Online appendix for "A Theory of International Organization"

Our aim is to write a succinct book with a simple message: the tension between scale and community has profound implications for the basic set up of an IO, the range of policies it handles, and the authority it exerts. The empirics should support—not overwhelm—the message, and in this spirit, we choose to keep the analyses in the book sparse and direct.

This appendix is a companion to the empirical chapters in *Community, Scale and International Organization*: chapters 5, 6, and 7. The reader can find here the fully specified models with controls and brief discussions of their (non-)significance. One can also read analyses that test alternative operationalizations of some variables of interest. For example, to what extent does our argument that community facilitates policy expansion depend on a particular operationalization of community? And we formulate and test alternative hypotheses. For example, does trade interdependence lead to an expanding policy portfolio? Does the problem structure—the type of policy problem that an IO manages—affect institutional design? What is the effect of epistemic communities on delegation or pooling?

If, in the end, we appear to travel a road seemingly as straight as an arrow, the reader should know that we only settled on this road after having tried plenty of detours and dead ends. Whether the destination is worthy of the circuitous journey is for the reader to judge.

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Chapter 5: Why do some IOs expand their policy portfolio?

Chapter 5 shows the power of community in shaping the breadth of the policy portfolio. Community—the cultural, geographical, political, and institutional similarity among the members of an IO—facilitates widening the policy portfolio directly, and indirectly, through its effect on the character of the IO contract. We make this argument in two steps: first, we show how community shapes contract, and next we show how both community and contract shape the development of an IO's policy portfolio. In this section, we consider alternative operationalizations of community, including disaggregating community into its components, as well as alternative model specifications.

This section examines also a contending explanation having to do with trade. We conceptualize and operationalize trade in two ways: understood as a formal policy competence of an IO, and understood as the extent of trade interdependence among an IO's members.

How community shapes contract

Table B.1 (B=online appendix) shows the full results of the multivariate models in Table 5.3 of the book, which examine how the extent of community among IO members shapes what kind of contract they conclude. The analysis corroborates the very strong association between community and contract and the weak impact of other factors.[1]

The first column is a cross-sectional between-effects regression, in which we control for democracy, power asymmetry, affluence and GDP dispersion (model 2 in Table 5.3). The second column shows a fixed-effects regression with the same controls (using one-year lags) and a year count to address pressures of time in an unbalanced panel (model 4 in Table 5.3). As a robustness test we substitute a fractional polynomial procedure of time for the year count (column 3). A fractional polynomial estimates the pressures of time inductively through a number of fractional transformations of the time variable. We use the default option of estimating two dimensions, and we do not impose a scaling on the year variable.

Since we are interested in how the policy portfolio develops over time as a function of its contractual and communal basis, we add a time dimension to our contract and community measures beyond the over-time variation that these variables exhibit. The baseline specification is the raw measure of contract/ community multiplied by the

[1] High collinearity between membership size and community precludes including members as a control.

age of the international organization (1 for the first year, 2 for the second year, 3 for the third, and so on).

Table B.1: Community and contract, 1950-2010

DV=contract dynamic	Crosssectional	Over time	Over time (fractional polynomial)
Community dynamic: factor	0.212***	0.124***	0.126***
	(0.033)	(0.026)	(0.027)
Democracy	–0.060	0.320	0.119
	(0.330)	(0.326)	(0.347)
Power asymmetry	–18.676**	–6.377	–10.219
	(7.432)	(10.515)	(11.632)
Affluence	–0.892***	–0.316	–0.373
	(0.248)	(0.211)	(0.241)
GDP dispersion	0.505	0.699*	0.651*
	(0.435)	(0.356)	(0.370)
Year count		1.104***	
		(0.097)	
Year polynomial 1			1.002***
			(0.115)
Year polynomial 2			0.000
			(0.000)
Constant	34.547***	–11.060	–5.300
	(4.738)	(4.701)	(7.013)
Observations	76 IOs	3203 IO-years	3203 IO-years
R^2 between	0.442		
R^2 within		0.920	0.921

Note: The cross-sectional model is a between-effects regression; the temporal model is a fixed-effects estimation with standard errors clustered by IO (and independent variables lagged by one year). *** p<0.01, ** p<0.05, * p<0.1. The last column reports results applying a fractional polynomial procedure which inductively fits time, whereby the maximum degree of the fractional polynomial to be searched is set at two dimensions.

Alternative operationalization of community

How robust is the effect of community on contract across an alternative specification of community? Table A.2 below uses *Historical Ties* as an indicator of community. An IO is categorized as having historical ties when at least two-thirds of the founding member states 1) share a history of membership within a federation, or 2) share experience of membership within—and resistance to—a colonial empire. *Historical ties* captures bonds

that were forged and sundered prior to the design of the IO, so these are exogenous to the decision to create or develop an IO.

Table B.2: Historical ties and contract, 1950-2010

	DV=contract dynamic				
	Cross-sectional		Over time		
Historical dynamic	0.807***	0.760***	2.169***	0.833***	0.826***
	(0.188)	(0.196)	(0.367)	(0.152)	(0.156)
Democracy		0.450		0.421	0.273
		(0.366)		(0.293)	(0.351)
Power asymmetry		–13.517		–13.368	–17.835
		(10.560)		(9.921)	(10.788)
Members		–4.873		–7.130	–8.790
		(3.972)		(7.570)	(8.001)
Affluence		–0.717**		–0.066	–0.115
		(0.285)		(0.193)	(0.203)
GDP dispersion		0.302		0.299	0.286
		(0.516)		(0.264)	(0.284)
Year count				1.248***	
				(0.111)	
Year polynomial 1					1.185***
					(0.114)
Year polynomial 2					0.000
					(0.000)
Constant	28.775***	40.478***	29.730***	–1.762	3.598
	(1.664)	(9.840)	(0.979)	(11.819)	(14.161)
Observations	76 IOs		3203 IO-years		
Controls	NO	YES	NO	YES	YES
R^2 between	0.200	0.296			
R^2 within			0.175	0.914	0.915

Note: The cross-sectional models are between-effects estimations; the temporal models are fixed-effects estimations with standard errors clustered by IO (independent variables are lagged by one year). *** p<0.01, ** p<0.05, * p<0.1. The last column reports results applying a fractional polynomial procedure which inductively fits time, whereby the maximum degree of the fractional polynomial to be searched is set at two dimensions.

The argument is that historical ties produced by a common political past can provide a normative foundation for congruent expectations regarding the intentions and behavior of others, and that this can ease broad-ranging international cooperation (see Book Appendix for conceptualization). In operationalizing the variable, we again model a time

dimension: we multiply *Historical ties* by the age of the IO, and we impose a decay factor on the intuition that the longer ago these historical ties ceased to exist the weaker the glue.[2] The ensuing variable is *Historical dynamic*. As Table B.2 shows, historical ties explains about one-fifth of cross-sectional and temporal variation in *Contract dynamic*, and this effect is robust under controls.

Hence, we can be relatively confident that the extent of community—whether operationalized as a factor of cultural, geographical or institutional features, or as shared political history—shapes the type of IO contract.

Disaggregating community

A possible objection is that the strong results of community could be an artifact of the composite measure. Are the results robust across each of the five components that feed into the community factor? To test this, we re-run the cross-sectional and temporal analysis for each of the five components. For sure, the association is strongest for the full factor, but each of the five components on its own is significantly and positively associated with having a broad-ranging contract. Table B.3 reports significance levels, and the t-value for the community component, and total variance explained for a model containing all controls.

Table B.3: Community's components and contract, 1950-2010

	DV=contract dynamic					
	Cross-sectional			Over time		
	p-value	*t-value*	R^2	*p-value*	*t-value*	R^2
Religion	**	2.61	0.19	***	4.49	0.92
Culture	**	2.12	0.16	***	3.48	0.91
Geography	**	2.54	0.18	***	5.91	0.93
Political regime	***	3.00	0.21	***	3.02	0.90
Legal tradition	***	2.93	0.21	***	3.95	0.91
Community factor	***	6.45	0.44	***	4.71	0.92

Note: The cross-sectional models are between-effects estimations; the temporal models are fixed-effects estimations with standard errors clustered by IO (independent variables are lagged by one year). *** p<0.01, ** p<0.05, * p<0.1.

[2] The formula for calculating the decay factor is y= e^ (-(inception of IO-final year of historical ties)/25) if *Historical ties*=1; otherwise y=0. *Historical dynamic* is then y*age of IO.

Alternative modeling

The models above use OLS regression, but logit is a plausible alternative modeling choice because the dependent variable is sluggish and, if we do not apply a time factor, it is nearly perfectly dichotomous (McCullagh 1980). So now we simplify the dependent variable to a dichotomous variable, *Contract,* with the chief independent variable being either the *Community factor* or *Historical ties*.

Table B.4 reports a logistic analysis using the community factor and historical ties respectively. For each operationalization, the first column shows log-odds and the next column calculates the percentage change in odds for a one standard deviation shift in each independent variable while holding all others constant. We use Scott and Freeze's *listcoef* command in Stata to estimate these effects (Long and Freese 2014).

Table B.4: A logistic analysis of community and contract, 1950-2010

DV=Contract	Log ratios	% change in odds for a standard deviation increase in X	Log ratios	% change in odds for a standard deviation increase in X
Community: factor	2.101***	336.4%		
	(0.464)	(1.984)		
Community: historical ties			12.049***	154.8%
			(10.808)	(0.376)
Democracy	0.894	−43.3%	0.917	−35.8%
	(0.066)	(5.078)	(0.061)	(5.078)
Members	--	--	0.122**	−69.7%
			(0.102)	(0.553)
Power asymmetry	0.703	−6.7%	0.385	−17.1%
	(1.372)	(0.197)	(0.833)	(0.197)
Affluence	0.936**	−42.3%	0.942**	−39.5%
	(0.026)	(8.356)	(0.025)	(8.356)
GDP dispersion	1.708	396.2%	1.627	329.5%
	(0.723)	(2.993)	(1.111)	(2.993)
Constant	3.412		44.458	
	(4.155)		(89.527)	
Observations	3279		3279	
Pseudo R^2	0.309		0.341	
Hosmer-Lemeshow test	6/7 groups with p>.10		7/8 groups with p>.10	

Note: The first and third columns report log odds ratios (cluster-corrected by IO). The second and fourth column calculate the percentage change in odds to move to an incomplete contract for a one standard deviation shift in the independent variable holding all others at their means (standard deviation in brackets). **** p<0.01, ** p<0.05, * p<0.1*

Community is the most significant variable in the model, and its substantive effect is considerable. For a one standard deviation (1.984) increase in the Community factor, we expect a 336 percent increase in the odds of shifting from a complete to an incomplete contract. For a one standard deviation shift in Historical ties (0.376), there is a 155 percent increase in the odds of shifting from a complete to an incomplete contract. Note also that all but one control variables are negative once we account for community. The exception is GDP dispersion, our estimate for economic interest divergence among member states, which is positively signed but not significant.

The last row reports the Hosmer-Lemeshow test goodness-of-fit statistic which is often used to assess whether a logistic model is correctly specified. The test splits the sample into groups depending on their predicted probabilities and assesses to what extent, for each subgroup, the model predicts outcomes reasonably close to the real outcomes. The optimal number of groups is the number of covariates increased by two (seven and eight respectively in our case) (Bartlett 2014; Hosmer and Lemeshow 2000). The results suggest that for all but one group the difference between predicted and actual outcomes is not significant. Further diagnostics indicate that the analysis is not vulnerable to outliers.[3]

How community and contract shape policy expansion

This section presents the full models (including controls) discussed in the book; they corroborate how community and contract facilitate the expansion of an IO's policy portfolio. We also report a two-stage model that subjects our two-stage to the text: community facilitates incomplete contracting, and incomplete contracting facilitates an expanding policy portfolio. And finally, we evaluate whether our explanation is robust if we model the dependent variable, *Policy scope*, as a count variable.

Table B.5 reports the control variables in models that probe how change in community or contract, or both, affect change in policy scope (Table 5.4. in Chapter 5).[4] Models 1–3 use fixed effects with controls for democracy, power asymmetry, membership size, affluence, GDP dispersion, and a year count. Model 4 also includes an indicator for foreign policy preferences (*Ideal points)*, which is often used in the literature to estimate preference heterogeneity. Models 5 and 6 are robustness checks to modeling time: model 5 excludes a control for time pressures to minimize collinearity, and model 6 applies a

[3] The United Nations is most extreme: it has an incomplete contract though the model predicts a 0.13 probability of having such contract, on average, across the time period (the UN's peak probability was 0.184 in 1972). By implication, its average Pearson standardized residual is high (2.79) as is the average deviance residual (2.05). However, its average leverage value (0.028) is well below the population mean (0.076), which implies that its outlier status does not bias the overall model. These statistics are calculated using the community factor model in Table B.3.

[4] Estimates of an IO's policy scope are annual. The Appendix in the book describes the coding protocol we applied to assess change in an IO's policy portfolio.

fractional polynomial that fits time pressures inductively by estimating two dimensions and adding these estimates to the model as controls. Model 7 includes a lagged dependent variable.

The results suggest that the extent of community and the incompleteness of an IO's contract – in tandem or separately – influence the dynamic character of the policy portfolio. The coefficients for *Community* and *Contract* are impressively robust across a range of model specifications. They also survive under bootstrapping (drawing 50 random samples from the data) and jackknifing (sequentially dropping an IO panel) (results not shown).

Once one takes into account community and contract, few IO characteristics reliably shape an IO's policy development. *Democracy* is the only control that is, most of the time, associated with the expansion of an IO's policy portfolio. The more democratic the membership of an IO, the more dynamic its policy portfolio tends to be. IOs composed of rich or poor states, large or small members, egalitarian or dominated by a hegemon, similar or divergent interests, may or may not develop a dynamic policy portfolio. What appears to determine their path is whether their contract is incomplete and whether their members share communal norms.

Table B.5: Community and contract on policy scope

	DV = Change in policy scope						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Community dynamic $_{t-1}$	0.030***		0.015**	0.015**	0.016**	0.016***	0.002**
	(0.007)		(0.006)	(0.006)	(0.007)	(0.006)	(0.001)
Contract dynamic $_{t-1}$		0.140***	0.111***	0.111***	0.062***	0.108***	0.008**
		(0.031)	(0.031)	(0.031)	(0.019)	(0.030)	(0.003)
Democracy $_{t-1}$	0.207**	0.137	0.157*	0.152*	0.063	0.098	0.017*
	(0.088)	(0.083)	(0.081)	(0.078)	(0.078)	(0.094)	(0.010)
Members $_{t-1}$	4.071	1.003	2.964	3.007	–0.338	2.587	0.148
	(2.637)	(1.796)	(2.143)	(2.158)	(1.713)	(2.282)	(0.218)
Power asymmetry $_{t-1}$	1.243	1.246	1.600	1.608	2.827	/0.303	–0.114
	(2.272)	(2.094)	(1.951)	(1.991)	(2.173)	(2.070)	(0.227)
Affluence $_{t-1}$	–0.069	–0.004	–0.037	–0.040	–0.106***	–0.061	–0.007
	(0.044)	(0.045)	(0.041)	(0.042)	(0.034)	(0.047)	(0.005)
GDP dispersion $_{t-1}$	0.001	–0.074	–0.072	–0.074	–0.016	–0.089*	–0.010
	(0.066)	(0.050)	(0.050)	(0.050)	(0.072)	(0.053)	(0.012)
Ideal points				–0.321	0.003	–0.434	–0.007
				(0.521)	(0.591)	(0.523)	(0.063)
Year count	–0.013	–0.128**	–0.125***	–0.126	--		–0.008*
	(0.035)	(0.049)	(0.046)	(0.046)			(0.005)
Scope $_{t-1}$							0.917***
							(0.012)
Year polynomial 1						–0.015***	
						(0.004)	
Year polynomial 2						0.003***	
						(0.001)	
Constant	–4.064	1.840	–1.073	–0.801	2.171	0.807	0.134
	(4.067)	(2.867)	(3.358)	(3.182)	(3.187)	(4.034)	(0.286)
R^2 within	0.383	0.452	0.477	0.478	0.431	0.490	0.920
AIC	12256	11873	11726	11724	11999	11650	5712

Note: N=3203 IO-year (76 IOs) for 1950-2010. Fixed effects estimation with standard errors clustered by IO. *** p<0.01, ** p<0.05, * p<0.1. Column 5 reports results without controlling for time; column 6 reports a fractional polynomial that inductively fits two dimensions of time.

A two–stage model of community, contract, policy scope

Our argument suggests a sequential path to changing policy scope. Incomplete contracting provides the flexibility necessary to adapt an IO's policy portfolio to new circumstances, and it is itself made possible by shared norms that underpin diffuse reciprocity. A parsimonious way to model this argument is through a two-stage model that, first, evaluates the extent to which community explains contract, and second, estimates the effect of contract on a changing policy portfolio (Table B.6). The analysis substantiates that a) community is the chief factor explaining contract, and b) contract mediates how community shapes change in an IO's policy portfolio.

Table B.6: Two-stage fixed effects OLS regression

	First stage DV=Contract dynamic	Second stage DV=Policy scope
Community dynamic $_{t-1}$	0.134***	
	(0.029)	
Contract dynamic $_{t-1}$		0.222***
		(0.048)
Democracy $_{t-1}$	0.448	0.107
	(0.306)	(0.087)
Members $_{t-1}$	9.984	1.851
	(7.952)	(1.798)
Power asymmetry $_{t-1}$	–2.858	1.878
	(11.450)	(2.400)
Affluence $_{t-1}$	–0.281	–0.006
	(0.201)	(0.047)
GDP dispersion $_{t-1}$	0.659**	–0.145**
	(0.310)	(0.056)
Year count	1.004	–0.236***
	(0.099)	(0.069)
Constant	–26.978	
	(12.736)	
R^2	0.921	0.383
Kleibergen-Paap Wald rk F statistic[a]	21.23	
F-statistic		9.62
Wald F p-value	0.000	0.000
Stock-Yogo test[b]	H0 rejected	

Note: N=3203 IO-year (76 IOs) for 1950-2010. Fixed effects estimation with standard errors clustered by IO. *** p<0.01, ** p<0.05, * p<0.1.

[a] The Kleibergen-Paap Wald F-statistic probes the null hypothesis that the excluded instruments are weakly associated with the endogenous variable (Kleibergen and Paap 2006). The higher the F-statistic, the more the instrument is well correlated with the endogenous variable.

[b] The Stock-Yogo test probes the null hypothesis that the instrument is weak, where weakness is estimated as the size of the bias of the IV estimator relative to the Kleibergen-Paap F test at a set threshold (Stock and Yogo 2005). Rejecting the null hypothesis at any level signifies that the instrument is *not* weak; rejecting it at the 10 percent level indicates that the instrument is somewhat weak.

A count model

Policy scope ranges from 1 to 25, and it could be treated as a count variable, for which count models such as Poisson or negative binomial are appropriate modeling choices. *Xtpoisson* (left column) allows for a conditional fixed-effects specification with standard errors clustered at the IO-year level. *Xtpoisson* tends to produce less biased parameters than a Poisson with panel dummies (Allison 2009).[5] The independent variables are lagged by a year, and a year count is used to address pressures of time in an unbalanced panel.

Table B.7: Poisson count model for policy scope

	Model 1	Model 2	Model 3
Community dynamic $_{t-1}$	0.003***		0.002**
	(0.001)		(0.001)
Contract dynamic $_{t-1}$		0.008***	0.004
		(0.003)	(0.003)
Democracy $_{t-1}$	0.021*	0.018	0.020
	(0.013)	(0.013)	(0.013)
Members $_{t-1}$	0.600**	0.272	0.558**
	(0.260)	(0.235)	(0.266)
Power asymmetry $_{t-1}$	0.403	0.361	0.418
	(0.276)	(0.302)	(0.277)
Affluence $_{t-1}$	–0.007	–0.002	–0.006
	(0.005)	(0.005)	(0.005)
GDP dispersion $_{t-1}$	–0.008**	–0.008*	–0.009**
	(0.004)	(0.004)	(0.004)
Year count	0.003	–0.001	–0.001
	(0.004)	(0.005)	(0.004)
Wald Chi-squared	102.34	85.89	102.88
AIC	10694	10727	10689

Note: N=3203; Poisson fixed effects model with standard errors clustered by IO.

[5] Diagnostics suggest that overdispersion is not an issue, and Poisson is preferable to a negative binomial specification. Since the count does not include zeros, a zero-inflated Poisson might have been best, but Stata does not provide this option for time series panel data.

The results are consistent with the difference that only one of the community variables attains significance in the full model. Community dynamic or Contract dynamic are each significant on their own. Democracy, Members attain significance in some models. In Model 1, IOs with members that become, on average, more democratic tend to expand their policy portfolio. In Model 1 and 3, IOs that enlarge membership tend to expand their policy portfolio. GDP divergence suppresses policy expansion in all models.

The argument that community influences the dynamic character of the policy portfolio is robust across alternative indicators of community as well as alternative model specifications. In the section below, we contrast the community-oriented explanation with its most plausible rival: the problem structure of trade IOs.

An alternative explanation: trade

Trade is a cross-cutting (or horizontal) activity which affects many issues from environment and transport and communications networks to health and labor rights. It seems therefore plausible that it provides powerful incentives to broaden the policy portfolio. Trade—not the contract or community—may drive policy dynamism. We examine two versions of this contending hypothesis.

The first is that IOs with a trade mandate are more likely to develop a more dynamic policy portfolio. We test this in Table B.8, an extended version of Table 5.5 in Chapter 5, which reports fixed effects estimations with Trade dynamic alongside controls for democracy, power asymmetry, membership, affluence, GDP dispersion and year count, and a full model that also includes Community dynamic and Contract dynamic. We find a significant effect of trade (model 1 and 2). In Model 4, we substitute fractional polynomial estimates of time for the standard year count, and we find, as is often the case with a polynomial time estimation, that the coefficients of our chief variables of interests gain marginally in precision. Note also that GDP dispersion now becomes significant at 0.10 level. Model 4 adds a lagged dependent variable, and Trade dynamic becomes insignificant.

Table B.8: Community-contract and Trade Models

	DV=Policy scope			
	(1)	(2)	(3)	(4)
Trade dynamic $_{t-1}$	0.106***	0.050**	0.049**	0.002
	(0.026)	(0.020)	(0.019)	(0.002)
Community dynamic $_{t-1}$		0.011*	0.011*	0.002**
		(0.006)	(0.006)	(0.001)
Contract dynamic $_{t-1}$		0.093***	0.091***	0.007**
		(0.028)	(0.027)	(0.003)
Democracy $_{t-1}$	0.127	0.132*	0.085	0.016
	(0.087)	(0.079)	(0.096)	(0.010)
Members $_{t-1}$	1.326	2.973	2.598	0.152
	(2.479)	(2.138)	(2.229)	(0.214)
Power asymmetry $_{t-1}$	1.363	1.834	0.592	–0.102
	(2.371)	(1.923)	(1.976)	(0.232)
Affluence $_{t-1}$	–0.029	–0.041	–0.060	–0.007
	(0.046)	(0.041)	(0.044)	(0.005)
GDP dispersion $_{t-1}$	–0.011	–0.078	–0.091*	–0.010
	(0.061)	(0.048)	(0.050)	0.012
Year count	0.017	–0.110**		–0.008
	(0.030)	(0.044)		(0.005)
Year polynomial 1			–0.013***	
			(0.003)	
Year polynomial 2			0.003***	
			(0.001)	
Policy scope $_{t-1}$				0.916***
				(0.013)
Constant	0.002	–1.068	0.345	0.125
	(3.956)	(3.416)	(4.106)	(0.297)
R^2 within	0.392	0.500	0.511	0.920
AIC	12208	11586	11516	5710

Note: N =3203 IO-year (76 IOs) for 1950-2010. Fixed effects estimation with standard errors clustered by IO. *** p<0.01, ** p<0.05, * p<0.1.

Table B.9 reports the effect on policy scope of a one-standard deviation change in *Trade, Community*, or *Contract dynamic* holding all other variables at their means as well as a shift from the 5th to the 95th percentile (using Model 2 in Table B.8). The substantive effects of Trade and Community are similar; that of Contract is almost three times that of Trade or Community.

Table B.9: Substantive effects of trade, community, and contract

	Range	Standard deviation (SD)	Effect of 1 SD shift on policy scope	Effect of a shift from 5th to 95th percentile
Trade dynamic	0—61	16.21	0.81	2.31
Community dynamic	0.03—352.4	68.97	0.74	2.21
Contract dynamic	1—122	25.46	2.38	7.65

Note: estimated under full controls (Model 2 in Table B.8).

A second argument links trade interdependence to IO design. We constrain the sample to IOs that have a mandate in trade on the intuition that it is reasonable to expect the logic of trade interdependence to work most strongly among such IOs. This reduces the sample to thirty-four IOs that have trade as a policy competence, are geographically delimited, and for which we could calculate trade interdependence data (since 1970).

We test the argument with three operationalizations of trade interdependence: intra-IO trade, trade intensity, and trade introversion, which are described in the appendix of the book. Each measure compares internal trade dynamics among IO members with those of third parties, and so it makes most sense to use these measures for IOs with a membership that falls significantly short of the global population. That is why we are testing the trade interdependence hypothesis on a subsample. Table B.10 below depicts how this sample—gray shaded—compares with the entire sample of 76 IOs used in Table 5.5 in Chapter 5.

Table B.10: IOs by trade interdependence and trade policy in 2010

	Are there data on trade interdependence?		
Does the IO have competence in trade?	**No**	**Yes**	
No	34	0	34
Yes	8[a]	34[b]	42
	42	34	76

[a] Crossregional or global IOs with trade competence (IMO, OECD, OIC, PCA, WCO, WIPO, WTO) or regional IOs without trade data (CCNR). [b] IOs in sample: AU, ALADI, AMU, APEC, ASEAN, Benelux, CAN, Caricom, CEMAC, CIS, Comecon, Comesa, EAC1, EAC2, EEA, ECCAS, ECOWAS, EFTA, EU, GCC, IGAD, LOAS, Mercosur, NAFTA, Nordic, OAS, OECS, PIF, SAARC, SACU, SADC, SCO, SELA, SICA.

The first column in Table B.11 produces the model with *Community dynamic* and *Contract dynamic* under controls for the 34 IOs. It establishes that, for this subsample as for the entire sample, the character of the IO contract is positively associated with an expanding policy portfolio. The more distal estimate of *Community* loses significance. These results stand when controlling for *Trade policy* and for a lagged dependent variable (results not shown). This constitutes the baseline for our inquiry into trade interdependence.

The next three models show that, in bivariate fixed effects models, two of the three trade independence measures reach significance, but their explanatory power is weak. In subsequent models we first add controls for democracy, members, power asymmetry, affluence, GDP dispersion and the year count (model 4), and then we add *Community dynamic* and *Contract dynamic* (model 5). We run the analysis for each of the three trade interdependence indicators, but we only report the model with the best AIC. However, the results are consistent across specifications, with the only difference being that membership and power asymmetry sometimes become significant.

The chief take-away is that trade interdependence washes out as soon as we control for confounding factors, and IO contract is significant whether we include trade interdependence or not.

Table B.11: Trade interdependence and policy scope

		DV=Policy scope				
	Base line	(1)	(2)	(3)	(4)[a]	(5)[a]
Community dynamic $_{t-1}$	0.022					0.023
	(0.026)					(0.026)
Contract dynamic $_{t-1}$	0.182**					0.179**
	(0.069)					(0.069)
Trade interdependence						
Intra-IO trade $_{t-1}$		0.266**				
		(0.129)				
Trade intensity $_{t-1}$			0.000			
			(0.000)			
Trade introversion $_{t-1}$				6.645***	0.947	0.522
				(1.698)	(1.522)	(1.488)
Democracy $_{t-1}$	0.203				0.118	0.202
	(0.161)				(0.167)	(0.163)
Members $_{t-1}$	10.446**				11.777***	10.545**
	(4.817)				(4.260)	(4.718)
Power asymmetry $_{t-1}$	11.196***				11.818***	11.111***
	(3.286)				(4.035)	(3.249)
Affluence $_{t-1}$	−0.058				−0.069	−0.056
	(0.073)				(0.064)	(0.074)
GDP dispersion $_{t-1}$	−0.143**				−0.105	−0.139**
	(0.060)				(0.053)	(0.058)
Year count	−0.235				0.200***	−0.236
	(0.198)				(0.049)	(0.198)
Constant	−6.357	6.680***	10.882***	6.415***	−15.967**	−6.647
	(4.842)	(2.051)	(0.015)	(1.145)	(6.014)	(4.820)
Controls	YES	NO	NO	NO	YES	YES
R^2 within	0.578	0.074	0.000	0.101	0.537	0.579
AIC	4169	4966	5041	4937	4263	4169

Note: 971 IO-year (n=34 IOs); [a] Model with best Akaike information criterion (AIC) of three trade models. Fixed effects with standard errors clustered by IO. *** p<0.01, ** p<0.05, * p<0.1.

The strong *positive* effects of power asymmetry and membership size in the fully specified models (B.11: base, models 4 and 5) are surprising. We did not find significance for either factor in the sample of 76 IOs. Table B.12, which adds controls step-wise, reveals that these findings for the subset of 34 trade IOs are not robust across alternative model specifications.

The effect of membership size is driven by a single case, the European Union, which is exceptional in that it combines by far the largest increase in membership (from six to twenty-eight members) and the steepest growth in its policy portfolio (from 4 to 24 policies). When we exclude the EU (model 5) or apply jackknifing (model 6), which drops each IO panel sequentially and then estimates average model fit, membership size is no longer significant.

The effect of power asymmetry is less easy to dismiss. One interpretation is that the finding is spurious on account of the observation that the association is not robust under alternative specifications. Indeed, while the bivariate correlation between change in power asymmetry and change in policy scope is negative (r=–0.33), the variable switches sign and gains significance under full controls. An alternative interpretation is that this is consistent with hegemonic stability theory. Hegemonic stability theory posits that a hegemon is willing to unilaterally create a public good whose benefits are joint across all states (Snidal 1994). The implicit assumption is that states agree on the value of the public good, so that there is some meeting of the minds in norms or interests. If those conditions are not met, a hegemon may not want to make the investment. A closer look at the 34 IOs suggests that this sample may meet these conditions. Table B.13 below contrasts the trade IO subsample with the non-trade IOs on two heterogeneity estimates: community and foreign policy preferences. We also report the mean in power asymmetry. To aid comparison, all measures are rescaled from zero to one. As a group, the 34 IOs are considerably more culturally and geopolitically homogeneous, and at the same time, they are much more likely to have an outsized member state capable and willing to unilaterally invest in public goods creation. A difference of means test confirms that each difference is highly significant.

Table B.13: Trade IOs, power asymmetry and heterogeneity

	Community	Foreign policy preferences[a]	Power asymmetry
34 trade IOs	0.668	0.810	0.480
Other 42 IOs	0.221	0.659	0.271

Note: [a]*Ideal points*, which is rescaled from zero to 1 and reversed so that higher values indicate similarity.

Table B.11 and B. 12 reinforce our conclusion about the causal power of the IO contract. We also note the consistent finding that GDP dispersion is negatively associated with policy scope. This means that trade IOs with economically similar members are more likely to have a dynamic policy portfolio. This effect is significant even when one controls for normative similarity (community). This gels well with our conjecture that similarity of interests is one thing, and normative commonality another.

Table B.12: Trade interdependence and policy scope—additional models

	DV= change in policy scope						
	Base line	(1)	(2)	(3)	(4)	(5) without EU	(6) jackknife
Contract dynamic $_{t-1}$			0.162***	0.154**	0.168**	0.165**	0.168*
			(0.050)	(0.056)	(0.064)	(0.064)	(0.096)
Trade interdependence			0.879	0.469	0.237	–0.146	0.237
			(1.494)	(1.480)	(1.507)	(1.504)	(2.005)
Democracy $_{t-1}$					0.178	0.202	0.178
					(0.161)	(0.164)	(0.184)
Members $_{t-1}$		9.031**	6.578*	5.601	9.455**	5.700	9.455
		(3.952)	(3.841)	(4.441)	(4.229)	(3.846)	(5.844)
Power asymmetry $_{t-1}$	–7.866*	4.958	5.589	5.889	11.981***	10.812**	11.982**
	(4.202)	(4.599)	(4.566)	(5.255)	(3.948)	(4.372)	(4.795)
Affluence $_{t-1}$				–0.082	–0.044	–0.081	–0.044
				(0.073)	(0.072)	(0.063)	(0.105)
GDP dispersion $_{t-1}$				–0.017	–0.132**	–0.110**	–0.132**
				(0.066)	(0.053)	(0.051)	(0.105)
Year count		0.194***	-0.099	–0.044	–0.115	–0.098	–0.115
		(0.037)	(0.069)	(0.099)	(0.125)	(0.124)	(0.194)
Constant	14.491***	–8.485	–1.436	–1.644	–8.060	–4.337	–8.060
	(1.919)	(5.910)	(5.020)	(6.126)	(5.121)	(5.205)	(6.608)
R^2 within	0.03	0.494	0.530	0.542	0.570	0.551	0.570

Note: 971 IO-year (n=34 IOs *** p<0.01, ** p<0.05, * p<0.1.

Chapter 6: The Resistible Rise of International Authority

The course of an IO's authority is the result of two distinct causal mechanisms: a primarily functionalist mechanism in which authority responds to, and perhaps reinforces, change in an IO's policy portfolio, and a political mechanism in which IO authority is swept up and contained in public debate—politicization. Community and scale are both present, and both are powerful. We also show that community shapes change in authority indirectly through its impact on policy scope. The models control for confounding factors: democracy, power asymmetry, membership size, affluence, GDP dispersion, a year count, and core state powers. In addition, we consider three arguments that are prominent in the literature:

- IOs that focus on *core state powers* are theorized to be more reluctant to increase delegation or pooling. This relates to an argument about the role of problem structure in international governance. We discuss the argument, different options for operationalizing the concept, and report analyses.
- IOs that empower *epistemic communities* are theorized to be more insulated from the negative effects of politicization on delegation and pooling. We discuss the argument and report results that give partial credence to it.
- *Interest similarity* among IO members may facilitate delegation or pooling in order to reap the benefits of closer cooperation (Martin 1995; Ostrom and Keohane 1995; Snidal 1995). We test several indicators of interest similarity, including GDP dispersion, foreign policy divergence, and trade interdependence. We find no significant effect.

An addendum to this section provides more detail on our operationalization of politicization.

Policy expansion deepens authority, politicization dampens it

Table B.14 reports the full results of the fixed effects models in Table 6.2. This confirms that expansion of the policy portfolio is a chief factor in deepening delegation and pooling, while politicization dampens delegation and pooling.

Note that virtually no controls reach significance in the models. Only *Democracy* has a positive effect on pooling. This effect is significant at the 0.01 level in the full model, and at the 0.05 level in the model without the year count.

The year count—both the simple version and the polynomial function—reaches significance in the case of delegation. This has no substantive meaning other than hinting at a potential aggregate time trend that could, if uncontrolled for, artificially amplify some causal relationships. A year count is meant to correct for this, but sometimes this may overcompensate in depressing estimates. That does not appear to be the case here: models

with and without the year count produce nearly identical estimates. The effects we capture seem neither overly liberal nor overly conservative.

Table B.14: Explaining change in delegation and pooling (full controls)

	DV = change in delegation			DV = change in pooling		
	(1)	(2)	(3)	(1)	(2)	(3)
Policy scope $_{t-1}$	0.015***	0.016***	0.015***	0.010***	0.010***	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Politicization $_{t-1}$	−0.011**	−0.012**	−0.012**	−0.018***	−0.018***	−0.018***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
Democracy $_{t-1}$	0.001	0.003	−0.001	0.005*	0.006**	0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Members $_{t-1}$	−0.034	0.026	−0.051	−0.000	0.031	0.001
	(0.036)	(0.031)	(0.035)	(0.026)	(0.030)	(0.032)
Power asymmetry $_{t-1}$	0.052	0.018	0.026	0.003	−0.015	0.002
	(0.053)	(0.048)	(0.065)	(0.049)	(0.050)	(0.048)
Affluence $_{t-1}$	−0.001	0.000	−0.002	−0.001	0.000	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
GDP dispersion	−0.003	-0.001	−0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Core state powers $_{t-1}$	−0.017	−0.021	−0.015	−0.013	−0.015	−0.014
	(0.026)	(0.027)	(0.026)	(0.019)	(0.019)	(0.019)
Year count	0.001***	--		0.001	--	
	(0.001)	--		(0.000)	--	
Year polynomial 1			0.011			−0.048
			(0.008)			(0.032)
Year polynomial 2			0.000**			0.000
			(0.000)			(0.000)
Constant	0.084	0.013	0.131	0.166***	0.128**	0.189**
	(0.057)	(0.058)	(0.080)	(0.049)	(0.060)	(0.079)
R^2 within	0.412	0.399	0.422	0.256	0.251	0.258
AIC	−10858	−10790	−10911	−10269	−10254	−10276
F-statistic	6.19	6.52	6.38	5.81	5.81	5.40

Note: Fixed effects estimations with standard errors clustered by IO. N=3199 IO-year (76 IOs) for 1950-2010. The dependent variables *Change in delegation* and *Change in pooling* vary between −1 and 1. *** p<0.01, ** p<0.05, * p<0.1. Controls for democracy, members, power asymmetry, affluence, GDP dispersion, core state powers and a year count.

Table B.15 reports the two-stage models summarized in Table 6.3 of the book. A two-stage model can address potential endogeneity by instrumenting *Community* and *Contract* for *Policy scope*. More importantly, it takes the analysis one step back into the causal chain to suggest that community facilitates the expansion of the policy portfolio, which in turn is a key

determinant of changing authority. The left column reports the first-stage results, and the two columns on the right report second-stage results with respect to delegation and pooling respectively. We employ *xtivreg2* to produce estimates. Hence the analysis connects chapters 5 and 6.

Table B.15: Two-stage model explaining change in delegation and pooling

	First stage	Second stage	
	DV=Change in policy scope	**DV=Change in delegation**	**DV=Change in pooling**
Community dynamic	0.012**		
	(0.005)		
Contract dynamic	0.105***		
	(0.024)		
Policy scope $_{t-1}$ instrumented		0.026***	0.018***
(instruments: community, contract)		(0.006)	(0.006)
Politicization$_{t-1}$	−0.000	−0.014*	−0.021***
	(0.365)	(0.007)	(0.006)
Democracy $_{t-1}$	0.096	−0.003	0.004
	(0.060)	(0.002)	(0.003)
Members $_{t-1}$	2.566*	−0.030	0.002
	(1.431)	(0.043)	(0.028)
Power asymmetry $_{t-1}$	1.901	0.045	−0.002
	(1.536)	(0.062)	(0.052)
Affluence $_{t-1}$	−0.040	−0.001	−0.001
	(0.033)	(0.001)	(0.001)
GDP dispersion	−0.089**	−0.001	0.001
	(0.034)	(0.001)	(0.002)
Core state powers $_{t-1}$	3.891***	−0.063**	−0.049*
	(0.884)	(0.030)	(0.029)
Year count	−0.107***	0.001	0.000
	(0.030)	(0.001)	(0.001)
R^2 within	0.595	0.331	0.205
F-statistic		6.28	5.23
Kleibergen-Paap Wald rk F statistic[a]	20.83		
Wald F p-value	0.0001		
Stock-Yogo test[b]	F>19.93 at 10%: H_0 rejected		
Hansen J Chi^2 of overidentification[c]		0.911	0.022
Chi^2 p-value		0.340	0.881

Note: Fixed effects estimations with standard errors clustered by IO. N=3199 IO-year (76 IOs) for 1950-2010. [a]The Kleibergen-Paap Wald F-statistic tests the null hypothesis that the instruments are weakly associated with the endogenous variable (Kleibergen and Paap 2006; Cragg and Donald 1993). This statistic replaces the Cragg-Donald

test for clustered observations; a high F-statistic signifies well correlated instruments. [b] The Stock-Yogo test probes the null hypothesis that the instrument is weak, where weakness is estimated as the size of the bias of the IV estimator relative to the Kleibergen-Paap F test at a set threshold (Stock and Yogo 2005); rejecting the null hypothesis at 10% signifies that the instrument is *not* weak.[c] The Sargan-Hansen Chi-square tests the null hypothesis that the excluded instruments are uncorrelated with the error term (Sargan 1958; Hayashi 2000). A non-significant Chi^2 p-value indicates *no* correlation.

The model confirms our theoretical conjecture. Moreover, the tests that tap the strength of the instruments and the likelihood that the instruments are unrelated to the error term are convincing. Instrumenting policy scope with *Community* and *Contract* does not weaken the results.

Core state powers and international authority

A longstanding stream of IR theorizing argues that the nature of the policy problem—problem structure—affects institutional design, but the difficulty is how to operationalize problem structure. In this section we summarize the argument with respect to core state powers, discuss how one might operationalize it, and report empirical tests.

The chief expectation is that a policy problem affects international authority to the extent that it has strategic properties that place distinctive demands on IO governance (Koremenos 2005, 2016; Martin 1992; Mitchell 2006; Sandler 2004; Snidal 1985, 1994; Stein 1982). In general, collaboration problems intensify demand for delegation because a third-party secretariat or court can help detect non-compliance or punish defection. Collaboration problems also encourage pooling because majority voting reduces the risk of states converging on a collectively suboptimal solution. However, this functional demand for deeper authority must navigate the sovereignty risks associated with weaker national control.

There are sound reasons to expect collaboration involving core state powers to face this trade-off between scale benefits and sovereignty risks (Genschel and Jachtenfuchs 2016; also Haftel 2011; Kono 2007; Simmons and Danner 2010). Core state powers relate directly to the state's monopoly of legitimate coercion: military force, police power, border control, public revenue, and administrative organization (Genschel and Jachtenfuchs 2016). Collaboration on core state powers is more intrusive than typical regulatory coordination because it requires creating government capacity at the IO level. States are asked to transfer resources: troops, border guards, central bankers, civil servants, taxation. Distributional conflicts about core state powers tend to be zero-sum: "every Euro [or dollar] of public revenue can only be spent once; every border guard can only be in one place at one time; every administrator can only be assigned so many tasks" (Genschel and Jachtenfuchs 2018: 181). Authority transfer may also touch national identity: "tax duties, welfare entitlements, military or other public service obligations, immigration rules and naturalization laws define who belongs to the national community, and what community members owe to each other" (Genschel and Jachtenfuchs 2018: 181). So, the expectation is twofold. On one hand, states should be less willing to delegate or pool authority with respect to core state powers. On the other, as IOs expand their

policy portfolio into the realm of core state powers, states should become reluctant to delegate or pool authority to the IO.

We test the first argument by adding a variable that captures whether an IO handles core state powers, and the second argument by means of an interaction between *Policy scope* and *Core state powers*. We develop a variety of measures, which have in common that they take off from Genschel and Jachtenfuchs' definition of core state powers and link this to information from the MIA dataset.[6] The measures differ in their operationalization of what constitutes a central policy to the IO,[7] whether to construct the variable as dichotomous or as a proportion, and whether to build in information on the extent to which an IO appropriates core state resources.[8] These operationalizations are described in Table B.16 (see next page).

Table B.17 summarizes the findings for four of these measures (see following page). The top panel explains change in delegation, and the bottom panel explains change in pooling. The gray cells flag significant effects for *Core state powers* or for the interaction term. We find consistent patterns across measures.

[6] The Measurement of International authority (MIA) dataset estimates the scope of an IO's portfolio across twenty-five policies, whether a policy is core or flanking, and what policy is most central to the IO. Estimates are annual. MIA is in the public domain, has the best coverage, and the most fine-grained estimation of an IO's policy portfolio.

[7] A liberal interpretation allows for a core state policy to be one of several core policies of an IO; a restrictive interpretation identifies the primary policy of the IO. For example, currency is one of several core policies of today's European Union, but trade is the primary core policy of the EU. Conversely, the North Atlantic Treaty Organization has military cooperation as primary policy and only core policy.

[8] This requires knowledge of the extent to which an IO appropriates core state powers. Does an IO deal mostly with coordinating decisions on core state policies or does it appropriate resources for the IO? Does it have the authority to pool national soldiers into in a rapid reaction force, employ national diplomats in a joint diplomatic service, operate a single currency, levy its own tax? Or does it chiefly engage in confidence building measures, coordinate monetary policy, conduct peer reviews? This information is not readily available in the MIA dataset, but we can draw from the IO profiles to eliminate IOs that mostly coordinate rather than build up own resources (Hooghe et al. 2017).

Table B.16: Operationalizing core state powers

Common basis:

- Genschel & Jachtenfuchs (2016: 44): core state powers refer to "Political External Relations; Criminal and domestic security; Macroeconomic & Employment; Money; Tax."
- MIA equivalent categories (Hooghe et al. 2017): foreign policy, diplomacy, political cooperation; military cooperation, defense, military security; justice, home affairs, interior security, police, anti-terrorism; migration, immigration, asylum, refugees; welfare state services, employment policy, social affairs, pension systems; financial regulation, banking regulation, monetary policy, currency; taxation, fiscal policy coordination, macro-economic policy coordination.

Core or primary? *Core=one/several policies of the list above is a core policy of the IO* *Primary=one policy of the list above is the IO's primary responsibility*	**Dichotomy or proportional?** *Dichotomy= 1 if at least one core policy/ 1 if primary policy* *Proportion= # core policies divided by policy scope*	**Information on cooperation problem?** *Expunge core state IOs that do not appropriate core state resources?*	**Reported in table B.17?**
Core policy	Dichotomy	No Yes	X (1)
	Proportion	No Yes	X (2)
Primary policy	Dichotomy	No Yes	X (3) X (4)

Table B.17: Measures of core state powers: explaining delegation and pooling

	At least one IO core policy is core state (IV=dummy)	At least one IO core policy is core state (IV=proportion)	Primary IO policy is core state (IV=dummy)	Primary IO policy is core state + IO centralizes resources (IV=dummy)
			DV=change in delegation	
Policy scope $_{t-1}$	0.012***	0.016***	0.016***	0.017***
Politicization $_{t-1}$	−0.014**	−0.006*	−0.011**	−0.011*
Core state powers	−0.043	0.497***	0.046	0.033
Core state powers x Policy scope $_{t-1}$	0.004	−0.044***	−0.011***	−0.011***
R^2 within	0.418	0.431	0.451	0.446
AIC	−10887	−10963	−11074	−11049
			DV=change in pooling	
Policy scope $_{t-1}$	0.009**	0.009***	0.008***	0.008***
Politicization $_{t-1}$	−0.019***	−0.017**	−0.019***	−0.019***
Core state powers	−0.021	0.087	−0.129**	−0.139**
Core state powers x Policy scope $_{t-1}$	0.001	−0.009	0.008	0.008
R^2 within	0.256	0.254	0.291	0.288
AIC	−10269	−10264	−10421	−10408

Note: Fixed effects estimations with standard errors clustered by IO. N=3199 IO-year (76 IOs) for 1950-2010. The dependent variables *Change in delegation* and *Change in pooling* vary between −1 and 1. *** p<0.01, ** p<0.05, * p<0.1. Controls for democracy, members, power asymmetry, affluence, GDP dispersion, scope, politicization, and a year count. Gray cells indicate significant effects for core state powers.

Three out of four equations explaining delegation show a significant interactive term. This is consistent with the idea that as an IO with core state powers expands its policy portfolio, states become more reluctant to delegate authority. Figure B.1 (top panel) shows a margins plot that compares the marginal effect of policy scope on delegation for IOs with a low content, medium content, and high content of *Core state powers* (model in the second column). It illustrates how the *slope* of the relationship between scope and delegation completely flips between IOs with low and high content.

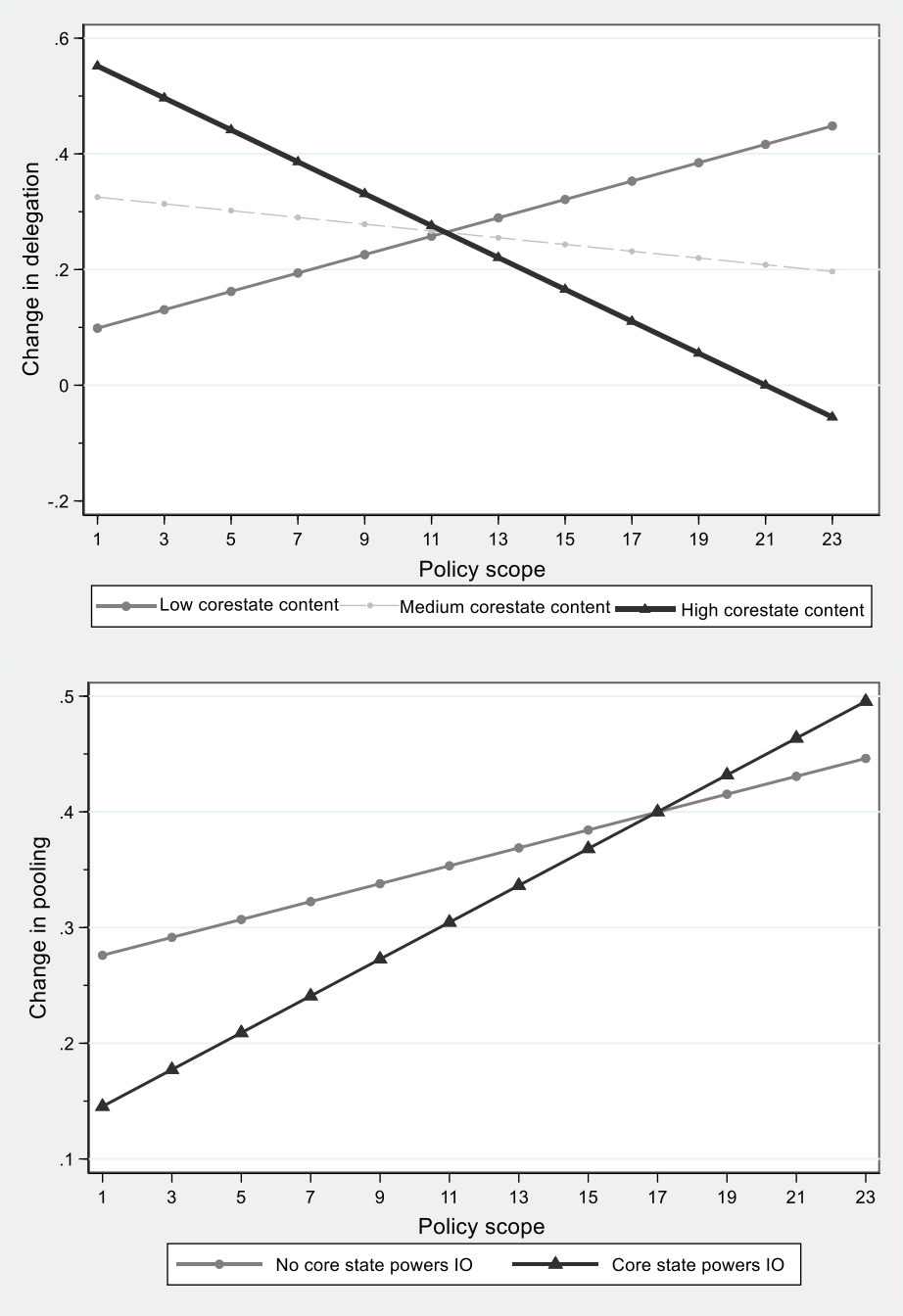
The substantive effect is real but the confidence bands (not shown) are wide. The difference in delegation for a high-content core state IO compared to a low-content core state IO is 0.28 on a 0-1 scale if the IO has five policies, narrows to just 0.02 for IOs with eleven policies, and then widens to 0.15 for IOs with fifteen policies.

In two of the four equations *Core state powers* impacts negatively on pooling. Hence the *level* of pooling in IOs with core state powers is systematically lower than in other IOs. This effect is strongest when we use the measure that captures IOs with core state powers as their primary policy. These are IOs whose raison d'être is to manage Westphalian sovereignty, such as NATO, Interpol, or the United Nations. States appear reluctant to pool sovereignty in these IOs. The difference in pooling between core state and non-core state IOs fades only when these IOs acquire general purpose scope—fifteen policies or more.

Figure B.1 (bottom panel) illustrates this graphically (model in the last column). The effect is substantial: a task-specific IO responsible for a core state power (e.g. security, currency) scores, on average, 0.13 points lower on pooling than a task-specific IO focusing on a non-core state power (e.g. trade, communications, transport), holding all other variables at their means.[9] This gap narrows steadily but only closes for IOs with seventeen policies or more and then reverses its sign.

[9] Calculations based on the last column in Table B.17.

Figure B.1: Marginal effects of policy scope by core state powers



Note: The top panel plots the marginal effect of *Policy scope* on *Delegation* for IOs with low, medium or high Core state content. The bottom panel presents the marginal effect of policy scope on pooling for IOs that have a core state power as their primary policy or not. Confidence intervals not shown.

In all, we find evidence suggestive of the notion that problem structure impacts international authority. However, our conclusions are provisional. A more conclusive test

requires better data. While the MIA data provide a fine-grained read on the range and type of policies of an IO, it does not assess the nature of the cooperation problem that underlies these policy commitments. Our hunch is that such information is necessary to do justice to the problem structure argument.

Epistemic community and politicization

Politicization is the chief constraint on international authority, but there are good reasons to anticipate that one kind of IO is less vulnerable to politicization: an IO that taps into a transnational epistemic community, a community of professionals with recognized expertise and integrity (Haas 1992; Haas and Haas 1995).

Epistemic communities bring knowledge and authority to bear "in articulating the cause-and-effect relationships of complex problems, helping states identify their interests, framing the issues for collective debate, proposing specific policies, and identifying salient points for negotiation" (Haas 1992: 2). By substituting knowledge and expertise for interests and ideology, they can shift the discourse from distributional or ideological politics to problem solving.[10] The upshot is less need—and less legitimate room—for politicization.[11]

In the absence of a dataset on epistemic community in IOs that we can use, we construct a straightforward variable, *Episteme,* that takes on a value of 1 if the IO contract explicitly requires professional or scientific qualifications for state representatives. Table B.18 explains and illustrates the criteria that we apply. Since we evaluate an IO at each time its contract is reformed, our coding allows for an IO to become (or lose) its *episteme* quality. In practice, there is no variation over time. Fifteen of seventy-six IOs draw on epistemic authority that meets our criteria.

[10] This does not rule out fierce debate among experts on their knowledge paradigm. As Haas and Haas (1995: 261-2) observe, competing epistemic communities may seek to promote their preferred knowledge scheme within an IO. However, these battles need to resolve themselves into one of these epistemic communities gaining the support of a broad coalition of IO member states, which can then empower experts to use the IO to transpose their knowledge into policy. If support breaks down, a dominant epistemic community may break up. A more refined coding of epistemic community would therefore seek to capture the relative cohesion within an IO epistemic community and its political support base.

[11] This argument hinges on the condition that states actually value scientific advice and the help of experts in governing complex transnational problems. What counts is not so much the quality of the expertise or argument, but the general reputation of experts in being able to solve a problem (Zürn 2018: 52). Rising populism is undermining popular trust in experts and may be weakening states' resolve to rely on them.

Box B.18: Operationalizing epistemic community

A dichotomous variable that takes the value of 1 if the IO has a provision in its constitution, treaty, regulations, or bylaws that requires the assembly or an executive of the IO to impose merit criteria for selecting state representatives: (a) representatives must have well-defined expertise directly related to the policy core of the IO (e.g. being a doctor) or position (e.g. head of the national health service); and (b) they must have some autonomy to deliberate and decide on the basis of their expertise rather than national interest. Source: own coding for 76 IOs over time. Positive examples:

ESA: "The Science programme committee is composed of delegates of the member states with specific competence in scientific matters. The general procedure is for each delegation to the SPC to be composed of two representatives, one of whom should be a scientist. … The SPC has total authority over the scientific projects" (Bonnet and Manno 1994: 25).

ICAO: Air navigation commission: composed of professionals with "suitable qualifications and experience in the science and practice of aeronautics" (Convention of International Civil Aviation, Art. 56). These persons shall be presumed to act in their personal expert capacity.

ITU: "The person appointed to serve on the Council by a Member State of the Council shall, so far as possible, be an official serving in, or directly responsible to, or for, their telecommunication administration and qualified in the field of telecommunication services" (CV Art. 4, Mod. 56).

15 IOs categorized as having epistemic community: Bank for International Settlements, CABI International, European Space Agency, Global Environmental Fund, World Bank, International Civil Aviation Organization, International Telecommunications Union, IWhale, Interpol, Intergovernmental Organization for International Carriage by Rail, UNESCO, Universal Postal Union, World Customs Organization, World Health Organization, World Meteorological Organization.

We first contrast the *level* of politicization among non-epistemic and epistemic IOs. Table B.19 reproduces average politicization over the entire period, since 1990 (when politicization began to rise), and 2006-2010 (last five years). Politicization is consistently three or four times lower for epistemic IOs. These are substantively large differences. They pass a conventional difference of means test, though not the severe test that uses standard errors clustered by IO. A closer look suggests that this is driven by one outlier among the fifteen epistemic IOs—the World Bank—which attracts 88 percent of the politicization targeted at epistemic IOs. A cluster-robust difference of means test without the outlier meets the threshold handily at the 95% confidence level. Excluding the extreme outlier, average politicization among episteme IOs is about one-twentieth of politicization in non-episteme IOs. The table reports averages with and without the extreme outlier, with robust standard errors in brackets.

Table B.19: Average politicization for epistemic and non-epistemic IOs

	Epistemic IOs	Epistemic IOs (outlier excluded)	Non-epistemic IOs
Across all years	2.351	0.286	7.784
	(1.996)	(0.120)	(3.160)
Post-1989	5.880	0.659	15.466
	(5.080)	(0.279)	(6.270)
Post-2005	4.160	1.276	20.524
	(2.864)	(0.618)	(8.786)

Note: Politicization is calculated as a moving average over three years. Average level of politicization with standard errors (clustered by IO) in brackets.

The *level* of politicization is lower among epistemic IOs. But it is still possible that politicization, however scarce, puts downward pressure on international authority in epistemic IOs. In other words, it is possible that the same logic of politicization is at work across the entire population of IOs. Table B.20 tests this by estimating models that include an interaction term between epistemic IOs and politicization—again with standard errors clustered by IO and with full controls. The analyses show that the constraining effect of politicization is flatter for epistemic IOs but not enough to generate a statistically significant difference with non-epistemic IOs.

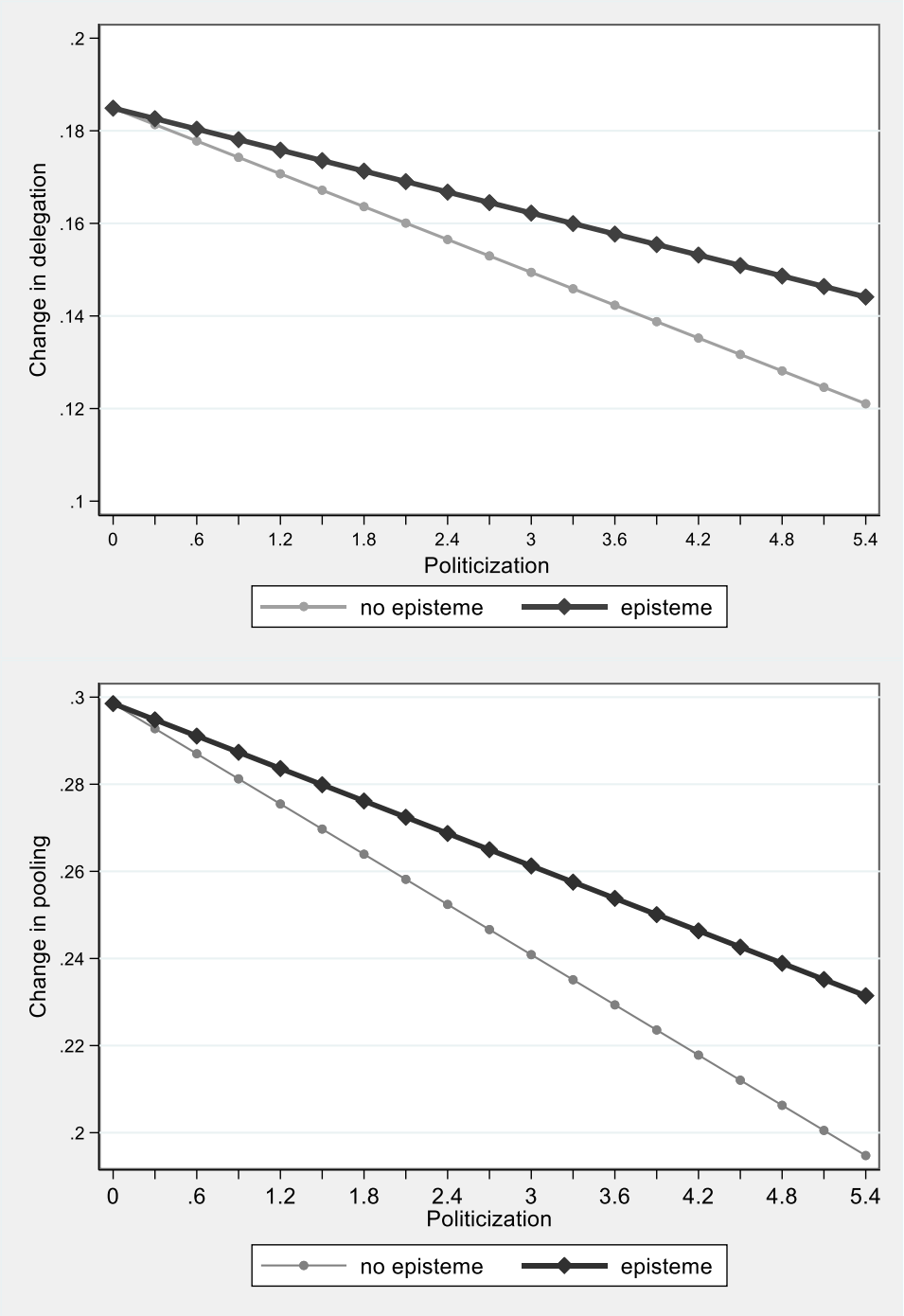
Table B.20: Politicization and epistemic community

	Change in delegation	Change in pooling
Policy scope $_{t-1}$	0.015***	0.010***
	(0.003)	(0.003)
No episteme IO*Politicization $_{t-1}$	−0.012**	−0.019***
	(0.005)	(0.007)
Episteme IO*Politicization $_{t-1}$	−0.008**	−0.012*
	(0.003)	(0.006)
Democracy $_{t-1}$	0.001	0.005*
	(0.002)	(0.002)
Members $_{t-1}$	−0.034	−0.000
	(0.037)	(0.026)
Power asymmetry $_{t-1}$	0.052	0.003
	(0.053)	(0.049)
Affluence $_{t-1}$	−0.001	−0.001
	(0.002)	(0.001)
GDP dispersion	−0.000	0.001
	(0.001)	(0.002)
Core state powers	−0.017	−0.013
	(0.026)	(0.019)
Year count	0.001**	0.001
	(0.001)	(0.001)
Constant	0.084	0.166***
	(0.057)	(0.050)
R^2 within	0.412	0.256
F-statistic	5.63	5.29
AIC	−10856	−10268

Note: Fixed effects estimations with standard errors clustered by IO. The dependent variables *Change in delegation* and *Change in pooling* vary between −1 and 1. *** p<0.01, ** p<0.05, * p<0.1.

This is also portrayed in Figure B.2 which plots the marginal effect of politicization on delegation (top) and pooling (bottom) for epistemic IOs and non-epistemic IOs. The slope for epistemic IOs is less steep than for non-epistemic IOs, and it widens as politicization intensifies. The difference fails to meet standard levels of statistical significance, but the direction is clear. Incidentally, the different slopes between top and bottom panel also corroborate that the effect of politicization on pooling is more pronounced than on delegation—the slope is steeper.

Figure B.2: Marginal effects of politicization by episteme



Note: The top panel plots the marginal effect of *Politicization* on *Delegation* for epistemic vs. non-epistemic IOs; the bottom panel lots the marginal effect of *Politicization* on *Pooling* for the same kind of IOs.

Heterogeneity of preferences and international authority

Does heterogeneity of preferences encourage or discourage states to empower IOs? The IR literature is divided, as aptly captured by Snidal: "the impact of heterogeneity is heterogeneous" (Snidal 1995: 62).

The predominant view is that preference heterogeneity makes states less likely to cede authority to non-state bodies (delegation) or to each other (pooling). We test this with an indicator that taps geopolitical congruence among the member states of an IO. *Ideal points* reflects the extent to which two states vote differently in the UN assembly (Bailey, Strezhnev, and Voeten 2017). We reverse the direction so that geopolitical convergence indicates how closely aligned, on average, a member state dyad within an IO is. The conjecture is that, if IO members do not see eye to eye on geopolitics, how could they see eye to eye on surrendering authority to IOs they both belong to? This logic should be sharpest for general purpose IOs, which are open-ended commitments to jointly solve an vaguely specified array of problems, and so we interact *Geopolitical convergence* with the type of IO. We expect the interaction to be positive and significant.

Table B.21: Geopolitical convergence and international authority

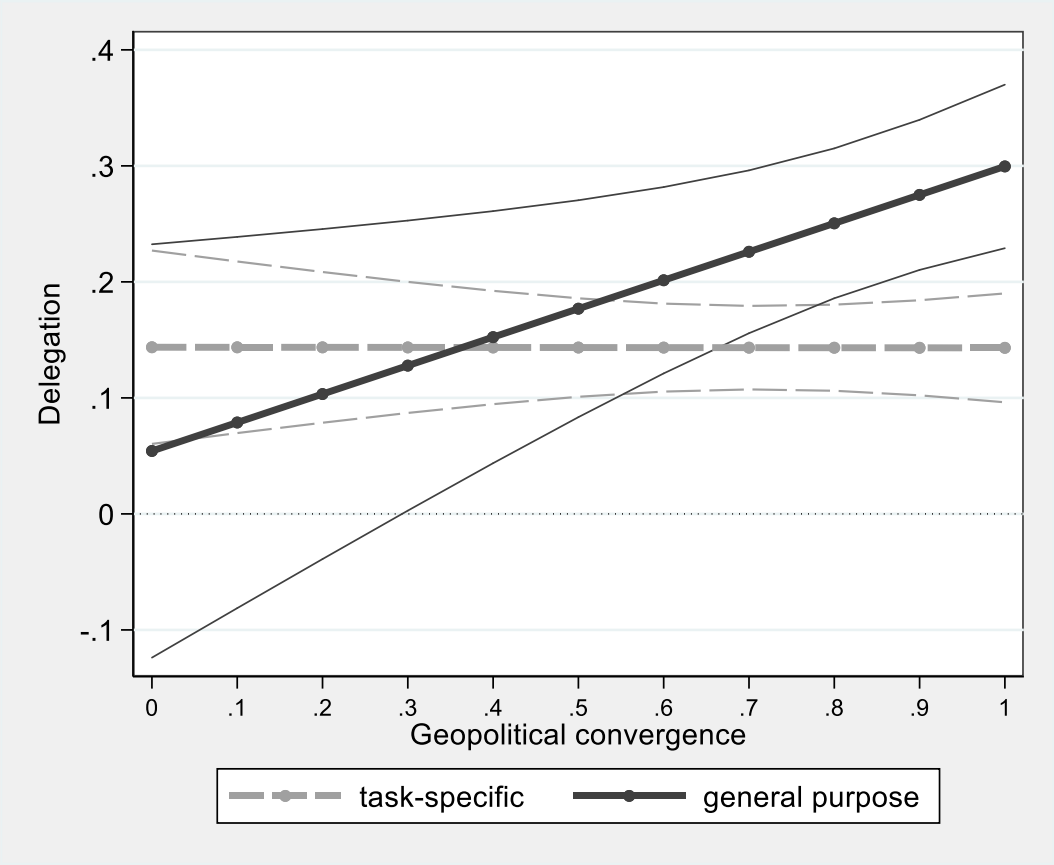
	DV=change in delegation			DV=change in pooling		
	(1)	(2)	(3)	(4)	(5)	(6)
Geopolitical convergence	0.101**	-0.001	0.065	0.158	0.073	0.091
	(0.049)	(0.054)	(0.043)	(0.142)	(0.131)	(0.109)
General purpose IO	-0.058	-0.089		0.063	0.040	
	(0.124)	(0.111)		(0.156)	(0.143)	
Geopolitics x	0.240*	0.246**		0.037	0.037	
General purpose IO	(0.239)	(0.114)		(0.173)	(0.160)	
Policy scope			0.015***			0.009***
			(0.003)			(0.006)
Constant	0.063	0.081	0.045	0.153	0.114	0.113
	(0.040)	(0.073)	(0.070)	(0.102)	(0.121)	(0.092)
Controls	NO	YES	YES	NO	YES	YES
R^2 within	0.105	0.253	0.417	0.076	0.192	0.251
AIC	−9512	−10088	−10885	−9594	-10006	−10326
F-statistic	6.46	4.66	6.16	9.06	5.34	5.28

Note: Fixed effects estimations with standard errors clustered by IO. N=3199 IO-year. Models 1 and 4 contain no controls. Models 2 and 5 include as controls politicization, democracy, membership size, power asymmetry, affluence, GDP dispersion, and year count, and models 3 and 6 also include policy scope.

Table B.21 shows results for delegation and pooling respectively. Columns 1 and 4 report simple models that include *geopolitical convergence*, *type of IO*, and an interaction term; columns 2 and 5 add full controls except for policy scope, which is closely aligned to the type of IO and might depress the effect of IO type. Finally, Columns 3 and 6 substitute policy scope for type of IO. Models with controls also include a year count to account for time pressures.

Geopolitical convergence has an effect on delegation in general purpose IOs (models 1 and 2). The substantive effect is sizeable: holding geopolitical convergence constant at 0.70 (which is the median), a general purpose IO can expect to have a delegation score that is 0.08 points higher than a task-specific IO. However, as Figure B.3 reveals, the confidence bands are wide and only for the most cohesive IOs (>0.80 on a 0-1 scale) does type of IO differentiate.[12] Moreover, the effect fades once we control for policy scope (model 3). Change in policy scope is a much more powerful predictor of change in authority than geopolitical convergence is.

Figure B.3: Marginal effects of geopolitical convergence by type of IO



[12] Estimated using Model 2.

A second line of argument relates economic preferences to institutional outcomes. We employ trade interdependence as a proxy for convergent economic interests since trading partners could be expected to have some convergent interest in deepening trade rules (Haftel 2013; Martin 1992). We narrow our sample to IOs that have competence on trade *and* for which we have longitudinal data on trade interdependence. This reduces the sample to 34 IOs and the post-1969 years.

As the models in Table B.22 show, trade interdependence is a weak influence on delegation and pooling in trade IOs. This fits poorly with functionalist theories that expect pressure from trade links on deepening international authority (Keohane 1984; Stone Sweet and Brunell 1998). Only bivariate models suggest some positive effect, but this dissipates under controls.

Table B.22 does highlight a different matter: a significant and positive effect on pooling from size of membership and power asymmetry. These effects do not appear driven by a lonely outlier such as the European Union, and they survive (though weakened) under a jackknife procedure that drops each panel sequentially (results not shown). Membership is the most robust of the two. This is interesting in light of Chapter Seven, which shows that membership size is the single most powerful predictor of the *level* of pooling in an IO. Our finding here is related, though different: for the subset of trade IOs, *change* in membership intensifies pressure for *increased* pooling. The effect is substantial: an increase in membership from six to twenty-eight members can be expected to lead to an increase in pooling by 0.08 on the 0-1 scale.

Table B.22: Trade interdependence and international authority

	DV=change in delegation			DV=change in pooling		
	(1)	(2)	(3)	(4)	(5)	(6)
Trade interdependence	0.119***	-0.017	-0.004	0.081***	-0.015	-0.011
	(0.035)	(0.024)	(0.024)	(0.025)	(0.017)	(0.013)
Policy scope			0.018***			0.006***
			(0.004)			(0.002)
Politicization			-0.021			-0.004
			(0.018)			(0.007)
Democracy		0.000	-0.002		0.002	0.001
		(0.005)	(0.004)		(0.003)	(0.003)
Membership		0.097	-0.124		0.196***	0.118***
		(0.088)	(0.098)		(0.043)	(0.039)
Power asymmetry		0.121	-0.109		0.164***	0.086**
		(0.078)	(0.111)		(0.055)	(0.040)
Affluence		-0.004**	-0.003*		-0.002**	-0.002**
		(0.001)	(0.001)		(0.001)	(0.001)
GDP dispersion		0.003**	-0.001		-0.001	0.001
		(0.001)	(0.001)		(0.001)	(0.001)
Year count		0.006***	0.003*		0.003***	0.002
		(0.002)	(0.001)		(0.001)	(0.001)
Constant	0.115***	-0.172*	0.101	0.115***	-0.237***	-0.143***
	(0.024)	(0.090)	(0.115)	(0.017)	(0.056)	(0.045)
Controls	NO	YES	YES	NO	YES	YES
R^2 within	0.035	0.344	0.546	0.046	0.530	0.594
AIC	−2070	-2403	−2744	−3069	-3697	-3830
F-statistic	11.30	7.83	6.78	10.28	16.28	33.00

Note: N=945 IO-years. Fixed effects estimations with standard errors clustered by IO. N=3199 IO-year. Models 1 and 4 contain no controls except for year count. Models 2 and 5 include as controls politicization, democracy, membership size, power asymmetry, affluence, GDP dispersion, and year count, and models 3 and 6 also include policy scope.

Note on the operationalization of politicization

Politicization estimates the salience and divisiveness of debate over an IO. Our measure taps media coverage of protests directed at an IO on the grounds that protests that reverberate in the news will intensify concerns about an IO's legitimacy and may motivate decision makers to adjust IO design. We adapt a measure developed by Tallberg et al. (2014), which captures annual media coverage of protests or demonstrations directed at an IO in the world's leading newspapers. Like them, we use the Lexis-Nexis database.

The estimate is the raw count of references that combine "PROTESTOR" or "DEMONSTRATOR" with the IO name or acronym. We use a three-year moving average—the equally weighted moving average of politicization at *t*, *t-1*, and *t-2*—to reflect the notion that the pressure for a response is strongest in the year of the protest and the first few years after the protest, but then recedes quickly. This moving average is divided by 100 to get more readable numbers.

Sources and temporal coverage

We use Lexis-Nexis' default "Major World Publications," which can be found under the "Source Type" section of Advanced Options. As of the time of the search (January 2018), 463 sources were included.

We search by year. We search as far back as the organizations go in our dataset, plus a two-year lead. In the case of the oldest organizations, this means that our search reaches back to 1948. However, it should be noted that Lexis-Nexis' coverage of newspapers is more limited for the years prior to the 1980s. For example, coverage of the Guardian extends as far back as 1975, the Financial Times as far back as 1982, and of the New York Times as far back as 1980.

Search process

The base line search segment, created in the "Build Your Own Segment Search" in LexisNexis, reads as follows:

> *"organization name" OR "organization acronym" w/p demonstrator OR protestor OR protester*

There are several possible sources of measurement error. One is when a valid reference may refer only to a protest involving a constituent body of an IO. Here we descend a level of analysis to the component bodies and modify the baseline search segment to include an IO's component bodies (Inset A for most common instances). An additional issue is that some IOs have acronyms that can also refer to something other than the organization (Inset B). There are also more general concerns related to the fact that the newspapers covered in Lexis-Nexis vary over time and appear biased to those in the West, particularly in the early years.

Inset A: IO component bodies that require separate search:

- **European Union**: *"European Commission" OR "European Parliament" OR "Council of the European Union" OR "European Council" OR "European Coal and Steel Community" OR "ECSC" OR "European Economic Community" OR "European Community" OR "European Union" OR "EEC" OR "ECJ" OR "European Court of Justice" OR "CJEU" OR "Court of Justice of the European Union" OR "Court of Justice of the European Communities" OR "European Central Bank" OR "ECB" OR "CJEC" OR "European Court of Auditors" OR "European Investment Bank"*
- **African Union**: *"African Court on Human and Peoples' Rights" OR "African Court of Human Rights" OR "African Union" OR "Organisation of African Unity" OR "Organization of African Unity" OR "OAU" OR "AU" OR " African Court of Justice and Human Rights"*
- **Council of Europe**: *"Council of Europe" OR "European Court of Human Rights" OR "Venice Commission" OR "Committee of Ministers" OR "ECtHR" OR "ECHR" OR "CoE"*
- **United Nations**: *"United Nations" OR "UNGA" OR "UNSC" OR "ECOSOC" OR "UN"*
- **World Trade Organization:** *"World Trade Organization" OR "General Agreement on Tariffs and Trade" OR "WTO" OR "GATT"*

Inset B: IO acronyms with dual meaning or particular challenges

- UNIDO refers also to a Philippine political party or to the Spanish/Portuguese word
- SACU refers also to a cricket union
- SPC refers also to a military title abbreviation
- PCA refers also to the Police Complaints Authority
- ICO refers to many things other than the International Coffee Organization
- FAO refers also to FAO Schwartz
- SELA refers also to a surname
- AMU refers also to first or surnames
- BIS refers to many things other than the Bank for International Settlements
- ISA/ISBA refers to a security act
- IMO refers to a province in Nigeria
- CoE refers often to a surname
- ESA combined with "demonstrator" can refer to a technical space-specific term
- "Commonwealth of Nations" may also refer to Commonwealth nations since Lexis-Nexis drops the word "of"
- Benelux may refer to the Benelux countries rather than the IO.

The politicization measure is correlated at 0.75 with an estimate of the salience of an IO, which was derived from a count of references to the IO in Google scholar. To minimize error, the search was set to cover publications dated between 2000 and 2014, and to search the organization's official name and acronyms with the "exact phrase" algorithm. Miscategorized references were removed after a manual check of each reference.[13]

[13] For IOs with a large number of references, the first hundred and last hundred cites were manually examined to calculate the proportion of valid references. The total number of references for this IO was then weighted with this proportion to estimate the total number of valid references for this IO.

Chapter 7: Why States Pool Authority

Chapter 7 argues that scale—understood as a function of the number of member states—explains the level of pooling in an IO. We first produce the full models of Table 7.1. In the following sections we discuss alternative specifications of our key independent variable, *Members,* and we examine the homogeneity thesis.

The power of numbers for pooling

The first three columns of Table B.19 display the full results of the models in Table 7.1. These highlight the effect of membership size, measured as the logarithm of the number of member states, on pooling. Remarkably, membership size in 2010 or average membership size is a better predictor of pooling in 1950 or year of foundation (whichever later) than membership in 1950. This suggests that states can anticipate fairly accurately how large an IO's membership might become *and* they allow this educated guess to influence their design decisions.

Core state powers has a significant negative effect on pooling. This is consistent with the expectation, well documented in the literature, that the sovereignty costs of international collaboration are particularly high in areas related to defense and security (Kono 2007; Snidal 1985; Stein 1982). Consequently, states are less inclined to pool. We already detected traces of this in Chapter 6 which explains change in authority over time, but the crosssectional analysis is unambiguous: a core state IO has, on average, a pooling score that is 0.07 lower than an IO that does not have a core state power as one of its core policies (holding all other variables at their means).

We further explore this finding in the last column, which substitutes *Security* for *Core State Powers.* This is based on a categorization by Boehmer, Gartzke, and Nordstrom (2004). We report only the strongest finding, which is significant at the 0.10 level. *Security* takes on a value of 1 if an IO's sole mandate is estimated to be collective (military) security, and just two IOs meet this criterion: the North Atlantic Treaty Organization and the Organization for Security and Cooperation in Europe. For these particular IOs the substantive effect is huge (their pooling score is 19 points lower than that of the average IO), but the overall causal impact is modest. Our hunch is that there may be more life in the security hypothesis than we are able to reveal, but our quest is hindered by data availability. More fine-grained estimation of problem structure could generate sharper insight into the causes and consequences of institutional design.

Table B.19: Time-series cross-section of Pooling

	Pooling 1950 (or first year)			
Members 1950	0.224***			
	(0.048)			
Members 2010		0.212***		0.213***
		(0.035)		(0.036)
Members 1950-2010 mean			0.224***	
			(0.039)	
Policy scope	–0.006	–0.001	–0.001	–0.004
	(0.006)	(0.006)	(0.006)	(0.006)
Core state powers	–0.073*	–0.078**	–0.070*	
	(0.040)	(0.037)	(0.038)	
Security				–0.190*
				(0.101)
Politicization	–0.077	0.316	0.201	0.278
	(0.722)	(0.666)	(0.676)	(0.671)
Democracy	0.001	–0.000	0.001	0.001
	(0.004)	(0.003)	(0.003)	(0.003)
Power asymmetry	–0.079	–0.073	–0.067	–0.080
	(0.112)	(0.099)	(0.101)	(0.100)
Affluence	–0.003	–0.002	–0.000	0.001
	(0.004)	(0.004)	(0.004)	(0.004)
GDP dispersion	0.015	0.012	0.012	0.008
	(0.014)	(0.013)	(0.013)	(0.013)
Constant	0.050	–0.010	–0.016	–0.024
	(0.117)	(0.107)	(0.112)	(0.108)
Controls	YES	YES	*YES*	*YES*
R^2	0.484	0.555	0.542	0.549
AIC	–65.35	–76.60	–74.54	–75.66

Note: 76 IOs in 1950 or first year in the dataset; OLS regressions. The year of estimation for *Members* varies: it is 1950 (or first year) for the first model; 2010 for model 2 and 4; the average across the period for model 3. *Members* is logarithmic (log10) because the expected effect of an additional member state declines as the absolute number of members increases. *** p<0.01 ** p<0.05 *p<.10

Alternative specifications for membership

The estimations in Table B.19 exert maximal controls, and the loss in degrees of freedom may blunt results. Table B.20 takes a simpler tack by reporting bivariate as well as

multivariate regressions. We also compare absolute numbers alongside the logarithmic measure of *Members*. These results confirm that:

- Membership in 2010 is a stronger predictor of pooling in 1950 than membership in 1950, which suggests the force of anticipation (Pearl 2009). The t-values for members 2010 explaining pooling in 1950 are consistently higher than those for members 1950.
- The variance explained by the bivariate models is similar to that in the fully specified models. This suggests that the power of membership in explaining crosssectional variation in pooling is robust.
- The models using the absolute number of member states are nearly as strong as those using the logarithmic measure. This is further corroboration of the scale hypothesis.

Table B.20: Bivariate regressions of *Members* on *Pooling*

	Pooling in 1950 (or first year)	
Members 1950 (log10)	0.268	0.225
	(t=7.38)	(t=4.66)
Members 1950 (absolute)	0.004	0.003
	(t=6.32)	(t=3.66)
Members 2010 (log10)	0.232	0.214
	(t=8.84)	(t=6.03)
Members 2010 (absolute)	0.002	0.002
	(t=8.59)	(t=5.82)
Members 1950-2010 mean (log10)	0.250	0.226
	(t=8.73)	(t=0.039)
Members 1950-2010 mean (absolute)	0.003	0.002
	(t=8.14)	(t=5.26)
Controls	NO	YES
R^2 *(log10)*	0.424–0.513	0.475–0.549
R^2 *(absolute)*	0.351–0.499	0.422–0.537

Note: 76 IOs in 1950 or first year in the dataset. Crosssectional OLS. Beta-coefficients with t-values in brackets.

The homogeneity hypothesis

A chief contender to the scale hypothesis is the homogeneity hypothesis. This is the idea that majoritarian decision making is more likely in homogenous groups (Maggi and Morelli 2006; Renou 2011).

The original formulation of this hypothesis understands homogeneity in terms of interests rather than culture or norms. To test this, we turn to a measure of foreign policy preferences developed by Bailey et al. (2017), which estimates the distance in the voting pattern on UN Assembly resolutions between two countries. We collect this information for each dyad of an IO's members, and then average the distances. The measure is annual. High values indicate heterogeneity. As Table B.21 shows, the measure is significant, but the sign is wrong, both in the bivariate model and under controls. In a model that adds members *Ideal points* washes out.

Table B.21: Preference homogeneity (ideal points) and pooling

	Pooling in 1950 (or first year)		
Ideal points (heterogeneity)	0.167***	0.104**	0.025
	(0.045)	(0.049)	(0.045)
Members 1950-2010 mean			0.216***
			(0.042)
Policy scope		–0.009	–0.001
		(0.007)	(0.001)
Core state powers		–0.079*	–0.072*
		(0.045)	(0.038)
Politicization		–0.149	0.059
		(0.854)	(0.727)
Democracy		0.001	0.001
		(0.874)	(0.003)
Power asymmetry		–0.317***	–0.063
		(0.105)	(0.102)
Affluence		–0.005	–0.002
		(0.005)	(0.004)
GDP dispersion		0.017	0.012
		(0.016)	(0.013)
Constant	0.122	0.378***	–0.029
	(0.040)	(0.098)	(0.115)
Controls	NO	YES	*YES*
R^2	0.157	0.360	0.545
AIC	–42.15	–49.07	–72.89

Note: N=76 IOs in 1950 or first year.

One might also conceive of homogeneity in epistemic terms. A common knowledge frame among experts should reduce risks of exploitation inherent in majoritarian voting, and so if states can to tap into epistemic communities to help them prepare—or make—IO decisions, they may be more willing to accept pooling. As before, we use our own measure, a dichotomous variable that takes on the value of 1 if an IO explicitly requires professional or expert qualifications for those who sit on its executive (see the more detailed discussion of *Episteme* earlier in this Appendix).

The correlation matrices below (Table B.22 and B.23) suggest some conditional support for our conjecture that epistemic authority facilitates pooling. The bivariate correlation between *Episteme* and *Pooling* is moderately high (r=0.41). However, it is not robust to the inclusion of membership (r=0.11 or r=0.13).

Table B.21: Correlation matrix between pooling, members, and episteme

	Pooling	**Members (log10)**	**Members (absolute)**
Members (log10)	0.7764 (p=0.000)		
Members (absolute)	0.724 (p=0.000)	0.923 (p=0.000)	
Epistemic community	0.413 (p=0.002)	0.457 (p=0.000)	0.458 (p=0.000)

Note: N=76 IOs averaged across years in the data set. We implement the Bonferroni correction when calculating correlations, which compensates for the fact that if multiple hypotheses are tested, the chance of a rare event and therefore the likelihood of incorrectly rejecting a null hypothesis increases. The Bonferroni correction tests each individual hypothesis at a significance level of α/m where α is the desired overall alpha level and m is the number of hypotheses.

Table B.22: Partial correlations between pooling, members and episteme

	Pooling
Members log (controlling for episteme)	0.711 (p=0.000)
Members absolute (controlling for episteme**)**	0.661 (p=0.000)
Episteme (controlling for members log10)	0.111 (p=0.345)
Episteme (controlling for members absolute)	0.132 (p=0.259)

Note: N=76 IOs averaged across the years in the dataset.

Table B.24, which reports full results of Table 7.2 from Chapter 7, places the analysis in a multivariate frame. The first model shows the bivariate relationship between *Episteme* and *Pooling*, and subsequent models add controls. Epistemic authority appears associated with higher pooling, but the effect is swallowed once we include membership size.

Indeed, nearly all IOs that rely on transnational epistemic community have global membership. The table also confirms the constraining effect of core state powers on pooling as well as the presence of an asymmetrically powerful member state.

Table B.24: Epistemic community and pooling

	Bivariate	With some controls	With full controls	With membership
Episteme	0.177***	0.121**	0.101*	0.006
	(0.053)	(0.052)	(0.053)	(0.047)
Core state powers			–0.090**	–0.072*
			(0.044)	(0.036)
Politicization			0.312	0.181
			(0.808)	(0.666)
Democracy		0.003	0.002	0.001
		(0.004)	(0.004)	(0.003)
Power asymmetry		–0.381***	–0.368***	–0.066
		(0.106)	(0.105)	(0.101)
Affluence		–0.001	–0.004	–0.002
		(0.004)	(0.005)	(0.004)
GDP dispersion		0.001	0.009	0.011
		(0.015)	(0.016)	(0.013)
Members 1950-2010				0.225***
				(0.039)
Constant	0.213	0.366***	0.409***	–0.027
	(0.024)	(0.068)	(0.071)	(0.096)
Controls	NO	YES	*YES*	*YES*
R^2	0.131	0.271	0.315	0.542
AIC	–39.79	–45.13	–45.87	–74.52

Note: N=76 IOs for 1950 or first year in dataset. The first model uses no controls, while the second, third, and fourth incrementally add controls. Policy scope is not included as control because narrow policy scope is a condition for an epistemic community.

There are sound reasons to suspend final judgment. True, the model suggests that membership beats epistemic community, but the mechanisms through which epistemic community and membership affect pooling may be different. Epistemic community facilitates pooling to the extent that it reduces the risk of exploitation under majority. Membership size induces pooling to the extent that it increases the decision costs of unanimity. Hence, it is possible for these two logics to work side by side. The measures available to us do not allow us to splice them apart. More refined estimates of epistemic

community that can snuff out the spurious correlation with membership size may therefore produce a sharper line of sight.

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