Legislative Review and Party Differentiation
in Coalition Governments

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Abstract

Multiparty governance requires compromise and this compromise can lead to electoral losses. I argue that coalition members are motivated to differentiate themselves from their cabinet partners in order mitigate potential electoral losses resulting from voters perceiving them as not rigorously pursuing their core policy positions or not possessing strong policy stands. I test this argument with original data on the scrutiny of over 2,200 government bills gathered from three parliamentary democracies incorporating information on voter perceptions of partisan ideology and parties' policy preferences as derived from their manifestos. I find that coalition partners that are perceived as more similar will amend one another's legislative proposals more vigorously in an effort to differentiate in the eyes of the electorate — to protect their brand — and therefore provide evidence for "pure" vote-seeking behavior in the legislative review process. Further, these original data provide answers to several open questions regarding the policy motivations of cabinet parties in legislative review and the role of committee chairs and external support parties on policy outcomes.

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Cooperation is vital to a coalition cabinet's ability to govern effectively, but may prove costly at the polls. Voters do not support a party so that it may accommodate its cabinet partners in an effort to smooth the process of governance or trade away its core policy positions in order to obtain a fancy office. Voters support a party with the understanding that it will 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, believing that its core positions have changed or that it is untrustworthy or incompetent (Bawn and Somer-Topcu 2012; Fortunato 2017).

This puts coalition parties in a difficult position. On the one hand, there are myriad benefits to cooperation — more efficient governance, easier extraction of benefits of office, and encouragement of reciprocal behaviors. On the other hand, cooperation may obscure a party's policy brand or otherwise alienate its supporters, leading to electoral losses. I argue that these potential losses provide powerful incentives for governing parties to squabble with their coalition partners in order to demonstrate to their supporters that they are "fighting the good fight" and to protect their ideological brand. Focusing on the legislative review phase of the policymaking process, I predict that as the electorate perceives coalition partners as more ideologically similar, they will debate and amend one another's proposals more vigorously in order to signal to the electorate their ideological distinctiveness and representational competence. Examination of original data on legislative amendments to cabinet proposals in three parliamentary democracies with long histories of coalition governance supports this prediction.

These findings demonstrate, for the first time, an explicit link between voter perceptions and multiparty policymaking behavior by placing voters directly into an empirical model of parliamentary action. The results contribute to our understanding of coalition politics and policymaking, how voters perceive parties, and democratic responsiveness; three related, but to this point largely separate literatures. Further, the novel nature of the data provides answers to several open questions in coalition policymaking such as the role of committee chairs in facilitating or discouraging amendments and the influence of pivotal opposition parties. Finally, the data reveal support for a "coalition-policing" model of legislative review, reaffirming previous research.

Compromise, differentiation, and collective responsibility

Compromise and cooperation can foster several intuitive preferred outcomes for cabinet parties. Cabinet parties that "play nice" with their partners are likely to find that behavior reciprocated, leading to more efficient policymaking and division of office spoils, while simultaneously signaling to the chamber that they are desirable partners in governance. These benefits, however, are not without cost. In short, multiparty governance obscures the strategically selected ideological positions taken by member parties and makes credit claiming for various accomplishments more difficult (e.g. Fortunato and Stevenson 2013; Martin and Vanberg 2008). Indeed, Fortunato (2017) presents evidence that voters who perceive higher levels of compromise by a coalition party — manifest in perceptions of increasing ideological similarity to its partner(s) — discount both the policy statements of that party and its contribution to the government's performance. This tendency provides powerful incentives for cabinet parties to differentiate from their partners in order to protect the integrity of their strategically selected policy positions and demonstrate competence to their supporters.

However, there are substantial obstacles to differentiation in multiparty governance. Member parties may only differentiate on issues that are salient and present, which effectively constrains public dissent to the contents of the legislative agenda. Further, coalition members are bound by *collective cabinet responsibility*, a set of formal and informal rules that set the parameters of behavior for members of government (Laver and Shepsle 1994). 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 rules may be the loss of portfolio, dismissal from cabinet, or even dissolution of the government.

Taking agenda limitations and collective responsibility together, coalition parties have precious few opportunities to differentiate from their partners without risking their position in cabinet. Parties must have a proposal over which to squabble and they must be able to express themselves *before* the proposal's final 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 three fora in which cabinet parties may demonstrate how they differ from their coalition partners: legislative amendments, parliamentary

debate, and direct communication strategies (e.g., the issuing of party press releases).

Here, I examine legislative amendments, which are, by their very nature, expressions of dissent with a policy proposal. They may only be offered to an initiated proposal, and are therefore germane to the agenda, and they are costly, requiring expertise, time, and labor to draft and propose, and therefore cannot be dismissed as "cheap talk." Thus, we may consider amendments credible signals of differentiation between coalition partners sent by the reviewing party in reference to the proposal's authoring party.

Importantly, there is qualitative and quantitative evidence that proposing amendments sends differentiating signals that are likely to be absorbed by the electorate by conditioning the information environment surrounding the cabinet. For instance, most parliaments publish daily reports cataloguing events in committee and plenary meetings, including ministerial questions, debate, and the proposal of amendments. These records are vital resources for understanding politics in general and the policymaking process in particular in both scholarly research (e.g., Martin and Vanberg 2011) and popular press (e.g., Lund 2013). Increasing conflict in the review process is quite likely to make for a more antagonistic tone in media reports of party interactions and new research cataloguing the effects of this reporting on party interactions (such as the review process) suggests that these behaviors do, in fact, substantially shape media narrative on politics *and* that voters receive and assimilate these messages into their perceptions (Adams, Weschle and Wlezien 2016). Taken together with Fortunato (2017), the research suggests that differentiation strategies may help mitigate electoral losses.

When will parties amend?

Assume that the cabinet parties have arrayed themselves along the left-right spectrum such that they have each maximized their expected electoral returns. Now assume that voters perceive the parties' strategic self-placement, but will update these perceptions in response to signals from the policymaking process. Parties that appear to be cooperative or compromising will be updated as more similar, converging upon each other and shrinking the distance between them. Parties that squabble and antagonize one another will be updated as more distinct, growing the perceived distance between them. This is supported by the extant literature (e.g. Fortunato and Stevenson

2013; Fortunato 2017; Adams, Ezrow and Wlezien 2016). We can think of the utility a cabinet party derives from amending the legislative proposals of their partners in government as a function of three parameters: the cost of amending, the policy payoff, and the electoral payoff, which is a function of the benefit of differentiation from the bill's authoring party.[1] As the perceived distance between the proposing party and the reviewing party closes (relative to their strategically selected positions), the benefit of differentiation via amendment increases.

- *The more similar a cabinet party pair is perceived, the more they will amend one another's legislative proposals.*

This hypothesis is the focus of the manuscript, however, considering it in a vacuum discounts both the policy benefits of amending as well as the costs, which previous research shows can be significant. As such, a discussion of both factors is warranted and, while this may not yield an exhaustive accounting of the predictors of review, it will yield a listing of measurable covariates that will help recover clean estimates of the focal relationship.

The canonical work on legislative review and coalition policymaking argues that ministers often propose bills at their own ideal point, rather than at the coalition compromise, in order to reap position-taking benefits — another form of differentiation (Martin and Vanberg 2011). This "ministerial drift" must be mitigated by the minister's coalition partners during legislative review to prevent agency loss. As such, one of the most important covariates in the model below is the ideological distance of the proposing party from the coalition compromise ("*compromise distance*"), which is the mean, seat-weighted ideological position of all cabinet parties. I also include the dyadic ideological distance between the proposing party and the reviewing party ("*CMP distance*"). In addition to providing a control required to recover the relationship of interest, this allows us to begin to understand whether the observed patterns of amending are more consistent with self-interest or coalition policing behaviors. As Martin and Vanberg's (2011) amendment data is collected at the bill-level (they sum the total number of article changes submitted by *all parties*), amendments motivated by private policy concerns are empirically inseparable from amendments motivated by coalition policing. The data I have gathered counts article changes submitted to a bill by each *individual party* and are therefore better able parse these behaviors.

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[1] A simple formalization can be found in the appendix.

Whether or not the cabinet controls a majority of seats in parliament is also critical. Minority cabinets will not only be compelled to monitor ideologically dissimilar coalition partners, but they must also appease some pivotal opposition party. In expectation, this need to maintain opposition support should *reduce* the number of amendments we observe. This is because every position of the opposition party relative to the ministerial and reviewing parties must accomplish one of the following: increase the minister's incentive to make an offer about the coalition compromise; increase the minister's incentive to make an offer about the reviewing party's ideal point; or decrease the reviewing party's incentive to amend. A formal explanation is given in the appendix.

As for the cost of amending, extant literature suggests that holding a committee chair empowers reviewing parties, substantially reducing the costs of legislative review, and Fortunato, Martin and Vanberg (2017) provide empirical evidence to this effect. The empirical model therefore includes indicators for the identity of the committee chairperson: the party of proposing minister, reviewing party, another cabinet member, or an opposition party. As my data are coded at the bill (rather than party) level, they allow more leverage in discovering whether committee chairs' primary influence is positive (increasing their party's ability to scrutinize), negative (inhibiting other parties from scrutinizing), or both. Relatedly, Thies (2001) and others have argued that junior ministers are the executive complement to committee chairs and the model controls for their presence as well.

Finally, amendments require expertise and labor to draft and submit and having more members in parliament to share the burden of scrutiny should enable the reviewing party to amend more freely. I therefore include the reviewing party's seat share to account for differences in the ability to amend as a function of information and labor resources across parties.[2]

Data and model specification

The dependent variable is constructed from original data on amendments offered to government proposals in committee. I gather information on over 2,200 draft bills introduced by cabinet ministers in three parliamentary democracies with long histories of coalition governance: Belgium (1992-2010), Denmark (1991-2004), and the Netherlands (1995-2013).[3] This is the largest collection

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[2]Committee seats are distributed proportionally to chamber seats in all three countries.

[3]The data includes all legislation on taxation, spending, and social services as this legislation most readily conforms to a traditional left-right dimension of political discourse. Other policy types, such as treaty ratification or

of such data assembled.

For each bill, I record the proposing minister's party and department,[4] the number of articles in the original proposal, and the number of article changes offered to the bill by all cabinet parties save the authoring party.[5] These article changes, which serve as the dependent variable in the coming analysis, are counted in the same manner as Martin and Vanberg (2011) with two exceptions. First, I count all *submitted* article changes, while Martin and Vanberg (2011) count all *accepted* changes; this is because I am interested in the use of review for signaling purposes while Martin and Vanberg are interested in its policy implications. Second, I attribute all amendments to a single party, whereas Martin and Vanberg sum all amendments proposed by all parties and use the bill as the unit of observation. Recording the data in this manner is critical. Only by coding changes at the bill-party level am I able to test the above hypothesis. Importantly, summing the amendments in my data to the bill level and replicating the Martin and Vanberg model produces very similar results, which, in addition to providing out-of-sample support for their argument, implies that the mechanics of review across the two samples are quite similar.

The independent variable of interest, the distance perceived by voters between the proposing party and the reviewing party, is calculated with survey data from the Eurobarometer, European Electoral Survey, and the Comparative Study of Electoral Systems surveys conducted between 1989 to 2014. In each module, respondents were asked to place their country's political parties on an ideological scale. These placements are rescaled to a common 0-10 scale and a placement is estimated for each party in each survey via linear regression.[6] These individual party placements are then used to create a voter perceived distance between each ministerial party and each reviewing party ("*voter distance*"). The expectation is that the parameter estimate on voter distance will be *negative*; the more similar voters believe the cabinet parties are, relative to their strategically selected positions, the more they should amend one another's proposals in order to differentiate. The model employs distance estimates derived from surveys administered about the time the cabinet was formed. This

transposition of EU directives may tap dimensions orthogonal to national political discourse, and, more importantly, are likely to be exogenous to domestic political debate. Budgets and constitutional alterations are omitted as they are subject to special rules. Caretaker cabinets are dropped. This follows Martin and Vanberg (2011).

[4] The leading minister is recorded for cosponsored bills.

[5] Amendments co-sponsored by the authoring party are omitted.

[6] The models employed are intercept-only regressions where the constant estimate serves as the party placement and the standard errors are recorded to model the uncertainty of these placements.

is because the theoretical process — that today's voter perceptions impact amendment activity to shape tomorrow's voter perceptions, which in turn impact tomorrow's amending — implies endogeneity among the outcome and covariate of interest. The distance measured about the cabinet's formation should be the most insulated from these effects.[7]

Also included are the controls discussed above — indicators for minority cabinets, junior ministers, and different committee chair types, as well as the manifesto distance between the ministerial and reviewing parties and the minister's distance from the coalition compromise. These are calculated with left-right preference estimates derived from the Comparative Manifestos Project data following Lowe et al. (2011). CMP was chosen over alternative measures, such as the Chapel Hill expert survey measures for two reasons: first, the manifesto estimates are explicitly estimates of parties' campaign platform, their strategically chosen positions, and therefore a better fit to theory; second, CMP has substantially better coverage for the sample period. Descriptive statistics are shown in Table 1.

Table 1: Descriptive statistics of key variables

Variable	Mean	SD	Min	Max
Article Changes	0.70	4.57	0.00	170.00
Voter Distance	1.48	0.92	0.00	3.74
CMP Distance	0.60	0.64	0.01	2.84
Compromise Distance	0.25	0.30	0.01	1.80

The correlation of the distance between coalition party pairs as perceived by voters and as selected by the parties is shown in Figure 1, where the overall correlation is plotted with both linear and LOWESS lines and the estimates of a linear model regressing CMP distance on voter distance (which are measured on different scales) is given in the lower right-hand corner. The figure shows that the relationship is positive, but weakly so. Given that the sample only includes cabinet dyads — parties sufficiently compatible to coalesce in the first place — this weakly positive

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[7]Using the most recent or most proximate survey estimate produces results that are more negative and statistically robust. Comparisons shown in appendix.

relationship makes sense (including opposition parties would make the relationship appear much stronger). The figure also reveals considerable variation between the similarity perceived by voters and the similarity manifest in the selected policy positions of cabinet partners. This variation is the focus of the manuscript.

Figure 1

Comparison of Voter–perceived and Manifesto Dissimilarity
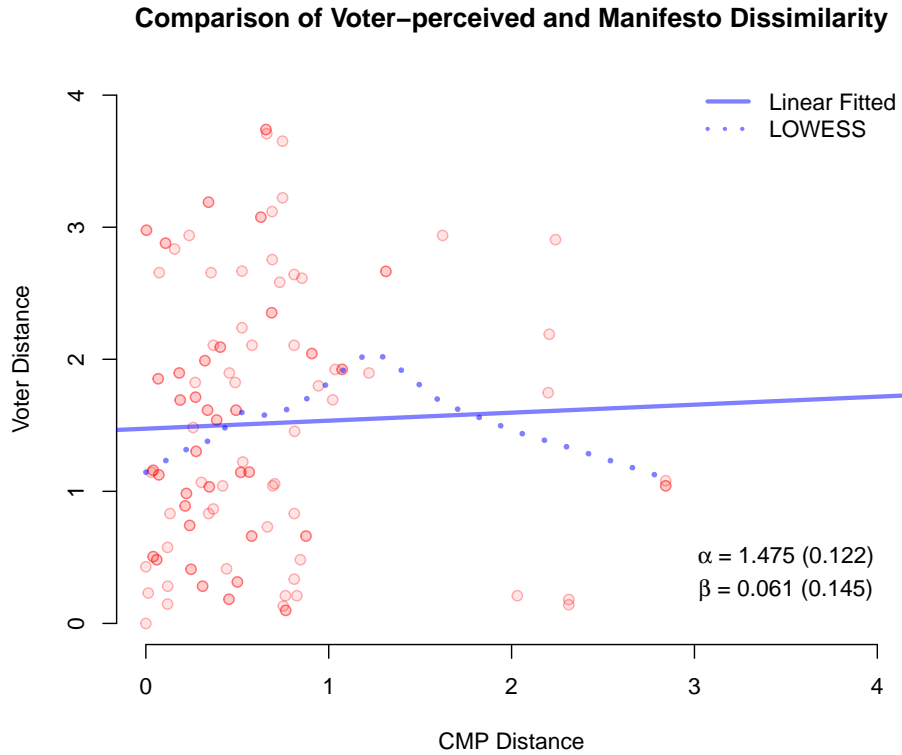
The data used to construct the remaining variables were all collected from the respective countries' legislative databases, the Constitutional Change and Parliamentary Democracies (CCPD) project data (Strøm, Müller and Bergman 2008) or the Parliament and Government Composition Database (Döring and Manow 2011). In addition to the variables discussed, the model includes the cabinet's size in number of parties (logged) and three exposure terms: the (logged) number of articles in the original proposal, the (logged) number of days the bill spent under review, and a dummy variable indicating that the plenary session expired before the conclusion of the bill's scrutiny.

Estimation and analysis

The dependent variable is the number of article changes offered by a cabinet party to the proposal of one its coalition partners. A poisson model is appropriate, but the data structure presents a small hurdle, grouping bills across different countries where amendment propensities vary systematically; Belgians amending more than Danes and Dutch amending more than Belgians on average.[8] The model therefore includes country fixed effects, but it is also possible that certain bills are offered more amendments for reasons not captured by the measured covariates, and, because multiple parties may amend the same bill, there is potential for correlations across rows of data within bills. The model therefore allows random intercepts at the bill level.[9] The results are presented in Table 2 along side results from a stripped-down model containing only the covariate of interest, exposure terms, and fixed and random effects. The comparison shows that, while voter distance is robust ($p < 0.05$, single-tailed) in the simple model, its magnitude is suppressed. This reinforces how important it is to consider the costs as well as the policy benefits of amending when examining review behavior.

The full model bears strong support for the central hypothesis. Voter distance has a robust negative effect on amending — the more voters perceive the reviewing party as similar to the ministerial party, the more the reviewing party will amend. Holding all other covariates constant at their mean, the effect of a one standard deviation *decrease* in voter distance between a pair of cabinet partners, is a 9% (4%, 13% CI) increase in the predicted number of amendments. To put this in context, this is roughly 1/3 of the magnitude of the effect of a similar change in compromise distance (a 29% decrease in amendments). This is strong evidence for the hypothesis and supports the central argument that cabinet parties, limited by collective responsibility in their ability to highlight policy differences with their partners in government, use the legislative review process to communicate their ideological distinctiveness by voicing dissent with their partners' policy proposals. These findings are supported by country-by-country estimates that can be viewed in the appendix.[10]

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[8]Additionally, the data are over-dispersed with zeroes. This is discussed in the appendix.

[9]The appendix contains the results of a non parametric bootstrap designed to model the error in the estimated variables (voter distance, CMP distance, and compromise distance). The hypothesized results hold in this exercise.

[10]Included covariates in the country-by-country estimates vary across countries due to a lack of within-country variation. This is detailed in the appendix.

Table 2: Main model results.

Parameter	Simple	Full
Voter Distance	-0.043	-0.092
	(0.025)	(0.028)
CMP Distance		-0.379
		(0.077)
Compromise Distance		1.167
		(0.321)
Minority Cabinet		-0.672
		(0.578)
Junior Minister		-0.081
		(0.062)
Reviewer Chair		-0.092
		(0.238)
Minister Chair		-0.259
		(0.315)
Partner Chair		-0.299
		(0.237)
Seat Share		2.294
		(0.310)
ln(Cabinet Size)		-0.569
		(0.786)
ln(Articles)	0.993	0.980
	(0.096)	(0.096)
ln(Days in Review)	0.959	0.914
	(0.130)	(0.130)
Plenary Expiration	-1.683	-1.640
	(1.522)	(1.497)
Denmark	-4.198	-3.892
	(0.463)	(0.713)
Netherlands	0.567	0.352
	(0.221)	(0.418)
Intercept	-9.446	-8.698
	(0.681)	(1.374)
var(Random Intercepts: Bills)	9.825	9.607
	(1.023)	(1.001)
N	4324	4324
$ln(likelihood)$	-2697.726	-2647.171

The remaining variables in the model conform to expectations, which suggests proper specification. The number of articles in the original draft bill and the length of the review period have estimates near 1 (typical for exposure variables) and the more substantively interesting variables also have estimates in the "right direction." The more MPs a party has to share the burden of amending, the more amendments it submits. If the reviewing party's seat share increases from, say 0.1 to 0.2, its predicted number of amendments increases by about 24%. The cabinets's minority status also yields the predicted relationship, reducing the number of proposed article changes by 40%, though this fails to reach traditional levels of significance.

Also interesting among the control variables are the estimates on dyadic CMP distance and

compromise distance. The estimate on compromise distance is positive, substantively large, and statistically robust. The farther away the proposing minister from the coalition compromise, the more the reviewing party tends to amend. It would appear that CMP distance exerts a negative effect, which no theory would predict. I note that the estimate on this covariate is among the more unstable across specifications and discuss this more in the appendix. Nonetheless, taken together, the patterns of observed amendment behavior are more consistent with a coalition-policing model of review behavior than with a self-interested model of review behavior, implying that conflict amelioration is paramount, as has been argued elsewhere (e.g., Bowler et al. 2016).

The final substantively interesting set of results regard the presence of a junior minister and identity of the committee chairman. Having a junior minister in the the department of the proposing minister has no discernible effect on amendment behavior in this sample, though it should be noted that the reviewing party only has a junior minister in the proposing minister's department in about 7% of the observations here and these are heavily clustered in the Netherlands. The model also suggests that the identity of the committee chair is largely irrelevant to legislative scrutiny amongst cabinet partners. This comports with previous research finding no difference in scrutiny between committees chaired by the party of the proposing minister and committees chaired by her partner in government, but substantial increase in scrutiny when the committee is chaired by a member of the opposition (Fortunato, Martin and Vanberg 2017). These results support the interpretation that the uptick in scrutiny under opposition chaired committees is evidence of opposition influence and that the chair's power *may* be exercised positively when held by opposition but is *almost certainly* exercised negatively when held by cabinet, protecting bills from opposition influence. This implies that committee chairpersons, in practice, act as more of a "backstop" than active policeman in reference to the coalition bargain and that their "watchdog" function, theorized by Carroll and Cox (2012), is a passive one.

Discussion

Coalition members lose votes when their supporters do not perceive them as rigorously pursuing their core policy positions (Fortunato 2017). Given that parties are electorally motivated, this penalty imposes a hard decision on cabinet parties; will they continue to compromise with their

partners in order to streamline policymaking and maximize benefits of office, or will they forgo these cooperative incentives to differentiate and protect their electoral fortunes? By examining the behaviors of cabinet parties in countries where coalition governance has been the norm for several decades, we uncovered strong evidence that parties condition their behaviors on the manner in which voters *perceive* them. When voters begin to perceive parties as becoming too similar to their partners in government, eroding the distinctiveness of their policy position and implying a failure to properly advocate for their supporters, parties leverage the means afforded to them by the legislative review process to differentiate from one another without breaking the rules of collective cabinet responsibility. We observe this behavior even when accounting for the level of real policy conflict between cabinet partners. This implies that parties are not only using the review process as a mechanism for mitigating ministerial drift, but are acting in direct response to voters. As such, the effects of voter perceptions on amending reflect "pure" vote-seeking behavior in an entirely new arena and complement (but also stand apart from) previous research assessing whether responsiveness exists at all (e.g., Jennings and John 2009).

A reasonable follow-up question to ask would be, why do parties tolerate the nuisance and inefficiency such differentiating behavior? One answer is that it is mutually beneficial. The proposing minister gets to signal her ideal point by submitting her most preferred policy. The reviewing party, in turn, gets to signal dissent and competence by amending the proposal. It is clear that if both parties were only policy-motivated and rational, they would look down the game tree and discover that the most efficient outcome is in legislating the coalition compromise in nearly every instance. But, of course, parties are not merely policy-motivated, they are also vote seekers, and as such reap reward from these legislative inefficiencies by burnishing their brand to the electorate. This inefficiency is not necessarily a net negative from a normative standpoint, however. The results presented here are encouraging in that they provide robust evidence that parties are listening and responding to voters throughout the electoral cycle and not merely during campaigns. Indeed, this is some of the first cross-national evidence that voter perceptions or preferences have real impact on parliamentary behavior.

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Supplementary Materials

Utility of amending

We can think of the utility a cabinet party derives from amending the legislative proposals of their partners in government as a function of three parameters: the electoral benefit, the policy benefit, and the cost of drafting and proposing the amendment. Here, electoral rewards are a function of the benefit of differentiation from the bill's authoring party. As the perceived distance between the proposing party and the reviewing party closes (relative to their selected positions), the benefit of differentiation increases and therefore the probability of amending should increase in kind. We can write a simple utility function for a cabinet party a in the legislative review process as: $U_a = (v - m) + p - c$, where U_a is the utility a reviewing party derives from amending the legislative proposal before it. Here, p captures the policy benefit of amending (think of this as the distance between the a's ideal point and the policy proposal), c captures the cost of drafting and submitting the amendment, v is the distance between the reviewing party a and the party of the proposing minister as perceived by voters, and m is the distance between the two as staked out in their electoral manifestos. Holding p and c at zero, the utility of differentiating through amendment is positive when voters perceive the pair as more similar than their selected positions, and negative when voters perceive the pair as more dissimilar than their selected positions. As such, for any constant value of m, the utility of amending increases as v falls.

Pivotal opposition

The main text notes that every position of the opposition party relative to the ministerial and reviewing parties must result in one of the following: increase the minister's incentive to make an offer about the coalition compromise; increase the minister's incentive to make an offer about the reviewing party's ideal point; or decrease the reviewing party's incentive to amend. To understand this, consider a two-party coalition composed of m and r, the proposing ministerial party and reviewing party, respectively, where the coalition compromise position x, is located $m < x < r$, with some opposition party o. For an arrangement of the parties $m < r < o$ when the cabinet controls a majority and does not require the support of the opposition, our expectation is that m would propose some policy p about its ideal point ($m \leq p < x$) and r would be compelled to amend the policy to drag it back to the coalition compromise x; this is the intuition of the Martin and Vanberg (2011) model. In the case of a minority cabinet, o becomes pivotal and m is much more likely to make an initial offer closer to x or perhaps even greater ($p \geq x$) in order to maintain the support of o, therefore shrinking $|p - x|$ and reducing the number of amendments r must propose to monitor the coalition agreement. We should observe a similar reduction in amendments given the arrangement $o < m < r$. Again, when the coalition controls a majority of the legislature, we would expect m to propose about its ideal point and r to rein the proposal in to the compromise position by amending. When the cabinet is a minority however, m may propose about its ideal point and r is constrained in its ability to amend, as each amendment it offers to bring the policy back to the coalition compromise increases the likelihood that o will reject the policy and vote against it. Finally, for an arrangement $m < o < r$ (which is observed very rarely), the placement of o serves as de facto enforcement of the coalition compromise. Thus, for any ideological rank-ordering of cabinet and opposition parties, we should observe fewer amendments submitted by minority coalitions due to the cabinet's obligation to maintain pivotal opposition support.

Martin and Vanberg replication

Below, I replicate the **?** model of legislative review by collapsing the data from the party-bill level to bill level and summing all amendments offered by all cabinet parties for the dependent variable. The variables in these models are calculated following **?** and the models specification is the same less three exceptions. There is no opposition divisiveness variable here as opposition amendments have been omitted. There is also no variable indicating the number of committee referrals as in the original model. This is because multiple committee referrals are vanishingly rare in the countries analyzed here. Indeed, going back to the original data used by Martin and Vanberg, it appears that multiple referrals are effectively limited to Germany. In their data 99% of their German bills are referred to multiple committees, while only 3% of their Dutch bills are referred to more than one committee and, in this, sample, there are no bills in which a second committee issues an independent report. Finally, because there are several minority cabinets in my sample, I include a binary variable indicating such, and also estimate a second model including the identity of the committee chairs: ministerial party, opposition party or the baseline category, a partner party to the submitting minister. In both models, random intercepts are allowed at the level of the cabinet.

As Table 3 shows, Martin and Vanberg's primary finding — that amendment activity increases with the proposing minister's ideological distance from the coalition bargain — is manifest in this sample. This is good as it not only reaffirms support for their theoretical model in new data, but also suggests that the determinants of policy-motivated amendments in my sample are similar to the determinants in their original sample, even though the time periods are quite different and the countries are different: Germany and the Netherlands in **?** and Belgium, Denmark, and the Netherlands here. This means that the mechanics of legislative review are likely to be relatively common in parliamentary democracies with strong committee systems, thus, the conclusions that I draw from the main text analysis are likely to travel outside of the sample countries.

Table 3: Replication of Martin and Vanberg (2005)

	Without Chairs		With Chairs	
	Parameter	(SE)	Parameter	(SE)
Compromise Distance	0.954	(0.363)	0.980	(0.363)
Junior Minister	0.943	(0.263)	0.923	(0.266)
Minority	-3.661	(0.600)	-3.549	(0.615)
ln(Days in Review)	0.696	(0.126)	0.694	(0.126)
ln(Articles)	0.670	(0.100)	0.695	(0.101)
Plenary Expiration	-1.557	(0.944)	-1.546	(0.941)
Minister Chair			-0.321	(0.292)
Opposition Chair			-0.357	(0.276)
Intercept	-4.021	(0.613)	-3.874	(0.622)
$ln(\theta)$	2.247	(0.076)	2.240	(0.076)
var(Random Intercepts: Cabinet)	0.795	(0.423)	0.841	(0.447)
N	2209		2209	
$ln(likelihood)$	-1578.337		-1577.374	

Comparisons of focal measure

In Table 4, I present the results of models measuring the focal variables at different points in time. Either using the public opinion survey and policy manifesto issued about the time of the cabinet's formation, or the survey and manifesto administered/issued most recently to the bill being analyzed, or the survey and manifesto administered/issued most proximate to the bill being analyzed. The effect sizes are very consistent across the measures, with the most proximate measure yielding the largest effects.

Table 4: Comparisons of focal measure

	Cabinet		Recent		Proximate	
	Coef	(SE)	Coef	(SE)	Coef	(SE)
Voter Distance	-0.092	(0.028)	-0.117	(0.031)	-0.105	(0.030)
CMP Distance	-0.379	(0.077)	-0.387	(0.077)	-0.101	(0.065)
Compromise Distance	1.167	(0.321)	1.158	(0.322)	1.103	(0.325)
Minority Cabinet	-0.672	(0.578)	-0.686	(0.579)	-0.735	(0.575)
Junior Minister	-0.081	(0.062)	-0.087	(0.062)	0.006	(0.059)
Reviewer Chair	-0.092	(0.238)	-0.119	(0.237)	-0.191	(0.236)
Minister Chair	-0.259	(0.315)	-0.277	(0.315)	-0.326	(0.315)
Partner Chair	-0.299	(0.237)	-0.337	(0.238)	-0.332	(0.237)
Seat Share	2.294	(0.310)	2.332	(0.310)	2.163	(0.307)
ln(Cabinet Size)	-0.569	(0.786)	-0.536	(0.788)	-0.916	(0.773)
ln(Articles)	0.980	(0.096)	0.984	(0.096)	0.975	(0.096)
ln(Days in Review)	0.914	(0.130)	0.909	(0.131)	0.918	(0.130)
Plenary Expiration	-1.640	(1.497)	-1.645	(1.499)	-1.591	(1.490)
Denmark	-3.892	(0.713)	-3.841	(0.714)	-4.054	(0.704)
Netherlands	0.352	(0.418)	0.409	(0.418)	0.189	(0.412)
Intercept	-8.698	(1.374)	-8.718	(1.376)	-8.215	(1.356)
var(Random Intercepts: Bills)	9.607	(1.001)	9.626	(1.003)	9.378	(0.979)
N	4324		4327		4519	
$ln(likelihood)$	-2647.171		-2645.361		-2658.308	

Full model comparison for over-dispersion

In the main text, I present the results of a poisson model where error components are estimated at the bill-level. However, the data are overdispersed with zeroes, violating poisson assumptions of mean-variance equivalence and conditional independence. Typically, researchers turn to the negative binomial in this case, or, less frequently, zero-inflated or hurdle models. However, the hierarchical model presented in the main text more efficiently models out the over-dispersion and has the added benefit of capturing bill-level correlations in the data. Nonetheless, I present comparisons of poisson and negative binomial models, assessing pooled models, hierarchical models (like the one presented in the main text), and zero-inflated models. First, in Table 5, I compared standard pooled models. Both support the hypothesis, however, the poisson model provides substantially better fit, despite the $ln(\theta)$ estimate in the negative binomial showing that the data are clearly over dispersed. This over dispersion, however, is rooted in a preponderance of zero counts, rather than violation of independence assumption — which, in data like these, is easier to think of as the amount "effort" is take to increase the count by one unit; is the transition 1 to 2 as "difficult" or as likely as the transition from 10 to 11? This means that the real data issue to contend with is the zeroes.

Table 5: Comparison of pooled poisson and negative binomial

	Poisson		Negative Binomial	
	Parameter	(SE)	Parameter	(SE)
Voter Distance	-0.175	(0.023)	-0.212	(0.088)
CMP Distance	-0.045	(0.054)	0.324	(0.222)
Compromise Distance	0.561	(0.071)	0.296	(0.299)
Junior Minister	0.537	(0.048)	0.282	(0.228)
Reviewer Chair	0.234	(0.054)	0.084	(0.211)
Minister Chair	0.394	(0.054)	-0.151	(0.206)
Partner Chair	0.380	(0.049)	0.253	(0.202)
Seat Share	4.129	(0.280)	3.695	(0.995)
ln(Cabinet Size)	-0.031	(0.158)	-0.166	(0.504)
Minority Cabinet	-0.321	(0.140)	-0.118	(0.442)
ln(Articles)	0.519	(0.015)	0.696	(0.068)
ln(Days in Review)	0.532	(0.020)	0.538	(0.081)
Plenary Expiration	-0.474	(0.346)	-0.928	(0.568)
Country				
Denmark	-3.090	(0.249)	-3.151	(0.482)
Netherlands	0.228	(0.082)	0.723	(0.292)
Intercept	-4.223	(0.271)	-4.545	(0.928)
$ln(\theta)$			2.360	(0.059)
N	4519		4519	
$ln(likelihood)$	-7123.246		-2607.313	
Mean Deviance $|\hat{y} - y|$	1.049		1.130	

Next, I compare poisson and negative binomial zero-inflated models in table 6. There is very little difference in fit across the two specifications and both support the hypothesis, however, some of the estimates are odd (negative estimates on compromise distance, positive estimates on minority status, or positive estimate on having a junior minister are discordant with theory), implying that a zero-inflated model is not the correct choice.

Finally, I compare fit across hierarchical specifications, where random effects are allowed at the bill level in Table 7. Notice that the fit is improved dramatically over the standard pooled models

and zero-inflated specifications. Notice also that the recovered logged scaling parameter in the negative binomial model is is 0. This implies that the random effects has successfully modeled out the over-dispersion and estimation of a negative binomial, rather than poisson, is unnecessary and this is reflected in the quality of fit differences between the model. Indeed, if we merely compare the quality of the predicted values, the poisson outperforms the negative binomial 3824 to 695. As such, we should not be concerned that the hierarchical negative binomial does not support the hypothesis, but does produce odd estimates.

The final estimates are the results of a non-parametric bootstrap, modeling the error in the estimated independent variables (voter distance, CMP distance, and compromise distance), given in Table 8. The process imputes new values of the estimated covariates, estimates the model, records the results and repeats 1,000 times. The hypothesized results stand, however, the error in the CMP estimates (which can be fairly large) wash out the robustness of the compromise distance estimates.

Table 6: Comparison of poisson and negative binomial zero-inflated models

| | | Poisson | | Negative Binomial | |
		Parameter	(SE)	Parameter	(SE)
	Voter Distance	-0.190	(0.023)	-0.208	(0.092)
	CMP Distance	0.106	(0.056)	0.046	(0.211)
	Compromise Distance	-0.051	(0.080)	0.051	(0.290)
	Junior Minister	0.234	(0.048)	0.244	(0.244)
	Reviewer Chair	0.300	(0.055)	0.414	(0.234)
	Minister Chair	0.289	(0.056)	0.400	(0.245)
	Partner Chair	0.460	(0.051)	0.429	(0.231)
	Seat Share	1.149	(0.285)	0.877	(1.151)
Count	ln(Cabinet Size)	0.015	(0.168)	0.639	(0.683)
	Minority Cabinet	-0.198	(0.145)	0.027	(0.554)
	ln(Articles)	0.150	(0.017)	0.198	(0.077)
	ln(Days in Review)	0.216	(0.021)	0.412	(0.088)
	Plenary Expiration	0.216	(0.389)	0.543	(0.901)
	Denmark	-1.638	(0.409)	-1.616	(0.756)
	Netherlands	0.583	(0.088)	1.015	(0.376)
	Intercept	-0.019	(0.296)	-2.736	(1.280)
	$ln(\theta)$			1.332	(0.147)
	Voter Distance	-0.028	(0.065)	-0.093	(0.119)
	CMP Distance	0.103	(0.156)	0.007	(0.329)
	Compromise Distance	-0.778	(0.208)	-1.616	(0.619)
	Junior Minister	-0.412	(0.169)	-0.494	(0.277)
	Reviewer Chair	0.218	(0.160)	0.665	(0.297)
	Minister Chair	0.171	(0.160)	0.726	(0.302)
	Partner Chair	0.183	(0.146)	0.614	(0.279)
	Seat Share	-4.340	(0.886)	-5.784	(1.411)
Inflate	ln(Cabinet Size)	-0.076	(0.388)	0.858	(0.843)
	Minority Cabinet	-0.169	(0.350)	-0.039	(0.605)
	ln(Articles)	-0.578	(0.046)	-0.939	(0.140)
	ln(Days in Review)	-0.381	(0.065)	-0.326	(0.116)
	Plenary Expiration	0.981	(0.661)	1.752	(0.953)
	Denmark	1.810	(0.509)	2.447	(0.836)
	Netherlands	0.376	(0.212)	1.191	(0.485)
	Intercept	5.065	(0.690)	3.053	(1.546)
	N	4519		4519	
	$ln(likelihood)$	-4004.898		-2513.992	
	Mean Deviance $|\hat{y} - y|$	0.699		0.702	

Table 7: Comparison of hierarchical poisson and negative binomial

	Poisson		Negative Binomial	
	Parameter	(SE)	Parameter	(SE)
Voter Distance	-0.105	(0.030)	-0.024	(0.069)
CMP Distance	-0.101	(0.065)	-0.107	(0.158)
Compromise Distance	1.103	(0.325)	1.033	(0.389)
Junior Minister	0.006	(0.059)	0.340	(0.184)
Reviewer Chair	-0.191	(0.236)	-0.265	(0.279)
Minister Chair	-0.326	(0.315)	-0.346	(0.344)
Partner Chair	-0.332	(0.237)	-0.399	(0.280)
Seat Share	2.163	(0.307)	4.881	(0.905)
ln(Cabinet Size)	-0.916	(0.773)	-0.659	(0.863)
Minority Cabinet	-0.735	(0.575)	-0.450	(0.633)
ln(Articles)	0.975	(0.096)	1.020	(0.107)
ln(Days in Review)	0.918	(0.130)	0.923	(0.146)
Plenary Expiration	-1.591	(1.490)	-1.320	(1.498)
Country				
Denmark	-4.054	(0.704)	-3.749	(0.744)
Netherlands	0.189	(0.412)	0.084	(0.461)
Intercept	-8.215	(1.356)	-9.019	(1.501)
$ln(\theta)$			-0.006	(0.114)
var(Random Intercepts: Bills)	9.378	(0.979)	8.397	(0.965)
N	4519		4519	
$ln(likelihood)$	-2658.308		-2318.375	
Mean Deviance $|\hat{y} - y|$	0.332		0.395	
Better Fit	3834		685	

Table 8: Bootstrap results. Hierarchical poisson with random intercepts allowed at the bill level.

| | Poisson | | |
	Parameter	(SE)	p
Voter Distance	-0.117	(0.033)	0.000
CMP Distance	-0.024	(0.110)	0.407
Junior Minister	0.016	(0.064)	0.404
Reviewer Chair	-0.236	(0.237)	0.161
Ministerial Chair	-0.290	(0.312)	0.176
Partner Chair	-0.377	(0.238)	0.058
Compromise Distance	0.062	(0.276)	0.410
Seat Share	2.171	(0.324)	0.000
Cabinet Size	-1.190	(0.768)	0.060
Minority Cabinet	-1.086	(0.569)	0.029
Articles	0.980	(0.096)	0.000
Days in Review	0.953	(0.131)	0.000
Plenary Expiration	-1.641	(1.500)	0.137
Country			
Denmark	-4.086	(0.710)	0.000
Netherlands	0.110	(0.411)	0.394
Intercept	-7.652	(1.337)	0.000
var(Random Intercepts: Bills)	9.625	(1.010)	
N		4519	
Average $ln(likelihood)$		-2660	

Stripped down and single-country models

In this section I show estimates from both stripped down and fully specified models, both within country and pooled. I begin with the stripped down models including only the covariate of interest, the exposure terms, and fixed and random effects (less Denmark which has too few governments of of three or more parties to efficiently estimate bill-level intercepts). These are given in Table9 and the estimates show that the focal variable is always signed in the correct direction, but fails to reach significance in the Netherlands model. To a degree, however, it is somewhat surprising that these uncontrolled models all produce at least the direction of the predicted effect. That is, there is a sufficiently large number of substantively and statistically significant predictors of amendment behavior (some of which are correlated with the covariate of interest) omitted from these models that the manifestation of the predicted effect is very encouraging.

Table 9: Stripped down by country

	Belgium		Denmark		Netherlands		All	
	Coef	(SE)	Coef	(SE)	Coef	(SE)	Coef	(SE)
Voter Distance	-0.077	(0.038)	-0.916	(0.449)	-0.032	(0.035)	-0.043	(0.025)
ln(Articles)	1.217	(0.168)	0.300	(0.232)	0.894	(0.124)	0.993	(0.096)
ln(Days in Review)	0.848	(0.202)	1.512	(0.362)	0.887	(0.179)	0.959	(0.130)
Plenary Expiration	-2.040	(1.709)					-1.683	(1.522)
Denmark							-4.198	(0.463)
Netherlands							0.567	(0.221)
Intercept	-9.510	(1.093)	-10.244	(1.574)	-8.458	(0.875)	-9.446	(0.681)
var(Random Intercepts: Bills)	9.605	(1.716)			10.286	(1.421)	9.825	(1.023)
N	1429		1693		1397		4324	
$ln(likelihood)$	-987.166		-106.135		-1620.355		-2697.726	

Table 10 displays the results of country-by-country regressions using all covariates as the full model in the main text. These models include fixed effects for the authoring ministers, rather than estimate random effects for bills, as in the main text specification. Differences in across countries in which submitting ministers/departments are included in the model are a function of which departments the countries have (for example, social affairs and labor exist within a single ministry in the Netherlands) and which departments are given spending/appropriating authority. Other covariates may be missing due to a lack of variation. For example, in the Denmark, nearly all observations are drawn from two-party cabinets. This makes the compromise distance and CMP distance variables collinear. Further, so-called "shadow chairs" are so common in Denmark, that bills almost never submitted to a committee chaired by the party of the authoring minister. Each of the models support the central hypothesis and it is encouraging to see that the hypothesized effect is manifest within each individual country as well as the pooled sample.

Table 10: Full model by country

	Belgium		Denmark		Netherlands	
	Coef	(SE)	Coef	(SE)	Coef	(SE)
Voter Distance	-0.152	(0.036)	-0.680	(0.574)	-0.081	(0.035)
CMP Distance	0.215	(0.069)			-0.149	(0.089)
Compromise Distance	-0.125	(0.103)	0.811	(1.382)	0.116	(0.143)
Junior Minister	-13.294	(456.648)			0.242	(0.053)
Reviewer Chair	0.127	(0.108)	-2.341	(1.055)	0.383	(0.069)
Minister Chair	0.454	(0.119)			0.678	(0.070)
Partner Chair	-0.149	(0.094)			0.576	(0.068)
Seat Share	1.127	(1.081)	5.525	(3.572)	3.965	(0.300)
ln(Cabinet Size)	-0.600	(0.248)			3.180	(0.708)
ln(Articles)	0.757	(0.028)	0.503	(0.299)	0.409	(0.018)
ln(Days in Review)	0.514	(0.038)	1.167	(0.375)	0.421	(0.027)
Plenary Expiration	-0.288	(0.373)				
Minority					0.843	(0.315)
Agriculture and Energy	base	base				
Commerce	0.824	(0.210)	base	base		
Defense	0.197	(0.211)				
Economy	1.071	(0.194)	0.246	(1.015)	base	base
Finance	-0.228	(0.198)	0.032	(1.252)	-1.720	(0.080)
Labor	0.503	(0.293)	-1.638	(1.249)		
Infrastructure/Regional	-0.016	(0.253)			-0.877	(0.217)
Social Welfare	-0.388	(0.296)	-1.950	(0.940)	-0.847	(0.050)
Intercept	-3.691	(0.513)	-9.413	(1.920)	-5.884	(0.789)
N		1429		1693		1397
$ln(likelihood)$		-2039.848		-78.69064		-4467.500

An anonymous reviewer raised the concern the three central measures of party distance (voter distance, CMP distance, and coalition compromise distance) may be sufficiently correlated to a) contribute to the main result or, b) induce the negative estimate on CMP distance. Tables 11 and 12 display diagnostics for the variables' collinearity and the results of models omitting coalition compromise and then CMP distance. The diagnostics suggest that we should be unconcerned with collinearity across these measures (quite low VIF and quite high tolerance estimates). However, the model results show instability in the CMP distance estimates — positive (though small) in pooled models and negative in hierarchical models. Meanwhile, the coalition compromise and voter distance estimates remain stable throughout specification. This leads to the conclusion that a) CMP distance is not a high quality predictor of amendment behavior (unlike voter distance and coalition compromise distance) and b) its relationship to amendments is dependent upon correlations in unmeasured characteristics within bill grouping in the data, though I cannot with any confidence conclude what those characteristics may be. It is worth noting that, of the three timings we could select for our measures — most proximate, most recent, measure at the time of cabinet formation — the most proximate measure of CMP distance comes closest to producing the robust positive relationship that one would expect, but only in the absence of random effects on bills.

Table 11: Collinearity diagnostics for central distance measures

Variable	VIF	Tolerance	R^2
Voter Distance	1.14	0.879	0.121
CMP Distance	1.17	0.858	0.142
Compromise Distance	1.03	0.973	0.027

Table 12: Stability of CMP and coalition compromise distance measures.

	CMP Distance		Compromise		All	
	Pooled	RE	Pooled	RE	Pooled	RE
Voter Distance	-0.082	-0.094	-0.075	-0.123	-0.075	-0.092
	(0.020)	(0.028)	(0.020)	(0.028)	(0.020)	(0.028)
CMP Distance	0.058	-0.360			0.002	-0.373
	(0.047)	(0.077)			(0.050)	(0.077)
Compromise Distance			0.228	0.715	0.228	0.881
			(0.062)	(0.340)	(0.066)	(0.340)
Junior Minister	0.567	-0.077	0.560	0.032	0.560	-0.081
	(0.048)	(0.062)	(0.047)	(0.057)	(0.048)	(0.062)
Reviewer Chair	0.167	-0.159	0.195	-0.189	0.194	-0.115
	(0.053)	(0.236)	(0.053)	(0.237)	(0.054)	(0.238)
Minister Chair	0.345	-0.229	0.355	-0.340	0.354	-0.270
	(0.055)	(0.312)	(0.054)	(0.314)	(0.055)	(0.314)
Partner Chair	0.339	-0.362	0.361	-0.325	0.360	-0.320
	(0.048)	(0.236)	(0.048)	(0.237)	(0.048)	(0.238)
Seat Share	3.980	2.324	3.918	2.272	3.917	2.310
	(0.281)	(0.310)	(0.281)	(0.310)	(0.282)	(0.310)
ln(Cabinet Size)	-0.254	-0.912	-0.181	-0.923	-0.182	-0.693
	(0.155)	(0.774)	(0.154)	(0.778)	(0.157)	(0.783)
ln(Articles)	0.508	0.987	0.511	0.982	0.511	0.986
	(0.015)	(0.096)	(0.015)	(0.096)	(0.015)	(0.096)
ln(Days in Review)	0.542	0.962	0.539	0.937	0.539	0.937
	(0.020)	(0.131)	(0.020)	(0.130)	(0.020)	(0.130)
Plenary Expiration	-0.490	-1.650	-0.485	-1.602	-0.485	-1.624
	(0.345)	(1.516)	(0.345)	(1.504)	(0.345)	(1.515)
Minority	-0.478	-1.095	-0.449	-0.948	-0.449	-0.956
	(0.138)	(0.566)	(0.138)	(0.569)	(0.138)	(0.570)
Country						
Denmark	-3.016	-3.924	-2.978	-3.923	-2.979	-3.826
	(0.251)	(0.715)	(0.250)	(0.710)	(0.251)	(0.716)
Netherlands	0.076	0.247	0.104	0.175	0.103	0.309
	(0.082)	(0.414)	(0.081)	(0.414)	(0.082)	(0.417)
Intercept	-3.795	-8.014	-3.968	-8.218	-3.967	-8.554
	(0.265)	(1.335)	(0.268)	(1.359)	(0.271)	(1.368)
var(Random Intercepts: Bills)		9.871		9.491		9.708
		(1.029)		(0.991)		(1.013)
N	4324	4324	4324	4324	4324	4324
$ln(likelihood)$	-7193.404	-2651.798	-7187.626	-2660.856	-7187.626	-2649.403