

Online Appendix to  
**Heuristic Inference and the Left-Right: An Experimental Analysis of  
How Voters Form Their Left-Right Images of Parties**

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## **Appendix to Chapter 1**

### **1.1. Additional Detail about footnote 2**

Most definitions of heuristics define them relative to some imagined “full information” alternative. However, this alternative is seldom well-defined. The most natural (and general) of such alternatives, however, is simply the inferential process in which an individual knows all the relevant cue values and integrates them in a way that mirrors the real-world process that generates the target of inference. This puts us at odds with, for example, Shah and Oppenheimer (2008) who suggest that the benchmark inferential process to which heuristics should be compared is a linear-additive integration of all relevant cues (with cue weights). Rather, we would argue that one of the ways individuals can use heuristics to reduce effort is exactly by simplifying a complex mental model of some process (matching the real-world process governing the process) to a simple, linear additive, one.

### **1.2. Additional Detail about footnote 3**

The idea of a cue’s weight in the real-world process driving outcomes is a bit tricky since the whole concept of a cue weight fits most naturally in outcome processes that are linear and additive. However, even for outcome processes that are not linear and additive, we can imagine that the role any given cue plays in such a process implies a “weight function” for the cue, where the weight function need not be a single number but can be a function that maps how the cue impacts the outcome in potentially complex ways – including in ways that depend on other cues. Thus, if a heuristic theory in some way relies on the concept of the “value of a cue weight in the real world” (as ecologically rational heuristics in which individuals come to know the values of these weights subconsciously do) then we need to think of the “value of the weight in the real world” not as the potentially complex weight function but rather the empirical correlation between a cue and the outcome, given the function that describes its real impact on outcomes (where, in general, the correlations could be conditional on other cues, etc.).

### **1.3. Additional Detail about footnote 4**

Relevant citations include: Rieskamp and Otto 2006, Matton et al. 2013, Pohl, et al. 2017, Zendeihrouh and Ahmadabadi 2020.

### **1.4. Additional Detail about footnote 6**

Citations include Castles and Mair 1984, Hooghe et al. 2002, Budge et al. 1987, Budge and Laver 1992, Budge et al. 2001, Noel and Therien 2008.

### 1.5. Additional Detail about footnote 8

Citations relevant to the historical and philosophical roots of the left-right as used by elites: Laponce 1981, Bobbio 1996, Gauchet 1996, Noël and Thérien 2008, Cochrane 2015. Work that has tried to measure the elite consensus about parties left-right positions using roll call votes: Poole and Rosenthal 1997, Clinton et al. 2004; legislative speeches: Lauderdale and Herzog 2016, Schwarz et al. 2017; elite interviews: Freire and Belchior 2013, Todosijević 2004; expert surveys: Benoit and Laver 2006, 2007, Steenbergen and Marks 2007, Bakker et al. 2022; party manifestos: Budge et al. 2001, Klingemann et al. 1994, 2006; and campaign rhetoric: Sim et al. 2013, Somer-Topcu et al. 2020, Jung and Somer-Topcu, 2022.

### 1.6. Additional Detail about footnote 9

Exact quotes from relevant citations: Downs 1957, Fuchs and Klingemann 1990, Inglehart 1990, van der Eijk et al. 2005, and Dalton 2008.

### 1.7. Additional Detail about responses to Adams (2011)

Relevant citations: Fortunato and Stevenson 2013, Fernando-Vasquez 2014, Adams et al. 2016, Spoon and Klüver 2017, Plescia and Staniek, 2017, Seeberg et al. 2017, Falco-Gimenez and Munoz 2017, Giebler et al. 2021; Somer-Topcu 2017, Fernandez-Vazquez and Somer-Topcu 2019, Adams et al. 2020.

### 1.8. Additional Detail about footnote 10

Inglehart and Klingemann 1976, Klingemann 1979, Huber 1989, Knutsen 1995, 1997, Freire 2006, 2008, Medina 2015, Vegetti and Sirinic 2019.

### 1.9. Additional Detail about footnote 11

Huber and Inglehart 1995, Bakker et al. 2014, Bakker et al 2022, Benoit and Laver 2006, McElroy and Benoit 2010, Rohrschneider and Whitefield 2012, Polk et al. 2017, Somer-Topcu, Tavits, and Baumann 2020, Budge and Farlie 1983, Laver 2005.

### 1.10. Additional Detail about footnote 12

Fortunato et al. (2016) calculated the Spearman's rank correlation between the ordering of parties on the general left-right dimension for each of 14 policy dimensions from 1992-2004 party manifestos. Of the 14 Western democracies they examined, Germany had the highest correlation (0.5) and Finland had the lowest (0.25). The two other countries we examine in this

paper, the UK and Canada where in the lower half of all the countries at 0.40 and 0.37, respectively.

### 1.11. Additional Detail about footnote 13

Lipset et al. 1954, Rokeach 1973, Lipset and Raab 1978, McClosky and Zaller 1984, Inglehart 1990, Bobbio 1996, Gauchet 1996, Noël and Thérien 2008, Freire and Belchior 2011, Cochrane 2015, Erikson and Tedin 2019, Jost 2021.

### 1.12. Additional Detail about footnote 14

Altemeyer, 1998; Duckitt, 2001; Jost et al. 2003; Thorisdottir et al. 2007.

### 1.13. Additional Detail about footnote 16

Another body of scholarship that suggests the importance of values vs. policies comes from the literature on voters' left-right self-placements, where political scientists like Knutsen (1995, 1997) have championed the importance of values in structuring individual's personal ideology. Likewise, Jost (2021), Goren et al. (2022), and others have connected personal left-right self-images to the large international (and cross-disciplinary) literature on individual "basic values," arguing that these values are pre-cursors of (and perhaps substitutes for) the kind of well-articulated ideological systems Converse found lacking.

### 1.14. Additional Detail about footnote 18

Work has shown that voters perceive parties' left-right positions to be closer together when those parties join formal governing coalitions: Fortunato and Stevenson 2013, Fortunato and Adams 2015, Fernandez-Vazquez 2014, Adams et al. 2016, Spoon and Klüver 2017; Work showing that this effect increases the longer the coalitions last: Fortunato 2012; and that this effect also occurs when cooperation is informal: Falco-Gimeno and Munoz 2017, Adams et al. 2021, Santoso et al. 2024.

### 1.15. Additional Detail about footnote 20

Duch and Stevenson (2008. Table 10.1) reported on the accuracy of voter expectations about which cabinets were likely to form in three elections in Norway, Denmark, and the Netherlands. Fortunato and Stevenson (2013) reported data from an original British survey done in 2012 and also identified surveys in the Netherlands (1998), New Zealand (1998) that had asked some relevant questions. In addition, they summarize results from cross-national survey of five eastern European countries. Further, few of these questions have been carefully examined in published research. This all led Rapeli (2016) to conclude that the Fortunato and Stevenson's

(2013) piece was the only published assessment of the question. Rapeli himself, however, added a careful analysis of a 2008 Finish survey that asked the necessary questions. Finally, Lin et al. (2025) have collected evidence, in a manuscript in progress, from surveys they have commissioned over the last decade that have asked relevant questions.

#### 1.16. Additional Detail about footnote 21

Relevant citations include: Bawn et al. 2012, Røed, et al. 2023, Allern and Bale 2017, McAdam and Tarrow 2010.

#### 1.17. Additional Detail about footnote 22

Empirical work that has demonstrated that many of the traditional bonds between social groups and parties have weakened over time: Nieubeerta and Ultee 1999, Evans and Dirk de Graaf 2013, Van der Brug and Rekker 2021, Knutsen 2010. Work showing that these bonds have only partially been replaced by bonds with new social groups: Ford and Jennings 2020.

#### 1.18. Additional Detail about footnote 23

Conjoint survey experiments by Goggin et al. (2020) demonstrate that American voters, including those with relatively low political knowledge, can accurately infer partisan affiliations from randomized candidate profiles varying in demographics (race, gender, religiosity, socioeconomic status). Ahler and Sood (2018) use surveys asking Americans to guess the percentage of each party composed of specific social groups, revealing widespread (albeit exaggerated) perceptions of American parties' basis of social support. Rothschild et al. (2019) employ open-ended survey questions to show that Americans spontaneously associate parties with particular demographic and social groups. Kane et al. (2021) utilize structured survey methods to measure American voters' explicit knowledge of partisan alignments with social groups, confirming extensive public awareness linked to elite polarization.

#### 1.19. Additional Detail about footnote 24

Konicki's (2025) review includes studies by Ahler and Sood (2018), Kane et al. (2021), Mason and Wronski (2018), Rothschild et al. (2019), Elder and O'Brian (2022), and Westwood and Peterson (2022).

#### 1.20. Additional Detail about footnote 25

Harteveld's 2021 study examines the relationship between social sorting and affective polarization across 40 countries, highlighting that greater alignment of partisan divisions with non-political divisions (e.g., income, education, religion, and region) correlates with stronger

dislike towards political outgroups. Similarly, Sczepanski's (2024) research investigates how perceived social sorting influences European and national identities in Austria and Italy. The study finds that individuals who perceive their favored social groups as supporting pro-European positions are more likely to adopt a strong European identity.

### 1.21. Additional Detail about footnote 26

The theory of ecological rationality actually suggests a more complicated measure. The theory is about the way that a pattern of social support cues impact voter's left-right images of parties, so the relevant empirical correlation that can vary across different specific social group cues is the association between the support of a group for each party and the party's true left-right position. A direct measure of that could be constructed by (1) estimating the extent to which each social group supports each party (so that a given group's support across parties would sum to 100%); and (2) using that as a weight on (and estimate of) each party's true left-right position. The more this weight deviates from the middle of the left-right scale, the more the group's pattern of support for parties would be more purely left or right and the clearer the signal of its support for the parties' left-right positions. Unfortunately, producing such estimates depends on having a good estimate of each group's support for each party, which (with the sample sizes we have available in our data) is not really possible. As such, we use the evidence in Figure 1.2 as a proxy. This will be a good proxy as long as groups that strongly prefer parties of the left (right) have average left-right self-placements that tend in the same direction. We think this is quite likely to be true and so the characterizations in Figure 1.2 are likely a good guide to which social group signals are likely to get the most weight in ecologically rational voters' left-right images of parties.

## Appendix to Chapter 2

### 2.1. Differences Between Research Designs in Left-Right Self-Placement Literature and Our Study

The empirical literature on left-right self-placement, like the typical research designs used in the literature on voters' left-right images of parties, is survey-based and observational. As such, researchers model the voter's left-right images of parties as a function of various variables and assumes that "the better a variable (or group of variables) predicts left-right positions [self-placements], the greater the contribution of that factor to citizens' understanding of what left and right mean." (Veggetti and Sirinic 2018: 9). Unsurprisingly, in these observational designs, empirical correlations among key predictors often lead to disagreements regarding the most influential factors shaping left-right self-placements. This makes the empirical models in this literature somewhat fragile, rendering inferences susceptible to small changes in model specifications, and fueling empirical disagreements. For example, there is a long-running debate in this literature about the relative impact that a person's party identification, policy preferences, and social position have on her left-right self-placement – with different empirical studies coming to different conclusions and a raft of arguments about research design, definitions of concepts, and the best statistical methods to use (Sani and Sartori 1983, Huber 1989, Fuchs and Klingemann, 1990, Knutsen 1995, 1997, Medina 2015, Veggetti and Sirinic 2019, Freire 2006).

Such mixed results are not surprising, given that many of the posited causal variables measured in these observational studies are deeply interrelated (e.g., a person's position in the social structure, policy preferences, and partisanship) so that independent variation in key measures is limited and potential unmeasured confounders abound.<sup>1</sup> Even more fundamentally, studies of left-right self-placement inevitably raise questions about a person's ideological identity – that is how they think of themselves, not (just) how they use left-right to understand the political world. While relevant, our main empirical goal in this Element differs fundamentally from the objectives of much of this literature. Studies of self-placement seek to understand how individuals position themselves on a left-right scale. On the other hand, our focus is on understanding how individuals place parties, not themselves, on the same scale. In contrast, when we consider voters' placement of parties, the situation becomes more complex. Unlike

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<sup>1</sup> Keele, Stevenson, and Elwert (2020) point out another problem with this kind of work: the tendency to interpret all the estimated associations in a single empirical model as causal, without paying attention to whether each such causal interpretation is justified by an appropriate causal diagram and corresponding set of control variables (Pearl 2009).

self-placement, where an individual's position is an intrinsic assessment of their own ideological leanings, placing parties involves a comparative judgment between political entities. This judgment is shaped not only by policy preferences and values of the parties but also by perceptions of inter-party relationships—particularly cooperation and conflict. As outlined in our theory in Chapter 1, one of the ways voters place parties on the left-right spectrum are influenced by how they perceived the cooperative or conflictual behavior of parties with one another. For instance, an individual may position two parties on the scale based on their understanding of how these parties interact with each other, particularly in coalition politics. The dynamics of partisan cooperation (i.e., how parties collaborate or oppose one another) introduces a level of complexity that does not present in the act of self-placement.

## 2.2. Survey Information and Conjoint Items

**Table A2.1. Survey fieldwork and samples**

	Country	Fieldwork	Sample
Pilot Study	Canada	Nov. 27 - Dec. 5, 2017	1105
	Germany	Mar. 28 - Apr. 5, 2018	1039
	UK	Nov. 27 - Dec. 5, 2017	1025
Main Study	Canada	Mar. 26 - May 1, 2019	1000
	Denmark	Oct. 30 - Nov. 30, 2019	1278
	Germany	Aug. 8 - Sep. 5, 2019	1017
	UK	Jan. 24 - Feb. 5, 2019	995

**Table A2.2. Attribute categories, attributes, and attribute levels**

<b>Slot 3 (Cooperation)</b>	
<b>Attribute</b>	<b>Attribute Levels</b>
Join /Support (CA) [INCUMBENT PARTY/Main Opposition Party]-led coalition/minority govt (CA)	Would join (support)/ Would not join (support)
Cooperates with [Target Party <sup>2</sup> ]	Often/Seldom cooperates
<b>Slot 4 (Policies and Values)</b>	
<b>Attribute category: Policies</b>	
Cutting taxes even if it leads to less public spending	Support/Oppose
Cutting taxes even if it leads to more public debt	Support/Oppose
Increasing govt regulation of financial markets	Support/Oppose
Increasing govt spending of the military	Support/Oppose
Increasing taxation of fossil fuels	Support/Oppose
Increase public spending on healthcare and pensions <sup>3</sup>	Support/Oppose
Limiting non-western immigration	Support/Oppose
Limit/Expand govt surveillance in public spaces	Support/Oppose
Legalization of marijuana use (CA, DE, DK)	Support/Oppose
Strengthening cooperation in the EU (DE, DK)	Support/Oppose
Policies that result in hard Brexit (UK)	Support/Oppose
<b>Attribute category: Values</b>	

<sup>2</sup> Target Parties used are: Canada: Cons, Lib, NDP, BQ, Green, Germany: CDU/CSU, SPD, FDP, AfD, Green, Linke, Denmark: Ven (Liberals), Sd (Social Democrats), DF (Danish People), Enh (Red-Green Alliance), Lib (Liberal Alliance), RadVen (Social Liberal), SF (Socialist People), KF (Conservative People), Alt (Alternative), NB (New Right), UK: Cons, Lab, Libdem, SNP, UKIP, Green.

<sup>3</sup> In Denmark, the statement only describes a party's position on old-age pensions.

Income redistribution	Society works better when wealth is more equally distributed/ those who are most productive get most of the wealth
Cultural diversity	Cultural diversity makes the nation stronger/ weaker
Civil liberty	Protecting civil liberties (improving national security) is more important than improving national security (protecting civil liberties)
Government's role	Govt should play a greater/ smaller role in the management of the economy
Gender inequality	Men and women should be treated equally in economic and public life/ There are legitimate reasons to treat men and women differently in economic and public life
<b>Slot 5 (Size, Scope, and Gender)</b>	
Geographical scope of party support	Draw support from only one region of the country/from many different regions of the country
Women's candidates	10%/30%/50%/70%
Gender quota	Party rule that requires 50%/70% female candidates
Party size in the next General/Federal election	Expected to gain no seats/ be the 3rd/4th/5th largest parties
<b>Slot 6 (Social Group)</b>	
Support among men/ women/ older/ younger/ union households/ university graduates/ rural/ urban/ suburban <sup>4</sup> / religious/ working class/ middle class/ upper class/ voters in Quebec (CA)	Support/Oppose

Note: CA=Canada, DE=Germany, DK=Denmark, UK=United Kingdom

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<sup>4</sup> On the advice of our German colleagues and translators, we do not ask about suburban voters' category in Germany.

## 2.3. The Samples

The set of surveys we fielded for this project included three pilot studies conducted in Canada, Germany, and the UK in 2017 and 2018, and four “main” surveys conducted in the UK, Canada, Germany, and Denmark in 2019. For each survey, we recruited about 1000 respondents from Qualtrics LLC’s online panels. Quotas were imposed on age and gender targeting census proportions for males and females and a set of discrete age categories in each country (see Table A2.1 in Online Appendix 2.2 for additional information).

Our participants were not informed in advance that the survey was about politics. Likewise, we prevented the same respondent from participating in both the pilot and main rounds of the surveys. Survey non-compliance was identified using a variety of strategies (e.g., straight-liners and speeders) and these respondents were excluded from the sample.

The tables below compare the average values of a variety of demographic and political variables between our surveys, relevant census data, and the CSES surveys that were most proximate to ours.

**Table A2.3: Survey and election timing**

	Canada	Denmark	Germany	UK
General election timing	Oct 2015 (next: Oct 2019)	June 2019 *EP May 2019	Sep 2017 *EP May 2019	June 2017 (next: Dec 2019)
<b>Our survey</b> (# of months since last election)	<b>Mar/Apr 2019</b> (3+ years)	<b>Nov 2019</b> (5 months) *EP: 6 months	<b>Aug 2019</b> (23 months) *EP: 3 months	<b>Jan/Feb 2019</b> (1.5 years)
CSES (# of months since last election)	Oct-Nov 2019 (3+years)	Jun-Sept 2019 (0-3 months) *EP: 1-4 months	Sept-Nov 2017 (0-3 months)	Jun-Sept 2017 (0-3 months)

\*EP indicates the European parliamentary election

**Table A2.4: Percent male**

	Canada	Denmark	Germany	UK
Most recent census	50	50	49	49
<b>Our survey</b>	<b>50</b>	<b>48</b>	<b>50</b>	<b>50</b>
CSES	57	49	52	42

**Table A2.5: Percent with tertiary education**

	Canada	Denmark	Germany	UK
Census	34	44 or 36 <sup>5</sup>	29	34
<b>Our survey</b>	<b>40</b>	<b>42</b>	<b>21 or 27<sup>6</sup></b>	<b>39</b>
CSES	45	47	41	31.5

**Table A2.6: Percent choosing the top two political interest categories, i.e., very interested + fairly interested (% very interested in parentheses)**

	Canada	Denmark	Germany	UK
<b>Our survey</b>	<b>70 (24)</b>	<b>70 (28)</b>	<b>84 (46)</b>	<b>73 (29)</b>
CSES	78 (45)	75 (27)	80 (39)	73 (25)

**Table A2.7: Percent responding positively to either the 1<sup>st</sup> or 2<sup>nd</sup> follow-up question**

	Canada	Denmark	Germany	UK
<b>Our survey</b>	<b>87</b>	<b>91</b>	<b>87</b>	<b>91</b>
CSES	76	75	69	81

There are several significant differences in the way that we asked the party identification question and the CSES versions to which we are comparing. First, while the CSES starts by asking if the respondent is “close to any party?” (yes, no), we ask “Regardless of which party you would vote for in any particular election, do you, generally speaking, identify with the [list of the main parties], or what?” Next, both the CSES and our survey follow up with a question asking those who indicate they are not close to a party or do not identify with one, whether you “generally think of yourself as a little closer to one of the parties than the others?” The CSES language for this question is quite similar. Given these differences, we should temper our expectations about the extent to which our absolute levels of partisanship will match the CSES. In contrast, for questions like political interest, in which we use the exact same format as CSES, we should expect a tighter correspondence.

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<sup>5</sup> Depending on source of census information.

<sup>6</sup> 27% if we include certified technical education. It is more than secondary education but less than a full university education. Census number does not provide detailed breakdowns, so there is some uncertainty about which categories are included as tertiary education in the census.

**Table A2.8: Percent Rs who indicate they are “very” or “fairly strong” identifier (“very close” or “somewhat close” for CSES)**

	Canada	Denmark	Germany	UK
<b>Our survey</b>	<b>60 (15)</b>	<b>72 (18)</b>	<b>56 (14)</b>	<b>59 (14)</b>
CSES	67 (14)	51 (7)	51 (7)	64 (11)

Note: We only ask the strength of partisanship question to those who answered the initial party id question affirmatively, but CSES asks this question of respondents who answered either the initial or follow up question affirmatively.

## 2.4. Profile Distributions

In this appendix, we examine the profile distributions used in our analysis and assess whether the uniform distributions we applied to key attributes of new parties provide a reasonable basis for inference. Specifically, we address whether assuming an equal probability for different attribute levels is warranted. Importantly, since our inferences about each specific party attribute depends on the profile distribution defined by the other party attributes, we include discussion here of all the attributes used in the study, not just the ones we focus on in this Element (e.g., the gender of the party leader or the size of the party).

To do so, we ask:

Are we satisfied with inferences about the impact of a given attribute on left-right placements of new parties that are only valid in a world in which new parties are equally likely to...

a. Support or oppose a given policy or value?

Since we chose the policy (and value) statements included in the surveys precisely because they were active areas of political conflict among existing parties, the answer is clearly yes. It is very easy to imagine real parties that could emerge on either side of each issue. Further, since we conducted a complete observational analysis that paralleled our conjoint experiment, we know the distribution of perceived policy positions of all the existing parties in each country on each of our policy and values statements. This evidence largely confirms that the respondents in our surveys thought that the support and opposing position on each policy was about equally represented among the existing parties at the time of the survey. In no case did less than 30% of respondent-party placements fall on one side of the middle position on the scale and most

often this proportion was close to 50% (since these statements only had two levels – support and oppose – this is the relevant comparison).<sup>7</sup>

b. Strongly or weakly supported by a given social group included in our study?

We can easily imagine the kinds of new parties that would be strongly or weakly supported by different social groups. For example, we have no reason to suppose that the supply of new parties that would appeal to (the “social group” of) educated voters is any more or less than the supply that would appeal to less educated voters. The same logic applies to all of the large (and common) social groups included in our analysis. That is, if imposing a uniform distribution over weak and strong support for a new party from one of these social groups means our inferences are only applicable to a world in which this assumption holds, we are happy to impose this condition. Further, since we only included large and common social groups in the study, this assumption is even more plausible. For example, we would likely not be satisfied with this assumption had we included fringe social groups (e.g., Satanists) in the study. For these social groups, we could not reasonably expect any (or at least not half) of plausible new parties to appeal to these groups and so would not expect them to support and oppose new parties with about equal probability.

c. Often or seldom cooperate with each of the existing parties?

While our main concern is exploring how adjustment to the profile distribution used to estimate the impact of partisan cooperation matters to our inferences about those variables, it’s worth thinking for a moment about whether the uniform distribution for these variables is satisfying (i.e., an equal chance that a new party would often or seldom cooperate with a given existing

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<sup>7</sup> It is also a mistake to pretend that this data is definitive on the question and so should only complement the logical argument that we would expect new parties to emerge on both sides of these issues. Specifically, these data are only about existing parties that were successful enough to be included in the study and so did not include several small parties in each system. Further, the designation of the midpoint of a seven-point policy scale as the cut-off between the dichotomous categories of support and oppose the policy is not obviously correct. The end point on each of our scales were labelled with the corresponding statement that appeared in the conjoint (i.e., opposes/supports increased public spending on healthcare and pensions) but the middle point was not labeled as the neutral position. If our respondents thought, for example that the dividing line between parties that support vs oppose a policy in their country a bit more right or left, then this would alter the relevant distribution we should examine. All of this is simply to say, there is no point in pretending to precision in our understanding of the relevant empirical distributions (even for existing parties, which itself is not really the most relevant target “population” which ought to be new parties). Instead, if we have no logical reason to worry that one side of a policy would be shunned by new parties and no very clear evidence of lopsided representation of a policy position among existing parties, then we should be satisfied with the uniform assumption.

party). After all, our inferences about the impact of the other variables depend on this distribution.<sup>8</sup> For the major existing parties on the left and right, as well as any other existing parties that regularly cooperate with at least some other parties, it's hard to imagine a world in which new parties are systematically more likely to cooperate (or not) with these parties. Certainly, some would likely cooperate only with rightist parties and others only with leftists, but over the whole set of imagined new parties, we have no reason to think an existing party that is willing to cooperate at all with other parties would not find some new parties to work with while rejecting working with others.

There are two caveats to this argument. First, there are certainly some existing parties that do not cooperate very much with any party. Anti-system parties, or pariah parties, can be shunned by most other parties and so it is likely a random new party would be much less likely to "often cooperate" with such a party than to "seldom cooperate" with it. Usefully, our surveys included a question which asked respondents to rate, on a scale of 0-10, the extent to which each pair of existing parties in the country tended to cooperate. In most cases, respondents thought that a given party cooperated with some parties but not with others (e.g., the median cooperation score for CDU-SPD was 7 but only 3 for CDU-Linke). However, across all four countries there was one party that cooperated with no other party (in the view of our respondents). This party was the German AfD, which had median cooperation scores of 1, 0, 1, 0, 0 with the five other German parties. Thus, in analyzing the impact of variables other than cooperation variables in Germany, it would be prudent to adjust the uniform distribution over seldom and often cooperating with the AfD to, perhaps, a 90/10 split. Of course, if it's possible a party cooperates with no one, it is also possible some parties cooperate with everyone. We have no existing parties, however, for which this is the case. While our respondents often assigned middle cooperation scores to most dyads involving a party, this is indicative of the opinion that the party is about equally likely to cooperate with the party as not and not that it always can be expected to cooperate with any party. Indeed, we have no cases in which the median cooperation scores for each dyad involving a party are above 6 on our 10-point scale.

d. Be led by a woman or man?

Our respondents were equally likely to see a new party that was headed by a woman or a man and, perhaps surprisingly to some, this comports well with the empirical record. Specifically, the percentage of female leaders among the existing parties that were included in our survey (at the time of our fieldwork) was 40% Canada, 40% for Denmark, 67% for Germany, and 50% for the UK. These facts, as well as the clear trend toward more equal representation of women in

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<sup>8</sup> Keep in mind that the cooperation variables are all in the same "slot" in our analysis, so inferences about the effect of one of these variables (i.e., cooperation with the Conservatives) does not depend on the profile distribution of other cooperation variables (i.e., cooperation with Labour).

the top leadership of parties suggests we should be satisfied with inferences that assume an equal for distribution of this variable.

e. Have no seats in the legislature, be the 5<sup>th</sup> largest, 4<sup>th</sup> largest, or 3<sup>rd</sup> largest?

In our choice of new party sizes, we only included the possibility that a new party would be relatively small and so we have already included in the design a distribution of party sizes tailored to the reality of how big parties tend to be when they first enter electoral politics. Still, we did assume a uniform distribution of party sizes among the categories we included. To examine the plausibility of this assumption, we looked back at all the new parties that entered politics in our countries since 2000 (and that eventually gained seats in the legislature) and determined where they fit into the categories above. We found that among the 5 new parties to gain legislative seats during this period in our countries, 2 became the 3<sup>rd</sup> largest, one became the 5<sup>th</sup> largest, one the 6<sup>th</sup> largest, and one the 7<sup>th</sup> largest. Thus, our choice of an equal chance that a new party gaining seats in the legislature might enter somewhere lower than third (with roughly equal chance) seems reasonable.

A different conclusion, however, applies to the category of “no seats.” First, several of the parties included above actually entered politics in an election in which they gained no seats and only gained seats in the subsequent election. Further, there are many examples of parties that ran in election and gained no legislative seats. Thus, it does seem to be a departure from reality to assign the same probability to the attribute level “no seats” as the others. Thus, in the target distribution we use below, we adjust this probability downwards so that any given party has an 80% chance of not obtaining a seat but, conditional on obtaining a seat the exact size is uniform.

f. Draw support for a single vs. multiple regions of the country?

In our study, respondents had an equal chance of being told a party was a regionally based party vs. a national party. Given the fact that it is easier for a new party to organize and compete regionally than across the whole country, this seems a plausible assumption. Indeed, our worry should not be that the uniform distribution gives too high a probability to regional parties but that it is too low. Here, however, we can look to the actual experience of new parties support (using the same parties discussed above) and we find that in fact it’s not uncommon for new parties to draw support from more than one region (which is the exact wording used in our survey) rather than only one. Thus, we are satisfied with the uniform distribution in this case.

g. Have 10%, 30%, 50%, or 70% of female candidates?

Data on the percentage of female candidates from the existing parties in our sample are not generally available, but O’Brien (2020) has collected the number of female MPs by party in recent elections. This data confirms a great deal of variability across different parties that

broadly covers the categories (10%-70%) we included in our design. For example, after the most recent German election, the AfD had only 12% female MPs while the Greens had 61%. The other parties were more or less uniformly distributed between (23% for the FDP, 20% for the CDU/CSU -- which was lower than their average of around 30% in previous elections, 42% for SPD, and 54% for the Left party). Our other countries are similar, though these top out at lower percentages than in Germany (with few parties having more than 50% female MPs). However, based on research that has examined the association between numbers of female candidates and numbers of female MPs, we can safely assume this average percentage is larger for candidates than MPs, which would mitigate the lower frequency for the higher categories.

Still, to be safe, we include an adjustment to the uniform profile distribution on this dimension in our re-analysis under alternative target distributions that brings it more into line with the frequencies above. Specifically, our target profile distribution assumes the category “10% female candidates” occurs in 10% of parties, “30% of female candidates” occurs in 35% of parties, “50% of female candidates” occurs in 50% of parties, and “70% of female candidates” occurs in only 5% of parties.

h. Have 30% or 50% party gender quotas?

Finally, respondents sometimes saw a statement that indicated that the new party had a 30% or 50% gender quota, with each level occurring with equal probability. The uniform assumption for these two quota levels is broadly consistent with the empirical record. For example, German parties with formal quotas for candidates set these at 40-50%, while those with informal quotas or targets (the CDU /CSU) set these at 30%.

To include all the different adjustments to the profile distribution mentioned above, we used the methods described in Cuesta et al. (2020). These adjustments made no difference to our conclusions and indeed had little impact on the numerical values of our estimates.

That said, there was one value statement we include in the study whose distribution, for both logical and empirical reasons, may need adjustment. This was a statement with two levels: the party “Believes that men and women should be treated equally in economic and public life” or “Believes that there are legitimate reasons to treat men and women differently in economic and public life.” Most of our respondents thought that the active parties in their systems believed the former rather than the later. The pattern is the same across all of our countries, with about 70% of the parties being perceived as holding the pro gender equality position.

Perhaps more importantly (given that the population of existing parties is not really the ideal target population to examine here), it makes logical sense that in modern western democracies most parties cannot reasonably maintain a position in favor of gender inequality. That is why we tried to phrase the unequal position as “softly” as we could – i.e., that there are “legitimate

reasons” for inequality rather than saying genders should be treated unequally. Nevertheless, most of our respondents did not think the unequal position was common among current parties and most reasonable projections to future new parties would likely agree that those espousing gender inequality are less likely. Thus, in the adjusted target distribution we construct, we use a 70/30 split for those two levels (adjusting the probability of all other levels accordingly).

## 2.5. Additional Information

### 2.5.1. Additional information about footnote 30

Studies about political candidates: Graham and Svolic (2020), Hainmueller and Hopkins (2014) for immigrants, van der Does and Kantorowicz (2021) for institutional reforms, Gallego and Marx (2017) for social policies, Bechtel et al. (2017) and Beiser-McGrath and Bernauer (2019) for climate policies, Bansak et al. (2021) for economic policies, Doherty et al. (2020) for foreign policies, and Kantorowicz-Reznichenko et al. (2022) for counter-terrorism strategies.

### 2.5.2. Additional information about footnote 31

While many scholars have raised this criticism, Hainmueller et al. (2015) showed that in certain cases, one can still achieve a high degree of similarity between the effects estimated from conjoint designs and those obtained from a behavioral benchmark. To achieve that, however, “survey experimental designs need to be carefully crafted to motivate respondents to seriously engage with hypothetical choice tasks to mimic the incentives that they face when making the same choices in the real world [...] merely matching the appearance of decision tasks is insufficient” (p.2400).

### 2.5.3. Additional information about footnote 33

These characteristics are independently randomized for each conjoint trial. Additionally, we chose (and pre-tested) party names and symbols to ensure that they did not carry connotations of being left or right-leaning.

### 2.5.4. Additional information about footnote 34

A key consideration in conjoint experiment design is managing respondents' cognitive load. Providing excessive information risks satisficing behaviors, such as only reading the first few attributes listed (Bansak et al. 2018, Druckman 2022). Thus, we limited each trial to six discrete attributes to maintain respondents' attention and enhance information processing. This does not create estimation problems – as the conjoint estimation theory and methods established in

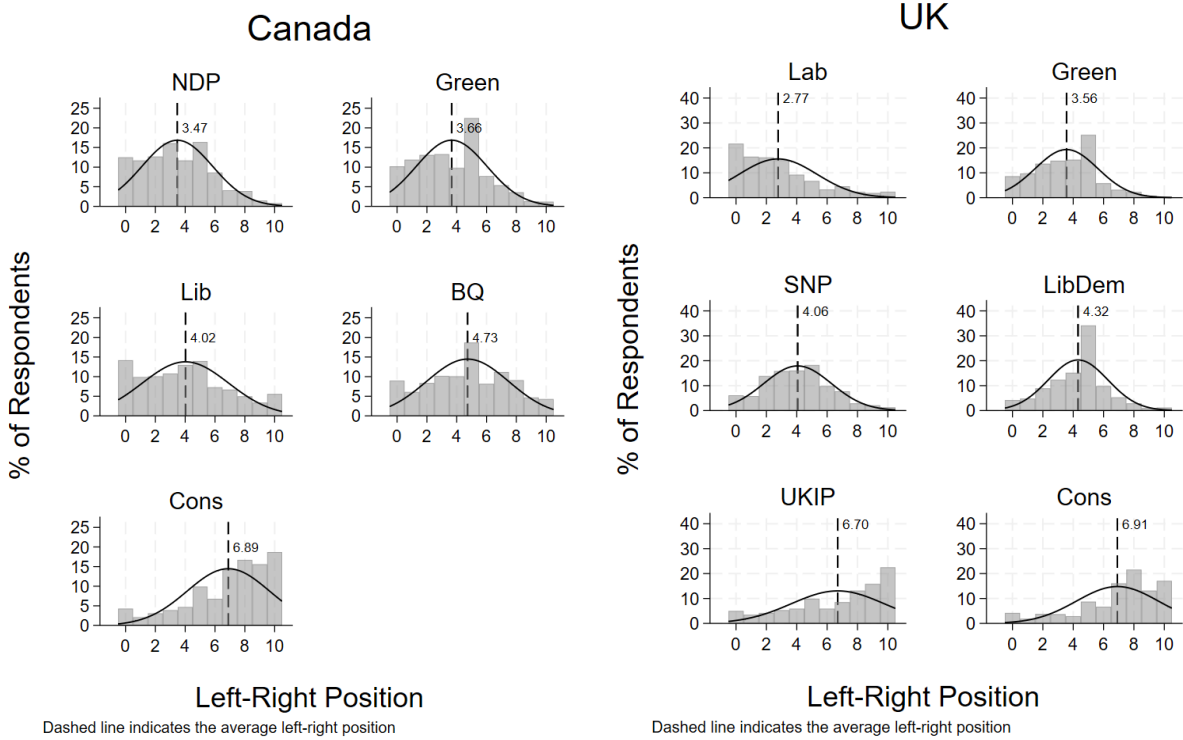
the methodological literature apply directly to this design (e.g., Hainmueller et al. 2014, Egami and Imai 2018, de la Cuesta et al. 2020).

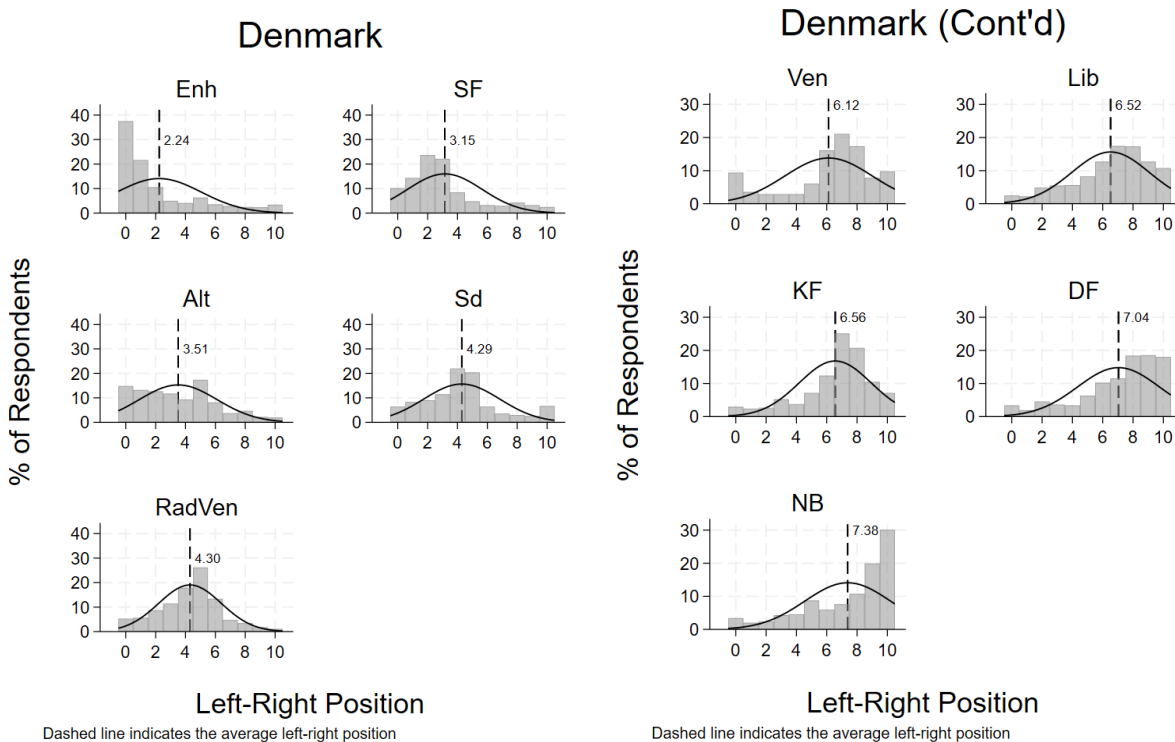
#### 2.5.5. Additional information about footnote 36

We did include some attributes that had more than two levels (e.g., expected party size) but none of these are the focus of this paper. Estimates for these are produced in the same way as the ones discussed here but require choosing an appropriate baseline.

## Appendix to Chapter 3

### 3.1. Distributions of perceived left-right positions of parties





### 3.2. Alternative measures for characterizing the salience and levels of partisan polarization of policy and values items

In the main text, we argue that understanding the heterogeneity in the impact of policy and values items on respondents' LR placements of parties requires considering both issue salience and the extent of party polarization on those issues. To measure salience, we use the number of "Don't Know" responses for each issue, while polarization is assessed using the Cluster Polarization Coefficient (CPC). In this section, we first provide more explanation about Mehlhaff's CPC measure and then introduce alternative measures and conduct robustness checks to validate the measures used in the main text.

#### Mehlhaff's CPC measure

Mehlhaff (p.1) observes that...

Polarization tends to manifest both between and within groups, emerging when group members disagree with members of other groups, agree with members of their own, or both. That understanding implies two conceptual features: distance from opponents (intergroup heterogeneity) and concentration within groups (intragroup homogeneity).

As such, Mehlhaff's CPC measure simultaneously estimates both intergroup heterogeneity and intragroup homogeneity and combines them such that the measure "increases when the distance between groups increases or when groups become more tightly concentrated around their collective ideal point."

Since we are concerned with voters' perceptions of the extent of partisan polarization on a given policy issue, and since we do not require these voters to be adherents of the party, we translate Mehlhaff's description as follows:<sup>9</sup>

Perceived partisan polarization on a given policy issue tends to manifest both between and within parties, emerging when voters believe that party positions on the policy are different from those of other parties, and agree with each other about these positions, or both. That understanding implies two conceptual features: perceived distance between parties on the policy issue (intergroup heterogeneity) and concentration of beliefs "within" parties (intragroup homogeneity).<sup>10</sup>

To obtain a CPC measure of perceived partisan polarization for each policy, we apply Mehlhaff's algorithm to our data on respondent placements of each party, policy by policy, where we specify the relevant groups to be the parties in each system (see Mehlhaff, 2024 for details).<sup>11</sup>

### **Random Effects Model**

An alternative way to measure the extent of party polarization on a given issue is to assess how much of the variation in respondents' party placements is explained by a set of party dummies. If we compare a highly polarized issue to a less polarized one, we expect party dummies to account for a greater share of the variation in party placements for the polarized issue than for the less polarized one.

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<sup>9</sup> Mehlhaff's description reflects the fact that most scholars of polarization have used self-identifications (about policy positions, values, other attitudes, or general left-right identities) to explore polarization. However, this is clearly not necessary: a voter who themselves does not identify with any party can believe that the parties are polarized (generally or on a specific issue) and can convey that belief when they are asked about the positions of the parties (rather than their own position).

<sup>10</sup> The quotes around the word *within* emphasize the idea that all respondents answer about all parties and so all respondents are "within" the group answering about each party. Thus, within party homogeneity in this case is about agreement in the beliefs of all respondents when answering about a party.

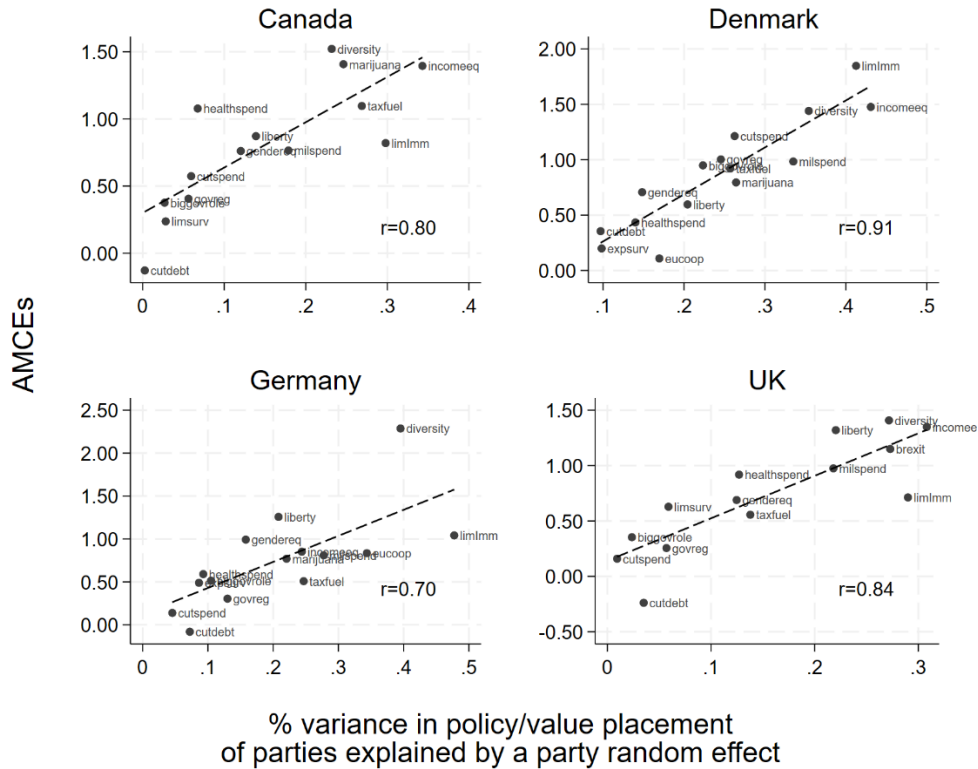
<sup>11</sup> We used the R package "CPC".

We can estimate this using a random intercepts-only model, where the dependent variable is party placement on a given issue, and the only predictor is a random intercept for party. This model allows us to estimate both the variance of the random intercept and the residual variance. Since these two components sum to the total variance in party placements on a given issue, we can use them to calculate the percentage of total variance that is explained by party.

More precisely, this measure of polarization for any policy or value items is the estimated variance of the distribution of party random effects for placements of the parties on a given issue divided by the total variance in such placements. This measure gets bigger when the numerator gets bigger (i.e., there is more inter-party variation in the average placements our respondents give to our parties) or when the denominator gets smaller. Since the denominator is just the variance explained by party plus residual variation, the measure gets smaller when residual variation is bigger – that is, more of the variance in party placements on an issue can be attributed not to difference on average across parties but to differences between individuals “within” parties (i.e., to idiosyncratic variation that reflects a lack of homogeneity in voter beliefs about a given party’s position). Thus, as with the CPC, this measure will get bigger when (voters believe) party positions to be more distinct from one another and the more they agree about those positions.

It turns out that there is quite a lot of difference, policy by policy (and over different values), in the percentage of the variance in respondents’ placements of parties on the issue that can be explained by party. Our prediction is that as this number is bigger, we should estimate a bigger AMCE because the fact that it is polarized gives voters a much clearer signal about the LR implications of the issue. As shown in Figure A3.1, this is exactly what we find. This also speaks to the important role parties play in mapping issues to the LR – i.e., one can imagine a world in which parties do not play this role, and voters themselves (or perhaps other non-party elites) do the mapping.

**Figure A3.1: Random (Party) intercept models of variation in respondent placements of parties on policy issues and values**

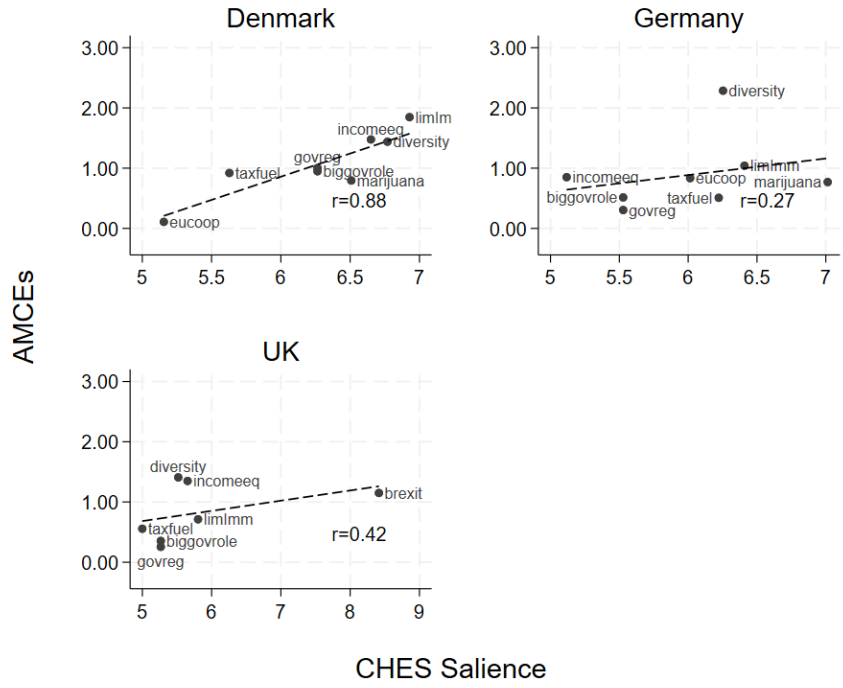


Note: The values on the x-axis are the percentage of variance explained by a party random intercept for respondents’ placements of parties on each policy or values cue.

**CHES Salience Measure**

As another robustness check, we can also substitute the Chapel Hill Expert Survey policy (and values) salience scores for the number of “Don’t Know” responses that we used as a measure of salience in the main text. Since CHES does not include Canada, however, we can only do this for Germany, Denmark, and the UK. Figure A3.2 provides the relationship between the CHES salience measure and our AMCEs estimated in our conjoint experiment. Generally, we find a positive association between the two measures across all three countries, with higher salience scores correlated with bigger AMCEs.

Figure A3.2: CHES Salience measures and the AMCEs for Policies and Values



### 3.3. Measuring Political Sophistication

For the respondents in each country, we had measures of knowledge spanning various topical domains:

1. Knowledge of the true relative left-right positions of each pair of parties in the country (yes or no)
2. Knowledge of the true relative positions of each pair of parties on a series of policies that we include in our conjoint attributes.
3. Knowledge of the true relative positions of each pair of parties on a series of five values that we include in our conjoint attributes.
4. R's level of political interest, level of familiarity with the "left-right" as used in politics, whether or not R identified the current PM correctly, the cabinet membership correctly, and gave the correct answer to 5 current events questions

The current event questions were specific to each country and were as follows:

A. UK:

1. What is the current unemployment rate in the UK? Please answer with a value between 0 and 100%.
2. Who is the current Finance Minister in UK?
3. Who is the current leader of the Social Democratic Party of Germany?
4. Who is the former Nissan chairman who was arrested for financial misconduct?
5. Which country recently granted asylum to a Saudi teenager, Rahaf Mohammed al-Qunun, who fled her family for fear of violence?
6. In mid-January 2019, Prime Minister May's proposed Brexit deal was rejected in the parliament. By how many votes was the proposed deal rejected?

B. Canada:

1. What is the current unemployment rate in Canada? Please answer with a value between 0 and 100%.
2. Who is the current Finance Minister in Canada?
3. Who is the current leader of the Labour Party in the UK?
4. In which country did Donald Trump and Kim Jong Un meet during their summit in February 2019?
5. Which two countries engaged in military conflicts over territorial disputes in late February 2019?
6. Which former Canadian Minister of Justice accuses the Liberal government of intervening in an independent criminal prosecution of SNC-Lavalin?

C. Germany:

1. What is the current unemployment rate in Germany?
2. Who is the current Finance Minister in Germany?
3. Who is the current leader of Labour party in the UK?
4. Who is the current (new) leader of CDU?
5. Who is the new prime minister of the UK?
6. Who is the German politician who was elected to be the President of the European Commission in July 2019?

D. Denmark:

1. What is the current unemployment rate in Denmark?
2. Who is the current Finance Minister in Denmark?
3. Who is the PM of Sweden?
4. From which middle east country did President Trump recently order US troops to withdraw?
5. Who is the new leader of Christian Democrats?
6. The Danish government has recently notified the European Commission of its plan to reintroduce temporary checks along its border with Sweden. For how long would the temporary boarder checks be in place?

5. Knowledge of the relative extent to which each pair of parties cooperated with each other.

We did an appropriate IRT model for each topic 1-4. This was straightforward for 1-3 since all the underlying items for these were 0/1 variables indicating a correct answer (i.e., a correct ordering of the dyad on the relevant dimension). For number 4, most of the questions were 0/1 as above, but two (level of political interest and familiarity with the left-right concept) were ordinal. Thus, we estimated a hybrid IRT for this case. For each IRT we then produce a domain-specific prediction of the domain-specific underlying trait (level of knowledge for that domain).

For cooperation, we proceeded somewhat differently. Specifically, in that case, what we wanted was a measure of the extent to which each respondent correctly understood which parties cooperates with each other the most and which the least. We had answers from a question, described in Chapter 1, that presented respondents with a pair of parties and asked the extent to which the two parties cooperated with each other. Taking the correct extent of cooperation to be the mean view of all respondents, we could then use that to produce a “true” rank for each pair of parties with higher ranks meaning the pair is the most cooperative and lower ranks meaning it is the least cooperative. With that, for each respondent, we could find the Spearman’s rank correlation between the respondent’s ranking of the cooperativeness of each party dyad and the true ranking.<sup>12</sup> Thus a higher correlation indicates the respondents’

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<sup>12</sup> We replicated this using Pearson’s correlation and tau-b and the results are the same.

understanding of the relative cooperativeness of the party dyads in her system matches the consensus view.

With these five measures of these domain-specific types of knowledge (all continuous and approximately normally distributed), we then combined them via a factor analysis to produce our overall sophistication score and a complete ranking of respondents in each country according to that score. All analyses were done for each country separately.

We also produced a version of the measure that replaces the IRT's for 1-3 (all dichotomous items) with simple sum scores of the underlying items, retains the IRT for number 4, the hybrid model, and uses the measure of knowledge about cooperation given above. These were then factor-analyzed to produce an alternative measure of sophistication.

These two measures are, as we should expect, at very high levels (e.g., 0.98 for Germany).

The resulting measure is sufficiently fine grained to give a unique sophistication rank to each respondent and we create our estimation sample for each similarly sophisticated group of 200 respondents by starting with the least sophisticated 200 respondents and then adding the next most sophisticated, while removing the least sophisticated. This results in approximately equal sized sub-samples of respondent trials.

The sub-samples are only approximately equal because the unit of response in our data is actually the respondent-trial, where respondents answered about 20 hypothetical parties unless a respondent was completely unfamiliar with the left-right, in which they were only asked 10. Since sophistication is an attribute of the respondent, when we add (or remove) a respondent from one of these sophistication groups, we add all their responses. Of course, only some of these responses will include the cues on which we are focused in any presentation (with the choice of cues being random), so the actual number of rows of data included in the 200-person sophistication groups will vary somewhat.

### 3.4. Party Cooperation and Conflict Attribute Statements

Canada		
Cue: party often or seldom...	Often	Seldom
Cooperates with the Conservative (Cons)	1240	1294
Cooperates with the Liberals (Lib)	1366	1307
Cooperates with the NDP (NDP)	1256	1313
Cooperates with the Greens (Green)	1243	1270
Cooperates with Bloc Quebecois (BQ)	1247	1231
Cue: party would or would not support a ...	Support	Not Support
Conservative-led minority government (ConsGov)	1227	1259
Liberal-led minority government (LibGov)	1318	1239

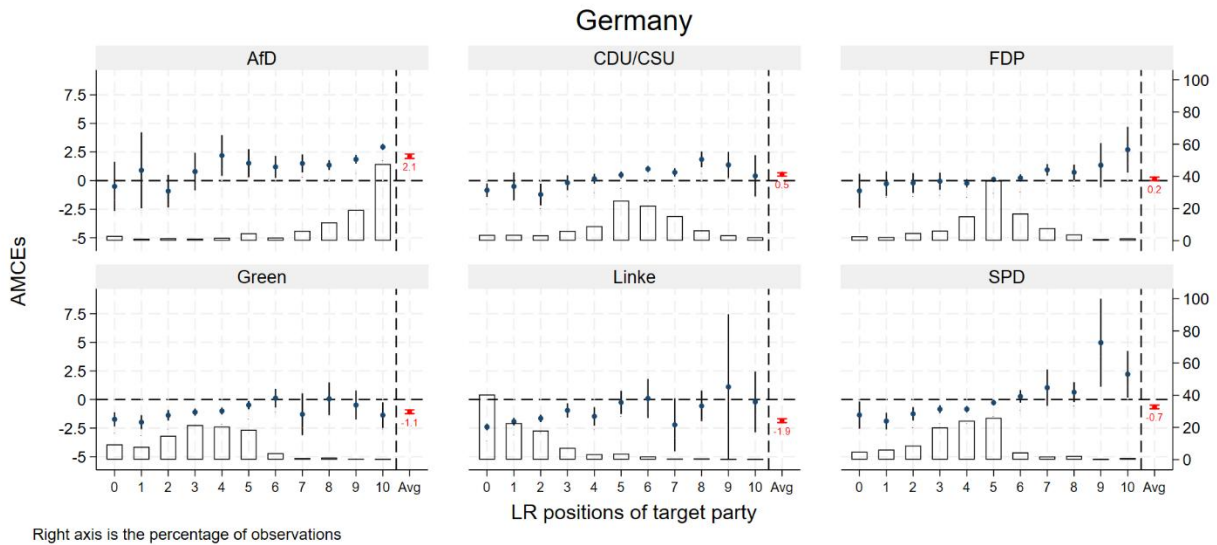
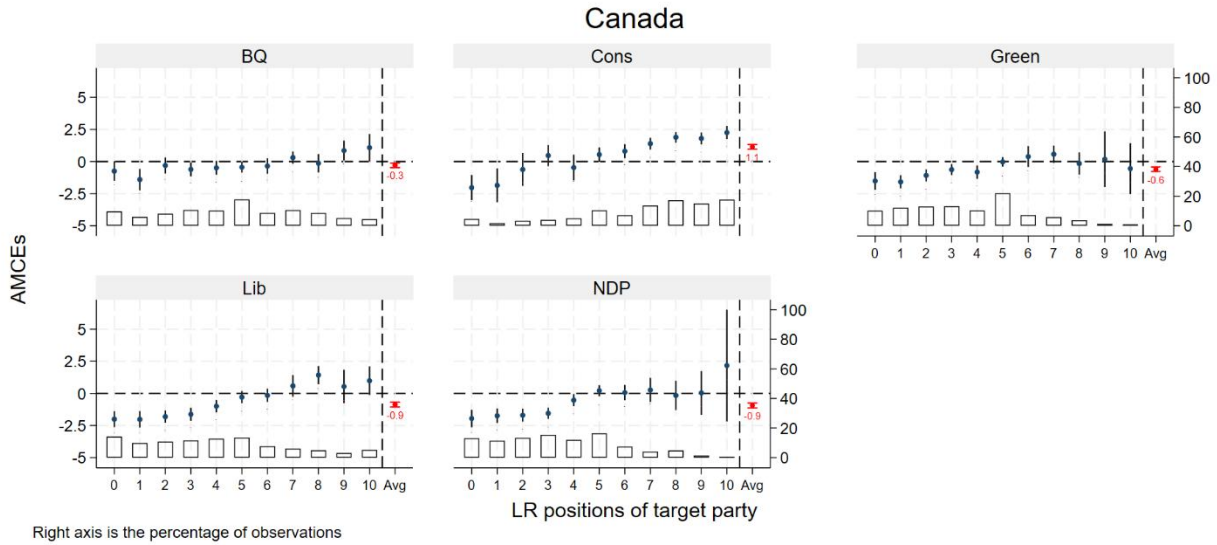
Denmark		
Cue: party often or seldom...	Often	Seldom
Cooperates with the Social Democrats (Sd)	1024	1013
Cooperates with Venstre (Ven)	1016	1079
Cooperates with Danish People's Party (DF)	1036	1007
Cooperates with the Red-Green Alliance (Enh)	1000	1021
Cooperates with Liberal Alliance (Lib)	991	1031
Cooperates with Radikal Venstre (RadVen)	1025	1020
Cooperates with Socialist People's Party (SF)	1010	1012
Cooperates with Conservative People's Party (KF)	1023	1009
Cooperates with the Alternative (Alt)	1036	1024
Cooperates with the New Right (NB)	1051	1050
Cue: party would or would not join a ...	Join	Not Join
Social Democrats-led coalition government (SdGov)	990	1047
Venstre-led coalition government (VenGov)	1003	1042

UK		
Cue: party often or seldom...	Often	Seldom
Cooperates with the Conservatives (Cons)	1171	1183
Cooperates with Labour (Lab)	1159	1101
Cooperates with the Liberal Democrats (LibDem)	1167	1100
Cooperates with the Scottish National Party (SNP)	1143	1127
Cooperates with UK Independence Party (UKIP)	1150	1087
Cooperates with the Greens (Green)	1118	1185
Cue: party would or would not join a ...	Join	Not Join
Conservative-led coalition government (ConsGov)	1145	1105
Labour-led coalition government (LabGov)	1112	1127

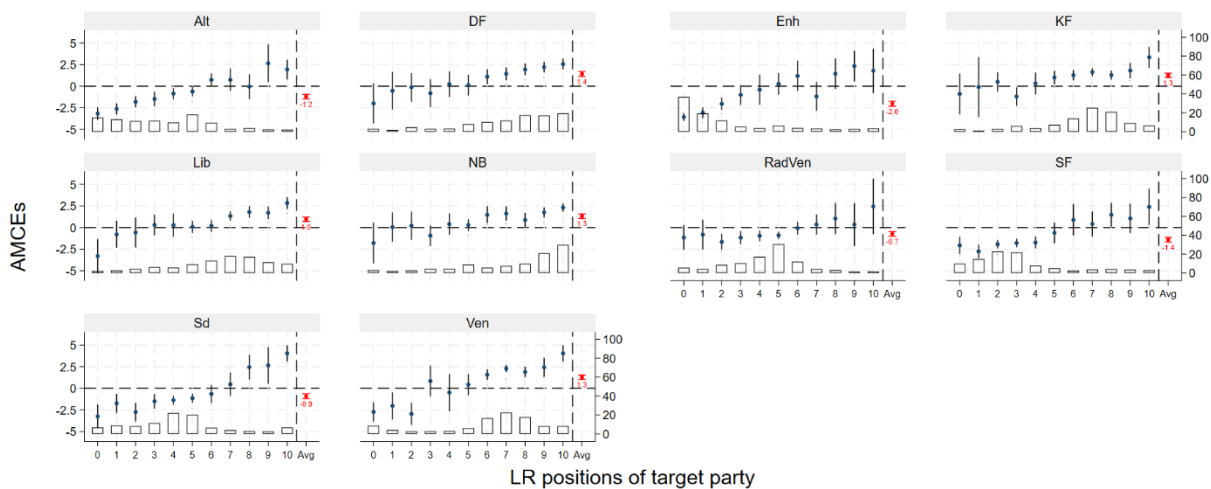
Note: The numbers in the cells represent the number of conjoint trials in which our respondents viewed either the often or seldom version of the cooperation attribute listed on the left. The short labels for the parties, which are used in the figures in the main text, are provided in parentheses.

**Party Abbreviations:** **Canada:** NDP=New Democratic Party, BQ= Bloc Québécois; **Germany:** CDU=CUD/CSU (Christian democratic union of Germany + Christian social union in Bavaria), SPD=Social democrats, FDP=Free Democratic party, AfD=Alternative for Germany, Linke=The Left, Green=Alliance90/The Greens; **Denmark:** Sd=Social Democrats, Ven=Liberal party (Venstre), RedVen=Social liberals (Radikale Venstre), DF=Danish people's party (Dansk Folkeparti), Enh=Red-Green alliance (Enhedslisten), Lib=Liberal Alliance, SF=Socialist people's party, or Green left (Socialistisk Folkeparti), KF=Conservative people's party (Det Konservative Folkeparti), Alt=the Alternative (Alternativet), NB=New right (Nye Borgerlige); **UK:** SNP=Scottish National party, UKIP=UK independence party.

### 3.5. The effect of partisan cooperation and conflict, conditional on the perceived left-right position of the target party



## Denmark

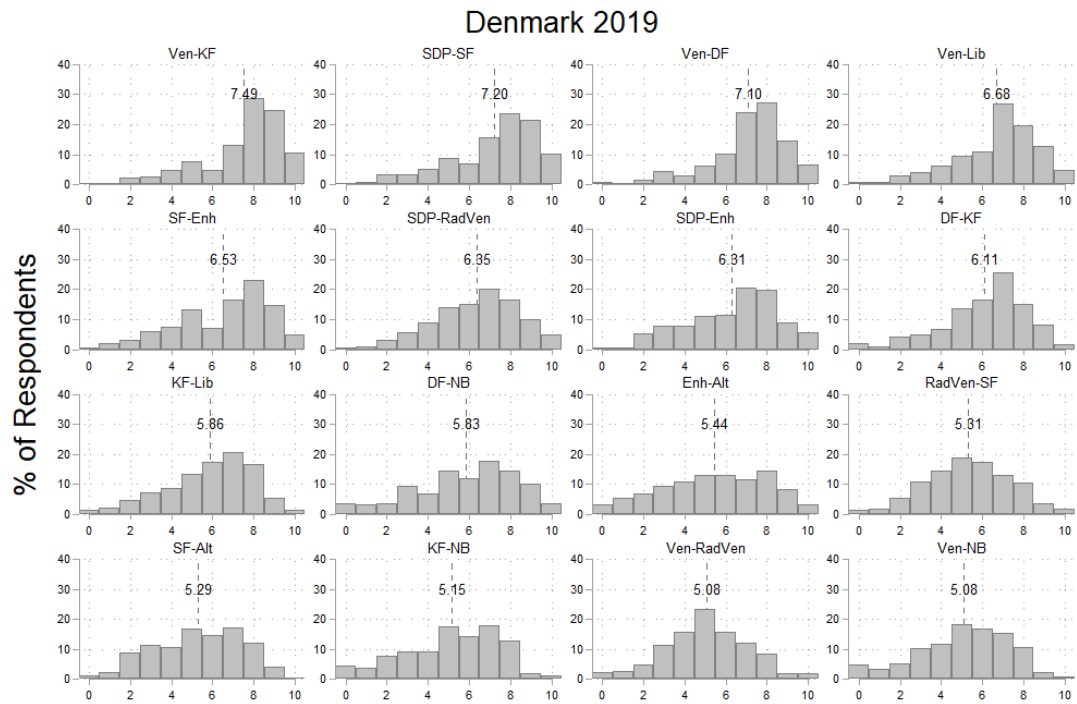
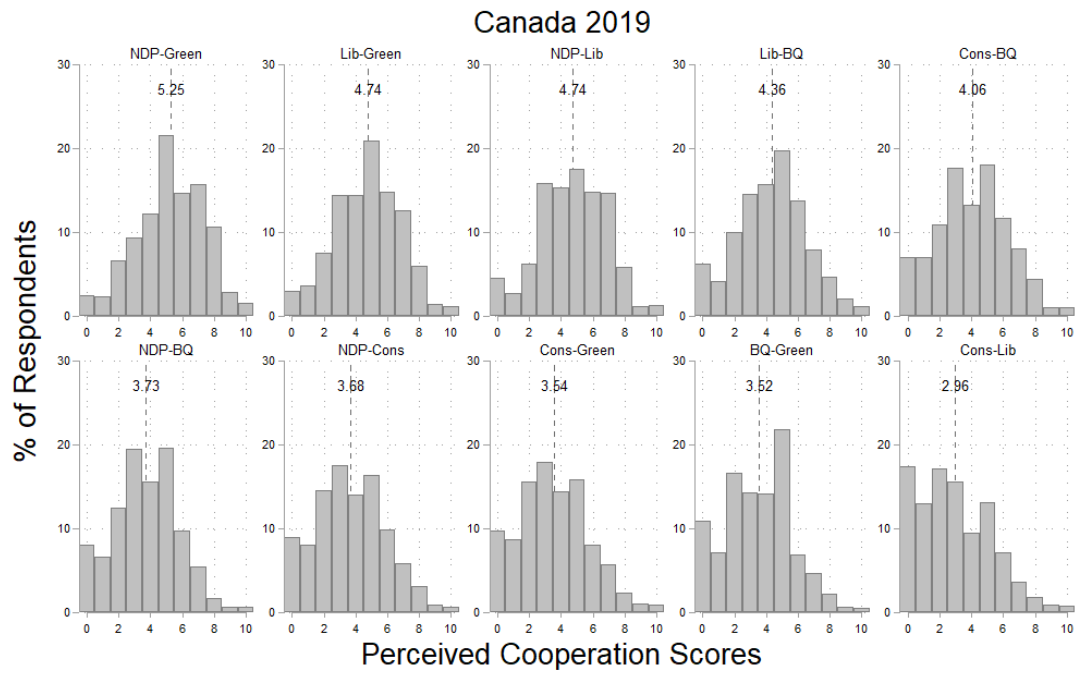


Right axis is the percentage of observations

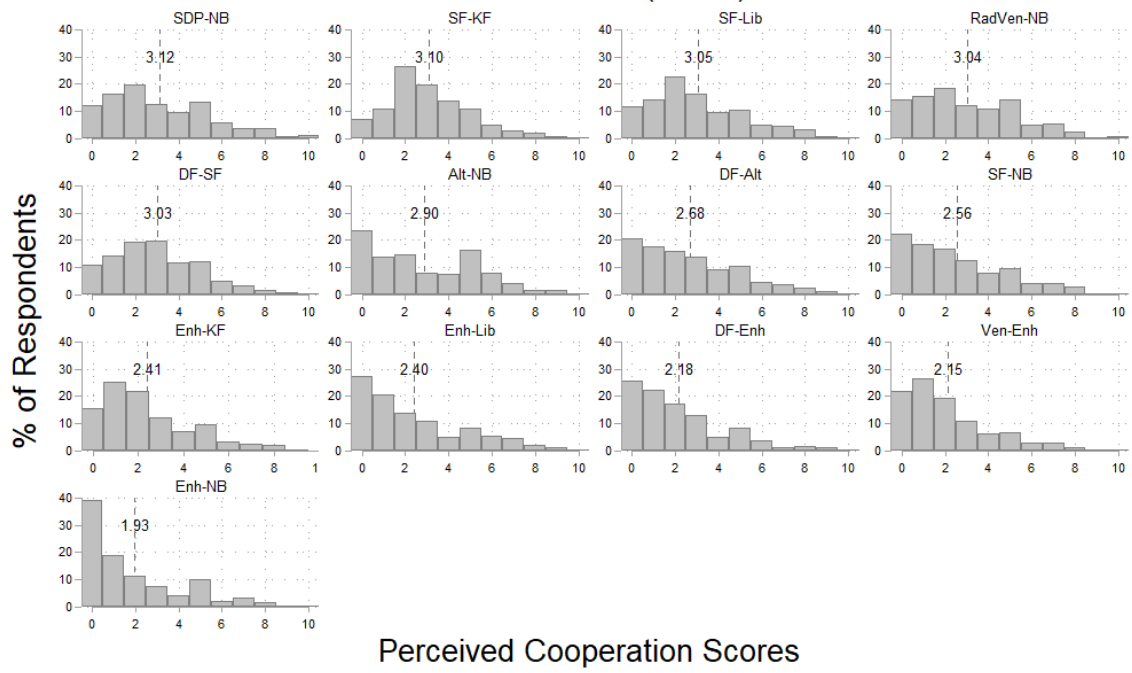
LR positions of target party

Note: The histogram at the bottom of each panel is the distribution of respondent LR placements of the indicated target party. The point estimates are the conditional AMCEs calculated using the group of respondents who located the target party at the indicated position on the left-right scale. The estimates on the far right of each graph labelled “Avg” (in red) are the weighted average of the conditional AMCEs. The vertical lines are 95% confidence intervals. The y-axis on the left is the size of the (conditional and weighted) AMCEs, and the y-axis on the right indicates the percentages of respondents.

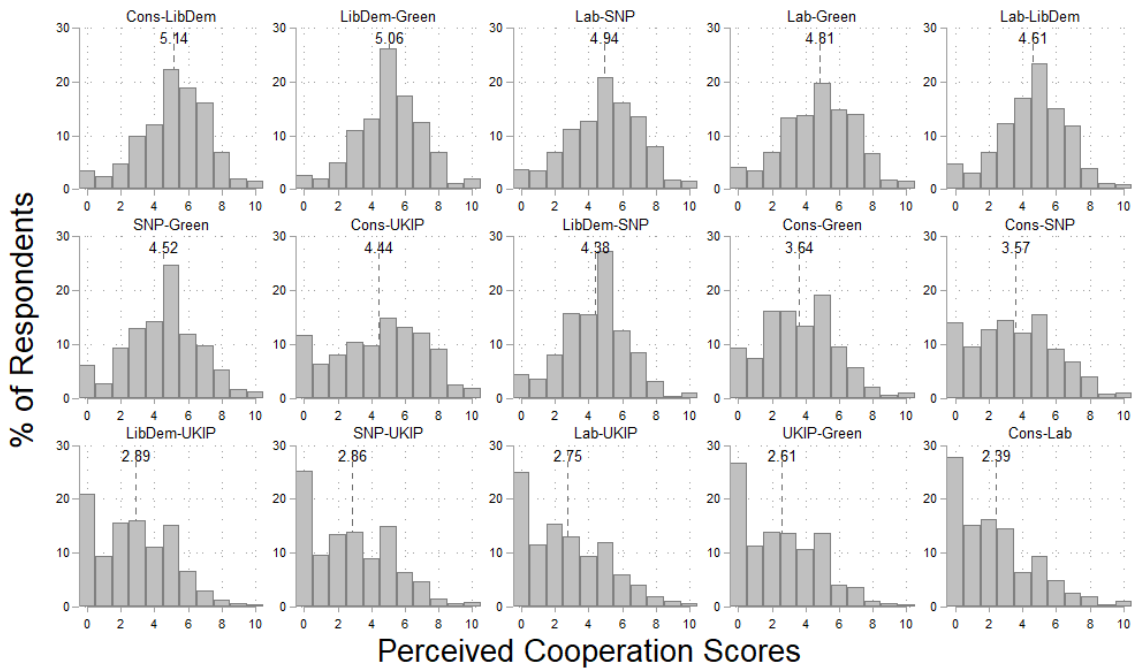
### 3.6. Distribution of Perceived Cooperation among Parties



### Denmark 2019 (cont'd)



### UK 2019



### 3.7. Multivariate analysis of ideological purity in partisan cooperation and the estimated AMCE on patterns of partisan cooperation

Since the extremity of a party's perceived LR position and the purity of its pattern of cooperation are likely correlated in the real world, we will need a multivariate model to sort out the individual impact of these two factors on the size of the AMCEs for cooperation. Further, we can use such a model to examine the two mechanisms by which cooperation with a target might matter to voters in accessing the LR position of a new party and what that means for variation in the AMCEs for cooperating with different target parties. One mechanism argued that voters get a stronger LR signal from cooperation with a party that is "ideologically pure" in their patterns of cooperation. In the other, we pointed out that even if voters do not know anything about a party's patterns of cooperation, she might still place the new party closer to where she thinks the target party is located. What this means is that if we want to isolate the various individual level effects of cooperating with a given target, then we must first control for the perceived LR position of the target when we estimate the effect of purity.<sup>13</sup>

To estimate that model, we first build a data set from all the conditional AMCEs of the kind graphed in Figure 3.11 in the main text. The data set has 11 rows for each target party, corresponding to positions 0-10 on the left-right scale. The dependent variable is the conditional AMCE for cooperating with a given target party who is believed to be at a given position on the left-right (as estimated using only the respondents who placed that target party at the given position on our left-right scale). To this we add indicators of the given left-right position as well as our ideological purity score for each party.

Table A3.1 gives the estimates of regressing the Conditional AMCEs for all the target parties on indicators for perceived LR position of the target party as well as its ideological purity score. We find the same strong relationship between the perceived left-right position of the target and the size (and direction) of the conditional AMCE that we saw in the (unconditional on ideological purity) graphs in section OA3.5 above. Likewise, apart from Canada, we find a strong positive association between the target party's ideological purity and the size of its AMCE. Indeed, since

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<sup>13</sup> One way to do this is to simply estimate the purity effect separately for each of the 11 positions of the LR scale. We have done this by first calculating conditional AMCEs for each target party and perceived LR position (i.e., calculating the AMCE only using respondents who received the message that the new party often or seldom cooperated with the given target party *and* that placed the target party at the given position on the LR) and then regressing these conditional AMCEs on our measure of purity for each party (who some voter placed at that position, which almost always includes all the parties since someone usually puts each party in each LR position). We have done this for all 11 LR positions, and we find that for all 11 of these models, the coefficient on purity is large and statistically significant.

the standard deviation of our average perceived purity score for a party is about 1, these results imply that increasing the average perceived purity score by one standard deviation (after controlling for the party’s perceived LR position) is to increase the size of the cooperation effect by about 0.40 – or about half a point on our left-right scale. When one excludes Canada (for which Purity clearly makes no difference), the pooled effect increases to 0.49. Given that the corresponding unconditional associations between ideological purity and the AMCEs for cooperation cues (provided in the note to Figure 3.14 in the main text) ranged from 0.96 to 1.39 for the three Non-Canadian cases, more than half the unconditional impact of purity illustrated in the figures in OA3.5 was due to confounding impact of the left-right position of the target party on the strength of the cooperation signal. Nonetheless, once we control for the extremity of the target party position, the association between ideological purity and the size of the AMCE for cooperation cues remains strong.

**Table A3.1: The association between ideological purity of the target party’s pattern of partisan cooperation and the size of the AMCE controlling for perceived LR position of the target**

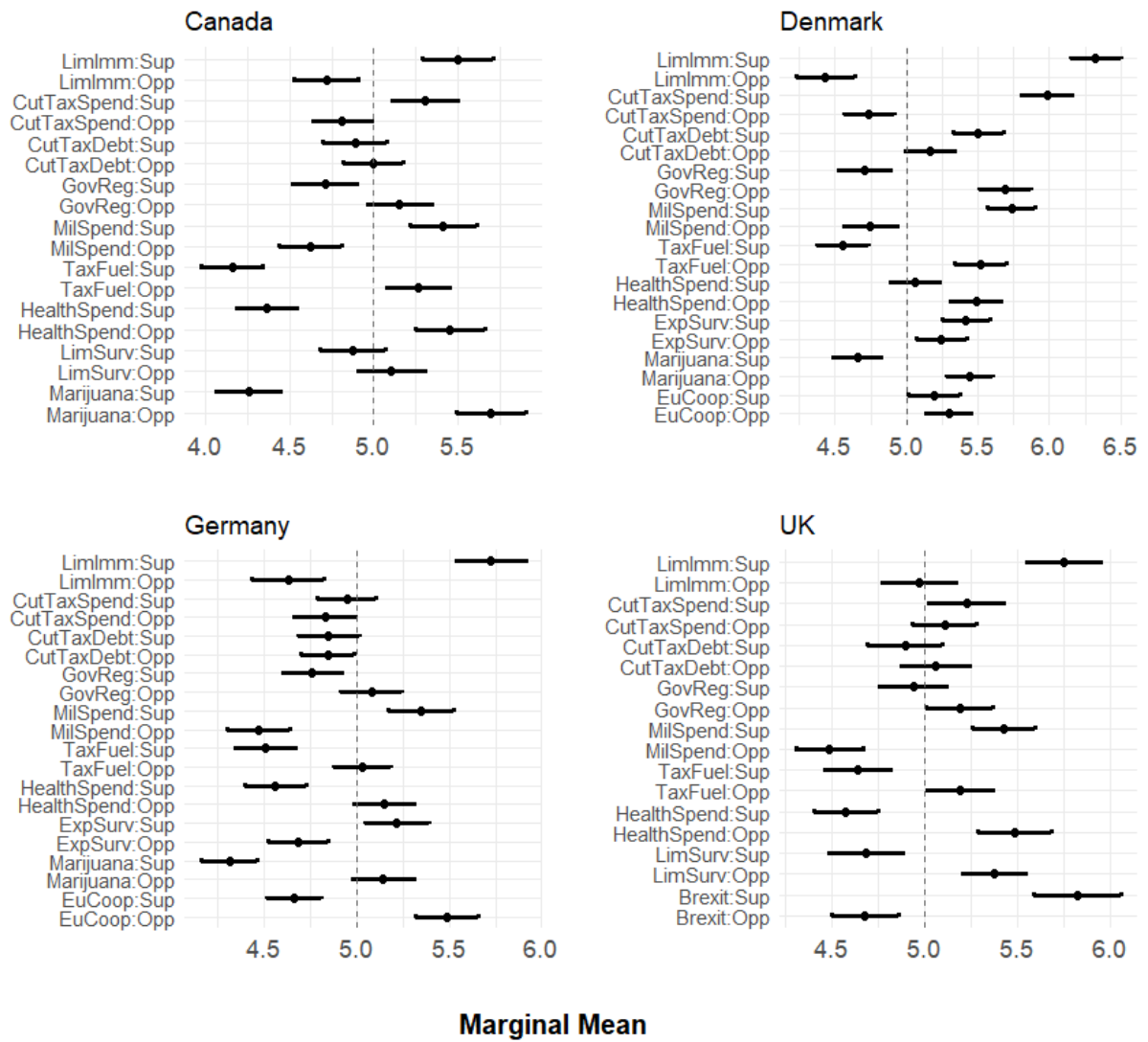
	Canada	Denmark	Germany	UK	All Countries
Purity Score	<b>0.00</b> (0.089)	<b>0.47**</b> (0.074)	<b>0.68**</b> (0.114)	<b>0.32**</b> (0.092)	<b>0.40**</b> (0.053)
LR positions of Target Party					
0	-1.65** (0.247)	-2.39** (0.279)	-1.29** (0.216)	-1.38** (0.226)	-1.78** (0.153)
1	-1.72** (0.106)	-1.44** (0.259)	-0.95** (0.253)	-1.54** (0.295)	-1.41** (0.147)
2	-1.10** (0.296)	-1.32** (0.320)	-1.10** (0.194)	-1.00** (0.201)	-1.16** (0.135)
3	-0.79* (0.386)	-0.87** (0.208)	-0.39** (0.106)	-0.61** (0.200)	-0.69** (0.120)
4	-0.66** (0.105)	-0.46** (0.153)	-0.20 (0.352)	-0.14 (0.086)	-0.37** (0.113)
5	-0.00 (0.179)	-0.11 (0.166)	0.18 (0.175)	-0.08 (0.134)	-0.02 (0.096)
6	0.15 (0.208)	0.77** (0.221)	0.49* (0.231)	-0.02 (0.262)	0.42** (0.131)
7	0.62** (0.206)	0.91** (0.198)	0.12 (0.490)	0.41 (0.237)	0.57** (0.164)
8	0.57 (0.454)	1.41** (0.236)	0.68* (0.335)	1.19** (0.330)	1.05** (0.172)
9	0.68* (0.317)	1.89** (0.276)	1.69* (0.770)	1.35** (0.356)	1.50** (0.221)
10	1.19* (0.317)	2.77** (0.276)	1.11* (0.770)	1.25 (0.356)	1.77** (0.221)

	(0.514)	(0.241)	(0.527)	(0.862)	(0.290)
Number of obs.	55	110	66	66	297
Adjusted R-squared	0.65	0.81	0.54	0.56	0.65

Note: \*\*  $p < 0.01$ , \*  $p < 0.05$ . Since both purity and the DV (the conditional AMCEs) are measured on scales that range from about -4 to +4, the positive relationship between purity and the AMCE we estimate is consistent with our expectations over the whole range of the purity scale: When purity is below zero, a more negative purity score is associated with a more negative AMCE and if purity is above 0, then a more positive purity score is associated with a more positive AMCE.

### 3.8. Marginal Means of Conjoint Attributes

Figure A3.3: Marginal Means of Policy Attributes



**Figure A3.4: Marginal Means of Values Attributes**

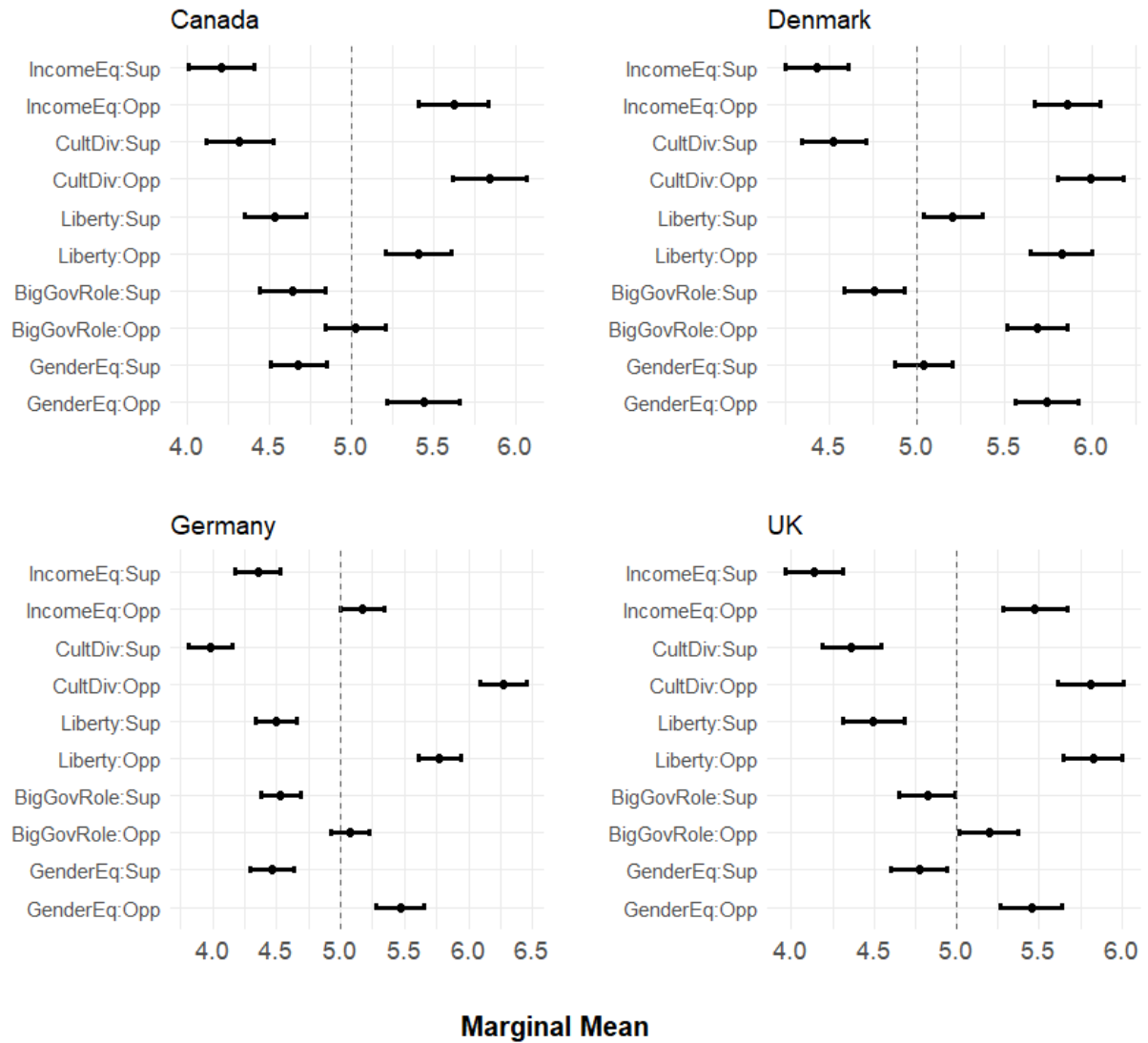


Figure A3.5: Marginal Means of Cooperation Attributes

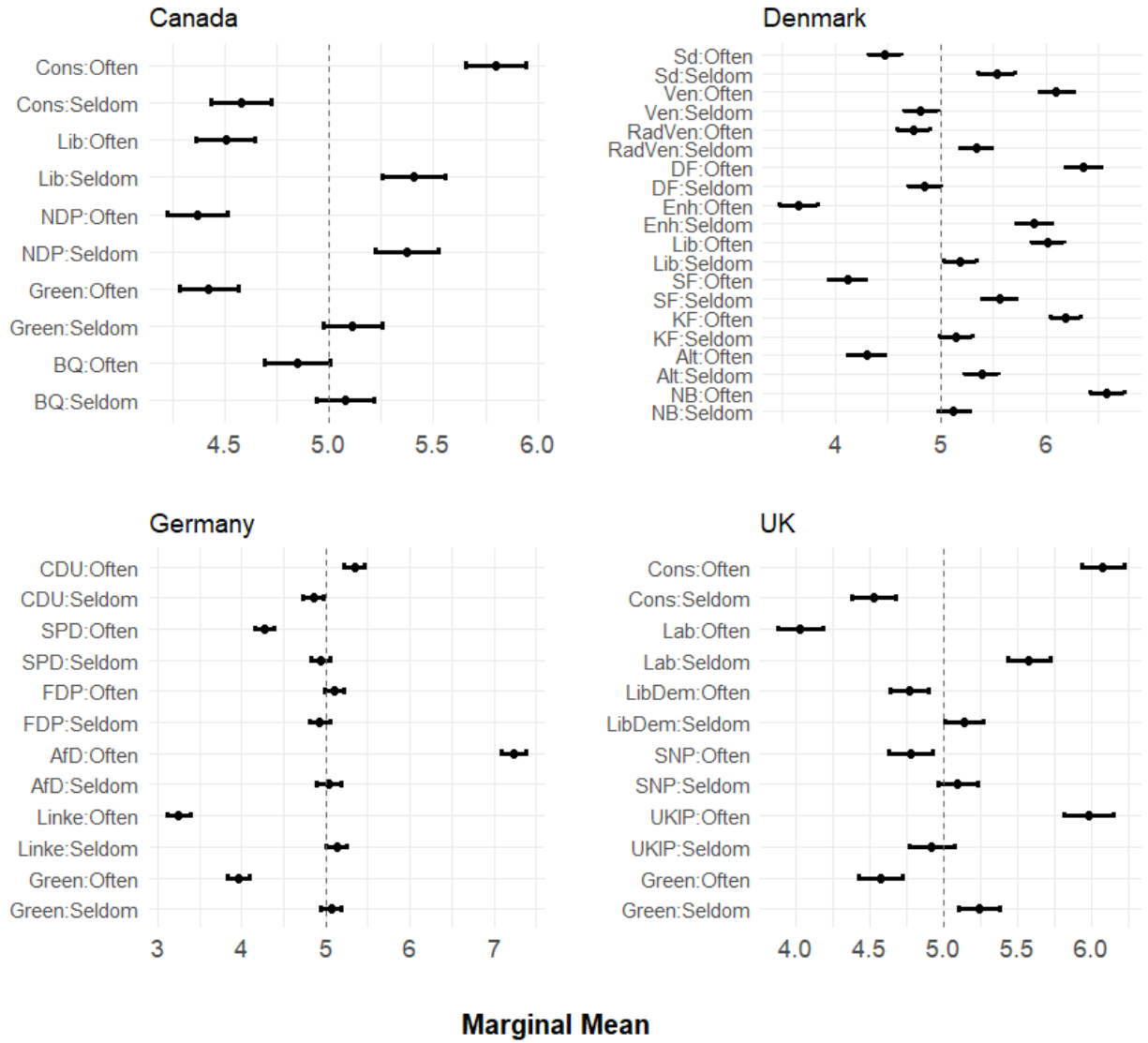
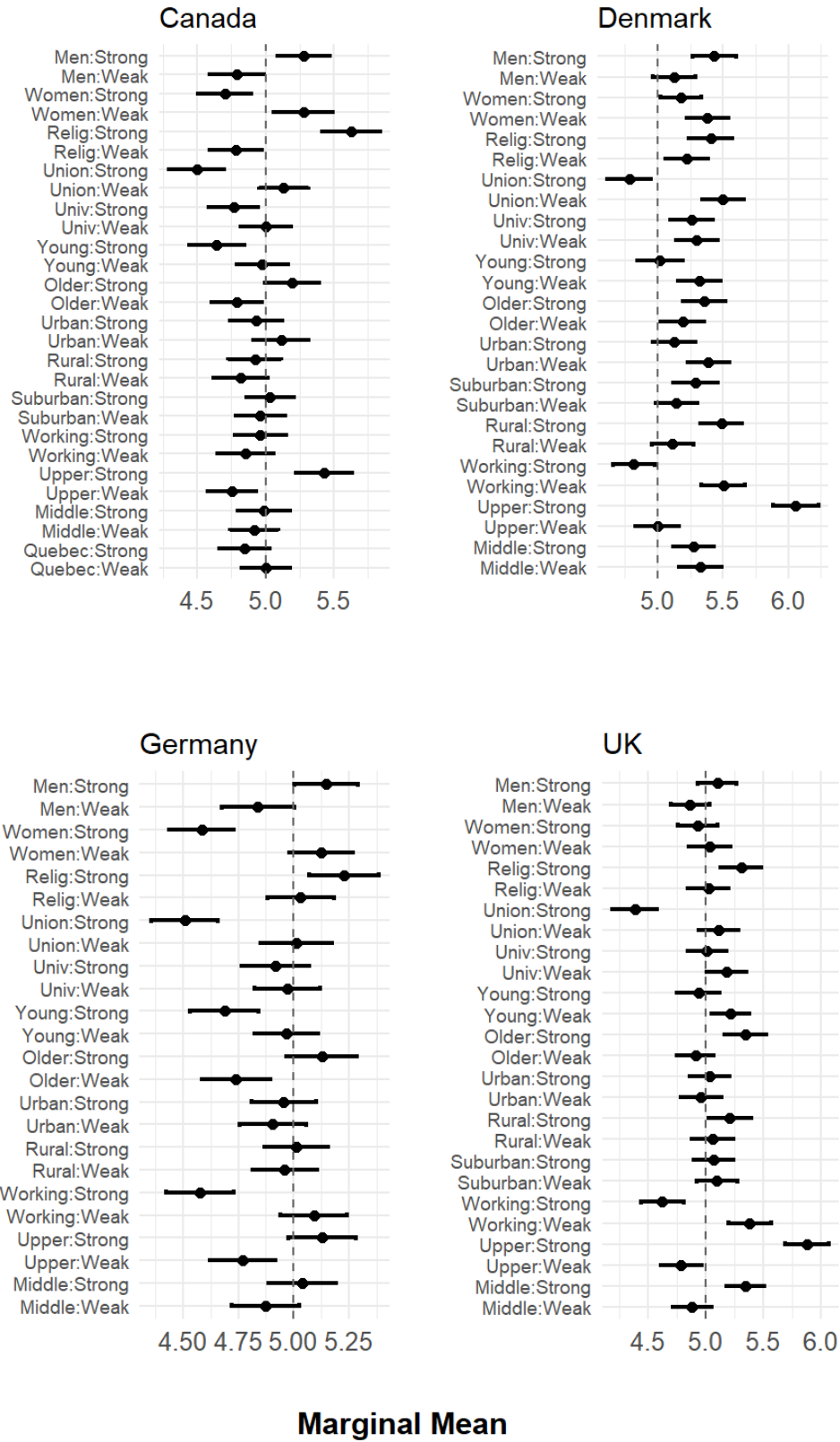


Figure A3.6: Marginal Means of Social Group Attributes



### 3.9. Building an Ideological Purity Score for Parties' Cooperative Behavior

Given perceived cooperation scores for each pair of real parties, as well as our respondents' left-right placements of each, it is straightforward to construct a perceived "ideological purity score" for each party (for each respondent), which reflects the extent to which the party is perceived to cooperate exclusively with other parties on the left or right. Specifically, we use the following formula to calculate respondent  $i$ 's perception of how "ideologically pure" party  $p$ 's pattern of partisan cooperation is:

$$\text{purity}_{ip} = \sum_{j \neq p} \left( \frac{\text{coop}_{ipj}}{\sum_{j \neq p} \text{coop}_{ipj}} \right) * (\text{leftright}_{ij})$$

In this formula,  $\text{coop}_{ipj}$  is respondent  $i$ 's cooperation score for party  $p$  and party  $j$ , and  $\text{leftright}_{ij}$  is her perceived left-right position for party  $j$ . Thus,  $i$ 's perceived purity score for party  $p$  is just the weighted average of the left-right positions of all the parties except party  $p$ , where the weights (which sum to 1) are the normalized cooperation scores between party  $p$  and each other party.<sup>14</sup> The more positive the purity score, the more the respondent thinks that party  $p$  cooperates only with other parties on the right, while a negative purity score indicates the same for the left. A score closer to zero means that the party is perceived to cooperate mainly with centrist parties and/or with a mixed group of leftist and rightist parties.<sup>15</sup> Finally, once we calculate perceived purity scores for each party, we normalize them over the parties in the system, so that a score for any particular party indicates how much above or below the mean party purity score (in the country) a respondent believes it to be.<sup>16</sup>

To illustrate the measure in a real-world case, Table A3.2 provides the purity scores for Germany (from our survey in 2019). As we can see, German voters perceived that the AfD and FDP, on one hand, and the Greens, on the other, cooperate much more exclusively with other parties on the right and left, respectively, than do the SPD, CDU, and Linke. This clearly makes sense given the recent political history of formal cabinet cooperation between the SPD and the CDU.

---

<sup>14</sup> This version of the score does not account for differential levels of overall perceived cooperation (e.g., German voters saw the AfD as only infrequently cooperating with any party).

<sup>15</sup> Its purity is also enhanced by cooperating more with more extreme parties on either the left or right (but not both)

<sup>16</sup> The normalization simply subtracts the mean purity across parties in the system from each party score and then divides by the standard deviation (one gets the same pattern if only does the first step). This is necessary to account for the fact that the perceived "center" of the party systems may differ across countries.

Indeed, at the time of our survey, the CDU/CSU and the SPD had been in a grand coalition since 2013 and had just renewed the coalition.

**Table A3.2: Ideological Purity of the Perceived Patterns of Partisan Cooperation for the German Parties**

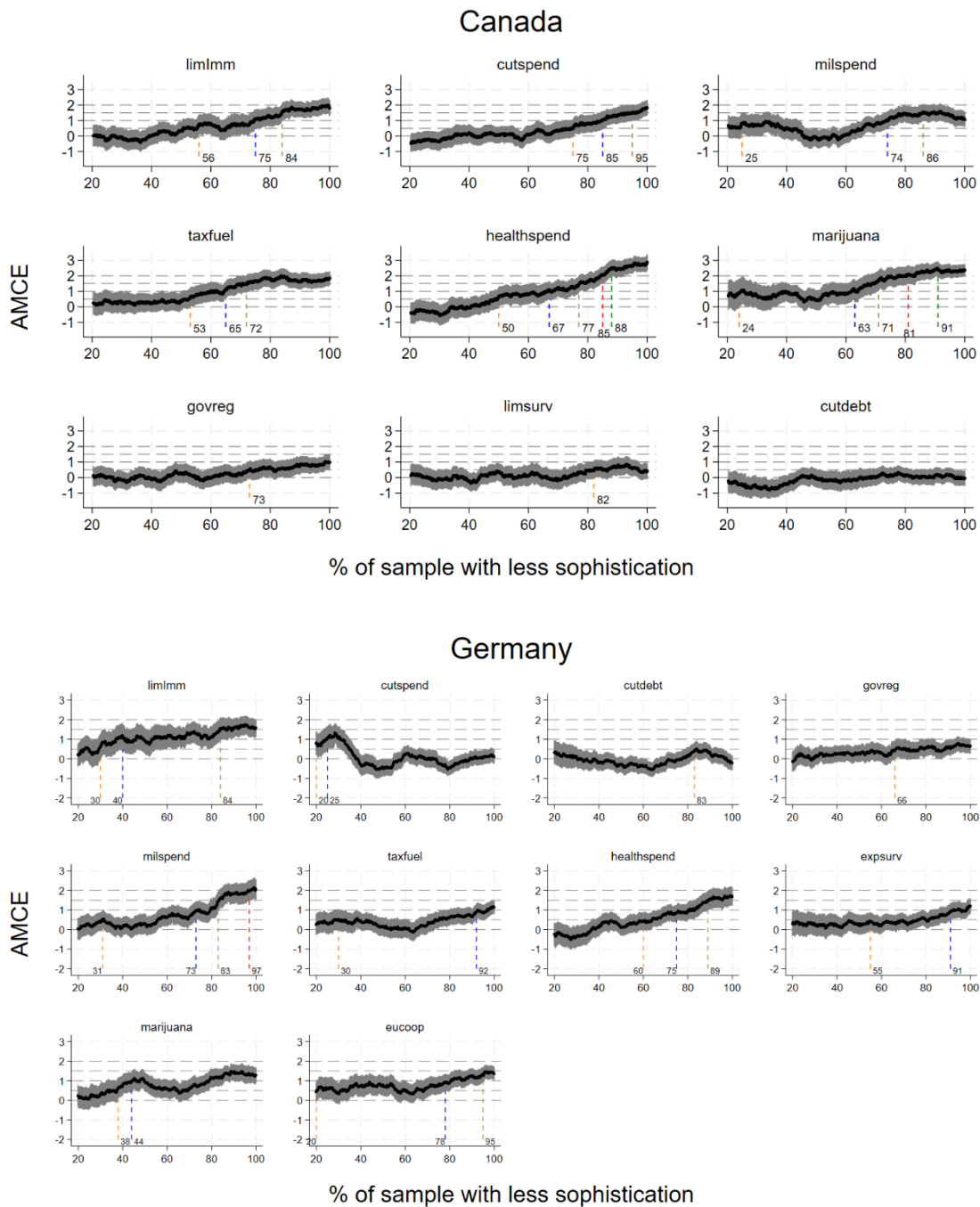
<b>Party</b>	<b>Purity Score</b>
Green	-1.12
SPD	-0.62
Linke	-0.45
CDU	-0.24
FDP	0.96
AfD	1.48

Note: The more positive the purity score, the more the respondent thinks that party cooperates only with other parties on the right, while a negative purity score indicates the same for the left. See text for discussion of how these scores were calculated.

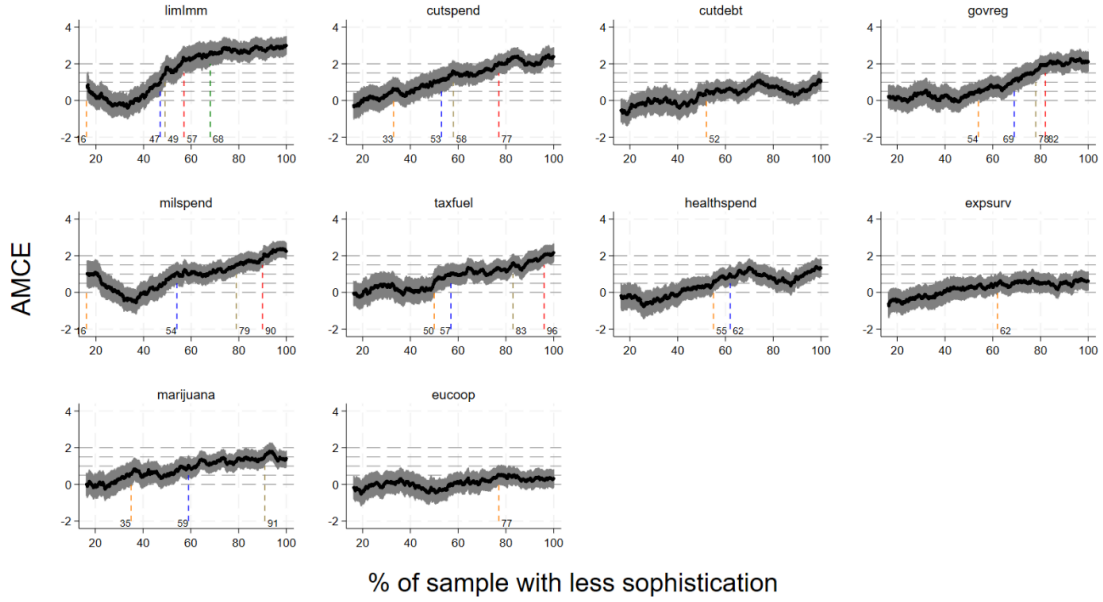
### 3.10. Attributes by Levels of Political Sophistication

The figures below were created in the same way as the aggregate estimates provided in Figures 3.6, 3.10, 3.15, and 3.19, except that they provide the estimated AMCEs for each specific cue separately.

**Figure A3.7: Policy Cues**



## Denmark



## UK

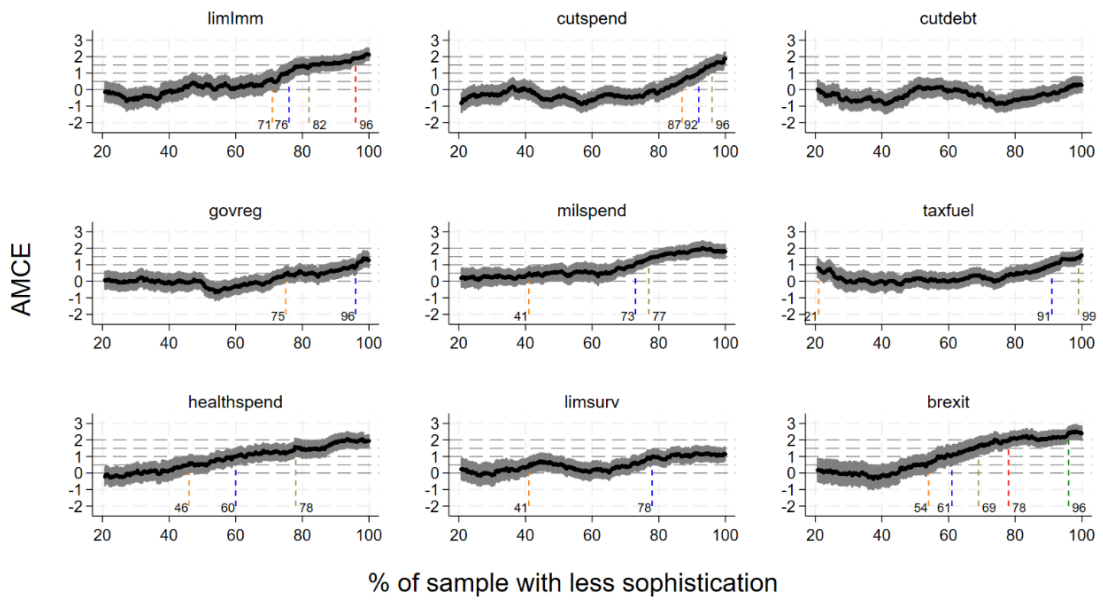
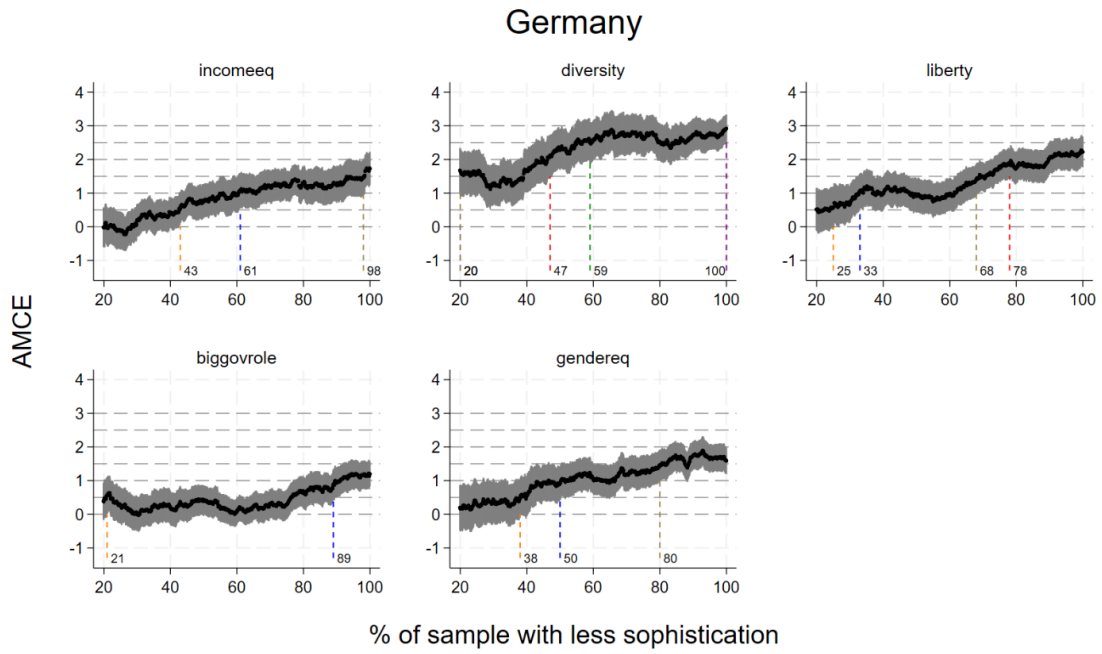
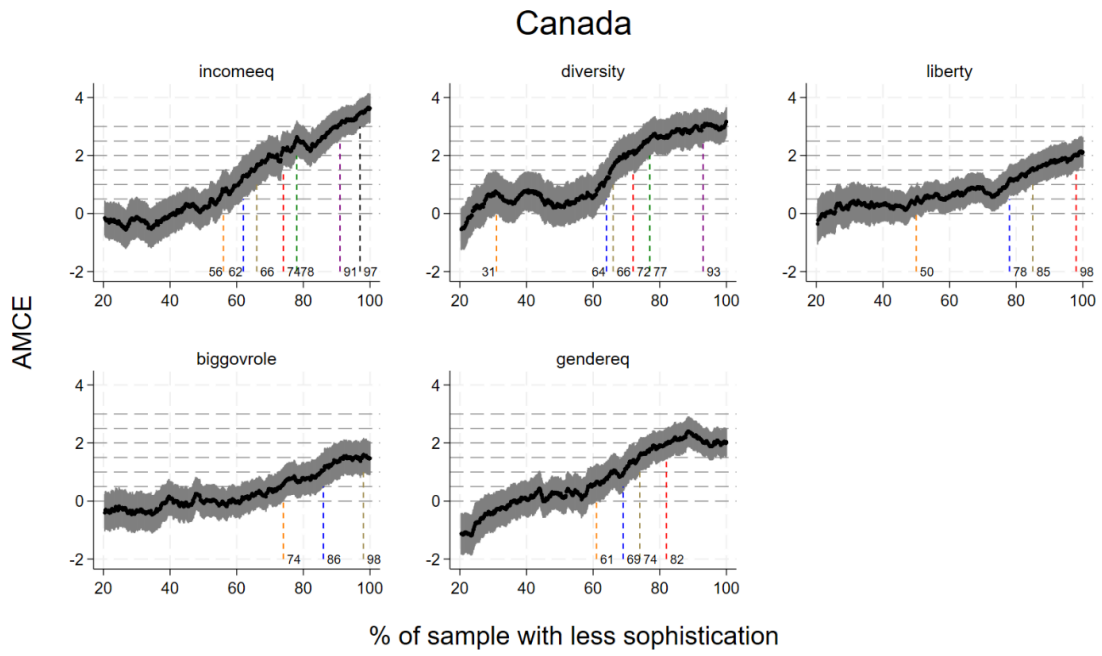
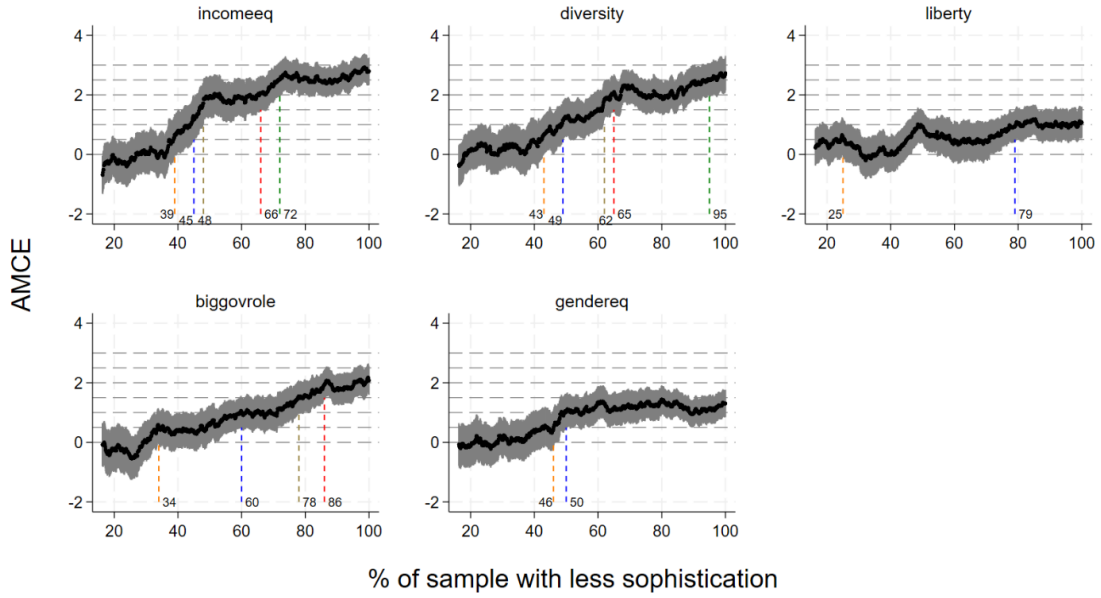


Figure A3.8: Values Cues



## Denmark



## UK

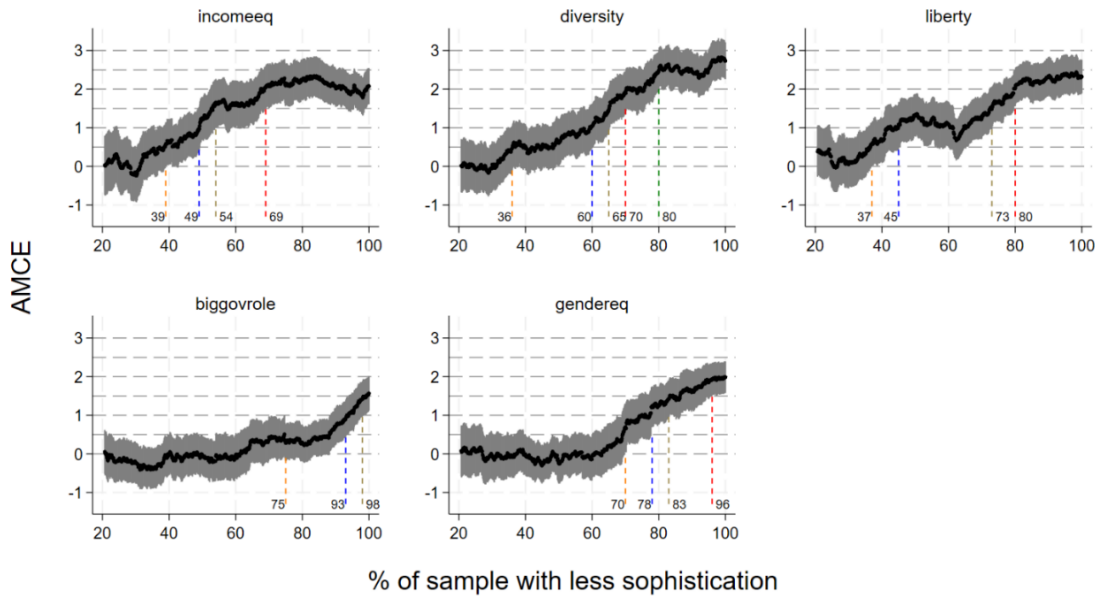
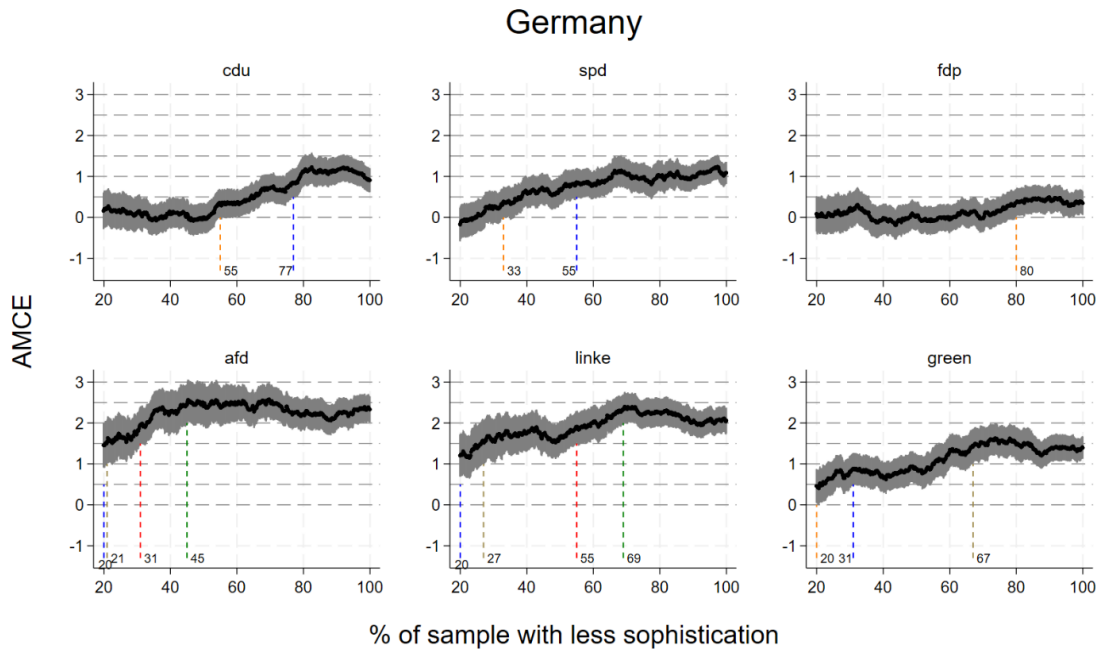
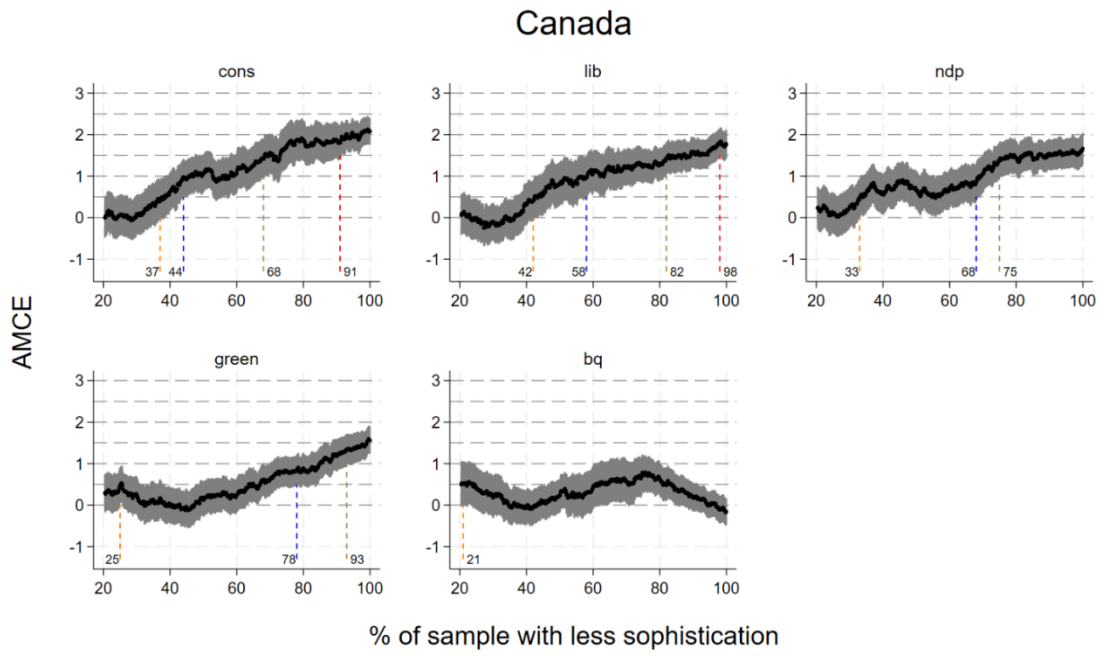
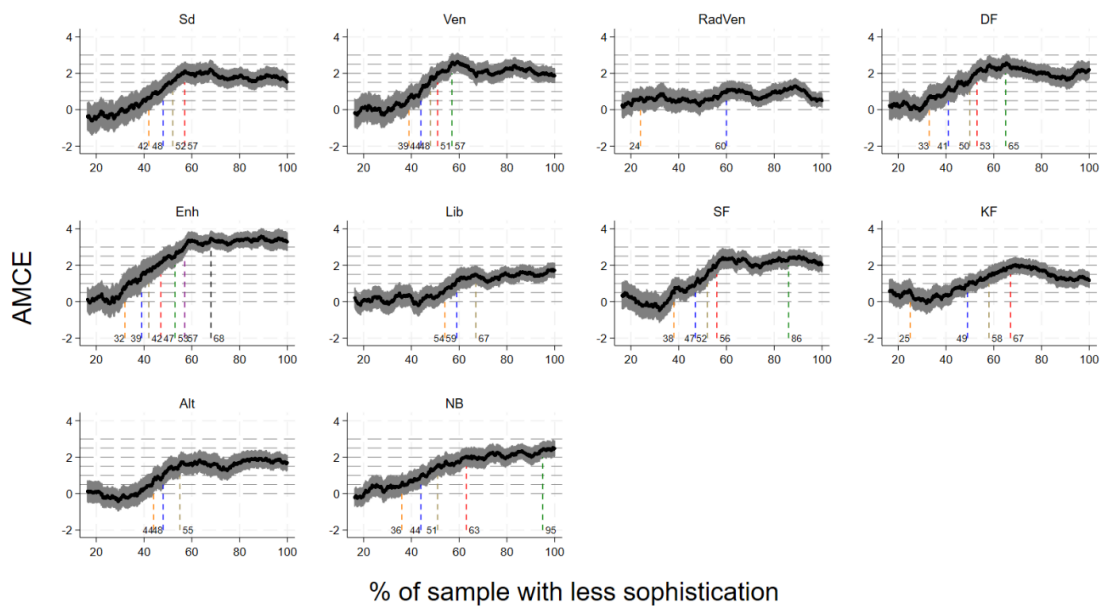


Figure A3.9: Partisan Cooperation Cues



## Denmark



## UK

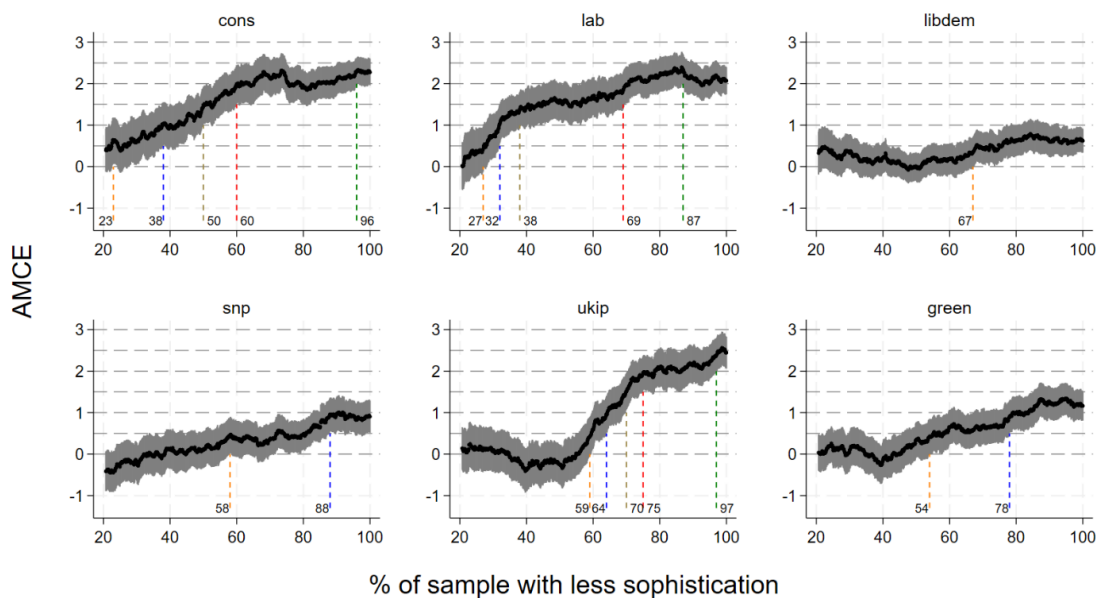
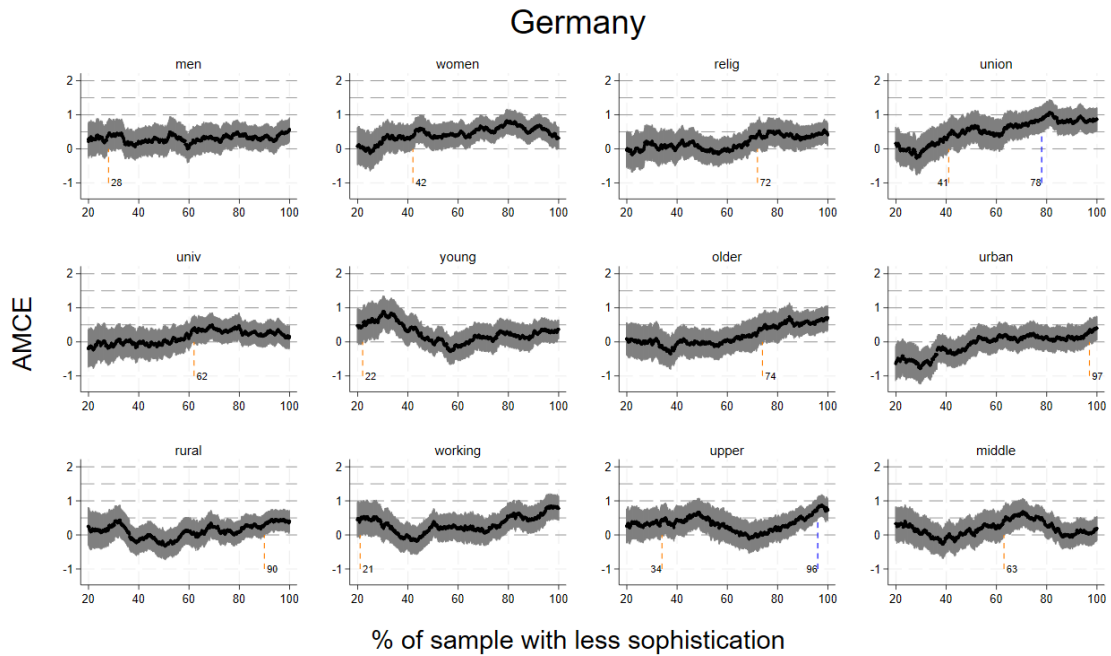
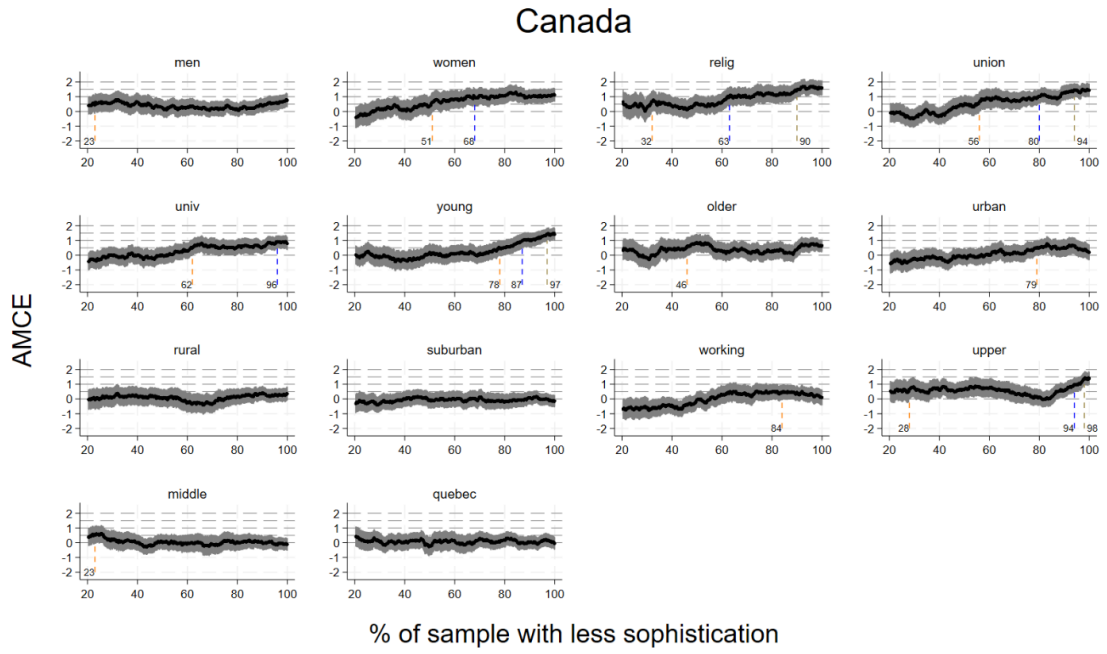
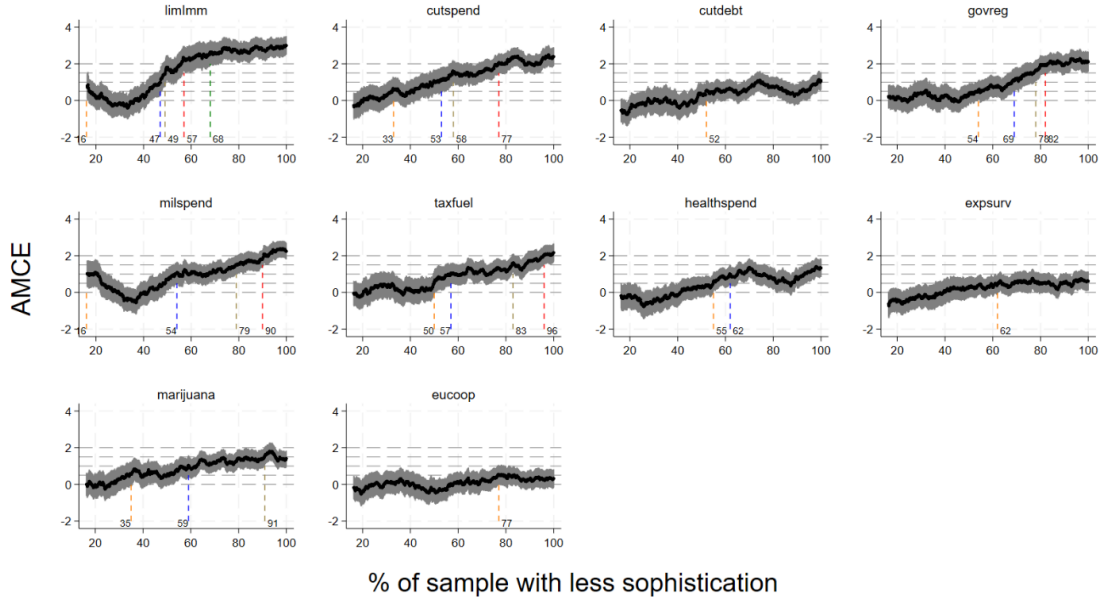


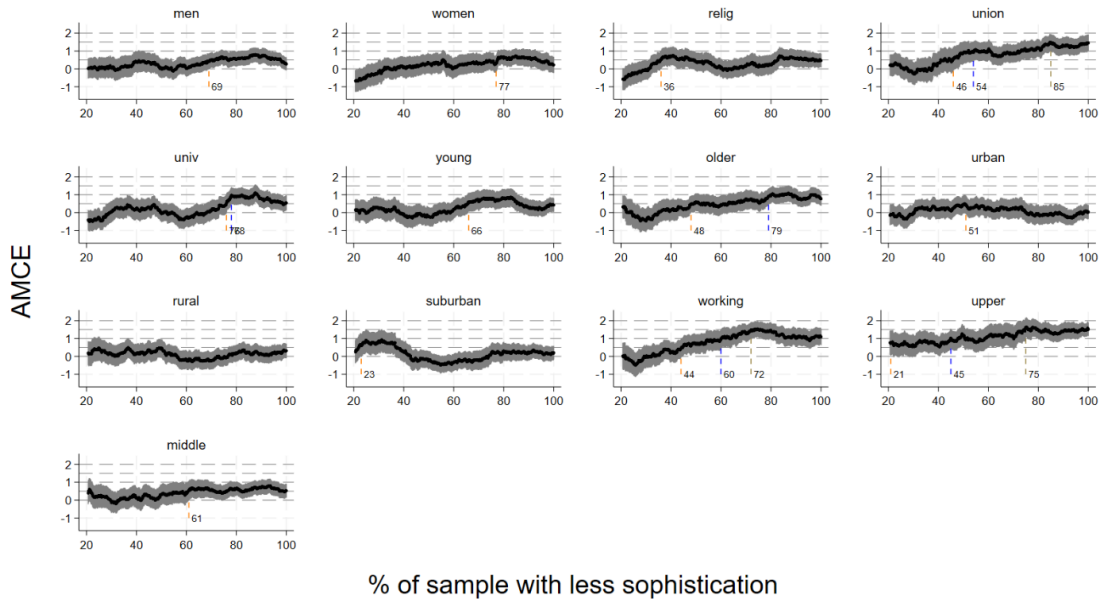
Figure A3.10: Social Group Support Cues



## Denmark



## UK



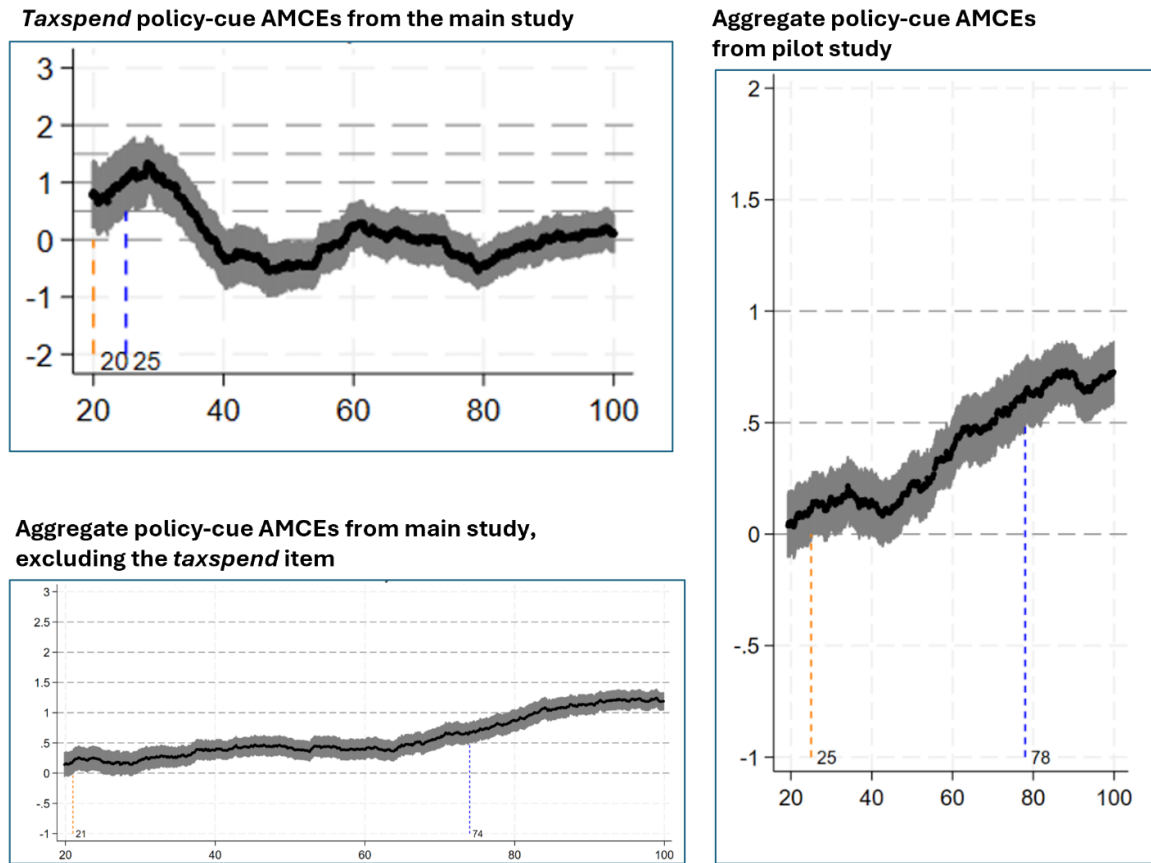
### 3.11. Investigation into the result that less-sophisticated Germans use policy-cues less than similar respondents in other countries

In this section we argue that the somewhat elevated impact of (aggregated) policy-cues for less sophisticated Germans that is apparent in Figure 3.6 is unlikely to reflect substantive differences in how these respondents infer the left-right images of parties relative to respondents in other countries or to more sophisticated German respondents.

First, if we examine the cue-specific versions of Figure 3.6 (Figure A3.7 above and Figure A3.11 below), we see that there is one cue, *taxspend*, that seems to be driving the aggregate result. This item indicated whether the hypothetical party supported or opposed “cutting taxes even if it leads to less public spending.” Importantly, in Figures 3.4 and 3.5, we have already seen that this cue was not particularly salient to voters or polarized by elites for the whole sample. Correspondingly, our estimate of its AMCE for all respondents is quite small. As we show in the upper-left panel of Figure A3.11, however, this does not hold for the least sophisticated Germans in our main-study sample – where the estimate for this cue is very high, reaching ~1.5 for respondents in the lower 30<sup>th</sup> sophistication percentile. As a result, when we include this estimate in our aggregate estimate of the policy AMCE for these respondents, our estimate is both a bit higher than in other countries and is consistently different from zero for these less sophisticated respondents (again unlike in other countries).

We have thoroughly investigated potential sources of this anomalous result, including whether our less sophisticated German voters are somehow unusual compared to other Germans or less sophisticated people in other countries. This investigation revealed nothing systematic in our sampling or design that could explain the result. Further, when we re-estimate our aggregate AMCE (at each level of sophistication) without this question as shown in the bottom-left panel of Figure A3.11, we see that the estimate (and especially the confidence intervals) change such that we would likely not identify the aggregate result as particularly unusual compared to other countries. For example, while there are some low-sophistication cohorts that continue to have AMCEs statistically different from zero, this is no longer consistent across the whole range of less sophisticated respondents and instead looks a lot more like the statistical fluctuations we might expect for a relatively small estimate.

**Figure A3.11: What’s driving the German aggregate policy-cue result for less sophisticated voters?**



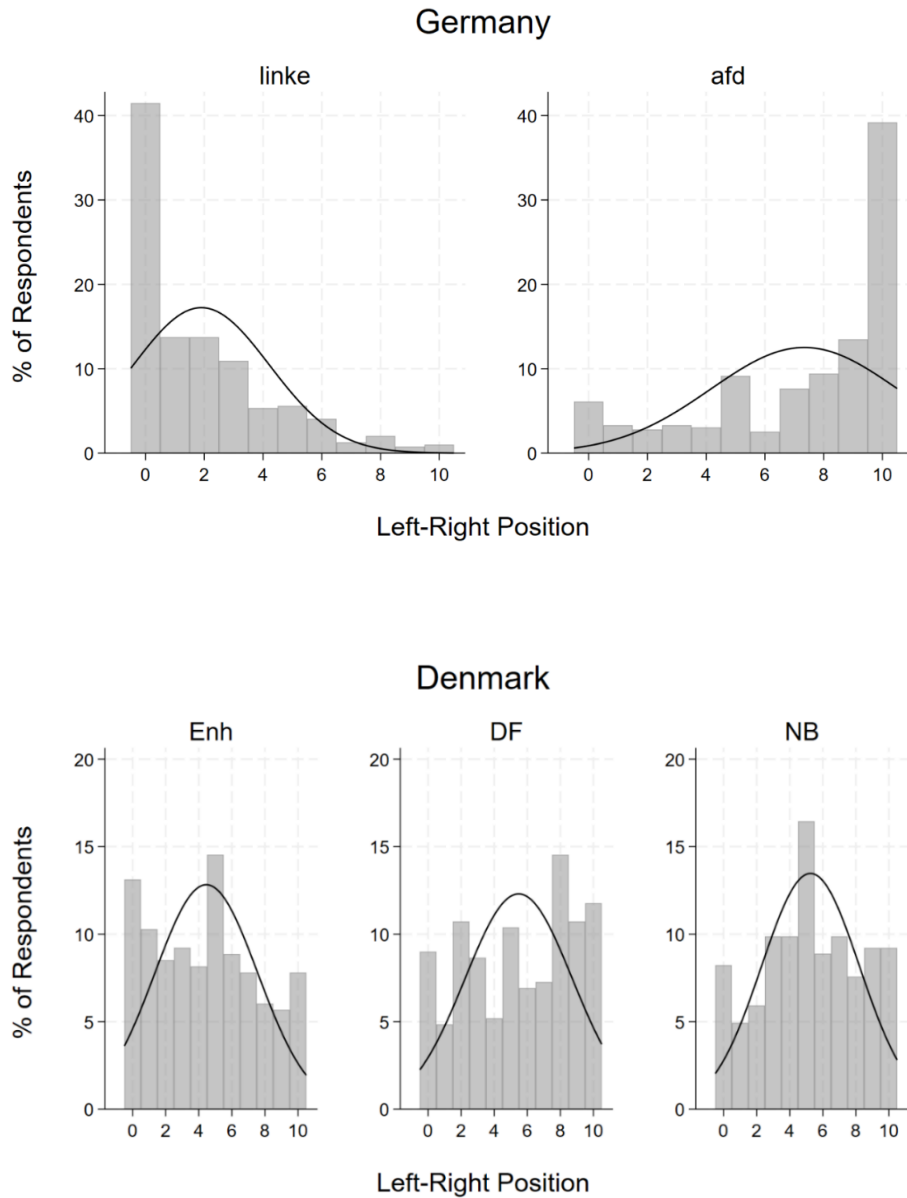
Finally, if we also estimate the AMCE for aggregated policy cues from the German pilot study (right-side panel in Figure A3.11) – which included many (but not all) of the items in the main study including a similar (but not identical) item about taxes and spending -- we see that, while there may be some small indication that less-sophisticated Germans use policy cues more than in other countries, this possibility is much less remarkable.<sup>17</sup>

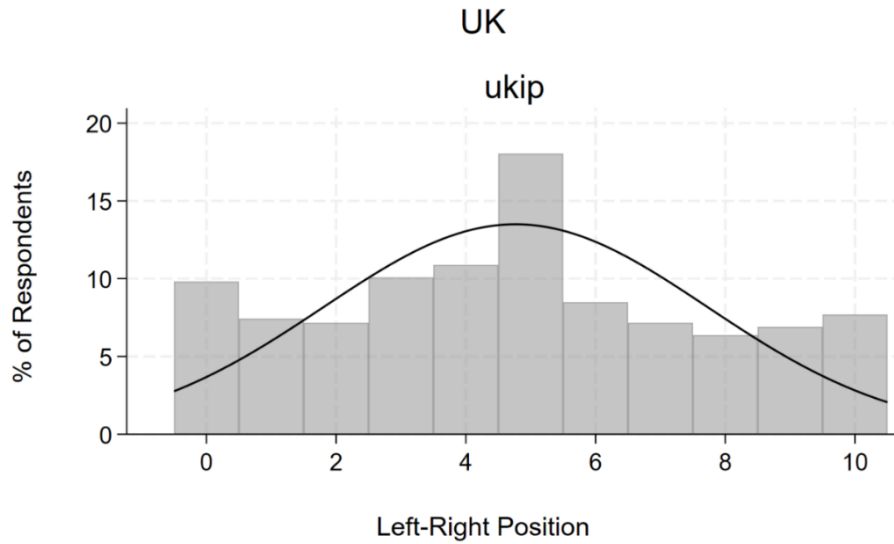
<sup>17</sup> The key difference between our pilot and main conjoint studies lies in how we phrase the attribute related to taxes and spending. In the pilot studies, the hypothetical party’s position was described as either supporting or opposing “cuts to taxes and public spending.” However, we found this phrasing to be potentially confusing, as it does not reflect a realistic policy trade-off—something parties routinely face when forming positions. To address this, we revised this attribute in the main surveys to better reflect meaningful trade-offs associated with tax policy. Specifically, we used two versions: one stating that the party supports or opposes “cutting taxes even if it leads to less public spending,” and another stating that the party supports or opposes “cutting taxes even if it leads to more public debt.”

Taken together, then, we doubt that the somewhat elevated impact of (aggregated) policy-cues for less sophisticated Germans that is apparent in Figure 3.6 reflects substantive differences in how these respondents infer the lefty-right images of parties relative to respondents in other countries or to more sophisticated German respondents.

### 3.12. Extent to which the least sophisticated respondents place extremist parties to the far left or right in our countries

In this section we compare the extent to which respondents in the 40<sup>th</sup> percentile (and below) of political sophistication place extremist parties on the left or right on the extremes.





### 3.13. Additional Information

#### 3.13.1. Additional Detail about footnote 38

To identify the most relevant policies for each county, we have cross-referenced party manifestos, Voting Advice Applications, media summaries of recent campaigns, answers to “most important issue” questions in national election surveys, and the large literature of political science on party competition in European countries.

#### 3.13.2. Additional Detail about footnote 39

While the *a priori* rightist position of “supporting cutting tax even if it leads to increasing public debt” is in the wrong direction in three out of the four countries, these AMCEs are not statistically different than zero. Further, it is likely that this cue conveys somewhat conflicting ideological information about a party that supports tax cuts (a typical right-wing position) but is also willing to accept higher public debt (which could be seen as inconsistent with traditional right-wing values).

#### 3.13.3. Additional Detail about footnote 40

We only use this measure to make comparisons across policies within countries (not across parties or voters), so the number of parties, the distribution of parties’ real positions on the

issues, the extent of real ambiguity in their positions, and any other party-system level variables that might impact the number of Don't Know responses for a given policy are constant, by design, for all respondents. Also, the idea of using the saliency of a policy to voters as a proxy for its salience to the parties relies on the assumption that the overall ability/willingness of our respondents to place parties on a given policy dimension directly reflects the extent to which the parties have successfully emphasized their (and their opponents) positions on the policy. This assumption strikes us as uncontroversial and it has been empirically verified in several studies (Costello et al. 2020, Krosnick 1990, Lavine et al. 1996).

#### 3.13.4. Additional Detail about footnote 41

Relevant citations include Aldrich 2011, DiMaggio et al. 1996, Maoz and Somer-Topcu 2010, Rehm and Reilly 2010.

#### 3.13.5. Additional Detail about footnote 45

The one policy cue that is as strong for sophisticated voters in Germany as it is in other countries is the military spending cue, which was by 2019 a particularly prominent and public source of conflict within the coalition, heightened by increasing U.S. pressure for increases (Gotkowska 2019). Indeed, the issue became an important part of the campaign for (and election of new, coalition-skeptical) left-wing SPD leadership in December 2019 (just after our study was in the field).

#### 3.13.6. Additional Detail about footnote 47

The relatively large impact of values cues AMCE for Germans at the lowest levels of political sophistication (compared to other countries) comes almost entirely from the AMCEs for the diversity value. This is perhaps not surprising given that by the time of our survey in the Fall of 2019, the rise of the AfD had brought issues of diversity around gender, national origin, and sexuality to the forefront of party competition. Indeed, the rise of the AfD and Angela Merkel's decision in 2018 to step down as CDU leader has been explained by some as a direct result of Merkel's decision in 2015 to allow a significant number of refugees into Germany. Further, the effort of the AfD to appeal, on these issues, to some of the least politically sophisticated voters is consistent with the idea that in this particular context, the robust partisan debate touched off by the AfD's strong stands on diversity issues (advocating a strongly hetero-normative, white, and traditional version of the German family) penetrated the consciousness of even quite unsophisticated voters on either side of the debate.

### 3.13.7. Additional Detail about footnote 48

These selections are based on election outcomes from the 2015 Canadian federal election (October), 2017 German federal election (September), 2019 Danish general election (June), and 2-17 UK general election (June). The only exception to this criterion is that we included party cooperation with UKIP as one of our cues although the party did not get a seat in the 2017 UK general election. We have excluded regional parties in Northern Ireland as our sample does not include residents from Northern Ireland.

### 3.13.8. Additional Detail about footnote 52

We entertained the possibility that voters might more easily obtain information about patterns of partisan cooperation for cabinet parties or larger parties (making cues about these target parties cheaper to obtain and so more impactful). However, we found only very weak (though directionally consistent) relationships between our AMCEs for cooperation with a given target and whether it was in the cabinet (or PM) and its size.

### 3.13.9. Additional Detail about footnote 54

One reason why the purity score may not predict a party's left-right position in Canada is the long-standing practice of "brokerage" politics in its political system (Carty and Cross 2010, Cochrane 2010). By engaging in brokerage politics, parties act as intermediaries between various interest groups within society. The aim is to balance these diverse interests to build a broad coalition of support, which involves negotiating, compromising, and distributing resources or favors to different groups to maintain political stability and achieve electoral success (Johnston 2010). This approach often leads parties to shift positions on major issues and adopt programs they previously opposed. For instance, the Liberal Party's success as a centrist party partly stems from campaigning on the Left while governing on the Right. Similarly, while the Conservatives are typically seen as a right-wing party, there have been times when they have positioned themselves to the Left of the Liberals. For both parties, the primary goal is to find the position that will secure them a majority government at that time (Carty and Cross 2010). Therefore, the "purity" a party's pattern of cooperation may not be a reliable predictor of its left-right placement in Canada.



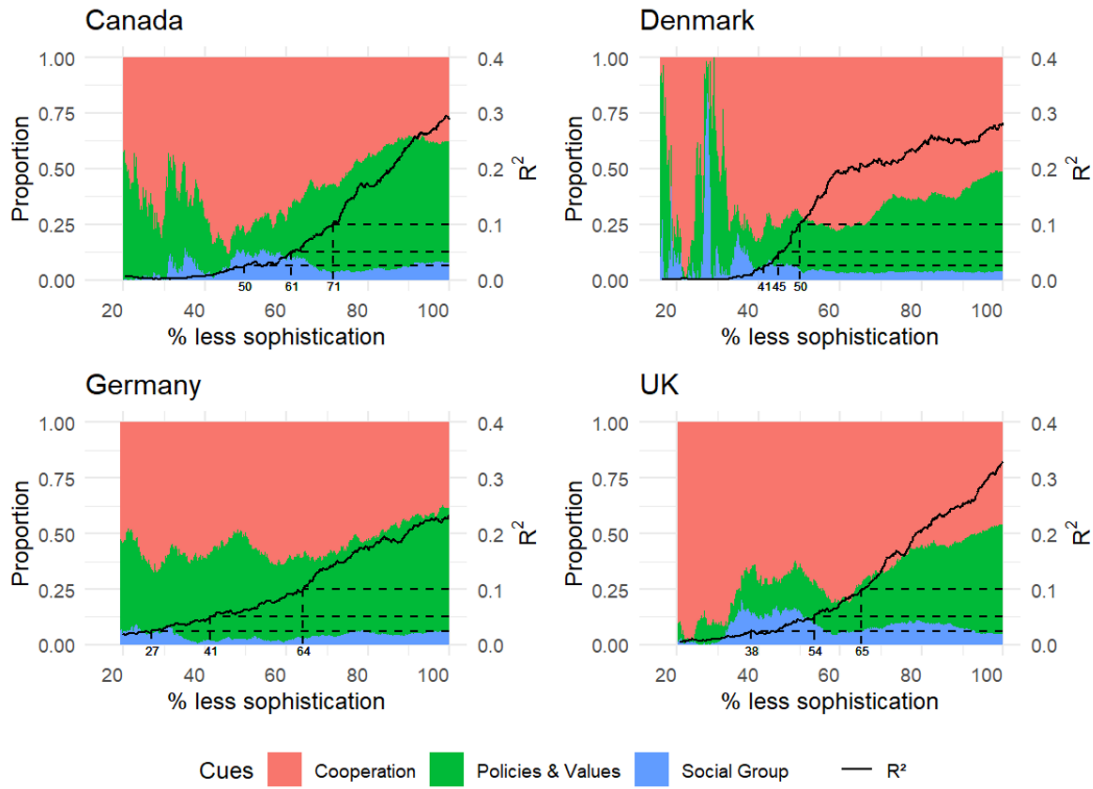
## Appendix to Chapter 4

### 4.1 Alternative estimation of the relative importance of cue categories to voters' left-right images of parties

The fact that we included policy and values in the same “slot” in our conjoint design complicates the evaluation of the variance explained by each cue category (with policy and values treated separately) since cues in these categories are, by definition, not independent. That is why in the main text we simply examine the proportion of the sum of our the AMCEs over cue categories to get a good approximation to this. Formal parsing of the variance explained, however, can be accomplished by combining the values and policy dimensions and then doing a Shapely decomposition of the variance explained by the model that includes these three cue categories. The computation is done in R using the subset procedure defined in Broto et al. (2020), which computes all the R-squares for all possible sub-models first and then estimating the Shapley weights according to the Lindeman et al. (1980) definition.

The resulting figure, which can be compared directly to Figure 4.3 in the text if one combines the policy and values categories there, is below and tells the same story as that in the main text, though it even more dramatically shows the differential reliance of respondents at different levels of sophistication on cooperation vs. policy/values cues.

**Figure A4.1: Proportion of variance in party placements attributable to each cue category**



## 4.2 Designing Observational Studies to be Parallel with the Conjoint Experiment

In this Online Appendix, we investigate whether the insights from our conjoint experiment extend to the voters' perceptions of the left-right positions of parties in the real world. This is an important question for any conjoint analysis because it speaks to a frequent criticism of such studies: that the psychological processes that govern respondents' answers to questions in the hypothetical environment of the conjoint survey may not be similar to the psychological process they use for related tasks in the real world. While we argue that this is likely less of a problem in conjoint studies of meaning vs. choice, by designing and conducting an observational study that closely parallels the conjoint design (with the same respondents), we can scrutinize this intuition to some extent.

We construct our parallel observational study by asking our respondents (after the conjoint tasks were completed) their perceptions of the left-right placements, policy positions, values, bases of social support, and patterns of partisan cooperation of the real-world parties in their country. Importantly, this parallel study asks the relevant questions in the same way as the attributes used in the conjoint study – allowing for meaningful comparisons between the two

approaches. We are aware of no other conjoint study that has been paired with this kind of parallel observational. However, we think that adoption of this practice could go a long way toward addressing the persistent doubts about the generalizability of conjoint results outside of the experimental setting (or pinpointing studies in which those criticisms have bite). More generally, such designs will allow researchers to better understand the extent to which the observational designs about real world attitudes and behaviors tell the same story as experimental ones.

In the sections below, we provide the details of the survey items from the observational study and discuss what we did to ensure we could use these items to produce estimated associations that are comparable to the AMCEs for the same attributes that we estimate from the conjoint design.<sup>18</sup>

#### 4.1.1 Policies and Values

In our surveys, after the respondents completed the conjoint trials, they were asked about their perceptions of the position of the real-world parties in their country on the same policy and values dimensions that we used in the conjoint experiment. For example, one of the policy attributes that we used in our conjoint experiment is about government regulation of financial markets. In the conjoint experiment, those respondents who were randomly selected to get information about this issue received a statement saying the hypothetical party either supports or opposes increasing government regulation of financial markets. Similarly, when questioning respondents about real parties, we asked, “On the issue of how much the government should regulate financial markets, where would you place the following parties? Please use a scale from 1-7 where 1 means supporting policies that increase government regulation of financial markets and 7 means supporting policies that decrease regulation of financial markets?”<sup>19</sup>

#### 4.1.2 Patterns of Conflict and Cooperation

Besides the policy and values questions, we also asked our respondents about their perceptions of how much each pairing of parties in their countries cooperate with each other. This is parallel with the conflict and cooperation attributes in our conjoint experiment where we presented our respondents information about whether the hypothetical new party in their respective countries cooperates with an existing party. For example, in our Canadian survey, respondents who were randomly chosen to get information about the hypothetical party’s relationship with

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<sup>18</sup> Since the effect of size, regional scope, and gender-specific characteristics of parties are relatively small, we do not include them in our observational analyses.

<sup>19</sup> We chose to use a 7-point scale rather than simply asking a 0/1 version of the question because ng so let us more readily use this question for other purposes, without (in our opinion) compromising the comparability of the variable with the corresponding conjoint attribute.

the Liberal Party, saw a statement that said the hypothetical party either “Often cooperates with the Liberals” or “Seldom cooperates with the Liberals”. In the observational part of that survey, respondents were shown each pair of Canadian parties and asked the following question:

How often do you think these two parties cooperate with each other in [your country's] federal politics? [answer categories: (0) “Never cooperate” to (10) “Almost always cooperate”]

This results in a set of cooperation scores for party dyads that we can use, as described in section 4.2 below, to build a set of variables that tell us, for each real party, how much each respondent believes that party cooperates with each of the others.<sup>20</sup>

Additionally, we ensured that respondents could not skip the question for any party pair or respond with "Don't Know." Instead, we encouraged them to make an informed guess if they were uncertain. The figure indicates that respondents had no difficulty providing sensible responses to the question.

#### 4.1.3 Social Group Support

Finally, to measure the perceived demographic support of the real parties, we asked each of our respondents which social groups normally give a strong support to each of the parties in their respective countries. This is parallel with the conjoint items that present the hypothetical parties as either having strong support or weak support from a particular social group. Figure A4.1 displays the survey question that our British respondents encountered when answering the question about which social groups support the Conservative Party.

If a respondent chose "working class people" in this question, we coded them as perceiving that the Conservative Party has strong support among working-class individuals. Conversely, if they did not choose "working class people," we coded the respondent as thinking that the Conservative Party had “weak” support from this demographic group.<sup>21</sup>

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<sup>20</sup> Respondents in Canada, Germany, and the UK were presented with all possible pairing of parties. However, for our Danish respondents, we randomly chose 6 parties and presented all possible pairing among these 6 parties to our respondents.

<sup>21</sup> We allowed a slight slippage in comparability to let us keep these questions compact and quickly answerable. Specifically, the observational question mentions strong support explicitly and only assumes that “not strong support” equal “weak support.”

**Figure A4.2: Example of Survey Questionnaire for Social Group Attributes (UK Example)**

Which of these groups do you think normally give a strong support to the **Conservative Party**? Please select all that apply.

<input type="checkbox"/> Working class people	<input type="checkbox"/> People who have university degree
<input type="checkbox"/> Middle class people	<input type="checkbox"/> People who do not have university degree
<input type="checkbox"/> Upper class people	<input type="checkbox"/> Religious people
<input type="checkbox"/> People living in rural areas	<input type="checkbox"/> Non-religious people
<input type="checkbox"/> People living in urban areas	<input type="checkbox"/> Women
<input type="checkbox"/> People living in suburban areas	<input type="checkbox"/> Men
<input type="checkbox"/> Union members	<input type="checkbox"/> Older people
<input type="checkbox"/> Non-union members	<input type="checkbox"/> Younger people

## 4.2 Empirical Analysis

To obtain the necessary estimates in the observational analysis that are comparable to the AMCEs from our conjoint design, we conduct a party-level observational study to estimate the effects of the same attributes from our conjoint experiments on left-right placements using respondents' perceptions of these attributes for real parties. To do this, we stack the data so that a row is a respondent-party (for all real parties) and the dependent variable is the respondent's placement of each real party on the 11-point left-right scale.

For the independent variables, the coding of the policy and values variables is straightforward – we just use the respondent's placement of each real party on each 7-point policy or values scale. In this coding, a higher number indicates a more rightist position, which means that a positive coefficient for one of these attributes indicates that if a party is perceived to have more rightist policies or values, it will also be placed more to the right in a left-right scale.

For the social support variables, since we included a question that allowed respondents to indicate which social groups they thought strongly supported (or did not strongly support) each party, we could simply include a dummy variable in the model for each social group, where a "1" indicates a respondent thought that the social group strongly supported the party. However, to ease interpretation of the results, we modify this a bit. Specifically, for groups that we judge, a priori, to usually support leftist parties (i.e. union households, women, university graduates), we code the corresponding dummy variable as a 0 if the respondent thinks the group strongly supports the party and 1 if they do not. Conversely, for groups that we judge to usually support rightist parties (i.e. religious voters, men, rural voters), we code the corresponding dummy variable as a 1 if the respondent thinks the group strongly supports the party and 0 if they do not. Thus, the coefficients on social group attributes can be interpreted as the extent to which perceptions of a party's left-right position shift to the right when the party is perceived as being supported by a given group

For partisan cooperation, which is measured in the survey as a dyadic variable (e.g., respondents are asked how much on an 11-point scale each pair of the real parties cooperated), we transform the dyadic measure to a party level variable, by using it to construct a set of variables for cooperation with each existing party in a country. Recall that the data is stacked so that a row is a respondent-party. Thus, for example, in the UK we include a variable in our model that is called "cooperates with the Conservatives" and for each respondent-party row in the data (for the UK respondents) we fill that variable in with the respondent's answer on our 11-point dyadic cooperation score for cooperation between that party (the one for that row for

that respondent) and the Conservatives.<sup>22</sup> This is repeated for each party in the system (so we get a set of variables called “cooperation with [party X]”). Thus, a coefficient on one of these variables tells us how much, on average, our respondents would move each real party to the right or left if they **were to believe that the party cooperates one point (on our cooperation scale) more with the Conservatives**. Note that we also control for how much the party cooperates in general, by including a variable that captures the sum of our cooperation scores for each party. Table A4.1 presents the regression results.

**Table A4.1: Regression Results from Observational Analyses.**

	Canada	Denmark	Germany	UK
CutTaxSpend	0.06* (0.03)	0.06* (0.02)	0.02 (0.02)	0.10** (0.02)
CutTaxDebt	-0.00 (0.03)	0.02 (0.02)	-0.01 (0.02)	-0.06* (0.02)
HealthSpend	0.00 (0.03)	-0.02 (0.02)	-0.02 (0.02)	0.05 (0.03)
LimImm	0.14** (0.03)	0.08** (0.03)	0.16** (0.02)	0.07* (0.03)
GovReg	-0.00 (0.03)	0.07** (0.03)	-0.02 (0.02)	0.05 (0.03)
Marijuana	0.01 (0.03)	0.04 (0.03)	0.04 (0.02)	
LimSurv/ExpSurv	0.04 (0.03)	-0.01 (0.02)	-0.04* (0.02)	0.05* (0.02)
MilSpend	0.22** (0.03)	0.11** (0.03)	0.05* (0.02)	0.14** (0.03)
TaxFuel	0.05 (0.03)	0.15** (0.03)	0.03 (0.02)	-0.05 (0.03)
EuCoop		-0.02 (0.02)	0.05* (0.02)	
Brexit				0.02 (0.03)
IncomeEq	0.21** (0.03)	0.20** (0.03)	0.11** (0.02)	0.23** (0.03)
CultDiv	-0.06 (0.03)	0.10** (0.03)	0.09** (0.02)	0.03 (0.03)
Liberty	0.15** (0.03)	0.05 (0.03)	0.04 (0.02)	0.08** (0.03)
BigGovRole	0.05 (0.03)	0.19** (0.03)	-0.01 (0.02)	0.01 (0.02)
GenderEq	0.04	-0.06**	-0.01	0.01

<sup>22</sup> We assigned a cooperation score of "10"—the highest value on our response scale—to the Conservative-Conservative dyad. Alternatively, coding this as missing yields the same results.

	(0.03)	(0.02)	(0.02)	(0.03)
Working	-0.28**	0.15	0.06	-0.01
	(0.10)	(0.09)	(0.08)	(0.09)
Middle	0.25*	0.13	0.15*	0.37**
	(0.10)	(0.08)	(0.07)	(0.09)
Upper	0.36**	-0.08	0.12	0.18
	(0.11)	(0.10)	(0.08)	(0.11)
Women	-0.19	-0.26**	0.34**	-0.08
	(0.12)	(0.10)	(0.08)	(0.09)
Men	0.13	-0.08	0.17*	0.06
	(0.11)	(0.08)	(0.08)	(0.10)
Union	0.24*	-0.06	0.06	0.18
	(0.11)	(0.09)	(0.08)	(0.10)
Relig	0.10	0.23*	-0.01	0.18
	(0.13)	(0.10)	(0.09)	(0.12)
Rural	0.08	0.10	0.11	0.18
	(0.12)	(0.10)	(0.08)	(0.11)
Urban	0.10	0.06	-0.21**	-0.04
	(0.12)	(0.11)	(0.07)	(0.11)
Suburban	0.25*	-0.06		0.23*
	(0.12)	(0.10)		(0.12)
Univ	-0.05	0.02	-0.05	-0.03
	(0.11)	(0.09)	(0.08)	(0.09)
Old	0.26*	0.10	-0.01	0.16
	(0.12)	(0.09)	(0.08)	(0.10)
Young	0.11	-0.01	0.04	-0.11
	(0.12)	(0.09)	(0.08)	(0.10)
Lib	-0.02	0.02		
	(0.02)	(0.01)		
Sd		0.01		
		(0.01)		
Ven		0.00		
		(0.01)		
DF		0.06**		
		(0.01)		
RadVen		-0.03*		
		(0.01)		
SF		-0.04**		
		(0.01)		
Enh		-0.09**		
		(0.01)		
KF		0.06**		
		(0.01)		
Alt		-0.05**		
		(0.01)		
NB		0.07**		
		(0.01)		
Green	-0.00		-0.00	-0.06**

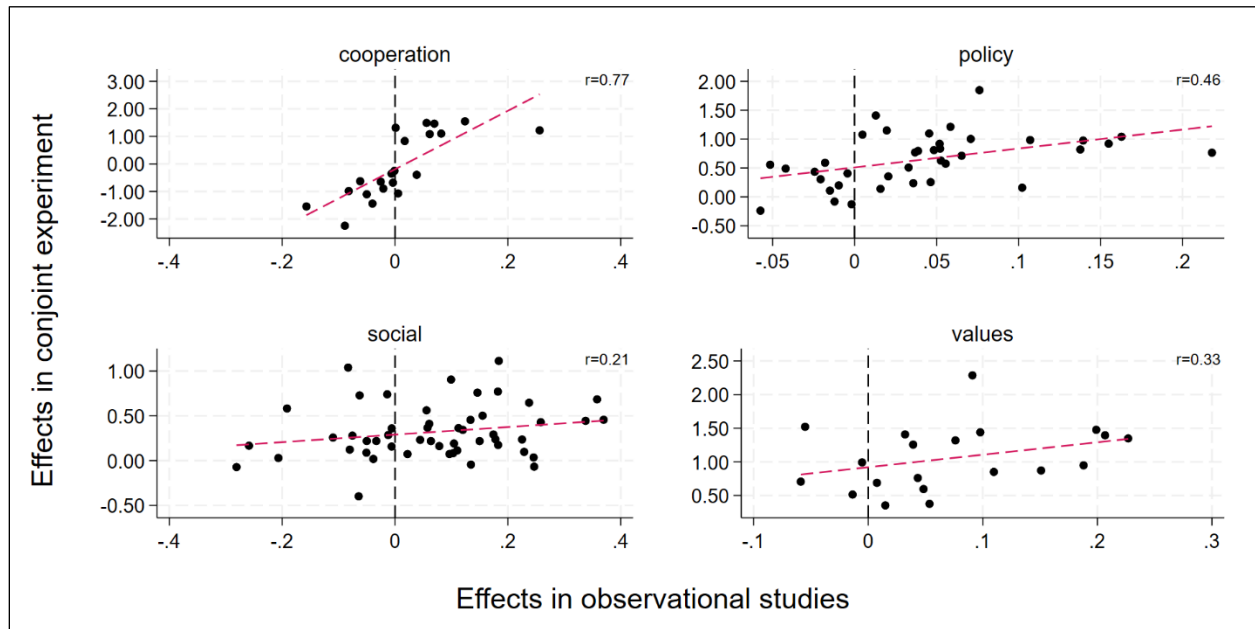
	(0.02)		(0.01)	(0.02)
NDP	-0.08**			
	(0.02)			
Cons	0.26**			0.12**
	(0.02)			(0.02)
BQ	-0.00			
	(0.02)			
CDU			0.10**	
			(0.01)	
SPD			0.01	
			(0.01)	
FDP			-0.01	
			(0.01)	
AfD			0.18**	
			(0.02)	
Linke			-0.29**	
			(0.01)	
Lab				-0.16**
				(0.01)
LibDem				0.04*
				(0.02)
SNP				-0.01
				(0.01)
UKIP				0.08**
				(0.02)
Total Coop	0.10	-0.03	-0.06**	-0.14**
	(0.08)	(0.04)	(0.01)	(0.04)
Constant	-2.76	2.03*	4.29**	5.68**
	(2.01)	(1.02)	(0.44)	(1.06)
Observations	2,535	3,728	3,417	3,280
R-squared	0.45	0.49	0.60	0.48

Standard errors in parentheses, \*\*  $p < 0.01$ , \*  $p < 0.05$

In Figure A4.3, we provide a direct comparison between the estimated effects of each attribute in our conjoint experiment to estimates from our observational study of the same attributes. This figure simply plots the raw coefficients from the conjoint models in Chapter 3 and those from the observational study in Table A4.1.<sup>23</sup>

<sup>23</sup> Note that while in the conjoint experiment, the randomization of attributes means we can estimate the effect of any given attribute by altering a single attribute within a profile while averaging over the distribution of remaining profile attributes, the appropriate comparison in the observational studies is the estimated effect for that attribute *controlling* for all the other attributes in the model. Thus, the observational estimates reported above are from a single model that includes all perceived attributes.

**Figure A4.3: Correspondence between the Estimated Effects in the Conjoint Experiment and the Estimated Effects in the Observational Studies**



Note: Values on the y-axis are the effects taken from Figures 3.1, 3.7, 3.12, and 3.16 in Chapter 3. The numbers on the x-axis are coefficients from the regressions in Table A4.1. Since we are only concerned with the correlation between the numbers on the x and y axes, we do not try to make their absolute levels comparable and instead leave both estimates in their raw, unstandardized forms.

The message from Figure A4.3 is clear. Across all attribute categories, there is a positive correspondence between our conjoint estimates and those from our observational analyses. Notably, the correspondence is strongest for cooperation ( $r = 0.77$ ), indicating that voters' left-right placement of parties in observational studies aligns particularly well with their judgments in the conjoint experiment. This suggests that both methods lead to similar conclusions about the relative impact of cooperation on how voters perceive party's placement on a left-right scale.

For policy ( $r = 0.46$ ), values ( $r = 0.33$ ), and social groups ( $r = 0.21$ ), the correspondence is weaker but still positive. However, the lower correlations indicate that we are less likely to reach the same conclusions about the relative impact of these attributes when comparing observational and experimental findings. This weaker alignment likely stems from statistical noise in the observational estimates, where multicollinearity makes it more difficult to isolate the effect of individual attributes on party placement.

To better understand how multicollinearity manifests in observational data and affects its correspondence with conjoint estimates, we examined the degree of correlation between different attributes in the observational study. Specifically, we calculated the pairwise

correlations between individual attributes and then averaged them within each attribute category. For example, we computed all correlations between values and policy attributes, between values and cooperation, and so on, before summarizing them into category-level averages. Table A4.3 presents these average correlations across four countries. Notably, values consistently rank among the most strongly correlated categories, appearing in the top two across all four countries. This suggests that voters do not evaluate values in isolation when positioning parties on the left-right spectrum, leading to greater statistical noise in observational estimates. As a result, multicollinearity makes it more difficult to isolate the independent effect of values in observational analyses, thereby weakening the correspondence between observational and conjoint results.

**Table A4.2: Average Correlations Between Attribute Categories in Observational Data**

Attribute Pairs	Average Correlation Coefficient			
	Canada	Denmark	Germany	UK
Values-Policy	0.3	0.32	0.33	0.29
Values-Cooperation	0.23	0.23	0.24	0.26
Policy-Cooperation	0.2	0.18	0.21	0.22
Values-Social Group	0.12	0.14	0.11	0.1
Cooperation-Social Group	0.11	0.12	0.1	0.11
Policy-Social Group	0.1	0.12	0.1	0.08

Nevertheless, the overall positive correlations across the four attribute categories suggest that our conjoint experiment does capture key patterns observed in real-world party’s placement. This should strengthen our confidence that the conjoint experiment is tapping into cognitive processes that are broadly similar to the ones voters use when they think about the left-right positions real parties. This should not be too surprising given the usual understanding of the way that concepts are schematically connected to other concepts in the brain. When our surveys ask about the left-right positions of hypothetical parties, many voters likely activate the same (or quite similar) set of schematic connections that govern how they think about real parties and their ideological relationships and so both analyses likely reveal similar connections (and the strength of those connections).<sup>24</sup> As such, we are assured that the results from our

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<sup>24</sup> We are not suggesting that the results in Figure A4.3 are a formal test of this claim, but simply emphasize that we should expect the relative strength of connections between the left-right and other concepts (like policy positions and levels of cooperation) to be similar no matter whether we try to measure the strength of these connections in a (properly controlled) observational design or a conjoint design. Further, other than the possibility that the observational design produces large biases in the estimates, we can think of no theoretical reason (i.e. one rooted in an argument about differences in the cognitive processes respondents use to answer both types of questions) to expect this is not true.

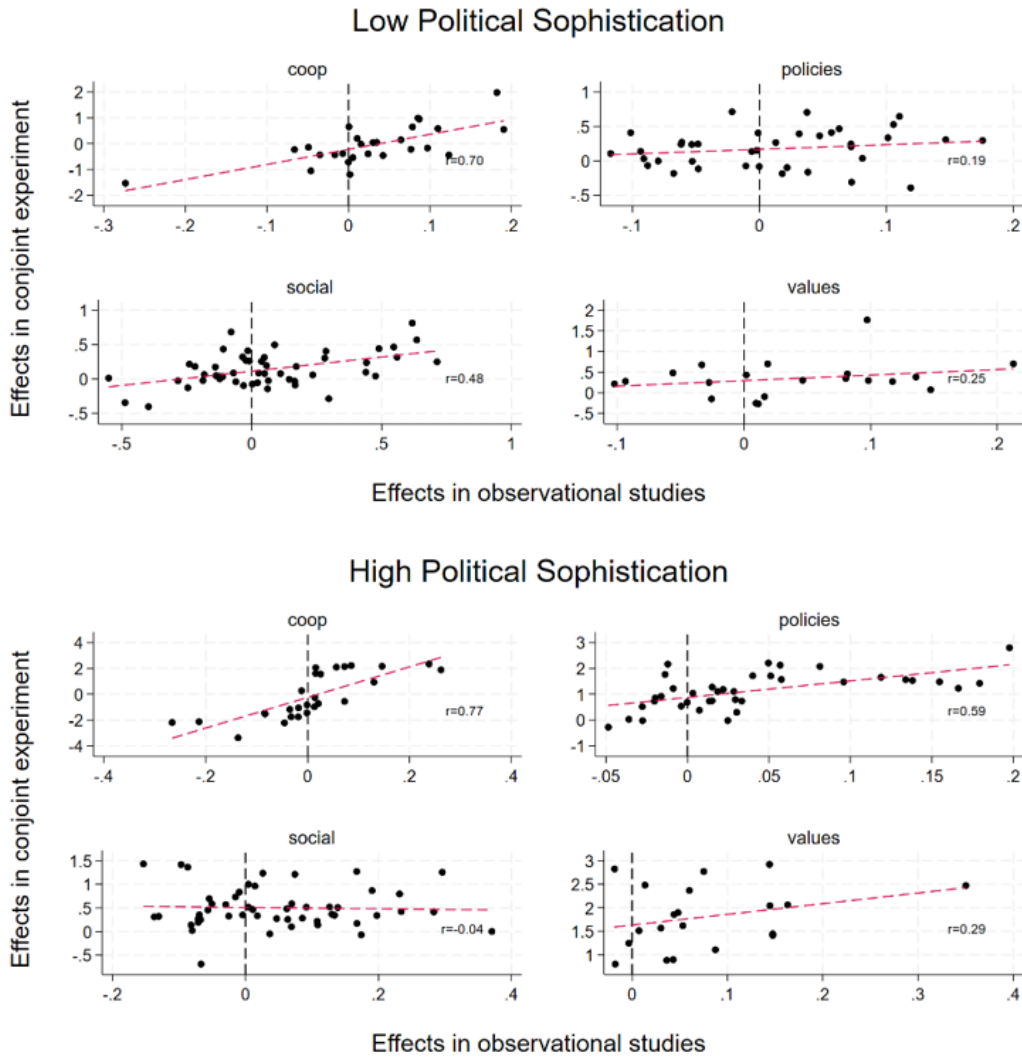
hypothetical parties in the conjoint from Chapter 3 can reasonably be generalized to real-world parties.

### 4.3 The Role of Political Sophistication

Given the importance of political sophistication in shaping how individuals weigh different cues in the conjoint experiment, we examine whether the correspondence between estimates from observational analyses and those from conjoint experiments varies by sophistication level. If politically sophisticated individuals show greater alignment across the two methods, it suggests that researchers can derive similar conclusions regardless of the approach used, even in observational analyses that are often affected by multicollinearity. To test this, we divide the sample into high and low political sophistication groups based on their sophistication score, as described in Chapter 3, with the top 50% classified as high sophistication and the bottom 50% as low sophistication.

Figure A4.4 illustrates the relationship between observational and conjoint estimates across the four attribute categories for both sophistication groups. The key takeaway is that the degree of correspondence between observational and conjoint estimates of party placement cues depends on both the cues and the respondent's level of political sophistication. Notably, the strong alignment for cooperation cues across both high and low sophistication groups confirms that even less sophisticated voters reliably use partisan cooperation as a heuristic when positioning parties. In contrast, while policy cues yield much greater consistency among high-sophistication respondents, we find less alignment in the weight of policies cues among lower sophistication respondents. Moreover, the weak correspondence for value cues—likely a consequence of multicollinearity in the observational data—suggests that respondents do not uniformly weigh values highly across different methods. Finally, although the conjoint experiment reveals a modest effect of social group cues for high-sophistication respondents (see Figure 3.19 in Chapter 3), this impact does not carry over into the observational analysis. Together, these results highlight that drawing equivalent conclusions from observational data on the relative weight of various cues on parties' left-right position depend on both the cue under consideration and the political sophistication of the voter.

**Figure A4.4: Correspondence between the Estimated Effects in the Conjoint Experiment and the Observational Studies by Different Levels of Political Sophistication**



#### 4.4. Differences in the Conclusions from the Conjoint and Observational Studies

What would we have concluded about the drivers of left-right party placements had we only done this observational analysis? Table A4.3 summarizes the difference between the results from our conjoint experiments with our observational analyses. As the previous section confirmed, several broad conclusions from our conjoint study are also reflected in these results. Most importantly, the impact of partisan cooperation is clearly apparent, and we see a similar pattern to the conjoint analysis: the largest effects tend to be associated with parties that are perceived as the least centrist (e.g., AfD and Linke in Germany, the Conservatives and Labour in the UK, the Conservatives in Canada, and Enhedslisten and NB in Denmark). Beyond that, the only party attribute that consistently shows a strong relationship to left-right placements across all four countries is parties' positions on income inequality.

Thus, while this one result supports our earlier conclusion that broad values—particularly economic values—play an important role in left-right placements, we do not see all (or even most) of the broad values we measured significantly impacting placements. Indeed, the overall lack of statistical significance for many value measures highlights a much broader issue with the observational results: while the conjoint analysis identified many significant drivers of left-right placements, the observational study does not. For example, the number of significant correctly signed effects is consistently higher in the conjoint analysis across all four countries, whereas the observational study frequently produces fewer significant effects and, in many cases, even wrong-signed estimates (e.g., in Germany and Denmark, where cooperation and social attributes show particularly high rates of incorrect signs).

Further, the estimated effect sizes in the observational study—except for the largest cooperation effects—are generally much smaller than their corresponding effects in the conjoint experiment. This pattern of results, which are mostly directionally consistent but weaker and often statistically insignificant, is exactly what we would expect from a large set of highly interrelated predictors in an observational study. The contrast with the experimental results, in which the predictors are uncorrelated by design, highlights the general advantage of an experimental approach: even when an observational analysis reveals the correct broad patterns, covariation between predictors makes it difficult to identify more subtle relationships—because estimated effects tend to be imprecise. Worse, in this case, simply adding more respondents to the observational study is unlikely to resolve the issue, as the problem stems from a lack of independent variation in party attributes rather than sample size constraints.

**Table A4.3: Summary of Attribute Categories in Conjoint and Observational Studies**

Country	Attributes	Largest		Smallest (Correct Sign)		No. of wrong signed effects		No. of significant correct signed effects	
		Conjoint	Observational	Conjoint	Observational	Conjoint	Observational	Conjoint	Observational
Canada	Policies	marijuana	limlmm	cutdebt	healthspend	0	2	7	3
	Values	diversity	liberty	biggovrole	biggovrole	0	1	5	2
	Cooperation	Cons	Cons	BQ	BQ	0	0	5	2
	Social	relig	upper	middle	urban	2 (0)	3 (1)	7	5
Denmark	Policies	limlmm	energy	expsurv	cutdebt	0	2	8	5
	Values	incomeEq	incomeEq	liberty	liberty	0	1 (1)	5	4
	Cooperation	Enh	NB	RadVen	Ven	0	1	10	6
	Social	union	relig	relig	univ	2	6 (1)	8	2
Germany	Policies	limlmm	limlmm	govreg	cutspend	0	4 (1)	8	2
	Values	diversity	incomeEq	biggovrole	liberty	0	2	5	2
	Cooperation	AfD	Linke	CDU	green	0	1	6	3
	Social	union	men	middle	young	0	4 (1)	8	3
UK	Policies	brexit	milspend	govreg	limsurv	0	1 (1)	7	4
	Values	incomeEq	incomeEq	biggovrole	genderEq	0	0	5	2
	Cooperation	Cons	Lab	SNP	SNP	0	1 (1)	6	4
	Social	upper	middle	men	men	0	3	9	2

Note: The values in parentheses indicate the number of attributes within that attribute category that are statistically significant.

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