

Public Opinion and EU Policy: How Measurement Matters

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Abstract

Do EU policy-makers adjust policy outputs to follow public opinion and, if so, do they adjust total legislative output or just the output of important legislation? Does this adjustment respond to meaningful signals regarding public reactions to the volume of legislation that is being produced or are policy-makers essentially taking account of meaningless opinion fluctuations? Researchers have made differing assumptions about what EU policy-makers would adjust if they followed public opinion and about what EU publics would notice if they noticed the policy productions of Brussels. In this Research Note we review past assumptions made in this field and assess the implications of different assumptions for our understanding of representation processes in the EU. We find strong and robust evidence of thermostatic reaction in public opinion to the cumulation of important European policy over time. We also find strong evidence of policy responsiveness to public opinion, mostly important directives. While patterns of public and policy responsiveness may not be perfectly symmetrical, it nevertheless appears that there is less of a democratic deficit in European politics than many observers claim.

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In a recent article Dimiter Toshkov (2011) discovers what he calls a ‘lost relationship’ between public opinion and the legislative output of the European Union (EU, previously EC). From 1973 until the middle 1990s EU legislative output appeared to follow the ups and downs in public support for the European Project, suggesting that EU policy-makers took cues from public opinion when deciding the extent (or at least the timing) of unification policies.¹ This relationship ceased to be evident after 1994, however.

At the same time, Toshkov (2011) finds no clear evidence of public responsiveness regarding the extent of unification legislation. Specifically, he finds a positive effect but reports that it is not robust to changes in model specification. Much theoretical work in political science, going back to Easton (1965) and Deutsch (1966), has argued that policy-making activities that are noticed by the public should generate not positive but negative responses. This insight, framed in terms of public reactions to policy-making, is summarized by Wlezien (1995) in his article that introduced and tested the thermostatic model (as quoted by Toshkov 2011: 174): ‘In effect the public will behave like a thermostat, where a departure from the favored policy temperature produces a signal to adjust policy accordingly, and once sufficiently adjusted, the signal stops.’ Feedback of policy on opinion should be negative because a public will eventually respond to unabating legislative activity in some policy area by demanding less (Soroka and Wlezien, 2010).² This insight has provided a basis for advances in political science theory and methodology (also see Jennings, 2013), focusing on how negative feedback provides guidance to the policy-making process.

A null finding in regard to feedback suggests that EU publics are unaware of EU policy-

making, which might come as no surprise. The EU has no press corps; the evening news seldom reports new EU initiatives (cf. Trenz, 2004; Weiler, 1999: 349–52). However, this finding contrasts with what Franklin and Wlezien (1997) found in their earlier study of the same relationship – a *strong and negative* effect of legislative output on public support for unification policies. The difference in findings is important because it implies very different things about public opinion – that it either is or is not informed by policy-making activities, which has implications for the nature of EU representation processes, as will be explained.

Why the difference in findings? Our answer is that Toshkov's analysis itself differs from Franklin and Wlezien's. Indeed, it differs in three important ways: (1) the measure of public support for policy-making; (2) the measure of legislative output; and (3) the statistical modeling approach. Differences in statistical modeling do not turn out to yield different results (see the online appendix). Instead, operationalization and measurement differences of both opinion and policy are critical for explaining the differences in findings. We begin by addressing the research designs of the two studies in more detail.

What do EU publics notice? What do EU policy-makers react to?

It is not widely recognized that it makes little sense for policy-makers to take account of a public desire for more or less legislation unless the public responds to policy-making activities. Without such responsiveness, the public shows itself to be uninformed about what policy-makers do, and a public whose relative preferences are uninformed by policy awareness cannot send meaningful signals to policy-makers. Discovering whether the public notices policy-making activity is thus of primary importance since any policy adjustments in

light of opinion change would otherwise be reacting to essentially random data.

One indicator of public support for policy-making is provided by responses to the Eurobarometer question ‘Is membership by your country in the EC [later EU] a … good thing?’ – a question that was asked regularly from late 1973 until 2011. This measure was employed by Toshkov (2011), as is common practice, though he was not very clear about what responses to the question actually tap. They might indicate absolute preferences, for example, whether and to what extent Europe should be unified or not. Or they might measure relative preferences, for example, that unification should go further or has gone too far. Franklin and Wlezien’s (1997) findings suggested that the ‘membership’ question indicates absolute preferences, giving little reason to expect answers to that question to respond thermostatically to EU policy. It is also possible, since it yielded the ‘lost relationship’, that the membership question does not tap what EU policy-makers represent when they respond – if they do – to public opinion.

A new – arguably more suitable – pair of questions were introduced into the Eurobarometer in 1986, the first of which asks how fast European unification is currently advancing. We label this ‘perceived speed’. The second question (wording in the online appendix) asked respondents at what speed they would like unification to advance. We label this ‘preferred speed’. When perceived speed is subtracted from preferred speed, the resulting ‘relative speed’ indicates how much faster (or slower) respondents want to see unification progressing than currently perceived. A relative indicator is what we expect theoretically to respond to changes in unification policy, but preferred speed is itself highly relative in relation to absolute preferences for unification, which is the relevant basis for

comparison, as discussed below. Policy-makers, whatever source of information they use, will not necessarily make fine distinctions of this sort and for the purposes of this Note we will not either. Analyses in the text rely on preferred speed while additional analyses using relative speed are reported in the online appendix. As the two correlate 0.65 in our data, they yield similar findings. Of course, it may be that whatever policy-makers follow is better indicated by responses to the membership question used by Toshkov, and we can directly evaluate this possibility.³

Modeling thermostatic responsiveness requires us to measure relative preferences. In theory, these equal the difference between the underlying (or absolute) preferred level of policy and policy itself. When absolute preferences increase, additional steps towards unification will be needed just to keep relative preferences the same. If absolute preferences decline, support for more policy may decline even when policy levels remain unchanged. In what follows we first use membership as a dependent variable in models of public responsiveness and then later as an independent variable in models of policy responsiveness, following Tohkov (2011). In both sets of models we then employ preferred speed using the same modeling approach for the sake of comparability.

What is it that citizens actually might notice regarding the productions of Brussels? There are two main issues here: (1) the types of policy that are salient to the public and (2) whether people respond to the flow or stock of policy. Toshkov (2011) and Franklin and Wlezien (1997) make different assumptions and these turn out to make big differences in analyses of public responsiveness. Let us begin with the type of policy.

Toshkov (2011) argued that only directives enacted by Council and European Parliament

should be seen as important enough to count as a reliable measure of EU policy-making activity.⁴ This makes good sense, as there is reason to suppose that citizens do not notice everything that the EU does, and ‘important directives’ appear to be more salient than other actions, though note that Trenz (2004) found the Commission far more newsworthy. Franklin and Wlezien (1997) used a broader measure that encompassed all directives and regulations – including those from the Commission. Specifically, they focused on the total number of lines of all this legislation. The assumption was not that the public noticed the number of lines or that all directives and regulations were equally important, but that the measure provided an indication of the general tenor of EU policy. Do European publics respond fairly equally to different types of policy? Or are they more responsive to certain, salient policies? Here we explicitly test these alternative possibilities by other means.

Though Toshkov (2011) and Franklin and Wlezien (1997) focus on different types of policy, they both assume that the public responds to annual flows of policy. That is, the public’s preferences for more policy are conceived to be a function of what policy-makers did in a particular year. This actually contrasts with what the thermostatic model implies, namely, that relative preferences equal the difference between the level of policy people want and the level they are getting. The level of policy can be seen not as what happens in a particular year but as the total stock of policies that have accumulated over time – in the EU, the cumulative total of legislative acts. For reasons discussed below and in the online appendix, cumulative total acts, whether of important directives or other legislation, is closely related to the annual total of lines of legislation. Of course, it may be that changes in preferences reflect changes in policy, i.e. flows of legislation. We can explicitly test for

responsiveness to both stocks and flows in our models.

It is worth noting that public opinion may not react to policy at all but respond instead to events. Developments such as ‘Project 1992’, which publicized the anticipated completion of the Single European Market, might have moved support for unification upwards while disasters such as the Mad Cow crisis of 1996, when the evening news featured stories about possibly contaminated beef, may have moved support downwards. The online appendix shows how opinion predicted by these and other events approximates the evolution of each series. The appendix also contains analyses of opinion variables that have been purged of the effects of events – analyses that yield very similar findings to those reported below.

What about policy-makers? Whatever the signals that inform them regarding public support for further unification, there remains the question: what it is that policy-makers would adjust in response to those signals? Again, Toshkov (2011) focused on important directives. However, just as we can consider what types of policy people respond to, so we can assess what types of policy officials adjust in response to public preferences. In what follows, we focus on the number of enactments of important directives, other directives, important regulations and other regulations. Our analysis of policy responsiveness concentrates on annual flows of policy, not stocks per se, as relative preferences are about change, whether people want more (or less) policy than currently is in place and policy-makers should respond (if they do) in kind. The focus on flows also is implied by the modeling approach, discussed below.

Methodology

Turning to differences in methodological approach, these arise from a concern for possible absence of stationarity in the data. The stock of legislation, whether directives or regulations, trends over time, and public preferences also appear to be nonstationary or close to it, consistent with Toshkov (2011). This would be methodologically complicating except that the critical variables employed in models on which we base most of our findings – preferred speed and the cumulation of important directives – appear to be co-integrated (see online appendix). To begin with, therefore, we employ error correction models (ECMs) rather than the vector autoregression (VAR) models used by Toshkov (2011).

When addressing effects of opinions on policy, the models necessarily focus on the flows of policy in each year, i.e. the difference between the total stock of legislation from one year to the next. The latter is equivalent to the number of pieces of legislation enacted in a particular year – the quantity investigated by Toshkov (2011).

Effects of policy-making on public opinion

We start by modeling effects of EU policy-making on public opinion, using the several measures already discussed. Our analysis starts in 1986, the year when the speed preference questions were first asked in their current format.⁵

Table 1, in Model A0, replicates Toshkov's (2011) finding that the membership measure responds positively to annual enactments of directives emanating from the Council and Parliament – Toshkov's ‘important’ directives. Model A1 and subsequent models replace the measure of yearly legislative output with one that captures the total stock of legislation,

following our earlier discussion. Doing this produces a negatively-signed, statistically significant lagged effect of the stock of important directives on the membership measure (see Model A1).⁶ This negative feedback implies that the membership variable at least partly taps the public's relative preference for unification – whether they want more or less unification than is currently in place – and contrasts at least to some extent with Franklin and Wlezien's (1997) view that the measure was indicative solely of absolute preferences.

This effect survives ($p = .06$, not shown) a robustness test in Model A2 that purges the cumulative important directives indicator of the trend that drives growth in the stock of those acts over time. (To make the coefficients directly comparable, we separated cumulative directives into a component predicted by linear trend and the residual component). However, note that the apparent negative feedback completely disappears when estimated over the full period for which we have membership data, which begins in 1974 (see the online appendix).

The remaining models in Table 1 replace the membership dependent variable with a more explicitly relative measure, the preferred speed of unification. The results in Model B1 demonstrate stronger and more reliable effects of the lagged stock of important directives; and we see in Model B2 that the residual effect also survives ($p = 0.06$, not shown) being purged of trend. Indeed, there is evidence of both short and long term effects of important directives on public opinion (see the online appendix for discussion of short-term versus long-term effects). The coefficients (-0.72 and -0.92) for the lagged opinion variables in Models B1 and B2 further indicate that there is substantial error correction – when opinion and policy are out of equilibrium, the former adjusts almost fully in the ensuing year.

One other measure of legislative output – important regulations – also yields a significant effect (see the online appendix), and that effect survives the purging of trend (Table 1, Model D2). This suggests that the public senses the accumulation of both directives and regulations emanating from the Parliament and Council over time and that this drives the evolution of public opinion over time. We cannot be absolutely sure that the public responds separately to directives and regulations, as the two cumulative measures are highly correlated even when purged of trend (Pearson's $r = 0.90$, not shown). That the sum of the two types of legislation outperforms the separate measures (see the online appendix) is suggestive, though the results are not definitive.

TABLE 1 ABOUT HERE

On the basis of these analyses, we have learned a number of things. First, it seems that the public does notice and respond to European policy – there is negative feedback – consistent with what Franklin and Wlezien (1997) discovered and in contrast with what Toshkov (2011) found. However, the public does not react just to the annual flows of (i.e. changes in) policy but more especially to the cumulative total of policy over time, which neither Toshkov nor Franklin and Wlezien considered. The findings are highly significant, statistically, and robust to different model specifications (see the online appendix).⁷

Second, it appears that the public responds mostly, if not solely, to very salient EU policies – specifically, important directives and/or regulations emanating from the Council and Parliament. Thus, there is no firm evidence for the supposition implicit in Franklin and Wlezien (1997) that the public is responding to the course of *all* EU policy. Indeed, what we find comports more with Toshkov's (2011) assumption about important legislation even

though it contrasts with his results, as we have seen.

Finally, we should make clear that Franklin and Wlezien (1997) found evidence of thermostatic public responsiveness not because they used a cumulative measure of policy but because their measure of annual policy – the total lines of directives and regulations – trended upwards over time. Perhaps this was because lines of legislation in each year had to expand over time in order to take account of expanding EU policy (see the online appendix). There are other possibilities, and the precise accounting, while important, is not relevant to our analysis of public responsiveness.

Effects of public opinion on policy-making

To assess effects of public opinion on policy-making, we need to reverse our dependent and independent variables. These effects might have been hard to disentangle from effects in the other direction, except that we expect them to have a different sign – policy-makers' reactions to opinion change should be positive, not negative – and the effects also are lagged, following Toshkov's (2011) assessment of the data's lag structure. We first estimate Error Correction Models (described in the online appendix), and so include the lagged difference in opinion. This lag structure is standard in representation studies where a delay is expected before policies can be adjusted in light of opinion change (see Soroka and Wlezien, 2010).

In the first four models of Table 2, effects over the full period for which data are available (starting in 1986) are paired with effects over a later period (starting in 1995) to assess whether the effect on policy vanished, as Toshkov (2011) found. As noted above, these models focus on annual flows of important directives. We begin by using the

membership variables to measure public opinion, also following Toshkov (2011). In the first column, we see a significant lagged (long-term) effect of membership on policy and, in the second column that indeed the relationship vanishes after 1994, just as Toshkov showed. However, if we instead use preferred speed of unification (Models B1 and B2) to measure support for important directives then, far from vanishing, the effect is actually stronger and more significant after 1994.

TABLE 2 ABOUT HERE

Models C-E in Table 2 investigate effects of opinion change on the production of each in turn of Commission directives, important regulations and Commission regulations. The specification in these equations differs from that in columns A-B because the flows of these policies are not stationary (see the online appendix), so we need to take differences in the flows as dependent variables. The results indicate that, of these three legislation types, only important regulations appear significantly affected by opinion change. This result might suggest that policy responsiveness is perfectly symmetrical to public responsiveness – both important directives and important regulations seemingly respond to public opinion, just as public opinion seemingly responds to both (or either) of them. We are reluctant to draw this conclusion, however, as the relationship in model D2 of Table 2 implies that it is changes in the flows of important regulations that follow preferred speed. This contrasts with theory and with the pattern we see for important directives, so we are less confident about the responsiveness of important regulations.⁸

Based on these results, what policy-makers mostly adjust in response to public opinion is important directives, just as Toshkov (2011) surmised and partially symmetrical to public

responsiveness. However, there is no evidence of error correction, as the coefficients for the lagged level of important directives (except in Model C2) all are small and mainly insignificant. This result implies that any cointegration between preferences and policy is unidirectional, flowing from policy to opinion, as we saw in Table 1. (This is not surprising given the results of the cointegration tests in the online appendix). But, it also may be that our opinion measure, preferred speed, really is not integrated. After all, we expect relative preferences that are thermostatic to be stationary (Soroka and Wlezien, 2010), and we are dealing with very short time series, where the probability of Type II error in Dickey Fuller tests is quite high (Gujarti, 2003). Moreover, even those tests put preferred speed only on the cusp ($p = .05$) of non-stationarity (see the online appendix). What happens if we relax our assumption about the statistical properties of public preferences?⁹

TABLE 3 ABOUT HERE

Columns 1 and 2 of Table 3 contain equations we would estimate if preferences were stationary, first for the period after 1985 and second after 1994. The only difference between these models and the ones in columns B1 and B2 of Table 2 is that lagged cumulative important directives are not included. As we can see, this makes a difference to the results, particularly over the full period, where there is clearer evidence of policy responsiveness; the coefficient for the shorter period after 1994 is effectively unchanged. However, we do not know whether preferred speed really is stationary. As we have mentioned, basic unit root tests indicate that it may be nonstationary ($p = .05$); further diagnostics suggest that it may be trend stationary – including trend reduces the probability of nonstationarity to .02 (not shown), implying that preferences trend downward over time but that the variation around

the trend is stationary. To account for this possibility in our analysis, we can incorporate trend into our equations. As we did for cumulative important directives in Table 1, we separate preferred speed into the component predicted by linear trend and the residual.

The results in the final two columns of Table 3 are very supportive of policy responsiveness. The flow of important directives in each period reliably follows both the downward trend in preferred speed and the residual variation around that trend, and these effects also are statistically indistinguishable from each other ($p = .71$, not shown). This increases our confidence that European policy really does follow public opinion, not just the trend in opinion preferences over time, and that it did so both before and after 1995.

Discussion

Measurement matters in analysis of European opinion and policy. We find strong evidence of thermostatic reaction to the *cumulation* of important European policy over time. We also find strong evidence that enactments of important directives respond to *preferences for policy change*. It ultimately appears that European policy-makers do pay attention to what the public thinks and wants, though what they adjust may not perfectly match what the public notices. While a possible disjuncture remains, there nevertheless seems to be less of a democratic deficit than many observers suppose.

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Notes

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1. This assumption is implicit in Toshkov (2011). In the online appendix, we discuss whether it is plausible to assume coordinated EU responses to public preferences.
 2. Note that Soroka and Wlezien (2010) acknowledge and allow for the possibility of positive feedback as well.
 3. Policymakers have various ways of gauging public opinion, including private polls, but in the EU the Eurobarometer (EB) is the only source yielding regular readings of public opinion over time. Of course, they have other sources of information regarding public sentiment that may be correlated with one or more of the EB variables. The speed variables are not asked in every year, yielding missing data on these variables that is imputed using Gary King's AmeliaII software (see online appendix).
 4. Policy-making in the EU is a complex process, undertaken by three different institutions – Commission, Council and Parliament – and consisting of a number of elements, of which we focus on directives and regulations. To avoid the possibility of data differences we employ data kindly made available by Dimiter Toshkov on his website (<http://www.dimiter.eu/Data.html>), including both directives and regulations from Council, Parliament and Commission – data was originally acquired from <http://eur-lex.europa.eu>.
 5. The speed preference questions were not asked in this format in until 1986; nor in 1989, 1991, 1992 or 2007. Missing values after 1986 are imputed on the basis of highly correlated questions present in both periods using Gary King's Amelia II software (King et al., 2001). Results are robust to omission of these years (as reported in the online appendix). The membership variable is available before 1985 but our membership models are generally restricted to the same period as our speed preference models for comparability. Models using membership over the entire period (as presented in Toshkov 2011) are in the online appendix.
 6. The flow (differenced stock) of these directives, which also is included in the model, has a small positive effect that is not remotely significant.
 7. The intercepts in Models B1 and B2 of Table 1 imply that underlying preferences for European unification actually increase over time, just not as much as European policy. Had unification policy increased more slowly, the public's preferred speed might not have decreased; and might even have increased. This is confirmed in an online appendix table that models preferred speed as a function of the cumulation of important directives and a linear trend variable, the coefficient for which is significantly positive. Notice that the relevant appendix analysis effectively assumes that preferred speed is stationary or trend stationary, which comports with the discussion in the text that leads to the analysis in Table 3.
 8. Analysis using the specification in model B1 does not reveal policy responsiveness for important regulations, nor for Commission directives or Commission regulations (see the online appendix).
 9. Here we focus on policymaking. See the online appendix for analyses of opinion when seen as stationary.

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Table 1. Effects of stocks of various types of legislative enactments on public support for unification policies, 1986-2008.

Input:	Outcome :	Model:	Input is Δ flow	Inputs are policy types modeled as both stocks and flows (differenced stocks are equivalent to un-differenced flows)					
			Δ membership approval (Toshkov's measure)			Differenced preferred speed of unification			
		Model A0	Model A1	Model A2	Model B1	Model B2	Model C2	Model D2	Model E2
Lagged outcome		-0.39 (0.26) ⁺	-0.43 (0.23)*	-0.43 (0.23)*	-0.72 (0.18)**	-0.92 (0.23)**	-0.84 (0.24)**	-1.07 (0.22)**	-1.06 (0.24)**
Δ flows of important directives ^T		0.34 (0.85)							
Flows of important directives _{t-1}		1.38 (1.28)							
Δ stocks of important directives ^T		0.02 (0.77)	-0.10 (0.76)	-4.12 (1.63)*	-4.36 (1.60)**				
Δ stocks of Commission directives ^T						-1.10 (3.32)			
Δ stocks of important regulations ^T							-2.47 (0.93)**		
Δ stocks of Commission regulations ^T								0.06 (0.05)	
Stocks of important directives _{t-1}		-0.07 (0.03)*		-0.29 (0.09)**					
Input predicted by linear trend _{t-1}			-0.06 (0.03)*		-0.36 (0.10)**	-0.27 (0.19)	-0.21 (0.06)**	-0.01 (0.01)	
Residual input			-0.40 (0.27) ⁺		-1.57 (0.96) ⁺	0.65 (0.85)	-0.45 (0.15)**	0.04 (0.02)	
Constant		0.52 (0.34)	0.74 (0.36)*	0.74 (0.36)*	4.16 (0.99)**	5.22 (1.25)**	4.34 (1.26)**	7.95 (1.64)**	5.45 (1.25)**
Adjusted R-squared		0.02	0.11	0.14	0.52	0.54	0.32	0.54	0.46
Observations		22	22	22	22	22	22	22	22

Note: Significant at ⁺0.10, * 0.05, ** 0.01, one-tailed except for constant term.

Measured in ^Tthousands of pieces of legislation.

Table entries are regression coefficients with standard errors reported in parentheses.

Table 2. Effects of public support for unification policies on flows of various types of legislative output, 1986-2008.

Input \ Outcome:	Annual flows of important directives [†] (differenced stocks)				ΔCommission directives [†]	Δimportant regulations [†]	ΔCommission regulations [†]
	From '86	From '95	From '86	From '95	From '86	From '86	From '86
Model:	Model A1	Model A2	Model B1	Model B2	Model C2	Model D2	Model E2
Stock of outcome _{t-1}	-0.01 (0.01)	-0.01 (0.02)	-0.01 (0.01)	0.01 (0.01)			
Flow of outcome _{t-1}					-0.65 (0.24)**	-0.36 (0.12)**	-0.31 (0.17)*
Δmembership _{t-1}	0.20 (0.07)**	0.13 (0.13)					
Membership _{t-2}	0.14 (0.06)*	0.07 (0.14)					
Δpreferred speed _{t-1}		0.04 (0.02) ⁺	0.04 (0.03)	-0.04 (0.02)	0.06 (0.07)	0.28 (0.91)	
Preferred speed. _{t-2}		0.07 (0.03)*	0.11 (0.04)**	-0.04 (0.02)	0.17 (0.08)*	-0.59 (0.78)	
Constant	-0.15 (0.10)	-0.05 (0.21)	-0.27 (0.15)	-0.49 (0.19)*	0.20 (0.12)	-0.77 (0.36)*	3.49 (3.79)
Adjusted R-squared	0.55	-0.16	0.45	0.33	0.25	0.25	0.05
Observations	21	14	21	14	21	21	21

Note: Significant at ⁺0.10, * 0.05, ** 0.01, one-tailed except for constant term.

Measured in [†] thousands of pieces of legislation.

Table entries are regression coefficients with standard errors reported in parentheses.

Table 3. Alternative models of the effects of public support for unification policies on flows of important directives, 1986-2008.

Input \ Outcome: Span of years: Model:	Annual flows of important directives ^T			
	From '86 Model A1	From '95 Model A2	From '86 Model B1	From '95 Model B2
Δpreferred speed _{t-1}	0.04 (0.02) ⁺	0.03 (0.03)	0.04 (0.02) ⁺	0.04 (0.03)
Preferred speed _{t-2}	0.08 (0.02)**	0.10 (0.03)**		
Preferred speed predicted by linear trend _{t-2}			0.08 (0.02)**	0.08 (0.04)*
Residual preferred speed _{t-2}			0.07 (0.03)**	0.11 (0.04)**
Constant	-0.31 (0.09)**	-0.42 (0.16)*	-0.34 (0.11)**	-0.32 (0.19)
Adjusted R-squared	0.47	0.34	0.45	0.34
Observations	21	14	21	14

Note: Significant at ⁺ 0.10, * 0.05, ** 0.01, one-tailed except for constant term.

Measured in ^T thousands of pieces of legislation.

Table entries are regression coefficients with standard errors reported in parentheses.

ONLINE APPENDIX for Public Opinion and European Public Policy: How Measurement Matters

This appendix accompanies the Research Note whose title appears above. It is called for because a Research Note does not permit sufficient words to address all relevant topics, so those of a more technical or methodological nature have been relegated to this appendix.

The appendix is organized as follows.

(1) We discuss the operationalization and measurement of public opinion. Measures need to address two questions: (a) what is it about public opinion that EU policy-makers would notice if they took account of public opinion in planning their legislative programme? (b) What is it about public opinion that would respond if European publics were aware of EU policy-making? Ideally the same measure would address both questions but, as explained in the main text, there is no necessary reason why this should be the case.

(2) We move to measures of policy outputs, where again we must consider (a) what policy-makers adjust (if they do) in responses to changes in public support and (b) what EU publics notice (if they do) in the productions of Brussels that lead to changes in support. Again the measures would ideally be the same – but they might not be.

(3,4,5) The appendix then considers a number of methodological/modeling issues before (6) presenting a set of supplementary results directed mainly at documenting the robustness (or otherwise) of the findings presented in the main text.

A detailed table of contents follows.

Table of Contents for online appendix

(1) Measuring opinion	2
Absolute preferences and the membership variable	2
Relative preferences and the unification, EU speed and speedup variables	3
Which measure would policy-makers consult	3
Who would co-ordinate responses to opinion change	4
(2) Measuring policy-making	4
Policy-making by Commission, Council and Parliament	4
Toshkov's view of 'important legislation' - Council and Parliament directives	4
Commission directives; Council and Parliament regulations	5
How important directives go with other (Commission) directives (or not)	5
How directives go with directives+regulations (or not)	5
What the public notices	5
Total volume of legislation measured in lines of text	6
Six-monthly versus annual data	7
Stocks versus flows of legislation	7
(3) Stationarity and unit roots	8
(4) Opinion and events	8
Events as sources of support for unification policies	8
Membership support speedup support, relative speed and events	9
Comparing actual opinion series with the same series predicted by events	9
Purging opinion series by removing the effects of events	10
Graph showing relationships of actual opinion with opinion predicted by events	10
When should we expect purged opinion to work better than unpurged opinion	11
(5) Treatment of missing data	12
(6) Supplementary findings	13
Table A.1: Table 1 omitting years with imputed missing data	13
Table A.1b: Table 1 with level measures rather than differenced measures	14
Table A.2: Table 2 omitting years with imputed missing data	15
Table A.3: Table 3 omitting years with imputed missing data	16

1. Measuring opinion

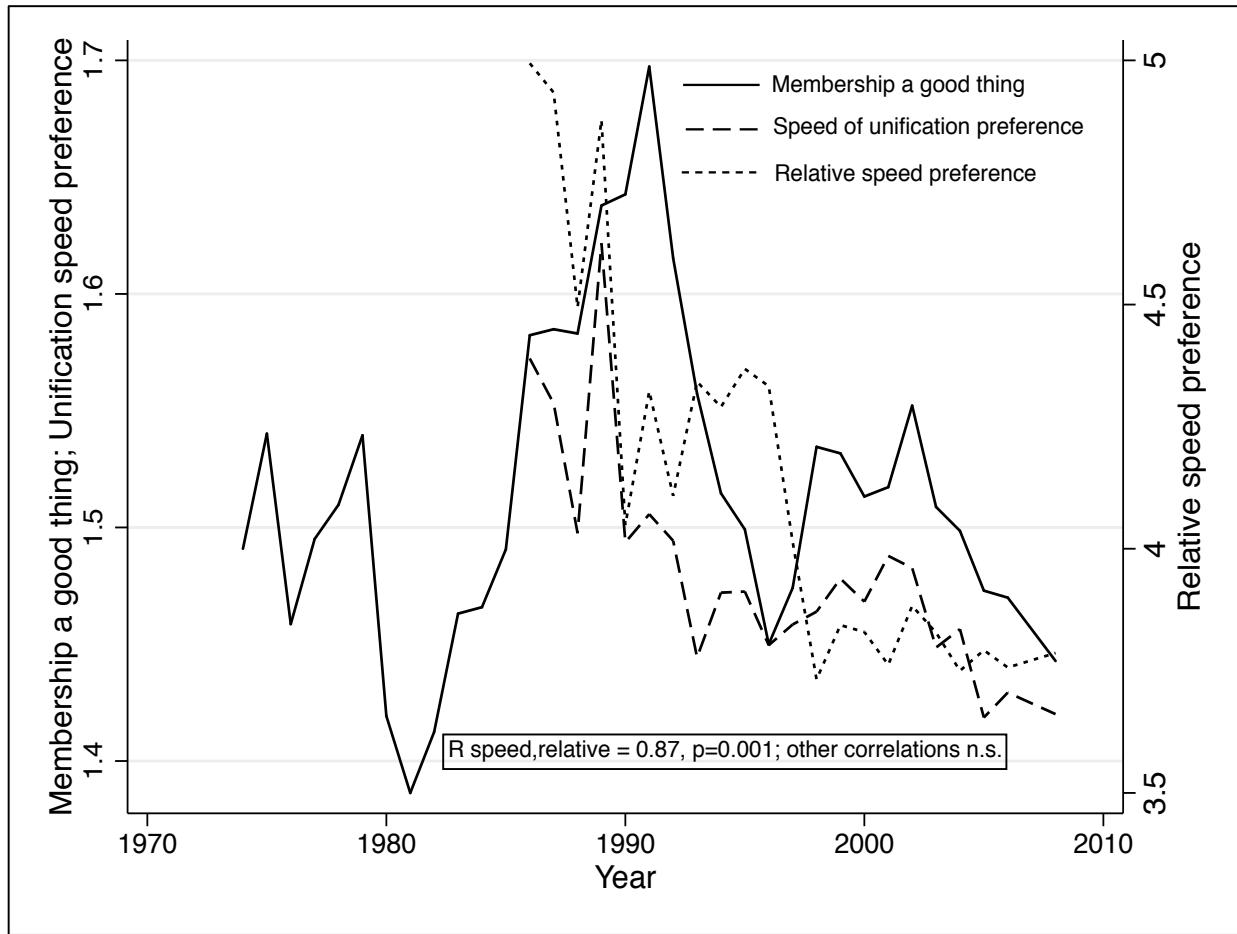
The membership variable (described in the text) seems on its face to be a measure that relates to a state of affairs – whether membership in the EU is a good thing or not. In the vocabulary of dynamic representation (Franklin and Wlezien, 1997), it is a measure of absolute preferences. Assessing dynamic representation also calls for a measure of relative preferences. Franklin and Wlezien (1997) measured relative preferences with a question that asked if respondents in general were 'for or against efforts being made to unify Western Europe?' This is arguably a question that taps respondent's preferences regarding relative speed of unification. After all, efforts to unify Europe must be moving the unification process forward to some extent. It is not a big leap to suppose that members of the public who are strongly in favor of these efforts would like to see unification proceeding faster than at present, and the reverse for those who strongly oppose these efforts. But the identification of this measure as relating to relative preferences did not have to rest on construct validity. The thermostatic model permitted its character to be assessed empirically. Analysis using the resulting 'unification' variable revealed a strong and growing thermostatic relationship – the variable responded negatively to policy change over time. It also was positively related to the membership variable, so that stronger preferences for EU membership went along with preferences for more efforts being made to unify Europe, other things being equal.

This touches on the theoretical difference that separates Franklin and Wlezien's approach from that of Toshkov, who only used one measure of public opinion. Both measures are arguably needed as their differently-signed effects are what are called for theoretically if negative feedback is to be used to evaluate democratic performance. This is because change in one measure might offset or enhance change in the other (for example due to new policies creating support for a higher absolute level of policy).¹ Of course it may be that membership partly taps relative preferences, if to a lesser extent, which some of the analysis reported in the text suggests. In the relevant tables of this appendix we use both types of measure. But the unification question was discontinued after 1995 and never reinstated. The question about preferred rate of unification (referred to in the EB codebook as 'speedup') introduced in 1986,² however, provides an excellent substitute for the unification question. It also may be that relative preferences are better captured by taking into account the other new questions (referred to in the EB codebook as 'EUspeed') that

^{A.1} Literature evaluating democratic performance in this fashion is surveyed and extended in Wlezien C. and Soroka S. (2012) 'Political Institutions and the Opinion-Policy Link.' *West European Politics* 35(6):1407-1432. See also footnote 3 in the main text.

^{A.2} This question asked 'please look at these people (Show card). N° 1 is standing still, N° 7 is running as fast as possible. Choose the one which best corresponds ... to what you would like?' A similar pair of questions, coded on three-point scales, had been asked intermittently (and seldom as a pair) in earlier years.

measures the perceived current rate of unification. The difference between the two yields a measure that by construction taps respondents' relative preferences for faster (or slower) unification (see Figure A.0).



Which, if any, of the measures policymakers reflect when making policy is an empirical question of course.³

This appendix considers responsiveness of and to the different relative preference questions and shows that, in practice, it makes little difference which is employed, though preferred speed purged of events appears to work best (Table A.1a).

It also is not clear that policy-makers responsible for specific policy acts consult Eurobarometer findings at all. Nor, incidentally, is it clear who would co-ordinate the reactions to changes in public opinion however these were

^{A.3} Relative speed is the measure called for theoretically when looking for opinion responsiveness but not necessarily when looking for representational responsiveness. Representation will respond to whatever measure best tracks what policymakers consult (if they do) or otherwise become aware of. Evidently, a good understanding of representation theory would lead them to consult a relative measure, but we cannot be sure there will be this degree of theoretical sophistication on the part of policy-makers, so the question which measure best tracks their source must be settled empirically.

ascertained. Our supposition is that the Commission is a 'talking shop,' like a Parliament, that is buzzing with information of all kinds. There can be little doubt that the 'corridors of power' are where information is communicated in Brussels as elsewhere. So EU bureaucrats are surely aware of the state of public opinion just as a by-product of being party to this buzz of information. In such an atmosphere it is possible for decisions to be made in a coordinated fashion even if no-one is explicitly coordinating those decisions (Toshkov, 2011:173, makes a similar point regarding apparent co-ordination of Commission responses to public opinion).

2. Measuring policy-making

Policy-making in the EU is a complex process, undertaken by three different institutions – the Commission, the Council and the Parliament – and consisting of several elements, of which we focus on directives and regulations. Toshkov (2011) suggests that the only policy-making of relevance to EU public opinion are directives emanating from the Council and Parliament (what he calls "important directives"). These constitute a small subset of EU policy, as illustrated in Figure A.1, and it is at least questionable whether EU publics can make such fine distinctions. Still, whether or not the public can discern this difference, EU policy-makers certainly can; and their reactions to opinion change may well do so too.

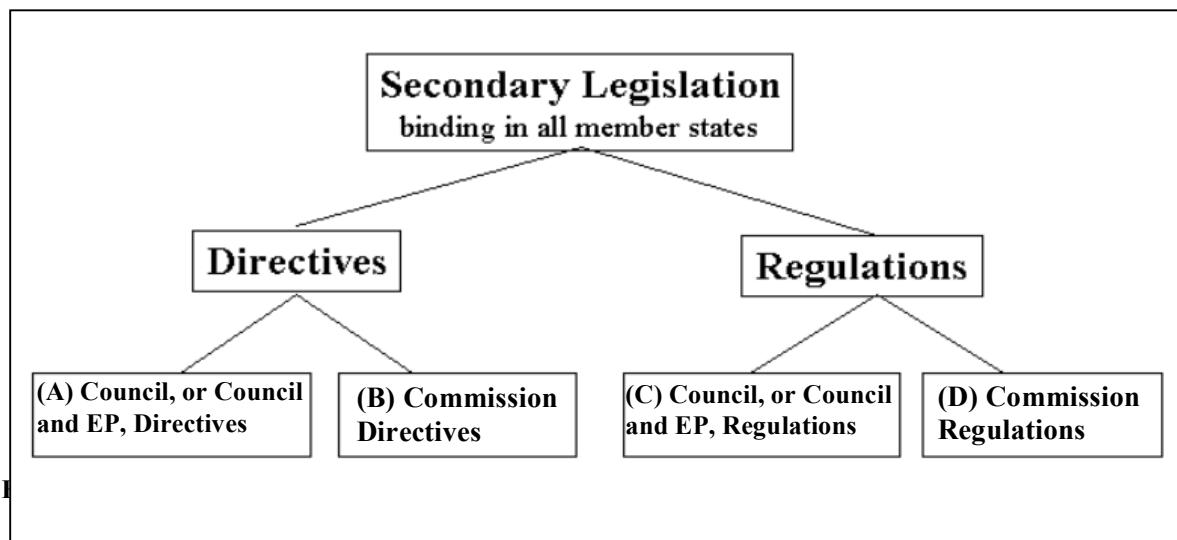


Figure A.1 visually depicts what portion of EU legislative output Toshkov focuses on (box A).⁴ Over the years,

^{A.4} EU policy-making encompasses additional legal instruments beyond the two we have highlighted. Our objective is to document what portion of legal acts, among those that are binding in all member states, Toshkov is focusing on and which he is ignoring. Our objective is not to give a comprehensive overview of

he omits from 95 to 98 % of EU legislative output (leaving aside decisions recommendations and opinions, which we also exclude). The choice to exclude such a large share of EU legislative output could be the right one but it has potential consequences, and actually might have contributed to the 'lost relationship' (though we find it does not). Toshkov justifies his focus by arguing that only directives by the Council and European Parliament – so-called 'ordinary legislation' – would have policy impact visible to the public. Commission directives he considers to be 'implementing acts that further specify or update provisions laid down in 'regular' directives and as such do not have a place in an index of important legislation' (Toshkov, 2011: 176). However, often these Commission directives, the implementing acts, are what the EU uses to define uniform rules and policies that may well be visible to the public.⁵ And, realizing this, policy-makers might well adjust both types of directive in response to changing public opinion.⁶

Because Council and EP directives go hand-in-hand with implementing acts, either one might have been a good indicator of the other but we see in Figure A.2 that this is not the case. While there is some correspondence of peaks and troughs – a correspondence in the way the amount of legislation changed from year to year – there is no such correspondence in the relative frequency of occurrence of the two types of legislation, as the levels do not move together over time. On average, Council and EP directives remain fairly constant while Commission directives show an increasing trend (Pearson's r is a meager 0.33).

The difference seems even starker when we look at the correspondence between the number of directives and the number of directives-plus-regulations, pictured in Figure A.3. However, the correlation between directives and each of the other series is considerably stronger ($r = 0.51$) than the correlation between important and all directives. Still, this implies that at most 25% of the over-time variation in the two is common, and which measure better reflects what EU citizens notice is best settled empirically.

all legal instruments; for instance we omit discussion of secondary legislation such as decisions, opinions and recommendations. It would in any case be hard to determine which decisions were generally binding.

^{A.5} The legislative acts considered in our analysis were collected by Dimiter Toskbov from, <http://eurlex.europa.eu> (the online version of the *Official Journal of the European Union*) and kindly made available on his website (<http://www.dimiter.eu/Data.html>).

^{A.6} Lack of explicit co-ordination (as discussed earlier) might have the same effect.

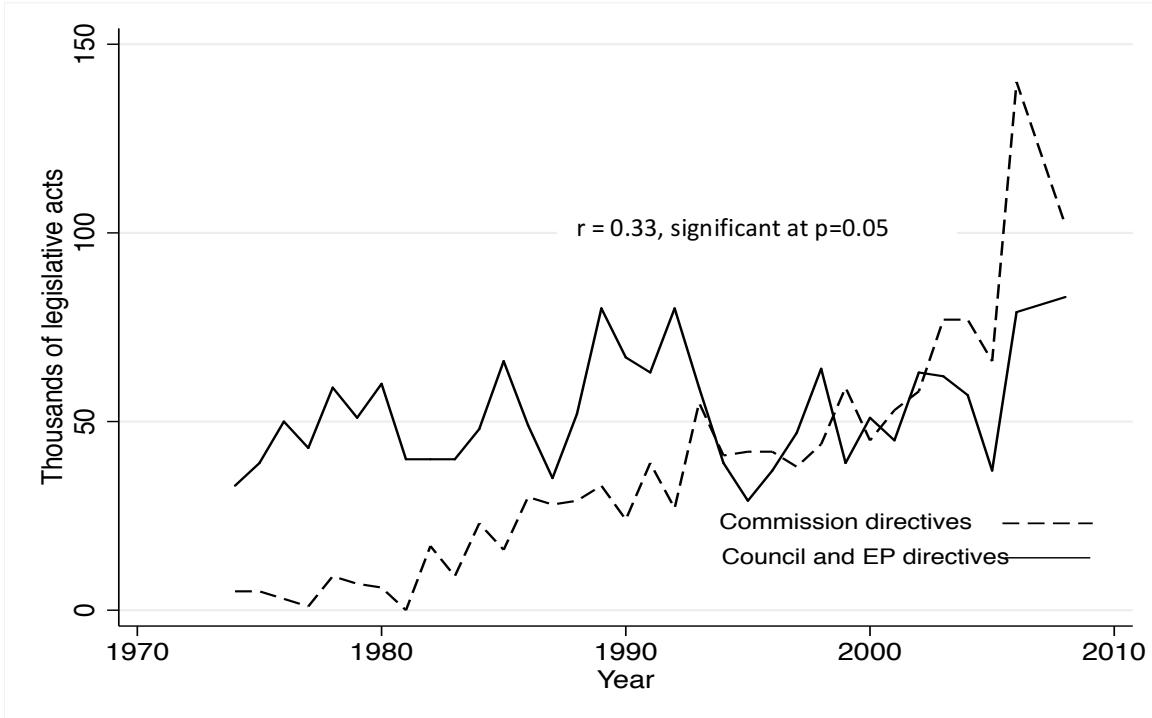


Figure A.2. Commission directives vs, Council and EP (important) directives, 1974-2008.

Franklin and Wlezien (1997) also studied the relationship between levels of support and levels of policy-making. They assumed that what the public was responding to was a general sense of what was going on in Brussels (the 'buzz around the hive'), best indicated by the total quantity of directives and regulations, both technical and substantive – all four of the boxes at the foot of Figure A.1 above. In the main body of this Note we see what difference it makes in practice as the definition of 'what is legislation' is widened to incorporate more of the different types of legislation illustrated in Figure A1.

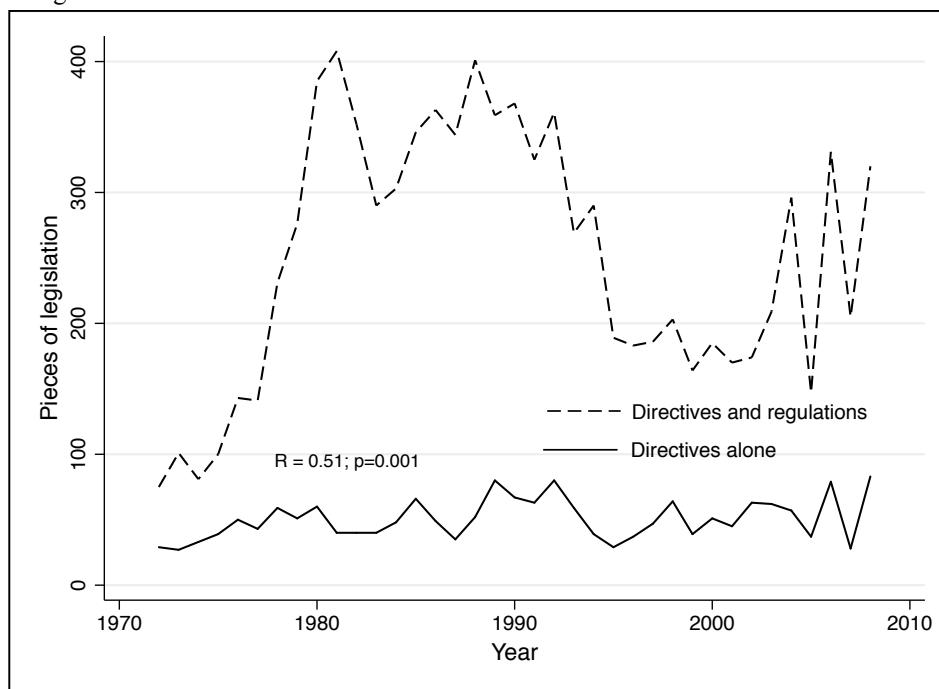


Figure A.3. Number of important directives versus number of important directives plus regulations 1974-2008.

Franklin and Wlezien used total lines of legislation as their measure of “what was going on.” In our Note and in this appendix we do not employ that measure, finding better responsiveness of public opinion to the cumulative total of legislative outputs over time. We suppose that the apparently good performance of total lines of legislation was due to the fact that, over time, legislation has to become more and more complex (and thus more and more lengthy in terms of lines) in order simply to enumerate the ways in which new legislation must take account of existing legislation. In our data, cumulative totals of important directives correlate 0.79 with lines of important directives plus regulations summed with Commission directives plus regulations (the measure employed by Franklin and Wlezien 1997).

Although we use his data, two major differences distinguishes our treatment from Toshkov's. First, he took his data at six-monthly intervals arguing that this corresponded to the six-monthly rotating presidency, which would be in the hands of a different national government with the passage of each of these periods. However, the legislative cycle in the EU is not tied to these six-month terms but is rather of an annual nature, if not at times even longer.⁷ Moreover, it would be hard to tie legislative acts to the presidencies in which they originated since very few pieces of legislation can be brought to fruition within six months. Finally, the Eurobarometer surveys, which give us our readings on the state of public opinion, are not generally fielded at the ends of presidential terms of office, but rather in the middle of those terms – though the cycle of Eurobarometers going into the field is not always a regular one. Sometimes the usual Spring EB is not fielded until June or July and sometimes the usual Fall EB is not fielded until January of the following year.⁸ With this amount of temporal indeterminacy for both dependent and independent variables it seems to us that six-monthly time periods might demand too much of the data.⁹

^{A.7} Reflecting this, Presidencies tend to focus on setting broad policy agendas and priorities. They are far less involved in the day-to-day legislative process, where the key actors are the Commission, Council, and Parliament.

^{A.8} In such cases we date the responses as having been given in the previous year.

^{A.9} Taking annual readings of the speedup variable is helpful because the questions are not asked in every survey. By using yearly readings we have less need to impute values for time-points when the question was not asked (see Section 5, below).

The second way in which our treatment of data departs from Toshkov's is in our focus, regarding feedback, on cumulative totals (stocks) of legislation rather than on flows. Flows are all that policymakers adjust, of course, but we theorize and find empirically that stocks (the total accumulation of all EU legislation) is what moves public opinion.

3. Stationarity and unit roots

A primary concern in our analyses regards the possible absence of stationarity in the data. Both the quantity of legislation and apparent public reactions to that legislation appear to trend sharply (see Figure 1 regarding public reactions, Figure A.2 regarding policy) when we take preferred speed and total legislation as our measures. This gives rise to the possibility of what are technically known as 'unit roots' in the data, meaning that there is no equilibrium to which a series returns over time – a pattern known as 'stationarity' – but rather a pattern known as a 'random walk' in which each step away from a starting configuration provides a new baseline from which further steps are taken. Our own diagnostic tests cannot disconfirm (at $p=0.05$) a unit root for any of the variables we employ; and speed preference has a p-value of 0.64, suggesting a quite strong likelihood of a unit root.

Toshkov (2011) assumed that if his series were subject to this problem then this would be true both for his dependent and independent variables, giving rise to pairs of variables being 'co-integrated' and therefore unproblematic. We employed the Engle-Granger test for co-integration of pairs of variables, which involve taking the residual from a prediction of the outcome from the input and testing that residual for stationarity. Results for analyses reported in the text are shown here in the appendix in Tables A.0a and A.0b. Values below 0.05 indicate confidence at a conventional level of significance in the absence of a unit root (and hence stationarity) for the residual concerned, using the Augmented Dickey-Fuller test. These p-values are also relevant to the possibility of biased findings in any subsequent tables involving the same variables as those in Tables A.0a and A.0b.

Table A.0a. Probability of non-stationarity and no co-integration for levels of variables used in Table 1.

Input	Outcome	Non-stationary input	Non-stationary outcome	Not co-integrated
Important directives	Membership	0.14	0.69	0.01
Important directives	Speedup	0.14	0.05	0.00
Other directives	Speedup	1.00	0.05	0.02
Important regulations	Speedup	0.00	0.05	0.06*
Other regulations	Speedup	0.99	0.05	0.03

Note: Low p-values correspond to a high probability that relevant variables are stationary and/or co-integrated, on the basis of the Augmented Dickey-Fuller Test.

Table A.0a shows that models involving stocks of legislation provide good levels of confidence in the co-integration of their variables. Models involving flows of legislation are more problematic. Indeed flows of important directives are stationary, with implications for our modeling approach that are mentioned in the main text of this Note.

Regarding Table 2, Table A.0b shows equivalent test results.

Table A0b. Probability of non-stationarity and no co-integration for levels of variables used in Table 2

Outcome	Input	Non-stationary input	Non-stationary outcome	Not co-integrated
Membership	Important directives	0.69	0.14	0.26
Speedup	Important directives	0.05	0.14	0.19
Speedup	ΔOther directives	0.05	0.17	0.01
Speedup	ΔImportant regulations	0.05	0.15	0.09
Speedup	ΔOther regulations	0.05	0.48	0.50

Note: Low p-values correspond to a high probability that relevant variables are stationary and/or co-integrated, on the basis of the Augmented Dickey-Fuller Test.

Supplementary tests (not shown) for other pairs of variables employed in tables later in this appendix were conducted and can be inspected using the replication data and dofile supplied with this appendix. Of the variables we single out for mention, the only one with possibly problematic test results is the residual from relative speed when taken as the independent variable. But this variable is also suspect for other reasons mentioned below.

4. Opinion and events

Addressing how European publics become aware of the policy productions of Brussels raises the question whether they rather react to events. As mentioned in the main text, events associated with Project 1992 (the project to complete the single European market) were highly popular. They also involved a great deal of legislation. The Mad Cow crisis of the middle-1990s and the Eurozone crisis that started in late 2009 might well have engendered a hiatus in legislation as policymakers reconsidered their objectives or were forced to use international agreements to achieve urgent goals. Treaty revisions also appear to the public as events but evidently go along with a great deal of legislative activity being rushed through prior to rule changes. So events that are noticed by the public might provide a route by which policy enactments gain their apparently causal effect on public opinion.

Recent work has shown that the membership series does respond strongly to events,¹⁰ with events explaining some 80 % of the variance in positive responses to the membership question over the period from 1986 to 2012. Over the period of our concern, which starts and ends earlier, the variance explained is even greater (84 % – see Table A.0c, Model A). To the best of our knowledge, no equivalent work has been done using preferred or relative speed; but replacing membership by relative speed in the analysis reported by Franklin and Wlezien (2013) yields 68 % of variance explained – less than effects on membership, but still substantial (Model C).

Table A.0c. Effects of events on the membership and speed preference series.

Inputs \ Outcome: Model:	Membership a good thing Model A Coef. (s.e.)	Preferred speed of unification Model B Coef. (s.e.)	Relative speed of unification Model C Coef. (s.e.)
Constant	1.57 (0.01)***	5.08 (0.05)***	1.53 (0.10)***
Project 1992	0.10 (0.03)**		-0.48 (0.22)*
Maastricht treaty	-0.09 (0.02)**	-0.27 (0.11)*	
Mad cow crisis	-0.09 (0.02)**	-0.20 (0.11)*	
Eurozone initiation			-0.53 (0.20)*
Amsterdam treaty			-0.69 (0.20)**
Eastern enlargement	-0.09 (0.02)**	-0.41 (0.10)**	-0.69 (0.20)**
Lisbon treaty	-0.09 (0.03)*	-0.24 (0.14)*	-0.34 (0.26)+
R-squared	0.84	0.58	0.68
Observations	23	23	23

Note: Significant at +0.10, * 0.05, ** 0.01, ***0.001, two-tailed.

Figure A.4 shows the correspondence between the path taken by (1) the membership series and (2) the relative speed preference series, each with their corresponding paths predicted from the cumulative effects of events. As can be seen, the membership series closely follows the path set by events whereas relative speed preferences diverge quite a bit from their predicted path – divergences that provides an opportunity for effects of policymaking to be distinguished from effects of events.

We can discover whether events drive the relationships we find by operationalizing our measures of support for EU legislation as residuals from analyses that predict answers to relevant EB questions from independent variables indicating event occurrences. If events drive our findings then these residuals will explain nothing. They will be opinion measures from which explanatory power has been extracted by removing the effects of events. On the other

^{A.10} Franklin M and Wlezien C (2013) *Spotlight on... Support for Europe at a Time of Crisis*. At: <http://www.eui.eu/Projects/EUDO-PublicOpinion/Documents/Spotlight/SpotlightEUDO5.pdf> (Accessed [April 7, 2015].

hand, if these residuals remain significant predictors of support for legislation, this will imply that something other than events drive the relationships we see.¹¹

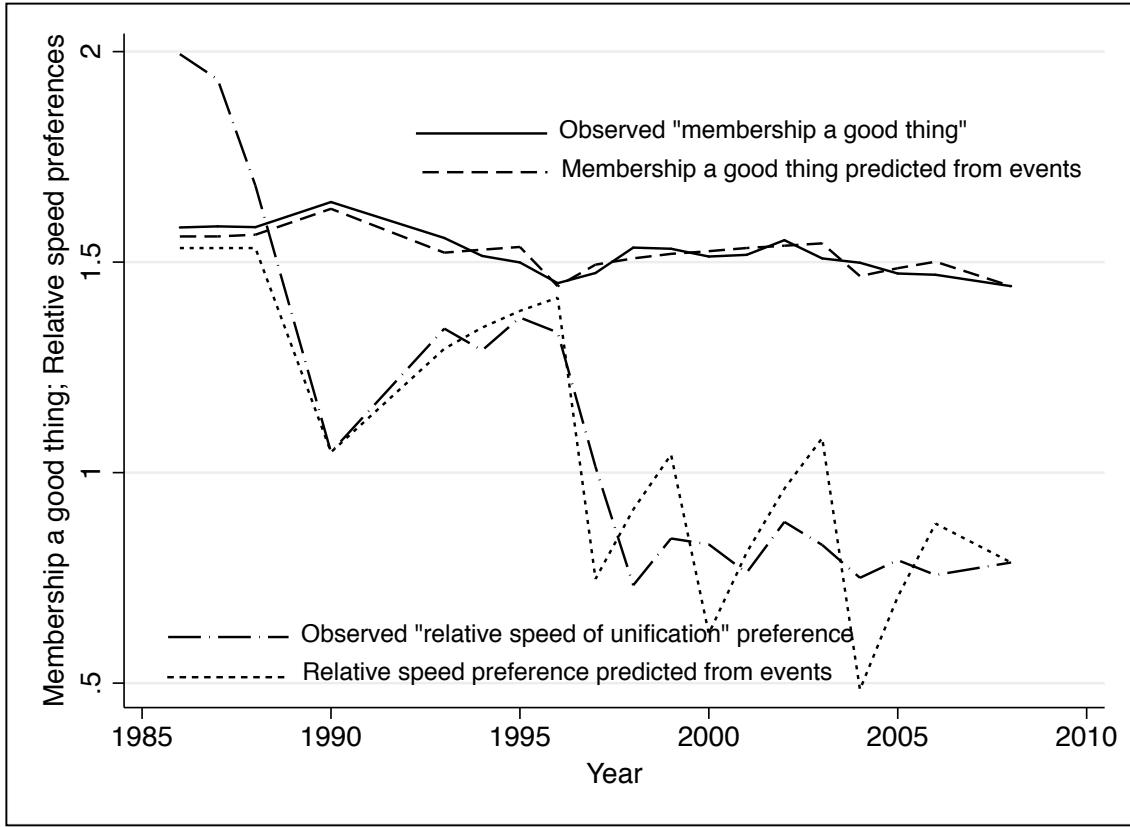


Figure A.4. Correspondence between two opinion series and the paths those series would take under the influence of events alone.

Which specific measure (preferred speed or relative speed preference) will work best cannot be decided theoretically since both are by construction highly relative;¹² but theory does tell us that purging an opinion series of fluctuations induced by events should reduce the amount of error the series contains, making it possible to explain more of the remaining variance. In practice Tables A.1a and A.1b tell us that preferred speed provides results that are more unambiguous than relative speed preference and that we explain more variance and get stronger effects of legislation on opinion when preferred speed of unification is purged of the effects of events.

A.¹¹. For clarity Figure A.4 shows only the two series for which predictions of models in Table A.0c are best. The matching of opinion to predicted opinion will be even less precise for preferred speed of unification, suggesting potentially greater residual effects.

A.¹². Here we do not depend on anyone but ourselves to actually construct the measures concerned – theory does not require EU publics or anyone else to consult the measure, in contrast to what is required in order for policymakers to represent opinion change (footnote A.3).

It is not likely that this would be true for effects on legislation. We do not expect policy-makers to construct a measure of relative unification speed, much less such a measure purged of events (see footnote A.3). Table A.2b below tells us that, if policymakers consult any of these indicators, it is preferred speed of unification that they consult. Lagged effects of relative speed preference and of each of these measures purged of events are not significant and two of them even have the wrong signs.

5. Treatment of missing data

The Eurobarometer does not ask exactly the same questions in every survey. We have already explained how the unification series was discontinued after 1995 and how the perceived and preferred speed of unification questions were added in 1986. However, those questions are not asked in 1989, 1991, 1992, 2007 and 2010 to 2012. That last gap is too large to be reliably bridged, especially as it occurred at a time of unprecedented turmoil in the affairs of the EU, but the other gaps can be bridged by employing statistical methods to 'impute' the answers that would have been given had the questions been asked, on the basis of highly correlated counterpart questions that bridge the gaps, using Gary King's Amelia II software (King et al., 2001). In particular, until 1995 the unification question provides very good predictions of the missing values (the absence of this question in 2010-12, along with the membership question in 2012, makes this gap particularly hard to fill).

6. Supplementary findings

Table A.0d. Descriptives for variables employed in this study over all years 1986-2008

Variable	N	Mean	Std.	Min	Max
Membership a good thing (three-point scale)	23	1.54	0.07	1.44	1.70
Speed of unification preference (7-point scale)	23	4.93	0.18	4.65	5.39
Speed of unification purged of events	23	0.00	0.12	-0.22	0.30
Relative speed preference (seven-point scale)	23	1.07	0.38	0.73	1.99
Relative speed preference purged of events	23	0.00	0.23	-0.52	0.44
Important directives (count)	23	55.39	15.88	23.00	93.00
Commission directives (thousands)	23	0.04	0.01	0.02	0.07
Important regulations (thousands)	23	0.28	0.12	0.14	0.53
Commission regulations (millions)	23	2.01	0.59	1.29	3.09

Table A.0e Intercorrelations between variables employed in this study, 1986-2008

Var#	Name*	Membership	2	3	4	5	6	7	8	9	10	11
2	Speed preference		0.66									
3	Speed pref purged		0.08	0.67								
4	Relative speed pref.		0.18	0.65	0.62							
5	Relative speed purged		-0.12	0.36	0.61	0.61						
6	Important directives		0.58	0.16	-0.27	0.00	-0.27					
7	Commission directives		-0.58	-0.64	-0.31	-0.56	-0.28	-0.37				
8	Important regulations		0.67	0.72	0.39	0.74	0.33	0.54	-0.75			
9	Commission regulations		-0.47	-0.25	-0.12	-0.28	-0.02	-0.43	0.30	-0.54		
10	Cumulative imp. dirs..		-0.73	-0.75	-0.36	-0.71	-0.28	-0.49	0.83	-0.94	0.36	
11	Cum. Commission dirs..		-0.70	-0.67	-0.28	-0.63	-0.23	-0.50	0.82	-0.87	0.25	0.98
12	Cum. Commission regs..		-0.69	-0.65	-0.29	-0.65	-0.23	-0.50	0.82	-0.88	0.31	0.98

* For full names see Table A.0d.

Table A.1. Table 1 omitting years with missing data for preferred speed of unification.

Input	Outcome:	Inputs are policy types modeled as both stocks and flows (difference stocks are equivalent to un-differenced flows)				
		Differenced preferred speed of unification				
		Model B1	Model B2	Model C2	Model D2	Model E2
Lagged outcome		-0.57 (0.19)**	-0.57 (0.29)*	-0.50 (0.21)*	-0.65 (0.32)*	-0.61 (0.30)*
Δ stocks of important directives _t ^T		-0.20 (2.43)	-0.23 (2.72)			
Δ stocks of Commission directives _t ^T				0.45 (3.26)		
Δ stocks of important regulations _t ^T					-0.64 (1.22)	
Δ stocks of Commission regulations _t ^{TT}						-0.43 (0.98)
Stocks of important directives _{t-1}		-0.21 (0.11)*				
Input predicted by linear trend _{t-1}			-0.21 (0.14)	-0.28 (0.23)	-0.09 (0.09)	-0.06 (0.06)
Residual input			-0.25 (1.31)	-0.28 (0.88)	-0.11 (0.22)	-0.07 (0.18)
Constant		3.08 (1.05)**	3.11 (1.63)*	2.60 (1.10)*	4.18 (2.62)	3.80 (2.28)
Adjusted R-squared		0.33	0.26	0.27	0.28	0.28
Observations		15	15	15	15	15

Notes: Significant at ⁺ 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in ^T thousands of pieces of legislation; ^{TT} millions of items of legislation.

As explained in this Appendix (section 5 above), missing data arises from the fact that the preferred speed question was not asked in five years relevant to our analyses. When we exclude cases with missing data we get the findings shown above, based on only 15 cases due to lagged missing values. Models relating to speed preference, shown here, yield substantively the same findings as those for the same models in Table 1 although, with the much reduced available N, effects (residual inputs) do not survive the purging for trend.

Table A.1a Models B and D of Table 1 using preferred speed purged of the effects of events

Input	Outcome:	Preferred speed of unification purged of events				
		Model B1	Model B2	Model C1	Model D1	Model E1
Lagged outcome		-1.16 (0.16)**	-1.24 (0.18)**	-1.02 (0.23)**	-1.00 (0.21)**	-0.97 (0.21)**
Δ stocks of important directives ^{t-1}		-5.30 (1.35)**	-5.77 (1.40)**			
Δ stocks of Commission directives ^{t-1}				-1.96 (3.11)		
Δ stocks of important regulations ^{t-1}					-0.28 (0.83)	
Δ stocks of Commission regulations ^{TT}						-1.86 (39.81)
Input $t-1$		-0.18 (0.06)**		0.02 (0.15)	-0.03 (0.05)	-1.27 (2.17)
Input predicted by linear trend $t-1$			-0.20 (0.06)**			
Residual input			-0.92 (0.65) ⁺			
Constant		0.53 (0.14)**	0.57 (0.15)**	0.06 (0.09)	0.34 (0.70)	0.03 (0.08)
Observations		22	22	22	22	22
Adjusted R-squared		0.72	0.73	0.49	0.50	0.49

Notes: Significant at ⁺ 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in ^t thousands of pieces of legislation; ^{TT} millions of pieces of legislation.

In this table we see that using preferred speed of unification purged of events produces a much less ambiguous picture of opinion being driven uniquely by important directives. There is no hint of significance in the effects or important regulations of or Commission directives or regulations. The long-term effect of stocks of important directives survives the purging of linear trend and, moreover, variance explained in Models B1 and B2 is very high.

Table A1b Models B and D of Table 1 using relative speed preference and that variable purged of the effects of events.

Input:	Outcome:	Inputs are policy types modeled as both stocks and flows (differenced stocks are equivalent to un-differenced flows)					
		Δ relative speed preference	Δ relative speed preference purged of events	Δ relative speed preference	Δ relative speed preference purged of events		
		Model B1	Model B2	Model B1a	Model B2a	Model D1	Model D2
Lagged outcome		-0.61 (0.16)**	-0.56 (0.19)**	-1.30 (0.18)**	-1.31 (0.19)**	-0.43 (0.20)*	-1.17 (0.26)**
Δ stocks of important directives _{t-1}		-8.77 (3.20)**	-8.01 (3.52)*	-9.87 (2.98)**	-10.02 (3.14)**		
Δ stocks of important regulations _{t-1}						-0.06 (1.99)	-0.35 (2.36)
Stocks of important directives _{t-1}		-0.47 (0.20)*		-0.34 (0.14)*			
Input predicted by linear trend _{t-1}			-0.42 (0.22)*		-0.34 (0.14)*	-0.04 (0.13)	-0.04 (0.15)
Residual input			0.42 (1.55)		-0.62 (1.37)	-0.05 (0.30)	-0.13 (0.34)
Constant		1.71 (0.54)**	1.55 (0.62)*	0.97 (0.31)**	0.99 (0.32)**	0.81 (1.78)	0.50 (2.00)
Adjusted R2		0.41	0.39	0.71	0.69	0.13	0.51
Observations		22	22	22	22	22	22

Notes: Significant at [†] 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in [†] thousands of pieces of legislation;

This table shows that findings for Models B and D of Table 1 in the main text also hold true for relative speed preferences and for relative speed preferences purged of the effect of events. None of these models, however, survive the purging of lagged legislation of the effects of trend, suggesting that preferred speed (previous table) is a better measure of unification support.

Table A.1c Table 1 Models C1-E1 with opinion inputs not predicted by linear trend; Models F1 and F2 with stocks of important directives + regulations, along with the same variable purged of linear trend

Input	Outcome:	Inputs are policy types modeled as both stocks and flows (differenced stocks are equivalent to un-differenced flows)				
		Differenced preferred speed of unification				
		Model C1	Model D1	Model E1	Model F1	Model F2
Lagged outcome		-0.72 (0.20)**	-0.79 (0.21)**	-0.65 (0.20)**	-0.75 (0.20)**	-0.99 (0.21)**
Δ stocks of Commission directives. ^T	-1.89 (3.22)					
Δ stocks of important regulations. ^T		-1.21 (0.83) ⁺				
Δ stocks of Commission regulations ^{TT}			0.00 (0.05)			
Stocks of legislation _{t-1}	-0.17 (0.16)	-0.12 (0.05)*	-0.00 (0.00)			
Δ stocks of important directives+regulations ^T				-1.32 (0.64)*	-1.95 (0.64)**	
Stocks of legislation _{t-1}	-0.17 (0.16)	-0.12 (0.05)*	-3.99 (45.65)	-0.11 (0.04)**		
Input predicted by linear trend _{t-1}					-0.15 (0.04)**	
Residual input					-0.34 (0.11)**	
Constant	3.70 (1.08)**	5.36\br/>(1.31)**	3.30 (1.05)**	5.24 (1.17)**	7.18 (1.37)**	
Adjusted R-squared	0.32	0.43	0.30	22	22	
Observations	22	22	22	0.48	0.58	

Notes: Significant at ⁺ 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in ^T thousands of pieces of legislation; ^{TT} millions of pieces of legislation.

This table shows that, apart from important directives (findings shown in Table 1 in the main text, Model B1), important regulations along with total important directives plus regulations also yield significant long-term effects on speedup preference. These are also the only types of legislation apart from important directives (as shown here and in Table 1 of the main text) to survive being purged of the effects of trend.

Table A1.d Replication of Table 1 with lagged cointegration residuals

Input:	Outcome:	Inputs are policy types modeled as both stocks and flows (differenced stocks are equivalent to un-differenced flows)				
		Δmembership Model A2	Model B2	Δ speed preference		
		Model C	Model D	Model E		
Δstocks of important dirs ^T	0.63 (0.79)	3.89 (1.39)**				
Δstocks of other dirs. ^T			0.20 (1.88)			
Δstocks of important regs. ^T				-0.34 (0.23)		
Δstocks of Commission regs ^T					0.01 (0.04)	
Lagged residual from Cointegrating regression	-0.29 (0.19)	-0.73 (0.18)**	-0.67 (0.19)**	-0.82 (0.21)**	-0.63 (0.19)**	
Constant	-0.03 (0.04)	0.18 (0.08)*	-0.04 (0.08)	0.06 (0.07)	-0.05 (0.08)	
Adjusted R2	0.02	0.54	0.34	0.43	0.32	
Observations	23	22	22	22	22	

Notes: Significant at + 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in ^T thousands of pieces of legislation; ^{TT} millions of lines of legislation.

In this table the lagged residual from the cointegrating regression is employed in place of the lagged outcome and legislation variables. Significant negatively-signed coefficients indicate that equilibrium errors between the two variables are corrected. This is the case for all but the membership variable whose effect might thus have been artifactual upon the common trend for both important directives and membership.

Table A1.e Effects on membership opinion including the early period
for which membership data are available, 1974-2008

Input:	Outcome:	Input is Δ flow		Input is Δ stock	
		Membership approval			
		Model A0	Model A1		
Lagged outcome		-0.41 (0.16)**	-0.27 (0.15)*		
Δ flow of important directives [†]		0.41 (0.68)			
Flow of important directives t_{-1}		1.60 (0.94)*			
Δ stocks of important directives			0.32 (0.75)		
Stocks of important directives t_{-1}			-0.00 (0.01)		
Constant		0.53 (0.21)**	0.39 (0.21)*		
Adjusted R-squared		0.13	0.03		
Observations		35	35		

Notes: Significant at ⁺ 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in [†] thousands of pieces of legislation;

In this table we see that over the entire period for which the membership variable is available (considerably longer than for speed preference), the long-term effect of important directives measured as flows remain incorrectly positive while effect of stocks of important directives, though correctly negatively-signed, verges on 0 and is far from statistically significant.

Table A.2. Table 2 omitting years with missing data for preferred speed of unification.

Input	Outcome: Years:	Annual flows of important directives ^T		Δ commission directives ^T	Δ important regulations ^T	Δ important regulations ^T	Δ commission regulations ^T
		From '86 Model B1	From '95 Model B2	From '86 Model C1	From '86 Model D1	From '95 Model D2	From '86 Model E1
Stock of outcome _{t-1}		-0.00 (0.01)	-0.00 (0.01)				
Flow of outcome _{t-1}				-0.69 (0.27)*	-0.38 (0.14)**	-0.37 (0.23) ⁺	-0.65 (0.23)**
Δ preferred speed _{t-1}	-0.02 (0.04)	-0.01 (0.03)	-0.07 (0.04)	-0.10 (0.11)	-0.12 (0.12)	1.89 (1.37)	
Preferred speed. _{t-2}	0.05 (0.03) ⁺	0.10 (0.03)**	-0.04 (0.02)	0.13 (0.08) ⁺	-0.11 (0.12)	-0.55 (0.76)	
Constant	-0.20 (0.16)	-0.42 (0.16)*	0.25 (0.13)*	-0.59 (0.38)	0.49 (0.60)	4.24 (3.92)	
Adjusted R-squared	0.53	0.53	0.25	0.39	0.36	0.35	
Observations	15	13	15	15	13	15	

Notes: Significant at ⁺ 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in ^T thousands of pieces of legislation; ^{TT} millions of lines of legislation.

As explained earlier in this appendix (section A.5 above), missing data arises from the fact that the preferred speed question was not asked in several years relevant to our analyses. When we exclude cases with missing data we get the findings shown in Table A.2 above. Results are very similar to those in Table 2 in the main text. As in Table 2, preferred speed has significant effects on important regulations as well as on important directives (effects on Commission directives have the wrong sign). But the fact that these effects prove non-significant after 1994 (in contrast to the stronger effects of important directives) suggests that such effects as we find of important regulations are artifactual on their strong correlations ($r = 0.54$ – see Table A.0d) with important directives. With intercorrelations such as these it is likely that the more significant of two effects identifies the variable responsible for both effects. Not shown in the Note's Table 2 for lack of space is that, when that Model D is run for years after 1984, it too shows the wrong sign for lagged preferred speed.

Table A.2a Effects on important regulations, and on commission directives and regulations, using specification of Table 2, Model B1 in the text

Input	Outcome:	Annual flows of legislation ^T from 1986		
		Commission directives	Important regulations	Commission regulations
		Model C2a	Model D2a	Model E2a
Cumulative depvar _{t-1}		0.00 (0.02)	0.02 (0.01)	-0.02 (0.01)*
Δpreferred speed _{t-1}		-0.02 (0.03)	0.07 (0.09)	0.25 (0.87)
Preferred speed. _{t-2}		0.01 (0.03)	0.13 (0.10)	-1.25 (0.85)
Constant		-0.06 (0.14)	-0.84 (0.59)	6.84 (4.41)
Adjusted R-squared		-0.07	-0.00	0.12
Observations		21	21	21

Notes: Significant at + 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in ^T thousands of pieces of legislation.

The specification of this table is, strictly speaking, inappropriate for reasons given in the main text. Long-term effects of preferred speed might be driven by common trends that are not allowed for in this table. However, we see that in practice there are no such effects.

Table A,2b Effects of different measures of public opinion on the output of important directives, 1986-2008

Input	Outcome:	Annual flows of important directives ^T from 1986			
		Model B1	Model B1a	Model B1b	Model B1c
Stock of important directives _{t-1}		-0.01 (0.01)	-0.03 (0.01)**	-0.04 (0.01)**	-0.03 (0.01)**
Δpreferred speed _{t-1}		0.04 (0.02)			
Δpreferred speed purged of events _{t-1}			-0.01 (0.03)		
Δpreferred relative speed _{t-1}				-0.02 (0.01)	
Δpreferred relative speed purged of events _{t-1}					-0.01 (0.01)
Input _{t-2}		0.07 (0.03)*	0.00 (0.04)	-0.02 (0.01)	-0.01 (0.02)
Constant		-0.27 (0.15)	0.09 (0.01)***	0.13 (0.03)***	0.10 (0.01)***
Adjusted R-squared		0.45	0.28	0.35	0.29
Observations		21	21	21	21

Notes: Significant at + 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in ^T thousands of pieces of legislation.

In this table, model B1 is the same as in Table 2 in the main text, investigating effects of preferred speed of unification on annual flows of important directives. In Model B1a we investigate the same input purged of the influence of events. In Model B1b we look instead at relative speed preference (preferred speed less actual perceived speed) and in Model B1c we look at that variable purged of the influence of events. None of these alternative models show remotely significant long-term effects of alternative measures of public opinion on legislative output and two of them even have the wrong signs. It seems that the measure that best taps whatever drives changes in policymaking is the uncorrected speed preference measure employed in the main text.

Table A.3 Table 3 omitting years with missing data for preferred speed of unification

Input	Outcome: Span of years:	Annual flows of important directives ^T			
		From '86 Model A1	From '95 Model A2	From '86 Model B1	From '95 Model B2
Δ preferred speed _{t-1}		-0.02 (0.03)	-0.00 (0.03)	-0.02 (0.03)	-0.00 (0.03)
Preferred speed. _{t-2}		0.05 (0.02)**	0.10 (0.03)**		
Preferred speed predicted by linear trend _{t-2}				0.05 (0.02)*	0.10 (0.03)**
Residual preferred speed _{t-2}				0.05 (0.03) ⁺	0.10 (0.03)**
Constant		-0.20 (0.07)*	-0.44 (0.13)**	-0.20 (0.09)*	-0.46 (0.16)**
R-squared		0.57	0.57	0.53	0.52
Observations		15	13	15	13

Note: Significant at ⁺ 0.1, * 0.05, ** 0.01, *** 0.001, one-tailed.

Measured in [†] thousands of pieces of legislation.

Table entries are regression coefficients with the standard errors reported in parentheses.

As explained earlier in this appendix (section 5 above), missing data arises from the fact that the preferred speed question was not asked in several years relevant to our analyses. When we exclude cases with missing data we get the findings shown in Table A.3 above. Results are very similar to those in Table 3.