but how often they would place pairs of parties in a different left-right ordering. If we examine data in this way, we can see immediately why our results will not depend on the choice of how we measure the left-right positions of parties.

Figure A1: Cases in the upper right and lower left quadrants are ones in which the measures agree on the ordering of parties and ones in the upper left and lower right quadrants are ones in which they disagree. The percentage is the percent of all comparisons that agree.



Figure A2: Cases in the upper right and lower left quadrants are ones in which the measures agree on the ordering of parties and ones in the upper left and lower right quadrants are ones in which they disagree. The percentage is the percent of all comparisons that agree.



Figure A1 graphs the signed ideological difference between each pair of parties in our sample using one of our left-right measures against the differences for the same pair of parties using each of the others. Thus, those cases in the upper right and lower left quadrants are ones in which the measures agree on the ordering of parties and ones in the upper left and lower right quadrants are ones in which they disagree. The listed percentage is the percent of all comparisons that agree. As one can see, in these graphs (which include all parties - even regional, single issue parties, and protest parties) there is a remarkable level of agreement among the different measures in the ordinal placement of parties. The situation improves even more if we examine only the large parties on the left and right (the fifth sample of cases we described above and for which we will present results). Figure A2 gives these results.

Here, the agreement between measures is essentially perfect, with even the exceptions proving the rule. Specifically, the two parties represented by the points in the south-east quadrant of the upper left graph and the north-west quadrant of the lower left graph are

Ireland’s Fianna Fail and Fianna Gael, the two large parties in our countries that almost any scholar of European politics would pick as the most ideologically indistinct (at least on a left-right scale). Given the broad compatibility of these measures (at least with respect to the information we take out of them), in what follows we only report results that use the method for which we have the best data coverage — which assigns the “true” position of parties according to their mean survey placement.

Appendix A3

Table A2: Number of party-dyads and survey-dyads in each country (only cases used in the estimation reported in Table C1)

Country	Number of survey-dyads	Unique dyads across surveys
Australia	5	3
Austria	6	3
Belgium	3	3
Belgium (Flanders)	6	5
Belgium (Wallonia)	2	1
Canada	9	6
Denmark	70	33
Finland	18	7
France	21	13
Germany	27	8
Great Britain	15	4
Iceland	7	6
Ireland	32	10
Italy	35	20
Luxembourg	12	7
Netherlands	30	9
Norway	36	24
Sweden	60	25
Total	394	187

Appendix B

Measurement of variables in second-stage models

Importance of relative ideology to selection of cabinet partners: Estimated by interacting the formateur distance variable in Martin and Stevenson's (2001) statistical model of cabinet formation with country dummies. The Baseline category was chosen so that zero was meaningful (indicating no effect of the variable) and so the value of the variable for non-coalitional systems was set to zero.

Ideological difference between parties in the dyad: absolute value of the difference in the "true" positions of the parties (where measurement of these "true" positions is described in the text)

Number of dyads included in the survey: This is a deterministic function of the number of parties included in the survey. Each survey included all parties represented in the legislature and almost all parties that ran for seats in the legislature, excluding only very insignificant parties. The measure is preferred to the effective number of electoral (or legislative) parties because it captures not only the complexity of the system, but also the difficulty of the immediate task being asked the respondent in the survey (to place, for example, ten parties as opposed to five). The correlation between the measure and the effective number of parties is .70.

Survey type: This is captured with two dummy variables for "Self-Administered Survey" and "Telephone Survey." The baseline category is an in-person interview. 51% are telephone, 11% self-administered, and 28% in-person.

Expert judgments of the overall importance of the left-right: Laver and Hunt (1992) and Benoit and Laver (2006) provide the average expert rating of the importance of each of 20 different dimensions to each party. This measure is the average of this value over the parties in a system for the main left-right dimensions (tax/welfare). We use the edition of the expert survey closest to the date of each election survey.

Percent of average party manifesto devoted to left-right for all parties in the system: For each party we calculate the percentage of the most proximate election manifesto that the party devoted to left-right topics, using the categorization of left-right topics adopted by Budge and Laver (1992) and used by scores of subsequent studies. The measure is the average of this score over the parties included in a given survey.

Saliency-weighted average Spearman's rank correlations between Lowe et. al.'s (2011) 14 policy categories and left-right placements: We used data from

Lowe et al. (2011), who define 14 policy areas in which party positions can be defined (these are about foreign alliances, militarism, internationalism, the EU, constitutionalism, decentralization, protectionism, Keynesianism, nationalism, traditional morality, multiculturalism, labor policy, welfare policy, and education spending). The absolute value of the Spearman rank correlation between each of these categories and their aggregate measure of the left-right was calculated for all the parties in the data set for each country, for the period from 1980-2010. So, for example, if the ordering of parties on the left-right scale predicted the ordering of the parties (during this period in a given country) on welfare policy, then the score would be high for this dimension. Once we had the scores for each dimension (for each country in this time period), we weighted these by the average relative importance of the dimension during the same time period using Lowe and Benoit's importance scores for each dimension (for this purpose, these were transformed so that they summed to one over the issues (making them explicitly relative to each other) and then these scores were multiplied by the absolute value of the Spearman rank correlations for their respective dimensions (and this weighted sum, which equals the weighted average in this case, used as the final score).

Salience-weighted average multiple regression coefficients of left-right placements on Lowe et. al.'s (2011) 14 policy categories: This was calculated exactly as the score above, except multiple regression coefficients were used instead of Spearman's rank correlation.

Variance explained by the first dimension of Laver and Benoit (2006)'s factor analysis of expert placements of parties on 9 policy dimensions: The total variance explained by the first dimension in Laver and Benoit's (2006) principal components factor analysis (with varimax-rotated loadings) of their expert survey data (reported in their Table 5.7a). This included nine issue areas (Tax and spending policy, social policy, environmental policy, decentralization, EU peacekeeping, immigration, EU accountability, EU authority, and deregulation) and attempted to identify the extent to expert placements of parties in these nine areas could be explained by a small number of factors. In focusing on the first factor, regardless of its substantive content, this measure — unlike all the others used here, which all focus explicitly on a substantive left-right dimension — simply asks how well voters would have done using any (or the best) underlying aggregative dimension (however defined — though in most cases in their results the first dimension looks like the left-right) to order more narrow policy dimensions.

Average time parties in dyad have been PM or in the cabinet (not as PM): For each party we calculate the percentage of the months between the first post-war election and the date of the survey that the party served in the cabinet as PM or as a non-PM partner. The measures for a dyad are the average of the scores for the two parties in the dyad. Versions of this measure that weighted more proximate service made not difference.

Party names that help/hurt respondent in ordering dyads: we account for the fact that some parties give fairly obvious left right information in their names. To do this we marked all cases in which the party names contained words that were the same as those used to anchor the left-right scale (as printed in the original questionnaires, in the native language). So, for example, the Danish “Radikale Venstre” and the Italian “Partido Democratico Sinistra” both use the same word for “left” in the party name that was used in the survey questionnaire to label an endpoint on the left-right scale on which respondents were asked to place parties. From this, we coded a “helpful” instance as one in which the party in the dyad so named was actually on that side its dyad partner (i.e., if the party name had “left” in it, then we coded dyads in which the party was left of its partner as helpful cases and ones in which it was to the right of its partner as likely to hurt).

Average size of parties in dyad: the average for the two parties in the dyad of the parties’ vote in the last election.

Appendix C

Main and alternative model results

The table on the following page contains the results to main model which produced the substantive effects reported in Table 3 in the main text as well as the models estimated with alternative measurements for the *LR-policy heuristic* tests. Note that the parameter estimate on the relative importance of left-right in determining coalition formation is significant in each estimation — leading to a positive change in the log ratio of correct to incorrect response probability. This supports the *LR-coalition heuristic* hypothesis. Conversely, there is no support for the *LR-policy heuristic* hypothesis.

Table C1: Full estimates from main text model (Model 1) and ancillary estimates.

	Model 1		Model 2		Model 3		Model 4		Model 5	
	Correct/ Incorrect	Don't Know/ Incorrect	Correct/ Incorrect	Don't Know/ Incorrect	Correct/ Incorrect	Don't Know/ Incorrect	Correct/ Incorrect	Don't Know/ Incorrect	Correct/ Incorrect	Don't Know/ Incorrect
Test of LR-coalition hypothesis										
Importance of Relative Ideology to Selection of Cabinet Partners	5.752 (3.100)	0.246 (0.067)	7.202 (3.389)	-0.093 (-0.025)	7.246 (3.711)	2.375 (0.651)	4.379 (1.727)	-2.046 (-0.377)	6.024 (2.694)	-1.165 (-0.257)
Test of LR-policy hypothesis										
Percent of Average Party Manifesto Devoted to Left-Right for all parties in the system	0.410 (1.017)	0.248 (0.665)	0.070 (1.526)	-0.058 (-0.527)						
Expert Judgments of the overall importance of the Left-Right										
Salience-weighted average Spearman correlation between Lowe et al.'s (2011) 14 policy categories and general left-right					-1.78 (-1.532)	-2.631 (-1.214)				
Salience-weighted average regression coefficients of Lowe et al.'s (2011) 14 policy categories and general left-right							1.986 (1.076)	0.899 (0.227)		
Variance explained by the first dimension of Benoit and Laver's (2006) factor analysis of expert placements on 14 policy dimensions									-0.002 (-0.283)	0.001 (0.037)
Controls										
Number of dyads (parties) included in the survey	-0.002 (-0.332)	-0.016 (-1.478)	-0.005 (-1.026)	-0.017 (-1.605)	-0.005 (-1.012)	-0.020 (-1.920)	-0.003 (-0.653)	-0.016 (-1.423)	-0.006 (-1.092)	-0.014 (-1.136)
Ideological difference between parties in the dyad	0.388 (21.500)	0.357 (25.420)	0.387 (21.450)	0.357 (25.360)	0.387 (21.490)	0.357 (25.350)	0.389 (21.500)	0.357 (25.150)	0.389 (20.800)	0.362 (24.980)
Telephone survey (base category is in-person)	-0.161 (-1.878)	-0.182 (-0.904)	-0.174 (-0.904)	-0.159 (-0.781)	-0.155 (-1.843)	-0.180 (-2.089)	-0.142 (-1.843)	-0.154 (-0.745)	-0.145 (-1.507)	-0.137 (-0.600)
Self-administered survey (base category is in-person)	-0.360 (-0.360)	0.119 (0.324)	-0.018 (-0.128)	0.145 (0.393)	-0.023 (-0.159)	0.209 (0.567)	-0.019 (-0.132)	0.08 (0.215)	0.011 (0.067)	0.067 (0.149)
Average time parties in dyad have been PM	0.283 (1.185)	0.192 (1.033)	0.298 (1.247)	0.190 (1.019)	0.279 (1.171)	0.188 (1.007)	0.287 (1.187)	0.191 (1.014)	0.343 (1.375)	0.275 (1.414)
Average time parties in dyad were in cabinet (not as PM)	0.452 (1.422)	-0.049 (-0.190)	0.504 (1.575)	-0.031 (-0.119)	0.543 (1.725)	-0.021 (-0.083)	0.475 (1.484)	-0.040 (-0.152)	0.525 (1.569)	0.038 (0.142)
Party names that mislead respondent in ordering dyad	-0.037 (-0.297)	-0.038 (-0.391)	-0.015 (-0.122)	-0.028 (-0.292)	-0.037 (-0.295)	-0.030 (-0.311)	-0.011 (-0.087)	-0.027 (-0.279)	-0.018 (-0.145)	-0.027 (-0.273)
Party names that help respondent in ordering dyad	0.212 (1.953)	0.226 (2.676)	0.231 (2.118)	0.231 (2.734)	0.207 (1.908)	0.230 (2.724)	0.230 (2.107)	0.231 (2.712)	0.228 (2.077)	0.230 (2.688)
Average size of parties in dyad	0.007 (1.333)	-0.006 (-1.545)	0.007 (1.402)	-0.007 (-1.551)	0.008 (1.477)	-0.006 (-1.473)	0.006 (1.198)	-0.007 (-1.584)	0.008 (1.426)	-0.007 (-1.715)
Constant	-0.671 (-2.486)	-1.211 (-3.560)	-1.455 (-2.128)	-0.210 (-0.126)	0.226 (0.482)	-0.021 (-0.023)	-0.657 (-2.540)	-1.123 (-2.130)	-0.308 (-0.646)	-1.131 (-1.193)
Random effects										
sd(Distribution of country random effects)	0.176 (3.982)	0.285 (1.999)	0.228 (3.945)	0.275 (1.985)	0.170 (4.300)	0.198 (1.276)	0.175 (3.639)	0.329 (2.179)	0.209 (3.660)	0.353 (1.901)
sd(Distribution of survey random effects)	0.229 (8.814)	0.746 (2.458)	0.205 (8.447)	0.742 (2.514)	0.224 (9.079)	0.755 (2.281)	0.231 (8.56)	0.728 (2.501)	0.231 (8.178)	0.769 (1.972)
sd(Distribution of dyad random effects)	0.290 (12.500)	0.206 (13.230)	0.289 (12.430)	0.206 (13.170)	0.287 (12.460)	0.206 (13.170)	0.291 (12.330)	0.208 (13.050)	0.290 (11.980)	0.209 (12.760)
sd(Distribution of residual)	0.313 (21.840)	0.257 (24.660)	0.314 (21.810)	0.257 (24.570)	0.314 (21.750)	0.257 (24.570)	0.313 (21.740)	0.258 (24.430)	0.316 (20.990)	0.260 (23.740)
Number of survey-dyads	394	394	394	394	394	394	386	386	365	365

Parameter estimates with *t*-ratios in parentheses.