

# Intergroup bias in rule enforcement: Evidence from floor debates

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## Abstract

In a variety of contexts, institutions accord individuals formal roles which require them to enforce rules without giving groups with whom they identify special treatment. But are such institutional roles likely to be successful? The study exploits a natural experiment in group affiliation and rule enforcement: parliamentary debates in Denmark, in which parliament chairmen drawn from parliamentary parties enforce speaking time. Analyzing 4,262 speeches from 2010 to 2013, each measured with second-level precision, the study provides evidence that debate speaking times are biased in favor of speakers belonging to the party of the presiding chairman. The results suggest that institutional roles cannot be assumed to be able to supersede preexisting group loyalties.

*Keywords:* legislatures, debates, intergroup bias, rule enforcement, social identity

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## 1. Introduction

In a variety of contexts, institutions accord individuals formal roles which require them to enforce rules governing the competing interest of societal groups. In some cases, by either design or necessity, these individuals are drawn from one of the groups subject to these rules. Hence, in order to enforce the rules in an unbiased manner, assuming the role requires the individual to disregard his or her group loyalties.

But are such institutional roles and norms likely to be successful? In this study, I exploit a natural experiment in rule enforcement where the enforcer belongs to a group subject to enforcement. The empirical setting is parliamentary debates in Denmark, in which speaking times, governed by a simple, uniform rule, are enforced by chairmen drawn from major political parties.<sup>1</sup> Since combinations of speakers and chairmen are effectively random, the setting allows for identifying whether chairmen disregard partisan loyalties in enforcing speaking time rules.

The evidence suggests they do not. Across a variety of specifications, chairmen accord significantly more speaking time to speakers of their own party (*copartisans*) than speakers from other parties (*non-copartisans*). Depending on the specifications, copartisans are accorded 1.7 to 3.8 seconds more speaking time per speech, of which there can be up to 748 during a single debate. The effect is moderate, but non-negligible, corresponding to 3.9 to 7.3 percent more speaking time allocated to co-partisan speakers per speech. The results suggest that institutional roles cannot be assumed to be able to supersede preexisting group loyalties, even in a highly consensual political environment characterized by strong legal-rational norms.

## 2. Existing literature

Substantively, the study ties into three distinct, but related, strands of research. First of all, *social identity theory* promotes the idea that, when perceiving the world in terms of ingroups and outgroups, humans are prone to discriminating against outgroup members and treating ingroup members favorably. This tendency is commonly referred to as intergroup bias (Hewstone et al., 2002). In lab experiments conducted under the *minimal group paradigm*, Tajfel and Turner (1979, 1986) show that this results holds even when the groups in question are minimally cohesive, put together just for the purpose of the experiment. By finding group-based discrimination in a setting where groups were clearly entirely arbitrary and cultivated with a near-minimum of socialization, Tajfel and Turner argued, the studies demonstrated the pervasive influence of intergroup bias on social behavior. Several other laboratory studies have since replicated Tajfel and Turner’s original results (Brewer, 1979; Abrams and Hogg, 1990; Brown, 2000; Chen and Li, 2009).

Second, the study ties into the empirical literature identifying *own-race bias in rule enforcement*. Some studies utilize data on referee judgments in sports. For example, Price and Wolfers (2010) show that NBA referees award more fouls against opposite-race players; for the average game, shifting from an all-white referee crew to an all-black crew, the chance of victory for the team with fewer black players is reduced by about three percentage points. Other studies provide evidence of racial profiling in policing (Donohue and Levitt, 2001; Antonovics and Knight, 2009). This literature provides evidence of intergroup bias outside the laboratory. Yet it largely leaves unresolved to what extent the effect is specific to the particular role of race in the United States.

Third, this study ties into the literature on *partisan bias in political governance*. In a variety of countries, studies have shown that the distribution of state-level funds sometimes

reflects partisan political concerns rather than social welfare or economic efficiency criteria (Dahlberg and Johansson, 2002; Stratmann and Baur, 2002; Golden and Picci, 2008; Larcinese et al., 2008), a phenomenon colloquially known as ‘pork-barrel politics’. In systems with politically appointed law enforcement, some studies suggest that prosecution patterns reflect partisan concerns (Gordon, 2009). Taken as a whole, the literature provides compelling evidence for the pervasiveness of pork-parrel politics. At the same time, studies attempting to infer partisan motives from policy outcomes remain vulnerable to concerns that the outcomes are explainable in terms of an omitted variable.

The study relates to each of these three literatures in distinct ways. While the empirical setting is in many ways analogous to the original minimal group paradigm experiments, it identifies intergroup bias in a natural setting, using unobtrusively obtained data, thus eschewing concerns about experimenter effects or lack of external validity. With respect to studies of racial bias in rule enforcement, it suggests that the effects these studies observe among sports referees and police officers is not specific to American race relations, but likely merely a localized case of intergroup bias in rule enforcement. Lastly, and perhaps most subtly, while it follows a in the heels of a substantial literature demonstrating partisan bias in political governance, it adds to previous studies of partisan bias by observing it directly in the behavior of elected officials, rather than indirectly inferring it from policy outcomes.

Methodologically, the study adds to a very sparse research literature using floor debates to study political behavior. The consensual view in the comparative politics literature is that floor debates in and of themselves serve no purpose of interest (Gallagher et al., 2005). Challenging this consensus, Martin and Vanberg (2008) use variation in the length of floor debates to show that coalition governments devote more time to debating bills on issues on which government is divided, particularly as election day approaches. The present

study builds and expands on Martin and Vanberg’s idea of using data from floor debate, demonstrating that floor debates can reveal patterns of not just political behavior but social behavior broadly construed. Because they pit actors with relatively well-defined identities and incentives against each other under a clear and commonly known set of rules, floor debates are an attractive empirical setting for studying social behavior under institutional constraint.

The paper is structured as follows. In section 3, I present the empirical setting. Section 4 presents the data obtained, and the statistical model I use to identify intergroup bias. Section 5 presents the results of the analysis. Finally, section 6 draws up the main conclusion and discusses perspectives for future research. In this last section I also discuss two important questions this study is not able to settle: the issue of intentionality and the role of self-censorship.

### **3. Empirical setting**

The empirical setting of this study is the Danish parliament, the *Folketing*. The *Folketing*’s legislative calendar is bracketed by two major debates: the *opening debate*, which takes place each year in early October, and the *closing debate*, which takes place the following year around the end of May. Major debates each begin with a speech by the Prime Minister, followed by speeches by party spokespersons in descending order of party size. However, the majority of the typical debate, which lasts between 12 and 16 hours, is spent on brief back-and-forth arguments between MP’s and party spokespersons. Hence, for the entire debate, the right to speak alternates quickly and continuously between parties.

Debates are governed by restrictive rules about speech time. The rules are as follows: each party spokesperson is allowed a speech up to 10 minutes long. After each speech, MP’s are allowed to make so-called ‘brief remarks’ (*korte bemærkninger*) of up to 60 seconds. Debate

continues around half an hour, after which the next party spokesperson gives his or her speech. Debates typically feature 600-800 brief remarks. Table 1 sums up each type of speech given; in the data used here, brief remarks account for 95 percent of all speeches across debates and 81 percent of all debating time.<sup>2</sup>

Table 1: Types of speeches in floor debates in the *Folketing*

Type	Number	Share (of number)	Share (of time)
Brief remark	8,363	95.3	81.2
PM speech	314	3.6	7.3
Spokesperson speech	101	1.2	11.5

The identification strategy of this study rests on the fact that debate rules are enforced in as-if-random order by ‘chairs’, i.e. members of the *Folketing* presidency, which consists of senior members from each of the five biggest parties. The ‘head chairman’, i.e. the head of Parliament, starts out, but the order of chairmen is otherwise arbitrary, determined by convenience. Hence, each chairman oversees a considerable amount of debate involving MP’s from his or her own party (in the following called *copartisans*) as well as MP’s from other parties (*non-copartisans*).<sup>3</sup>

The hypothesis tested here is whether chairmen, enforcing common rules about speech time, bias their enforcement to the benefit of copartisans. Conceptually, the hypothesis can be tested as follows:

*Hypothesis: Members’ speeches under copartisan chairmanship are on average longer than members’ speeches under non-copartisan chairmanship.*

The assumption implicit in the hypothesis is that any observable difference between speech lengths under copartisanship vis-à-vis non-copartisanship can be causally attributed

to copartisanship and not some omitted factor. This assumption is credible as long as the partisanship of the chair is *as-if randomly* assigned to individual speakers (Dunning, 2007). Insofar as this is the case, the setting can be characterized as a natural experiment.

As mentioned, the order of the chairmen's shifts is itself allocated as a matter of convenience in terms of chairmen's other private and professional obligations.<sup>4</sup> The order of chairmen's shifts is thus in itself plausibly random with respect to the debate schedule. This assumption is supported by the seemingly haphazard order of shifts observed in the data, shown in figure 3 below.

However, this does not rule out another challenge to as-if randomness, namely the possibility that speakers self-censor. Since speakers know the partisan affiliation of the chair, it is possible that they deliberately shorten their speeches under non-copartisan enforcement, anticipating that the non-copartisan chairman is biased against them.

The extent of self-censorship is difficult to assess even based on qualitative scrutiny of archived recordings of floor debates. Recordings of debates indicate that chairmen rarely interrupt MP's to stop speeches, which in turn suggests extensive self-censorship. However, chairmen will often stand up from their chair as a sign to the speaker that their time is up, a sign which is not shown in televised recordings. Neither are actual speech lengths conclusive. Though speech lengths shorter than 60 seconds would seem to suggest that speakers self-censor, chairmen are allowed to interrupt speakers before their time is up in the interest of allowing for more remarks.<sup>5</sup> Hence, though each of these factors suggest at least some self-censorship, its precise extent is difficult to assess from the data at hand. Section 6 discusses the implications of this for whether the observed pattern can be characterized as intergroup bias.

#### 4. Data and measurement

The data used in this study is obtained by extracting detailed information about individual speeches from online transcripts of opening and closing parliamentary debates since the opening debate in 2010.<sup>6</sup> The unique feature of this data is that the lengths of speeches is recorded down to the number of seconds, yielding highly precise measures of how much speaking time each debate participant is given. The full data set contains information about speaker and length for 8,778 speeches. Around half of these are merely transcriptions of chairmen calling the next speaker in line, yielding an effective N of 4,262 actual speeches by members of parliament. Table 2 summarizes the available data.

Table 2: Summary statistics

Variable	N	Mean	St. Dev.	Min	Max
Seconds	8,778	31.254	43.862	0	701
Copartisan	4,380	0.188	0.390	0	1
Spokesperson	8,778	0.012	0.109	0	1
Chair	8,778	0.501	0.500	0	1
PM	8,778	0.036	0.186	0	1
Left-wing MP	8,778	0.512	0.500	0	1
Left-wing chair	8,778	0.544	0.498	0	1
Co-bloc	4,380	0.506	0.500	0	1
Leadership party	8,778	0.884	0.320	0	1
Speech no.	8,778	319.917	190.319	1	766
Chair no.	8,778	4.923	2.717	1	10
Speaker party	8,778	-	-	-	-
Presiding chair party	8,778	-	-	-	-

Figure 3 in the appendix is an attempt to visualize the partisan distribution of speeches, showing the partisanship of each speech in all of the debates included in this study. The figure illustrates the rapid alternation between parties during the debate and the ensuing variation in chair-speaker copartisanship.

The key variables in the data are *Seconds*, which measures the length in seconds of each



speech, and *Copartisan*, a dummy indicator of whether the member speaking is a copartisan of the presiding chairman. Hence, the hypothesis presented above can be stated in its simplest regression model form as follows:

$$Y_i = \alpha + \beta CP_i + \gamma \mathbf{X}_i + \epsilon_i \quad (1)$$

Where  $Y_i$  is the length of speech  $i$  in seconds,  $CP_i$  is the *Copartisan* indicator for speech  $i$ , and  $\mathbf{X}_i$  is a vector of additional control variables including question order and various fixed effects terms. The hypothesis tested is whether the coefficient for  $CP_i$ , i.e.  $\beta$ , is positive.

## 5. Results

Since the dependent variable measuring speech length is continuous, the model presented in equation 1 is estimated using OLS. Before proceeding to the regression results, figure 1 shows the raw distribution of speech lengths for copartisans vis-à-vis non-copartisans.

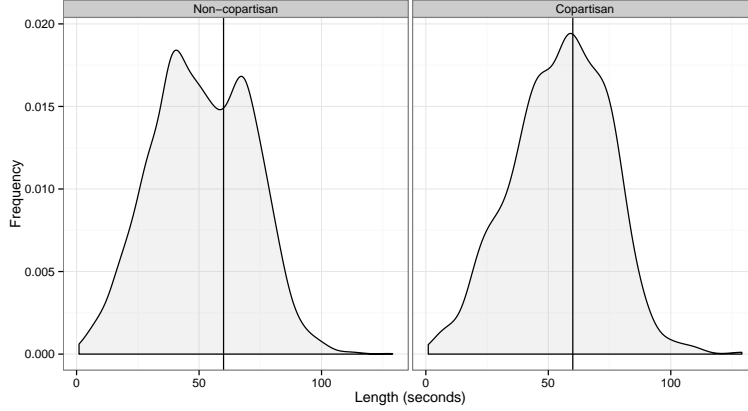


Figure 1: Distribution of speech lengths by speaker-chairman copartisanship. The vertical line indicates the 60-second limit for short speeches. Non-copartisans drop off before reaching the limit. No such pattern is apparent for copartisans.

The pattern is evident: the frequency of non-copartisan speeches drops markedly as speech lengths approach the 60-second limit. By contrast, copartisan speeches are almost perfectly symmetrically distributed around the limit. The figure lends *prima facie* support to the notion that speech lengths depend on the partisan relation between chair and speaker.

Table 3 presents estimations of various specifications of equation 1. Model 1 presents a bivariate regression of speech length on copartisanship; models 2-4 add the spokesperson variable as well as various controls. Accounting for the spokesperson role significantly increases the precision of the copartisan effect estimate, reducing the standard error of the estimate by half. Across models 2-4, the estimated effect of copartisanship is robustly significant and in the predicted direction.

Table 3: Effect of speaker co-partisanship on speech length

	Speech length, seconds			
	(1)	(2)	(3)	(4)
Co-partisan	3.76*** (0.77)	2.16** (0.87)	1.93** (0.86)	1.65* (0.95)
Speech order		-0.01*** (0.002)	-0.004** (0.002)	-0.002 (0.002)
Constant	51.26*** (0.34)	61.23*** (1.02)	48.30*** (1.57)	47.84*** (1.77)
N	4,262	4,262	4,262	2,989
R <sup>2</sup>	0.01	0.05	0.11	0.11
Adjusted R <sup>2</sup>	0.01	0.05	0.10	0.10
Residual Std. Error	19.79 (df = 4260)	19.34 (df = 4245)	18.80 (df = 4231)	18.65 (df = 2967)
F Statistic	23.52*** (df = 1; 4260)	14.83*** (df = 16; 4245)	17.11*** (df = 30; 4231)	17.49*** (df = 21; 2967)

\*p < .1; \*\*p < .05; \*\*\*p < .01

The results thus support the hypothesis that copartisanship is associated with longer speeches. The estimated effect of copartisanship ranges from 4.4 additional seconds (without party fixed effects) to 2.8 seconds (with party fixed effects). In absolute terms, the estimated effects thus seem fairly small, but the effect is equivalent to an appreciable ‘copartisanship premium’ of 5 to 8 percent of the expected speech length.

The models presented in table 3 also control for two possible confounders. For one, one

may suspect that the observed copartisanship effect reflects the fact that members of major parties, which hold the chairman positions, speak earlier in the day, and have more energy to use the full extent of their speaking time. Models 2-4, which include a speech order control, suggest that there is in fact a ‘fatigue effect’: speeches later in the day are slightly, but significantly, shorter. The latest speeches in the data are an estimated nearly 8 seconds shorter than the earliest. But controlling for order does not affect the significance of the estimated effect of copartisanship.

Second, one could suspect that the effect is driven by minor parties, which do not hold chairman positions, being awarded less speaking time as a matter of sheer proportionality. Such a difference could produce a spurious copartisanship effect. Model 4 accounts for this by estimating the model for only parties which hold chairman positions. As shown, the effect is undiminished.

Finally, the copartisanship effect could be driven solely by a few chairmen rather than reflecting broad-based bias. Estimating effects for individual chairmen would require more data than is available, but figure 2 provides a partial answer by estimating model 2 for left- and right-wing chairs separately. As shown, the effect is similar for left- and right-wing chairs, suggesting that the observed biased rule enforcement cuts across party lines.

## **6. Conclusion**

The key finding from this study is that speaking time rules in Danish parliamentary debates are not enforced equally. Specifically, speeches are significantly longer when the person speaking shares party affiliation with the chairman enforcing the rules.

The explanation proposed here is that this difference reflects intergroup bias on the part of the rule enforcer, i.e. the chairman. The intergroup bias explanation is consistent with a

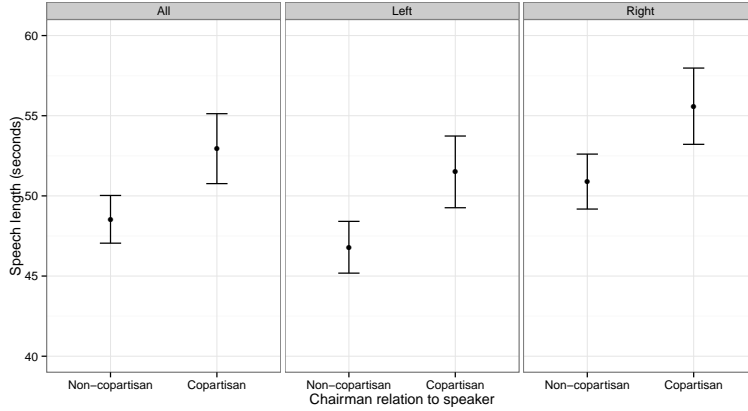


Figure 2: Speech lengths by speaker-chairman co-partisanship for all speakers, left-wing chairmen, and right-wing chairmen, respectively. As the figure indicates, intergroup bias is not exclusive to either political wing.

wealth of laboratory experimental evidence demonstrating the impact of group identity on distributional preferences. The study contributes to this literature by showing intergroup bias in a natural setting. Compared to other studies showing partisan bias in political governance, this study contributes by observing bias directly in the behavior of elected officials.

Two concerns about the nature of the observed effect linger. First, assuming chairs to treat copartisans and non-copartisans unequally, the study cannot identify whether the unequal treatment is intentional. While both intentional and unintentional mechanisms could meaningfully be conceived as intergroup bias, they point to very different substantive interpretations of the evidence. In one, chairmen deliberately skew the rule enforcement in favor of their own party in order to gain a political advantage. In the other, chairmen are subconsciously swayed by group identities, even while honestly trying to enforce the rules equally.

Second, and more importantly, the study cannot determine whether the observed difference reflects actual biased enforcement as opposed to speakers voluntarily cutting their speeches

short in anticipation of biased enforcement. In the latter, copartisans could theoretically gain an advantage solely by virtue of anticipating more lenient rule enforcement, even if rules are in fact enforced equally.

The ambiguity of the psychological mechanisms involved is in many ways the flipside of this study’s advantage, observing bias in a natural, political setting. In all likelihood, the real mechanism involves both intentional and unintentional bias and actual biased enforcement as well as some degree of partisan self-censorship. Regardless of the specific composition of these mechanisms, the conclusion remains that even when institutional norms militate strongly against bias, governance entirely blind to group loyalties remains an elusive ideal.

## Notes

<sup>1</sup>In the interest of fluency, the paper refers to both chairmen and -women using male pronouns, reflecting the model speaker gender.

<sup>2</sup>The number of PM speeches in table 1 is overestimated, since brief remarks by the PM are classified as PM speeches. As shown in table 3, results are robust to the exclusion of all PM speeches.

<sup>3</sup>The distinction borrows from Habyarimana et al. (2007), who distinguish between *coethnics* and *non-coethnics* in experimental studies of group identity and rule enforcement.

<sup>4</sup>Personal correspondence with *Folketinget*’s legal office.

<sup>5</sup>Source: Personal correspondence with the *Folketing*’s legal office.

<sup>6</sup>Transcripts were accessed at <http://www.ft.dk/Dokumenter.aspx> and scraped for information using the statistical software R. Transcripts from earlier debates do not provide second-level data and are thus not included.

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## Appendix

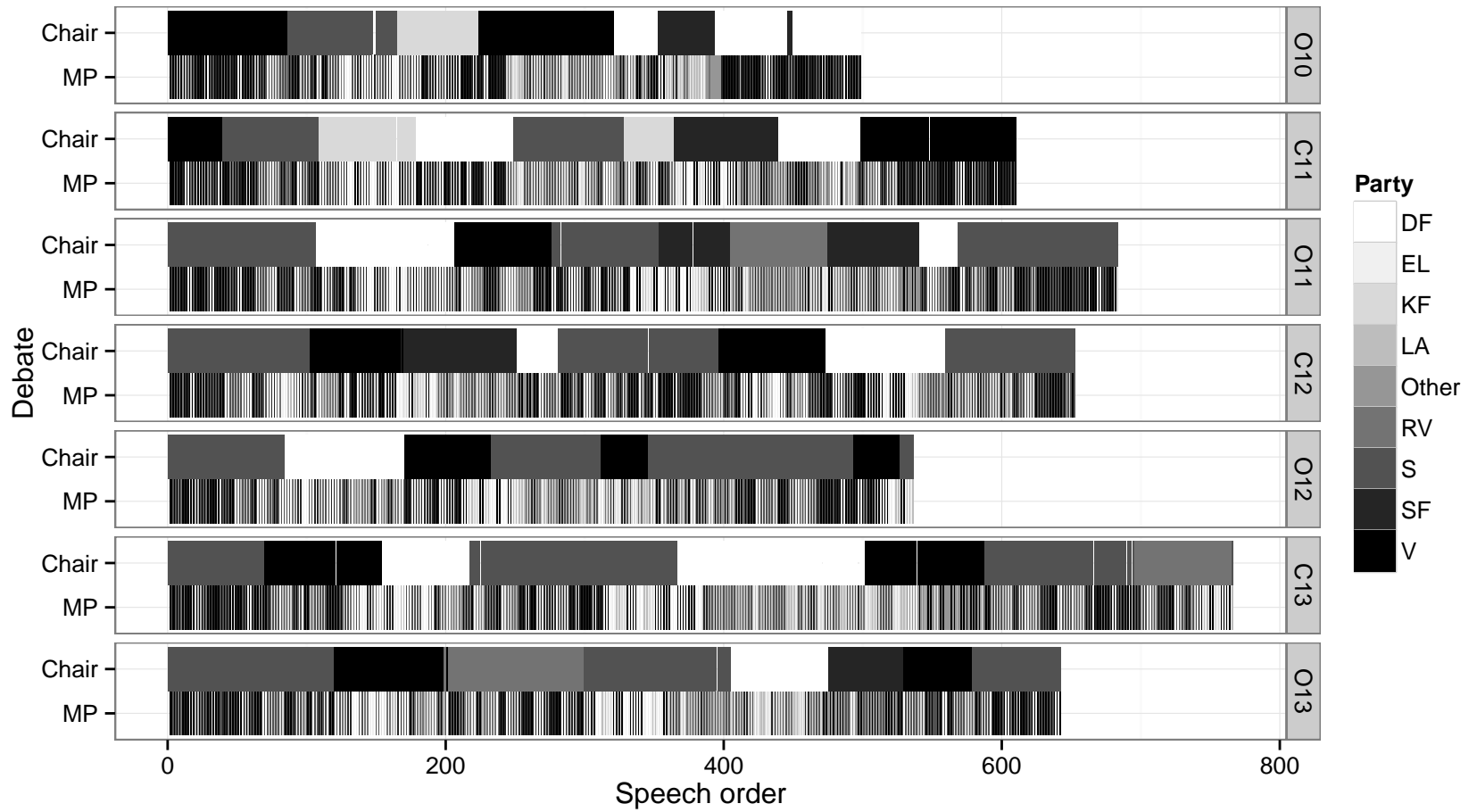


Figure 3: Partisanship of chairmen and MP's across debates. Although the head chairman always takes the first shift, the rest of the chairman schedule exhibits no systematic pattern.

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	Speech length, seconds		
	(1)	(2)	(3)
Co-partisan	3.76*** (0.77)	2.16** (0.87)	1.93** (0.86)
Speech order		-0.01*** (0.002)	-0.004** (0.002)
EL		-13.30*** (1.30)	-11.78*** (1.29)
IA		-9.82*** (3.18)	-8.27*** (3.17)
JF		-10.29** (4.15)	-3.70 (4.51)
KD		-7.02 (4.43)	-10.04** (4.34)
KF		-7.83*** (1.40)	-6.50*** (1.40)
LA		-7.77*** (1.39)	-5.57*** (1.39)
RV		-12.08*** (1.38)	-11.37*** (1.35)
S		-6.82*** (1.17)	-5.73*** (1.16)
SF		-1.91 (1.33)	-1.31 (1.30)
SIU		-9.89** (3.86)	-6.45* (3.89)
SP		-4.17 (4.16)	-0.87 (4.20)
T		-2.14 (11.21)	-5.14 (10.93)
TF		-5.50 (9.72)	-8.11 (9.48)
V		-5.99*** (1.12)	-4.97*** (1.12)
Soeren Espersen			13.46*** (1.48)
Bent Boegsted			17.76*** (2.42)
John Dyrby Paulsen			3.31 (2.85)
Karen J. Klint			19.21*** (3.14)
Anne Baastrup			2.73 (3.24)
Helge Adam Moeller			19.84*** (1.93)
Holger K. Nielsen			8.90*** (1.80)
Bertel Haarder			11.30*** (1.40)
Mogens Lykketoft			16.88*** (1.76)
Mogens Lykketoft			7.90*** (1.30)
Thor Pedersen			15.68*** (1.59)
Camilla Hersom			2.71 (1.88)
Holger K. Nielsen			12.75*** (2.15)
Marianne Jelved			13.03*** (2.58)
Constant	51.26*** (0.34)	61.23*** (1.02)	48.30*** (1.57)
N	4,262	4,262	4,262
R <sup>2</sup>	0.01	0.05	0.11
Adjusted R <sup>2</sup>	0.01	0.05	0.10
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