Supplementary Materials for Constituency Diversity, District Magnitude, and Voter Coordination

Joshua D. Potter
Assistant Professor of Political Science
Louisiana State University
jpotter@lsu.edu

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APPENDIX 1: MEASURING CONSTITUENCY DIVERSITY

This section contains more details on the preparation and coding of the individual-level survey data from the CSES. To begin, missing values across each of the seven sociodemographic components were multiply imputed on a country-by-country basis. This approach ensures two things: first, that values were not created along questions which were not asked in a country (such as ethnicity in Spain or religion in Sweden) and, second, that patterns of association between variables in one country were not applied to other countries in the imputation of missing values (there could, after all, be a relationship between income and language in one country, but not another; we would want this pattern to inform the imputation process in the first country, but not others). Thus, where the CSES did not ask a question, no missing values were imputed. The variables and district-level response rates in each country are included in Table A1.1 below.

Table A1.1: Countries Included in the Analysis.

Country	Total Resp	Average Resp/Dist	Ethnic	Language	Religion	Rural	Income	Regime
Austria	1,046	29.9		x	x	x	x	x
Croatia	1,004	100.4	x	x	x	x	x	x
Finland	2,323	178.7	x	x	x	x	x	x
Ireland	3,446	85.1			x	x	x	x
Norway	6,116	321.9		x		x	x	x
Poland	5,823	142.0	x	x	x	x	x	x
Portugal	6,484	360.2		x	x	x	x	x
Romania	2,724	51.4	x	x	x	x	x	x
Slovenia	3,033	252.8	x	x	x	x	x	x
Spain	3,632	56.8		x	x	x	x	x
Sweden	3,746	129.2		x		x	x	x
Switzerland	6,630	261.7	x	x	x	x	x	x
Total	**46,007**	**127.9**						

Notes: The second column contains the total number of respondents (pooled across survey waves) included from each country. The third column contains the average number of CSES respondents per district included in the analysis, while subsequent columns indicate which traits were utilized in the diversity calculations.

Respondents who were polled during different waves of the CSES – but resided in the same district – were pooled together for the calculation of constituency diversity; thus, each district takes on one time-invariant value of diversity. Pooling survey respondents in this way offers several advantages. First, this approach increases the average number of respondents at the district level (reported in the second column above). Second, evidence suggests that the distribution of attributes across districts will not change markedly during any 8- or 10-year period; thus, pooling essentially gives the researcher 2 (or even 3) snapshots at the same (relatively static) distribution of sociodemographic attributes. For example, the modal "ethnic" response category in the same district across different waves of the CSES remained static from year to year in 89% of the cases, the modal "language" category remained constant in 74% of the cases, and the modal "religion" category remained constant in 85% of cases.[2] Thus, we are not losing out on over-time nuances by pooling across waves.

Because the CSES surveys do not sample at the district level, their representativeness of the district aggregate is questionable. The main text analysis drops low-response rate districts in an effort to ensure that the results are not predicated on the inclusion of those districts most likely – but *not necessarily* – to be unrepresentative of the district-level aggregate. Appendix 8 below replicates the results by employing three other cut-off thresholds than that employed in the main text, demonstrating that – even with a significantly more stringent drop criterion – the results are remarkably robust. As justified in the main text and as supported by previous work such as Stoll (2008), relying on the CSES to construct district-level demographic pictures of voters is a "best case" scenario in the field of comparative electoral politics given extant data repositories.[3]

While the selection of countries and years for inclusion in the study might at first glance appear odd, these decisions were dictated by data availability. As discussed in the manuscript, in order to be included in the analysis, a country required (a) inclusion in the CSES survey database, with district-level identifiers intact, which was not always the case; (b) a series of elections covered in the Global Elections Database with complete seat and vote data and no major changes to district boundaries; and (c) an average district-level CSES survey response rate (pooled across CSES waves) at least above 20 individuals in order to ensure passable district-level representativeness of the full set of voters residing in the constituency.[4] Thus, some countries – such as Denmark – were therefore ruled out by the first criterion. Others – such as New Zealand and the Czech Republic – were removed due to the second criterion. Finally, the single-member district countries included in the CSES – Australia, Canada, the United Kingdom, and the United States – all possess insufficiently high district-level response rates. While this raises concerns about generalizing the paper's results to single-member district countries, there are, however, many individual single-member districts included in the analysis – just those that are situated in broader, multi-member district contexts.

[2] Even for those nascent Eastern European democracies with multiple waves of the CSES, we might be especially concerned with demographic fluctuations in the short-run. I can demonstrate that this is not a problem. For example, the modal ethnic, language, and religion responses in Romania's districts remain constant 78%, 86%, and 76% of the time, respectively. Language and religion in Slovenia remain constant in 100% and 75% of districts, respectively. Only one CSES wave was utilized in Croatia, thus no pooling occurred for this country.

[3] Potter (2014) and Crisp, Olivella and Potter (2013) also discuss at greater length several other issues related to the coding of constituency-level diversity, such as relying on the nominal (versus ordinal) variant of Krippendorff's alpha, comparisons to other diversity metrics, external validation of the measures, and the robustness of the measures to different response rates, district magnitudes, and trait inclusions.

[4] The earliest year a country could be included in the data was the first year it was surveyed by the CSES in such a way that its district-level identifiers appeared in the survey. Thereafter, I included every election year that was included in the Global Elections Database, even if that particular year had no accompanying CSES module. However, every effort was made to maximize the inclusion of countries and years in the analysis. When this could be accomplished by turning to supplementary data sources, I did so. Thus, data for Ireland in 2002 and 2007 came from Adam Carr's online electoral data repository; data for Portugal in 2009 and Romania in 2000 came from the European Election Database's online holdings. These critical supplements allow for the inclusion of these three countries, whereas they would have otherwise been dropped.

Even though the database draws on 46,007 survey respondents and many election years, only several hundred observations appear in the analysis. This is due to the unit of analysis being aggregated constituencies, rather than individual voters. Table A1.2 below indicates which country-years were covered by the CSES and which were included in the analysis. As noted above, the first point at which a country became eligible for inclusion in the analysis was the first year a CSES survey was administered in the country *that bore district-level identifiers which could be matched to elections in the GED*. Thereafter, each additional election year was eligible for inclusion in the study, even those that did not have an accompanying CSES survey. Because CSES modules were pooled, the values of constituency diversity were simply invariant across all election years. Given that the district-level distributions of sociodemographic traits tend to vary little over time, future research might extend the values of constituency diversity backward in time to include a broader range of elections within each country (although this seems more difficult to justify than the approach taken here).

Table A1.2: Country-Year Coverage and Inclusion in Analysis.

Country	**Year**	**CSES**	**Analysis**
Austria	2008	X	X
Croatia	2007	X	X
Finland	2003	X	X
	2007	X	X
Ireland	2002	X	X
	2007	X	X
Norway	1997	X	X
	2001	X	X
	2005	X	X
Poland	2001	X	X
	2005	X	X
	2007	X	X
Portugal	2002	X	X
	2005	X	X
	2009	X	X
Romania	1996	X	X
	2000		X
	2004	X	X
Slovenia	1996	X	X
	2000		X
	2004	X	X
	2008		X
Spain	1996	X	X
	2000	X	X
	2004	X	X
	2008		X
Sweden	1998	X	X
	2002	X	X
	2006	X	X
Switzerland	1999	X	X
	2003	X	X
	2007	X	X

Table A1.3 below includes the number of district-election observations from each country in the data set, as well as the distribution of district-level diversity scores across constituencies in each country. It is readily apparent that the sample of districts is unbalanced across countries, although Appendix 6 explores the extent to which this fact might be unduly influencing the regression's results. The distribution of values below indicates a couple of important and inherently intuitive things about district-level diversity cross-nationally: first, that some countries are on average more diverse than others; second, that virtually every country has *some areas* within it that are more diverse than *other areas*. Previous national-level measures of cross-cutting cleavage structures, ethnic fractionalization, and linguistic fractionalization are unable to provide much insight into this latter point.

Table A1.3: Distribution of Constituency Diversity by Country.

Country	N_{cd}	**Min**	**Mean**	**Max**
Austria	35	0.29	**0.51**	0.68
Croatia	10	0.45	**0.54**	0.61
Finland	26	0.41	**0.48**	0.57
Ireland	81	0.53	**0.66**	0.79
Norway	57	0.46	**0.61**	0.66
Poland	123	0.40	**0.49**	0.53
Portugal	54	0.61	**0.67**	0.73
Romania	106	0.29	**0.52**	0.76
Slovenia	24	0.64	**0.65**	0.67
Spain	192	0.34	**0.59**	0.77
Sweden	87	0.62	**0.73**	0.79
Switzerland	76	0.53	**0.66**	0.76

APPENDIX 2: DISTRICT MAGNITUDE

Although there are comparatively few observations in the data set where M=1, there are several "low magnitude" districts where M=5 or less. The marginal effects derived from Model 4 indicate that increasing diversity increases coordination failure in about 15% of districts in the data set (specifically, those districts where M=1, 2, or 3). Employing the coarser low-high magnitude dichotomous indicator in Model 5, increasing diversity increases coordination failure in about 37% of districts in the data set. I demonstrate in Appendix 6 below that excluding any one of these countries does not undermine the results presented in the main text. Furthermore, I demonstrate in Appendix 5 that excluding districts where M=1 does not undermine the results – and, in fact, actually strengthens their substantive impact on coordination failures.

Table A2.1: Detailed Distribution of District Magnitude by Country.

Country	N	**1**	**2**	**3**	**4**	**5**	**6-10**	**>10**
Austria	35	15	10	5	3	2	0	0
Croatia	10	0	1	3	4	2	0	0
Finland	26	0	0	0	0	0	12	14
Ireland	81	0	0	30	27	24	0	0
Norway	57	0	0	0	5	6	32	14
Poland	123	0	0	0	0	0	57	66
Portugal	54	0	3	7	6	5	18	15
Romania	106	0	0	0	13	13	65	15
Slovenia	24	0	0	0	0	0	0	24
Spain	192	0	0	28	34	38	72	20
Sweden	87	0	3	0	0	3	31	50
Switzerland	76	15	7	4	2	8	22	18
Total	871	30	24	77	94	101	309	236

As discussed in the main text, the two primary explanatory variables in this study – district magnitude and constituency diversity – are not endogenous to one another. That is, the two variables are poorly correlated with one another (r = 0.06) and larger-M districts are not innately more or less diverse than smaller-M districts.[5] In particular, the average level of diversity among "low" magnitude districts is 0.59 (with a standard deviation of 0.10, n = 326) and the average level of diversity among "high" magnitude districts is 0.60 (with a standard deviation of 0.11, n = 545). There are not, then, collinearity problems within the model's main explanatory variables.

[5] Recent works by Bochsler (2010) and Alesina and Spolaore (2003) offer more extensive and instructive discussion on this point.

APPENDIX 3: DESCRIPTIVE STATISTICS

Table A3.1: Descriptive Statistics.

Level of Analysis	Variable	Mean	S.D.	Min	Max	*N*
District-Election	Coordination Product	1.95	1.84	0.00	14.95	871
	Hopeless Votes / 1000	139.12	147.47	0.00	1611.83	871
	District Magnitude	8.54	6.45	1	48	871
	New Parties as % of magnitude	1.15	1.59	0.00	10.80	871
	Lagged Vote Volatility	0.11	0.07	0.00	0.42	724
District	Constituency-Level Diversity	0.59	0.11	0.29	0.79	871
	Aggregated L-M-R Diversity	0.58	0.10	0.26	0.79	796
Country-Election	Prior Experience ≤ 1	0.07	0.27	0	1	16
	Prior Experience = 2	0.11	0.32	0	1	16
	Prior Experience = 3	0.15	0.36	0	1	16
	Prior Experience = 4	0.11	0.32	0	1	16
Country	Federal	0.25	0.45	0	1	12
	Cross-District Diversity	0.39	0.13	0.17	0.62	12
	Compensatory Tier	0.42	0.51	0	1	12

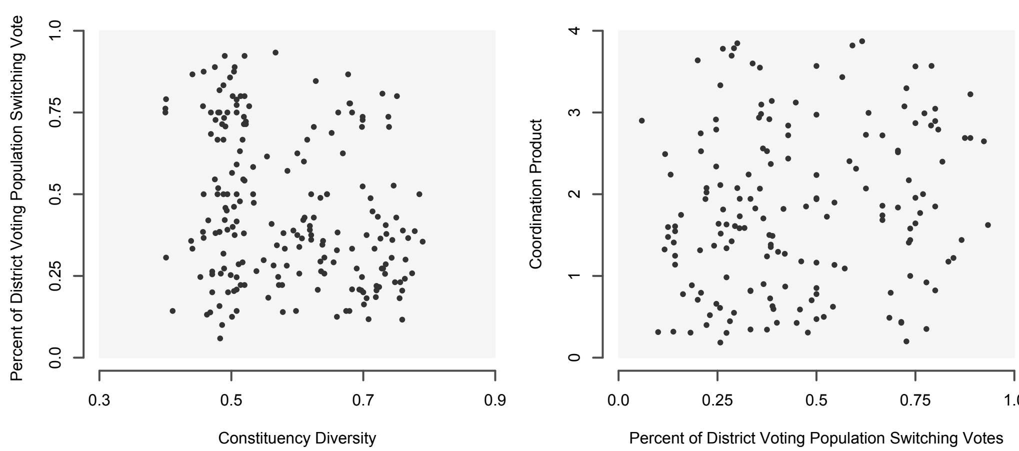
Whenever a country underwent a major electoral institutional reform, its *experience* counter was reset. These institutional changes were chronicled in the supplementary materials provided in the database by Kollman et al. (2011). Finland, Slovenia, Spain, Sweden, and Switzerland all underwent no major reforms within a four-election window of the earliest elections included in the analysis. In 1992, Austria moved to the adoption of a three-tier seat allocation system. In 2000, Croatia switched from a mixed-member to a pure proportional representation system. Ireland moved to a single transferable vote system in 43 new districts in 1992. Norway adopted a second national seat allocation tier in 1985. In 2001, Poland reduced the number of electoral constituencies and eliminated its national minimum vote threshold. In 1991, Portugal instituted a reduction in the number of parliamentary seats and electoral constituencies. Finally, in 2000 and 2004, Romania underwent reforms to its national minimum vote threshold and number of legislative seats.

APPENDIX 4: DIVERSITY AND VOTE SWITCHING

A helpful reviewer suggested the need to rule out a competing causal mechanism for the results presented in the main text: rather than constituency diversity leading to coordination failures as a result of *collective action problems*, it might instead result from voters' increasing reluctance to *switch their support to another party*. The logic behind this competing story is driven by individual-level behavioral research in comparative electoral studies from Blais (2002), Blais et al. (2011), Blais et al. (2012), and others which suggest that voters might be more reluctant to switch their support from a losing (in anticipation) to a winning (in anticipation) party in more sociodemographically diverse constituencies. This reluctance in the mind of individual voters is borne out of the fact that – in diverse environments – party elites have limited options in appealing to the electorate: either they can appeal to all voters (and risk appearing opportunistic and disingenuous) or simply settle on matching some voters' needs well and other voters' needs rather poorly. Voters, thus, might hold onto wasted votes in diverse constituencies more frequently than in homogenous constituencies because, in the former, they have a dearth of plausible options.

This competing mechanism deserves to be wrestled with on both empirical and theoretical fronts. I address the question of empirical validity first. The third module of the CSES (from which about one-third of my database is drawn) asks respondents *not only* for which party they voted in *this* election, *but also* for which party they voted in the *last* election.[6] In order to avoid dramatically shifting the level of analysis of my study (thereby bringing into the picture a number of new challenges), I opt to calculate for each district the *percentage of voters in that district who changed which party they supported* between the prior and the current election. Thus, the key variables of interest here – constituency diversity, coordination failure in the district, and the percentage of voters changing their party support – are all measured at the same level of analysis.

Figure A4.1: Constituency Diversity, Share of Survey Respondents who Changed their Party Support from the Past Election to the Current Election, and Coordination Product.



[6] I must grant, of course, that voters might misremember their balloting decisions from years prior. However, by making the assumption that at least most of them can remember correctly at least most of the time, we're able to indirectly investigate the alternative causal mechanism at hand.

If the competing causal story is accurate, we should expect to see a couple of critical relationships in the data: first, that more diverse constituencies take on *lower* percentages of voters switching their vote choice between elections; and, secondly, that lower percentages of voters switching their vote choice between election induces *higher* levels of the coordination product variable – that is, a lower willingness to switch votes leads to larger failures of coordination amongst voters around viable party labels. The left panel of Figure A4.1 above dispels the first notion. Although the two variables are loosely negatively correlated ($r = -0.24, n = 205$), this is hardly a robust relationship; indeed, many comparatively low-diversity constituencies appear to harbor a voting population that is generally *unwilling* to switch, while many comparatively high-diversity constituencies take on rather large values on the percentage of voters switching their party choice. The right panel of Figure A4.1 dispels the second notion. Whereas we would expect to see a strong *negative* correlation between the percentage of voters switching and the coordination product (that is, more willingness to abandon an unviable party leads to smaller coordination failures), we instead see a loosely *positive* correlation ($r = 0.20, n = 205$).[7] Ultimately, working with the data at hand, there appears to be no empirical support for the alternative mechanism of diversity leading to voters' unwillingness to change their support for parties.

But it is also worth discussing *why*, in theoretical terms, empirical support for this idea is absent. I would submit that the causal mechanism I advance (the collective action dilemma) is more plausible than the vote switching mechanism because the latter is predicated on a possibly erroneous assumption about elite party leaders trying to navigate diverse constituencies: if a party tries to appeal to every (diverse) voter, then it will be punished by every voter for appearing disingenuous and untrustworthy. This assumption seems to be at odds, however, with a recent literature that studies party position taking strategies. Indeed, if we think of "appealing to everyone" in spatial terms as a "broad appeal" strategy, then there is a string of recent projects that argue that parties successfully purpose the broad appeal strategy in an effort to appeal to diverse blocs of voters.

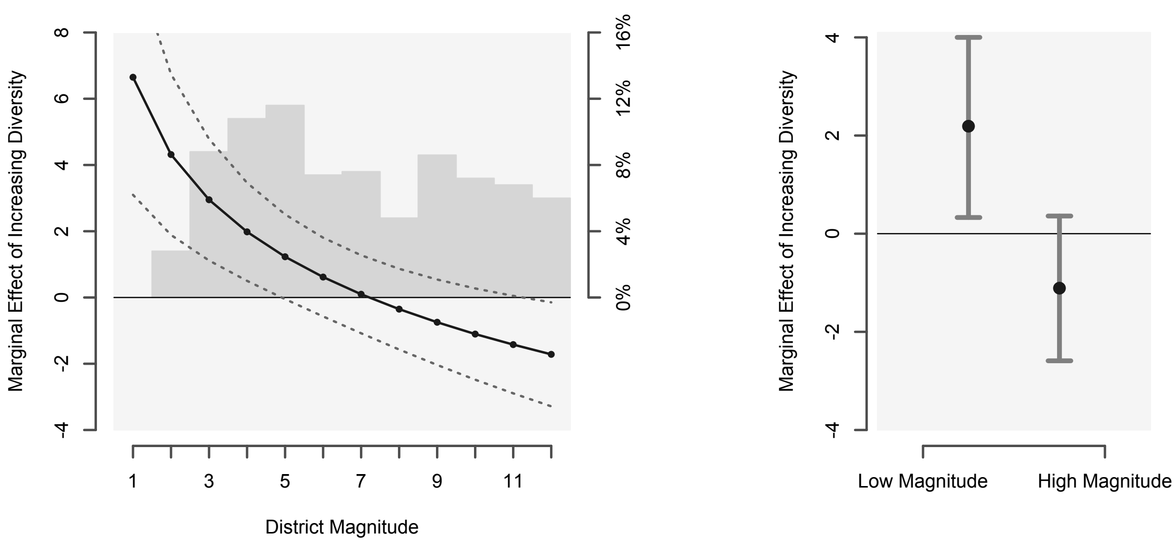
Lo, Proksch and Slapin (2014), for example, find that ideological ambiguity in party platforms – which is one way to think about party elites' "broad appeal" strategy in the face of a diverse set of voters – can "enhance the appeal of party platforms" in some circumstances (p. 1). Recent work by Somer-Topcu (2014) similarly finds that "parties gain votes when they appeal broadly" (p. 1). Finally, working within European democracies, Rovny (2012) finds that voters react to "policy blurring" strategies advanced by party elites by switching their vote choices. These cross-national results are built on the individual-level idea that voters are predisposed to cognitively prioritize those aspects of a party's platform they agree with while also de-prioritizing those aspects with which they disagree. This intuition has been empirically supported in laboratory settings (Tomz and Van Houweling, 2009) and developed under rigorous theoretical treatment through formal modeling by Glazer (1990), Aragones and Postlewaite (2002), and Callander and Wilson (2008). All of this to say that party elites might indeed *not* be so restrained in appealing broadly in diverse districts and, accordingly, voters should be no more or less reticent to switch their party vote.

[7] Additionally, substituting in the percentage of voters changing their party vote for constituency diversity in the multivariate regression (and interacting it with logged district magnitude, as in Model 4 in the main text) does not return statistically or substantively intuitive results.

APPENDIX 5: ROBUSTNESS: OMITTING M=1 DISTRICTS

To demonstrate that the presence or absence of districts where $M = 1$ is not driving the results for "low" magnitude districts, in this appendix I exclude those observations and repeat Models 1 and 2 from the main text. Below, in Figure A5.1, I redraw the marginal effects plots assessing the effect of increasing diversity on coordination failure at different district magnitudes. The results are essentially *strengthened* in the absence of $M = 1$ observations: utilizing a dichotomous measure of low-versus-high magnitude districts, we see greater separation in the 90% confidence bounds from the $x = 0$ horizontal (right pane, based on Model 2) and utilizing a logged continuous measure of magnitude, we see that diversity's propensity to drive up coordination failures is now differentiable from 0 up to and including districts where $M = 5$ (left pane, based on Model 1).

Figure A5.1: Marginal Effect of Increasing Constituency Diversity on Coordination Failure at Different District Magnitudes.



Notes: The left panel is based on a continuous measure of magnitude included in Model 1 while the right panel is based on a dichotomous measure included in Model 2 ($M \leq 5 =$ "low" and $M > 5 =$ "high"). In both panes, 90% confidence bands around estimated effects are represented either by dashed lines (left) or brackets (right). The left panel also includes a histogram of the distribution of magnitude values (percentage of distribution tracked on the righthand y-axis), minus $M = 1$ observations.

On the next page, Table A5.1 reports the full regression output for the models underlying the two panes of Figure A5.1 above. There is very little – if any departure – from the mainline models in terms of fit to the data and the performance of any individual explanatory variable in determining the level of coordination failure.

Table A5.1: Repeating Models 1 and 2 while Dropping $M = 1$ Observations.

	Model 1	**Model 2**
Constituency Diversity	6.65^{***}	-1.11
	(2.16)	(0.90)
Magnitude (Logged)	6.45^{***}	
	(1.53)	
Low Magnitude District		-2.85^{***}
		(0.86)
Diversity × Magnitude (Logged)	-7.75^{***}	
	(2.47)	
Diversity × Low Magnitude		3.30^{**}
		(1.40)
New Parties as % of Magnitude	0.77^{***}	0.76^{***}
	(0.04)	(0.04)
Federal System	-0.76^{***}	-0.89^{***}
	(0.26)	(0.25)
Cross-Constituency Diversity	2.19^{**}	1.95^{**}
	(0.95)	(0.92)
Compensatory Tier	0.47^{*}	0.30
	(0.25)	(0.24)
Constant	-5.09^{***}	1.63^{***}
	(1.44)	(0.63)
σ_{cde}	1.33	1.33
σ_{cd}	0.60	0.65
σ_{c}	0.28	0.26
AIC	3039	3052
Log Likelihood	-1509	-1515
N observations	841	841
N districts	304	304
N countries	12	12

Notes: Coordination product is the dependent variable, where more positive values indicate more coordination failure. Standard errors appear below coefficient estimates in parentheses. (*) indicates significance at the 10% level; (**) at 5% level; and (***) at 1% level.

APPENDIX 6: ROBUSTNESS: JACKKNIFED RESULTS

Model 1 from Table 2 in the text is the mainline model. As the observations in the data set are not uniformly distributed across countries, however, it is important to assess the robustness of Model 1 by "jackknifing" – or dropping each country one at a time and repeating – the regression analysis. In Table A6.1 below, I show what happens to the model's covariates of interest when I drop individual countries from the analysis (including the entire regression output of 12 separate models would be unwieldy for present purposes). Across the models, the coefficients' signs and significance levels are remarkably consistent: the signs *never* switch direction and in only one case – dropping Sweden – does the interaction coefficient lose its statistical significance. In particular, the countries providing the largest number of observations (Ireland, Romania, and Spain) do not appear to be exerting undue influence on the model and excluding the country that supplies the most "low magnitude" observations (Ireland) also has no problematic effect on the results presented in the main text.

Table A6.1: Jackknifed Results for Model 1 from the Main Text.

	Austria	**Croatia**	**Finland**	**Ireland**
Constituency Diversity	7.01***	4.60**	4.41**	4.17**
	(2.18)	(1.85)	(1.89)	(1.97)
Magnitude (Logged)	6.44***	5.23***	5.06***	4.94***
	(1.50)	(1.32)	(1.38)	(1.38)
Diversity × Magnitude (Logged)	−7.80***	−5.75***	−5.41**	−5.28**
	(2.46)	(2.15)	(2.24)	(2.25)
N Decrease when Omitted	-35	-10	-26	-81
	Norway	**Poland**	**Portugal**	**Romania**
Constituency Diversity	4.47**	4.35**	3.89**	2.31
	(1.85)	(1.94)	(1.98)	(1.82)
Magnitude (Logged)	5.37***	4.84***	4.34***	3.59***
	(1.34)	(1.41)	(1.39)	(1.39)
Diversity × Magnitude (Logged)	−5.70***	−5.16**	−4.52**	−3.67*
	(2.19)	(2.29)	(2.33)	(2.22)
N Decrease when Omitted	-57	-123	-54	-106
	Slovenia	**Spain**	**Sweden**	**Switzerland**
Constituency Diversity	4.09**	4.15**	0.86	5.12**
	(1.84)	(1.97)	(1.87)	(2.00)
Magnitude (Logged)	4.74***	4.62***	2.81**	5.40***
	(1.31)	(1.32)	(1.37)	(1.40)
Diversity × Magnitude (Logged)	−5.05**	−5.39**	−0.90	−6.23***
	(2.14)	(2.15)	(2.26)	(2.30)
N Decrease when Omitted	-24	-192	-87	-76

Notes: Standard errors appear below coefficient estimates in parentheses. (*) indicates significance at the 10% level; (**) at 5% level; and (***) at 1% level.

APPENDIX 7: ROBUSTNESS: ADDITIONAL CONTROLS

It is common in district-level studies of voter coordination and party competition to have measures of both the age of democracy and the extent to which the election in *this* period looks similar to the election in *the prior* period. Democratic age is typically included to proxy voters' and parties' level of familiarity of how electoral institutions translate votes into seats while electoral volatility tends to stand in as a proxy for the level of similarity in competitive dynamics between any two elections. For the research question at hand – and given data constraints – accounting for these two variables limits somewhat considerably the size of my data set. Furthermore, I have omitted them from the mainline analysis because the variables fall rather far afield of conventional statistical significance. Excluding these variables allows the modeling exercise to focus on those variables which more parsimoniously – and persuasively – determine the extent of coordination failure without artificially reducing the scope of the data set. In this appendix, however, I demonstrate that my results hold even controlling for these additional variables.

Volatility. I control for *lagged volatility*; that is, substantial shifts in voter support across parties between election $t-2$ and $t-1$ when analyzing coordination failure at election t (including volatility calculated between $t-1$ and t would be endogenous to the outcome at time t). Where preferences are stable, voters can rely on prior electoral outcomes to inform the current electoral outcome. Where preferences have become volatile, then voters are unable to draw from previous experience to form their expectations about contemporary viability (Cox and Shugart, 1996; Gschwend, 2007; Sartori, 1968). Powell and Tucker (2014) define two types of volatility: that pertaining to shifts in voter support among parties contesting *both* elections (Type B) and that pertaining to shifts in voter support arising from entrance by new parties or exit by old parties (Type A). Because I am including a measure of new parties in the analysis, focusing on Type A volatility would result in colinearity issues. Accordingly, I focus on Type B volatility between the elections $t-2$ and $t-1$.

Experience / Familiarity. When studying voter coordination cross-nationally, Dawisha and Deets (2006), Tavits and Annus (2006), and Reed (1990) have observed learning effects emerging from voters' *level of experience* with institutions. The logic is that, as voters live through more permutations of the vote-to-seat translation under a given set of institutions, they will be better able to anticipate which parties are viable or not. Although precise estimates vary somewhat, evidence of these learning effects has been uncovered in as many as five elections after the transition to democracy or the adoption of major electoral reform. Experience has been operationalized in several ways in prior work, but I follow Crisp, Olivella and Potter (2012) and adopt a series of dummy variables for elections where voters have experience with one or fewer prior elections, two prior elections, three prior elections, and four prior elections. This operationalization strategy should recover not only support for the experience hypothesis (if any exists), but also the rate at which experience effects drop off.

As is shown in Table A7.1 on the next page, not only are these variables' role in determining district-level coordination failure statistically indistinguishable from zero, but they also do little to unseat the main findings related to diversity, district magnitude, and their interaction. They do, however, decrease the size of the data set by about 17% relative to the models reported in the main text.

Table A7.1: Repeating Models 1 and 2 with Additional Control Variables.

	Model 1	**Model 2**
Constituency Diversity	4.79**	−0.84
	(2.01)	(1.04)
Magnitude (Logged)	5.32***	
	(1.50)	
Low Magnitude District		−2.50***
		(0.94)
Diversity × Magnitude (Logged)	−5.89**	
	(2.42)	
Diversity × Low Magnitude		2.84*
		(1.52)
New Parties as % of Magnitude	0.71***	0.68***
	(0.05)	(0.04)
Volatility$_{t-1}$	0.53	−0.19
	(1.12)	(1.10)
Experience ≤ 1	−0.33	−0.09
	(0.50)	(0.41)
Experience $= 2$	−0.07	−0.03
	(0.31)	(0.29)
Experience $= 3$	−0.08	−0.05
	(0.25)	(0.24)
Experience $= 4$	−0.29	−0.23
	(0.27)	(0.27)
Federal System	−0.80**	−1.05***
	(0.34)	(0.28)
Cross-Constituency Diversity	2.99***	2.46***
	(1.15)	(0.93)
Compensatory Tier	0.85***	0.58**
	(0.31)	(0.26)
Constant	−4.36***	1.23*
	(1.39)	(0.69)
σ_{cde}	1.27	1.28
σ_{cd}	0.74	0.80
σ_{c}	0.29	0.16
AIC	2622	2643
Log Likelihood	-1295	-1305
N observations	724	724
N districts	322	322
N countries	12	12

Notes: Coordination product is the dependent variable, where more positive values indicate more coordination failure. Standard errors appear below coefficient estimates in parentheses. (*) indicates significance at the 10% level; (**) at 5% level; and (***) at 1% level.

APPENDIX 8: ROBUSTNESS: RESPONSE RATE CUT POINTS

Despite the fact the unit of observation is the individual electoral district, the CSES does not randomly sample at the district level. In the mainline analysis, two strategies were adopted to attempt to correct for this shortcoming of the data. First, for those countries in which two or more survey waves had been implemented, respondents in the same district were pooled over time to increase the n-size of the share of voters surveyed in the district. Secondly, I dropped from the analysis those districts that fell below the 1st percentile of the overall distribution of share of voters surveyed. The general strategy here is predicated on the idea that larger samples tend to be more representative; a 1 percentile cut point therefore excludes those district where responses were probabilistically *least representative* of the actual voting population. In this appendix, I demonstrate that the main text's results (from Model 1) were not contingent upon (a) using any kind of response rate cut point at all, (b) using a more stringent cut point of the 5th percentile, and (c) using an even more stringent cut point of the 10th percentile. All coefficients remain statistically significant, similarly signed, and similarly sized.

Table A8.1: Repeating Model 1 Employing Different Response Rate Cut Points.

	No Cut Point	**>5th Percentile**	**>10th Percentile**
Constituency Diversity	4.17**	3.23*	3.38*
	(1.83)	(1.97)	(1.97)
Magnitude (Logged)	4.78***	4.24***	3.89***
	(1.31)	(1.37)	(1.36)
Diversity × Magnitude (Logged)	−5.05**	−4.25*	−4.10*
	(2.14)	(2.24)	(2.21)
New Parties as % of Magnitude	0.75***	0.77***	0.75***
	(0.04)	(0.04)	(0.04)
Federal System	−0.83***	−0.82***	−0.91***
	(0.25)	(0.24)	(0.25)
Cross-Constituency Diversity	2.20**	2.15**	2.09**
	(0.92)	(0.87)	(0.90)
Compensatory Tier	0.40*	0.37*	0.38*
	(0.24)	(0.22)	(0.23)
Constant	−3.50***	−2.86**	−2.69**
	(1.23)	(1.31)	(1.31)
σ_{cde}	1.32	1.34	1.37
σ_{cd}	0.60	0.60	0.48
σ_{c}	0.27	0.24	0.26
AIC	3160	3028	2810
Log Likelihood	-1569	-1503	-1394
N observations	879	836	779
N districts	326	315	304
N countries	12	12	12

Notes: Coordination product is the dependent variable, where more positive values indicate more coordination failure. Standard errors appear below coefficient estimates in parentheses. (*) indicates significance at the 10% level; (**) at 5% level; and (***) at 1% level.

APPENDIX 9: ROBUSTNESS: ALTERNATIVE D.V. MEASURE

Extant literature argues that the coordination product is the most well-rounded measure of coordination failure because it takes into account not only the number of wasted votes, but also the distribution of these votes across losing parties. However, prior work has also utilized other measures of coordination failure, most notably hopeless votes. As defined by Tavits and Annus (2006) and predicated in part on the $M+1$ logic articulated by Cox (1997), hopeless votes are those votes cast for the "second losing" and all smaller losing parties. These scholars have argued that, while a vote cast for the "first losing" party is wasted in the sense that it did not ultimately count toward the allocation of seats, these votes were at least coordinating on the $M+1$ party offering and, thus, they are not evidence of coordination failure per se. Table A9.1 below replicates Models 1 and 2 from the main text substituting in the number of hopeless votes for the dependent variable.[8]

Table A7.1: Repeating Models 1 and 2 with Alternative Dependent Variable.

	Model 1	**Model 2**
Constituency Diversity	75.16***	−25.85***
	(15.58)	(9.04)
Magnitude (Logged)	75.52***	
	(11.65)	
Low Magnitude District		−37.58***
		(7.34)
Diversity × Magnitude (Logged)	−101.49***	
	(18.87)	
Diversity × Low Magnitude		47.67***
		(11.90)
New Parties as % of Magnitude	1.61***	1.46***
	(0.32)	(0.31)
Federal System	9.73	−7.48
	(6.61)	(6.41)
Cross-Constituency Diversity	3.99	0.07
	(23.50)	(22.83)
Compensatory Tier	5.92	3.43
	(5.81)	(5.64)
Constant	−50.79***	28.41***
	(14.15)	(10.96)
σ_{cde}	9.10	10.11
σ_{cd}	5.67	6.19
σ_{c}	10.20	8.82
AIC	6771	6783
Log Likelihood	-3374	-3380

Notes: N countries, districts, and observations are the same as for mainline models.

[8] Because the number of hopeless votes cast in a constituency can be quite large, the outcome variable has been rescaled by dividing into 1000. Thus, for interpretation, the effect of the linear regression coefficients should be multiplied by 1000 to intuit the effect of a one-unit increase in the explanation on the outcome.

APPENDIX 10: ROBUSTNESS: ALTERNATIVE I.V. MEASURE

It might be argued that the operationalization of constituency diversity in the main text is a poor measure of the relevant coordinating groups in the electorate: that is, we might think of sub-constituency groups (such as "leftist voters") being concerned about coordinating *within their sub-group* but not necessary *at the level of the district as a whole.* After all, why would a voter of the far left see the utility in coordinating with a voter of the far right, regardless of the level of diversity? We might, it could be argued, be primarily concerned with the level of diversity within, for instance, the sub-group of leftist voters as well as the level of diversity within the sub-group of moderates, of rightists, etc. – and all of this existing under the aggregated metric of constituency-level diversity utilized in the mainline analysis.

Although it tends to be difficult to exactly specify where the "left" ends and the "center" begins on a left-right scale in a cross-national survey of multiple countries (let alone across individual electoral constituencies), I can roughly approximate such a measure based on the CSES survey response data. Due to the fact that the CSES also asks survey respondents to situate themselves on a 0 (most left) to 10 (most right) ideological scale, I can calculate a new measures of diversity aggregated to the district level that – nonetheless – leaves intact important information about diversity within these ideological sub-groups:

$$\textit{aggregated L-M-R diversity} \quad = \quad \big(v_L(1-\alpha_L)\big) + \big(v_M(1-\alpha_M)\big) + \big(v_R(1-\alpha_R)\big)$$

In essence, this new *aggregated L-M-R diversity* measure first sections off voters identifying themselves as either left, middle, or right; calculates Krippendorff's alpha separately amongst each of these subsets of voters; and then constructs a weighted sum of these three diversity scores with v_L, v_M, and v_R indicating the share of voters that fell into each of these three areas. With the intent of evenly dividing voters into the three ideological camps, those responding 0-4 were coded as "left" (30.5%), those responding 5 were coded as "middle" (29.4%) and those responding 6-10 were coded as "right" (40.2%).[9] Across the electoral districts included in the data set, the mean level of diversity within the left, middle, and right constituencies all tended toward 0.6 on a 0-to-1 scale, although maximum values of diversity on the left tended to be higher than those on the right.[10]

Despite the additional nuance applied in the construction of this alternative measure of the independent variable, however, it remains exceedingly highly correlated with the diversity variable utilized in the main text; indeed, the two are correlated with one another at $r = 0.95$ and substituting this version into the mainline analysis does not substantively undermine the model's results. This is most likely due to the fact that – on average – each sub-group tends to have similar levels of diversity.

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[9] This coding is unfortunately coarse. However, an alternative coding of voters into left (0-3), middle (4-6), and right (7-10) constituencies results in values that are highly correlated with this coding scheme. The drawback of this latter approach is that it slots most voters into the middle category.

[10] There are other reasons to be wary of this operationalization, as it implies that the district-level sample size is not only enough to gain some traction on the sociodemographic composition of *the district*, but now also *each individual subgroup within that district.* I thank two anonymous reviewers for raising these concerns and defer to their judgment that the constituency-level measure is about the best we can hope for given the data limitations and questionable operationalization decisions inherent in aggregating up from sub-groups.

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