2021

Appendix to Chapter 22 of the Oxford Handbook of Political Participation

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Supplementary material for Chapter 22 in Giugni and Grasso (2022)

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Appendix A: Maintaining equilibrium levels of party choice and turnout, using IMD data

In the chapter I co-authored with Georg Lutz on partisanship in the process of party choice (Franklin and Lutz 2020) we demonstrated the way in which variations in policy congruence between voters and parties govern fluctuations in party support around a central tendency established by partisanship, consistent with classic theorizing (Campbell et al. 1960). In this appendix, I revisit and build on those findings to suggest a linkage between party support and turnout such that voter-party proximity maintains not only an equilibrium level for party choice but for turnout as well.

Operationalizing the discussion surrounding Figure 1 in the main text, two equations govern the equilibrium level of party support. These equations define a balance of forces that push party support in opposite directions. Here I propose that a corresponding equilibrium level of turnout is governed by a third equation (an equation that I will divide between two models for analysis purposes), having terms taken from the first two (all three equations will be explained in coming paragraphs).[1]

First, voters give increased support to parties that become closer to them in policy terms:

$$\Delta \text{Support}_{t} = \text{Support}_{t-1} + \Delta \text{Proximity}_{t} + \text{Proximity}_{t-1} \qquad \text{(1: party support)}$$

Second, parties adjust their policy offerings in reaction to any loss of support:

$$\Delta \text{Proximity}_{t} = \text{Proximity}_{t-1} - \Delta \text{Support}_{t-1} - \text{Support}_{t-2} \qquad \text{(2: feedback)}$$

Third, turnout (a multi-party view of party support) reflects the first two processes:

$$\Delta \text{Turnout}_{t} = \text{Turnout}_{t-1} - \Delta \text{Proximity}_{t} - \text{Proximity}_{t-1} + \Delta \text{Proximity}_{t-1} + \text{Proximity}_{t-2} \qquad \text{(3: turnout)}$$

The functioning of the different terms in these equations will be described in light of the findings they give rise to. In Table A1, the first two models (A and B) use data that aggregates, to the party

[1] These models take the form of so-called Error Correction Models (ECMs detailed in Jennings 2013). Such models come with methodological concerns (De Boef and Keele 2008) discussed in Appendix C.

level, the proportions of votes received by each party and similarly recasts proximity as a party attribute.[2] Models C and D instead focus on proportions voting among birthyear-cohorts, treating as a single unit all respondents born in the same year in a particular country. Aggregation is a straightforward way to obtain time-series cross-section data from individual-level survey data; and these two aggregations (to the party and birthyear cohort levels) also makes sense substantively – the party level for obvious reasons and the birthyear level because my individual-level theorizing focuses not on the behavior of individuals but on the behavior of cohorts of respondents who enter their electorates together at the time of specific elections.[3] All the models in this table contain coefficients that focus on how past values of inputs (independent variables) affect current outcome (dependent variable) values; but it is important to notice how the lag structure for these effects differs between models. In Models A and C the focus is on effects coming from the previous election (at t-1) whereas in Models B and D the focus is rather on effects coming from the election before that (at t-2).

In Model A the outcome (dependent variable) is change in party support between the current and previous time-points – referred to in this research tradition as a "differenced outcome" and signalled

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[2] Franklin and Lutz (2020) used as their data source the Integrated Module Dataset (IMD) from the Comparative Study of Electoral Systems (CSES 2018) and for this appemdix I use the same source. Doing so adds election studies conducted after 1995 in Brazil, Czechia, Greece, Ireland, Korea, Latvia, Mexico, Peru, Poland, Portugal, Solvenia and Spain to the countries that are the main focus of the chapter (Italy and the Netherlands were dropped because some election studies were missing from the IMD, producing estimation problems). Several of these countries did not conduct free and fair elections continuously since 1945, as implied in the main text. But at the party level we do not need continuity in generational cohorts. The number of parties available for analysis is a more pressing concern. This dataset differs from the one used for my work with Georg Lutz in containing additional surveys that became available only in December 2020 with Release 2 of the IMD. This increases the number of parties available for analysis but hardly affects the findings.

[3] Birthyear cohorts, of course, fit within electoral cohorts while giving us aggregate units of roughly the same sizes (but see Appendix C regarding the weighting of those units). Had we aggregated to the electoral cohort level these would have had very different sizes depending on the time elapsed between elections (inter-election periods can sometimes be quite short, producing rather small incoming cohorts at the next election).

Table A1 Party and birth-year level (fixed effects) analyses of party support and turnout (IMD data)

Level of analysis:	Party-level analyses		Birthyear cohort analyses	
	Model A	Model B	Model C	Model D
Concept:	Representation (party support)	Feedback (policy adjustment)	Negative concomitant †	Feedback (correction)
Outcome:	Voted for party	Proximity to party	Turnout	Turnout
Input:	Coef. (s.e.)	Coef. (s.e.)	Coef. (s.e.)	Coef. (s.e.)
1) Lagged outcome	-1.13 (0.07)	-1.32 (0.09)	-1.36 (0.02)	-1.46 (0.03)
2) **Δ**.left-right proximity $_{t}$	0.46 (0.10)		-0.23 (0.06)	
3) Left-right proximity $_{t-1}$	0.72 (0.15)		-0.30 (0.10)	
4) **Δ**.voted for party $_{t-1}$		-0.09 (0.07) n.s.		
5) Voted for party $_{t-2}$		-0.34 (0.11)		
6) **Δ**.left-right proximity $_{t-1}$				0.28 (0.10)
7) Left-right proximity $_{t-2}$				0.43 (0.16)
Intercept	-0.40 (0.10)	0.98 (0.06)	1.27 (0.07)	0.82 (0.12)
R-squared	0.64	0.75	0.66	0.73
Observations	364	184	4,363	2,387
Number of party-elections	174	103		
Number of country-birthyears			2,006	1,324

Note: All coefficients significant at $p<0.001$, one-tailed unless marked n.s. (not significant).

† Model C is viewed as a concomitant of Model A (see accompanying text).

by use, as prefix, of the Greek letter delta (Δ). The first input is a lagged but not differenced version of the same outcome variable. Known as the "error correction parameter" (ECP), its value reflects how long it takes for deviations from an equilibrium outcome to decay. An ECP of -1.0 would correspond to an equilibrium that was restored over the course of a single time-point (in these data, the 4-year average gap between elections). Models in Table A1 all have ECP values within 0.5 of -1.0, suggesting that equilibrium would be restored within the final two years of each inter-election period (or, in other words, that short-term deviations from equilibrium are ephemeral).

The other inputs to each model come in pairs: differenced inputs being paired with lagged inputs. For each pair, the differenced input tells us the short-term effect of the variable in question – the effect that will dissipate over the period governed by the ECM – whereas the lagged input tells us the long-term effect. So, in Model A, the short-term effect (0.46 in Row 2) will have dissipated by the time the next election is due, leaving an effect of 0.72 (Row 3) to carry over into the longer term.[4]

Of course, I do not suppose that survey respondents necessarily see this process in left-right terms, but the way in which voters' left-right positions react to parties' left-right policy stances evidently involve (and apparently provide good stand-ins for) voters' cognitive processes, whatever those may be. The results tell us (unsurprisingly) that voters reward their parties (with additional votes) as proximity improves and punish them (by withdrawing electoral support) as proximity declines.

Model B changes the focus from support to proximity, assessing to what extent parties respond to changes in voter support by adjusting their policy stances. Here we expect negative feedback (Deutsch 1963; Easton 1965; Franklin, Soroka and Wlezien 2014), with parties responding to reduced support by trying to improve the fit of their policies to voter preferences and responding to increased support by permitting those policies to stray from voter preferences (cf. Harkvardien 2010).[5] However, we do not expect this feedback to be immediate. It takes time for parties to alter their policy stances and yet more time for them to communicate those changes to voters. So in Model B we look for effects from party support at the election before last, with a short-term (differenced) measure from t-1 (Row 4) and a long-term (lagged) measure from t-2 (Row 5). The short-term effect is small and not statistically significant, but the long-term effect (-0.34 in Row 5) is of substantial magnitude, within sampling error (2-sigma confidence interval) of matching the corresponding

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[4] Initially the short- and long-term effects are felt in conjunction, but only the long-term effect persists. That effect can also move the series to a new equilibrium (see discussion regarding the chapter's Figure 1).

[5] Actually, we cannot tell from these data whether parties move closer to their voters by changing their stances or whether they succeed in persuading their voters to accept the positions they (the parties) have taken. In other work (Franklin 2015, 2017) I have shown that about half the movement is in party stances, with the remainder being produced either by parties' persuasive success or by assimilation for voters who "persuade themselves" that they stand with their parties.

long-term effect in the opposite direction (+0.72), seen Model A's Row 3.

In Models C and D we shift our attention to turnout. Here the outcome is the proportion casting a ballot among different cohorts of respondents. Again the pair of models show effects with different signs at different lags, but for these two models the outcome (dependent) variable is the same. And, importantly, we see negative effects in the first model of the pair being corrected by positive effects two elections later, providing support for the theorizing expounded in the main text.

It may be surprising to see that turnout is lower when parties receive higher support, but bear in mind that turnout responds to electoral competition (Franklin 2004). Parties might well receive maximum support at precisely the point when competition is lowest, and *vice versa*. There is no necessary delay inherent in decisions whether to vote or not.[6] So the delay we see following the negative effect (in Model C) before its reversal (in Model D) is telling. It corresponds exactly with the time delay underlying the transformation of maliable younger individuals into older members of the electorate with established habits of voting (or not) – a delay that was the focus of the correspon-ding individual-level analysis presented in the main text's Table 22.1. What we see are short-term individual-level turnout variations apparently responding to competitive variations at the party level – turnout responses that are subject to later corrections due to generational replacement.

But these findings must be treated as tentative. The connection between high proximity and low levels of electoral competition is still speculative; and the use of error-correction models in cross-sectional time-series as short as those used here may presents problem, as suggested in Appendix C.

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[6] In the main text I argued that younger, more maleable, voters can respond immediately to novel features of an electoral situation while for older voters, already set in habits of voting or non voting, change can come only with generational replacement. In Models C and D we see both types of change occurring: change due to the maleability of yourger voters in Model C and change due to generational replacement in Model D. The distinction makes it clear that individual-level short-term effects can have what, in ECM terminology, are seen as long-term effects. As stressed in the main text, these concepts (while broadly consistent across modeling strategies) are not the same and can overlap.

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Appendix B: Supplementary findings and robustness checks using the 14-country dataset

As explained in the main text, I chose to use linear probability models (OLS) for the analyses reported in the chapter because interactions are central to those analyses and first differences from logit results are undefined for interaction terms (Ai and Norton 2003; Buis 2010). The raw logit results are exceedingly hard to interpret, and would have made impossible the sorts of statements about coefficient sizes that are called for in the main text. In Table B1 I show logit results for coefficients in the main text's Table 22.1, Model D, that are not interaction terms, demonstrating that the findings from a logit analysis would be substantively the same, if different in detail. Note that comparisons are made difficult by the fact that, since interaction effects are undefined, colinearity

Table B1 Comparison between OLS findings and Logit findings for Table 22.1, Model D

Outcome: Voted Inputs:	Logit coefficients and first differences (Robust Coef. Std. Err.)	 dy/dx (Std.Err.)	Reference results from main text Coef. (Std. Err.)
Quasi-lagged outcome	1.41 (0.01)	0.19 (0.00)	0.25 (0.00)
<u>Long-term covariates</u>			
Partisanship	1.42 (0.03)	0.19 (0.00)	0.15 (0.00)
Proximity to party	0.08 (0.05) n.s.	0.01 (0.01) n.s.	0.01 (0.01) n.s.
Newly adult recently	-0.21 (0.08)	-0.03 (0.00)	-0.08 (0.00)
<u>Demographic and knowledge covariates</u>			
Age	0.00 (0.00) n.s.	0.00 (0.00) n.s.	0.05 (0.03) n.s.
Age squared	0.00 (0.00) n.s.	0.00 (0.00) n.s.	-0.05 (0.03) n.s.
Education	0.32 (0.01)	0.04 (0.00)	0.03 (0.00)
Relition	0.15 (0.04)	0.04 (0.00)	0.08 (0.00)
Marital status	0.82 (0.02)	0.08 (0.00)	0.11 (0.00)
Income	0.11 (0.01)	0.02 (0.00)	0.02 (0.00)
Knowledge	0.71 (0.04)	0.11 (0.00)	0.07 (0.00)
Union membership	0.23 (0.03)	0.03 (0.00)	0.00 (0.00) n.s.
Interactions with newly adult	YES	YES	YES
Fixed effects	YES	YES	YES

Note: Fixed effects. All coefficients significant at p<0.001 except where marked n.s. (not significant).

effects for interactions are also undefined. Thus, even though the logit model contained all the same terms as the linear probability model in the main text, there may be differences in coefficient values even for constitutive terms, due to model misspecification in the Logit model that go beyond differences due simply to use of a logit transformation for that model's dependent variable.

Tables 22.1 and 22.2 in the main text are incomplete because interactions with the variable 'New at recent election' were not displayed for reasons of space. In Table 22.2 most of the effects for demographic constitutive terms were also omitted for the same reason. Table B2 shows the missing coefficients for Tables 22.1, (Model D) and 22.2 (Model C). These are the age-related inter-

Table B2 Coefficients missing from Tables 22.1 and 22.2 in the main text

Constitutive term / interaction with	Table 22.1, Model D, interactions with recently adult	Table 22.2, Model C constitutive terms	Table 22.2, Model C, interactions with recently adult
Age	-0.00 (0.01)		-0.00 (0.00)
Age-squared	0.00 (0.00)		0.00 (0.00)
Education	0.02 (0.00)	0.02 (0.00)	0.03 (0.00)
Religion	0.05 (0.01)	0.01 (0.00)	0.06 (0.01)
Marital status	-0.05 (0.00)	0.12 (0.00)	-0.06 (0.00)
Income	0.01 (0.00)	0.01 (0.00)	0.01 (0.00)
Knowledge	0.10 (0.01)	0.07 (0.01)	0.07 (0.01)
Union membership	0.02 (0.00)	0.01 (0.00)	0.02 (0.00)

Note: Fixes effects OLS. All coefficients significant at $p< 0.001$.

actions (referred to as such in the main text) that help to render age itself insignificant by specifying at least some of the ways in which start-up effects arise (a topic for future research).

In the main text I raised the question how many elections does it take for a voter to become set in his or her ways and asserted that it makes little difference to the findings whether the cut-off point was taken at two elections or three. Table B3 demonstrates the small differences made by choice of cut-off using a subset of the variables employed in Table 22.2, Model C in the main text. Differences are only of substantive importance for one variable: party cohesiveness (party discipline).

Table B3 Demonstration effects of the indicator 'Newly adult at recent election' with different codings, using the 14-country dataset

Outcome: Voted Inputs:	Newly adult after 2 elections Coefficient (s.e.)	Newly adult after 3 elections Coefficient (s.e.)
Quasi-lagged outcome	0.278 (0.002)	0.278 (0.002)
Long-term effects		
Partisan strength	0.156 (0.004)	0.156 (0.004)
Proximity to party	0.021 (0.005)	0.020 (0.005)
Clarity of election	-0.029 (0.006)	-0.038 (0.006)
Marginality	0.013 (0.005) n.s.	0.012 (0.005) n.s.
Time-gap from previous election	0.013 (0.007) n.s.	0.006 (0.008) n.s.
Cohesive party system	0.007 (0.005) n.s.	0.024 (0.005)
Newly adult	-0.089 (0.015)	-0.092 (0.015)
Short-term interactions		
New * partisanship	0.036 (0.006)	0.035 (0.006)
New * proximity	-0.056 (0.008)	-0.050 (0.008)
New * clarity	0.110 (0.009)	0.127 (0.009)
New * marginality	0.047 (0.008)	0.054 (0.008)
New * time-gap	-0.078 (0.014)	-0.059 (0.013)
New * cohesive	0.029 (0.006)	0.002 (0.006) n.s.
Fixed effects	YES	YES
Intercept	0.406 (0.010)	0.389 (0.010)
R-squared	0.218	0.217
Observations	286,654	286,654

Note: Fixed effects OLS. All coefficients significant at p<0.001 except where marked n.s. (not significant).

In the main text (and analyses reported in this appendix) I weighted the data to official turnout, as reported by the International IDEA Voter Turnout project. However, the findings are only marginally affected by whether weights are used or not, as shown in Table B4 that uses the same demonstration variable subset used for Table B3. The only substantively meaningful difference is for long-term time-gap since the previous election which, with unweighted data, gains a significant effect in the wrong direction. Only four other pairs of coefficients show differences larger than can be accommodated by the third digit, and none of these differences are substantively telling.

Table B4 Demonstration effects of findings with and without weighting to official turnout (14-country data)

Outcome: Voted	Findings with unweighted data		Findings with weighted data	
Inputs:	Coefficient	(s.e.)	Coefficient	(s.e.)
Quasi-lagged outcome	0.219	(0.002)	0.278	(0.002)
Long-term effects				
Partisan strength	0.100	(0.004)	0.156	(0.004)
Proximity to party	0.022	(0.005)	0.020	(0.005)
Clarity of election	-0.033	(0.006)	-0.038	(0.006)
Marginality	0.006	(0.005) n.s.	0.012	(0.005)
Time-gap from previous election	-0.015	(0.005)	0.006	(0.008) n.s.
Cohesive party system	0.052	(0.003)	0.024	(0.005)
Newly adult	-0.106	(0.010)	-0.092	(0.015)
Short-term interactions				
New * partisanship	0.032	(0.004)	0.035	(0.006)
New * proximity	-0.049	(0.006)	-0.050	(0.008)
New * clarity	0.117	(0.005)	0.127	(0.009)
New * marginality	0.051	(0.006)	0.054	(0.008)
New * time-gap	-0.020	(0.008) n.s	-0.059	(0.013)
New * cohesive	0.010	(0.004) n.s.	0.002	(0.006) n.s.
Fixed effects	YES		YES	
Intercept	0.507	(0.010)	0.389	(0.010)
R-squared	0.169		0.217	
Observations	286,654		286,654	

Note: Fixed effects OLS. All coefficients significant at p<0.001 except where marked n.s. (not significant).

A note on respondent perceptions of party positions

A final topic that should be addressed in this appendix is the accuracy of voter perceptions of party positions in left-right terms. In the chapter I wrote with Georg Lutz we positioned parties on the basis of expert judgements. In this replication of the Lutz analysis I preferred to use voter judgements because those are theoretically preferable (and, incidentally, performed better). I should note, however, that the judgements that performed better were judgements on the part of voters who placed themselves differently than they placed the parties they voted for. The ranking of the three measures of party location in terms of the performance of resulting models were (1) respondent rankings of parties, omitting the party they reported voted for, (2) rankings by experts recruited by the PIs of the respective election studies and (3) respondent rankings of parties, including the party each respondent reported voting for.

This ranking is not surprising given that the main criticism of respondent-judged party positions is that respondents would tend to be biassed by seeing themselves as closer to the party they voted for than was actually the case. By excluding respondents who might have been biassed in this fashion I obtained what appeared to be better judgeents even than were obtained from so-called “experts.”

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Appendix C: Methodological concerns regarding error correction models as used in Appendix A

Appendix A employed error correction models that describe the way in which equilibrium levels are maintained in support for political parties (building on work reported in Franklin and Lutz 2020) and in turnout. I suggest that turnout equilibriation is a by-product of processes yielding equilibrium party support. Linkages between the two processes involve well-established relationships between electoral outcomes, party competition and turnout. An appropriate statistical approach to representing such relationships, and the resulting dynamics of turnout and party support, is to use error correction model (ECMs). This is not my idea. ECMs have been used previously for modeling linkages between voter preferences and government policy stances (for a survey see Jennings 2013). My innovation is to use the same means for studying party policy stances.

ECMs have the great advantage of being extremely flexible, making no assumptions about the nature of dynamic processes under study – provided the error correction parameter (described in Appendix A) is negative and statistically significant (Kennedy 2008). Instead, as in other auto-regressive distributed lag (ADL) models, of which the ECM is a variant, the dynamic processes can be discovered empirically by trying out different lags and discarding those that prove not significant. In this research I was not able to go very far in evaluating different possibilities since the short series of time-points available to me in the IMD would not let me try out lags beyond the third. However, the third lag was never statistically significant, suggesting that later lags would not be either.

ECMs, used in this way, are not subject to problems that beset many other approaches to time-series modeling (problems of non-stationarity, unit roots, and the like) for reasons expounded in De Boef and Keele (2008). However, I have been unable to find any examples in the statistical, econometric or political science literature of ECMs employing data with as small a number of time-points as are available to me here. But neither have I found any cautions against using datasets such as the IMD that get their power in statistical analysis from the number of panels rather than from the number of time-points.[7] Still, I focused the main text of this chapter on analyses that use more conventional methods for spelling out the implications of my insights.

A note on weighting

Although the statistical demands of ECM analysis seem unproblematic for my purposes, the data do have to be aggregated to the party level for Models A and B of Table A1 and to the birthyear cohort level for Models C and D. Since we start with survey data that itself is weighted, and which has different numbers of respondents in different birthyear cohorts, the question how to weight the survey data for each of these aggregation exercises is an important one.

For the party-level analyses I weighted by the same weight used at the individual level: the weight that produces proportions voting that match official turnout figures. This should also give us close to accurate data for the support given to each party, and presumably also for the left-right location of each party. The actual number of respondents associated with each party is irrelevant because each party is a separate unit irrespective of now many supporters it has.

Things are different when it comes to birthyear cohorts. Again these need to be weighted to the average turnout found empirically, but they also need to be weighted according to the number of respondents in each cohort. So, for analyses in Models C and D of Table A1 the aggregation process is doubly weighted, by the size of each cohort as well as the turnout rate.

[7] It was not possible for me to attempt a replication of Appendix A findings using my 14-country dataset because that dataset does not have standardized codings of political party IDs, as are provided for the IMD. So I could not aggregate my 14-country dataset to the party level of analysis. See Appendix D footnote 8 for a circumstance where IMD data appears unsuited to ECM analysis, for lack of a sufficiently lengthy time-series.

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Appendix D: Replication of Table 22.2's findings using ECMs with the 14-country dataset

Given the importance of negative feedback in the theorizing that underlies Chapter 22 – theorizing strongly supported by findings from error correction models that are the focus of Appendix A – readers may wonder why I do not also use error correction models (ECMs) in the main text. The reason is that error correction models are suited to studying a different set of concerns than the models in the main text. ECMs evaluate a series of observations (often labeled "realizations") in terms of the statistical requirements for those observations to be held in a state of dynamic equilibrium through the operations of so-called "long-term" and "short-term" effects. The long-term and short-term forces that are the subject of the chapter itself are tied to a quite different sort of theory regaring the dynamic properties of a sequence of observations – a social science theory about how human beings acquire certain patterns of behavior. There is no requirement that the resulting series of observations be in any sort of equilibrium. However, it is sometimes mentioned that the progression of turnout realizations occurring in practice do appear to follow a pattern suggesting not only conformity with a social science theory but also conformity with the existence of a statistical equilibrium. Appendix A addressed this conjecture by spelling out and confirming (so far as such things can be confirmed) the nature of the supposed equilibrium. The chapter itself tries (*inter alia*) to spell out the observable implications of such an equilibrium for human behavior, as studied with conventional methods.

Nevertheless, it can be instructive to discover the extent to which the coefficients we get from a model based on social science theory match those we get from a model based on statistical theory. Since both theories talk about long-term versus short-term effects there should be at least *some* points of comparison if only to validate our use of the same labels in both contexts. Table D1 shows the coefficients resulting from an error correction analysis of turnout at the birthyear cohort level and juxtaposes these coefficients with coefficients to which the same labels ("long-term" versus "short-term") are applied, taken from Table 22.2 in the main text but re-arranged to suit the format I have employed for the display of ECM coefficients.[8]

Perhaps surprisingly, the ECM version of these findings (Model A) provides a generally clearer picture regarding long-term coefficients, matching more closely the theorizing and findings in Franklin (2004) that were confirmed by later research (especially Johnston et al. 2007); Bruter and Harrison 2009). For effects theorized to be long-term, the short-term effects in that model are all non-signficant (at the slightly lower significance level chosen for this table), quite in contrast to the rather muddy findings seen in Model B. But Model A's findings also include a bigger anomaly than

[8] The data for Model A in Table D1 come from the same source as the data used for Model B: my 14-country dataset, used for analyses in the main text of this chapter. Those data have a much longer time-span than the IMD data, going back to the 1960s for some countries. A separate analysis of the same model using IMD data (not shown) brings to light a problem that can apparently result from using short time-panels: no coefficients are significant. This might perhaps be due to a relatively low error correction parameter (lagged depvar), also seen in Model A. Speculatively, slower error correction might call for longer time-series.

any other in Table D1: no significant effect of eligiblity to vote at 18, either long-term or short-term.

Table D1 Comparing ECM findings with original findings for Table 22.2, Model C

Outcome: turnout Inputs:	Model A ECM version of Table 22.2, Model C Coef.	(s.e.)	Model B Restructured Table 22.2, Model C Coef.	(s.e.)
(Quasi-) lagged outcome	-0.82	(0.01)	0.26	(0.00
Newly theorized variables				
Δ.(short-term) partisanship	0.22	(0.02)	0.18	
L.(long-term) partisanship	0.12	(0.02)	0.16	(0.00)
Δ.(short-term) proximity	-0.10	(0.03)	0.02	
L(long-term) proximity	-0.19	(0.03)	0.00	(0.01) n.s.
Variables derived from past theorizing				
Δ.(short-term) electoral clarity	-0.01	(0.01) n.s.	0.10	
L.(long-term) electoral clarity	0.01	(0.01) n.s.	-0.05	(0.01) n.s.
Δ.(short-term)marginality	0.05	(0.01)	0.07	
L.(long-term) marginality	0.04	(0.01)	0.01	(0.01) n.s.
Δ.(short-term) time-gap	-0.03	(0.01)	-0.07	
L.(long-term) time-gap	-0.06	(0.02)	0.05	(0.01)
Δ.(short-term) cohesive	-0.00	(0.01) n.s.	0.02	
L.(long-term) cohesive	0.03	(0.01)	0.02	(0.01)
Δ.(short-term) compulsory voting	0.04	(0.02) n.s.	0.22	
L.(long-term) compulsory voting	0.06	(0.01)	0.12	(0.01)
Δ.(short-term) electorate size	-0.08	(0.19) n.s.	0.08	n.s.
L.(long-term) electorate size	-0.07	(0.03)	0.08	(0.03)
Δ.(short-term) legislative responsiveness	0.00	(0.00) n.s.	0.11	
L (long-term) legislative responsiveness	0.13	(0.03)	0.06	(0.02)
Δ.(short-term) eligible to vote at 18	-0.01	(0.01) n.s.	-0.02	
L.(long-term) eligible to vote at 18	0.00	(0.01) n.s.	-0.07	(0.00)
Recently adult	-0.04	(0.01)	-0.14	(0.02)
Fixed effects	YES		YES	
Effects of demographics and knowledge	NO		YES	
Intercept	0.65	(0.04)	0.10	(0.02)
R-squared	0.50		0.25	
Observations	6,463		261,879	
Birthyear-cohorts / countries	1,130		14	

Note: All coefficients significant at $p<0.01$ except where marked n.s. (not significant). Short-term effects for Model B sum the short-term interactions and the long-term effects shown in Table 22.2, Model D (standard errors cannot be computed for these summations). Demographics are not relevant to Model A and so are omitted from that model.

By contrast, for votes at 18 as well as for effects theorized to be short-term, the picture is clearer in Model B. For effects theorized to be primarily short-term (clarity and marginality) long-term coefficients fail to reach statistical significance in this model, as theorized, whereas the long-term coefficient for votes at 18 shows the dominance expected from past theorizing. So, although lacking the crispness seen for most of the long-term findings in Model A, Model B coefficients (derived from this chapter's main text) more closely align with past findings.

Turning to variables (partisanship and proximity) introduced to support this chapter's theorizing, the ECM coefficients in Model A give a much better sense of their overall importance, as was only to be expected. To repeat: a standard regression model is suited for representing the outcomes of forces with origins in human behavior; an ECM can only show whether those operations result in an equilibrium outcome. There is no reason why forces mainly responsible for that equilibrium should mirror the pattern of forces seen in the original regression model.

Bibliography for Appendix D

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