RICE UNIVERSITY

Essays on Voter and Legislative Behavior in Coalitional Democracies

by

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A Thesis Submitted
in Partial Fulfillment of the Requirements for the Degree

Doctor of Philosophy

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April, 2012
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Abstract

In this dissertation I examine the reciprocal relationship between voters and political parties in coalitional democracies in three essays. First, I investigate how voters alter their perceptions of political parties in response to their participation in coalition cabinets. I argue that voters view coalition participation as broad and wide-ranging policy compromise and update their perceptions of the policy positions of cabinet participants accordingly. I find that voters perceive coalition partners as more similar than parties that are not currently coalesced, all else equal. In the following essay, I examine the electoral repercussions of this shift in perceptions by proposing a model of voting that considers coalition policy-making. I argue that voters will equate the policy compromise they perceive in the cabinet with a failure to rigorously pursue the policies they were promised and that voters who perceive compromise will punish the incumbent. The data reveals that this perception may cost incumbent cabinets about 2.5% of their voteshare. Finally, I move from the electorate to the legislature to investigate if and how these perceptions condition legislative behavior. The previous essays suggest that coalition parties have substantial motivation to differentiate themselves from their partners in cabinet when voters perceive them as becoming more similar. I test this argument by examining partisan behavior in legislative review. The data show that coalition partners who are perceived as more similar are more likely to amend one another’s legislation.
Acknowledgements

The completion, or, for that matter, the launching of this project, would not have been possible without the help and support of several people.

Academically, the feedback and assistance of my advisors Lanny Martin and Randy Stevenson was invaluable, as was the training I received from Lanny, Randy, and Royce Carroll both in and out of the classroom. I believe that the quality of my work should reflect the quality of their mentorship and I will spend my career trying to make good on my half of the equation. I would also like to thank Rick Wilson who, like Lanny, Randy, and Royce, aided in shaping me as a (hopeful) scholar, but who also provides the ideal example of a citizen of the discipline, regardless of whether or not he is willing to admit it. Finally, my work also benefitted from conversations with colleagues outside of Rice. I would like to thank Thomas König, Oli Proksch, and especially Georg Vanberg for taking the time to chat with me about my research. I came into graduate school wanting to study silly things in a silly way and, because of these gentlemen, I may actually learn and share something of value. That would be nice.

Personally, graduate school is hard and I have my family to thank for making it out in one piece, in a relatively timely fashion: Logan Fitzgerald, Anne, Chris, Destinee, and Karla Fortunato, Jennifer Monroe, and Colleen Petrik. I would like to thank my friends as well. Locally, I am grateful to Bobbi, Brent, Erica, Jesse, Johnny, and Sean as well as the greater Valhalla community for always providing moral (and occasionally physical) support and to Lanny and Randy for getting me to Europe and generally providing novel social experiences. Back in Connecticut, I thank Jim, Marty, and Patrick for, you know, being rad.
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Chapter 1

Motivation

The majority of the world’s industrialized democracies are governed by coalition cabinets. As such, governing coalitions have been the focus of large amounts of political research. Yet, despite the normative importance of understanding coalition governance and the energy devoted to researching it, we know relatively little about democratic accountability in these systems. That is, we possess scant understanding of the connection between parties and voters in parliamentary democracy where coalition governance is the norm. We know little of how the processes of coalition cabinet formation and policy-making influence the manner in which voters perceive, communicate with, and ultimately support or reject political parties. Similarly, we know almost nothing about how voter perceptions and behaviors condition legislative behavior in the policy-making process. In this dissertation, I provide three essays that seize this opportunity in the literature. The first investigates how coalition governance affects the way voters perceive the policy positions of cabinet parties. The second examines the electoral implications of these perceptions. In the third essay, I turn from the electorate to the legislature and ask how these perceptions and their electoral implications condition the legislative behavior of coalition parties.
What do we know about coalition cabinets?

The lion’s share of coalition research has been devoted to studying cabinet formation and reasonably so. Knowing who will get into cabinet allows us to forecast, albeit roughly, what types of policy we can expect from the government. Over the years, our ability to predict the formation of cabinets has gotten quite impressive. With the most up-to-date data, the canonical empirical model of cabinet formation (Martin and Stevenson 2001) — which builds on decades of theoretical research including, but not limited to Austen-Smith and Banks (1988), de Swaan (1973), Gamson (1961), Laver and Schofield (1990), Laver and Shepsle (1996), and Riker (1962) — successfully predicts the outcomes of coalition bargaining episodes 53% of the time (Duch et al. 2010). Given the number of potential outcomes to each episode (typically several thousand), 53% is extremely successful.

This model has given empirical support to several implications of the formal work cited above and others. We have learned that coalitions are more likely to form when they are ideologically compact and when they effectively divide the opposition. We have also learned that coalitions that include anti-system parties are incredibly unlikely to form, minority coalitions are quite rare, particularly where an investiture vote is required, and that pre-electoral pacts increase the likelihood of co-governance for the parties that enter them.

After cabinet formation, the discipline has spilled the most ink investigating cabinet duration; how long will the cabinet last after it has been formed? From several formal models of cabinet durability (e.g., Diermeier and Merlo 2000; Laver and Shepsle 1998; Lupia and Strøm 1995) we produced several empirical models (e.g.,
Diermeier and Stevenson 1999, 2000; King et al. 1990; Warwick 1992). We have learned that majority cabinets tend to last longer as do cabinets who have survived investiture. Polarized party systems tend to limit the viability of alternative coalitions and thus increase the life of cabinets, but all cabinets become more fragile in the midst of economic crisis. The duration literature has been, more or less, dormant for a decade, but scholars have recently returned to the question as methodological advances have allowed them to begin to solve the problem of selection bias in cabinet duration (Chiba et al. 2011). That is, we suspect that cabinets are formed with an eye toward durability, thus the parameters of formation must be factored into the investigation of their duration. Therefore, the proper question to ask is not, “how long with this cabinet survive?” but, rather, “how long will this cabinet survive, given that it has formed?”

The efforts to understand formation and duration comprise the overwhelming majority of coalition research. Unfortunately, these studies tell us little about how coalition cabinets actually govern. For example, we know that parties face substantial agency risk in delegating policy authority to ministers representing parties with disparate policy interests. Recently, a new segment of the literature has evolved to address this question. Carroll and Cox (2012), Kim and Loewenberg (2005), and Thies (2001) have asked how parties can monitor each other by strategically placing committee chairs and junior ministers. However, these studies, though informative, are essentially extensions of the question of coalition formation and therefore tell us little about how coalitions govern.

There are, however, a few studies on coalitional policy-making. For example, Martin (2004) presents an empirical model coalitional agenda-setting. Martin and
Vanberg (2004, 2005, 2011) present theoretical and empirical models of how coalition parties utilize legislative institutions to mitigate ministerial drift in policy-making. Martin and Vanberg (2008) investigate how coalition parties can communicate with their supporters via legislative debate. These types of studies help us understand how coalition cabinets actually govern; how they deal with the day-to-day struggles of being forced to coexist, and even cooperate, with their competition. This is integral to the future of coalition research.

What do we know about coalitional voters?

We know surprising little about coalitional voters. When Downs came to the coalitional voter in his 1957 work on prospective, outcome-oriented voting, he concluded that when these voters contemplated the complexity of the decision, all of the different factors they would have to weigh in order to vote “rationally,” they would simply give up. Similarly, when Powell and Whitten (1993) considered the coalitional voter in their canonical work, they concluded that retrospective voting would be too difficult in these systems as well. As a result, we have yet to pay due consideration to how coalitional voters cope with the complexities of post-electoral bargaining in order to cast prospective, outcome-oriented votes; just as we have yet to consider how they may interpret and react to collective policy-making in order to cast retrospective, outcome-oriented votes.

Because of the pessimistic conclusions of Downs (1957) and Powell and Whitten (1993), study of the coalitional voter has, with very few exceptions, simply ignored the fact that the institutions of coalition policy-making — the processes that aggregate
votes into policy — fundamentally change the manner in which voters interact with their elected officials. The result is that the majority of our theoretical and empirical models of how voters perceive, support, and punish political parties in coalitional systems have little or nothing to do with coalition politics.

We have begun to make progress, however, at least on the issue of prospective voting. For example, work by Bowler et al. (2010), Duch et al. (2010), Kedar (2005), and Meffert and Gschwend (2010) has considered how voters may anticipate the way their votes are aggregated into policy via post-electoral bargaining. Although these works tend to take fanciful views of how informed voters are and of the types of calculations they are capable of performing, the mere fact that we are beginning to bring coalitions back into the study of coalitional voters is encouraging.

**What are we missing?**

What we are missing is the connection between voters and legislators. As mentioned above, we do not know how voters perceive and react to coalition policy-making, whether it be the process of cabinet formation, legislative agenda-setting, legislative review, or premature cabinet failure. As such, we can not say with any certainty how voters perceive and evaluate parties to make their voting decisions. In a manner of speaking, we are starting form square one when it comes to the coalitional voter, with only what we have learned from the American dominated scholarship on voting, qualitative accounts of voting, and, perhaps more importantly, a broad base of knowledge from the social-psychological literature on how people make decisions and cope with high levels of uncertainty in general.
As I alluded above, the voter must come first. When democracy is strong, legislators are responsive to voters. This responsiveness may be born out of desires for office, policy, votes, or merely of a normative ideal of how democracy should work. Until we understand the voter, we can not begin to study how legislators make decisions in representing them. For example, Martin and Vanberg (2008) make a valuable contribution to our understanding of coalition politics by investigating how political parties use a particular institution, legislative speech, to communicate with voters. They argue that coalition compromise obscures the carefully crafted policy positions that cabinet parties have taken and, to mitigate any electoral losses they may sustain as a result, the parties will use parliamentary speeches to reassure voters of their positions on various issues; particularly on issues where there is disagreement within the coalition.

Out of necessity, Martin and Vanberg are agnostic (or intentionally vague) regarding the rich variation among party supports, who are the driving force of their argument. Now imagine for a moment that we fully understood the coalitional voter. We would be able to make so much more of their contribution. If we had already learned (as I will argue in the essays ahead) that more interested voters are more likely to pay attention to the goings on in parliament and therefore more likely to receive the signals that parties are sending, how would that change the content of the signal? How would the content change conditioned on the ideological variation (or lack thereof) across more or less interested voters? Are more interested voters more likely to be strong partisans and therefore less likely to be affected by these messages? Having an answer to even one of these questions would increase the precision with which Martin and Vanberg, and each scholar who follows them, make predictions
regarding legislative behavior. At this point, however, such refined predictions are beyond our reach.

**What we will learn**

The opportunity in the literature is clear and pronounced. In the dissertation I will take a few small steps toward filling the gap in our understanding of democratic accountability in coalitional systems. The dissertation will progress in three essays. First, I investigate how voters alter their perceptions of political parties in response to their participation in coalition cabinets. I argue that voters view coalition participation as broad and wide-ranging policy compromise and react by updating their perceptions of the policy positions of the cabinet participants accordingly; in short, I find that voters tend perceive coalition partners as more similar than parties that are not currently coalesced, all else equal.

In the following essay, I examine the electoral repercussions of this shift in perceptions by proposing a model of voting that gives honest consideration to the relationship between voters and parties and how theses connections are influenced by coalition policy-making. I argue that voters will equate the policy *compromise* they perceive in the cabinet with a failure to rigorously pursue the policies they were promised. Thus, the notion of compromise (or failure to pursue promised policies) that manifests in the perception that coalition parties are growing more similar, is electorally costly.

In the third essay, I move from the electorate to the legislature to investigate if and how these perceptions condition legislative behavior. I argue that coalition parties have substantial motivation to differentiate themselves from their partners in
cabinet when voters perceive them as becoming more similar. I test this argument by examining partisan behavior in legislative review. I find that coalition partners who are perceived as more similar are more likely to amend one another’s legislation. Finally, I conclude with a short summation of what we have learned and briefly entertain a few next steps.
Chapter 2

Perceptions of Partisan Ideologies: The Effect of Coalition Participation*

Introduction

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

The importance of this question is further heightened in systems in which the policy-making process is complicated by formal power sharing among parties. Indeed, the literature on cabinet coalitions consistently highlights the tension cabinet partners

*A version of this chapter, co-authored with Randolph T. Stevenson, is forthcoming in the American Journal of Political Science.
face between maintaining distinct partisan ideological identities and participating in the kinds of policy compromises that are necessary to govern in coalition. For example, Martin and Vanberg tell us that,

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

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

In this chapter, I provide a wealth of new evidence on the sources of voters’ per-

\footnote{A small but important line of related work on European parties shows that voters slowly adjust their perceptions of the policy content of party ideologies (as opposed to the policy positions of parties) in response to broad, long-term changes in the political environment. For example, scholars have explored the process by which voters seem to have incorporated “new politics” issues into their understanding of the ideological profiles of European parties following the emergence of Green parties (Middendorp 1992; Knutsen 1995; Bauer-Kaase 2001). In my view, the long-term nature of this change is consistent with the distinction I make below between the results of research based on party actions versus that based on party promises.}
ceptions of the ideological positions of European parties and, in so doing, suggest a simple resolution to the apparent disagreement in the American and European literatures. Specifically, I point out that almost all of the relevant research on European parties has measured change in the ideological positions of parties using measures of policy promises (usually from election manifestos), while the new research on the American case has used various measures of policy actions (e.g., actual votes in the legislature). In my view, this suggests the simple hypothesis that voters (in both Europe and America) respond more to the observable actions of parties than to party promises. Consequently, if the analysis of the perceptions of European voters were to change its focus from party promises to party actions, I may well find a link between party positions and voter perceptions that is more robust than that found in the previous literature.

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

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

Our theory and empirical analysis are motivated by intersecting literatures in comparative and American politics. In comparative politics, my work responds to the empirical literature on the impact of changes in party ideology on both party support and voter perceptions of the ideological positions of parties. This literature, which has almost universally used campaign promises (election manifestos) to characterize party positions, has produced only weak evidence that changes in party manifesto positions affect aggregate electoral support. In addition, these effects, when found at all, are delayed (Adams and Somer-Topcu 2009a; Somer-Topcu 2009) and conditional (Tavits 2007). In fact, the most recent, relatively comprehensive, examination of the data (Adams et al. 2011) finds no systematic relationship between changes in party manifesto positions and either voter perceptions of those positions or electoral support. Interestingly, however, that study does find a strong connection between changes in the aggregate perceptions of the left-right positions of parties and aggregate support — a finding that mirrors the long-established importance of the perceived policy positions of parties on vote choice at the individual level (Canes-Wrone et al. 2002; Erikson 1971; Erikson and Wright 1980; Whitten and Palmer 1996; Wright 1978; Wright and Berkman 1986).

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

Our response to this work in comparative politics is to suggest a move away from party promises as the source of voter perceptions of party ideology and toward observable party behavior.\(^3\) This suggestion builds from both an older and a more recent stream of work in American politics. The older work has always doubted the power of party promises to move voters. V. O. Key is perhaps the most notable proponent of this view, famously arguing that,

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

Others have echoed Key’s idea. Fiorina’s (1981) theory of retrospective voting explicitly adopts this position, as does most of the large literature on retrospective voting in general (although, perhaps, more implicitly).\(^4\) Of course, if voters heavily discount party promises and instead focus on partisan actions in formulating their impressions

\(^3\)Again, it is important to emphasize that the party behaviors that are even potentially “observable” are different across most coalitional and non-coalitional systems. Thus, I am not advocating the adoption of the specific measures of party behavior used in the American politics literature (e.g., analyses of roll call voting) but rather a more general shift in conceptual focus away from party promises towards party behavior.

\(^4\)Given the well-known irrelevance of American party platforms as a guide to the ideological positions of presidential candidates or congressional delegations, it is no wonder that scholars of American politics have not been lured into using party manifestos as a convenient measure of party positions. One important exception is Erikson, MacKuen and Stimson, who use party platforms but emphasize that election platforms only reflect parties’ actual positions when smoothed over time since

> “past promises reflect past realities because parties do more or less follow up on their promises.” (2002, 258)

In a later section of the essay, I explain how this notion of delay is consistent with my point that voters focus on a party’s actions rather than promises, for exactly the reasons expressed in the quote: if parties eventually enact policy that they previously promised and perceptions reflect this action in some way, then previous promises will be predictive of current perceptions.
of parties’ policy positions, this may well be reflected in exactly the kind of weak and empirically unstable relationship between promises and perceptions that comparativists have so far found. Thus, if I can focus my empirical analysis on actions rather than promises, I may yet recover a robust relationship between party ideology, voter perceptions, and voting behavior and finally establish the widespread importance of this straightforward mechanism of democratic accountability.

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

On the empirical front, Hetherington (2001) has shown that public perceptions of the distance between the ideological positions of the Republican and Democratic parties increases as the votes of the members of the House delegations becomes more polarized (as measured by mean NOMINATE scores). Further, Woon and Pope (2008) show that American voters seem to understand not just the positions of parties,
but also the extent to which the parties’ are unified or divided — as indicated by their legislative records. Likewise, they show that the electoral support of parties responds systematically to these features of the legislative record of parties. Overall, Woon and Pope conclude,

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

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

“voters absorb the aggregate roll call record of the parties and use this to make relatively informed judgments about the parties’ issue positions.”

(2009, 16)

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

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

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

**Theory**

Since my concern in this chapter is with voters’ perceptions of the policy positions of political parties, it is natural to begin with the assumption that change in these views responds, in some way, to the actual policy-making behavior of parties. As I suggested above, however, the cost of monitoring the on-going policy-making process is likely to be prohibitive for most voters. Thus, in updating their views of the policy positions of the political parties, many voters are likely to rely on a set heuristics that allow them to make sensible judgments about how the positions of political parties are changing, without the need to pay close attention to news about the policy-making activities of parties. I believe cabinet membership is one of the most important of these heuristics.

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

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

I could state my main theoretical claim as:

“To the extent that voters care about the ideological or policy positions of individual [parties] but face uncertainty about the specific views and stances of the individual [parties], then [cabinet membership] provides a natural shortcut because it conveys [ideological] information about the set
of [parties] that [are in the cabinet].”

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

Cabinet membership: A widely available and cheap piece of information

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

Perhaps the best evidence on the question that currently exists comes from the 1998 Dutch Parliamentary Election Study, which asked voters to classify each of the thirteen parties running in the election as members of either the government or opposition. The average success rate across all respondents and parties was over 90%. Even more remarkably, this knowledge, while not unresponsive to levels of political interest (as one would suspect), was very high even for individuals at the lowest levels of political interest. Table 2.1 summarizes these data, showing that an impressive 75% of individuals at the lowest level of political interest identified the cabinet status of at least 11 of the 13 parties correctly.

Overall, there is an increasing body of evidence that most voters in parliamentary democracies know the membership of the cabinet — even those who pay relatively little attention to politics. This gives relatively direct evidence that this kind of information is so pervasive that it is must be almost costless to obtain. Thus, it should be an ideal heuristic to substitute for more costly ideological information about parties — as long as it actually contains some relevant ideological information.
Table 2.1: Ability of Dutch voters to classify parties as either government or opposition according to their level of political interest. Source: 1998 Dutch Parliamentary Election Survey.

<table>
<thead>
<tr>
<th>Political Interest Score</th>
<th>Number of Parties Correctly Classified</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Low = 0</td>
</tr>
<tr>
<td>0 - 6 Correct</td>
<td>15.7%</td>
</tr>
<tr>
<td>7 - 10 Correct</td>
<td>8.8%</td>
</tr>
<tr>
<td>11 - 13 Correct</td>
<td>75.4%</td>
</tr>
<tr>
<td>Total</td>
<td>100%</td>
</tr>
</tbody>
</table>

The ideological information in cabinet membership

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

First, various lines of research in the coalition formation literature point to the idea that parties are more likely to participate in coalitions together once they have
actually moved closer together. For example, Martin and Stevenson (2001) as well as many other empirical models of government formation that have adopted their empirical approach (e.g., Martin and Stevenson 2010; Martin and Vanberg 2003; Proksch and Slapin 2006; Warwick 1996) show that the cardinal ideological distance between parties — not merely the rank order of ideological positions as in previous work — is a strong predictor of cabinet partnerships. Further, this rings true with qualitative accounts of coalition decisions within parties, like the German FDP, that periodically switch internal leadership between ideologically disparate coalitions.\(^5\) In these cases, the outcomes of factional struggles between competing ideological camps within the party often precede the decision of whether to coalesce with the left or right. In such cases the choice of coalition partner is a signal of “true” ideological change within the party.\(^6\)

A second source of evidence that leads us to posit that cabinet membership is a generally useful heuristic for inferring the direction of change in the policy positions of cabinet parties are studies that have asked whether parties that govern in formal coalitions together are more open to policy compromise than parties that are otherwise

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\(^5\)For example, Conradt’s (1993) textbook account of coalitional change in Germany emphasizes that policy change within the FDP was an important driver of the party’s withdrawal from coalitions with the CDU (1957 and 1966), joining the SDP in 1969, and switching back to the CDU in 1982.

\(^6\)Another mechanism that that can create ideological congruence between parties that form cabinets together begins with the observation that the senior members of most parties are, at least to some degree, ideologically heterogeneous. Thus, in negotiating to join cabinets, party leaders have a choice about which senior politicians to appoint to different cabinet positions — a choice that can be consequential both for whether a proposed cabinet can form and what policies will ultimately be produced. Indeed, when this idea is combined with the general result that parties whose ministers are more ideologically compatible are more likely to form cabinets together (Laver and Shepsle 1990), it implies that, even among the set of potential cabinets with the same partisan composition (i.e., all the different arrangements of specific ministerial appointments), the ones whose actual ministers are more ideologically compatible will be more likely to form. Some evidence for this idea comes from Bräuninger and Debus (2005), who estimate the ideal points of party factional leaders in Germany (for 87-90 and 98-02) in order to show that the allocation of ministerial portfolios was compatible with this story.
similar but not in formal coalitions (e.g., Goodin 1996, Ganghof and Bräuninger 2006). Such studies ask: what are the differences in legislative behavior one can expect from two parties that are the same on all relevant dimensions except that in one case they are in a formal governing coalition and in another case not? The expectation of most such studies is that this difference in coalitional status will be consequential because,

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

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

Third, there is a growing line of research that suggests that cabinet parties strive to enforce and maintain policy compromises over the course of the government — successfully overcoming their partners’ incentives to “highjack” policy in those domains in which they have informational and resource advantages (e.g., those policy domains in which they control ministries). Martin and Vanberg (2004, 2005) and Thies (2001)
have found that coalition partners utilize legislative institutions, such as committees or junior ministers, to prevent their partners from “drifting” from policy bargains. This is important to my argument about the ideological content of the cabinet membership heuristic because it suggests that parties (contrary to a view of cabinets as a collection of autonomous ministers) will not be able to promise compromise to get into cabinet and then successfully avoid policy compromise once in office.\(^7\) Thus, a cabinet membership heuristic that assumes policy moderation and accommodation among the partners is consistent with the most recent evidence about what parties actually do in cabinet.

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

- **Hypothesis 1**: Coalition partners will be perceived as more ideologically similar than non-coalition partners, all else equal.\(^8\)

\(^7\)In Laver and Shepsle’s (1996) influential work, the stability of the cabinet itself is predicated on the ability of ministers to dictate policy on dimensions they control. If they also tend to control those ministries most salient to their supporters, then it is entirely possible that cabinet membership would reinforce ideological distinctions between partners rather than blurring the ideological lines between them. The work cited above, however, is incompatible with this view.

\(^8\)The relevant comparison in this hypothesis (which I try to capture directly in the empirical research design described below) is between any pair of parties serving in cabinet together and any other pair of parties in which one or both are not in cabinet. I expect voters to perceive a pair of cabinet parties as more ideologically similar to each other than a pair of parties in which one or both
Heterogeneity in the use of the cabinet membership heuristic

While my main concern in this chapter is evaluating Hypothesis 1, I would be remiss if I failed to recognize that not all voters need to rely equally on heuristics. While I think that almost all voters will find cabinet membership a useful guide to the direction of partisan ideological change among parties, there are two kinds of voters I do not expect to rely on this heuristic as much as the typical voter.

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

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

“...parties go into coalition governments for lots of reasons, and lots of things happen when parties are in coalition governments.”

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

It is important to point out, however, that the argument above (that better informed voters rely less on a cabinet membership heuristic) does not imply the direction in which the perceptions of the politically informed should deviate from those of the less informed – it only implies that a cabinet membership heuristic will be a generally less reliable guide to the perceptions of these voters on average. There is another argument, however, that gives a more precise prediction — suggesting that the well-informed will systematically perceive cabinet parties to be more ideologically disparate than will less-informed voters who rely solely on a cabinet membership heuristic.

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

This implication comes from Martin and Vanberg’s extensive new data set on legislative debates in Germany and the Netherlands. They use this data to show that (as elections approach) coalition partners use debates in the legislature to try to differentiate themselves ideologically from their partners. Thus, to the extent that voters receive and pay attention to such messages, they will tend to work against a simple heuristic that leads voters to perceive cabinet partners as more ideologically similar. The key point, however, is that not all voters are likely to receive and pay attention to these kinds of political messages. Indeed, it is hard to imagine the average voter ever paying attention to reports of legislative debates — or even that the average political story in the media would give them much attention. Instead, it is much more likely that these kinds of messages are found in more specialized accounts of party politics that are consumed by the politically interested — including interested party partisans, who are likely the intended audience. Even if, as Martin and Vanberg argue, the content of legislative debates reflect a more general effort by the party to communicate a particular message to voters via other communication channels (its website, generating news stories, public speeches), it is clear that those most likely to receive these messages will be those most interested and attentive to politics (Zaller
Thus, if all voters rely to some extent on a cabinet membership heuristic, it follows that it is the more attentive voters who will be most likely to receive messages from cabinet parties that attempt to distinguish themselves from their partners — despite the policy compromises they may have made.9

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

Data and analysis

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

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]?”

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9Martin and Vanberg give a particularly helpful example of how this works in practice. In their example the German coalition partners (the SPD and the CDU/CSU) had compromised on unemployment and welfare benefit reform. Each party however used legislative debates and speeches to try to justify to the (a highly informed) party faithful why the compromise did not actually represent an abandoning of party principals — the Social Democrats claiming that the bill did not represent a departure from their commitment to the working class and the Christian Democrats espousing the benefits of self reliance and the need for state efficiency. Such subtle messages are simply not going to be received by most voters.
Our theory suggests that voters’ perceptions of the similarity of any two parties’ policy positions will depend on whether those two parties are in cabinet together. The most direct measure to use as my dependent variable is simply the absolute value of each respondent’s perceived distance between each pair of parties. Thus, I transform my survey data from the respondent level (one line per respondent) to the respondent-dyad level (one line per party-dyad per respondent) and ask what factors cause a voter to perceive the policy positions of any two parties as more or less distant? This design makes it easy to examine my main hypothesis directly by comparing (in a properly controlled statistical model) dyads in which both parties are in the cabinet to those in which either one or both the parties in the dyad are not in cabinet.\textsuperscript{10}

**Structure of the data**

In developing appropriate empirical models with which to test my hypotheses, it is important that we understand the relatively complex, hierarchical structure of the data. Specifically, I have data on respondents, answering questions about parties, which are converted into party dyads, in as many as 54 election surveys in 18 countries (where the sets of parties either completely or partially differ across surveys). The

\textsuperscript{10}Since the same dyads appear in different surveys (at different times) these comparisons include (as with any time-series cross-sectional data) comparisons across different dyads at the same time, the same dyads at different times, and different dyads at different times. Unfortunately, there are simply not enough dyads in the data that span different surveys (and that change their cabinet partnership status) to pull out only these cases and do a meaningful analysis of the “within dyad” variation in the data. That said, in the next chapter, I use panel data tracking Dutch respondents over time to show that respondents do actually change their perceptions of the cabinet parties over the life of the cabinet — systematically identifying cabinet parties as more ideologically. This result on the individual-level dynamics of the process gives us some assurance that the cross-sectional (or “between dyad”) comparisons, which constitute the main source of variation in my data, reflect (at least in part) an underlying dynamic process of perceptual change that occurs as parties serve together.
dependent variable in all my analyses records how far apart each respondent in a particular survey perceives the positions of the parties in each party dyad included in the survey to be. This means that each respondent enters the data $m_j$ times, where $m_j$ is the number of dyads in survey $j$. Likewise, each dyad within a survey, or “survey-dyad” enters the data $n_j$ times, where $n_j$ is the number of respondents to survey $j$. Thus, in the language of hierarchical data structures, the dyads and respondents are “crossed.” In addition, dyads are crossed with surveys. That is, each party dyad may appear in multiple different surveys (for example the FDP-SDP dyad appears in all the German surveys) and each survey has multiple dyads. Finally, surveys are nested in countries, since each survey applies to one, and only one, country.

This data structure leads to six possible sources of both measured (fixed) and unmeasured (random) effects on the dependent variable.\footnote{One 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.}

1. \textit{Country}: effects that vary over countries but that are constant over surveys, dyads, and respondents within a country

2. \textit{Survey}: effects that vary over surveys but that are constant over dyads and respondents within surveys

3. \textit{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. \textit{Respondent}: effects that vary over respondents but that are constant over all
dyads evaluated by the same respondent

5. *Survey-Dyad:* effects that are constant over respondents evaluating a given dyad, but that vary from survey to survey for the same dyad

6. *Dyad-Respondent:* effects vary from dyad to dyad for the same respondent (when this is unmeasured it is the “residual” error)

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

Likewise, my hypotheses identify two factors (that I will measure) at the survey-dyad level that I think will impact the way all respondents in a particular survey will place the parties in a given dyad (cabinet membership and the “true” distance between the parties in the dyad) and we can imagine that there are other factors at this level that may be unmeasured.\footnote{For example, perhaps a party dyad at the time of one of my surveys has entered into an electoral}
at the dyad level that systematically impact how similarly the voters perceive the positions of the parties in the dyad (i.e., that apply to a pair of parties in all surveys in which the dyad appears). These would tend to be more enduring characteristics of dyads (for example, if two parties are in the same ideological family).

Finally, there may be measured and unmeasured characteristics of individuals that would cause him or her to systematically perceive all dyads as more similar or more different. Indeed, with respect to measured factors, my hypotheses suggest that political information is one of these factors (at least in interaction with coalition participation). Other measured characteristics of respondents that I have examined are gender, income, and others. That said, unlike models in which the question is whether perceptions of policy positions are accurate (Stevenson and Vonnahme 2010), thinking of individual characteristics that should have a systematic impact on the perceived similarity of all dyads in a survey is a challenge. For example, while previous literature on political knowledge strongly suggests that gender is an important control for a study of the accuracy of perceptions of party positions (with females generally having less accurate perceptions), we have no clear intuition for how gender would impact the perceived similarity of party positions. Given this, while I have included various measures of individual level characteristics as controls in my models for the sake of robustness (available from the authors), I have not made the inclusion of a long list of respondent characteristics in the model a priority. Likewise, while I think there may well be some unmeasured factors associated with individuals that systematically impact how far apart they perceive the positions of all pairs of alliance with another. If this is not measured, it will create correlations across respondents who will systematically perceive this survey-dyad as closer together.
parties to be, I am not overly concerned that my results will be sensitive to how I deal with these particular unobserved characteristics statistically.\textsuperscript{13}

**Measured variables**

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

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

\textsuperscript{13}One exception to this expectation concerns individuals who systematically answer the survey questions about party positions by giving all (or many) parties the same position (usually at the center of the scale). To the extent that voters with given characteristics (measured or unmeasured) are more likely to do this, these characteristics will be systematically related to more similar ratings for dyads. I have investigated this possibility as well as the impact of such responders on my results. Only about 6\% of my responders appear to answer in this way. One option is simply to drop them, and my results are all robust to this approach. However, it is possible that the response is genuine — that is, it expresses the respondent’s belief (perhaps in an over dramatic fashion) that all the parties are the same. Given this, and because it is inconsequential for the results reported below, I leave these individuals in the data).
In addition to manifesto distance, I also include a measure intended to capture the longer-term “true” policy positions of the parties. The idea here is that parties develop, over time, a particular policy profile that should have an important impact on voter perceptions. My measure is a ten year rolling average of manifesto positions. That is, I take the average of the left-right position from each manifesto published over the ten year period leading up to my survey. I then take the absolute distance of these averaged positions as above. This measure has the significant advantage of being available for nearly every party in my sample. However, it is reasonable to ask, given the previous literature which has shown the disconnect between changes in manifesto positions and changes in perceptions, whether it is sensible to use manifestos to measure the parties “true” positions? I think that it is reasonable to do so because the critique in the previous literature is that manifestos do not predict change in policy perceptions about the same parties compared over time — not that they are not broadly predictive of the relative absolute positions of different parties compared at the same time. There is no question, for example, that manifestos generally order the parties correctly, though there are certainly fluctuations in individual elections that can create odd, one-off orderings that no doubt contribute to the negative finding for perceptions of change reported above (for example, the rank order violation between the British Labour Party and the Liberals in the early 60’s, which is discussed by

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14I select the most proximate, rather than the most recent, survey because I am trying to use manifestos to capture a snapshot of the evolving policy message of parties at a given time — a concept better captured by the most proximate manifesto than the last published one (usually at the time of the previous election), which can be several years old by the time my surveys are in the field. Finally, rescaling of this control variable is not essential, but it does facilitate substantive interpretation of the results.

15Benoit and Laver (2006) and Lowe et al. (2011) demonstrate that their manifesto derived measurements correlate quite well to measurements of policy position taken from expert surveys.
A third control, which I call “familiarity” is measured at the level of the survey-dyad (i.e., it will vary for each party dyad in each survey) and captures the (discounted) history of joint cabinet participation for the dyad at the time of the survey. The idea is that for all party dyads, a recent history of joint service in cabinet may (by my own argument) have resulted in voters previously perceiving the parties in the dyad as closer together — an effect that could, plausibly, linger. To measure “familiarity” I calculate, at the time of the survey, the proportion of all months (since 1945) that the two parties in the dyad served in the same cabinet. Then, I discount periods of service that occurred previous to the time of the survey, with more distant periods of service discounted more heavily. The measure was developed by Martin and Stevenson (2010) and is described in detail in that article.

A quick look at the distribution of this measure (figure given in the appendix), however, suggests that simply including it, as is, in the additive specification of controls is a mistake. Specifically, a histogram of the variable reveals that about 85% of the cases of this variable take on a zero value (i.e, the parties in the dyad have never been in cabinet together — or at least not very recently). This suggests that there might be a discontinuity between the effects of a zero value for this variable and anything positive (i.e., the parties in the dyad have some recent history of joint cabinet service). I can allow for this kind of discontinuity by coding a dummy variable that takes a value of zero when familiarity equals zero and one when it is positive. Including this dummy variable in the equation along with familiarity means that the total effect of familiarity for any case in which familiarity is greater than zero is the value of familiarity multiplied by the linear coefficient, plus the coefficient on the
dummy variable — thus, allowing for a jump at zero followed by a linear function of familiarity after that.\footnote{Note that, by construction, simply including the dummy variable accomplishes what I want, without the need for an interaction term.} As it turns out (see the results below), exactly this kind of discontinuity is evident in the data. Finally, I allow for the impact of familiarity to be different for dyads in which both parties are currently in cabinet, both in opposition, or where one party is in and one is out.

Next, I include two controls at the respondent level. While above I explained that my focus on the respondent’s perceived distances between parties leads us to discount the importance of individual characteristics that may systematically influence the manner in which voters perceive individual parties (e.g., gender, income, etc.), there is one exception to this intuition: the voter’s placement of themselves on the left-right scale. Specifically, there are two ways in which I think this could impact the respondent’s placements of the parties.\footnote{Notice that in all cases, the respondent places themselves on the scale first, so this is formally a “pre-treatment” variable and so properly included as a control.} First, Stevenson and Vonhamme (2010) show that when respondents locate themselves between the parties in a given dyad, they are much more likely to correctly identify the left-right ordering of the parties. In that case, the fact that the voter can “look” from their own position and “see” one of the parties to their left and one to the right helps them to order the parties more accurately. Similarly, I suspect that when a voter places herself between the two parties in a dyad, she will have a natural tendency to place the parties farther apart than she would if both parties were to her left or to her right. Thus, I include a control variable which is coded one when the respondent is located between the parties in the relevant dyad and zero otherwise.
The second control variable that I derive from the respondent’s left-right self-placement is her ideological extremity. The idea here is that voters who place themselves on the extreme left or the extreme right will tend to see most parties as very different from themselves. Likewise, these respondents are the ones most likely to conflate the parties’ ideological positions on the left-right dimension with alternative dimensions (e.g., radical or anti-system versus not). While, I do not have a strong intuition about the expected direction of the overall effect of extremity on perceived distance (i.e., one can think of plausible arguments for both a systematic positive and negative effect), I do worry that such respondent’s are sufficiently atypical that I need to be sure this is not consequential for the main point of this chapter. Consequently, I include this control.

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

Finally, I confront issues with missing data in my measured variables by examining a number of different specifications.\textsuperscript{18} Specifically, when I include the set of familiarity

\textsuperscript{18}The best approach for handling missing data is to use multiple imputations. The problem in my
variables, I lose a significant number of observations. Thus, I provide results with and without these measures. The bigger missing data problem, however, comes from my measure of political interest. While the theory clearly calls for a measure of how attentive an individual is to politics, including this measure reduces the sample from 54 to 38 surveys (and from 18 to 15 countries). In order to combat this problem, I provide results with an alternative, but certainly more noisy, measure of attentiveness to politics that is asked in all my surveys: level of education. Given the different samples created by these different specifications, I provide descriptive statistics for all variables, for each of the four samples, in the appendix.

**Unmeasured variables**

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

Such models have been used extensively in many disciplines. However, estimating case, however, is the multi-level structure of my data. It is not at all clear how one should impute data collected from multiple surveys in different contexts, especially when the focus of the analysis is the impact of a contextual variable (coalition membership) on individual behavior. Indeed, this is an open area of research in statistics. At the very least, I know that all the variables at both the individual level and the other higher levels would have to be included in the imputation model, but since some of the higher level variables are missing as well, the existing literature provides no solutions. Efforts, however, to address these issues are underway (e.g., Gelman, King, and Liu 1999; Yucel 2008).
such a six-level “random-effects” model (with two crossed levels) on my several hundred thousand observations is not practically possible. Thus, I have to compromise. In particular, I estimate a series of different models in which I attempt to deal with the correlation of the error term in the regression — caused by unmeasured factors at each level — in different ways. Specifically, I first estimate a series of regressions in which I use robust standard errors clustered on the groups of observations defined by the various levels of the hierarchy. This is not ideal. Since one cannot cluster on all the levels at once, in accounting for correlation between observations stemming from one level, one ignores correlation stemming from another. As Stevenson and Vonnahme (2010) discuss, however, this exercise is useful in that it can help pinpoint those levels of variation in the hierarchy that are most important to the estimates. That is, if one clusters standard errors on a group of observations defined by a particular level of the hierarchy and it changes the standard errors of the estimates a great deal (relative to a baseline without clustered standard errors) then one can conclude that this is a level for which one should be concerned about correlation. Obviously, if this is true for all levels, it does not help much; but, if it is only true for one level then one is more confident in concentrating on modeling that level using the more computationally demanding (and more correct) techniques of multi-level random effects modeling.

Results

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

Table 2.2: Error clustering experiment. Standard errors clustered, respectively, on: none, survey(s), dyad(d), survey-dyad(|sd), respondent(r). Dependent variable is perceived dyad distance.

<table>
<thead>
<tr>
<th>Variable Name</th>
<th>Coefficients (Standard Errors)</th>
<th>Coefficients (Standard Errors)</th>
<th>Coefficients (Standard Errors)</th>
<th>Coefficients (Standard Errors)</th>
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</thead>
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<tr>
<td>10 Year Average CMP Distance</td>
<td>331 (.006, 0.066, 0.071, 0.007)</td>
<td>407 (.006, 0.066, 0.064, 0.007)</td>
<td>448 (.004, 0.056, 0.063, 0.006)</td>
<td>549 (.004, 0.088, 0.075, 0.059, 0.006)</td>
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<tr>
<td>Most Proximate CMP Distance</td>
<td>336</td>
<td>295</td>
<td>273</td>
<td>200</td>
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<td>Coalition Partners</td>
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<td>-731</td>
<td>-731</td>
<td>-583</td>
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<tr>
<td>Opposition Partners</td>
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<td>0.055</td>
<td>0.042</td>
<td>0.041</td>
</tr>
<tr>
<td>Political Interest * Opposition Partners</td>
<td>0.001</td>
<td>0.018</td>
<td>0.008</td>
<td>0.009</td>
</tr>
<tr>
<td>Education</td>
<td>-0.026</td>
<td>-0.030</td>
<td>-0.026</td>
<td>0.006</td>
</tr>
<tr>
<td>Education * Coalition Partners</td>
<td>0.017</td>
<td>0.015</td>
<td>0.015</td>
<td>0.007</td>
</tr>
<tr>
<td>Education * Opposition Partners</td>
<td>-0.018</td>
<td>0.025</td>
<td>0.020</td>
<td>0.009</td>
</tr>
<tr>
<td>Between Partners</td>
<td>2.766</td>
<td>2.812</td>
<td>2.470</td>
<td>2.705</td>
</tr>
<tr>
<td>Extremism</td>
<td>362</td>
<td>368</td>
<td>347</td>
<td>358</td>
</tr>
<tr>
<td>Familiarity</td>
<td>-547</td>
<td>-547</td>
<td>-547</td>
<td>-547</td>
</tr>
<tr>
<td>Familiarity &gt; 0</td>
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<td>-0.179</td>
<td>-0.179</td>
<td>-0.179</td>
</tr>
<tr>
<td>Familiarity*Coalition Partners</td>
<td>2.122</td>
<td>2.122</td>
<td>2.122</td>
<td>2.122</td>
</tr>
<tr>
<td>Familiarity*Opposition Partners</td>
<td>-0.217</td>
<td>-0.217</td>
<td>-0.217</td>
<td>-0.217</td>
</tr>
<tr>
<td>Telephone Survey</td>
<td>-3.65</td>
<td>-3.65</td>
<td>-3.65</td>
<td>-3.65</td>
</tr>
<tr>
<td>Self Administered Survey</td>
<td>1.98</td>
<td>1.98</td>
<td>1.98</td>
<td>1.98</td>
</tr>
<tr>
<td>Intercept</td>
<td>1.298</td>
<td>1.298</td>
<td>1.298</td>
<td>1.298</td>
</tr>
</tbody>
</table>

Second, there is a very strong pattern in the standard errors that is essentially identical across specifications. That is, that effects at the survey level (recall that
surveys are nested within countries) are most important in generating correlated observations. This is to be expected as this level will capture not only idiosyncrasies in survey administration, but, more importantly, characteristics unique to the party systems of the different countries included in my sample. Further, effects at the dyad/survey-dyad level are quite salient as well, exactly the levels that I suggested should theoretically be important to explaining variation in perceptions of similarity between parties in a dyad (indeed, the survey-dyad level is the one at which most of my theoretical variables are measured). Importantly, respondent level correlation seems to have relatively minimal impacts on standard errors, which contrasts with the Stevenson and Vonnahme (2010) analysis using the same data but in which the dependent variable was accuracy of perceived party positions (a variable I expect to be greatly impacted by individual respondent characteristics) instead of the similarity of perceived positions.

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

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

Of course, I hesitate to go too far in interpreting these effects, given that the use of robust standard errors in these models is not an ideal way to deal with the correlation caused by unmeasured variables at various levels. Thus, I prefer to present results from a more properly specified hierarchical model. That said, I have already indicated that it is not practically possible to estimate the full six-level hierarchical model. However, the pattern of results for the standard errors in Table 2, as well as the theoretical arguments I made earlier, give us a clear indication that the troublesome levels (i.e., those in which there are probably unmeasured factors creating correlations between rows in my data) are the levels which involve surveys and dyads rather than respondents. Thus, the model reported below is an error components (or “random effects” hierarchical) model that allows for two kinds of random intercepts: one for each survey and one for each dyad within a survey. This is the most general model that I could practically estimate and while it is not perfect, it does simultaneously
account for the two most problematic levels of grouping in the data.\textsuperscript{19} The results are given in Table 2.3 and are quite consistent with those in Table 2 for my covariates of interest. Coalition participation causes voters to perceive parties as more similar and this relationship is not only robust statistically, but the substantive magnitude is also large. For example, for the least informed voter (political interest level of 1), the estimated effect of coalition partnership on perceived dyad similarity from Table 3 is \(-.588\). For the average coalition partner dyad (ten year rolling average CMP distance of \(.69\) and proximate CMP of \(.66\)), this effect is larger in absolute magnitude than the effects of both CMP derived distances \textit{combined} \((.537)\). Again, this is substantively significant and statistically robust support for Hypothesis 1.

Examining Table 2.3 also reveals the same systematic relationship between political interest and perceived dyad similarity that I saw in Table 2. Specifically, higher levels of political interest have a mitigating effect on the impact of coalition partnership on perceived similarity. This supports my hypothesis that highly interested voters will be less reliant on the heuristic of cabinet membership and thus less likely to systematically place coalition partners closer together in the policy space. It is important, however, to understand that the effect, while not negligible, is nowhere near large enough to eliminate the negative impact of coalition membership on perceptions of dyad similarity, even for the most informed voters. A simple calculation using the coefficients in Table 2.3 shows that the impact of coalition membership for

\textsuperscript{19}There was no practical way to estimate a crossed model between dyads and surveys and so I treat dyads as nested within surveys. Substantively, this means I treat a dyad like the German FDP-SDP in one survey as if it were different than the FDP-SDP dyad in another. This is exactly the same thing that researchers using time-series cross-sectional data do when they specify a random intercept model in which years are nested within countries (as is the common practice) rather than estimating a crossed model.
Table 2.3: Hierarchical regression analysis

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient</th>
<th>(Std. Err.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>10 Year Average CMP Distance</td>
<td>.447</td>
<td>(.066)</td>
</tr>
<tr>
<td>Most Proximate CMP Distance</td>
<td>.315</td>
<td>(.063)</td>
</tr>
<tr>
<td>Coalition Partners</td>
<td>-.588</td>
<td>(.138)</td>
</tr>
<tr>
<td>Opposition Partners</td>
<td>-.029</td>
<td>(.082)</td>
</tr>
<tr>
<td>Political Interest</td>
<td>-.006</td>
<td>(.005)</td>
</tr>
<tr>
<td>Political Interest * Coalition Partners</td>
<td>.036</td>
<td>(.012)</td>
</tr>
<tr>
<td>Political Interest * Opposition Partners</td>
<td>.003</td>
<td>(.007)</td>
</tr>
<tr>
<td>Between Partners</td>
<td>2.228</td>
<td>(.007)</td>
</tr>
<tr>
<td>Extremism</td>
<td>.328</td>
<td>(.002)</td>
</tr>
<tr>
<td>Familiarity</td>
<td>-.236</td>
<td>(.346)</td>
</tr>
<tr>
<td>Familiarity &gt; 0</td>
<td>-.041</td>
<td>(.089)</td>
</tr>
<tr>
<td>Familiarity * Coalition Partners</td>
<td>.415</td>
<td>(.498)</td>
</tr>
<tr>
<td>Familiarity * Opposition Partners</td>
<td>.090</td>
<td>(.436)</td>
</tr>
<tr>
<td>Telephone Survey</td>
<td>-.152</td>
<td>(.144)</td>
</tr>
<tr>
<td>Self Administered Survey</td>
<td>.079</td>
<td>(.263)</td>
</tr>
<tr>
<td>Intercept</td>
<td>1.361</td>
<td>(.130)</td>
</tr>
</tbody>
</table>

Random Effect: Survey (38) .335 (.063)

Random Effect: Dyad (525) .790 (.026)

Random Effect: Residual 1.749 (.002)

N 305909
Log Likelihood -78461.860

the most politically interested respondents in my sample is a decrease in perceived distance of -.444 (compared to the -.588 reported for the least interested above).\textsuperscript{20}

Conclusion

Previous empirical studies of voters in coalitional systems have failed to find a strong link between changes in parties’ policy positions and changes in voters’ perceptions of these positions. At the same time, such links have been relatively easy to identify

\textsuperscript{20}This remains highly significant.
in the American case. This finding, with its implicit message that there must be something different about coalitional systems that inhibits the ability of voters to track changes in the policy positions of parties, is in keeping with a long line for research and discussion that questions the ability of voters in coalitional systems to navigate their complexities. Downs (1957), for example, famously suggested that voters in coalitional systems will, because of the difficulty in forming expectations about the likely outcomes of coalition formation, forgo prospective policy voting. Similarly, in a very influential line of work, Lewis-Beck (1988) and Powell and Whitten (1993) suggested that voters in coalitional systems will also retreat from retrospective performance voting (with its requirement that one sensibly attribute responsibility over coalition parties) in favor of less informationally demanding voting strategies.

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

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

This conclusion resonates with a growing body of work on voter responses to coalition government that asserts, in contrast to the decades-old common wisdom, that voters can (and do) use simple heuristics to negotiate the complexities of coalition cabinets. This includes using simple rules of thumb to form sensible expectations about which coalitions form (Armstrong and Duch 2010; Duch et al. 2010; Fortunato, Stevenson, and Spiegelman 2011), to decide how to attribute policy-making responsibility over cabinet parties (Duch et al. 2011) and, given the above results, how to form useful perceptions of the ideological movements of cabinet parties. All of this work on voter perceptions, expectations, and attributions in coalitional systems supports an even larger body of work (though almost as recent) that demonstrates voters use these
expectations, perceptions, and attributions in their vote choices. Duch and Stevenson (2008) for example have demonstrated that voters’ expectations about which cabinets form, as well as their attributions of responsibility for policy-making, systematically impact retrospective performance voting. Likewise, Kedar (2005), Blais et al. (2006), Bargsted and Kedar (2009), Bowler et al. (2010), Meffert and Gschwend (2010), and Carman and Johns (2010) all demonstrate voters use their expectations about which coalitions will form to cast policy-oriented “coalition-directed” votes — votes intended to help a preferred coalition form.

Overall, then, recent work on voters in coalitional systems is beginning to overturn the traditional view that the complexity of these systems encourage voters to forgo either prospective policy voting (e.g., Downs 1957) or retrospective performance voting (e.g., Powell and Whitten 1993) in favor of less informationally costly voting strategies. my study identifies another heuristic that voters in coalitional systems can use to efficiently inform themselves about the fundamental aspects of political competition in their systems and so underscores the general message of this emerging, revisionist, view of the abilities and knowledge of the coalitional voter.
Chapter 3

Voter Perceptions and Incumbent Vote Loss*

“Few facts are so robust — and so little discussed — in political economy as the one that it costs votes to rule.”

—Nannestad and Paldam (2002, 18)

Motivation

One of the most pervasive empirical regularities in advanced industrial democracies is that incumbent governments tend to lose votes. Further, this loss remains constant even when we control for the government’s performance (Paldam and Skott 1995; Powell and Whitten 1993; Stevenson 2002). As Powell pointed out in his study of over 150 elections in twenty contemporary democracies,

“[Across] a quarter of a century, the party or parties in office lost votes much more often than not...In only a quarter of the elections did the incumbents actually gain votes.” (2000, 47-8)

Paldam and Nannestad (2002) survey 15 parliamentary democracies where coalition governance is the norm (“coalitional” systems) and estimate that this electoral “cost of ruling” averages over 2.5% of the government’s vote share across their sample and

*Research support provided by the Collaborative Research Center SFB 884, Universität Mannheim.
that this average vote loss is substantially more stable (i.e., less variance around the
mean) in coalitional systems — the most common form of government in the world
— than in majoritarian democracies. This vote loss is not insignificant. In 2010, for
example, there were six parliamentary elections in the coalitional systems of Europe.
In three of these six elections an opposition party won a plurality of votes over an
incumbent party runner up by a margin of less than 2%. Thus, in 2010 alone, as
many as three European governments were determined by a vote difference less than
the mean cost of ruling.

It is critical to understand this systematic vote loss. If governing coalitions expect
their baseline electoral performance to be negative from one election to the next, how
does that effect their motivation to govern in accordance with the preferences of the
electorate? However, despite widespread acceptance of this cost, we still lack a strong
understanding of it. This is because our existing explanations are a) overwhelming
aimed at explaining behavior in the aggregate (e.g., Mueller 1970; Paldam 1986) and
thus may fail to hold up to scrutiny at the individual level, or b) require that the
voter be extravagantly knowledgeable and informed (e.g., Paldam and Skott 1995),
an assumption famously dismissed as incredible by Downs (1957).

In this essay, I offer an alternative, micro-level explanation for the cost of ruling.
This explanation requires only that the voter have very basic expectations for cabinet
behavior and very basic perceptions of cabinet behavior. I argue that, because coali-
tion governance requires compromise and cooperation, voters are likely to alter their
perception of the policy positions of cabinet parties over the life of the government,

\footnote{Stevenson (2002) estimates the cost of ruling still exceeds 2% after institutions and economic
performance are accounted for.}
perceiving them as growing more similar. I propose a mechanism — surveillance effects — adapted from emotion-based explanations of vote choice (e.g., Brader 2005; Marcus et al. 2000), that will cause all voters that adopt this perception to weigh performance more highly and partisan allegiance less highly than their counterparts that do not adopt this perception when casting their ballot. Next, I propose a mechanism — mandate effects — that will cause incumbent cabinet supporters to be less likely, and incumbent partisans to be less likely still, to support the cabinet, given the perception that the cabinet parties have grown more similar, even when economic performance is accounted for.

In the rest of the paper I examine the governance penalty in more detail and discuss the strengths and weakness of the prevailing models seeking to explain it. Then, building on the comparative behavioral and institutional legislative literatures, I provide the motivation for my model of incumbent vote loss. I then present the theory, drawing on existing emotional voting literature, predominantly aimed at the American case, and derive three testable hypotheses. Next, I present the data employed to test the hypotheses, a collection of panel survey from the Netherlands and Norway and execute two simple statistical tests of the empirical implications of my theory. The analysis provides robust support for the arguments I lay out.

The governance penalty and policy balancing

In the American case, the notion that executive incumbency is a disadvantage in midterm legislative elections, has been widespread for decades. Edward Tufte’s (1975) seminal work on midterm congressional elections is now nearly forty years old and was
hardly the first to consider the issue. Classic research by Key (1958) and Campbell (1960) discuss how the party of the president regularly loses congressional seats in midterm elections. Many works attributed the regularity of these losses to "surge and decline" effects. Converse’s (1966) idea of a "normal vote" perhaps best captures this school of thought. Very simply, we can consider a party’s potential vote as a distribution. Each election represents a draw from that distribution. The greater each draw from the distribution, the more likely the subsequent draw is to be lower. Though some shock may prompt a significantly greater draw in the current election, it also significantly increases the probability of a lesser draw in the following election. Thus, the observed pattern of surge and decline.

The notion of electoral change being powered by randomized exogenous shocks has been more or less abandoned in favor of other, more systematic explanations having evolved out of the retrospective voting literature (e.g. Fiorina 1981; Key 1966). Tufte’s (1975) theory, that midterm losses are driven by a mixture of presidential approval and real economic change, is one such example. This line of research has a clear parallel in the comparative literature, the economic voting literature. This research attributes incumbent vote loss to voter evaluations of government performance, conditioned on institutional contexts (Powell and Whitten 1993; Duch and Stevenson 2008). These models typically utilize macro level metrics of economic performance (as in Powell and Whitten 1993) or micro level perceptions of economic performance (as in Duch and Stevenson 2008) to stand in as a proxy of general government performance. That is, voter perceptions of total government competence can be captured roughly by voter perceptions of economic welfare. The principal prediction of this is literature is simple: the government can expect to lose votes when voters perceive
that economic performance has been poor and this penalty can be exacerbated or mitigated by how easy it is for voters to attribute responsibility for these economic conditions to the government.

Retrospective voting models generate excellent predictions for the electoral fortunes of the incumbent executive conditioned on economic performance. But governments lose votes much more often than the economy falters. Indeed, Stevenson estimates that governments can expect to lose, on average, approximately 2% of their vote share, “even after controlling for various economic and political influences” (2002, 158). What explains these losses? The predominant compliment to economic voting is the policy balancing explanation. Here, voter evaluations of government performance are assumed to be different than in the economic voting literature. In addition to having a preference for “good policy,” i.e. controlled inflation, low unemployment, etc., voters also have some spatial preferences for policy, with a large amount of the voters falling somewhere toward the middle of ideological continuum. In the American case, these voters are located between the Democrats and Republicans and have a preference for moderate policy. The Alesina and Rosenthal (1989, 1995) model predicts that these voters will systematically vote against the party of the president in midterm elections in order to facilitate more moderate policy outcomes via inter-branch bargaining.

This model of incumbent vote loss has been adapted to comparative context by Paldam and Skott (1995) in what has become known as the “median gap” model. Here, the intuition is that, in a simplified competition space in which voters have the choice between “left” and “right” governments, pivotal voters about the median will alternate support between the left and right in order that they may, on average, over
time, obtain more moderate policies. Paldam and Skott predict that for each polity there will be a number of voters who will always vote against the incumbent and that, on average, the incumbent government’s vote loss will increase as it moves the status quo further and further from the median. Stevenson (2002) extends this model to derive hypotheses on the effects of cabinet duration and government efficiency on the cost of ruling.\footnote{These hypotheses are: 1) “holding realized duration and policy efficiency constant, incumbents facing electorates that expect cabinet duration to be longer will lose less votes than those in which expected cabinet duration is shorter” and, 2) “holding both realized and expected duration constant, incumbents facing electorates that perceive policy efficiency to be higher will lose less votes than those in which policy efficiency is perceived to be lower” (173-4). We will return to these hypotheses in the empirical section.}

Though simple on their face, policy balancing models have substantive value as they predict routine incumbent losses. Given Stevenson’s insight that incumbent governments can expect a vote loss of about 2% after the economy is accounted for, such a prediction has face validity. There are, however, several troubling aspects of these models. Particularly, they apply only to a small subset of voters about the median, ignore that most parliamentary governments span the median (Martin and Stevenson 2001, 2010), and disregard the affects of economic or other performance evaluations, partisanship, etc. Perhaps even more troubling is the cognitive burden these models places on voters.

Consider what is expected of the voter in the most basic form of the Alesina and Rosenthal (1989, 1995) framework. Moderate voters must 1) know who is in government, 2) have some perceptions of government policy output, and 3) have sufficient understanding of the policy-making process to realize the midterm election presents an opportunity to balance policy. Most would agree that these assumptions
are reasonable in simple contexts, such as the U.S. or U.K., where there are only two parties, thus a moderate voter need only know the party of the executive in order to know which party to vote for. However, as the number of parties increases or the complexity of the policy-making process increases, the difficulty in determining which party to vote for gets more difficult. This is perhaps the largest obstacle to importing policy balancing models to coalitional systems.

What is expected of the voter in the median gap model (Paldam and Skott 1995) in coalitional systems, where there is no separation between legislative and executive, thus no midterm elections? Just as before, the moderate voter must know 1) which parties are in government and 2) have some perceptions of government policy output. Given that they wish to moderate policy from where the current cabinet has brought it, the voters must now possess several pieces of information. They must 3) have perceptions of the policy positions of all parties, 4) understand how votes are aggregated into policy (i.e., understand the cabinet formation process), 5) have expectations for which cabinets are likely to form and 5) what the distribution of agenda power (of policy influence) will be in those potential cabinets, and 6) given these factors, calculate which party to vote for in order to maximize the probability of policy moderation. Many doubt the ability of voters to make these calculations, most notably Downs when he famously concluded that, when faced with this task,

“Eventually each voter either abstains, votes after cutting off his deliberation at some unpredictable point, or decides it is easier just to vote for his favorite party.” (1957: 163)

Clearly, voting decisions of this type put tremendous strain on voters, but there is
still more to it. Not only must moderate voters possess all of the relevant information and cognitive abilities listed above, but their information and calculations must be sufficiently accurate, and the number of such voters must be sufficiently high, as to bring about the levels of incumbent vote loss that we actually observe. Further, there would need to be a sufficient number of these moderate voters, dissatisfied with the position of incumbent policy and capable of making these calculations, to bring about the incumbent losses we observe controlling for the effects of partisanship, economic performance, and electoral institutions. Stated alternatively, for these incredibly sophisticated and moderate voters, preferences for policy position must outweigh the effects of economic evaluations, partisanship, etc. Given the requirements in terms of knowledge, cognitive ability, and preference ordering that policy balancing models place on individual voters, it is no wonder that such models aimed at explaining incumbent vote loss have received so little individual level empirical testing outside of the American case.⁴

What level of voter sophistication is reasonable? My model of incumbent vote loss places minimal requirements on the voter. Here, voters need only 1) know which parties are in cabinet, 2) have expectations of cabinet behavior (these expectations are discussed in more detail below), and 3) have perceptions of the cabinet’s behavior. However, unlike the policy balancing model, only one of these factors must be factual information — voters must only have high quality information regarding the composition of the government, an assumption that has empirical support in previous

⁴Kedar 2005 and Bargsted and Kedar 2009 present empirical tests of prospective voting models that take post-electoral bargaining into account, but these models do not explain systematic incumbent vote loss. Further, there has been, to my knowledge, only one policy balancing model tested empirically in a non-American context (Kedar 2006) and this is an aggregate level test in a federal context — with expectations and decision-making difficult similar to the American case.
chapter. Their perceptions and expectations need not be accurate, so long as they have them. This a major departure from previous models that assume not only a cognitively sophisticated voter, but a cognitively sophisticated voter that is *perfectly informed*.

**Expectations and reactions**

The research on the role of emotion on political decision-making has advanced quite a bit in the last decade. Since Marcus et al. (2000), it has become accepted that voters draw emotional stimuli from information that they encounter and that these stimuli alter decision-making processes. More specifically, Marcus et al. argue that two emotional processes may be activated. The first, enthusiasm, is activated when voters receive information that matches their pre-existing notions or reinforces their expectations. Having their expectations reinforced, these voters are likely to rely on habit (e.g., partisanship and previous vote choice) when it comes time to make voting decisions. For example, a Labor voter in the UK may have voted against Cameron’s Conservatives in the 2010 election, fearing that they would dismantle the National Health Service. For this voter the reception of information suggesting the Cameron government slashed NHS funding would trigger the enthusiasm mechanism and this voter would be less likely to pursue new information and more likely to simply vote for Labor again, her fears of Conservative animosity toward the NHS having be reinforced.

The second emotional mechanism is surveillance. Here, voters who receive countervailing information, information that runs contrary to their expectations, are likely
to abandon habit in favor of contemporary evaluations of the political context. Revisiting the example above, if our Labor voter received information suggesting that the Cameron government increased the NHS budget, an action that is incongruent with her expectations, she would be likely seek out more information and recalculate her position toward the Conservatives. To quote Marcus et al.,

“...people would no longer rely on their political habits ... [and] would experience heightened motivation to learn, to gather contemporary information...” (2000: 61)

In their book, the authors support their theory with evidence drawn from the American National Election Survey. This theoretical framework has been utilized by others. Taber et al. (2001) offer a theory similar to Marcus et al. Further, the theory, in slightly altered forms, has been retested in the laboratory (e.g., Brader 2005; Redlawsk 2002) and these tests have delivered robust support.

I adopt a similar framework here. I argue that voters have simple ex ante expectations for coalition behavior and voters that perceive behaviors contrary to these expectations will abandon voting habit. More specifically, all voters expect some level of conflict in the policy-making process between coalition partners — this conflict is evidence that individual parties are fighting for their preferred policies. Voters who do not perceive this conflict, whose ex ante expectations are not reinforced, will be more likely to rely on contemporary evaluations (perceptions of performance and policy position) than habit (previous vote and partisanship) — following Marcus et al. (2000), I call this the “surveillance effect.”

There is a second, more important step to my arguments. As I explain in detail
below, voters will associate conflict in the policy-making process as evidence of the cabinet rigorously pursuing the policies it promised its supporters. Thus, incumbent cabinet supporters, those who voted for the incumbent government in the previous election, who do not perceive conflict will have a negative reaction to their perceptions of cabinet behavior. I argue that voters will perceive this lack of conflict as the cabinet’s failure (or neglect) to ardently pursue the policies they have promised their supporters. Therefore, previous supporters who do not perceive the conflict they expect, will be less likely to support the incumbent government even when cabinet performance and policy position is controlled for. Further, incumbent partisans, who are nearly all supporters, but have a stronger relationship with the cabinet, should exhibit even stronger negative effects in reaction to this perception. These effects are called “mandate effects.”

**Why voters expect coalition conflict**

Powell (2000) notes that voters understand that entry into coalition government assures parties the opportunity to bargain for the policies they campaigned on. Indeed, he describes elections as policy mandates. In a way, voters and parties enter into a contract. Voters pledge their support to a party, and, in exchange, this party agrees to pursue a set of policies. Voters understand that coalition governance requires that parties with disparate preferences must bargain over policy, thus, if cabinet parties are fervently pursuing their mandated platforms, voters should receive signals (from the media, etc.) of conflict from the cabinet. Indeed, theoretical work on coalition policy-making argues that parties actively utilize agenda powers and legislative institutions to send such signals to their supporters and empirical tests support this.
Martin and Vanberg (2011) argue that, because parties have a mandate from their supporters to pursue certain policies, coalition parties feel substantial pressure to undermine cooperative policy-making processes in order to signal to their voters that they are “fighting the good fight” (144). Others have discussed the temptation that cabinet members feel to utilize their agenda powers signal to voters that are pursuing their core policies. Michael Thies (2001) and Kim and Loewenberg (2005) also discuss the dilemmas this motivation causes in the process of policy delegation within the cabinet. Each study emphasizes the need for cabinet parties to signal their resolve in pursuing certain policies on behalf of their supporters.

**Why voters may not perceive coalition conflict**

Above, I argued that voters expect to observe some conflict between coalition partners in the policy-making process. This is because parties have a mandate to honor, certain policies that they have agreed to pursue. Given that different parties have different policy mandates and each party must fight for their particular policy, the ex-ante expectation is that policy-making process should be fraught with conflict. But recent empirical research suggests that the majority of voters do not perceive this conflict.

In investigating the effect of coalition participation on the manner in which voters perceive political parties, I found in the previous chapter that coalition partners are perceived as significantly more similar than parties that are not currently coalesced. I argued that voters utilize cabinet composition as an inexpensive heuristic with which they may update their perceptions of the policy positions of parties in their system. I offered three principal reasons for why this heuristic would cause voters to push cabinet partners together on the ideological continuum:
“[1] . . . empirical findings in the cabinet formation literature relating ideological moderation (in the direction of one’s partners or potential partners) to the probability of cabinet formation, [2] theoretical and empirical work suggesting that the actual experience of governing together creates incentives for policy accommodation and compromise, and [3] empirical work on the legislative behavior of cabinet parties suggesting that once government parties have entered government on the promise of making specific policy compromises, they actively work to realize these promises in legislation.” (21)

In other words, during the campaign, parties make promises to pursue certain policies. For parties that actually enter into government, every aspect of the coalition policy-making process — from cabinet formation to legislative review to norms regarding collective responsibility — conspire to signal that these parties are not honoring their mandates. Parties signal a broad and wide-ranging policy compromise upon their entry into coalition. The formation of a government formalizes incentives toward cooperation. And, over the life of the government, cabinet partners utilize parliamentary institutions to actively enforce the terms of the compromise. Thus, voters are treated with a strong signal of compromise that is reinforced throughout the life of the government via policy-making procedures and outcomes that may condition the messages they receive. These signals cause voters perceive cabinet partners as converging on each other spatially, that their policy positions are growing more similar.

The implication of this finding is that the more similar voters perceive cabinet
parties growing over the tenure of their government, the more likely they are to believe that parties have broken their mandate. However, I go to great lengths to argue that this perception, while pervasive, will not be adopted equally across the electorate. I reference the Zallerian (1992) framework and predict that more politically sophisticated voters would be more apt to receive countervailing signals from the cabinet that combat this perception.\(^5\) Indeed, the previous chapter presents empirical evidence that more sophisticated voters are significantly less likely to have this perception than less sophisticated voters. It is important to bear in mind, however, that my theory makes no claims as to which voters will adopt this perception, indeed, when it comes to testing, perceptions are assumed as given and only their effects are assessed.

**Hypotheses**

I have argued that voters, regardless of the partisanship or previous vote choice, should will have an ex ante expectation of conflict in the coalition policy-making process. This is because conflict is indicative of cabinet parties rigorously pursuing their policy mandate. Some voters will not perceive conflict, but cooperation, they will perceive the policy positions of cabinet parties growing more similar over the government’s tenure. These voters will have their surveillance mechanism triggered by this perception, which is incongruent with their expectations. The surveillance mechanism will prompt voters part with voting habit, partisanship and past choice, and rely more heavily on contemporary evaluations of the cabinet, satisfaction with

\(^5\)The signals may include, but are not limited to, signals from ministerial drift (Martin and Vanberg 2004, 2005) or from parliamentary debates (Martin and Vanberg 2008; Proksch and Slapin 2011).
its economic performance and their perceptions of its policy positions. Thus, the surveillance hypothesis:

- Surveillance hypothesis: Voters who perceive the cabinet as converging will rely more heavily on their perceptions of cabinet performance and policy positions and less heavily on previous support and partisanship than voters who do not perceive the cabinet as converging.

Surveillance effects apply to all voters, but mandate effects do not. Voters who supported the incumbent previous are different from previous opposers. While I argue that all voters, including previous opposers, have expectations of cabinet behavior and observing behaviors contrary to their expectations will trigger a surveillance effect, this effect may increase or decrease a voter’s likelihood of incumbent cabinet support, conditioned on her perceptions of the cabinet (Redlawsk 2002). For example, a previous opposer may observe unexpected behaviors, survey the state of the world, the economy, the policy positions of the cabinet, etc. and decide, in the coming election, to support the incumbent that they previously opposed. Likewise, a previous support may choose to support again or oppose the incumbent conditioned on their perceptions of the cabinets performance. Thus, surveillance effects merely explain a departure from habit that, not a systematic loss of votes for the government controlling for performance.

Mandate effects do explain this systematic loss of votes for incumbents, as, unlike surveillance effects, they are exclusively negative and only effect incumbent supporters. The reason is because incumbent supporters are fundamentally different form incumbent opposers. While I have argued that all voters, opposers included, have
ex ante expectations of conflict in the policy-making process, only supporters should react with disappointment when this conflict is not observed. This is because only supporters have a mandate with the incumbent government.

As discussed above, borrowing from Powell (2000), voting for a given party is akin to entering into a contract with them. They receive the vote and, in exchange, they agree to pursue to certain policies. Supporters expect this pursuit; they have paid for it in advance. When supporters observe behavior that is discordant with this expectation, not only is the surveillance mechanism triggered, but the behavior draws and negative reaction. Supporters become disappointed in their cabinet and are thus less likely to support it in the coming election, even when its performance is controlled for, when they perceive the ideological convergence within the cabinet. Hence the mandate hypothesis.

- Mandate Hypothesis 1: Voters who supported the incumbent in the previous election will be less likely to support it in the next election when they perceive the cabinet as converging.

These effects become compounded for the subset of voters that are incumbent cabinet partisans (almost universally previous supporters). Cabinet partisans will exert an even more negative reaction than non-partisan supporters because they have a deeper relationship with the cabinet. Indeed, it is this deeper relationship that defines the power of partisanship; without it our models of vote choice would show no effect for partisanship when performance and policy proximity considerations are accounted for (e.g., Gomez and Wilson 2006). Research in organizational behavior has found that these relationships suffer substantially when trust expectations are
violated. Thus the expectation is that, given the perception of a converging cabinet, thus the violation of trust, incumbent cabinet partisans will punish the government even more harshly than their non-partisan counterparts.

- Mandate Hypothesis 2: *Incumbent government partisans will be less likely to support the incumbent when they perceive the cabinet as converging.*

The theoretical framework presented above has been summarized in Figure 3.1. Various perception inputs (discussed at length in the last chapter) lead the voter to either perceive the cabinet as convergent, a perception discordant with their expectations, or not. Given the perception of convergence, all three voter types discussed — cabinet supporters, cabinet partisans, and opposition supporters — will rely more heavily on contemporary evaluations (cabinet performance and policy position) and less heavily on habit (partisanship and previous vote choice) when casting their next vote. Then, controlling for perceptions of cabinet performance and policy position, cabinet supporters will react negatively to the perception of convergence, becoming less likely to support the cabinet. This effect is magnified among cabinet partisans. Opposition supporters who perceive convergence, having no mandate with the cab-

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6 See literature regarding the “psychological contract” (Robinson and Morrison 1995; Robinson and Rousseau 1994).

7 These arguments are not entirely new to political science. There has been substantial research on the effects of general trust in a voter’s political institutions on propensity to support the incumbent both in general (i.e. thermometer ratings) and at the polls (e.g. Hetherington 1998, 1999). While such studies show that a decline in general political trust can hurt the incumbent, as I predict here, the mechanism is not the same. Specifically, Hetherington (1999) suggests that general political trust becomes salient as a heuristic in candidate centered elections. The mechanism I am suggesting here has a better parallel with research on personal relationships. I am not suggesting that perceived cabinet convergence is indicative of violating trust in institutions, but rather trust in individuals operating within those institutions. For example, while an incumbent partisan may feel betrayed by the cabinet if she perceives it as flaunting its mandate, it is unlikely that she will feel betrayed by the parliament as a whole. Similarly, a married individual who has been the victim of infidelity may feel betrayed by their spouse, but not the institution of marriage.
Figure 3.1: This chart shows the effect of perceived convergence

Data, operationalization, and estimation

To test these hypotheses, I have collected five panel surveys conducted in the Netherlands and Norway as part of the Dutch Parliamentary Election Survey and the Norwegian National Election Study, respectively. The Dutch panels were administered
at the time of the 1981 and 1982 elections, 1982 and 1986 elections, 1986 and 1989 elections, and the 1989 and 1994 elections and the Norwegian panel administered at the the 2001 and 2005 elections.\footnote{There was another such study available from Norway from the 1997 and 2001 elections. Unfortunately, this period saw a midterm replacement of the government formed after the 1997 election (the first Bondevik cabinet, composed of the Christian Democrats, the Center Party, and the Left). Such a replacement may not have excluded the data from consideration, however, the new cabinet was a single party minority caretaker, thus inappropriate for consideration here.}

These data provide the best possible testing ground for the theory I have laid out. Consider what we would require in our ideal data set. As the argument advanced above presents a micro-level relationship, our data must be at the individual level. Further, we would ideally have several pieces of information about several voters at two subsequent elections. We need to know whether or not each voter supported the cabinet in both elections and whether or not the each voter was a cabinet partisan at the time of first election. Our ideal data would also include measures of voter perceptions of the cabinet (in terms of policy position) at the time of both elections, so we can determine how those perceptions have changed, as well as measure of voter perceptions of cabinet performance between the first and second election. Finally, our ideal data would have measures of voter policy position, so we could derive a measure of policy preference congruence for the voter and the cabinet.

These are extremely specific and constraining requirements, but the five surveys I have collected satisfy each criterion. Further, these surveys yield a great deal of variation in terms of cabinet type. The data includes both two and three party coalitions. Cabinet duration varies from one to five years and each year in the range is represented. Further, the policy position of the included cabinets ranges from true right, to center-right, to centrist proper, to center-left. Lastly, because the institutional
structures of the Tweede Kamer and the Storting are so similar, all of this variation in context comes without the cost of possibly confounding institutional differences. Thus, the data provide every required variable, with cabinet-level variation and a constant institutional context. Table 3.1 shows how this data is distributed across the surveys.

Table 3.1: Distribution of data across surveys

<table>
<thead>
<tr>
<th>Country</th>
<th>Cabinet Composition</th>
<th>Cabinet Tenure</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Netherlands</td>
<td>CDA, D66, PVDA</td>
<td>1981 - 1982</td>
<td>949</td>
</tr>
<tr>
<td>Netherlands</td>
<td>CDA, VVD</td>
<td>1982 - 1986</td>
<td>561</td>
</tr>
<tr>
<td>Netherlands</td>
<td>CDA, VVD</td>
<td>1986 - 1989</td>
<td>573</td>
</tr>
<tr>
<td>Netherlands</td>
<td>CDA, PVDA</td>
<td>1989 - 1994</td>
<td>775</td>
</tr>
<tr>
<td>Norway</td>
<td>H, V, KRF</td>
<td>2001 - 2005</td>
<td>756</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td></td>
<td></td>
<td><strong>3562</strong></td>
</tr>
</tbody>
</table>

What, precisely, are the measurements that will be used in the analysis? The operational vote choice variable was obtained by asking the survey respondents, in similar language before two successive elections,

“Which party do you intend to vote for in the coming election?”

The response from the latter wave of the survey was transformed into a dummy indicating the voters intends to support the cabinet and defines the the dependent variable in the analysis. Thus, the level of observation is a voter within a survey, and the dependent variable is whether or not that voter intends to support the incumbent cabinet
To capture voters’ perceptions of the cabinet parties, I utilize the spatial placement element of the survey. That is, respondents were asked the following question in each survey, in similar language:

“In political matters people talk of ‘the left’ and ‘the right’. Please indicate your views using any number on a 10-point-scale. On this scale, where 1 means ‘left’ and 10 means ‘right,’ which number best describes [party X]?”

Using the responses from this question, I create a measure of perceived cabinet dissimilarity. Specifically, I produce a measure of cabinet policy dissimilarity modeled after the cabinet divisiveness term presented in Martin and Vanberg (2005, 2011) — the mean absolute seat-weighted distance for each set of party pairs in a given cabinet. The lower the value assumed by the term, the more similar the voter believes the cabinet parties are, the higher the value, the more dissimilar the respondent believes the cabinet parties are. By comparing the value of this term at the first election to its value at the second election, we can capture a measure of the respondent’s perceived cabinet convergence over the course of the government. The actual term used in the analysis is \( \log\left( \frac{d_{x2}}{d_{x1}} \right) \). That is, the logged ratio of perceived dissimilarity at the current election to perceived dissimilarity at the previous election. The logged ratio is employed to free the measurement of bias induced from differences in individual scale that we may observe using a subtractive operationalization.\(^9\) Thus, negative values denote perceived convergence, implying that the voter believes the cabinet is

---

\(^9\)To be specific, there are some voters who may view parties as clustered toward the median of the ideological space, or pressed to the extremes, or otherwise idiosyncratic in the manner that they perceive political parties. Using a standard subtractive term, these individual differences in scale may add unneeded noise to our attempts to measure the impact of perception change. Employing the ratio term, to a large degree, curtails this noise.
not pursuing its mandate, and positive values denote perceived divergence, implying that the voter believes the cabinet is pursing its mandate.

Before moving on to the other variables used in the analysis, an examination of the convergence term is warranted. Here, I evaluate the change in cabinet perceptions recovered from the panel data against expectations drawn from the cross sectional examination of the previous chapter. If the data generating process behind the covariate I employ here is the process suggested previously (i.e., voters interpreting various media messages and observations as cabinet parties not fighting for mandates and thus, appearing more similar), we would expect, on average, that the majority of survey respondents perceive the cabinet as converging rather than diverging. Further, this perception of convergence should be mitigated by increased levels of political interest, as more interested voters should be less reliant on simplifying heuristics and more prone to receive countervailing signals from cabinet parties or simply possess a heightened understanding of coalition policy-making (thus, perhaps understanding the necessity of cooperation).

Table 3.2, below, shows the distribution of perceived cabinet change. Recall that negative values indicate the voter believes the cabinet has grown more similar, positive values indicate the voter believes the cabinet has grown more dissimilar. The table supports the expectation that most voters perceive the cabinet as converging over the duration of the government. Indeed, nearly two thirds of voters believe that the cabinet has grown more similar over its term. Further, voters who perceive convergence perceive more change overall than voters who perceive divergence.

Table 3.3 shows a replication of last chapter’s perception updating model.\textsuperscript{10} In

\textsuperscript{10}Because the model here considers the cabinet as a whole and only the cabinet as a whole several
Table 3.2: Distribution of perceived cabinet convergence

<table>
<thead>
<tr>
<th>Perceived Convergence?</th>
<th>Yes</th>
<th>No</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Respondents</td>
<td>2283</td>
<td>1279</td>
</tr>
<tr>
<td>Perceived Change Mean</td>
<td>-4.90</td>
<td>3.77</td>
</tr>
<tr>
<td>Perceived Change SD</td>
<td>11.37</td>
<td>10.38</td>
</tr>
</tbody>
</table>

In this model, the dependent variable, a respondent’s perceived change in cabinet dissimilarity, is regressed on the respondent’s political interest, whether or not the respondent locates herself between cabinet parties, and the respondent’s ideological extremity (absolute distance from the median position on the continuum). As the table shows, each of the voter level covariates are significant and the same direction as the previous analysis.

Given that the sample analyzed here is smaller in comparison to the previous sample in terms of the number of countries, elections, and parties considered as well as the total number of observations, the similarity in the results reported in Table 3.3 is encouraging. Indeed, the similarity in results suggests that the Dutch and Norwegian voters I will analyze here are quite similar to the larger sample of voters across 16 parliamentary democracies in the previous analysis, at the very least in the manner in which they update their perception of partisan ideology. With the variables in the previous chapter model fall out. Specifically, all party level variables (cabinet or opposition status and governmental familiarity), and all survey level variables (administration type). Further, as I am employing real change in perceptions taken from the panel as the dependent variable and not merely trying to infer perception from cross-sectional data, there is no need to consider proxies such as CMP position. Thus we are left with only the respondent level independent variables, political interest and the two ideological controls.

As this measure takes different values in the surveys, this variable is transformed into quantiles. Further, the similarity in results suggests that my measure of cabinet dissimilarity is not inducing
Table 3.3: Determinants of perceived cabinet dissimilarity change. The model results show that more interested voters are substantially less likely to perceive the cabinet as converging than less interested voters.

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Coefficient</th>
<th>(Standard Error)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Fixed Effects</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Political Interest</td>
<td>0.140</td>
<td>(0.070)</td>
</tr>
<tr>
<td>Between Cabinet Parties</td>
<td>0.210</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Respondent Extremity</td>
<td>0.138</td>
<td>(0.048)</td>
</tr>
<tr>
<td>Intercept</td>
<td>-0.322</td>
<td>(0.079)</td>
</tr>
<tr>
<td><strong>Random Effects</strong></td>
<td>Panel Level Variance</td>
<td>0.016 (0.126)</td>
</tr>
</tbody>
</table>

Data Break

- N(Panels): 5
- N(Respondents): 3570
- AIC: 8148.947

Evidence suggesting that the data generating process behind the principal covariate utilized here is the same as proposed in previous research and evidence suggesting the representativeness of our sample is high, I turn my attention to other covariates I will include.

To test the first hypothesis (surveillance), we must identify covariates pertaining to habit and contemporary evaluations. For habit, I utilize two indicator terms; whether or not the respondent previously supported the incumbent cabinet and whether or not they are a member an incumbent party. For the former, I simply create a dummy indicating whether or not the respondent reported voting for a member of the current government in the first wave of the panel. In the analysis below, this variable is referred to as “previous support.” For the latter, I create an indicator for whether the respondent considered herself a member of a current cabinet party in the first wave bias at the level of the voter.
of the panel. In all surveys respondents were asked, in similar language, whether they considered themselves a member of or adherent to a political party and this question was leveraged to create the covariate called “incumbent partisan” below.

For contemporary evaluations, I also use two terms. The first captures whether or not the voter approves of the cabinet’s economic performance. Each survey asked, in similar language, whether or not respondent believed the economy got better, worse, or stayed the same. I follow Duch and Stevenson (2005, 2008) and convert these responses into an indicator signaling the respondent’s approval.\textsuperscript{13} In the tables below, this covariate is labeled “approves of incumbent.”

The second evaluation covariate captures the congruence of the cabinet’s policy positions and respondent’s policy preferences and indicates whether or not she perceives a cabinet party as being the most proximate to herself. That is, using the voter’s self placement and placement of all political parties on the ideological continuum, this covariate takes on a value of 1 if the smallest absolute distance between the voter and a party belongs to a member of the cabinet, and 0 otherwise (ties are awarded to the government). This term is labeled as “closest to incumbent” in the analysis below.

\textbf{Surveillance model}

To assess the impact of perceived convergence on habit and evaluation voting considerations, I estimate a model in which each independent variable discussed above

\textsuperscript{13}The number of possible responses ranges from three to seven over the surveys. The measure is folded such that respondents who answered that the government’s performance was neither good nor bad, were coded as “approving” rather than “not approving”. Of course, the substance of the results presented below are robust to the reversal of this coding decision.
(previous incumbent support, incumbent partisanship, incumbent approval, and incumbent policy congruence) is interacted with a variable indicating whether or not the respondent perceives cabinet convergence and these covariates are regressed on whether or not the respondent chose to support the incumbent government. The model could be written down thusly:

\[
\text{incumbent vote} = \alpha + \beta[\text{convergent}] \times \left( \beta[\text{habit}] + \beta[\text{evaluations}] \right) + \epsilon \quad (3.1)
\]

The model should not be evaluated as is, however. Recall that the data presents in a hierarchical form. As the data capture voting in five different elections, we can imagine several sources of bias from unmeasured variables in each of these contexts. In order to account for the possibility of such bias, I estimate a hierarchical regression model where error is allowed to vary at the survey level. This higher level error is summarized in the “random effects” portion of Table 3.4 and the estimates on the covariates of interest are reported in the “fixed effects” portion.

The parameter estimates and standard errors are presented in two separate columns to simplify comparison.\textsuperscript{14} The estimates in the “not convergent” column on the left are the parameter values when the perceived convergence indicator is turned off and the estimates in “convergent” column on the left are the parameter estimates when the perceived convergence indicator is turned on. For example, the “convergent” estimate of the previous support covariate is the estimate from the previous support constituent term plus the estimate from the previous support - perceived convergence interaction. The predictions are noted in the center column and the larger parameter estimate

\textsuperscript{14}This presentation is modeled after Brader (2005). However, I estimate a fully interacted model rather than desegregating my sample and estimating two separate models.
Table 3.4: Analysis of Dutch and Norwegian voters’ incumbent cabinet support in five panel surveys from 1981 to 2005. Coefficients and standard errors are calculated from a fully interacted model (each listed parameter interacted with the perceived convergence indicator).

<table>
<thead>
<tr>
<th>Type</th>
<th>Covariate</th>
<th>Not Convergent</th>
<th>Prediction</th>
<th>Convergent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Habit</td>
<td>Previous Support</td>
<td>4.850</td>
<td>&gt;</td>
<td>3.973</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.185)</td>
<td></td>
<td>(0.200)</td>
</tr>
<tr>
<td></td>
<td>Incumbent Partisan</td>
<td>1.112</td>
<td>&gt;</td>
<td>0.854</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.165)</td>
<td></td>
<td>(0.178)</td>
</tr>
<tr>
<td>Evaluation</td>
<td>Approves of Incumbent</td>
<td>0.844</td>
<td>&lt;</td>
<td>1.385</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.190)</td>
<td></td>
<td>(0.201)</td>
</tr>
<tr>
<td></td>
<td>Closest to Incumbent</td>
<td>0.453</td>
<td></td>
<td>0.539</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.166)</td>
<td></td>
<td>(0.177)</td>
</tr>
<tr>
<td></td>
<td>Intercept</td>
<td>-4.364</td>
<td></td>
<td>-4.517</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.429)</td>
<td></td>
<td>(0.495)</td>
</tr>
</tbody>
</table>

Random Effects

| Panel Level Variance | 0.659 |
|                     | (0.812) |

Data Break

| N(Panels) | 5     |
| N(Respondents) | 3850  |
| AIC        | 2188.261 |

groups are shaded.

The analysis reveals strong support for the surveillance hypothesis. Voters who perceive cabinet convergence, indicative of behavior that is discordant with voter expectations, rely more on their contemporary evaluations of the cabinet and less on voting habit than their counterparts who do not perceive cabinet convergence. The difference in the parameter weights is quite significant substantively. A simulation exercise reveals that effects of the habit parameters is greater for voters who do not perceive convergence with probability 0.998. The significance on the evaluation estimates is similarly robust. The substantive effect of evaluations is greater for those who perceive convergence with probability 0.994.
These relationships are plotted in Figure 3.2. Here, the distribution of habit and evaluation effects for both voter types are plotted. The top plot shows the effect of going from a non-incumbent partisan previous opposer to an incumbent partisan previous supporter, holding evaluation effects constant at 0. The bottom plot shows the effect of changing from a disapproving voter who places herself closest to an opposition party to an approving voter that places himself closest to an incumbent party while holding partisanship and previous cabinet support constant at 0. Note that there need not be no overlap in predicted effects for the difference to be significant and that the significance of the differences between groups (calculated via simula-
tion and bootstrapping) is given on the plots. This is very strong support for the surveillance hypothesis.

**Mandate model**

The theory presented posited two mechanisms: surveillance effects and mandate effects. The analysis above yielded strong support for the surveillance hypothesis. Voters that observe behavior that is discordant with their expectations weight contemporary evaluations more heavily and habit less heavily in their voting calculous than they would otherwise. The mandate mechanism predicts that, given the effects of evaluations and habits, incumbent supporters and partisans will be less likely to support the cabinet when they observe perceived convergence as this perception as it is indicative of the cabinet flaunting its mandate. To evaluate the mandate hypotheses, I build an empirical model that estimates the effects of perceived convergence for supporters and partisans while controlling for influence from their evaluations of the cabinet as well as factors from the competing model of incumbent vote loss, the median gap model.

In this model, the covariates of interest are the interaction of change in each respondent’s perception of the cabinet (the continuous term summarized in Table 3.2) with whether or not they are a previous incumbent supporter or an incumbent partisan. The mandate hypotheses is supported if these interactions are positive and statistically significant and the interaction of change, previous support, and partisanship is not significant and negative. The model also includes the evaluation covariates from the above analysis; whether or not the respondent approves of the cabinet’s performance and whether or not the respondent perceives a cabinet party as the closest
to him. Also, as one may imagine the attribution of economic responsibility may be easier when the cabinet is smaller, I include a dummy variable that indicates the incumbent is a two-party coalition (the baseline is a three-party cabinet).

With the covariates typically utilized in retrospective and spatial voting analyses included, I may now move onto to secondary concerns. Here, I include a series of covariates aimed at accounting for effects stemming from the implications of the median gap model. According to this model, we would think that a voter who is located close to the median and perceives all cabinet parties as located to one side of her, would be more likely to vote against the incumbent to “balance” policy outcomes in the long term. Thus, I include a covariate indicating that the voter has located himself between cabinet parties in the competition space. This measure is interacted with a term capturing voter distance from the median, a simple folded measure of respondent extremity. The expectation is that these terms will generate a net positive effect on the probability of supporting the incumbent when voter extremity is not low and the voter perceives herself as being between cabinet parties. Given the nature of the data, the expectation is that $\beta_{[\text{between}]} + \beta_{[\text{between} \times \text{extremity}]} > 0$.

The other median-gap control come form the the Stevenson (2002) extension. The most salient prediction of that extension to this study regards duration. Stevenson predicts that, holding cabinet efficiency (of policy change) and voter expectations of cabinet duration constant, the longer a cabinet survives, the more votes it should lose. To account for these effects, I include each cabinet’s duration in whole years.

The results of the model are above in Table 3.5. Before discussing support for my hypotheses, it is important to note that the parameters on the control variables relating to retrospective and spatial voting are robust and in the proper direction.
Table 3.5: Analysis of Dutch and Norwegian voters in five panel surveys from 1981 to 2005. Logistic regression with random effects allowed at the panel level. The dependent variable takes on a value of 1 if the respondent intends to support the incumbent cabinet and 0 otherwise.

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Coefficient</th>
<th>(Standard Error)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Previous Support</td>
<td>3.862</td>
<td>(0.164)</td>
</tr>
<tr>
<td>Incumbent Partisan</td>
<td>-0.894</td>
<td>(0.356)</td>
</tr>
<tr>
<td>Previous Support * Incumbent Partisan</td>
<td>2.368</td>
<td>(0.393)</td>
</tr>
<tr>
<td>Approves of Incumbent</td>
<td>1.110</td>
<td>(0.146)</td>
</tr>
<tr>
<td>Closest to Incumbent</td>
<td>0.605</td>
<td>(0.132)</td>
</tr>
<tr>
<td>Perceived Change</td>
<td>-0.263</td>
<td>(0.155)</td>
</tr>
<tr>
<td>Previous Support * Perceived Change</td>
<td>0.379</td>
<td>(0.193)</td>
</tr>
<tr>
<td>Incumbent Partisan * Perceived Change</td>
<td>0.844</td>
<td>(0.405)</td>
</tr>
<tr>
<td>Previous Support * Incumbent Partisan * Perceived Change</td>
<td>-0.655</td>
<td>(0.447)</td>
</tr>
<tr>
<td>Between Incumbent Parties</td>
<td>0.061</td>
<td>(0.266)</td>
</tr>
<tr>
<td>Respondent Extremity</td>
<td>0.737</td>
<td>(0.266)</td>
</tr>
<tr>
<td>Between Incumbent Parties * Respondent Extremity</td>
<td>0.050</td>
<td>(0.673)</td>
</tr>
<tr>
<td>Cabinet Duration (in years)</td>
<td>-0.804</td>
<td>(0.188)</td>
</tr>
<tr>
<td>Two Party Cabinet</td>
<td>-1.151</td>
<td>(0.399)</td>
</tr>
<tr>
<td>Intercept</td>
<td>-1.614</td>
<td>(0.631)</td>
</tr>
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Random Effects Panel Level Variance 0.121 (0.348)

Data Break

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<table>
<thead>
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<tr>
<td>N(Panels)</td>
<td>5</td>
<td></td>
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<tr>
<td>N(Respondents)</td>
<td>3537</td>
<td></td>
</tr>
<tr>
<td>AIC</td>
<td>2012.847</td>
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</table>

Respondents are more likely to support the incumbent if they approve of their past performance and if they place themselves closest to a cabinet party. While these parameters do not affect the argument I have presented above, they do signal that the data are robust to our expectations of voting behavior given the existing literature. If these variables were insignificant or in the wrong direction, it may give us pause in drawing inferences regarding the validity of the findings.

Unlike the covariates included to account for spatial and retrospective voting, the covariates included to test the predictions of the original median-gap model do
not yield the anticipated estimates. As noted above, the median-gap model would
predict that the covariate indicating the respondent is between coalition parties plus
the term interacting this covariate with respondent extremity would be positive and
significant. They are insignificant. The covariate included to account for the duration
prediction of the Stevenson (2002) median-gap extension, however, is significant and
in the predicted direction. The longer a cabinet survives, the less likely voters are to
support it in the next election, even when performance is controlled for.

Moving on to the covariates of interest, the analysis reveals support for the man-
date hypotheses: previous cabinet supporters are less likely to support the incumbent
in the coming election if they perceive cabinet convergence, signaling the failure of the
cabinet to pursue its mandate. This is evidenced by the net positive relationship on
the perceived change and perceived change - previous support interaction. The second
mandate hypothesis is also supported. Partisans that perceive convergence are much
less likely to support the incumbent than partisans that do not perceive convergence.
Further, as we would expect, given the argument that previous supporters observe
cabinets differently than previous opposers, the constituent perception change covari-
ate presents an opposite, though far less statistically and substantively robust, effect
for previous government opposers.

There are two more recovered relationships worth noting. First, incumbent par-
tisans that did not support the cabinet in the previous election are less likely to
support in the current election than non-partisans. The data seem to suggest that
these voters have, in essence, abandoned their party. Second, the parameter estimate

\footnote{These voters are not an insignificant proportion of the population. Nearly one quarter of voters with a partisan affect voted against the incumbent in the previous election. Of these, nearly 95% voted against the incumbent in the next election.}
on the supporter-partisan-perceived change interaction is quite small and does not even approach statistical significance. This implies that the effects of perceived convergence are additive for supporters and partisans. In other words, supporters may be disappointed that their contract was violated, and supporters may be disappointed that their trust was violated, but these two affects do not cancel each other out.

Figure 3.3: Predicted probability of incumbent support over the range of perception change in the data. These probabilities are plotted for incumbent opposers, supporters, partisans, and partisan supporters. The dots indicate the mean predicted probability for each group at the minimum, median, and maximum values of perceived change.

The substantive effects of perceived change in cabinet dissimilarity are shown in Figure 3.3 for four voter types: previous incumbent opposers, incumbent supporters, incumbent partisans, and incumbent partisan supporters. The graph plots the con-
idence interval for the predicted probability of supporting the incumbent over the sample range of perceived cabinet dissimilarity change. The far left of the x-axis is most convergent perception, the far right is the most divergent perceptions. The predicted probabilities are calculated assuming that voters place themselves closest to a cabinet party and approve of the cabinet’s performance, but that the voters are not located between cabinet parties and place themselves at the median of the political continuum. The voters are evaluating a three party cabinet with a duration at the sample mean, approximately two and a half years.

There are several interesting takeaways from the plot. First, despite the middling statistical significance of perceived change for opposers, the substantive impact is significant and directionally different than for the other three voter types. Next, we see that the effect of perceived convergence is quite significant and negative for partisans, supporters, and partisan supporters. Indeed the predicted probability for partisan supporters, the most reliable incumbent voters, drops from nearly one to just over 0.85. The cabinets that display the most internally contentious policy-making behaviors can be assured of their core supporters continued approval, but the least contentious cabinets face decline of nearly 0.15 in the predicted supporter of their base.

The effects for other voter types are similar. Indeed, for incumbent partisans that did not support the cabinet in the previous election, the effects of perceived contention in the policy-making process are huge. By demonstrating that they are fight vehemently for the policies they promised, cabinet parties can increase the probability of support from these voters from nearly zero to nearly a coin-flip. These results imply that the incumbent is capable of winning back a large portion of these voters
that have abandoned the cabinet by sending a clear message that they will “fight the
good fight” (Martin and Vanberg 2011).

What are the real effects of perceived convergence for the incumbent in terms of
votes? To assess these effects I simulate several thousand elections and assess the
difference in predicted incumbent votes from a first difference reduction in perceived
change. That is, how many votes will the incumbent lose when the level of convergence
increases by one standard deviation? The results of this exercise are plotting in Figure
3.4 below. The figure reveals that, for the sample analyzed here, the effect of a first
difference increase in perceived cabinet convergence is a penalty of 2% or more with
probability greater than 0.99. Given that the last chapter has shown that the tendency
to perceive cabinets as converging is pervasive and the relative stability of Dutch and
Norwegian voting behavior (e.g., Thomasson 1993), this is an enormously robust
result on a substantive and normative level. Indeed, this swing in vote share would
be more than enough to, for example, alter the allocation of formatuer privileges
following two of the last three Dutch parliamentary elections.

Discussion

In this paper I presented an alternative, micro-level explanation for the cost of ruling
in coaltional democracies based on the simple notion that voters have expectations
for coalition behavior and the perception of behaviors that are discordant with expec-
tations has consequences. The analysis above provide evidence for the surveillance
hypothesis, adapted form Marcus et al. (2000). When behaviors contrary to expecta-
tions are observed, all voters are more likely to break with habit and rely more heavily
Figure 3.4: This figure shows the substantive effect of a first difference reduction to the perceived cabinet convergence term. The frequencies displayed are net changes in the predicted vote share for the incumbent cabinet. The distribution of these frequencies indicates that perceived cabinet convergence significantly decreases incumbent cabinet support.

Simulated Incumbent Vote Loss
Resulting from a First Difference Reduction in Perceived Cabinet Change

on contemporary evaluations of the cabinet when casting their vote. This implies an increase to the penalty or reward a cabinet may expect from its economic management (and other policy decisions) when voters perceive the cabinet as accommodative in the policy-making process.

The analysis also bore strong evidence for the mandate hypotheses. Holding current evaluations constant, voters who supported the incumbent previously are
less likely to support it in the future if they perceive behaviors implying the that the cabinet is not fervently pursuing the policies it promised voters. This effect is heightened when voters have a deeper relationship with the cabinet, as evidenced by the additional negative effects for incumbent partisans who perceive this behavior.

The vote penalty that may by levied by perceived cabinet convergence, a perception that is ubiquitous in coalitional systems, is quite often more than enough to change the allocation of formateur privileges, and thus substantively change the composition of government. The substantive effects presented above imply that governments may lose at least an additional 2% of their vote share from an increase in perceived convergence, holding voter all of other covariates constant.

The findings reported here, though novel, should not come as a surprise. The role of emotions in voting behavior has been well researched in the American case, and the arguments laid out here are intuitive. The implications of these findings, however, are wide-reaching, incredibly interesting on a substantive and normative level, and, given the state of the current literature, not necessarily intuitive.

The findings reported here suggest that cabinet parties have substantial motivation to take action to differentiate themselves from their partners in government. This motivation has been mentioned before, notably in Martin and Vanberg’s (2005, 20011) work on legislative behavior in coalitional systems. In the current literature, it is believed that cabinet parties must do their best to signal to supporters that they are pursuing their core policy positions, and that this is typically accomplished through ministerial drift — the act of a single minister attempting to pull policy toward the ideal point of his party at the expense of proximity to coalition bargain. In the Martin and Vanberg model, this drift increases as the dissimilarity of policy
preferences within the cabinet increases and the behavior we observe in the legislative review process (amendments) is in response to this drift.

The theory and findings here suggest a different mechanism for differentiation. These results imply that cabinet parties have a substantial interest to differentiate when voters perceive them as growing more similar, even when real differences in policy preferences are absent. That is, ministers are motivated to drift and non-ministers are motivated to amend, and all parties are motivated to argue on chamber floor when perceive voters their positions as similar, though not necessarily when their real positions are dissimilar. Thus, the behavior that we actually observe in the policy-making process is more likely to be a product of posturing and signaling for voters than it is a product of dissimilarity in policy preferences.
Chapter 4

Legislative Review and Party Differentiation

“What works in real life is cooperation, what works in politics is conflict.”

—Bill Clinton

Introduction

In September of 2011, the ailing Liberal Democrats limped into their annual conference to discuss their new political strategy. In the year and half since joining the Tories in Britain’s first formal coalition government since the second World War their popularity had been nearly halved and it was time for a change.

“On becoming deputy prime minister last year, Nick Clegg implored the Lib Dems to ‘own’ everything the coalition did. Only then would the party get any credit if the government, led by David Cameron, the Tory prime minister, proved successful. That strategy has in recent months given way to one of conspicuous ‘differentiation’. Some of this is a calculated wooing of left-leaning voters who deserted the Lib Dems in protest at their collaboration with the Tories.”

Clegg and the Liberal Democrats had spent the last year learning what many of their continental counterparts have long known: cooperation with one’s coalition partners can be vital to the success of policy-making and credit claiming on popular policies, but may also be quite costly at the polls. Voters do not lend their support to a party so that it may accommodate its partners in coalition, trade away its core policy positions in order to obtain a fancy office, make the process of governance smoother, or, in the case of Clegg and the Liberal Democrats, secure a referendum seeking an unpopular change to the electoral system that they believed would benefit them. In the previous chapter I argued that voters support a party to pursue a certain set of policies, and, when they believe that the party has not rigorously fought for these policies, they are likely to abandon it.

This puts coalition parties in an awkward position. On the one hand, there are myriad benefits to cooperating with one’s partners in governance. By cooperating, a cabinet party facilitates more efficient governance, finds it easier to extract benefits of office, encourages reciprocal behaviors in its partners, and signals to potential future formateurs that it is an attractive party to coalesce with. Yet, for all of the benefits, there is a penalty: the alienation of supporters, and thus electoral losses.

In this essay, I explore this dilemma in detail. Building on recent research in the comparative literature on legislative institutions and mass behavior, I argue that the electoral losses parties risk by being perceived as too cooperative are real. I then propose that, in response to this risk, parties have substantial motivation to adopt a strategy of differentiation, like Clegg and his Liberal Democrats, and discuss how cabinet parties may differentiate from their coalition partners within the institutional confines of coalition policy-making. As will become more clear below, the
rules and norms of coalition governance constrain parties to a small window in the policy-making process — the period of legislative review — in which they are able to differentiate from their partners without risking dismissal from cabinet violating collective responsibility.

I then present a framework for how the perceptions of voters condition the legislative behavior of cabinet parties in the review phase of policy-making and derive hypotheses to test this theory. These hypotheses are tested with original data incorporating information on legislative amendments and voter perceptions of cabinet parties in three industrialized countries with long histories of coalition governance: Belgium, Denmark, and the Netherlands. I find that cabinet parties that are perceived as more cooperative by voters are more likely to differentiate in the policy-making process by amending each other’s legislative proposals more often. In so doing, I contribute to two related, but largely separate comparative literatures. I demonstrate empirically that parties act with an eye toward elections in the time between the birth and death of coalition governments, that these behaviors have real effects on policy outcomes, and that considering differences in policy preferences alone is insufficient to understand this behavior. These are findings that appeal to the coalition literature, one that is typically more institutionally focused. Further, I demonstrate, for the first time, a concrete empirical link between voter perceptions and coalition policy-making by placing voters directly into an empirical model of legislative behavior in the coalitional context. This is a finding that reinforces our beliefs that the electoral connection affects the behavior of government parties and should appeal to behaviorists.
Compromise and its costs

Coalition governance requires compromise. As mentioned above, this compromise can foster several preferred outcomes for cabinet parties. Many of these benefits are intuitive. Cabinet parties that are cooperative with their partners will find the policymaking process more easy and efficient. As Martin and Vanberg (2004) note, each day a policy is delayed is a day where its benefits may not be enjoyed. A cooperative, efficient coalition is able to maximize the benefits it derives from its policies in a way that inefficient coalitions cannot. Further, parties that are in sync and not endlessly squabbling over state resources may dole out benefits of office more easily as well, for example, an attractive cabinet post or a fancy office. Finally, a cabinet party that is cooperative in legislative review, and allows the proposals of its partners to pass through the scrutiny process unmolested, may be much more likely to find their own policies receiving similar treatment.

There are benefits of cooperation that are less intuitive, as well, particularly in signaling. Coalition governance is a repeated game. There are several coalitions in several states that seem to form over and over again, while there are also collections of parties that seem to form relatively new coalitions at every episode. To the extent that a party hopes to govern with its current partners in the future, it is motivated to reaffirm its willingness and ability to compromise. To the extent that a party believes its current coalition is doomed, it may still wish to cooperate in order to signal potential future formateurs or junior members that it is a desirable coalition partner. After all, a partner that continually flaunts the coalition bargain in its policy proposals or is regularly overtly confrontational in the review process may be
very likely to find itself in opposition after the next formation episode. Recall the finding by Martin and Stevenson (2010) that coalitions that terminate under friendly conditions are substantially more likely to form again.

These benefits from compromise, however, are not without cost. Powell (2000) tells us that voters understand that cabinet inclusion guarantees their selected party’s ability to negotiate on behalf of their policy interests, yet these same negotiation processes inhibit voters’ abilities to accurately allocate responsibility for policy outcomes. Stated alternatively, coalition participation provides parties the opportunity to secure their preferred policies and other benefits while simultaneously obscuring their strategically selected ideological positions. I provided empirical support for this assertion in Chapter 2. They suggest that voters utilize coalition participation as a cue that parties have moderated their policy positions toward their partners in government. Thus, ceteris paribus, voters will perceive coalition parties, in terms of their policy position, as converging on each other.

This perception is problematic. Many scholars have argued that parties develop their policy positions strategically to maximize their expected electoral returns (Adams and Merrill 1999; Adams 2001; Schofield 2004; Laver 2005). These works show that parties shift policy position in response to changes in public opinion (Adams et al. 2004), past electoral results (Somer-Topcu 2009), and quality of valence image (Schofield 2004; Adams and Merrill 2009). If elections are, as Powell (2000) suggests, voter mandates based on parties’ selected policy positions, then it is in the best interest of parties to ensure that their policy positions are correctly perceived by voters. However, as Martin and Vanberg point out, a key dilemma facing coalition partners is “reconciling the tension between the need to compromise on policy with
the need to maintain the party’s public profile with respect to certain policy commit-
ments” (2008: 502-503). Such arguments lead to the conclusion that parties would be motivated to differentiate from their partners simply to protect the integrity of their strategically selected policy positions in the eyes of their supporters.

Compromise, however, can be costly even if we assume away its disturbances to the parties’ ideological positions. Take the example of Britain’s Tory-Liberal Democrat coalition. The Liberal Democrats were able to secure a referendum proposing a change to the electoral system that, they presumed, would substantially increase their expected number of parliamentary seats.\(^1\) The widespread expectation, in the event that the referendum passed, was that these Liberal Democrat gains would largely come at the expense of Labour and the Tories.\(^2\) Why would the Tories consent to this proposal? The answer is that they were able to extract several policy concessions from the Liberal Democrats. The situation seems mutually beneficial. Unfortunately, one of these concessions included allowing universities to increase their tuition fees by nearly 300\%, a concession the Liberal Democrats pledged never to make\(^3\) and one that enraged their supporters (Bagehot 2011).

Compromises of this type are common occurrences in coalition government. Further, compromises as self-serving as these (though typically not as transparent) are common as well. Martin and Vanberg note that,

“party leaders consume additional benefits of holding government office

— prestige, ministerial salaries, a fancy office. . . Consequently, leaders are

\(^1\)This change was the institution of the alternative vote.
\(^2\)Analyses by voting scholars, however, discount the magnitude of these gains. See, for example, Aldrich et al. 2011.
\(^3\)See the 2010 Liberal Democrat Manifesto.
likely to be more eager to join a cabinet than their followers...” (2011, 12)

That is, party leaders have various incentives to compromise on behalf of their supporters, but they also have incentives to compromise at the expense of their supporters. In the case of the Liberal Democrats, the latter was a common perception, as recently noted by Economist opinion columnist “Bagehot.”

“At previous low points, the party’s big problem was irrelevance. They have a wholly new problem now. Lots of voters hate them, and think they have sold out for a perch in a ministerial Jaguar.” (September 24, 2011, 70)

In the last chapter, I proposed an emotional mechanism through which perceived compromise or accommodation between coalition partners could exact electoral costs on the cabinet even after controlling for how these perceptions shape the way voters view parties spatially. This framework requires little of voters in terms of sophistication and political knowledge. They need only have expectations for how their cabinet will behave and perceptions of how their cabinet has behaved. More specifically, I argue that voters will expect to perceive conflict in the policy-making process — as this is indicative of their parties vehemently pursuing their core policies — but are more likely to perceive cooperation. This perceived cooperation, a perception that is discordant with expectations, makes previous supporters less likely to support the cabinet in future elections. Therefore, parties have two sources of concern stemming from the perception of cooperation: obscured policy positions and the appearance of selling out.
Differentiation in coalition governments

The need to differentiate has powerful implications for legislative behavior. Parties must be able to signal to voters that they are “fighting the good fight” in government and doing their best to represent the preferences of their supporters by differentiating themselves from their partners. Indeed, this may become key to remaining in government after the next election. But cabinet parties face several obstacles in differentiating. First, parties cannot differentiate on issues that voters do not care about. For example, it makes little sense for a cabinet member to demonstrate that their views on international security are different from their partner’s during peace time, just as discussions over how to handle unemployment would be irrelevant if the unemployment rate were 0. These are extreme examples, but the intuition is sound. Voters should only care about policy differences when these differences are relevant.

These saliency constraints exist in all states. In the United States, for example, a Congressional minority can only credibly criticize the majority’s stance on relevant issues that reach the floor for debate. The contexts of coalition governance, however, create even more problems for parties needing to differentiate. Even when the issues of debate are defined, the venues for debate within coalition are even more scarce than debate between cabinet and opposition. This is because coalition members are constrained by collective cabinet responsibility, a set of norms that determine the parameters of behavior for cabinet members (Laver and Shepsle 1996). Collective responsibility inhibits coalition parties from, for example, voting against proposals offered by their partners or speaking out against the proposal once it has been passed into policy. The penalty for violating these norms may be the loss of portfolio,
dismissal from cabinet, or even the loss of confidence resulting in a new government formation episode.

Taking agenda constraints and collective responsibility together, coalition parties have precious few opportunities to differentiate from their partners. Parties must have a proposal over which to squabble and they must be able to express themselves before the proposal’s formal vote. Thus, cabinet parties are constrained to the legislative review process, the window of time between a bill’s initiation and passage (or death) during which the proposal may be scrutinized. In practice, this leaves two legislative outlets in which cabinet parties may demonstrate how they differ from their coalition partners: legislative amendments and parliamentary debate.

In this essay, I examine legislative amendments. Amendments, by their very nature, are expressions of dissent. They may only be offered to an initiated proposal and are therefore germane to the agenda. Further, amendments are costly. They require expertise, time, and labor to draft. For these reasons, and because parties attempt to claim credit on the proposals they offer (Martin and Vanberg 2011), we may consider amendments credible differentiating signals sent by the reviewing party in reference to the party that initiated the bill.

**When will parties amend?**

The balance between the need to compromise with coalition partners to maximize office and policy benefits and the need to maintain a distinct policy platform and the faith of supporters is a precarious one. I have discussed the benefits of cooperation as well as the costs that foster the need to differentiate, but at what point are the benefits
of cooperation outweighed by the benefits of differentiation? The discussion above suggests that parties will be more likely to differentiate if voters perceive them as more similar to their partners. Thus, the naïve expectation is that increases in the perceived similarity of a pair will increase the amount of observed amendment activity between the two. This leads to the primary hypothesis.

- **Perception Hypothesis:** *The more similar a party pair is perceived, the greater their amendment activity on one another’s proposals.*

This hypothesis is the focus of the essay, however, consideration of this hypothesis in a vacuum discounts the costs of amending legislation. Indeed one of the justifications for examining amendments is that they are costly and therefore likely to send credible signals of the reviewing party’s discontent with the proposal in question. Therefore, it is important to consider factors that may condition the costs of review or otherwise affect the likelihood that amendments are observed.

For parties endowed with ministerial portfolios, differentiation is accomplished through “ministerial drift,” the act of purposefully defecting from the policy position of the coalition bargain (Huber and Shipan 2002; Indridason and Kam 2008). By offering a policy that disregards the coalition agreement the minister signals to her party supporters that she is both sensitive to their policy demands and sufficiently competent to flaunt the constraints of her coalition partners. These proposals leverage policy costs on the minister’s coalition partners, thus increasing the propensity of partners to amend (and Martin and Vanberg 2005, 2011). Because the focus of this essay is legislative review in response to voter perceptions, it is important to control for effects of differences in policy preferences, i.e., statistically parse amendments
meant to correct ministerial drift from amendments meant as signals to supporters for the express purpose of differentiation. Thus, in the analysis to follow, I control for the level of policy position dissimilarity between the party pair.

Previous research suggests that it is important to consider committee chairmanships. The literature on parliamentary committees suggests that holding a committee chair substantially reduces the costs of legislative review. Döring (2001) discusses the substantial agenda-setting powers committee chairs may control. Strøm (1998) suggests that committee chairs enjoy benefits such as “expert staff assistance” that can substantially reduce informational and transaction costs in the review process. Indeed, more recent works by Carroll and Cox (2011) and Kim and Loewenberg (2005) suggest that holding a committee chair is the most efficient way for cabinet parties to mitigate the ministerial drift of their partners. Indeed, Fortunato et al. (2009) provide empirical evidence that bills referred to committees chaired by the proposer’s coalition partner receive greater scrutiny. This study, however, is conducted at the coalition level, thus including amendments offered by all parties, and not simply the focal party as is the focus here.

This distinction is important because committee chairmanship conditions our expectations for review differently in the dyadic set up employed here than it does in the coalition-level set up used in previous work. The results presented by Fortunato et al. (2009) may be the result of shadow committee chairs (i.e., chairs controlled by a coalition partner of the initiating minister) amending proposals more, or of committee chairs that are aligned with the initiating minister somehow protecting the bill. The dynamics of this relationship change dramatically in the dyadic framework.
Assume that the committee chair has three options: 1) allow the bill to pass through committee as is; 2) allow the bill to pass through committee after amending; and 3) shelve the bill, effectively taking it off the agenda.\footnote{The explicit powers of the chair vary across chambers, however, Döring (2001) suggests that all chairs in strong committee systems possess strong scheduling power and notes, for example, that the majority of Italian legislation that fails, does so because the committee chair keeps it from reaching the floor.} If ministers are able to credit-claim on the policy they initiate, despite the fact that it may have passed in altered form or not at all, then it follows that committee chairs may claim credit on bills they shelve and do not necessarily amend.\footnote{This is not to say that shelving a bill is just as visible as amending it. This relationship merely assumes that legislative actions that do not lead to policies being passed are observable.} In this case, the expectation would be that we will observe few amendments when the reviewing party controls the committee chair, as it may simply shelve the bill to differentiate, rather than expending the resources to amend it. Conversely, if credit claiming may only occur in the event that the bill reaches the plenary for debate, then we would expect to observe more amendments when the reviewing controls the chair as it may amend at a comparatively lower cost relative to reviewing parties that do not hold the chair. Whether the former to latter is the case, it is important to control for whether or not the amending party controls the committee chair.

The costs of review are not only conditioned by whether or not a party holds the chair. Amendments require expertise and labor. Someone must interpret the initial proposal, identify the most salient points to take issue with, and then write and submit the amendments. Having more members in parliament, specifically more members on the committee in question, would substantially decrease the reviewing party’s amendment costs. A greater proportion of the seats means more expertise and
more members to allocate review responsibilities to. Thus, I expect that amendment activity will increase with the seat share of the reviewing party. As committee seats are, by rule, allocated proportionately in all of the chambers I consider here, I simply utilize the reviewing party’s parliamentary seat share as the operational variable.

Finally, it is probable that contexts of the cabinet beyond voter preferences may condition the value of differentiation. Borrowing from the literature on economic voting (Duch and Stevenson 2008; Powell and Whitten 1993), one would expect that parties’ incentives to respond to voters would be conditioned by the extent to which voters are able to attribute responsibility for actions to individual parties. That is, the likelihood of a differentiating signal being carried from the parliament to the voter, and thus the benefit of differentiation, is conditioned by the overall complexity of the policy-making environment. Specifically, we would expect that parties would derive more benefit from differentiation when it is easier for voters to observe this behavior; when the clarity of responsibility is high. The more players in the legislative game, the lesser the clarity of responsibility, and the noisier the signals of any given party’s legislative behavior.

Data

Testing these hypotheses requires data on amendments offered in the legislative review process as well as data measuring the voter perceptions that motivate the decision to amend. I discuss the former first.

To construct the dependent variable I assemble original data. I gather information on a selection of bills offered in the year before, year of, and year after the adminis-
tration of electoral surveys (explained below) in the lower houses of three European states: Belgium, Denmark, and the Netherlands. Data collection is focused on one year intervals about the administration of electoral surveys is because voter perceptions are prone to change. Assuming that the perceptions held at the time of survey administration are stable through a five year legislative term in unreasonable.

These three countries make for an ideal sample. Unlike the United Kingdom, a country still struggling to learn the ropes of coalition governance, these countries are well acquainted with coalition governance. Indeed, Denmark has not had a single party government in over thirty years, Belgium has been continually governed by coalitions for over fifty years, and the Netherlands has not had a single party majority since the Liberal Union of the 1890’s.\(^6\) In addition, each of these countries has a strong committee system that allows for the submission of amendments to cabinet proposals to be made, and subsequently credited to, individual parties. These countries are quite institutionally similar to several European countries at varying stages of development. The behaviors I uncover here are theoretically generalizable to Italy before its convergence to a (de facto) two party system, the UK if coaltional governance continues, as well as the European Union, the Czech Republic, Finland, Iceland, Luxembourg, and Sweden, and a host of developing Central and Eastern European democracies. Conversely, there are, for example, states with equally strong committee systems that do not produce a party-specific record of legislative amendments. They typically choose to credit an amendment to the committee as a whole, as in Germany, or to the “majority” or “minority,” as in Norway. In states such
\(^6\)It should be noted that there have been instances of single party caretaker governments that shepherded the state from a premature cabinet dissolution into the next election.
as these, parties must find alternate media through which to differentiate, such as parliamentary debate.

The data consists of a random sample of 35% of the cabinet bills dealing with issues of governmental spending and general socio-economic policy that were proposed in my temporal range.\(^7\) The intuition behind concentrating on such legislation is simple. I am interested in how voter perceptions drive amendment behavior. Voter perceptions are nearly always thought of as unidimensional and the policy areas to which that singular dimension conforms most easily are those that concern taxing and spending. Therefore, I want to examine legislation concerning more generalized left-right policy and avoid policies that may have divide parties or the electorate on orthogonal dimensions. Thus, economic legislation is the point of focus, while bills concerning foreign affairs or similar issues are discarded. This strategy has the added benefit of avoiding potential bias arising from differing issue saliencies across policy areas.

For each bill in the data I record the number of articles in the original proposal, the initiating party, and the number of article changes offered to the bill in the review process by all coalition partners save the party that introduced the legislation.\(^8\) Recording the data in this manner is critical. Previous studies of legislative review in parliamentary democracies (Fortunato et al. 2009; Martin and Vanberg 2005, 2011) have utilized data with article changes coded in the aggregate. That is, the dependent variable is the total number of amendments offered by all parties. Only by coding

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\(^7\)This excludes national budget legislation as these bills are subject to special amendment rules (Martin and Vanberg 2005).

\(^8\)This means that amendments that are co-sponsored by the party of the initiating minister are not counted.
changes at the party level am I able to test the hypotheses presented above. In addition to noting the origin, length, and level of scrutiny for the bills, I also note the party controlling the committee reviewing the bill and the number of parties in the cabinet.

The number of article changes on a bill by a given party is coded as the total number of article changes across all amendments submitted by that party. If, for example, the CDA offered three amendments to a PVDA proposal, with each amendment changing two, three, and two articles, respectively, the number of article changes recorded in the data would be seven. This coding was modeled after Martin and Vanberg (2005, 2011) with the exception that amendments are attributed to their individual proposers rather than summed across the parliament. Summary statistics for the dependent variable, the number of article changes offered by a given party on a given bill, are displayed in Table 4.1. Notice that the data contain many cases in which no amendments are offered — these 0 counts present an interesting methodological hurdle which I will discuss below. Notice also that the standard deviation of the dependent shows that there is a substantial amount of variation across the sample to explain. This variation implies that parties make decisions in context. If nearly every bill went unamended, we may conclude that amendments that have little policy affect or that they are a useless signaling device. Conversely, if every bill received many amendments, we may conclude that parties simply squabble at all points of the policy-making process and that they never sought to capture the benefits of cooperation discussed above. This level of variation is encouraging.

For the independent variable of interest to the main hypothesis I use data from election year EES and CSES surveys conducted from 1994 to 2004 in Belgium, Den-
mark, and the Netherlands — a subset of the same data used in Chapter 2 — where respondents were asked to place the parties of their country on an ideological scale (my data includes surveys conducted in Belgium in 1994, in Denmark in 1999 and 2004, and in the Netherlands in 1994). With these data I construct a measure of the absolute distance in perceived policy difference for each coalition partner dyad. The greater the distance, the more dissimilar voters believe the cabinet parties are. The smaller the distance, the more similar the parties are perceived and the greater their incentive for differentiation. Thus, the expectation is that the parameter estimate on voter distance will be negative. Descriptive statistics for this variable are shown in Table 4.1.

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<tr>
<th></th>
<th>Voter Distance</th>
<th>CMP Distance</th>
</tr>
</thead>
<tbody>
<tr>
<td>Median</td>
<td>0.33</td>
<td>0.95</td>
</tr>
<tr>
<td>Mean</td>
<td>1.17</td>
<td>0.96</td>
</tr>
<tr>
<td>SD</td>
<td>1.13</td>
<td>0.67</td>
</tr>
</tbody>
</table>

Finally, I must include a measurement of policy preference dissimilarity in order to control for preference dissimilarity. To this end I turn to the comparative manifestos project and create the same measure of cabinet party distance described above using CMP position estimates in the place of voters’ perceived distance. The expectation is that the more dissimilar the policy preferences of the party pair, the greater the amending activity. Thus, I expect the parameter estimate on this term to be positive. Descriptive statistics for this variable are also given in Table 4.1.
Analysis and findings

Model specification

The dependent variable is the number of article changes offered by some cabinet party on a given bill initiated by their coalition partner. The vector of regressors are selected according the theory laid out above and the hypotheses derived from it. To address my central hypothesis, the independent variable of interest is voters’ perceived distance between the minister and reviewing party (voter distance). In addition, I include several control terms; the distance between the manifesto positions of the two parties, whether or not the reviewing party holds the chair of the committee reviewing the bill, the percentage of seats controlled by the reviewing party, and the total number of parties in the coalition. Also, as a short hand control for institutional variation across the three countries, the model includes fixed effects on indicator variables for Belgium and Denmark (with the Netherlands serving as the baseline). Finally, I include the (logged) total number articles in the original bill. This is an exposure variable. More articles in the original bill means more articles that could potentially be amended in review. Further, the number of articles in the original bill is an artificial constraint on the number of amendments that may be observed that violates the underlying boundedness assumptions of the distribution I will use to model the data. Failing to account for this could induce bias to the parameter estimates.

Model selection and interpretation

Because the dependent variable (the number of article changes offered by a given party on a proposal) is a count, an event count model is most appropriate. I use a
count model utilizing the negative binomial distribution rather than the poisson for the stochastic portion. This distribution frees the model of the more constraining assumptions of the poisson distribution. Specifically, the negative binomial distribution does not assume a variance equal to the mean (after conditioning on the covariates) and, more importantly, does not assume conditional independence of events within observations. This means that the model does not assume the change from 0 to 1 amendments is the same as the change from 5 to 6 amendments (King 1988). Modeling the stochastic process with this distribution places more computational demand on the data (reduces the likelihood of finding statistically significant relationships), however, given the structure of the data and my beliefs about the data generating process (specifically, that the initiation of one amendment makes the initiation of a second more likely), this is the more sound choice.

The standard negative binomial model, however, is still an inappropriate for this data. Because there is an over-dispersion of zeroes in the data I consider a hurdle model (Zeileis et al. 2008). I perform both Vuong and Akaike Information Criterion tests to determine which model best fits the data.\footnote{Model comparison is given in Table 5.3 in the appendix.} The Vuong test reveals that the hurdle model provides better fit with $p < 0.001$. As for the AIC, the standard negative binomial regression reports an AIC of 630.494 and the hurdle model reports an AIC of 594.701, suggesting that the hurdle model provides a better fit the data.

The hurdle negative binomial model can be conceptualized as estimating a nested model. First, a logit model regressing a vector of independent variables on the dependent variable is estimated to determine the likelihood of observing a non-zero count where, in this case, $y = 1$ if there is at least one amendment offered and 0 if other-
wise. If \( y = 0 \), the model terminates. If, however, \( y = 1 \), the model continues and estimates a negative binomial regression. These models are considered discreet. That is, covariance between parameters in hurdle portion and the count portion is assumed to be 0. This means that altering the specification of one portion of the model will not change the estimates recovered from the other portion of the model. Each coefficient from each set of parameter estimates may be interpreted intuitively. Positive parameters indicate that more amendments will be realized at higher values of the corresponding variable. Table 4.2 below shows the model results with statistically significant relationships in the predicted direction highlighted.

The substantive effects of the model estimates must be interpreted jointly. That is, the predicted number of amendments for some set of values of the independent variables is equivalent to the probability derived from the hurdle portion of the model, multiplied by the predicted number of amendments from the count portion. However, we can see that the terms corresponding to four of the five hypotheses each realize a statistically significant parameter estimate in the predicted direction in one portion of the model and this is not counterbalanced by significant parameter in the opposite direction in the adjoining portion of the model. The one exception to this is the CMP distance covariate. The data suggest that parties are less motivated by policy differences than they are by voter perceptions. Interestingly, when voter perceptions are omitted, the CMP distance performs as we would expect given previous research. The variable is positive and quite significant statistically. This means that the data analyzed here generally conform to the expectations of previous research, voter perceptions simply seem to be more salient in conditioning the behaviors of parties than differences in their policy preferences.
Voter distance and coalition size have the predicted relationship in the count portion of the model, with highly insignificant effects in the hurdle portion. Likewise, the committee chair and seat share parameters have the predicted relationship in the hurdle portion, with highly insignificant effects in the count portion. It should be noted that in a traditional count model framework, each of these parameters take on their predicted relationship (these results are reported in the appendix). This model puts greater strain on the data and thus biases against finding substantively
significant relationships, however, it provides significantly better fit for the data.

Because we have to consider the effects of the covariates on both the probability of amending and the number of amendments, the simplest way to interpret the model is by examining the substantive effect of covariate changes on the predicted number of amendments. Following King et al. (2000), I simply generate a distribution of parameter estimates by taking 10,000 random draws from a multivariate normal distribution with mean equivalent to the parameter estimates and variance equivalent to the variance-covariance matrix of the model. I then use the distribution of parameter draws to generate the predicted values. I begin with the focus of the essay, voter perceptions. From each parameter draw, I generate three predicted values: one holding each independent variable constant at its mean, one increasing the voters’ perceived distance between the minister and reviewer by one standard deviation, and one decreasing voter distance by one standard deviation. Figure 4.1 shows the effect of a first difference increase or reduction from the mean perceived distance. Note that the bean plots show the full distribution of predicted values with the thicker blue line representing the mean effect and the thinner lines representing the 95% confidence interval of these effects. Note that more than 95% of both distributions are on the correct side of the red 0 line.

A first difference decrease in voters’ perceived distance between the minister and reviewer will increase the predicted number of amendments by about 120% (from 0.40 to 0.89). Similarly, a first difference increase from the mean reduces the number of predicted amendments by over 50% (from 0.40 to 0.20). These are significant effects. Given that the model finds no effects for preference dissimilarity, but substantial evidence for the perception hypothesis, it is safe to say that parties amend with
Figure 4.1: The substantive effect of perceived voter distance.

The Effect of First Difference Change in Perceived Similarity on the Predicted Number of Article Changes

Change in Predicted Article Changes
Change to Perceived Dyad Distance from the Mean

−1 Standard Deviation + 1 Standard Deviation

95% confidence

their eyes set directly on the electorate, giving them the conflict they expect, rather than hoping that the electorate will reward them for controlling the policies of their partners.

The effects of controlling a committee chair are examined as above. All else equal, the predicted number of amendments is reduced by over two thirds, falling from about 0.51 to about 0.17 when the reviewing party holds the committee chair. Again, a substantial effect, and an interesting one at that. Recall from the discussion above that there were two competing expectations for the effect of a committee chairmanship. Given that parties could credit claim on delaying or even shelving a bill, we would expect fewer amendments given a committee chair. Given that parties could credit
claim only on behaviors once the bill reached review, we would expect more amendments given a committee chair. These data suggest that the former is true; that the reviewing party is able to extract value from delaying or shelving a bill. However, it would require further analysis to confirm this. Specifically, one would need to analyze the duration of review or whether or not the bill was passed or died given that the reviewing party held a committee chair. This is beyond the scope of this study. Further, there may be confounding factors at work. For example, it may be that the minister, observing a hostile committee, chooses to introduce a bill that is more pleasing to the chair, hence trading off some policy benefits to avoid the embarrassment of getting his bill killed. Again, more research is needed.

The remaining control variables conform to expectations. The more MPs a party has to gather expertise, read bills and forecast their policy ramifications, write, submit, and argue in favor of amendments, the more amendments will be submitted. If the reviewing party’s seat share increases from, say 10% to 20%, its predicted number of amendments increases about 50% (from 0.31 to 0.46). Increases in coalition size decrease the number of predicted amendments substantially. In a two party coalition, the mean predicted number of amendments is about 1.81 (with upper and lower bounds of 5.12 and 0.29, respectively). In a four party coalition, that number quickly plummets to 0.10 (with 0.38, 0.01 confidence).

**Discussion**

In January of 2011 the Conservative Minister of Health, Andrew Lansley, introduced the *Health and Social Care Bill*, an initiative that would push the privatization of
the National Health Service, placing ultimate control of health care administration into the hands of clinical commissioning groups. The media described the bill as sufficiently liberal as to, “let Andrew Lansley wash his hands of health service” (Boseley 2011; Main 7). That is, the bill was meant to take accountability for the NHS out of the cabinet’s hands and formally sever the connection between voters and the administration of their health care. After the bill’s submission, the Liberal Democrats embarked on an aggressive, ten month campaign of legislative scrutiny, culminating in a series of amendments that reinstated ultimate accountability for the NHS to the cabinet.

In reference to this campaign of scrutiny, Nick Clegg said that, “[i]n a coalition, we have two kinds of power: The power to hold our coalition partners back and the power to move the government forwards” (2011). When it came to the NHS, the Liberal Democrats went out of their way to demonstrate that they were ideologically distinct from their partners in government, utilizing the powers of legislative review to send these signals to their supporters. Here, we see that, even in the UK, where committee systems have been comparatively weak, and parties are new to coalition governance, the review period provides a powerful medium, indeed, one of the only media, through which cabinet parties are able to differentiate themselves from their coalition partners.

The analysis above makes this point in a more generalized, more robust manner. We learned in the last chapter that cabinet parties lose votes when their supporters do not perceive them as rigorously pursuing their core policy positions. Given that parties are electorally motivated, this penalty imposes a hard decision on cabinet parties; will they continue to defer to partners in order to maximize benefits of office
or policy in other areas (Müller and Strøm 1999), or will they forego these cooperative incentives to differentiate and protect their electoral fortunes? The analysis above implies that the answer is a dynamic one. By examining the behaviors of parties in systems where coalition governance has been the norm for several decades, we uncovered the typical responses to this dilemma, unobscured by the learning curve currently being negotiated in the UK. Parties condition their behaviors on the level of perceived similarity, differentiating more when voters perceive them as more similar to their partners, even when the level of policy conflict is controlled for. As noted above, this implies that parties leverage legislative institutions to condition voter perceptions directly, not merely as a mechanism for mitigating ministerial drift or otherwise policing the coalition bargain, as previous research suggests (Carroll and Cox 2012; Martin and Vanberg 2011).

The argument, supporting analysis, and discussion compliment the existing literature while simultaneously capitalizing on an opportunity in it. Previous studies of legislative behavior in coalditional democracies have addressed the competing incentives of policy and votes, but have failed to adequately model them empirically. Specifically, previous work has used dissimilarities in policy preferences as a proxy for concerns regarding the electorate which conflates these competing incentives, thus muddying the substantive interpretation of results. This essay avoids this pitfall by incorporating measurements for both of these concerns into the empirical model. The data suggest that parties respond to voter perceptions directly and that policy preferences influence behaviors in a separate manner, if they influence behaviors at all in dyadic review interactions. The essay also speaks to previous work by finding support for the notion that legislative institutions are an integral part of the policy-making
process in the coalitional systems, and not merely rubber stamps for ministerial policy. Indeed, the arguments and suggestions of several scholars regarding the power of legislative institutions are supported here (for example, Cox and Carroll 2012; Kim and Loewenberg 2005; Martin and Vanberg 2011; Thies 2001).

There is a more general contribution of this essay. Over the past decade, coalition scholarship has evolved to recognize the importance of legislative institutions, moving beyond the ministerial autonomy model of coalition governance as noted above. That is, scholars have developed a keen interest in studying coalition governments between formation episodes rather than assuming that policy outputs could be perfectly predicted by the allocation of ministerial portfolios. This essay not only takes a valuable next step in that direction, but also brings to light a heretofore unexamined component of the coalition game: voters. I find that fluctuations in voter perceptions (and therefore, expected electoral outcomes) exert a pronounced effect on legislative behavior, even when differences in policy preferences are accounted for. Consider the implications of this finding. If voter perceptions of party policy position play such an instrumental role in the legislative review process, how else may fluctuations in voter sentiment condition the behavior of political parties in coalitional systems?

The evidence above suggests that voter sentiment may play a much more immediate role in legislative outcomes than previously suspected. It seems quite reasonable that fluctuations in voter perceptions could condition everything from government formation and dissolution, to parliamentary speech or the calling of special hearings. Indeed, while most formal models of coalition politics point to a direct connection between voters and parties, this connection has often been absent or loosely proxied in empirical research, thus creating an opportunity in the literature.
Finally, these results present some normative concerns. Recall the Bill Clinton quote that began the essay, “[w]hat works in real life is cooperation, what works in politics is conflict” (2011). This was in reference to the bickering that caused Congressional gridlock and inability to efficiently tend to America’s economic crisis. In other words, good politics does not necessary lead to good governance. It is normatively desirable for parties to respond to voters, but the type of response that this article examined may do more harm than good. What is the degree to which parties are sacrificing efficiency and productivity in governance in order to stage the conflict that voters expect? What cost is this behavior exacting on the welfare of the public? Currently, the answers to these questions are unclear. More research is needed to understand if this particular type of responsiveness to shifts in public opinion degrades the quality of governance, and, if the answer is ‘yes,’ what can be done remedy the issue.
The dissertation opened with a brief discussion of where the study of coalition governance needs to go in order to advance our understanding of democratic accountability such systems — the majority of the world’s democracies. In sum, in researching coalition cabinets, we have overwhelming focused on the birth and death of governments and, in researching coalitional voters, we have focused on how factors unrelated to coalition governance condition vote choice. That is, the majority of our research on coalitional voters has ignored the structure of their political institutions and the majority of our research on coalitional governments has excluded the processes of governance. Further, the connection between legislators and voters had yet to be satisfactorily modeled empirically.

A brief recap

The preceding three chapters have taken important steps toward filling the gap in the literature. I concentrated on a single, central aspect of the coalitional voter — his perceptions of the policy positions of political parties — and studied how coalition policy-making alter these perceptions, how these perception changes may prove electorally costly for governing parties, and, finally, how parties react to these perceptions in the legislature.
We have learned that voters tend to interpret the formation of coalition cabinets as the formalization of a broad and wide-ranging policy compromise. This perception of compromise can be costly to parties. If voters believe that cabinet parties are too eager to compromise with, or defer to, their coalition partners, then the cabinet is likely to lose votes — a perception I found to be pervasive across sixteen European democracies. Indeed, the vote loss resulting from these perceptions is commensurate with Stevenson’s (2002) estimation of the “cost of ruling,” the expected vote loss for incumbent cabinets can expect after economic outcomes and political institutions are accounted for; the baseline expectation for an incumbent cabinet’s electoral performance. The findings in Chapter 3 imply that what political scientists had failed to understand about democratic accountability in coalitional democracies were the potential costs of coalitional policy-making.

In Chapter 4 I turned to legislature and investigated how parties may attempt to mitigate these expected electoral losses. I found that coalition partners will amend each other’s legislation more freely when they are perceived as converging in their policy positions. That is, parties will use the process of legislative review to differentiate themselves, to attempt to signal voters that their policy positions are distinct and that they are going to the wall on behalf of their supporters’ interests.

**The general contribution**

The preceding essays have had several “firsts” for coalitional research. In Chapter 2, I presented one of the first models of voter perceptions of political parties built upon heuristics. I believe that this will prove the defining characteristic of comparative
behavioral research in the coming decade. This is a move that is long overdue. The tradition of the spatial competition literature, research dedicated to analyzing how voters react to the policy positions of parties, has implicitly assumed that voters are perfectly attentive to policy pronouncements, acting as if all voters read each and every party manifesto and update their perceptions accordingly. This is assumption is, at best, a stretch. As such, the model presented in Chapter 2 represents a valuable move forward in comparative behavioral theory as it requires very little of voters in terms of real information, but respects that voters do have some information (specifically, the composition of the cabinet) and that there is variation among voters in terms of receptiveness to informational cues and reliance on simplifying heuristics.

Chapter 3 is also makes several general contributions. This is the first model of vote choice to factor in the role of coalition policy-making. Also, the micro foundations of the model are relatively generous to the voters. I do not assume that voters are perfectly informed, or even attentive. Voters are assumed to possess only three items: knowledge of which parties are in cabinet, expectations for how those parties will behave, and perceptions of how they have behaved. This is a much more reasonable take on voters, especially when compared to the policy-balancing models that have been offered to explained incumbent coalition vote-loss in the past.

In Chapter 4’s essay, I present the first empirical model of coalitional legislative behavior to incorporate voter perceptions directly. In the past models, to the extent that political scientists were interested in voter perceptions/preferences, we have proxied for voter perceptions with measurements of partisan policy preferences. In such cases, partisan preferences play a dual role in the empirical model, serving as both party preferences and supporter perceptions/preferences. This approach assumes that
parties and voters are in perfect agreement over where parties are or should be located and that parties are perfect delegates of their supporters.

These assumptions are simply not realistic. Even if we are to believe that parties are perfect agents (and they certainly are not), the disconnect between voter perceptions and party pronouncements is well documented (e.g., Adams et al. 2011). In my model, party preferences and voter perceptions are given unique measures and the data suggest that parties are more concerned with controlling how they are perceived than acting upon differences in their policy positions in the review phase of policy-making. I believe that the future of coalitional legislative research lies in uncovering the relationship between legislators and voters and have provided a small but important first step here.

Where do we go from here?

Instead of attempting a sweeping, grand theory of democratic accountability in coalitional democracy, this dissertation took a more manageable approach to pushing coalition research forward by tracing voter perceptions of political parties: how they are updated, what their electoral implications are, and how they condition legislative behavior in a particular portion of the policy-making process. Though I believe that these essays will prove an important contribution, they also lead to several more questions. Does the role of voter perceptions in legislative review transfer to other parts of the policy-making process such as the formation of the legislative agenda or legislative debate? How are these behaviors conditioned by institutional structures such electoral systems, bicameralism, pre-electoral coalitions, or presidents?
The manner in which voters perceive political parties is just a small part of the interaction between parties and voters. Future research will need to investigate more rigorously the connection at every stage of the political process. For example, we know precious little about how voters view coalition formation or what they know about it. We simply do not understand the degree to which voters are able to make sense of post-electoral bargaining and we are therefore incapable of inferring the degree to which they are able to factor such processes into their voting decisions.

The problem becomes more clear when we consider some substantively interesting questions about voters. Take for example, the notion that voters reward and punish parties for decisions regarding the formation and dissolution of cabinets. It is intuitive to believe that voters may punish a preferred party for coalescing with a hated party, or that voters may reward a particular party for terminating an unsuccessful or unpopular coalition. At present, these intuitions are not testable because we have yet to uncover just how well voters understand these processes or how closely they monitor them.

Without fully understanding the parameters of vote choice and varying levels of voter knowledge we can not understand how parties play the coalition game. Just as certainly as there electoral repercussions for coalition policy-making, there must be electoral implications for the formation and termination of coalition cabinets. I have uncovered just one small part of how voters react to coalitional policy-making, but we know little more about how coalitional voters respond to coalition politics. I believe that behavioral and institutional coalition research will only be able to move forward by bridging the gap between these complimentary but heretofore separate agendas.
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Appendix

Figure 5.1: Distribution of familiarity (from Chapter 2)
Table 5.1: Descriptive statistics by model (from Chapter 2)

Descriptive statistics by model

<table>
<thead>
<tr>
<th>Variable Name</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Dependent Variable</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Voter Perceived Distance</td>
<td>3.077</td>
<td>3.126</td>
<td>3.064</td>
<td>3.124</td>
</tr>
<tr>
<td></td>
<td>(2.399)</td>
<td>(2.413)</td>
<td>(2.351)</td>
<td>(2.380)</td>
</tr>
<tr>
<td><strong>Covariates</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10 Year Average Distance Between Partners</td>
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<td>1.077</td>
<td>1.055</td>
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<tr>
<td></td>
<td>(.820)</td>
<td>(.803)</td>
<td>(.808)</td>
<td>(.814)</td>
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<tr>
<td>Proximate Distance Between Partners</td>
<td>1.060</td>
<td>1.031</td>
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<td>1.053</td>
</tr>
<tr>
<td></td>
<td>(.851)</td>
<td>(.828)</td>
<td>(.808)</td>
<td>(.873)</td>
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<td>.085</td>
<td>.096</td>
<td>.077</td>
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<tr>
<td></td>
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<td>(.279)</td>
<td>(.295)</td>
<td>(.267)</td>
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<td>Opposition Partners</td>
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<td>(.488)</td>
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<td>(.492)</td>
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<td></td>
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<td>Education</td>
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<td></td>
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<td>5.633</td>
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<td></td>
<td></td>
<td></td>
<td>(2.136)</td>
<td>(2.119)</td>
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<td>Between Parties</td>
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<td>.250</td>
<td>.259</td>
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<td></td>
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<td>(.431)</td>
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<td>(.438)</td>
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<td>(1.485)</td>
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<td>Familiarity</td>
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<td>Familiarity &gt; 0</td>
<td>.161</td>
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<td>Telephone Survey</td>
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<td>.498</td>
<td>.471</td>
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<td></td>
<td>(.495)</td>
<td>(.498)</td>
<td>(.500)</td>
<td>(.499)</td>
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<tr>
<td>Self Administered Survey</td>
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<td>.158</td>
<td>.216</td>
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<tr>
<td></td>
<td>(.304)</td>
<td>(.382)</td>
<td>(.365)</td>
<td>(.412)</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>395909</td>
<td>478797</td>
<td>671749</td>
<td>832604</td>
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Table 5.2: Distribution of data over country (from Chapter 2)

Distribution of data over country

<table>
<thead>
<tr>
<th>Country</th>
<th>Survey Year</th>
<th>Percent of Data</th>
</tr>
</thead>
<tbody>
<tr>
<td>Austria</td>
<td>1999, 2004</td>
<td>1.00</td>
</tr>
<tr>
<td>Finland</td>
<td>1999, 2003, 2004</td>
<td>4.43</td>
</tr>
<tr>
<td>Iceland</td>
<td>1999, 2003</td>
<td>1.67</td>
</tr>
<tr>
<td>Italy</td>
<td>1994, 1999, 2004</td>
<td>20.97</td>
</tr>
<tr>
<td>Luxembourg</td>
<td>1994, 1999, 2004</td>
<td>3.00</td>
</tr>
<tr>
<td>Norway</td>
<td>1997, 2001</td>
<td>5.09</td>
</tr>
<tr>
<td>Portugal</td>
<td>1994, 1999, 2004</td>
<td>1.93</td>
</tr>
<tr>
<td>Spain</td>
<td>1994, 1996, 1999</td>
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</tr>
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</table>
Table 5.3: Model comparison: Poisson, negative binomial, and negative binomial hurdle model (from Chapter 4)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Parameter Estimate and Standard Error</th>
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<tbody>
<tr>
<td></td>
<td>Poisson</td>
</tr>
<tr>
<td><strong>Count</strong></td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>1.955 (1.162)</td>
</tr>
<tr>
<td>Voter Distance</td>
<td>-0.523 (0.120)</td>
</tr>
<tr>
<td>CMP Distance</td>
<td>0.477 (0.222)</td>
</tr>
<tr>
<td>Coalition Size</td>
<td>-1.341 (0.400)</td>
</tr>
<tr>
<td>Committee Chair</td>
<td>-0.628 (0.135)</td>
</tr>
<tr>
<td>Seat Share</td>
<td>4.819 (1.209)</td>
</tr>
<tr>
<td>(\log(\text{Number of Articles}))</td>
<td>0.487 (0.042)</td>
</tr>
<tr>
<td>Belgium</td>
<td>2.992 (0.603)</td>
</tr>
<tr>
<td>(\log(\theta))</td>
<td>-4.271 (0.552)</td>
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<tr>
<td><strong>Hurdle</strong></td>
<td></td>
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<tr>
<td>Intercept</td>
<td>-3.659 (2.543)</td>
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<td>Voter Distance</td>
<td>-0.091 (0.286)</td>
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<td>CMP Distance</td>
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<td>Coalition Size</td>
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<tr>
<td>Committee Chair</td>
<td>-1.114 (0.479)</td>
</tr>
<tr>
<td>Seat Share</td>
<td>5.692 (3.253)</td>
</tr>
<tr>
<td>(\log(\text{Number of Articles}))</td>
<td>0.940 (0.181)</td>
</tr>
<tr>
<td>Belgium</td>
<td>0.907 (1.082)</td>
</tr>
<tr>
<td>Denmark</td>
<td>-2.341 (1.021)</td>
</tr>
</tbody>
</table>

**Data Break**

| N (bills/parties) | 198/295 | 198/295 | 198/295 |

**Comparison**

| AIC            | 1069.179 | 630.494 | 594.701 |

**Vuong**

- Poisson<Neg Binomial \((p < .001)\)
- Neg Binomial<Hurdle \((p < .001)\)