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## SUPPORTING INFORMATION

# *Territorial Representation and the Opinion-Policy Linkage: Evidence from the European Union*

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### 1) Formalization of the theoretical argument

Here, I present some formalization of the argument I make in the theory section of the article. First, we can define representatives' preference functions in the cases of policy change (1) versus no policy change (2) as follows:

$$U_{i,j}^{Change} = S_{i,j} * (O_{i,j} - 0.5) \quad (1)$$

$$U_{i,j}^{SQ} = 0 \quad (2)$$

Where  $i$  denotes representatives and  $j$  policy issues.  $S_{i,j}$  is the salience (or importance) a representative's constituents attach to a policy issue and  $O_{i,j}$  is the constituents' support for policy change on the issue (i.e. the fraction of constituents supporting change). Note that this utility is positive in the case of policy change that is supported by a majority of constituents (i.e. the median voter in the state or sub-national unit), and negative otherwise. This represents that constituents will use the next elections to reward (sanction) representatives for past policy-making that was in line with (against) their preferences. The size of the reward (sanction) depends on issue salience, which ties in with key arguments and findings from the policy representation literature (Jennings and John 2009; Lax and Phillips 2009; Soroka and Wlezien 2010; Wlezien 2004). If no policy change occurs, the status quo (SQ) prevails and representatives' utility is zero.

Under no territorial representation, for any given issue  $j$  the utility from policy change is identical for all representatives, since they all are elected in the same polity-wide constituency:

$$U_{i,j}^{Change} = S_{i,j} * (O_{i,j} - 0.5) = S_j * (O_j - 0.5) = U_j^{Change} \quad (3)$$

This leads to straightforward expectations about the relationship between opinion and policy change, since all representatives in the coalition will agree on whether to support policy change or not. First, consider *responsiveness*, i.e. the question of whether a change in opinion induces changes in policy. The probability of policy change on issue  $j$ ,  $\Pr(Y_j^{Change})$ , simply increases with the utility representatives de-

rive from the change, which is in turn a function of constituents' opinion and salience:

$$\frac{\Delta \Pr(Y_j^{Change})}{\Delta U_j^{Change}} > 0 \quad (4)$$

Second, consider the *congruence* of policy-making with opinion, i.e. an actual match between majority opinion (i.e.,  $O_j - 0.5$ ) and policy. The joint probability that majority opinion is congruent with adopted policy. That is, the majority favors change and it occurs,  $\Pr(Y_j^{Change} | O_j - 0.5 > 0)$ , or the majority favors the status quo and it prevails,  $\Pr(Y_j^{SQ} | O_j - 0.5 \leq 0)$ , simply increases with the difference in representatives' utility between policy change and the status quo (i.e.,  $|U_i^{Change} - U_i^{SQ}| = |U_i^{Change} - 0|$ ):

$$\frac{\Delta \Pr(Y_j^{Change} | O_j - 0.5 > 0) + \Delta \Pr(Y_j^{SQ} | O_j - 0.5 \leq 0)}{\Delta |U_j^{Change}|} > 0 \quad (5)$$

In contrast, under territorial representation, equation (3) does not hold any longer, that is, the utilities from policy change vary between the representatives in the coalition due to regional variations in voter preferences.  $U_{i,j}^{Change}$  does not simplify to  $U_j^{Change}$ . The more (the products of) opinion and salience vary between constituencies, the more likely it is that some representatives favor policy change on an issue, while others favor the status quo.

To maximize their utility from a set of issues  $J$ ,  $\sum_{j=1}^J U_{i,j}$ , representatives engage in log-rolling or vote trading across issues. In essence, representatives agree to undesirable policy changes or SQs on issues that weigh little with their constituents, and in return receive the support of other representatives on issues their constituents care about and have a clear opinion on. More precisely, representatives will trade their vote on issues for which they face a small utility difference between policy change and the SQ, and they ask for the support of others on issues where this difference is substantial.

Let exogenous and endogenous bargaining power in these exchanges be represented by  $P$  for representative  $i$  and a total number of representatives  $I$  in the policy-making coalition, such that  $\sum_{i=1}^I P_i = 1$ . According to Coleman (1966b, 1966a), the ideal collective decision under such a setting is given by choosing either policy change or no change depending on which option has the greater sum of power-weighted utility. That is, whether  $\sum_{i=1}^I P_i U_{i,j}^{Change}$  is greater 0 (since  $U_{i,j}^{SQ} = 0$ ). Hence, considering responsiveness,  $\Pr(Y_i^{Change})$  should increase with either the power of the supportive representatives in the coalition, or the utility they derive from the change, which is in turn a function of constituents' opinion and salience (see (1)):

$$\frac{\Delta \Pr(Y_j^{Change})}{\Delta \sum_{i=1}^I P_i U_{i,j}^{Change}} > 0 \quad (6)$$

As a result, policy-making under strong territorial representation should be more responsive to certain constituencies with power, clear opinions, and to whom the issue is highly salient than to mean polity-wide opinion. These constituencies may vary on an issue-by-issue basis and their citizens may favor different policies than the system's median citizen.

Similar conclusions apply to congruence. The probability that majority opinion in a particular constituency is congruent with adopted policy should increase with the representative's power as well as the difference in utility between policy change and the status quo:

$$\frac{\Delta \Pr(Y_j^{Change} | O_{i,j} - 0.5 > 0) + \Delta \Pr(Y_j^{SQ} | O_{i,j} - 0.5 \leq 0)}{\Delta (P_i | U_{i,j}^{Change})} > 0 \quad (7)$$

Hence, as voters in a constituency either care more about or have more uniform opinions on an issue, congruence of policy-making with their views becomes more likely, as their representative will focus on these issues in her log-rolling strategies. Note that without territorial representation, congruence is theoretically unrelated to constituency-level opinion and salience.

## 2) Variable definitions, sources and data collection

Table A1 provides an overview of the definitions and sources of all main variables used in the article. The codebook in the replication files provides definitions and sources for all other variables used in the analyses.

**TABLE A1: OVERVIEW OF VARIABLES' DEFINITIONS AND SOURCES**

Variable	Definition	Source
<i>Policy change</i>	<p>"1" = Policy change occurred (adoption degree <math>\geq 80\%</math>)</p> <p>"0" = No change occurred (adoption degree <math>&lt; 80\%</math>)</p>	Author's own data
<i>Congruence</i>	<p>"1" = Policy change occurred and opinion majority in favor of change, or no change occurred and opinion majority against change</p> <p>"0" = Otherwise</p>	Author's own data, Eurobarometer survey series
<i>Opinion under "Council: equal power" specification</i>	0-1, weighted mean of support for policy change across member states participating in the policy area (as fraction, excluding DK/refusal), where the weights are given by the <i>Salience</i> (see below) in each member state rescaled between 0 (highest fraction of DK/refusal across whole sample) and 1 (lowest fraction of DK/refusal across whole sample)	Eurobarometer survey series
<i>EU-wide mean opinion</i>	0-1, EU-wide support for policy change (as fraction, excluding DK/refusal) using EU-25/27 post-stratification weights	Eurobarometer survey series
<i>Opinion under "Council: unequal power" specification</i>	0-1, double-weighted mean of support for policy change across member states participating in the policy area (as fraction, excluding DK/refusal), where the weights are given by the <i>Salience</i> (see below) in each member state rescaled between 0 (highest fraction of DK/refusal across whole sample) and 1 (lowest fraction of DK/refusal across whole sample) as well as the number of votes of each member state in the Council rescaled between 0 (Malta) and 1 (Germany, France, Italy, UK), and both weights are added	Eurobarometer survey series
<i>Opinion under "Council-</i>	<i>For issues potentially decided by QMV (i.e., also those</i>	Eurobarometer survey series

<i>EP/Commission” specification</i>	<i>with unclear decision rule):</i> Midpoint between the opinion measure under “Council: equal power” specification and EU-wide mean opinion  <i>For issues decided under unanimity:</i> Opinion measure under “Council: equal power” specification	
<i>Salience</i>	1 minus fraction of DK/refusal responses as percentage of all responses using national post-stratification weights	Eurobarometer survey series
<i>Opinion majority size</i>	0-0.5, absolute value of support for policy change (as fraction, excluding DK/refusal) minus 0.5 using national post-stratification weights	Eurobarometer survey series
<i>EU competence level</i>	“1” = Mainly national competence “2” = Mixed competence “3” = Mainly EU competence	Author’s own coding following Börzel (2005) and Hix and Høyland (2011)
<i>Decision rule</i>	“1” = QMV “2” = Unclear decision rule “3” = Unanimity	Author’s own coding

### Data collection

Policy issues were selected by screening all “Standard,” “Special,” and “Flash” Eurobarometers with a fieldwork start date between 1<sup>st</sup> of May 2004 and 31<sup>st</sup> of December 2011 for the following key terms: “oppose,” “agree,” “approve,” “favour,” “for it or against it,” “do you think,” “in your opinion,” “do you believe,” “would you say,” “should,” and “would you like.” For each question item that contained these terms, coders ascertained whether it fulfilled the following four criteria (cp. Gilens 2012: 57-58):

- 1) *Opinion*: The item asked respondents for their personal opinion on a policy issue, or policy change, rather than whether some policy is “useful,” “important,” or “efficient.”

- 2) *No conditionality*: The item surveyed straight opinion on the policy without hypothetical or conditional twists (e.g., “given that X, would you oppose Y”).
- 3) *Specificity*: The item wording was specific enough for human coders to be able to ascertain adoption.
- 4) *Competence*: The adoption of the policy lay within the potential competences of the EU level, and given this competence, the EU had a realistic chance of unilaterally adopting the policy.<sup>1</sup> This excluded any questions on areas with exclusive national competences, but it included areas of weak and potentially growing competence (e.g., public health, nuclear energy).<sup>2</sup>

All question items that fulfilled these four criteria were included in the final dataset. A few issues (especially on EU enlargement) were surveyed several times in consecutive years. Each issue was included only once per calendar year, with the most recent survey being selected (cp. Gilens 2012: 58). Including issues more than once reflects the idea that policy issues that re-appear on the Eurobarometer may have higher importance (which seems to be confirmed by the prominence of EU enlargement). Different question wordings on the same issue were treated as separate policy issues. This resulted in a final dataset of 250 question administrations on policy change relating to 211 distinct policy issues.

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<sup>1</sup> The second criterion led to the exclusion of questions on EU enlargement to Switzerland and Iceland, as these countries have proven considerably unwilling to join the EU and unilateral implementation from the EU’s side is not realistic. In turn, Turkey (at least in the time period covered) and countries in the Balkans and Eastern Europe are generally assumed to be willing to join the EU, and the EU has a realistic chance of enabling their accession.

<sup>2</sup> As many policy areas are “shared competences” between the EU and the national level, the EU has a chance to adopt policies at any time, even though no EU legislation is in force so far. In addition, treaty change can provide the EU with more competences, but “candidate” items rarely related to issues of exclusive national competence.

46 out of the 211 policy issues (relating to 47 adoption opportunities) were included that mildly violated the inclusion criteria.<sup>3</sup> They were earmarked and in section 9) on robustness checks below I show that their exclusion does not influence the results of the analyses. Initially, 256 Eurobarometer questions on 215 distinct policy issues were identified. But the responsiveness and congruence analyses in the article and in the robustness check section below had to be performed on the slightly reduced dataset of 250 questions on 211 policy issues. For three questions adoption could not be ascertained due to a lack of cooperation by the DG Agriculture and Rural Development. For two questions the EU was theoretically incapable of adopting policy within the maximum adoption time lag (see below) due to the timing of the next negotiations of the multiannual financial framework, and for one question public opinion estimates were fully missing from the Eurobarometer data provided by GESIS. Table A2 displays the distribution of questions included in the dataset across 14 hand-coded policy areas.

As various question formats were used across the 211 policy issues, for each format it was necessary to decide which response options relate to support for policy change and which to endorsement of the status quo. For instance, “totally in favour” and “somewhat in favour” were merged as both representing support for policy change, while “totally disagree” and “tend to disagree” were joined as support for the status quo. Allocating response options was mostly straightforward except in the following notable cases:

- 1) *Response indicating indifference*: A very small number of questions offered a “neither ... nor ...” or alternative option indicating that respondents were indifferent between policy change and the status quo. In these cases, half of the

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<sup>3</sup> The data also includes three policy issues regarding the euro currency. For these cases, public opinion was only measured in the member states that had adopted the common currency. The general logics of joint decision-making at the EU level should nevertheless apply and the questions were therefore included in the data.

indifferent respondents are counted to be supportive of policy change and the other half in favor of the status quo.

- 2) *Bi-directional policy change*: Some questions relating to the EU budget offered two directions of policy change, i.e. an increase or decrease in the budget, as well as a “no change” or “maintain” option. For some of these items, the question text clearly suggested that respondents should view either an increase or a decrease as change. Hence, either increase or decrease was coded as support for policy change and the other response option as well as the “no change” option were coded as support for the status quo. If the question text provided no suggestion, an increase in the budget was assumed to represent change, and the middle option was split as under 1).

**TABLE A2: DISTRIBUTION OF QUESTIONS ACROSS POLICY AREAS**

	Questions	%	Cum. %
Constitutional affairs, agencies & enlargement	54	21.60	21.60
Internal market & consumer protection	48	19.20	40.80
Justice & home affairs	26	10.40	51.20
Economic & financial affairs	20	8.00	59.20
Environment, animals & energy	20	8.00	67.20
Health	20	8.00	75.20
Agriculture & rural development	11	4.40	79.60
Transport, infrastructure & public safety	10	4.00	83.60
Research, development & space	9	3.60	87.20
Trade & international development	9	3.60	90.80
Foreign policy, defense & neighborhood	8	3.20	94.00
Cohesion policy	7	2.80	96.80
Employment & social affairs	4	1.60	98.40
Other	4	1.60	100.00
<b>TOTAL</b>	<b>250</b>	<b>100</b>	

For all questions, support for policy change (as a percentage) was calculated on the basis of opinion-revealing responses, i.e. DKs and refusals were excluded. In contrast, salience was calculated as 1 minus the fraction of DK and refusal responses on the basis of all responses. Post-stratification weights provided by the Eurobarometer were used to obtain member-state and EU-wide estimates of opinion and salience. Note that all respondents from Bulgaria and Romania were excluded from the calculation of any opinion and salience estimates in case the survey fieldwork started before 1<sup>st</sup> of January 2007, the day the countries joined the EU. I also excluded all respondents from Denmark and related national estimates on justice and home affairs as well as on defense issues, since the country does not participate in these areas. However, I included estimates for Ireland and the UK as well as other countries with potential opt-outs, since all other opt-outs allow opt-ins, and hence, governments can decide to participate on a case-by-case basis.

Human coders ascertained the adoption record for each policy issue, wherever possible on the basis of publicly available information by the EU institutions, and where necessary by written requests to the Directorate-Generals (DGs) of the Commission. Coders assessed the adoption degree, i.e. to what extent the policy change had occurred (scale from 0-100), and the adoption date, i.e. when the change was agreed. Thereby, they also checked whether the proposed policy change had already been adopted *before* the survey's fieldwork, i.e. whether the question asked about support for the status quo rather than about policy change. This was the case for 30 issues (relating to 33 adoption opportunities). For these cases, the public opinion estimates were swapped so that they represented support for change and coders checked whether change occurred to the (recently established) status quo. In section 9) below I demonstrate that excluding these issues yields the same results. Coders also provided URL internet addresses to all information used as well as a short, written justification for their assessment of the adoption degree. The coding instructions used are in section 13) of this supporting information. One coder first collected the

adoption records, before a second coder performed an independent search. A small number of disagreements between the coders were settled in discussion.

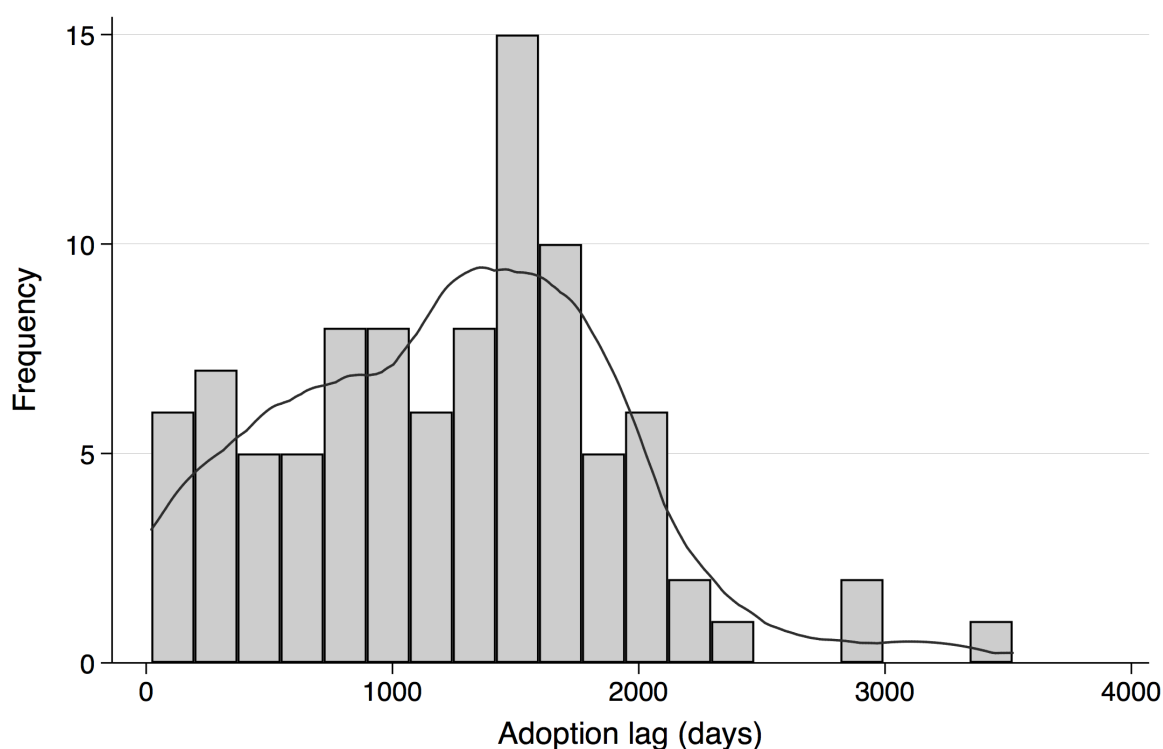
While coders were generally advised to ascertain the adoption degree as the “percentage of the proposed change that was implemented,” more specific guidelines were provided for two types of issues:

- 1) *EU enlargement*: As question items on enlargement were asked three times within the period of Eurobarometers covered (seven and a half years), coders were instructed to consider a six-year (instead of indefinite) coding window from the start date of survey fieldwork. They ascertained the number of stages (16 in total, from negotiations over an EU Association Agreement to the joining date) countries working towards EU accession had completed within this period. The adoption degree was defined as the percentage (rounded to the nearest 5) of completed stages out of the total number of remaining stages at the start date of survey fieldwork.
- 2) *Differentiated integration*: In some areas (justice and home affairs, open coordination, soft law) EU-level policies are agreed but not applied or enforced in all geographies (see e.g., Holzinger and Schimmelfennig 2012). In these cases of differentiated integration, coders weighted the adoption degree by the number of member states that the change applied to, e.g. if the adoption was complete but only applied or transferred into necessary national law in 24 out of 27 member states, the adoption degree was 90 (rounded to the nearest 5).

For the adoption date coders were instructed to determine the “date on which the policy change is passed.” In the case of secondary law, this is the date all relevant EU institutions (e.g., EP, the Council) arrived at a political agreement on the relevant legislative act (as reported in EUR-Lex). In the case of primary law, it is the day the last national parliament or government ratified the relevant international treaty.

The histogram in Figure A1 shows the distribution of the adoption lag (adoption date minus start date of survey fieldwork) across the data. This illustrates that questions on policy change peak during the phase of policy preparation (i.e. 1,000-2,000 days before a political agreement is reached), but are also frequent in the phase of policy adoption (i.e. less than 500 days before an agreement is reached). This distributional shape is in line with the idea that DGs use the Eurobarometer to survey opinion at critical moments during the policy process (see also Hartlapp, Metz, and Rauh 2014: 234-235). On the one hand, during policy preparation data on public opinion may be used by some DGs as a “bargaining chip” when negotiating with other DGs about the Commission’s proposal. On the other hand, just before policy adoption – in the “end game” of negotiations – the Commission may use opinion data when defending policy positions vis-à-vis the Council or the EP.

**FIGURE A1: HISTOGRAM OF ADOPTION LAG**



*Notes:* Adoptions with adoption degree  $\geq 80\%$ ; Kernel density estimate (Epanechnikov, bandwidth minimizing mean integrated squared error) as black line; Based on cases with adoption degree greater than 0.

Compared to studies of policy representation in the U.S.(e.g., Gilens 2005, 2012), in which about 90% of policies that are adopted have a time lag between the survey's fieldwork date and the adoption date of less than two years, the median adoption lag in the EU data is three and a half years (1,271 days or 1,191 days – when excluding outliers, see below). On the one hand, this may be evidence of the complex and lengthy policy process at the EU level. On the other hand, it may also be due to the fact that Eurobarometer questions are posed for the purpose of policy preparation (see above), whereas U.S. survey companies presumably pose questions in times of public attention, e.g. shortly before votes in Congress. To the extent that this conjecture is accurate, the opinion data used here is arguably more exogenous to the adoption of the policy than typical U.S. opinion data, since it is often recorded years ahead of adoption.

However, the long adoption lags also pose problems, since opinion on policy issues may change over time and it seems problematic, at the very least, to speak of “responsiveness,” if citizens' preferences are adopted after eight or 10 years. For this reason, I decided to correct the adoption degree to 0 for any adopted change that happened after more than six years (or 2190 days). This cut-off only excludes outliers, making up about 5% of observations. In the section on robustness checks below, I also show that results are substantively the same, with some variations in statistical significance, when using a cut-off of five and a half or five years.

The binary adoption variable used in the article was constructed on the basis of the adoption degree and is “1” if the adoption degree was 80% or more, and “0” otherwise. This threshold of 80% adoption is in line with established practice in the field (see e.g., Gilens 2005: 782). Importantly, apart from policy issues on EU enlargement only seven issues were assessed with an adoption degree greater than 0 but smaller than 80%. In the section on robustness checks below I demonstrate that setting the threshold for adoption to 50 instead of 80% does not affect any results.

In the article, I discuss the potential problem that the Commission may try to instill biases in the Eurobarometer questionnaires in order to receive “desired an-

swers.” I argue that even if such biases exist, relative figures of support across member states are still a valid proxy of differences in opinion across member states, as it is hard to influence these figures through question design and as it is unclear whether the Commission has any incentive to do so. While I acknowledge that it might be possible to construct questions in such a way as to artificially obtain higher figures of support by – say – French or Italian respondents, for instance, by presenting the policy in a certain frame, this may at the same time have unintended consequences, lowering the support of the Germans or the Dutch. Control is limited. Moreover, while occasionally it may be strategically beneficial for the Commission to showcase high support in certain member states whose governments are blocking legislation, there is no reason to assume that this bias is systematic across policies. Lastly, one should consider that many U.S. studies rely on questions from survey companies that are conducted for particular media outlets with a particular political slant. Absence of bias is unlikely in such questions.

#### EU competence level

The competence level of the EU with regard to adopting policy change was coded on the basis of assessments in Börzel (2005) and Hix and Høyland (2011). Constitutional issues which can only be agreed on by international treaties between the member states were coded as “mainly national competence.”

#### Decision rule

The decision rule in the Council of the European Union that has to be used to agree policy change was assessed on the basis of the Lisbon Treaty rules. In some cases, the legal basis on which the change would be agreed was not clear. The decision rule for such issues was recorded as “Unclear decision rule.” For example, if questions asked whether the EU should spend more money on an issue (e.g., financial assistance to member states to deal with irregular migration), it is often unclear

whether this would be politically feasible by adjusting program allocations within the annual budgets, which would only require QMV, or whether the ceilings of the multiannual financial framework would have to be raised, which would require unanimity.

In contrast to theoretical expectations, the results in the responsiveness analyses indicate that unanimity as decision rule makes policy change more likely instead of less likely. I suspect that this result could be due to some pre-selection by the DGs, which perhaps only put policy questions with unanimity as decision rule on surveys when they expect strong support across member states and a real chance of adoption, while they may be less selective on QMV issues.

### 3) *Measurement of salience*

I use “don’t know” (DK) and refusal responses to measure cross-national differences in the salience of policy issues for the national public. While this methodological choice has important precedents in the extant literature on policy representation (e.g., Brooks 1990; Gilens 2005, 2012; Page and Shapiro 1983), it is not uncontroversial. Below I discuss the central arguments in the debate about DK responses and assess to what extent they affect my empirical strategy. I also consider alternative measures of salience with a view towards their strengths and weaknesses in my specific application.

#### *a) “Don’t know” indicating ambivalence*

Some work has found that respondents that feel ambivalent about an issue may be more likely to select non-opinion responses like “don’t know” (e.g., Bishop 1987; Coombs and Coombs 1976; Klopfer and Madden 1980; Presser and Schuman 1980; Turgeon 2009). Such respondents may actually care about an issue but have a mixed opinion on it that makes it hard for them to decide between polarized answer options. However, some of the findings on hidden ambivalence in DK responses (e.g., Coombs and Coombs 1976; Klopfer and Madden 1980) relate to attitude scales on issues like capital punishment, Sunday observance, and abortion that are relatively distinct from questions on policy change from the Eurobarometer. Hence, I focus on the findings by Presser and Schuman (1980) as well as Bishop (1987) who use questions that are very similar to items on policy change from the Eurobarometer.

What fraction of DK responses is likely due to ambivalence according to these studies? Both studies use split-ballot survey experiments that either force respondents into a polarized choice or offer them a middle category to express attitudinal ambivalence. While Presser and Schuman (1980) find that DK responses are slightly lower when offering a middle category, in nine out of 10 experiments the difference in DK responses is statistically insignificant, and on average more than 80% of DK

responses do not disappear when offering a response indicating ambivalence (calculations by the author based on Presser and Schuman 1980: 75-76). Bishop's (1987) study provides similar results. When focusing on the designs resembling Eurobarometer questions (i.e., DK not introduced in the preface of the question and offered in last position as answer option) his experiments with a reliable sample size ( $n > 400$  in each condition) indicate that 70% of DK responses remain, on average, if an ambivalent middle category is added to the question (calculations by the author based on Bishop 1987: 224-225).

Moreover, it is not clear whether all switchers between DK and the middle category are really feeling ambivalent. Sturgis, Roberts, and Smith (2014) have recently reported results from questionnaires querying respondents in the middle category why they chose this category. It turns out that in the design with a DK option, still about 70 to 75% of the respondents in the middle category report that they “don’t really have an opinion on this issue” rather than “an opinion which is right in the middle on this issue” (calculation by the author based on Sturgis et al. 2014: 24, 31). This suggests that a fraction of the switches from DK to a middle category may not actually indicate ambivalence but rather reflect respondents’ use of the middle category as a convenient, “face-saving don’t know.” Assuming that switchers from DK to the middle category are as likely as other middle-category respondents to select the middle category as a “face-saving DK,” a “back-off-the-envelope” calculation<sup>4</sup> suggests that typically less than 10% of all DK responses on policy questions should reflect ambivalence. While this is only a rough calculation, it demonstrates that ambivalence is a significant but likely not the main meaning of DK responses.

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<sup>4</sup> This calculates as follows. Up to 30% of the DK respondents switch to the middle category if offered (Bishop 1987). But at most 30% of the middle-category respondents choose the middle category because they “have an opinion which is right in the middle on this issue.” Hence, at most about 9% ( $0.3 * 0.3$ ) of all DK respondents are feeling ambivalence.

Last but not least, respondents that choose DK due to ambivalence may not differ much from other DK respondents. Faulkenberry and Mason (1978) task interviewers with distinguishing respondents into those that “don’t know” and those that are merely ambivalent. While they observe slight differences between the two groups (in particular, the ambivalent respondents are a bit more knowledgeable), they also show that both groups of respondents are much more similar to each other than to respondents that have either a favorable or an opposing view on the policy issue. In sum, this review of the extant literature suggests that the vast majority of DK responses, likely over 90%, should not indicate attitudinal ambivalence, and ambivalent DK respondents resemble other DK respondents more than respondents with manifest opinion.

To what extent may DK responses indicating ambivalence bias the empirical findings? First, while at the *individual* level ambivalence is clearly different from salience, theories of policy representation operate at the *aggregate* level and conceptualize aggregate public opinion as a signal to a national government that cares about re-election. From the perspective of the government, it makes little difference whether citizens are ambivalent about a policy issue or whether they do not care about it. Clearly, if they do not care, the issue should not influence their vote choice, and hence governments are neither punished nor rewarded for their behavior on the issue. In contrast, if citizens are ambivalent, some of them may feel so “in-between” the sides that they abstain from electoral sanction, no matter how the government behaves, others may eventually decide to punish, and yet others to reward. Ambivalence typically delays the formation of voting intentions and weakens the predictability of vote choices (Lavine 2001). Hence, in aggregate, ambivalent voters likely send no clear signal to the government of how it can maximize its vote by either pushing for or against policy change. More formally, ambivalent voters pull the government’s utility from policy change towards zero, precisely as low salience does. Hence, from a theoretical point of view, ambivalence and salience have the same impact on the

government's utility function (see equation (1) in section 1) of this supporting information).

Second, from an empirical perspective, the central question is whether the fraction of DK responses that is due to ambivalence can be treated as a nuisance. While it is possible that the level of ambivalent DK responses may vary by question, the statistical models in the article actually never compare absolute levels of salience across questions. Instead, my main analyses only compare relative levels of salience *within* a question. In the responsiveness analysis, I only use state-level salience as a within-question weight to calculate an EU aggregate measure of salience. In the congruence analysis, fixed effects for questions results in a within-transformation comparing the effect of deviations in salience from the question mean. Hence, any systematic variation in the extent of ambivalent DK responses between questions will not affect the results. Moreover, there is no obvious theoretical argument why the extent of ambivalent DK responses should vary within questions across member states in ways that are related to the "true" fraction of DK responses or public support for policy change.

In sum, there is no reason to assume that my results are significantly biased by DK responses that reflect attitudinal ambivalence.

*b) "Don't know" indicating lack of knowledge*

There is some debate whether DK responses may reflect a lack of knowledge about the issue in question. In such a case, the respondent may genuinely care, or at least have a feeling that the issue could be important to her, but due to a lack of knowledge about it, may select DK. Several studies find that respondents with less generic (Rapoport 1981, 1982) as well as less issue-specific (Faulkenberry and Mason 1978) political knowledge more often respond DK to questions on political attitudes. Moreover, some work shows that most citizens respond DK to attitude items on fictitious political issues, at least if the DK response is not probed by the interviewer

(Bishop, Tuchfarber, and Oldendick 1986). This indicates that respondents view DK as a knowledge-related response category under some circumstances. Relatedly, in the literature on the measurement of political knowledge, scholars disagree to what extent DK responses on factual knowledge questions indicate an actual lack of knowledge or can still conceal “hidden knowledge” (e.g., Jessee 2017; Luskin and Bullock 2011; Mondak 2000, 2001; Mondak and Davis 2001; Sturgis, Allum, and Smith 2008). However, knowledge items are quite different from the opinion items used in the dataset.

An inherent problem in assessing what fraction of DK responses is due to a lack of knowledge versus a lack of salience is that both concepts are arguably closely related empirically: Individuals that do not care about an issue may not acquire knowledge on it, and those that have no knowledge on the issue are disincentivized to care about it. In this regard, it is important to recall the definition of salience as an issue’s electoral importance to the public. A long-established literature on issue voting shows that political information and knowledge is a key prerequisite for issues to become electorally important (e.g., Alvarez 1998; Basinger and Lavine 2005; Delli-Carpini and Keeter 1996; Goren 1997; de Vries, van der Brug, van Egmond, and van der Eijk 2011; Zaller 1992). If voters know little about a policy issue, they will not punish or reward the government for its behavior on it. Hence, aggregate knowledge of an issue may be viewed as a key determinant if not component of salience. To the extent that DK responses reflect a lack of knowledge they should then also reflect lower salience on average.

Crucially again, the distinction between knowledge and salience on the level of the individual voter is much less significant from the aggregate-level perspective the government takes. For the government, it makes no difference whether an issue is not electorally salient because voters do not know about it or because they more genuinely do not care. The essence is that the electoral consequences from governments’ behavior on such issues will be muted, and this is captured in a reduction of

the  $S_{i,j}$  term in the government's utility function (see equation (1) in section 1) of this supporting information).

Even if there is a reason why governments should distinguish between variations in salience due to knowledge of an issue and variations in salience that have a more “genuine” quality, my empirical analyses actually control for variations in knowledge across questions, as they do for those in ambivalence (see above). This is due to the fact that I only weight opinion by DK responses within questions in the responsiveness analyses, and use question fixed effects in the main congruence specification. In addition, I do not see any straightforward argument why we should assume that the fraction of DK responses that is due to a lack of knowledge varies across member states in ways that would seriously bias the estimates of salience or opinion on policy change.

In sum, salience may, at least to some extent, reflect knowledge but there are neither important theoretical nor empirical consequences flowing from this for my application.

*c) “Most important problem” as an alternative measure of salience*

An alternative to using DK responses as a measure of salience is using responses to the “most important problem” (MIP) question, which queries respondents to indicate the “most important problem” facing the nation. There are numerous variants of this question, including one asking for the “most important issue” instead of “problem”. MIP has been widely used in the policy representation literature, especially in studies focusing on policy agenda responsiveness and issue attention (e.g., Alexandrova, Rasmussen, and Toshkov 2016; Hobolt and Klemmensen 2008; Jennings and John 2009; Jones and Baumgartner 2005). However, it has actually mainly been used as an unmoderated, primary independent variable that is supposed to measure the public's “issue priorities” with little clarity of whether this relates to electoral salience or broader priorities.

In fact, to my knowledge, only little work has used MIP as a moderator in models that interact preferences for policy change with salience. Moreover, much work I am aware of could not detect any substantively important impact of MIP on the effect of public preferences for policy change, especially when focusing on temporal variation in MIP (Soroka and Wlezien 2010; Wlezien 2004). The most influential argument is that these null findings are due to the fact that MIP actually does *not* measure the importance or electoral salience of issues for the public but rather the degree to which the public perceives something to be a “problem” (i.e., “problem load” or “problem status”) (Jennings and Wlezien 2011; Johns 2010; Wlezien 2005). This view on MIP is even held by some (previous) users of MIP. Crucial is Johns' (2010) study that demonstrates that MIP responses are neither related to respondents' knowledge about issues nor do they explain the weight of issue positions in respondents' vote calculus (see also Bartle and Laycock 2012). Hence, MIP “is not an accurate gauge of salience effects in models of vote choice” (Johns 2010: 143). This is not to say that MIP has no adequate uses (e.g., when studying political attention), but that it does not qualify for my purpose as a measure of salience for voters at the ballot box.

Moreover, there is a very practical problem with applying MIP in my design. Since the data contains opinion on policy change for very specific and concrete issues, the MIP response categories are poor matches, since they are simply very coarse. For instance, the data includes opinion and DK rates for policy issues such as special data protection rules for minors or the banning of tobacco vending machines. The standard MIP categories in the Eurobarometer do not even offer something like “consumer protection” in the first place. My data simply is collected on a much more fine-grained level than MIP, and many questions could not be matched at all or only poorly. I therefore have little confidence in the validity of any estimates from an MIP-based measure of salience. In contrast, DK responses are obtained on the level of each policy issue and they show good variation across questions and countries (see section 4) in this supporting information).

#### 4) Variation in opinion and salience

A central prerequisite for territorial representation to make a difference is that opinion and salience significantly vary across member states. The amount of this variation is demonstrated in Table A3 that displays the variation in question-demeaned opinion and salience across EU member states by policy area in terms of percentages. This is obtained by subtracting average opinion / salience across states from specific opinion / salience in each member state (e.g., for opinion,  $O_{i,j}^{Demeaned} = O_{i,j} - \bar{O}_j$ ). Hence, the variable represents positive or negative deviations of state-specific opinion / salience from average opinion / salience. Importantly, the standard deviations in question-demeaned opinion and salience of about 4-14 percentage points depending on the policy area show that both measures vary significantly across EU member states, even on the same policy issue.

**TABLE A3: VARIATION IN QUESTION-DEMEANED OPINION AND SALIENCE BY POLICY AREA**

	Opinion			Salience			N
	S.D.	Min	Max	S.D.	Min	Max	
Agriculture & rural development	9.48	-25.78	27.92	5.67	-28.08	12.07	297
Cohesion policy	6.33	-22.10	15.31	5.77	-27.10	19.18	187
Constitutional affairs, agencies & enlargement	14.00	-44.13	42.01	7.31	-24.69	19.63	1388
Economic & financial affairs	8.14	-34.23	21.39	7.90	-33.67	18.57	496
Employment & social affairs	10.49	-29.19	35.40	3.99	-17.12	6.00	108
Environment, animals & energy	8.48	-29.23	31.82	5.01	-21.31	12.57	522
Foreign policy, defense & neighborhood	8.27	-25.90	36.98	6.00	-16.62	13.00	202
Health	8.44	-28.64	27.86	4.82	-26.03	23.16	516
Internal market & consumer protection	8.93	-30.51	30.73	6.52	-35.74	24.96	1282

Justice & home affairs	7.64	-33.33	22.40	4.60	-19.54	13.30	668
Other	14.41	-33.89	36.61	7.01	-26.74	14.48	102
Research, development & space	8.40	-21.59	27.98	7.62	-27.41	19.69	235
Trade & international development	7.00	-25.97	19.66	5.17	-17.09	10.72	235
Transport, infrastructure & public safety	7.43	-40.78	20.17	3.71	-17.70	10.42	268
TOTAL	9.95	-44.13	42.01	6.22	-35.74	24.96	6506

In addition, Table A4 below depicts summary statistics of question-demeaned opinion and salience by member state instead of policy area (as in Table A3).

**TABLE A4: VARIATION IN QUESTION-DEMEANED OPINION AND SALIENCE  
WITHIN MEMBER STATES**

	Opinion				Salience			
	Mean	S.D.	Max	Min	Mean	S.D.	Max	Min
AT	-5.87	13.22	-41.20	31.79	2.25	3.53	-9.62	13.44
BE	-1.49	6.87	-18.79	15.00	5.69	4.79	-19.27	19.69
BU	3.93	10.74	-25.97	35.88	-8.09	6.70	-29.54	2.18
CY	4.22	10.39	-26.34	35.33	1.24	3.74	-14.87	17.43
CZ	-2.84	8.89	-40.04	26.79	2.26	3.82	-15.68	11.31
DE	-3.47	10.73	-31.09	23.64	3.40	3.42	-5.18	16.48
DK	-4.38	11.50	-44.13	36.98	3.67	3.34	-4.83	17.22
EE	0.02	7.54	-21.72	18.79	-3.59	4.96	-24.63	6.42
EL	2.28	8.90	-22.92	30.92	5.80	4.98	-17.29	23.16
ES	3.16	8.72	-25.22	27.28	-4.10	6.25	-21.14	9.21
FI	-3.75	9.78	-38.86	18.01	4.62	3.23	-2.70	14.65
FR	-0.68	7.74	-27.24	23.98	2.04	2.98	-10.76	14.52
HU	1.76	7.83	-40.78	26.98	0.22	5.21	-28.32	17.17
IE	2.61	8.15	-19.10	27.86	-5.16	7.93	-26.03	24.96
IT	0.63	9.87	-21.09	30.73	-0.11	3.82	-9.96	20.15
LT	2.08	9.51	-30.76	29.55	-6.05	5.83	-35.74	1.97

LU	-3.15	9.67	-31.85	19.66	2.64	3.97	-9.16	18.32
LV	-0.28	8.12	-28.88	23.61	-2.44	4.90	-22.70	10.34
MT	3.90	10.53	-34.23	36.61	-6.25	7.43	-33.67	11.50
NL	-4.76	10.30	-28.84	31.82	4.07	3.39	-4.27	16.43
PL	2.89	9.25	-18.78	29.68	-2.62	3.89	-26.41	6.56
PT	1.98	7.37	-19.26	24.14	-4.62	5.25	-22.53	13.65
RO	4.08	10.64	-21.61	42.01	-7.85	5.02	-25.44	6.51
SE	-1.32	13.21	-33.60	28.78	2.89	3.91	-12.94	15.80
SI	2.20	8.62	-27.39	23.51	3.80	3.43	-10.14	13.75
SK	0.99	7.99	-32.22	23.55	1.76	4.36	-21.88	12.67
UK	-2.30	8.23	-26.44	20.41	-0.95	4.18	-13.93	18.72
TOTAL	0.00	9.95	-44.13	42.01	0.00	6.22	-35.74	24.96

*Notes:* AT: Austria; BE: Belgium; CY: Cyprus; CZ: The Czech Republic; DK: Denmark; EE: Estonia; FI: Finland; FR: France; DE: Germany; EL: Greece; HU: Hungary; IE: Ireland; IT: Italy; LV: Latvia; LT: Lithuania; LU: Luxembourg; MT: Malta; NL: The Netherlands; PL: Poland; PT: Portugal; SI: Slovenia; SK: Slovakia; ES: Spain; SE: Sweden; UK: The United Kingdom.

It demonstrates that the variation in opinion and salience cannot be traced back to stark differences between member states, e.g. it is not the case that citizens in some member states favor policy change much more across issues or find EU policy-making much more salient than others (see column “Mean”). The average differences in opinion and salience across member states are rather small, i.e. within 10 to 15 percentage points for both measures, and for each member state we have policy issues with strong deviations above and below the average across member states (columns “Max” and “Min”). In addition, the average differences across countries do not seem to follow any obvious structure. In particular, EU policy-making is not consistently more salient in the old than in the new member states or in the big than in the small. While it is true that Bulgarians and Romanians regard EU policies as least salient, citizens in other new and small member states such as Slovenia or the Czech Republic care more about EU policies than the Irish and as much as the French or Germans.

These differences are in line with a large body of literature that investigates and highlights cross-national differences in opinion and salience, especially with regard

to welfare state issues (e.g., Blekesaune 2003, 2007; Finseraas 2009; Jæger 2009; Rasmussen, Mäder, and Reher 2018; Shapiro and Young 1989; Singer 2011).

### 5) *Do governments represent public opinion in Brussels?*

A central assumption behind territorial representation is that territorial representatives, in this case national governments, represent differences in opinion and salience when negotiating in the Council of the EU or in intergovernmental conferences. In particular, governments' initial positions – before log-rolls and vote-trades take place – should reflect opinion differences. The study by Wratil (2018) is particularly important for this assumption, since it shows that governments' initial policy positions in the Council, taken at the time when a legislative proposal is introduced, are strongly shaped by public opinion. He finds that on issues that are connected to left-right conflicts, governments are generally responsive to domestic mean opinion, when negotiating in Brussels. Moreover, their responsiveness is heightened, when electoral pressures are strong (e.g., when a national election approaches). Schneider (2018), in addition, shows that governments which face high electoral pressures at home are also more likely to defend their initial positions during the negotiation process. While this might suggest that governments under more electoral pressure may represent public opinion more precisely and vigorously, we also know that such governments are often able to delay EU decision-making until after electoral pressure has faded (Kleine and Minaudier 2017). This may equalize the degree to which governments' positions at the time when deals are actually struck and decisions are taken are representative of national public preferences.

The assumption that governments broadly represent public opinion is also supported by work that shows that governments react to public Euroscepticism at home when voting on EU legislative acts (Hagemann, Hobolt, and Wratil 2017). We also know that the amount of policy output at the EU level is related to EU-wide public Euroscepticism (Arnold and de Vries 2009; Bølstad 2015; Toshkov 2011; de Vries and Arnold 2011), which has been viewed as an indication that policy-makers in various EU institutions represent citizens' preferences. In sum, I therefore contend that the extant literature provides ample support for the idea that governments broadly represent public opinion in Brussels. Importantly, public opinion may only be one of

multiple sources of governments' positions, and hence shape policy-making to some degree. I am not arguing that opinion determines governments' positions and EU-level policies, but that the influence of opinion on EU-level policies yields different results depending on whether the system reacts to EU-wide opinion or to opinion in those member states where citizens view the issue as salient (see sections "Does Territorial Representation Make A Difference?" in the article and in this supporting information).

In addition to this review of the literature, the dataset can – at least to some extent – also be used to investigate whether governments represent domestic public opinion when they take negotiation positions in Brussels. Regrettably, I have no information about governments' negotiation positions on the policy issues in the dataset, and therefore do not know whether they were representative of variations in domestic public opinion. However, what I can do is compare the structure of the opinion differences in the sample of policy issues with the structure of governments' negotiation positions in the Council that previous work has discovered for other samples of policy issues. At least, this reveals whether the broad structure in public opinion in the data resembles prominent structures in governments' negotiation positions.

For this purpose, I first use exploratory factor analysis to uncover the latent patterns of opinion differences across member states. Twenty-five manifest variables measure the question-demeaned opinions in each EU member state.<sup>5</sup> Using the Kaiser-Guttman criterion of an eigenvalue greater than 1, I retain five factors with the principal factor method on 219 observations of questions on policy change.<sup>6</sup> The ei-

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<sup>5</sup> As Romania and Bulgaria joined the EU in January 2007 and are therefore not included in about 40% of the data, I decided to exclude them here.

<sup>6</sup> Three questions concerned the common currency and non-euro member states were not surveyed on these questions. Moreover, 28 policy issues on justice and home affairs as well as defense had to be dropped in order to keep Denmark that does not participate in these policy areas in the estimation

genvalues and factor loadings are reported in Table A5. The five factors explain about 88% of the variation and the first two factors already sum to 62%.

**TABLE A5: FACTOR ANALYSIS OF STATE-LEVEL DIFFERENCES IN OPINION**

	Factor 1	Factor 2	Factor 3	Factor 4	Factor 5	Uniqueness
	Eigenvalue	Eigenvalue	Eigenvalue	Eigenvalue	Eigenvalue	
	6.25	3.86	1.89	1.47	1.15	
AT	<b>0.768</b>	0.120	-0.052	0.315	0.136	0.275
BE	<b>0.559</b>	0.263	-0.137	0.100	-0.380	0.445
CY	-0.448	<b>0.450</b>	0.132	0.195	-0.053	0.539
CZ	-0.111	-0.140	<b>-0.692</b>	-0.091	0.374	0.340
DE	<b>0.830</b>	0.019	-0.089	0.308	-0.011	0.209
DK	0.446	<b>-0.555</b>	-0.051	-0.416	0.082	0.310
EE	<b>-0.533</b>	<b>-0.484</b>	0.268	0.307	0.158	0.291
EL	-0.164	<b>0.531</b>	0.191	0.135	0.199	0.596
ES	-0.420	<b>0.535</b>	-0.101	-0.324	-0.177	0.391
FI	<b>0.539</b>	<b>-0.451</b>	0.264	0.057	0.297	0.345
FR	<b>0.762</b>	0.220	-0.074	0.098	-0.145	0.335
HU	-0.365	0.020	<b>-0.527</b>	0.105	0.037	0.577
IE	0.045	<b>0.501</b>	<b>0.462</b>	-0.135	0.204	0.473
IT	0.281	<b>0.720</b>	-0.001	-0.048	0.040	0.398
LT	<b>-0.655</b>	-0.283	-0.027	0.267	-0.156	0.394
LU	<b>0.783</b>	0.072	0.031	0.200	-0.283	0.262
LV	-0.275	-0.311	0.363	<b>0.475</b>	0.105	0.459
MT	<b>-0.632</b>	0.055	<b>0.457</b>	-0.194	-0.155	0.328
NL	<b>0.462</b>	<b>-0.497</b>	-0.141	-0.285	-0.316	0.339
PL	<b>-0.834</b>	-0.044	-0.066	-0.123	-0.095	0.274
PT	-0.174	<b>0.701</b>	0.039	-0.264	-0.025	0.407
SE	-0.027	<b>-0.739</b>	0.199	-0.304	-0.145	0.301
SI	-0.449	-0.282	-0.027	0.114	-0.284	0.625
SK	<b>-0.506</b>	0.030	<b>-0.521</b>	0.077	0.241	0.408
UK	<b>0.487</b>	-0.040	0.250	-0.400	0.383	0.392

Notes: Principal factor method; five retained factors (eigenvalue > 1); factor loadings greater than  $\pm 0.45$  in bold;

AT: Austria; BE: Belgium; CY: Cyprus; CZ: The Czech Republic; DK: Denmark; EE: Estonia; FI: Finland; FR:

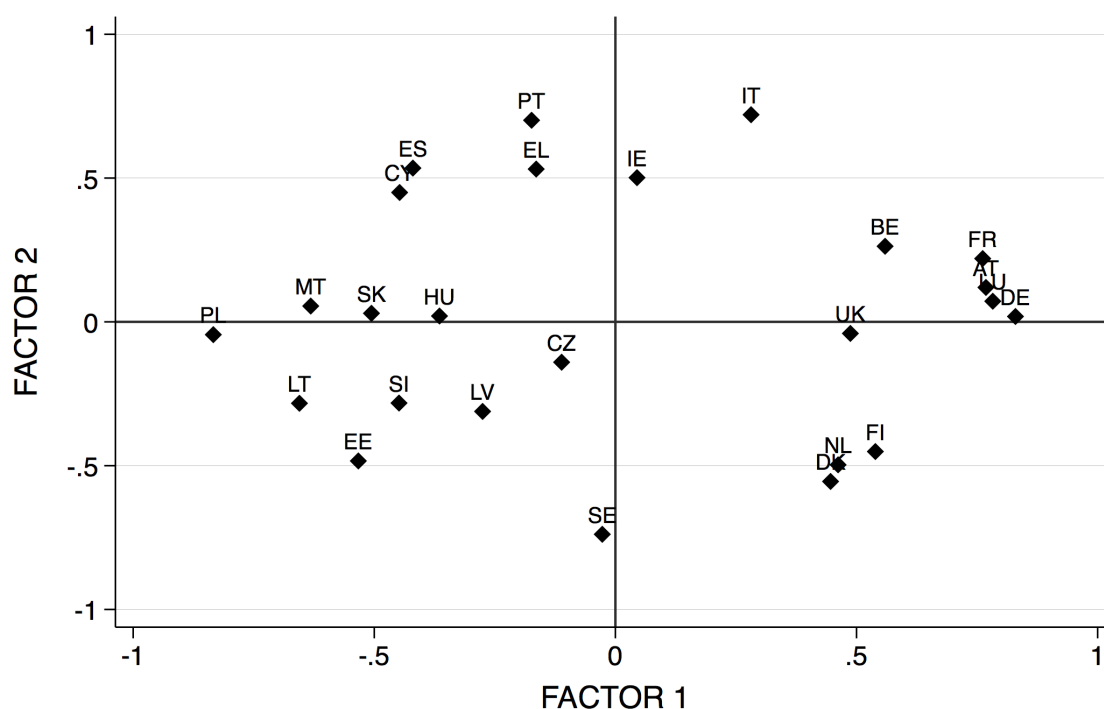
France; DE: Germany; EL: Greece; HU: Hungary; IE: Ireland; IT: Italy; LV: Latvia; LT: Lithuania; LU: Luxembourg; MT: Malta; NL: The Netherlands; PL: Poland; PT: Portugal; SI: Slovenia; SK: Slovakia; ES: Spain; SE: Sweden; UK: The United Kingdom.

sample. However, results do not yield a different space if Denmark is excluded. Due to these choices, the number of observations is 219 instead of 250 (the number of questions) as in all other analyses.

While a screeplot does not show any clear “elbow,” the eigenvalue of the third factor is less than half of the eigenvalue of the second factor. I therefore focus here on the interpretation of the first and the second factor.

Figure A2 plots the factor loadings for factors one and two. This shows that factor one captures a contrast between “core” Western European member states (Germany, France, Belgium, Austria, and Luxembourg) and the new member states (in particular, Poland, Lithuania, and Malta). In turn, the second factor relates to a contrast between northern member states (Sweden, Denmark, Netherlands, Finland, and Estonia) and southern member states (Portugal, Italy, Spain, and Greece, but also Ireland).

**FIGURE A2: THE EU’S PUBLIC OPINION SPACE**



*Notes:* Factor loadings for factors one and two. AT: Austria; BE: Belgium; CY: Cyprus; CZ: The Czech Republic; DK: Denmark; EE: Estonia; FI: Finland; FR: France; DE: Germany; EL: Greece; HU: Hungary; IE: Ireland; IT: Italy; LV: Latvia; LT: Lithuania; LU: Luxembourg; MT: Malta; NL: The Netherlands; PL: Poland; PT: Portugal; SI: Slovenia; SK: Slovakia; ES: Spain; SE: Sweden; UK: The United Kingdom.

Strikingly, this structure of old versus new and north versus south in opinions is *exactly* the same structure studies have found to be present in initial negotiation positions of governments in the Council of the EU (Plechanovova 2011; Thomson 2009, 2011b). This suggests that state representatives do indeed represent broad opinion patterns in EU policy-making.

Investigating the factor scores of the question items, it turns out that factor one relates to the difference between prioritizing environmental protection and phasing out the common agriculture policy (CAP) versus support for enlargement of the union. While the publics in the old member states are relatively more supportive of ambitious changes in environmental protection as well as cutting the CAP's budget, the citizens in the new member states prioritize the inclusion of more countries (without a specific focus). In turn, the second factor pits southern publics that prioritize support for federalist competence extension (especially in foreign affairs and symbolic politics) and some integration in financial affairs (Eurobonds, consumer rights in financial services) against northern publics that favor differentiated enlargement (esp. Balkans, Ukraine) and cuts to the CAP.

These substantive differences in opinion are in line with the substantive differences found in studies of governments' initial policy positions in the Council (Thomson 2009, 2011a). For instance, these studies also find that old and new member states differ starkly in their preferences regarding the CAP and harmonization of policies. Similarly, they also find that the north-south divide relates to questions of regulation versus market-based solutions, which is in line with the finding that publics in the south are more in favor of regulatory projects in financial affairs (such as financial transaction tax or consumer rights in financial services). Taking into account that these studies could not cover policy positions on enlargement or federalist competence extension, which are covered in the data used here, the overlap in substantive meaning of the two divides (new versus old, north versus south) is quite striking.

## 6) *Assumptions about EU decision-making*

In the article, for the baseline specification of territorial representation (“Council: equal power”) we must make assumptions about the EU decision-making process. First, I assume that, on average across issues, bargaining power of national governments is largely uniform (even though it may vary considerably by issue due to log-rolling and vote-trading). I also assume that the EP and the Commission do not influence decision-making to a degree that could pull representation towards EU-wide mean opinion. While I also test specifications of territorial representation that are based on alternative assumptions about EU decision-making, I discuss the plausibility of the two baseline assumptions in depth below.

### *a) Uniform bargaining power of member states*

It is a common public perception that larger member states have more power in the EU. In academic theorizing, this view is not only supported by the most influential theory of European integration, namely “liberal intergovernmentalism” (Moravcsik 1993, 1998), which highlights the economic power resources of the three biggest member states, but also by institutionalist accounts (e.g., Thomson, Stokman, Achen, and König 2006) that incorporate the distribution of votes in the Council, which to some extent reflects population size. Ex ante, a higher number of votes should render large member states more likely to be pivotal players in whether policy change occurs or not, at least under QMV. However, even from a theoretical point of view, bargaining power is not only determined by votes but also by endogenous factors such as the positions of other member states in the bargaining space (e.g., Bailer 2004; Schneider, Finke, and Bailer 2010).

A large body of empirical literature has relied on measures of member states’ positions and salience with regard to different bargaining issues in the Council and conceptualized power as the (salience-weighted) distance between the position of a member state and the final negotiation outcome. Stunningly, virtually no work finds

that, on average, large member states are more powerful in EU policy-making than small ones. While some work finds little to no significant differences between member states (Arregui and Thomson 2009; Bailer 2004; Thomson 2008, 2011d), other studies even point towards a surprising power advantage of smaller states (Cross 2013; Golub 2012b). In some studies, unanimity as decision rule renders power more equal than QMV, as theoretically expected. However, usually it just brings the bigger states, which are otherwise disadvantaged, on a level playing field with the smaller states (e.g., Arregui and Thomson 2009; Golub 2012b).

A power advantage of large member states is also inconsistent with the common finding that larger member states cast more opposition votes against legislative proposals in the Council (e.g., Mattila 2004). Similarly, a rather equal distribution of power is also found in work that investigates bargaining dynamics in intergovernmental negotiations on constitutional issues, i.e. EU treaty change (Finke 2009; Slapin 2008). Major differences in power across member states have only been found when sub-setting the data by issue (areas) (Arregui 2016; Arregui and Thomson 2009). However, this is entirely in line with a rather uniform distribution of overall power that is then differentiated across issue (areas) due to log-rolling that reallocates influence to member states on an issue-by-issue (or area-by-area) basis (e.g., Aksoy 2012; Crombez 2000; Golub 2012a; Kardasheva 2013; König and Junge 2009).

In total, the assumption of a uniform distribution of power between member states is reflective of a “conservative” interpretation of the extant empirical findings that could even be read as suggesting power advantages of small states.

#### *b) Influence of EP and Commission*

It is a general perception that particularly the EP has become an important force in EU policy-making due to increases in its formal power in the Lisbon treaty (e.g., extension of co-decision procedure). However, the empirical evidence on the power of the EP and the Commission is mixed. While some work finds that the EP “wins”

many conflicts with the Council in conciliation committees under co-decision (König, Lindberg, Lechner, and Pohlmeier 2007), other work has put its power at consistently less than 30% of the Council's power (Costello and Thomson 2013; Thomson 2011c). Similar power shares have been estimated for the Commission, although its power appears to be very limited under the now dominant co-decision procedure (Costello and Thomson 2013). All the findings on bargaining power in EU policy-making I just referred to rest on data collected through expert interviews. Interestingly, projects that have interviewed experts affiliated with the EP (König, Lindberg, Lechner, and Pohlmeier 2007) attribute more power to this institution, while projects relying mainly on experts affiliated with Council stakeholders attribute most power to the Council (Costello and Thomson 2013; Thomson 2011c). This suggests that findings on bargaining power deduced from expert data might be rather sensitive to features of the data collection approach (see also Bueno de Mesquita 2004; Slapin 2014).

The study by Franchino and Mariotto (2012) is one of the few on inter-institutional bargaining power that does not rely on expert data but on quantitative text analysis comparing whether the Council's or the Commission's favored legislative text is closer to the joint text coming out of conciliation committees. The authors find that, on average, in 70% of the cases, the joint text is closer to the Council's favored text than the EP's favored text. However, the decision rule makes a considerable difference, since unanimity in the Council decreases the probability that the EP gets a text closer to its position by about 25 to 27 percentage points (Franchino and Mariotto 2012: 359).

These findings support my most fundamental assumption that the Council has considerable power in shaping EU policy-making, and hence, territorial representation in the Council is likely to alter the opinion-policy nexus. However, they also highlight that the supranational institutions may significantly influence policy under some circumstances. Nevertheless, it is not clear whether the supranational institutions actually attempt to pull policy away from territorial dynamics to EU-wide mean opinion. While some recent work suggests that the parties in the EP attempt to

represent the EU-wide median voter (Sorace 2018), other shows that MEPs are also loyal to domestic principals (e.g., Hix 2002, 2004; Hix, Noury, and Roland 2007). In fact, representation in the EP is also territorially divided by design, since EP elections are conducted in (sub-)national constituencies. Hence, probably the prime reason why the EP may weaken the effects of territorial representation in the EU is that the EP's party political groups are arguably more integrated than parties in the Council. Consequently, the "Council-EP/Commission" specification tests the sensitivity of the findings to an alternative specification in which representation is less skewed towards the territorial model under some circumstances. The empirics reveal that this specification of the territorial model still fits the data better than the standard model.

## 7) Detailed results of responsiveness analysis

This section reports detailed results of the responsiveness analyses in the article. First, Tables A6.1 (full sample) and A6.2 (sample of conflict issues) contain the results of the frequentist logistic regression models used for the Clarke test. Note that the operationalization of *Territorial opinion* varies according to the specification of the territorial model as described in the article and in Table A1 of this supporting information.

**TABLE A6.1: MODELS OF RESPONSIVENESS (FULL SAMPLE)**

<i>Specification</i>	<b>Territorial model</b>			<b>Standard model</b>
	<i>Council: equal power</i>	<i>Council: unequal power</i>	<i>Council-EP/Commission</i>	
Territorial opinion	5.404 (0.936)**	5.353 (0.931)**	5.472 (0.949)**	
EU-wide mean opinion				4.995 (0.877)**
Mixed competence	1.867 (0.538)**	1.838 (0.535)**	1.867 (0.538)**	1.722 (0.525)**
Mainly EU competence	1.687 (0.539)**	1.665 (0.537)**	1.706 (0.541)**	1.541 (0.525)**
Unclear decision rule	-0.296 (0.667)	-0.295 (0.665)	-0.264 (0.669)	-0.320 (0.657)
Unanimity	2.283 (0.549)**	2.271 (0.547)**	2.311 (0.552)**	2.206 (0.535)**
Constant	-6.340 (1.019)**	-6.281 (1.012)**	-6.415 (1.032)**	-5.895 (0.953)**
Number of policy issues	211	211	211	211
N	250	250	250	250
Log-likelihood	-128.19	-128.55	-128.02	-130.08

Notes: All are logistic regressions; Standard errors in parentheses; \*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

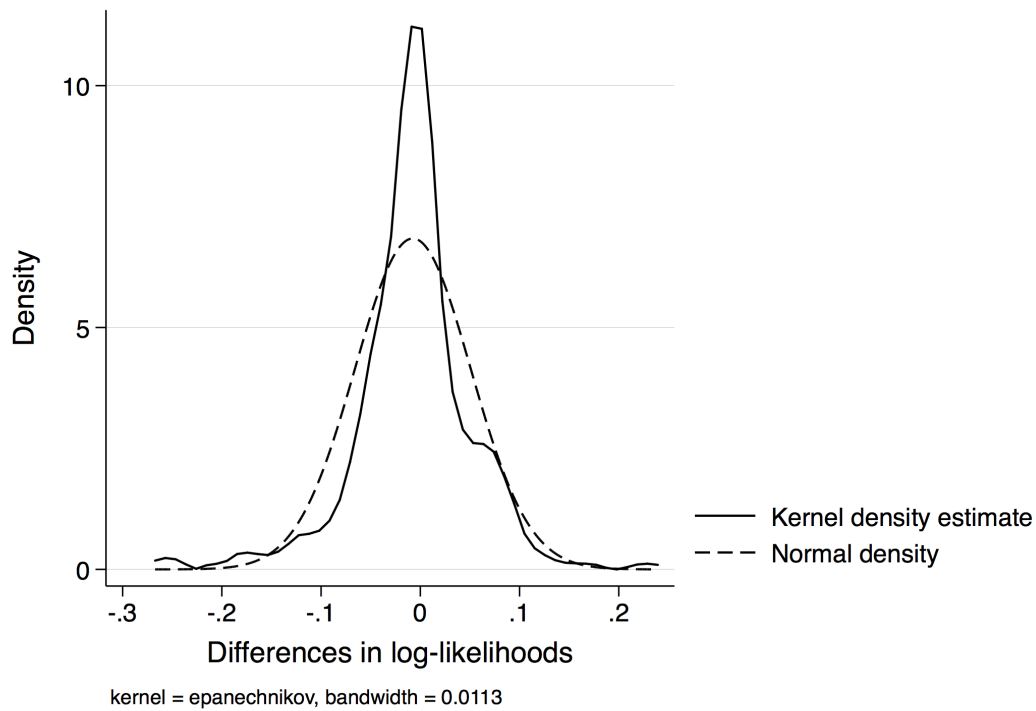
**TABLE A6.2: MODELS OF RESPONSIVENESS (CONFLICT ISSUES)**

<i>Specification</i>	<b>Territorial model</b>			<b>Standard model</b>
	<i>Council: equal power</i>	<i>Council: unequal power</i>	<i>Council-EP/Commission</i>	
Territorial opinion	20.752 (6.364)**	20.716 (6.385)**	21.515 (6.579)**	
EU-wide mean opinion				14.324 (4.870)**
Mixed competence	1.642 (1.102)	1.463 (1.080)	1.645 (1.103)	0.713 (0.978)
Mainly EU competence	1.950 (1.082)	1.909 (1.068)	2.140 (1.113)	1.132 (0.941)
Unclear decision rule	-0.704 (1.351)	-0.708 (1.361)	-0.624 (1.364)	-0.763 (1.328)
Unanimity	2.567 (1.144)*	2.700 (1.156)*	2.734 (1.163)*	2.516 (1.088)*
Constant	-14.475 (4.180)**	-14.435 (4.183)**	-15.080 (4.350)**	-10.157 (3.100)**
Number of policy issues	55	55	55	55
N	72	72	72	72
Log-likelihood	-32.82	-32.99	-32.52	-35.84

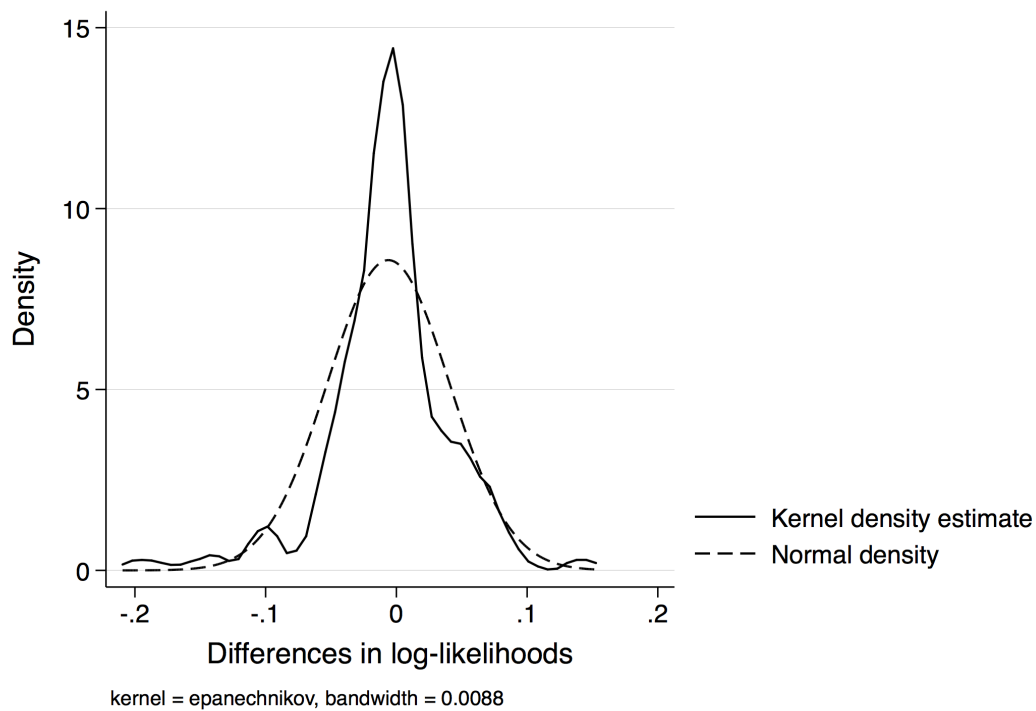
Notes: All are logistic regressions; Standard errors in parentheses; \*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

Figures A3.1 to A3.6 plot kernel density estimates of the observation-specific differences in the log-likelihoods between the specifications of the territorial model and the standard model using the full as well as the sample of conflict issues. I also include a normal density for comparison. This demonstrates that the distribution of the log-likelihood differences is clearly leptokurtic for all model comparisons. In this case, the Clarke test is asymptotically more efficient than the alternative Vuong test (Vuong 1989).

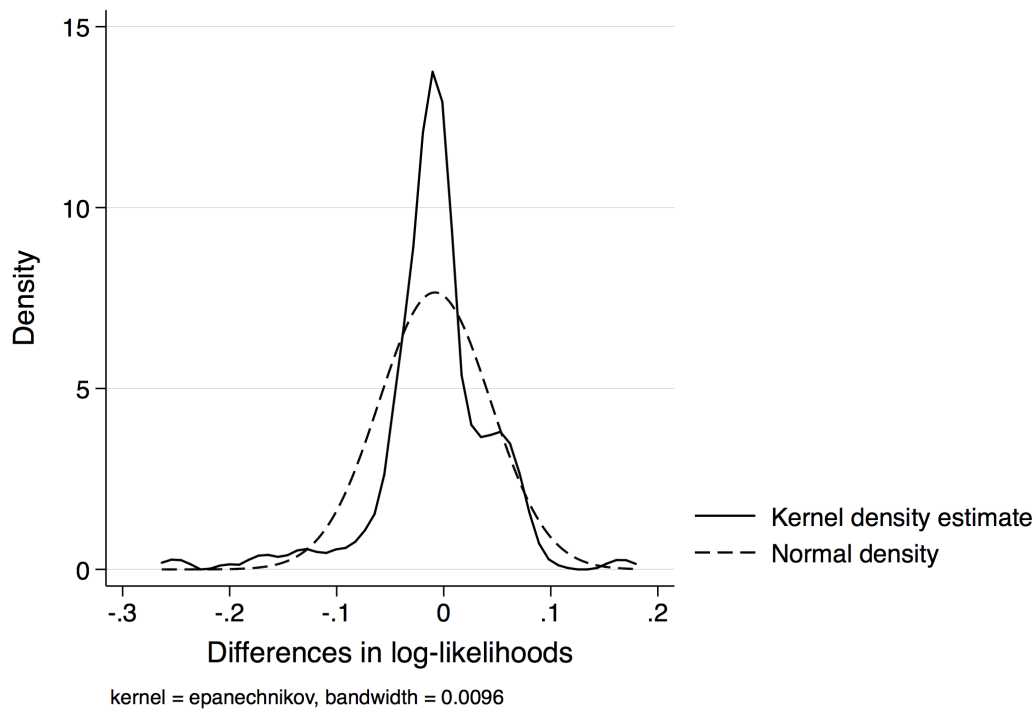
**FIGURE A3.1: DISTRIBUTION OF LOG-LIKELIHOOD DIFFERENCES IN FULL SAMPLE (COUNCIL: EQUAL POWER)**



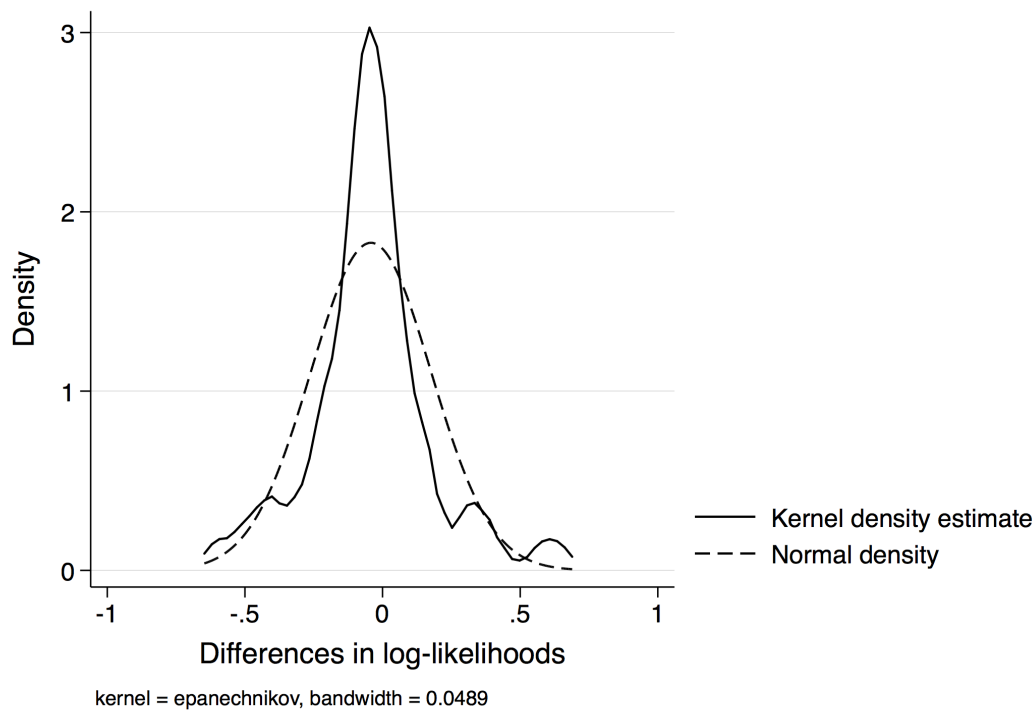
**FIGURE A3.2: DISTRIBUTION OF LOG-LIKELIHOOD DIFFERENCES IN FULL SAMPLE (COUNCIL: UNEQUAL POWER)**



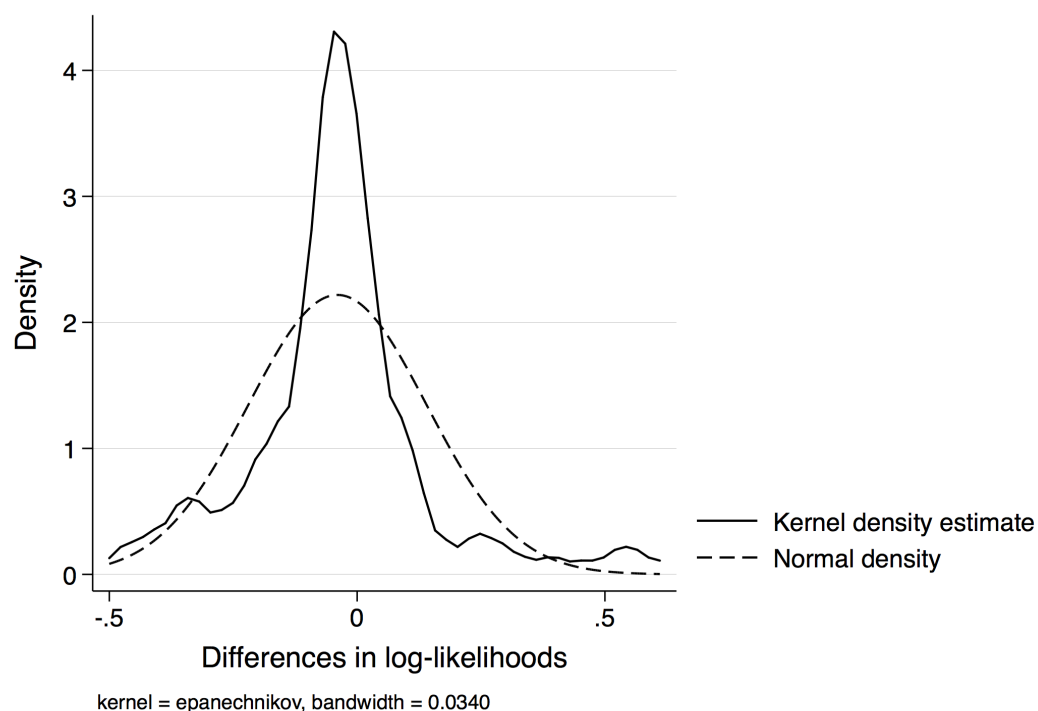
**FIGURE A3.3: DISTRIBUTION OF LOG-LIKELIHOOD DIFFERENCES IN FULL SAMPLE (COUNCIL-EP/COMMISSION)**



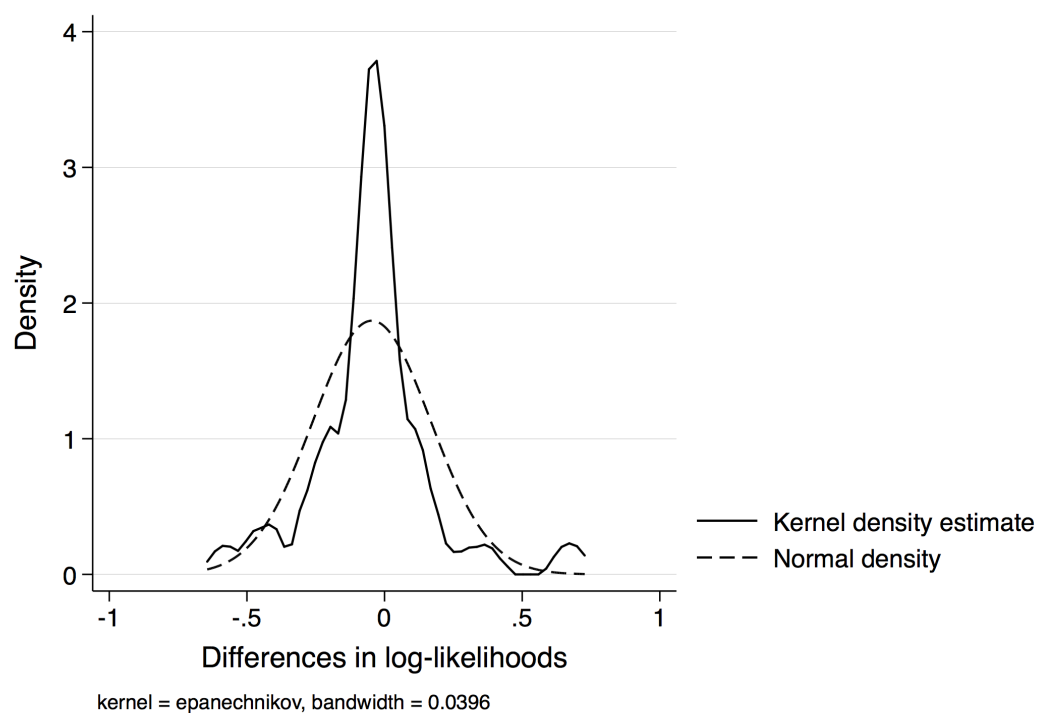
**FIGURE A3.4: DISTRIBUTION OF LOG-LIKELIHOOD DIFFERENCES IN SAMPLE OF CONFLICT ISSUES (COUNCIL: EQUAL POWER)**



**FIGURE A3.5: DISTRIBUTION OF LOG-LIKELIHOOD DIFFERENCES IN SAMPLE OF CONFLICT ISSUES (COUNCIL: UNEQUAL POWER)**



**FIGURE A3.6: DISTRIBUTION OF LOG-LIKELIHOOD DIFFERENCES IN SAMPLE OF CONFLICT ISSUES (COUNCIL-EP/COMMISSION)**



The Bayesian equivalents of the logistic regression models reported in Tables A6.1 and A6.2 are fit using a random-walk Metropolis algorithm implemented in the “MCMCpack” package in *R*. For the  $\beta$  I use a multivariate normal prior centered at 0 and with very low precision of 0.001. I also test models with prior means of 100/-100 and obtain substantively the same results. I run a Markov chain of 100,000 iterations with a burn-in of 1,000 as well as a thinning interval of 20 using the maximum likelihood estimates as starting values. To obtain the marginal likelihoods, the Laplacian approximation is used. The results are reported in Tables A7.1 and A7.2. Traceplots for all parameters from all models do not provide any indications of non-convergence (available upon request). I further check for the convergence of the sampler to its stationary distribution with the Geweke statistics. It is less extreme than  $\pm 2$  for 47 of the 48 parameters I estimate across the eight models.

An alternative way of testing whether EU policy change follows territorial or EU-wide mean opinion is including both measures in a single regression model. Intuitively, this should reveal whether the territorial opinion measures are significant predictors of policy change even if we control for EU-wide mean opinion. However, this strategy is not without problems, since territorial and EU-wide opinion are highly correlated and their measurement error may also be correlated, since they stem from the same survey fieldwork (actually, the answers of the same individuals contribute to both measures) (e.g., Achen 1985; Bashir 2015). This leads to violations of regression assumptions and potentially erroneous results. Nevertheless, I provide the results of such logistic regression models in Table A8. All models include EU-wide mean opinion and the territorial opinion measure as well as the EU’s competence level and the decision rule in the Council as controls (not reported).

**TABLE A7.1: BAYESIAN MODELS OF RESPONSIVENESS (FULL SAMPLE)**

<i>Specification</i>	<b>Territorial model</b>			<b>Standard model</b>
	<i>Council: equal power</i>	<i>Council: unequal power</i>	<i>Council-EP/ Commission</i>	
Territorial opinion	5.610 [3.746 7.402]	5.577 [3.778 7.426]	5.693 [3.899 7.632]	
EU-wide mean opinion				5.202 [3.626 7.038]
Mixed competence	1.947 [0.898 3.022]	1.914 [0.869 3.006]	1.934 [0.880 3.027]	1.797 [0.763 2.848]
Mainly EU competence	1.768 [0.773 2.878]	1.732 [0.596 2.787]	1.780 [0.732 2.900]	1.612 [0.585 2.653]
Unclear decision rule	-0.363 [-1.750 0.971]	-0.370 [-1.811 0.948]	-0.353 [-1.761 0.966]	-0.406 [-1.733 1.024]
Unanimity	2.395 [1.299 3.439]	2.380 [1.310 3.533]	2.418 [1.305 3.448]	2.317 [1.232 3.372]
Constant	-6.600 [-8.706 -4.654]	-6.549 [-8.606 -4.570]	-6.674 [-8.747 -4.663]	-6.146 [-7.982 -4.225]
Number of policy issues	211	211	211	211
N	250	250	250	250
Log marginal likelihood	-154.17	-154.54	-153.98	-156.17

Notes: All are Bayesian logistic regressions; 95% highest posterior density intervals in parentheses.

**TABLE A7.2: BAYESIAN MODELS OF RESPONSIVENESS (CONFLICT ISSUES)**

<i>Specification</i>	<b>Territorial model</b>			<b>Standard model</b>
	<i>Council: equal power</i>	<i>Council: unequal power</i>	<i>Council-EP/ Commission</i>	
Territorial opinion	22.040 [10.182 34.450]	22.052 [9.631 34.496]	22.839 [10.584 35.915]	
EU-wide mean opinion				15.353 [5.540 25.177]
Mixed competence	1.772 [-0.521 4.043]	1.579 [-0.709 3.826]	1.783 [-0.368 4.279]	0.800 [-1.255 2.971]
Mainly EU competence	2.065 [-0.203 4.266]	2.019 [-0.238 4.278]	2.263 [-0.031 4.649]	1.191 [-0.900 3.079]
Unclear decision rule	-1.143 [-4.454 1.980]	-1.164 [-4.470 1.868]	-1.047 [-4.152 2.267]	-1.170 [-4.384 1.866]
Unanimity	2.860 [0.535 5.285]	3.004 [0.621 5.398]	3.041 [0.703 5.512]	2.855 [0.655 5.229]

Constant	-15.511 [-23.794 -7.770]	-15.492 [-23.628 -7.321]	-16.151 [-25.278 -8.412]	-11.032 [-17.460 -5.020]
Number of policy issues	55	55	55	55
N	72	72	72	72
Log marginal likelihood	-53.52	-53.70	-53.20	-56.80

*Notes:* All are Bayesian logistic regressions; 95% highest posterior density intervals in parentheses.

The results in Table A8 reveal clear further evidence for the territorial model of policy representation. In both samples, all territorial opinion measures remain significant, positive predictors of policy change when EU-wide mean opinion is included. Moreover, all coefficients on EU-wide mean opinion are either pointing in an unexpected negative direction or are statistically indistinguishable from zero.

**TABLE A8: RESPONSIVENESS MODELS WITH BOTH VARIABLES INCLUDED**

Specification	<i>Council: equal power</i>	<i>Council: une- qual power</i>	<i>Council-EP/ Commission</i>
<i>Full sample (n = 250)</i>			
Territorial opinion	14.977 (6.036)*	18.402 (7.549)*	19.986 (6.953)**
EU-wide mean opinion	-9.362 (5.782)	-12.723 (7.249)	-14.033 (6.591)*
<i>Sample of conflict issues (n = 72)</i>			
Territorial opinion	28.296 (12.033)*	38.835 (15.795)*	34.687 (13.979)*
EU-wide mean opinion	-7.897 (10.437)	-17.610 (13.613)	-13.234 (11.943)

*Notes:* All are logistic regressions with the same set of control variables as specified in Tables A6.1 and A6.2; Standard errors in parentheses; \*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

I also test whether the results are robust to different definitions of “conflict issues.” In Table A9 I report the results of Clarke tests and Bayes factors re-defining “conflict issues” at three alternative thresholds.

**TABLE A9: RESULTS OF DISCRIMINATION TESTS FOR THREE ALTERNATIVE SAMPLES**

Specification	Council: equal power	Council: unequal power	Council-EP/Commission
<i>Sample of issues with more than 5% of national publics dissenting (n = 98)</i>			
<i>Clarke test:</i>			
Expected higher log-likelihoods	49	49	49
Observed higher log-likelihoods	65	65	65
P-value $H_0$ : median of differences = 0	0.002	0.002	0.002
<i>Bayes factor:</i>	9.69	7.08	13.34
<i>Sample of issues with more than 10% of national publics dissenting (n = 87)</i>			
<i>Clarke test:</i>			
Expected higher log-likelihoods	43.5	43.5	43.5
Observed higher log-likelihoods	55	56	57
P-value $H_0$ : median of differences = 0	0.018	0.010	0.005
<i>Bayes factor:</i>	7.74	5.91	10.09
<i>Sample of issues with more than 20% of national publics dissenting (n = 55)</i>			
<i>Clarke test:</i>			
Expected higher log-likelihoods	27.5	27.5	27.5
Observed higher log-likelihoods	38	41	42
P-value $H_0$ : median of differences = 0	0.007	0.000	0.000
<i>Bayes factor:</i>	22.1	26.3	37.6

*Notes:* Clarke tests are based on logistic regression models, Bayes factors on Bayesian logistic regression models; Bayes factors indicate evidence for the territorial model relative to the standard model; All models include a single measure of opinion and the EU competence level as well as decision rule as controls; Full details available upon request.

Specifically, I reduce the sample to those policy questions on which majority opinion in more than 5, 10 or 20% of the member states pointed in the opposite direction

to majority opinion in the other states. The results of all these tests entirely support the results in Table 1 in the article.

Last, note that according to the standard model of policy representation, responsiveness may also be moderated by EU-wide salience. In unreported models, I tested for this alternative by including an interaction term between EU-wide mean opinion and EU-wide mean salience predicting policy adoption. The coefficient on the interaction term is statistically indistinguishable from zero. Hence, I used the simpler specification of the model throughout the article and the supporting information.

## 8) *Detailed results of congruence analysis*

This section reports detailed results of the congruence analyses in the article. First, I assess whether the “Council: unequal power” or the “Council-EP/Commission” specification of the territorial model better fit the congruence data than the “Council: equal power” specification operationalized in the article. For this purpose, I operationalize the two specifications with two different models: 1) A model with a three-way interaction between the state-level opinion majority size, state-level salience, and the member state’s number of votes in the Council as a proxy for bargaining power. If bargaining power was clearly unequal and more vote-proportional, we would expect that governments with many votes are particularly able to shape policy change according to their citizens’ wishes. This would be indicated by a positive, statistically significant three-way interaction term. 2) A model with a three-way interaction between the state-level opinion majority size, state-level salience, and a dummy variable for unanimity as likely decision rule in the Council. If the supranational institutions were able to pull policy significantly towards EU-wide mean opinion under the potential use of QMV in the Council, we would expect that state-level opinion and salience should matter more under unanimity. This would again be indicated by the three-way interaction term.

Note that since the number of votes and the decision rule only vary on the member state and the question level respectively, their base terms could not be estimated in a model with fixed effects for member states and questions. Hence, to simplify the comparison of the models, I use a random effect for questions and no fixed effects for member states. I also re-estimate the “Council: equal power” specification with the adjusted set of random and fixed effects. The results are reported in Table A10. They reveal no evidence for the superiority of the “Council: unequal power” or the “Council-EP/Commission” specifications of the territorial model over the theoretically more parsimonious “Council: equal power” specification. The crucial three-way interactions are both pointing in the expected direction but are statistically indistinguishable

from zero. In turn, the results of the “Council: equal power” specification are as expected.

**TABLE A10: CONGRUENCE WITH ALTERNATIVE SPECIFICATIONS**

	<b>Council: equal power</b>	<b>Council: une- qual power</b>	<b>Council-EP/ Commission</b>
Opinion majority size	-8.990 (4.972)	-3.999 (7.847)	-9.605 (7.631)
Salience	0.340 (1.075)	1.606 (1.659)	-0.913 (1.765)
Opinion majority size x Salience	11.262 (5.595)*	6.085 (8.750)	9.532 (8.427)
Votes		0.104 (0.099)	
Votes x Opinion majority size		-0.429 (0.538)	
Votes x Salience		-0.109 (0.111)	
Votes x Opinion majority size x Salience		0.450 (0.599)	
Unanimity	1.601 (1.172)	1.598 (1.171)	-0.340 (2.364)
Unanimity x Opinion majority size			-4.493 (10.278)
Unanimity x Salience			1.173 (2.245)
Unanimity x Opinion majority size x Salience			8.890 (11.510)
Mixed competence	4.647 (1.228)**	4.650 (1.227)**	4.732 (1.322)**
Mainly EU competence	3.198 (1.239)**	3.198 (1.239)**	3.158 (1.320)*
Unclear decision rule	0.221 (1.278)	0.225 (1.278)	0.055 (1.379)
Constant	-2.126 (1.492)	-3.346 (1.870)	-0.359 (2.054)
Random effects	Questions	Questions	Questions
Number of policy issues	250	250	250
Number of member states	27	27	27
N	6,506	6,506	6,506
Log-likelihood	-1845.87	-1844.95	-1839.45

*Notes:* All are mixed effects logistic regressions; Standard errors in parentheses; \*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

The congruence analysis also allows us to ascertain whether the determinants of congruence vary between issues on which policy change occurs and those on which no change occurs. I do this with an interaction effect between the state-level opinion majority size, salience, and the binary adoption measure. Otherwise, I simply use the same model specifications as in Table A10. The results are reported in Table A11.

**TABLE A11: CONGRUENCE DEPENDING ON POLICY CHANGE**

	<b>Estimates</b>
Opinion majority size	-19.804 (6.914)**
Salience	3.057 (1.312)*
Opinion majority size x Salience	17.992 (7.692)*
Policy adoption	13.043 (2.383)**
Policy adoption x Opinion majority size	15.728 (12.646)
Policy adoption x Salience	-10.296 (2.588)**
Policy adoption x Opinion majority size x Salience	-1.917 (14.186)
Mixed competence	1.361 (0.900)
Mainly EU competence	1.043 (0.861)
Unclear decision rule	0.902 (1.016)
Unanimity	-0.252 (0.852)
Constant	-4.097 (1.425)**
Random effects	Questions
Number of policy issues	250
Number of member states	27
N	6,506
Log-likelihood	-1706.41

Notes: All are mixed effects logistic regressions; Standard errors in parentheses;

\*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

The results reveal no evidence that the structure between congruent and incongruent policies and the patterns of opinion and salience are different depending on whether policy change occurred. The three-way interaction is statistically indistinguishable from zero. However, the results reveal that policy adoption is related to a higher probability of congruence. We also see that the direct relationship between salience and congruence is weaker under policy adoption.

Last, I check whether the congruence results are sensitive to different thresholds for defining the sample of conflict issues. In Table A12 I re-estimate the specification from Model 2 in Table 2 in the article, re-defining the sample to questions on which majority opinion in more than 5, 10 or 20% of member states pointed in the opposite direction to majority opinion across the other states. The results hold using each alternative definition of conflict issues.

**TABLE A12: RESULTS FOR CONGRUENCE FOR THREE ALTERNATIVE SAMPLES**

	<b>Conflict issues (5%)</b>	<b>Conflict issues (10%)</b>	<b>Conflict issues (20%)</b>
Opinion majority size	-12.042 (5.993)*	-12.525 (6.229)*	-13.014 (7.062)
Salience	-0.538 (1.393)	-0.268 (1.434)	-1.150 (1.590)
Opinion majority size x Salience	15.438 (6.753)*	15.704 (7.017)*	17.426 (7.955)*
Fixed effects	Questions, member states	Questions, member states	Questions, member states
Number of questions	98	87	63
Number of member states	27	27	27
N	2,541	2,258	1,623
Log-likelihood	-1323.08	-1246.39	-992.57

*Notes:* All are (mixed effects) logistic regressions; Standard errors in parentheses;

\*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

## 9) *Robustness checks*

In this section, I conduct several robustness checks regarding the analyses in the article. All results are reported in Tables A13 and A14.1/2. All checks are conducted using the full sample as well as the sample of conflict issues. For the responsiveness analyses, if not specified otherwise, I estimate and perform the models and tests used in the first column of Table 1 in the article comparing the “Council: equal power” specification of the territorial model with the standard model. For the congruence analysis, I re-estimate Model 1 in Table 2 in the article when using the full sample. However, since the number of observations is significantly diminished for some robustness checks (e.g., excluding certain observations), I use a random effect instead of fixed effects for questions when using the sample of conflict issues, i.e. I use the specification from Model 3 in Table 2 in the article.

First, I check the results for one further operationalization of salience-weighted opinion in the responsiveness analysis. While in the article the salience weights  $\omega_{Salience_{i,j}}$  are rescaled so that the highest observed value for salience in the sample is re-assigned “1” and the lowest “0,” in Model A-R1 I simply use the “raw” observed values for salience as weights. The results are substantively exactly the same.

Second, I address two concerns regarding the measurement of adoption. On the one hand, the binary measure of adoption is based on a continuous measure (0-100) and successful adoption is recorded if at least 80% of the proposed policy change was adopted. This threshold established in the literature (Gilens 2005, 2012) may seem arbitrary and therefore Models A-R2 and A-C1 re-estimate all key results on responsiveness and congruence with a threshold value for successful adoption of 50%. If anything, this strengthens the results on responsiveness. On the other hand, using a binary measure of adoption is necessary for the congruence analysis, but not the responsiveness analysis, where the binary measure removes variation from the data. To test whether this has any consequences, I re-estimate the baseline model from the responsiveness analysis with the continuous adoption degree as dependent variable

using linear regression in Model A-R3. All tests still favor the territorial model over the standard model, but on the sample of conflict issues the Clarke test results are more unequivocal than the Bayes factor.

Third, I address two concerns regarding problematic policy issues in the dataset. As I have alluded to above, 47 questions were included in the data that violate aspects of the four criteria that had, in principle, to be fulfilled for inclusion. To check whether these “borderline” issues drive any results, I perform the responsiveness and congruence analyses without these issues in Models A-R4 and A-C2. In the responsiveness analysis, the results remain the same. In the congruence analysis, all results remain using the full sample. In contrast, using the conflict sample the coefficient on the interaction term between the opinion majority size and salience is just above the 5% significance level with a p-value of 0.053. However, this is also the smallest sample I use for the congruence analysis, and hence, I do not view this borderline case for statistical significance as problematic for the main conclusions. On the other hand, the coding of the adoption records revealed that 33 questions in the data surveyed support for a status quo rather than for policy change. In these cases, I swapped the public opinion estimates so as to reflect support for change. Since this may be problematic, I re-estimate the results in Models A-R5 and A-C3 excluding the potentially problematic questions. All results firmly hold.

Third, I test the results’ sensitivity with regard to the maximum adoption lag that still counts as successful adoption. While I use six years as maximum coding window in the article, I re-estimate the main analyses with alternative windows of up to five and a half, and five years (i.e., 2008 and 1825 days). The results are reported as Models A-R6 and A-R7 as well as A-C4 and A-C5. The findings from the article are clearly supported by all analyses, on both samples.

In total, the robustness checks fully support the main results reported in the article.

**TABLE A13: ROBUSTNESS CHECKS ON RESPONSIVENESS ANALYSIS**

	Model A-R1	Model A-R2	Model A-R3	Model A-R4	Model A-R5	Model A-R6	Model A-R7
<i>Full sample</i>							
Clarke test: p-value of $H_0$ (compared to standard model)	0.002	0.000	0.004	0.017	0.014	0.002	0.001
Bayes factor (compared to standard model)	7.73	22.4	7.35	6.46	3.52	6.35	6.87
N	250	250	250	203	217	250	250
<i>Sample of conflict issues</i>							
Clarke test: p-value of $H_0$ (compared to standard model)	0.001	0.001	0.001	0.001	0.002	0.006	0.000
Bayes factor (compared to standard model)	32.5	57.1	1.69	38.6	19.2	20.8	32.5
N	72	72	72	63	64	72	72
Robustness check	“Raw” sali- ence weights	Adoption from >=50%	Continuous DV	Exclude items violat- ing inclusion criteria	Exclude items on support for SQ	Five-and-a- half-years coding win- dow	Five-years coding win- dow

*Note:* Clarke tests are based on logistic regression models, Bayes factors on Bayesian logistic regression models; Bayes factors indicate evidence for the territorial model relative to the standard model; All models include a single measure of opinion and the EU competence level as well as decision rule as controls.

**TABLE A14.1: ROBUSTNESS CHECKS ON CONGRUENCE ANALYSIS (FULL SAMPLE)**

	<b>Model A-C1</b>	<b>Model A-C2</b>	<b>Model A-C3</b>	<b>Model A-C4</b>	<b>Model A-C5</b>
Opinion majority size	-12.282 (5.642)*	-11.060 (6.316)	-10.381 (5.935)	-12.405 (5.725)*	-17.242 (5.832)**
Salience	-1.228 (1.360)	-0.535 (1.537)	-0.688 (1.434)	-0.579 (1.364)	-1.198 (1.367)
Opinion majority size x Salience	16.037 (6.363)*	15.071 (7.122)*	13.778 (6.711)*	15.711 (6.447)*	20.516 (6.561)**
Fixed effects	Question, member states	Question, member states	Question, member states	Question, member states	Question, member states
Number of member states	27	27	27	27	27
N	2,853	2,310	2,619	2,853	2,853
Robustness check	Adoption from ≥50%	Exclude items violating inclu- sion criteria	Exclude items on support for status quo	Five-and-a- half-years cod- ing window	Five-years coding win- dow

*Notes:* All are logistic regressions; Standard errors in parentheses; \*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

**TABLE A14.2: ROBUSTNESS CHECKS ON CONGRUENCE ANALYSIS (CONFLICT ISSUES)**

	<b>Model A-C1</b>	<b>Model A-C2</b>	<b>Model A-C3</b>	<b>Model A-C4</b>	<b>Model A-C5</b>
Opinion majority size	-13.238 (6.523)*	-10.930 (7.155)	-12.123 (6.911)	-16.006 (6.603)*	-23.429 (6.739)**
Salience	-0.908 (1.376)	0.199 (1.542)	-0.147 (1.489)	-0.532 (1.389)	-1.332 (1.393)
Opinion majority size x Salience	17.466 (7.351)*	15.590 (8.063) <sup>T</sup>	16.440 (7.818)*	20.283 (7.432)**	27.742 (7.576)**
Mixed competence	0.401 (0.310)	0.721 (0.366)*	0.700 (0.395)	0.474 (0.320)	0.470 (0.323)
Mainly EU competence	-0.298 (0.314)	0.091 (0.376)	-0.197 (0.338)	-0.212 (0.324)	-0.193 (0.328)
Unclear decision rule	-0.569 (0.348)	-0.310 (0.412)	-0.553 (0.403)	-0.546 (0.359)	-0.524 (0.363)
Unanimity	0.346 (0.326)	0.659 (0.378)	0.532 (0.392)	0.313 (0.336)	0.316 (0.340)
Constant	-0.172 (1.295)	-1.384 (1.458)	-0.932 (1.414)	-0.229 (1.309)	0.734 (1.313)
Fixed effects	Member states	Member states	Member states	Member states	Member states
Random effects	Questions	Questions	Questions	Questions	Questions
Number of member states	27	27	27	27	27
N	1,859	1,626	1,650	1,859	1,859
Robustness check	Adoption from ≥50%	Exclude items violating inclu- sion criteria	Exclude items on support for status quo	Five-and-a- half-years cod- ing window	Five-years coding win- dow

Notes: All are logistic regressions; Standard errors in parentheses; <sup>T</sup> $p < 0.054$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

### *10) Does Territorial Representation Make a Difference?*

In the section “Does Territorial Representation Make a Difference?” in the article, I discuss the substantive magnitude of the difference territorial representation makes when we try to compare it to a counterfactual system without territorial representation. Here, I provide results from additional procedures to gauge the substantive impact of territorial representation.

First, we can assume that the estimates of the territorial model accurately describe the “true” impact of public opinion on policy and that the only difference between the real EU system and a counterfactual EU system without territorial representation would be that the effect of opinion was not valid for salience-weighted but EU-wide mean opinion. Of course, this assumption is not unproblematic and its invalidity may result in bias. However, under this assumption, we can estimate the impact of territorial representation by comparing predicted probabilities when opinion is the salience-weighted measure (as in the estimation) to those when we counterfactually plug in the EU-wide mean measure. The results of this alternative procedure reveal even more striking differences. While the average difference in predicted probabilities is 1.9 percentage points using the full sample, it is 12 percentage points using the sample of conflict issues, with several cases of over 30 percentage points difference.

Second, instead of thinking in terms of “raw” differences in predicted probabilities, some readers may prefer to assess substantive differences in terms of classification, i.e. how often policy change would have occurred under territorial representation, while it might not have occurred without it (and vice versa). We can approximate this by simply classifying the predicted probabilities at the 0.5 threshold. Note however that this threshold, of course, is somewhat arbitrary. Nevertheless, it is most frequently used. Applying the procedure used in the article, we can see how often the territorial model predicted policy change ( $>0.5$ ), while the standard model made the opposite prediction (and vice versa). This reveals that using the full sample, the models make different directional predictions in 3.2% of the cases. However, when only using the sample of conflict issues, 11.1% of the policies turn out differently in

the two models. When using the alternative procedure described above, these figures are 4% for the full sample and 15.3% for the sample of conflict issues. Hence, when there is conflict between national publics, we would expect about 15% of EU policies to turn out differently compared to a counterfactual system that reacts to EU-wide mean opinion.

Again, we can compare these differences to the difference opinion makes itself. For this purpose, I use the results from the standard model on the sample of conflict issues and calculate predicted probabilities at the observed values as well as when shifting EU-wide mean opinion 3 percentage points (i.e., the mean over-time change in opinion) up as well as 3 percentage points down for all observations. How often do we get different classifications when discriminating at the 0.5 threshold? When shifting opinion up, we obtain different classifications in 11.1% of the observations, when shifting it down, 8.3% of the policies change. Hence, typical changes in public opinion, even if they are artificially uniform across policies as in this example, make less or just as much difference as territorial representation makes. The result from the article that the impact of territorial representation is about the size of the impact of opinion therefore is also valid when focusing on differences in classification.

Last, I consider the two issues in Table 3 in the article, on which the prediction of the standard model was closer to the outcome than the prediction of the territorial model (“Accession of Croatia” and “Decrease in EU budget share for agriculture”). Interestingly, both of these issues are borderline cases with regard to adoption. First, the question on Croatia’s accession was asked in 2006. Hence, adoption was checked up to 2012, while Croatia joined just after the cut-off in 2013. Second, the question on the EU’s budget share in agriculture asked for a cut of funds to less than 40% of the EU budget, and the EU adopted a cut to around 38-39% in the 2014-2020 multiannual financial framework. In turn, the adoption records for the eight other issues are inconspicuous. This should provide further confidence in my conclusions.

Taken together, these results suggest that territorial representation has significant substantive consequences for the opinion-policy linkage.

### 11) Direction of causality

Last, I address concerns about the direction of causality in the opinion-policy linkage. If national governments can anticipate negotiation outcomes at the EU level years ahead of adoption, they could potentially send cues to their national public to influence public opinion in a direction that is consistent with the expected outcome (e.g., to increase the attention or interest of the public regarding issues that will be adopted in their favor). Governments could more influence opinion than being influenced by it. A problem for this endogeneity thesis is the time lag between the survey fieldwork and the adoption date. The *longer* the lapse of time, the *less* likely it is that the government in office during survey fieldwork *a)* can anticipate the eventual negotiation outcome at the EU level, and *b)* has an incentive to manipulate opinion, as it may be replaced in due course. Hence, if national publics were rather taking cues from their governments than governments listening to public opinion, we would expect the strongest relationships between public opinion and EU-level policy-making the closer opinion is surveyed to the adoption date.

We can test this implication of reversed causality in the congruence analysis by restricting the sample to the 36% of issues on which change occurred (and we therefore know the time lapse).<sup>7</sup> For this purpose, I add a three-way interaction between the adoption lag (in days between survey fieldwork and political decision), opinion, and salience to the specification of Model 3 in Table 2 in the article. The results are reported in Table A15 and show no impact of the adoption lag on the congruence analysis. If elite manipulation explained the correlation between opinion and salience at the state level and EU-level policy change, we would expect this correlation to be particularly strong if the time lag between the survey and policy adoption is short. The data provides no evidence for this. For defenses of the causality assumption per-

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<sup>7</sup> As the adoption date is only known if policy change occurs, I could not perform a similar test for the responsiveness analysis.

taining to very similar datasets see also the online appendix in Lax and Phillips (2012) as well as Gilens (2012: 93-96). Note also the recent findings on responsive policy-making in the EU that stress a causal direction from opinion on policy-making (e.g., Bølstad 2015; Toshkov 2011).

**TABLE A15: CAUSALITY IN CONGRUENCE ANALYSIS**

	<b>Estimates</b>
Opinion majority size	-10.055 (24.539)
Salience	-7.145 (4.878)
Opinion majority size x Salience	24.016 (27.589)
Adoption lag	-0.003 (0.003)
Adoption lag x Opinion majority size	0.004 (0.018)
Adoption lag x Salience	0.004 (0.004)
Adoption lag x Opinion majority size x Salience	-0.006 (0.020)
Constant	7.529 (4.576)*
Control variables	Yes
Fixed effects	Member states
Random effects	Questions
Number of questions	90
Number of member states	27
N	2,368
Log-likelihood	-392.93

*Notes:* Mixed effects logistic regression; Standard errors in parentheses;

\*  $p < 0.05$ ; \*\*  $p < 0.01$  (two-tailed).

## *12) Generalizability of findings*

In the article, in the section “The Case of the European Union,” I argue that the EU is a “likely case” for the impact of territorial representation on the opinion-policy nexus. This poses the question of the extent to which the findings are likely to apply and hold in other political systems. Let me briefly recap what factors amplify the consequences of territorial representation for the opinion-policy nexus compared to the standard model of policy representation to mean opinion. In other words, when will the theoretical predictions of the two models be most different? *Ceteris paribus*, the following factors should amplify the consequences of territorial representation:

- The extent of **differences in opinion and salience** across territories
- The existence and **legislative powers of a territorial/upper chamber** of parliament as well as of other territorially divided decision-making bodies vis-à-vis territorially-undivided bodies
- The degree of **disproportionality between voter populations and the power of territorial representatives** in the upper chamber/territorially-divided bodies
- The degree of **disintegration of the party system** across territories

How peculiar is the EU’s political system with regard to these factors compared to other systems? I actually think that while the EU is a likely case among the most studied Western political systems, it is not a singular case. I illustrate this with the U.S. political system as the system political scientists have paid most attention to.

First, while it is hard to directly compare the extent of territorial differences in opinion and salience in the EU versus the U.S., several important works on American politics stress substantial differences in voter preferences across states (e.g., Cohen 2006; Enns and Koch 2013; Erikson, Wright, and McIver 1993; Lax and Phillips 2012). In particular, since a lot of very controversial policy domains, such as social policy or direct taxation, have never been integrated and remained off the EU’s agenda, it is fair to assume that policy-making at the federal level in the U.S. has to accommodate

similar differences in opinion and salience across states. Second, the U.S. Senate as the upper chamber of the political system is a full co-legislator whose approval is needed for any bill to pass. The Senate also exercises additional ratification and confirmation rights. It therefore has, in principle, similar influence over policy-making as the EU's Council. Moreover, representation in the House is also territorially divided (as it is in the EP).

Third, vote apportionment in the Senate is *more* population-disproportional than in the Council under the increasingly used QMV rule. Whereas each U.S. state has two senators, representing as much as 39 million Californian citizens and as few as 580 thousand in Wyoming, formal votes in the Council used to be mildly population-disproportional (with 29 votes held by Germany and only three by Malta). Since November 2014, member states' vote weights in the Council are equal to their share of the total EU population. The equal unit representation in the Senate seems to translate into a largely uniform distribution of bargaining power between states, some research even highlights the success of small states (Ansolabehere, Snyder, and Ting 2003; Knight 2003; Lee 1998, 2000). Fourth, while the U.S. party system may be more integrated than the EU party system, party control over representatives is generally very limited due to the personalized majoritarian electoral system (e.g., Thorlakson 2009).

Hence, I argue that a priori there is no reason why the findings should not extend to the U.S. case. Similar conclusions would follow if comparing the EU to the political systems of Brazil, Canada, Switzerland or Germany. Future work should test the argument with data from these systems.

## Implementation Check Codebook

When checking the implementation record for a proposed policy change, please adhere to the following rules:

1. Wherever possible use **official information from EU institutions** (e.g., the European Commission, the European Parliament, the Council of the EU, EU agencies). The websites of the European Commission as well as EUR-Lex are particularly helpful.
2. Wherever possible point to **legislative activity by the institutions** in order to determine whether policy change occurred. Most policy changes occur in the form of EU regulations and directives and their specific provisions. However, in some instances, informal activity by the institutions represents policy change (e.g., coordination effort, joint planning), or policy change occurs as treaty change.
3. Wherever official information on legislative activity is not sufficient to evaluate the implementation record use information from **objective sources**. These could be, in particular, Wikipedia and news agencies.
4. If the implementation record can neither be determined from official information nor from objective sources, please code the **implementation variables as missing** (blank cell) and add an explanation in the "Comment" variable. (NOTE: Finding no evidence for implementation should be coded as "no implementation" and not as missing. Meanwhile, missing should be coded where the information in official and objective sources is ambiguous or does not allow accurate assessment of the exact policy change proposed).

### Variables

#### Implemented before survey?

YES = The proposed measure was already implemented by the time the survey was conducted ("political agreement" by Council / EP is sufficient in the case of legislation).

NO = The proposed measure was not already implemented by the time the survey was conducted ("political agreement" by Council / EP is sufficient in the case of legislation).

➔ IF YES: Check whether the change was abolished again after being implemented.

### Implementation Degree

0-100 (rounded to the nearest 5) = Indicates the percentage of the proposed policy change that was implemented following the time the survey was conducted (“political agreement” by Council / EP is sufficient in the case of legislation; last national ratification is needed for international treaties). For instance, if the proposed change is that Croatia becomes a member of the EU, the number of stages in the accession process that have been completed compared to the total number of stages that have to be completed for admission indicates the degree of implementation.

### Implementation Time

DD/MM/YYYY = The date on which the policy change is passed (or the last significant change occurred in cases of partial implementation). In the case of legislation, this is the date on which the “political agreement” is reached in all institutions that have to consent (e.g., Council and EP). In the case of international treaties, this is the date when the last national ratification took place. In the case of non-legislative change, this is the date when the proposed policy change was bindingly agreed upon by the relevant decision-makers.

### Sources

Web links to the relevant information sources should be included here.

### Comment

An informal explanation of the assessments should be included here (e.g., “Data Protection Directive was passed by Council after EP amendments on 19/07/2010. It includes new provisions for the protection of minors that are “special” in the sense that higher standards of protection apply compared to adults. Hence, policy change has occurred and to a degree of 100.”)

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