

Does Political Participation Improve Governance? Evidence from Cross-National and Within-Country Panel Comparisons

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Abstract

Democratic theory suggests that political participation improves governance, yet most evidence relies on cross-sectional analyses that cannot distinguish between-country associations from within-country causal effects. Using 270 country-year observations from the World Values Survey and the World Bank's Worldwide Governance Indicators across 108 countries (1996–2021), this study examines whether political participation predicts governance quality. Cross-sectional models show that activities such as petition signing, boycotting, and demonstrating are positively associated with governance. However, these relationships disappear once country fixed effects account for unobserved national characteristics. The findings remain robust after controlling for GDP per capita, applying two-way fixed effects with clustered standard errors, estimating lagged models, and using inverse probability weighting and propensity score matching, and estimating a dynamic panel system GMM model that explicitly accounts for governance persistence. The results are more consistent with the observed relationship being driven by stable country characteristics than with a direct causal effect of political participation on governance quality.

JEL Classification: D72, P16, O43, C33, H11

1. Introduction

A foundational premise in democratic theory holds that an engaged citizenry constitutes a necessary condition for effective governance. Where citizens vote, protest, petition, and organize, officials are presumed to face credible sanction for malfeasance, generating accountability mechanisms that improve institutional performance over time (Putnam 1994; Diamond 1999). This theoretical logic has informed a substantial body of policy prescription advocating civic-engagement interventions as a means of improving governance outcomes in developing and transitional democracies.

The empirical literature substantiating this claim, however, is subject to a persistent identification problem. The majority of cross-national studies regress a governance outcome on a participation measure using data pooled across countries at a single time point, or across country-years without accounting for time-invariant national characteristics (Möhring 2021). Such specifications conflate two analytically distinct relationships: first, whether countries exhibiting historically higher levels of participation have attained correspondingly higher levels of governance quality (a between-country association); and second, whether an increase in participation within a given country is followed by a corresponding improvement in that country's governance (a within-country association). Only the latter relationship bears directly on whether participation functions as a policy-relevant causal mechanism. The former is equally consistent with confounding, for instance economic development may raise both participation and state capacity simultaneously absent any causal link between them. Kamei et al. (2024) identify an analogous limitation in the literature linking civic engagement to reduced corruption, noting that the correlational nature of most existing evidence precludes reliable causal inference, a

concern that traces to Treisman's (2007) earlier critique of cross-national governance research more broadly.

This paper addresses this identification problem directly by estimating both relationships within a single, internally consistent empirical framework. We merge five behavioural and attitudinal participation items from the World Values Survey with all six dimensions of the World Bank's Worldwide Governance Indicators, yielding 270 country-year observations across 108 countries spanning WVS Waves 3 through 7 (1996–2021). For each participation-governance pairing, we estimate (i) a bivariate correlation, (ii) a pooled ordinary least squares model identifying the between-country association, and (iii) a country fixed-effects model identifying the within-country association, thereby permitting direct comparison of estimates derived from the same underlying data.

The results reveal a marked divergence between these two estimation strategies. Behavioural participation, and petition-signing in particular, ranks among the strongest bivariate predictors of governance quality reported in the cross-national literature, with pooled regressions accounting for as much as 56 percent of the variance in individual WGI dimensions. Once country fixed effects absorb all time-invariant national characteristics, these associations attenuate substantially toward zero and, for two participation items, reverse to a statistically significant negative association. These findings are more consistent with an interpretation in which the observed participation-governance correlation reflects stable, country-level heterogeneity than with a causal process whereby increased participation produces governance improvements within a country over time, although the limited temporal span of the WVS panel constrains the strength of this within-country inference.

To our knowledge, no existing study undertakes this specific between-country versus within-country comparison using the complete set of WVS participation variables matched against all six WGI dimensions. The most closely related contributions are Quintelier and van Deth's (2014) individual-level panel analysis, which demonstrates that participation shapes political attitudes to a greater extent than attitudes shape participation, applying an analogous within-unit logic at the individual rather than country level, and to attitudinal rather than governance outcomes and Zhao and Du's (2025) country-panel application of a comparable between/within framework to a related but distinct governance outcome, discussed further in Section 2.2. The present study extends this analytical approach to political participation at the country level, evaluated against the complete six-dimension WGI outcome set, with direct implications for governance policy.

The remainder of the paper is organized as follows. Section 2 reviews the theoretical accounts of the participation-governance relationship and derives competing hypotheses. Section 3 details the data and empirical strategy. Section 4 presents the results. Section 5 situates these findings within the competing theoretical frameworks, and Section 6 concludes with a discussion of limitations and directions for future research.

2. Literature Review and Hypotheses

2.1 Participation as a Cause of Governance Quality

The argument that civic participation can improve the quality of governance is most closely associated with Putnam's (1994) study of Italian regional governments. Putnam showed that regions with stronger associational networks and greater civic engagement tended to have more effective regional administrations, even decades later. He attributed this relationship to accumulated social capital, which facilitates collective monitoring and makes public officials more accountable to citizens. Knack and Keefer (1997) extended this argument using cross-country evidence, showing that trust and civic norms are associated with both economic growth and institutional performance. Subsequent research has similarly suggested that participation through voting, protest, and membership in civic organisations can provide mechanisms through which citizens monitor governments, constrain corruption, and encourage more effective public administration (Diamond 1999; Norris 2002). More recent survey evidence also points to the importance of distinguishing between actual political participation and political attitudes. Citizens who engage in concrete forms of political activity tend to report more favourable assessments of local governance than those who simply express positive attitudes towards politics (Zhang et al. 2025).

There is, however, an important difference between the forms of participation examined in this literature and those used in the present study. Putnam's measure of the civic community in *Making Democracy Work* is based on four indicators: associational density, newspaper readership, referendum turnout, and preference voting (Putnam, Nanetti, and Leonardi 1994, pp. 91–94). These measures primarily capture regular and institutionally embedded forms of civic engagement. The forms of participation examined here, including boycotting, petitioning, and demonstrating, are more closely related to what Tilly (2008) describes as contentious performances. Such activities are often episodic and are undertaken to place pressure on public officials rather than to monitor them through established institutional channels. The distinction matters because the social capital argument provides a more direct explanation for why trust and associational membership might be associated with better governance than for why contentious political activity should have the same effect. The WVS measures used in this study therefore extend the participation literature to a different set of behaviours rather than providing a direct test of Putnam's original measure.

Evidence at the sub-national level also suggests that the relationship between participation and governance depends on the form of participation considered. Coffé and Geys (2005), for example, find that associational density and voter turnout are associated with local government performance in Flemish municipalities, whereas more contentious forms of civic activity have a weaker and less consistent relationship with performance. Bracco et al. (2021), examining Italian municipalities, similarly find that social capital improves government performance partly by extending politicians' time horizons and reducing political turnover. This mechanism operates gradually and differs from the more immediate disciplining effect that might be expected from protest or other contentious forms of participation.

These findings suggest that the relationship between participation and governance may depend not only on what type of participation is considered, but also on the time horizon over which its effects are expected to emerge. Routine participation may contribute to governance through relatively slow-moving processes such as social trust, monitoring, and political accountability, whereas contentious participation may arise in response to particular political events or governance failures. This distinction is important for the present study because the use of WVS participation measures makes it possible to examine several forms of political behaviour and attitudes rather than relying on a single indicator of civic engagement. It also motivates the comparison between differences across countries and changes within countries.

2.2 Competing Accounts: Reverse Causation and Common Cause

Although the participation literature provides a plausible explanation for a positive relationship between political participation and governance quality, the observed relationship does not necessarily imply that participation causes better governance. Two alternative explanations are particularly important.

The first is reverse causation. Governance quality may itself influence citizens' willingness and ability to participate. Weak, unresponsive, or repressive institutions may discourage participation through conventional political channels such as voting and lobbying. At the same time, poor governance can encourage citizens to engage in more contentious forms of participation, including protest and boycotts, particularly in response to corruption scandals, disputed elections, or other governance failures (Tilly 2008). Recent evidence supports this direction of causality directly: drawing on event-study analysis of more than 170 high-profile corruption scandals across 17 Latin American countries alongside a randomized field experiment in Mexico, Rivera, Seira, and Jha (2024) show that revelations of apex corruption cause a substantial rise in violent protest and a corresponding decline in support for democratic institutions, rather than the reverse. Gehrke and Yang (2025) similarly show that corruption convictions of former heads of government produce divergent political consequences depending on regime context: convictions boost government approval in less democratic settings, where incumbents can frame the conviction as evidence of their own personal integrity, but have a negligible effect on approval in more democratic contexts. This suggests that the political fallout of a governance failure, and by extension the contentious response it provokes, is conditional on institutional context rather than automatic. Under this explanation, participation may be both a response to institutional weakness and a mechanism through which citizens attempt to challenge it. The resulting relationship is therefore not necessarily positive within countries. Countries with effective institutions may exhibit high levels of routine participation, while countries experiencing governance failures may experience temporary increases in protest and other contentious activities.

A second possibility is that participation and governance quality are jointly influenced by a third factor. Economic development is one particularly plausible common cause. Modernisation theory suggests that rising income and education increase citizens' capacity and willingness to participate politically

(Inglehart 1997). Economic development may also strengthen state capacity by increasing the resources available to governments and improving their ability to provide effective administration and public services (Fukuyama 2004). If both participation and governance are partly products of economic development, a positive cross-country association between the two variables could emerge even when participation has little independent effect on governance.

This distinction between cross-country differences and changes within countries is central to the empirical strategy of this study. The WVS and WGI contain substantial variation across countries that is likely to reflect relatively persistent characteristics, including income, regime type, and institutional history. A common cause of this kind can therefore produce a strong relationship between participation and governance across countries without necessarily generating a corresponding relationship between changes in participation and subsequent changes in governance within countries. Recent cross-national panel research also highlights the importance of distinguishing these sources of variation. Zhao and Du (2025), using data for 112 countries from 2014 to 2023, show that the relationship between a governance-related factor and political stability is sensitive to how potential confounding factors, including economic development, are accounted for in the analysis. This provides an important methodological caution for the present study and motivates the approach adopted in Section 3.5.

Related studies address parts of this identification problem but differ from the present analysis in important respects. Quintelier and van Deth (2014), for example, use a three-wave panel to examine whether political participation precedes changes in political attitudes. Their evidence provides support for a temporal relationship between participation and attitudes, but their outcome is political attitude rather than governance quality, and the unit of analysis is the individual rather than the country. Zhao and Du (2025) adopt a between-country and within-country perspective more closely related to the approach used here but examine political stability in the context of digital governance and anti-corruption efforts, using a different set of data and a more recent and narrower time period.

The present study brings together the six dimensions of the World Governance Indicators with the behavioural and attitudinal participation measures available in the World Values Survey. This allows the relationship to be examined separately across different dimensions of governance rather than treating governance quality as a single outcome. More importantly, comparing between-country differences with within-country changes provides a way of assessing whether the positive association commonly observed across countries is also evident when countries are compared with themselves over time.

The competing explanations generate different expectations about this distinction. If political participation directly contributes to better governance, increases in participation within a country should be associated with subsequent improvements in governance quality. By contrast, if the cross-country relationship primarily reflects reverse causation or a common underlying cause, the within-country relationship should be considerably weaker, absent, or potentially negative. The latter possibility is particularly relevant for contentious forms of participation, which may increase precisely when citizens perceive governance to be deteriorating. The empirical analysis therefore does not treat a positive cross-

country correlation as sufficient evidence of a causal effect. Instead, it examines whether the relationship persists when attention shifts from differences between countries to changes within countries, and whether the pattern is consistent across the six dimensions of governance.

2.3 Hypotheses

Political participation is often theorized as a channel through which citizens hold governments accountable, suggesting that countries with more participatory citizenries should, on average, exhibit better governance.

H1: Political participation is positively associated with governance quality across countries.

A cross-sectional association could reflect reverse causation or confounding by stable national traits; a true causal effect implies that changes in participation over time should precede changes in governance within the same country.

H2: Increases in political participation within a country are associated with subsequent improvements in that country's governance quality, controlling for time-invariant country characteristics.

Behavioral participation (e.g., petitioning, boycotting, demonstrating) depends on organizational capacity and civic infrastructure in ways that attitudinal participation (e.g., interest in politics) does not, so the former should be more predictive of governance outcomes.

H3: The association between participation and governance is stronger for behavioral measures of participation than for attitudinal measures.

3. Data and Methods

3.1 Data Sources and Sample

Participation measures are drawn from the World Values Survey (WVS Association 2022), an internationally coordinated survey of political attitudes and behavior fielded across more than 100 countries in seven waves since 1981. We use Waves 3 through 7 (1996–2021), the period of overlap with the governance data described below. Governance measures are drawn from the World Bank's Worldwide Governance Indicators (WGI; World Bank 2025; Kaufmann et al. 2011), which provide six standardized dimensions of governance quality — Voice and Accountability, Political Stability, Government Effectiveness, Regulatory Quality, Rule of Law, and Control of Corruption — for more than 200 countries annually since 1996, each scored on a scale from approximately – 2.5 to + 2.5.

The WVS is used here rather than an alternative such as the International Social Survey Programme (ISSP) because it is the only cross-national survey that repeats the same behavioral participation items across enough waves and countries to support a within-country panel comparison; the ISSP fields comparable items but with narrower country coverage and fewer repeated waves, which would leave too

few countries with multiple observations to estimate country fixed effects (see Oser, Feitosa, and Dassonneville 2023 for a recent application of ISSP to a related political-efficacy question). This choice carries a known limitation. The WVS behavioural items ask respondents whether they have ever engaged in an activity or done so in some cases within an unspecified recent period rather than a fixed reference window, so a country-year average conflates participation reported at different points in a respondent's life; Kołczyńska (2020, n. 2) discusses this recall-period ambiguity in harmonizing cross-national protest-participation data. Because the ambiguity applies to the participation measure at every wave and in every country, it is more likely to add noise to both the pooled and fixed-effects estimates than to bias one relative to the other, but it means the country-year averages used throughout this paper should be read as a general propensity to have participated rather than a precise measure of participation in a given year.

Constructing country-year participation measures. WVS data are collected at the individual respondent level and must first be aggregated to the country-year level to match the structure of the governance data. The WVS codes non-substantive responses using negative numeric values (e.g., don't know, no answer, not asked in this wave, missing). All such values were recoded as missing prior to aggregation so that they would not distort national averages. Respondents were then grouped by country and by the actual calendar year in which the survey was administered in that country rather than by wave number since a single WVS wave can span several calendar years across different countries. Within each country-year group, the arithmetic mean of each participation item was computed across all respondents with a valid response; because item coverage varies by wave, the number of respondents contributing to each average differs by variable and country-year.

Merging with governance data. Country-year WVS averages were merged with WGI estimates using an exact match on country and calendar year, with no interpolation to adjacent years. Two sources of missingness remain in the merged file. First, WGI was collected biennially prior to 2002 and annually thereafter, so WVS survey years falling in a non-WGI year have no exact governance match. Second, a small number of WVS entities (e.g., Andorra, Northern Ireland) are not separately listed in WGI and so carry missing governance values regardless of year. Rather than impute or borrow governance estimates from an adjacent year, these observations were retained with missing values on the affected dimensions.

The merged dataset comprises 270 country-year observations across 108 countries. Of these, 233 (86 percent) have a full match across all six WGI dimensions; the remainder are missing a subset of dimensions for the reasons described above. A subset of 69–73 countries contributed two or more WVS waves and forms the analytic sample for the country fixed-effects models, since within-country estimation requires at least two observations per country. Table 1 summarizes the dataset.

Table 1
Dataset overview.

Feature	Value
Country-year observations	270 (108 countries)
Observations with full WGI match	233 of 270 (86%)
WVS waves covered	Wave 3–Wave 7 (1996–2021)
Participation items retained	5 ($\leq 14\%$ missing each)
Participation items excluded	7 (26–89% missing, or conceptually distinct)
Governance dimensions (WGI)	6, plus 1 combined index
Countries with ≥ 2 WVS waves (fixed-effects sample)	69–73

3.2 Participation Measures

Five WVS participation items were retained for analysis, selected on the basis of data completeness (≤ 14 percent missing in the merged sample); all were rescaled so that higher values indicate more participation. Two are attitudinal (self-reported importance of and interest in politics) and three are behavioural, capturing whether the respondent has signed a petition, joined a boycott, or attended a peaceful demonstration. This behavioural and attitudinal split follows a broader distinction in the participation literature between what people say about politics and what they actually do, a distinction Prats and Meunier (2021) also use in comparing political efficacy to concrete participatory acts across European publics. Treating both categories under the umbrella term "participation" while analysing them separately throughout is a deliberate choice rather than a default one: the two categories are given identical treatment in every model in Section 3.4, and H3 is stated specifically to test whether they behave differently rather than assuming in advance that they do. Grouping them under one label is a matter of exposition; nothing in the analysis pools an attitudinal and a behavioural item into a single score, and Section 4.1 reports that the two categories in fact diverge sharply, which the shared label does not obscure. Table 2 lists the retained items.

Table 2
Retained participation items (n out of 270 country-years).

Code	Item	WVS scale	% missing	n
A004	Importance of politics	1 = very important... 4 = not at all	1.1%	267
E023	Interest in politics	1 = very interested... 4 = not at all	1.5%	266
E025	Signed a petition	1 = have done, 2 = might, 3 = never	4.1%	259
E026	Joined boycotts	1 = have done, 2 = might, 3 = never	5.2%	256
E027	Attended demonstrations	1 = have done, 2 = might, 3 = never	5.2%	256

Seven additional WVS participation items were excluded, primarily because they were fielded only in a subset of waves and consequently carry high missingness in the merged country-year sample (26–89 percent missing); one item (unofficial strikes) was excluded on conceptual grounds because it conflates organized labour action with general protest participation. Table 3 lists the excluded items and the reason for exclusion.

Table 3
Excluded participation items.

Code	Item	% missing	Reason excluded
E028	Joined unofficial strikes	26%	Conflates labour action with general protest
E025B, E026B, E028B	“Last 5 years” recency variants	61–80%	Fielded only in a subset of waves
E029	Occupying buildings/factories	71%	Fielded only in a subset of waves
E150	Follows political news	89%	Fielded only in a subset of waves
E282	Searches political info online	76%	Fielded only in a subset of waves

3.3 Governance Measures

Governance is measured using all six WGI dimensions, each scaled from approximately – 2.5 to + 2.5, together with a combined index computed as the simple average of the six dimensions ($n = 233$). Because all six WGI dimensions are constructed on the same approximately – 2.5 to + 2.5 scale using a comparable underlying estimation procedure (Kaufmann et al. 2011), an unweighted average is a standard simplification in the governance literature and is used here only as a summary companion to the dimension-by-dimension results, not as a replacement for them; results are reported both by individual dimension (Sections 4.1–4.3) and via the combined index (Section 4.4) precisely so that no single weighting choice drives the paper's conclusions. The six dimensions and the combined index are each modelled separately against each of the five participation items, yielding 35 distinct governance-participation pairs.

Using WGI as the sole governance measure is itself a limitation worth stating directly. The six dimensions are constructed from aggregated expert and perception-based surveys rather than from direct, physically measured administrative outcomes, and Langbein and Knack (2010) show that the six WGI dimensions load heavily on a single underlying factor, which raises the question of whether they capture six distinct aspects of governance or one broad perception of it measured six times. This does not obviously favour the between-country or the within-country estimate over the other, since both are constructed from the same WGI series, but it does mean the paper's conclusions describe how participation relates to perceived governance quality as WGI defines it, and a measure built from

administrative or spending data, such as government efficiency in budget execution, could in principle show a different within-country pattern.

3.4 Empirical Strategy

For every participation-governance pair, three models are estimated in sequence to isolate the between-country and within-country relationships:

Model 1: Bivariate correlation. The Pearson correlation between the participation item and the governance dimension, pooled across all country-year observations.

Model 2: Pooled OLS. $\text{Governance}_{it} = \alpha + \beta \cdot \text{Participation}_{it} + \varepsilon_{it}$, estimated across all country-year observations without country-specific intercepts. This coefficient identifies the between-country association: it captures whether countries with systematically higher participation also have systematically higher governance quality, pooling within- and between-country variation together.

Model 3: Country fixed-effects OLS. $\text{Governance}_{it} = \alpha_i + \beta \cdot \text{Participation}_{it} + \varepsilon_{it}$, which adds a country-specific intercept α_i for each of the 69–73 countries with at least two WVS waves. Because α_i absorbs every time-invariant country characteristic, such as historical institutions, geography, colonial legacy, and any other stable confound, the coefficient β in this model is identified purely from within-country change: it asks whether a country's governance score moves when that same country's participation score moves, relative to that country's own average.

All models are estimated separately for each of the six WGI dimensions and for the combined index, producing a total of 35 coefficient estimates per model type. Standard errors in Models 1–3 are conventional OLS standard errors, not clustered by country. Because most countries contribute more than one observation to the pooled sample, these standard errors likely understate the true sampling uncertainty and should be read as an unadjusted baseline; Section 3.5 re-estimates the pooled and fixed-effects models with country-clustered standard errors, and it is that specification (Table 8), not Table 5 or Table 6, that we treat as the more credible basis for inference throughout the paper. Given the modest number of countries in the fixed-effects sample (69–73), coefficients in Model 3 should in any case be interpreted with the wide confidence intervals appropriate to a short panel, a point we return to in Section 6.

3.5 Extended Empirical Strategy: GDP, Two-Way Fixed Effects, and Temporal Ordering

The three-model design described above is extended in six ways to address the most plausible remaining threats to validity: economic development as a common-cause confound, global time trends, within-country error correlation, reverse causation, and confounding from observed country characteristics, specifically GDP and regime type. GDP is entered directly as a linear covariate in the pooled and fixed-effects models; regime type is instead addressed through the inverse probability weighting and propensity score matching checks in Sections 4.6 and 4.7, which balance high- and low-

participation countries on regime type directly rather than assuming its relationship to governance is linear.

GDP Control

Log GDP (current-USD, log-transformed) is added as a covariate to the pooled and fixed-effects models, directly testing whether the between-country association in Table 5 survives conditioning on economic development.

Two-way fixed effects

The country fixed-effects model (Model 3) is extended to include year fixed effects as well as country fixed effects. Country effects absorb time-invariant national characteristics; year effects absorb global shocks and trends, such as a worldwide shift in governance quality, that could otherwise be mistaken for a within-country participation effect. Imai and Kim (2021) show that two-way fixed-effects estimators can behave in unexpected ways when treatment effects vary across units and over time, which is a further reason to treat the within-country coefficients here as suggestive rather than as a fully identified causal estimate.

Cluster-robust standard errors

All models now cluster standard errors by country rather than using conventional OLS standard errors. With 2–5 observations per country, errors are almost certainly correlated within a country over time; conventional SEs understate this and can produce spurious significance.

Lagged specification: A fourth model tests whether participation in the prior WVS wave predicts a change in governance, controlling for governance in the prior wave and current GDP: $\text{Governance}_i = \alpha + \beta_1 \cdot \text{Participation}_{i-1} + \beta_2 \cdot \text{Governance}_{i-1} + \beta_3 \cdot \log \text{GDP}_i + \varepsilon$. This is estimated pooled (without country dummies) because combining country fixed effects with a lagged dependent variable in a short panel ($T = 2-5$) is known to bias the lagged-DV coefficient (Nickell bias); the lagged-DV specification is used here as an alternative identification strategy, not a complement to Model 3.

Inverse probability weighting (IPW)

As a complementary check on observed confounding, countries are reweighted using propensity scores estimated from log GDP and Polity2 regime type, directly balancing high- and low-participation country-years on these two covariates without discarding observations.

Propensity score matching (PSM)

The same propensity scores are used to construct one-to-one nearest-neighbour matches between high- and low-participation countries, enforcing a common-support requirement that reweighting does not. Results are reported across a range of caliper widths to ensure robustness to the choice of matching threshold.

All models are re-estimated for every participation item against every governance dimension and the combined index (35 pairs per model).

4. Results

4.1 Correlations

Table 4 and Fig. 1 report the bivariate correlation between each participation item and each WGI dimension. The two attitudinal items show weak, inconsistent, and sometimes negative correlations with governance quality ($|r| \leq 0.16$). All three behavioural items are strongly and positively correlated with every WGI dimension, with petition-signing showing the strongest relationship ($r = 0.62\text{--}0.75$ across dimensions, all $p < .001$). Figure 2 illustrates this pattern for the single strongest pair, petition-signing and Voice and Accountability.

Table 4
Pearson correlations (r); all behavioral-item correlations significant at $p < .001$.

Participation item	Voice & Acct.	Pol. Stability	Govt Effect.	Reg. Quality	Rule of Law	Control of Corr.
Importance of politics	-0.16	-0.12	0.05	-0.02	-0.02	0.01
Interest in politics	0.01	0.05	0.11	0.06	0.13	0.13
Signed a petition	0.75	0.62	0.66	0.63	0.70	0.71
Joined boycotts	0.52	0.40	0.50	0.44	0.53	0.53
Attended demonstrations	0.53	0.32	0.31	0.32	0.39	0.39

4.2 Pooled Regression

Table 5 reports pooled OLS coefficients for every participation-governance pair, and Fig. 3 summarizes mean explanatory power by participation item. Consistent with H1 and H3, behavioral items show large, highly significant positive coefficients across all six governance dimensions, with boycotting and demonstrating producing the largest point estimates (β up to 2.29) and petition-signing producing the highest explained variance (mean $R^2 = 0.46$ across dimensions). Attitudinal items show small, generally non-significant coefficients, with the exception of a significant negative coefficient for importance of politics on Voice and Accountability.

Table 5

Pooled OLS coefficients (β), unclustered SEs; see Table 8 for the country-clustered, GDP-adjusted estimates used for inference. *** $p < .001$, * $p < .05$.

Participation item	Voice & Acct.	Pol. Stability	Govt Effect.	Reg. Quality	Rule of Law	Control of Corr.
Importance of politics	-0.53*	-0.44	0.19	-0.07	-0.06	0.04
Interest in politics	0.03	0.18	0.40	0.22	0.45*	0.49*
Signed a petition	1.62***	1.43***	1.51***	1.41***	1.61***	1.77***
Joined boycotts	1.96***	1.62***	1.99***	1.73***	2.14***	2.29***
Attended demonstrations	2.07***	1.32***	1.27***	1.30***	1.61***	1.74***

4.3 Country Fixed-Effects Regression

Table 6 reports the within-country estimates from the fixed-effects specification. The pattern is not consistent with H2: every behavioural item loses its pooled association once country fixed effects are introduced. Boycotting turns significantly negative for two dimensions (Government Effectiveness and Regulatory Quality), and interest in politics turns significantly negative for three dimensions (Political Stability, Government Effectiveness, and Rule of Law). Petition-signing, the strongest pooled predictor, shows no significant within-country coefficient for any dimension. Figure 4 displays the full pooled-versus-within-country contrast across all six dimensions simultaneously, making clear that the pooled coefficient (blue) collapses toward zero in the within-country estimate (orange) in every panel.

Table 6

Country fixed-effects OLS coefficients (β), unclustered SEs; ** $p < .01$, * $p < .05$. $n = 193-204$; 70–73 countries.

Participation item	Voice & Acct.	Pol. Stability	Govt Effect.	Reg. Quality	Rule of Law	Control of Corr.
Importance of politics	0.02	-0.27	-0.12	0.05	-0.16	-0.05
Interest in politics	-0.11	-0.33*	-0.30*	-0.22	-0.31**	-0.15
Signed a petition	0.14	0.09	-0.09	0.07	0.19	0.29
Joined boycotts	-0.07	-0.01	-0.47*	-0.45*	-0.27	-0.11
Attended demonstrations	0.18	0.06	-0.30	0.00	-0.04	0.08

4.4 Combined Governance Index

Table 7 and Figs. 5–7 repeat the analysis against a single combined governance index (the average of all six WGI dimensions, $n = 233$), summarizing the dimension-by-dimension pattern in a single comparison. The combined-index results reproduce the dimension-level pattern exactly: petition-signing shows the strongest pooled relationship ($r = 0.71$, $R^2 = 0.51$) but a near-zero, non-significant within-country coefficient ($\beta = 0.11$); interest in politics is the only item that remains significant under fixed effects, and its sign flips from positive (though non-significant) in the pooled model to significantly negative within countries.

Table 7

Combined governance index: correlation, pooled OLS, and fixed-effects OLS. *** $p < .001$, * $p < .05$.

Participation item	r (corr.)	Pooled β	Pooled R^2	FE β	FE n / countries
Importance of politics	-0.04	-0.14	0.00	-0.09	203 / 73
Interest in politics	0.09	0.30	0.01	-0.24*	204 / 73
Signed a petition	0.71***	1.56***	0.51	0.11	195 / 70
Joined boycotts	0.51***	1.95***	0.26	-0.23	194 / 70
Attended demonstrations	0.39***	1.55***	0.15	-0.00	193 / 70

4.5 Results with GDP, Two-Way Fixed Effects, and Lagged Specification

GDP does not explain away the between-country association: Adding log GDP to the pooled model leaves the behavioural-participation coefficients essentially unchanged from Table 5 (e.g., petition-signing on the combined index: $\beta = 1.50$, $p < .001$, vs. 1.56 without GDP). GDP itself is a significant positive predictor of governance in most specifications, but it operates alongside participation rather than substituting for it. This means the between-country correlation documented in Section 4.2 is not simply relabelled economic development. Some other stable, country-level factor, or a bundle of factors that includes but is not limited to GDP, is doing the work that the country fixed effects in Model 3 absorb.

The null within-country result survives, and becomes more clearly null under correct inference: Under two-way fixed effects with GDP control and country-clustered standard errors, no behavioural-participation coefficient reaches significance on the combined governance index, and the few significant negative coefficients reported in the original Table 6 (joined boycotts on Government Effectiveness and Regulatory Quality; interest in politics on three dimensions) mostly do not survive once standard errors are clustered and year effects are added. This is a more conservative conclusion than the original fixed-effects table, and it strengthens rather than weakens H2's rejection: the small number of "significant" within-country findings in the original design look more consistent with using under-clustered standard errors on a 30-plus-test panel than with a genuine suppressive effect.

The lagged specification provides the strongest available test and finds essentially nothing:

Governance is highly persistent. The coefficient on lagged governance ranges from 0.82 to 0.99 across all 35 pairs, all significant at $p < .001$, so most of the variance in current governance is explained by where a country's governance already stood one wave earlier. After accounting for that persistence and for GDP, lagged participation predicts virtually none of the remaining variation. Of 35 participation and governance pairs, only 2 reach $p < .05$: joined boycotts predicting Regulatory Quality and interest in politics predicting Regulatory Quality. This is close to what chance alone would produce at a 5 percent threshold across that many tests, rather than a systematic pattern. No participation item shows a consistent lagged effect across governance dimensions.

Table 8

Pooled + GDP, two-way fixed effects + GDP (country-clustered SEs), and lagged-participation coefficients, combined governance index. *** $p < .001$.

Participation item	Pooled + GDP (β)	Two-way FE + GDP (β)	Lagged participation (β)
Importance of politics	-0.47	0.04	0.06
Interest in politics	0.18	-0.06	0.12
Signed a petition	1.50***	0.27	-0.01
Joined boycotts	1.75***	0.06	0.15
Attended demonstrations	1.55***	0.09	0.07

4.6 Robustness Check: Inverse Probability Weighting on Observed Confounders

Country fixed effects address confounding from stable, unmeasured country characteristics, but they say nothing about confounding from characteristics that can be measured. As a complementary check, this section applies inverse probability weighting (IPW) following Rosenbaum and Rubin (1983) to ask whether the pooled participation and governance association survives once countries are directly balanced on two observed confounders: GDP and regime type. Regime type is measured with the Polity2 score from the Polity5 project (Marshall and Gurr 2020), merged into the dataset by country and year. Polity2 coverage is unavailable for a small number of territories that Polity does not code as independent states, such as Hong Kong, Puerto Rico, and Andorra, leaving an analytic sample of 210 of the 270 country-years.

Countries were split into high and low participation groups at the median value of petition-signing, the strongest behavioural predictor in Table 5, producing 105 country-years in each group. Appendix Table A1 reports covariate balance before and after weighting. Before weighting, the two groups differ enormously in regime type: the high-participation group averages a Polity2 score of 8.2, solidly democratic, against 3.0 for the low-participation group. A propensity score model predicting group membership from log GDP and Polity2 was then used to construct inverse probability weights, and after weighting the two groups are closely balanced on both covariates.

Full covariate-balance results are reported in Appendix Table A1.

The unweighted difference in the combined governance index between high- and low-participation countries is 0.99, close to the raw pooled association reported in Table 7. Once the sample is reweighted to balance GDP and regime type, this difference falls to 0.47 and remains highly significant ($p < .001$). GDP and regime type together account for roughly half of the raw cross-sectional gap between high- and low-participation countries, but a sizeable and statistically significant association remains even after balancing on both.

This result sits in some tension with the country fixed-effects estimates in Table 6 and Table 7, which found the within-country association close to zero. There are two ways to read that tension. One possibility is that country fixed effects are absorbing additional stable confounders beyond GDP and regime type, such as historical institutions or colonial legacy, that IPW cannot reach because it only balances the confounders actually included in the propensity model. The other possibility is that the short WVS panel simply leaves the fixed-effects estimates underpowered relative to a cross-sectional comparison, so a moderate true association could survive an IPW check while remaining statistically undetectable within countries over only two to five waves.

4.7 Robustness Check: Propensity Score Matching

As a complementary check to the inverse probability weighting analysis in Section 4.6, the same propensity scores (estimated from a logit model of participation-group membership on log GDP and Polity2) were used to construct one-to-one nearest-neighbor matches between high- and low-participation countries, rather than reweighting the full sample. Matching enforces a common-support requirement that reweighting does not: a treated country-year without a control country-year at a similar propensity score is left unmatched rather than downweighted. Matches were restricted to a caliper on the logit propensity score, and results are reported across a range of caliper widths from 0.1 to 0.5 standard deviations to check that the estimate is not an artifact of any single arbitrary threshold.

Full propensity score matching results across caliper widths are reported in Appendix Table A2.

Across all four caliper widths, the matched-sample estimate is stable, ranging from 0.33 to 0.42, and remains statistically significant throughout ($p < .05$ in every specification). At the conventional 0.2 standard-deviation caliper, 54 of the 105 treated country-years find an acceptable match, an estimated difference of 0.37 (SE = 0.13, $p = .008$), with post-match covariate balance closely comparable to the IPW result in Appendix Table A1 (log GDP: 25.21 versus 25.64; Polity2: 6.89 versus 6.59).

The substantive conclusion is unchanged: participation and governance remain associated even after directly balancing on GDP and regime type, at a magnitude consistent with both the IPW estimate in Section 4.6 and, more loosely, the raw cross-sectional gap in Table 7. The more consequential finding from this check is that only about half of the treated sample has an adequate match on observed confounders. Roughly half of the high-participation country-years have no low-participation counterpart with comparable wealth and regime type, meaning the two groups occupy substantially non-overlapping

regions of the GDP and regime-type space. Inverse probability weighting, which reweights rather than discards observations, does not surface this limited overlap, since every observation continues to contribute to the weighted estimate regardless of how poor its match would be. This limited common support is itself informative: it strengthens the case for the country fixed-effects design as the more credible identification strategy in this paper, since that design relies on within-country variation over time rather than requiring a comparable country to exist in the cross-section.

4.8 Robustness Check: Dynamic Panel System GMM

The lagged specification in Section 3.4 controls for governance persistence but cannot be combined with country fixed effects in a panel this short, since the resulting Nickell bias would contaminate the lagged-dependent-variable coefficient (Nickell 1981). Blundell-Bond system GMM (Blundell and Bond 1998), an extension of the difference GMM estimator of Arellano and Bond (1991), was designed specifically for this situation: it instruments the lagged dependent variable using its own past levels and differences, allowing governance persistence and country-specific effects to be modeled simultaneously without the bias that rules out a naive fixed-effects-plus-lag specification. Each of the five participation items was estimated separately against the combined governance index in a two-step system GMM model, with Windmeijer (2005) finite-sample-corrected standard errors, collapsed instruments to limit instrument proliferation given the short panel, and wave dummies. Log GDP and the participation item enter as instrumented variables using their own lagged levels and differences.

Because system GMM identifies effects from first-differenced variation, only country-years with an adjacent prior wave contribute to estimation. Of the 97 to 100 countries retained in each model (after requiring non-missing governance, participation, and GDP data), 29 contribute only a single wave and cannot be differenced at all; roughly 40 countries with three or more waves supply most of the identifying variation. This is the same short-panel constraint noted throughout this paper, made explicit here rather than left implicit.

Appendix Table A3 reports the participation coefficient, the lagged-governance coefficient, and model diagnostics for each of the five items. No participation item reaches conventional significance; coefficients range from -0.47 to 0.08 , all with p -values above 0.5 . The lagged-governance coefficient is large and consistently positive, reaching statistical significance for petition-signing ($\beta = 1.07$, $p < .001$) and marginal significance for joined boycotts ($\beta = 0.70$, $p = .069$), confirming that governance is highly persistent and that little room remains for participation to explain additional variation once that persistence is modeled directly. Both diagnostic tests support the validity of this specification: the Hansen test of overidentifying restrictions does not reject for any of the five models ($p = 0.24$ – 0.94), and the Arellano-Bond test for second-order autocorrelation does not reject for any model ($p = 0.36$ – 0.80), indicating no evidence of instrument invalidity or residual serial correlation.

Full dynamic panel system GMM results and diagnostics are reported in Appendix Table A3.

These results converge with the fixed-effects, GDP-controlled, and lagged specifications reported in Sections 4.3 through 4.5: across five separate econometric approaches, each addressing a distinct potential weakness in the others, political participation does not predict governance quality once a country is compared to itself over time.

5. Discussion

The central finding of this paper is the contrast between Tables 5 through 7 and Figs. 4 and 7. Behavioural political participation is among the strongest bivariate predictors of governance quality reported anywhere in the cross-national literature when countries are compared to one another, but this relationship is not detectable when a country is compared to itself across time. H1 is strongly supported; H2 is not supported by the within-country evidence presented here. This pattern fits the common-cause account described in Section 2.2 better than a direct causal account in which rising participation functions as a lever that improves governance. Economic development and democratic consolidation plausibly raise both participation and governance at the same time, the former by expanding civic capacity and the latter by expanding state capacity, without either variable causing the other.

The result also supports H3: behavioural items (petition-signing, boycotting, demonstrating) show a dramatically stronger between-country association than the two attitudinal items (importance of and interest in politics), consistent with the idea that behavioural participation proxies for civic and organizational infrastructure that itself correlates with governance capacity, whereas self-reported political interest does not require that infrastructure to be expressed.

The negative within-country coefficients for interest in politics and boycotting (Table 6) merit separate attention, since a negative sign is the opposite of what either the direct-causal or the simple common-cause account would predict. One explanation consistent with the reverse-causation account in Section 2.2 is short-run reverse causation. A governance shock, such as a corruption scandal, a contested election, or a sudden decline in government effectiveness, may provoke a temporary rise in political interest and boycott activity within the same country-year, generating a negative association between the change in participation and the already falling level of governance. This design cannot distinguish that account from a genuine, modest suppressive effect of these specific participation modes on short-run governance scores. Both processes would produce the same signed coefficient in a fixed-effects model with this panel structure.

This conclusion is reinforced, not weakened, by additional robustness checks reported in Section 4.5. GDP is a significant covariate but does not account for the pooled participation and governance association on its own, so the common cause described above should be understood as a bundle of stable national characteristics, of which GDP is one candidate, rather than economic development alone. The within-country null result is robust to correcting the two main statistical weaknesses of the original fixed-effects design (uncorrected standard errors and the omission of global time trends), and a more temporally demanding test, lagged participation controlling for governance persistence and GDP, finds

no systematic effect. A fifth and most demanding test, dynamic panel system GMM estimated separately for each participation item (Section 4.8), reaches the same conclusion: once governance persistence is modelled directly, rather than left to a lagged-dependent-variable specification that cannot be combined with country fixed effects in a short panel, no participation item predicts governance quality, and all five models pass Hansen overidentification and second-order autocorrelation diagnostics. Across five increasingly demanding specifications, H1 holds and H2 does not.

The inverse probability weighting check in Section 4.6 adds a useful qualification to that picture rather than a contradiction of it. Balancing high- and low-participation countries on GDP and regime type cuts the raw association roughly in half, but a moderate, statistically significant gap remains, which is not what the near-zero fixed-effects estimates would lead one to expect if GDP and regime type were the whole story. Of these two readings, the power explanation is more consistent with the rest of the evidence in this paper. If fixed effects were absorbing meaningful additional confounders beyond GDP and regime type, that same absorption should also compress the pooled GDP-adjusted association reported in Section 4.5, yet Table 8 shows the pooled association essentially unchanged once GDP is added. It is difficult to reconcile a story in which unmeasured stable confounders are large enough to erase the within-country association, but too weak to move the between-country association even when the model directly conditions on the observed confounders. A simpler account is that the true within-country effect is real but modest, and a panel of two to five waves per country lacks the power to distinguish a small effect from zero. The IPW estimate, drawn from the full cross-section rather than a short time series, is not subject to that power constraint, which is consistent with it detecting an association the fixed-effects design cannot.

6. Conclusion

This paper's central contribution is to show that a claim central to democratic theory, that civic participation disciplines officials and improves governance, depends entirely on the level of comparison at which it is tested. The cross-national correlation that has anchored this claim in the literature is large, robust to GDP and regime-type controls, and consistent with decades of prior work. But once the same relationship is tested within countries over time, using the same data and the same governance measures, the association is not detectable. This pattern is difficult to reconcile with a direct causal account in which participation itself improves governance, and instead favours an account in which participation and governance are joint products of deeper, slower-moving national characteristics, a distinction the existing cross-national literature has generally not been positioned to test, since it typically compares countries to each other rather than to themselves over time. The methodological device that makes this test possible, adapted from the within-unit logic used to resolve similar ambiguity in individual-level participation research (Quintelier and van Deth 2014), is a means to that theoretical end rather than the paper's primary contribution in its own right.

Whether political participation predicts governance quality depends critically on whether the comparison is made between countries or within them. Pooled, cross-sectional analysis of WVS and WGI data shows

large, statistically significant, positive associations, led by petition-signing (R^2 up to 0.56 against individual WGI dimensions, 0.51 against the combined index). This is a relationship consistent with much of the existing cross-national literature on civic engagement and governance. These associations are not detectable once country fixed effects remove stable, time-invariant national characteristics, nor under a dynamic panel system GMM specification that explicitly models governance persistence (Section 4.8), indicating that the cross-national correlation more plausibly reflects a shared upstream cause, such as economic development, than a direct causal pathway from participation to governance. Given the short underlying panel, this conclusion is best read as a consistent absence of a detectable within-country effect across two independent identification strategies, rather than definitive proof that no causal channel exists.

6.1 Limitations

Several scope conditions bound this conclusion. The fixed-effects design isolates within-country change from stable national confounders, which is precisely what the between/within comparison is built to do, but by design it cannot adjudicate reverse causation or confounders that move over time, questions that call for a different identification strategy than the one used here. The WVS panel structure, with most countries contributing two to five waves, means the within-country estimates carry wide confidence intervals; a null result under these conditions constrains the range of plausible effects without foreclosing a small one. The IPW and PSM checks are best read as a targeted test of the observed confounders most theorized to matter (GDP and regime type) rather than as a general-purpose causal estimate, and the common-support constraints in the matched sample are consistent with that more targeted role. The system GMM estimates in Section 4.8 were included specifically because a short panel with a lagged-dependent-variable specification cannot sustain a fixed-effects causal claim on its own; those estimates draw their identifying variation mainly from the roughly 40 countries with three or more WVS waves, so like the fixed-effects results, they are best understood as evidence against a detectable effect at this panel length, rather than as a final causal test.

6.2 Directions for Future Research

Several extensions would further sharpen the causal interpretation offered here, beyond the dynamic panel system GMM estimates already reported in Section 4.8. An instrumental-variables design would offer a more direct causal test than fixed effects alone: a plausible instrument would need to shift participation without an independent effect on governance, such as historically determined mobilization infrastructure or exogenous variation in mobile and internet access that predates its effect on governance capacity. The negative within-country coefficients for interest in politics and boycotting reported in Table 6 also warrant closer study in their own right; an event-study design centered on specific governance shocks, such as contested elections or corruption scandals, could help distinguish the reverse-causation account described in Section 2.2 from a genuine short-run suppressive effect, since this paper's design cannot separate the two. Finally, as additional WVS waves accumulate, longer

panels will increase the power of the within-country test and help distinguish a genuine null effect from one that is simply difficult to detect with two to five observations per country.

Declarations

Ethics approval:

Ethical approval was not required for this study because it relied exclusively on publicly available secondary datasets and did not involve human participants or identifiable personal data.

Competing interests:

The authors have no competing interests to declare that are relevant to the content of this article.

AI Disclosure Statement

Generative AI (Claude, Anthropic) was used in preparing this manuscript in two ways. First, for copyediting, prose revision, and formatting of existing content, however this did not affect the methodology, analysis, or conclusions. Second, it was used to help conduct the propensity score matching analysis in Section 4.7 and Table 10 (including the matching procedure, sensitivity analysis across caliper widths, and related prose). The author reviewed and verified this AI-assisted analysis and takes full responsibility for all claims, interpretations, and conclusions in the manuscript.

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Author Contribution

Hira Yousaf: Conceptualization; Methodology; Formal analysis; Data curation; Investigation; Visualization; Writing – original draft; Writing – review & editing. Ghulam Mustafa Mir: Methodology; Validation; Formal analysis; Interpretation of results; Writing – review & editing.

Data Availability

Replication code for all analyses reported in this paper, along with the merged country-year dataset, will be made publicly available in an online repository upon acceptance. Underlying survey data are publicly available from the World Values Survey and the World Bank Worldwide Governance Indicators.

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Appendix

This appendix reports full results for three complementary robustness checks discussed in Sections 4.6 through 4.8: covariate balance under inverse probability weighting (Table A1), propensity score matching across caliper widths (Table A2), and dynamic panel system GMM estimates and diagnostics (Table A3).

Table A1

Covariate balance before and after inverse probability weighting. Polity2 ranges from - 10 (fully autocratic) to + 10 (fully democratic).

Covariate	Treated mean (raw)	Control mean (raw)	Treated mean (weighted)	Control mean (weighted)
Log GDP	26.21	25.34	25.41	25.62
Polity2 (regime type)	8.22	2.98	4.96	5.21

Table A2

Propensity score matching results across caliper widths. Estimated difference is the mean matched-pair difference in the combined governance index between high- and low-participation countries.

Caliper (SD of logit ps)	N matched pairs	Estimated difference	SE	p-value
0.10	48	0.33	0.14	.026
0.20	54	0.37	0.13	.008
0.25	56	0.41	0.13	.004
0.50	58	0.42	0.13	.002

Table A3

Dynamic panel system GMM: participation coefficient and diagnostics, combined governance index. Two-step estimates with Windmeijer-corrected standard errors and collapsed instruments. *** p < .001.

Participation item	Coef. (β)	SE	p-value	Lagged gov. (β)	Hansen p	AR(2) p	N
Importance of politics	0.06	0.23	0.78	0.66	0.24	0.36	100
Interest in politics	-0.10	0.44	0.81	0.52	0.24	0.46	100
Signed a petition	0.08	0.45	0.86	1.07***	0.79	0.49	98
Joined boycotts	-0.47	0.71	0.51	0.70	0.94	0.39	97
Attended demonstrations	-0.33	0.94	0.72	0.74	0.80	0.80	97

Figures

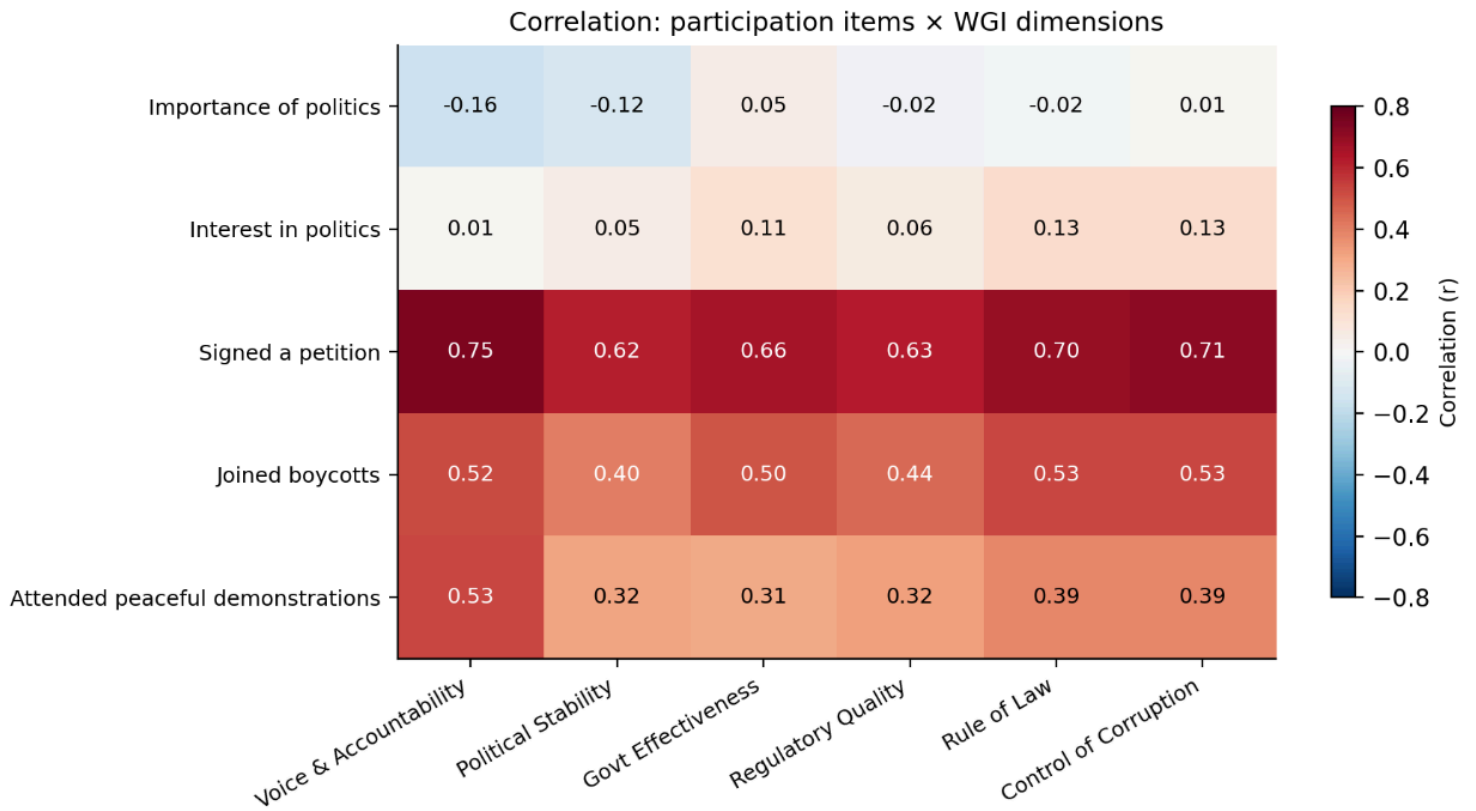


Figure 1

Correlation heatmap, participation items × WGI dimensions.

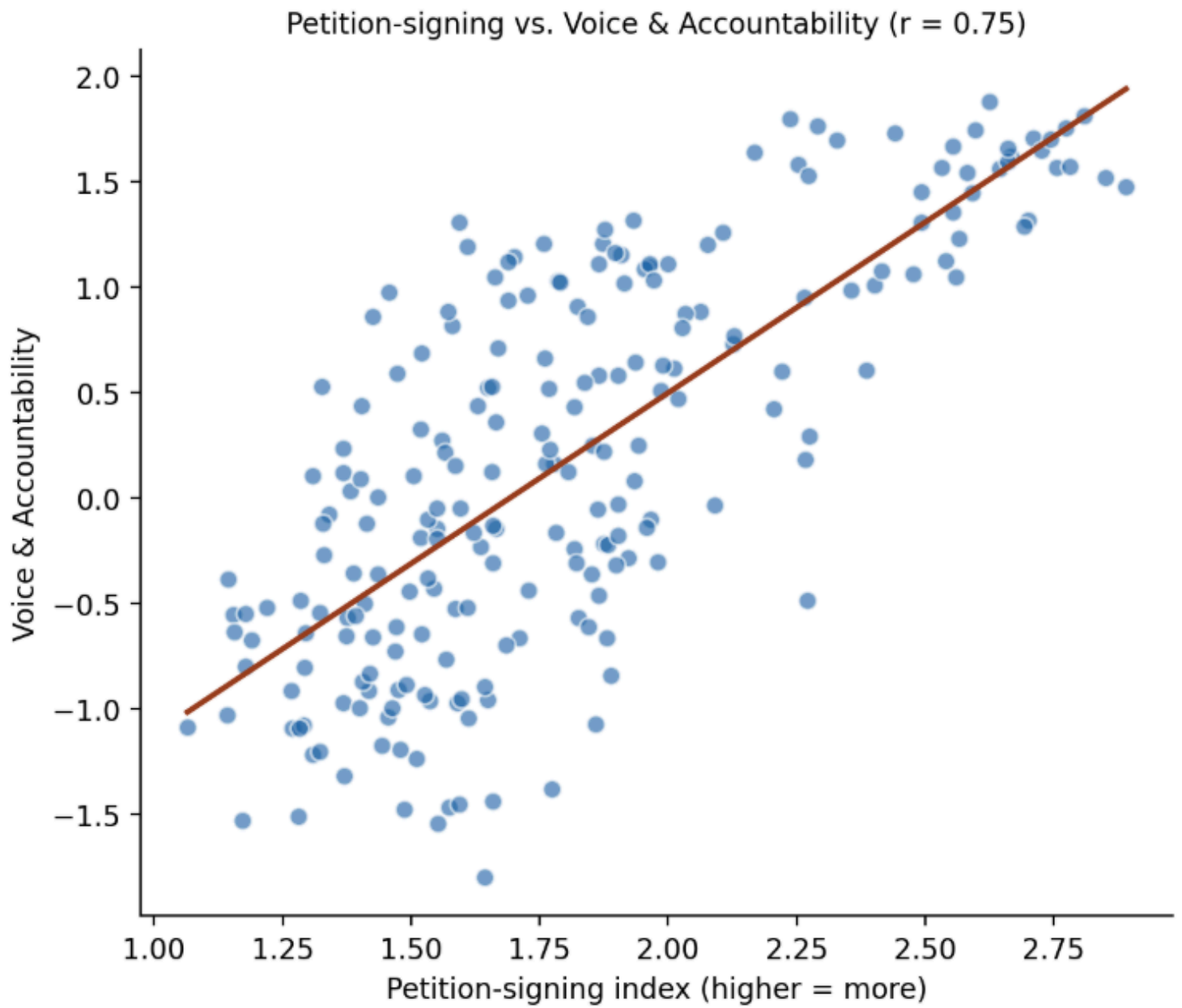


Figure 2

Petition-signing vs. Voice & Accountability, the single strongest pairwise relationship ($r = 0.75$, $n = 224$).

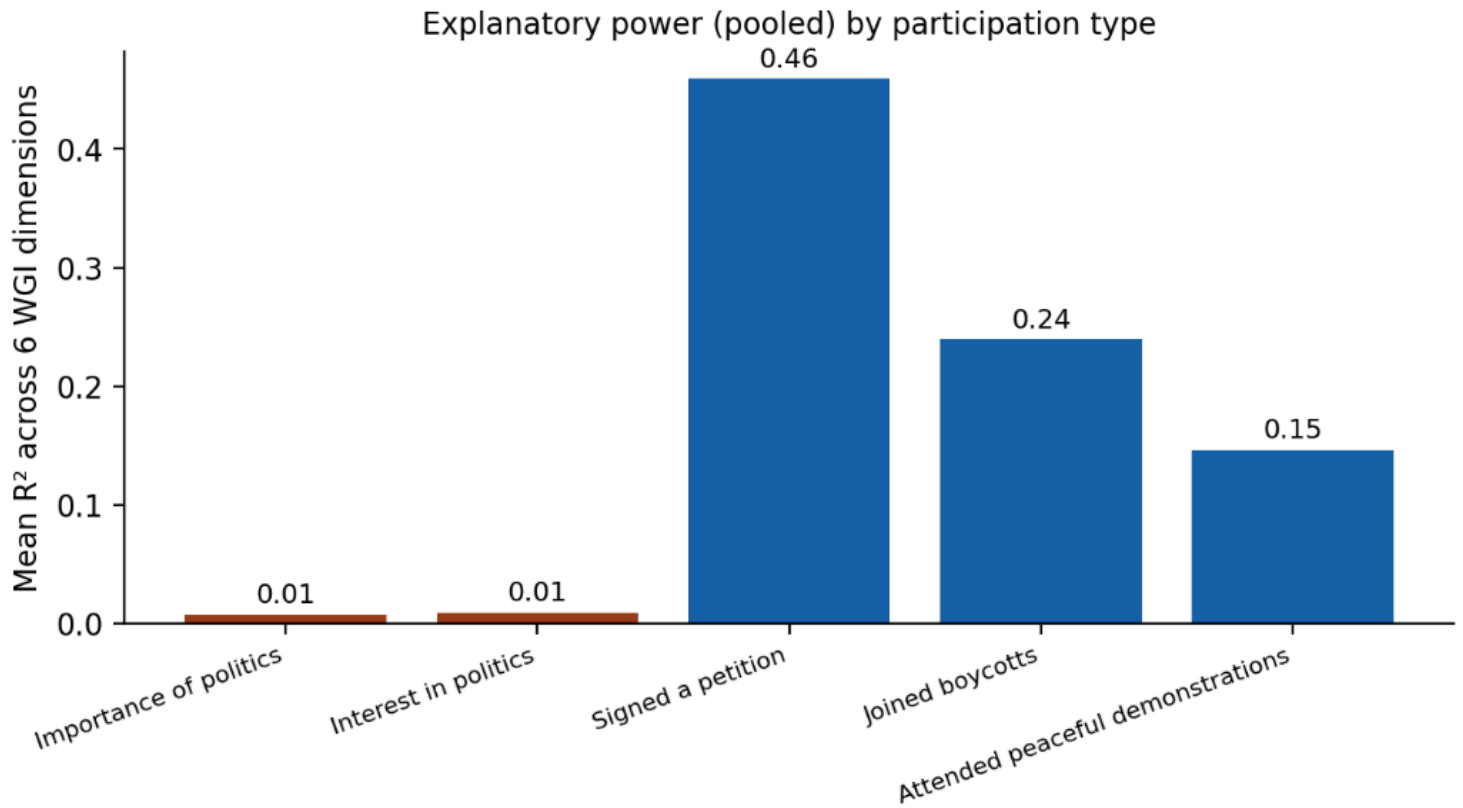


Figure 3

Mean R^2 across all six WGI dimensions, by participation item (pooled models).

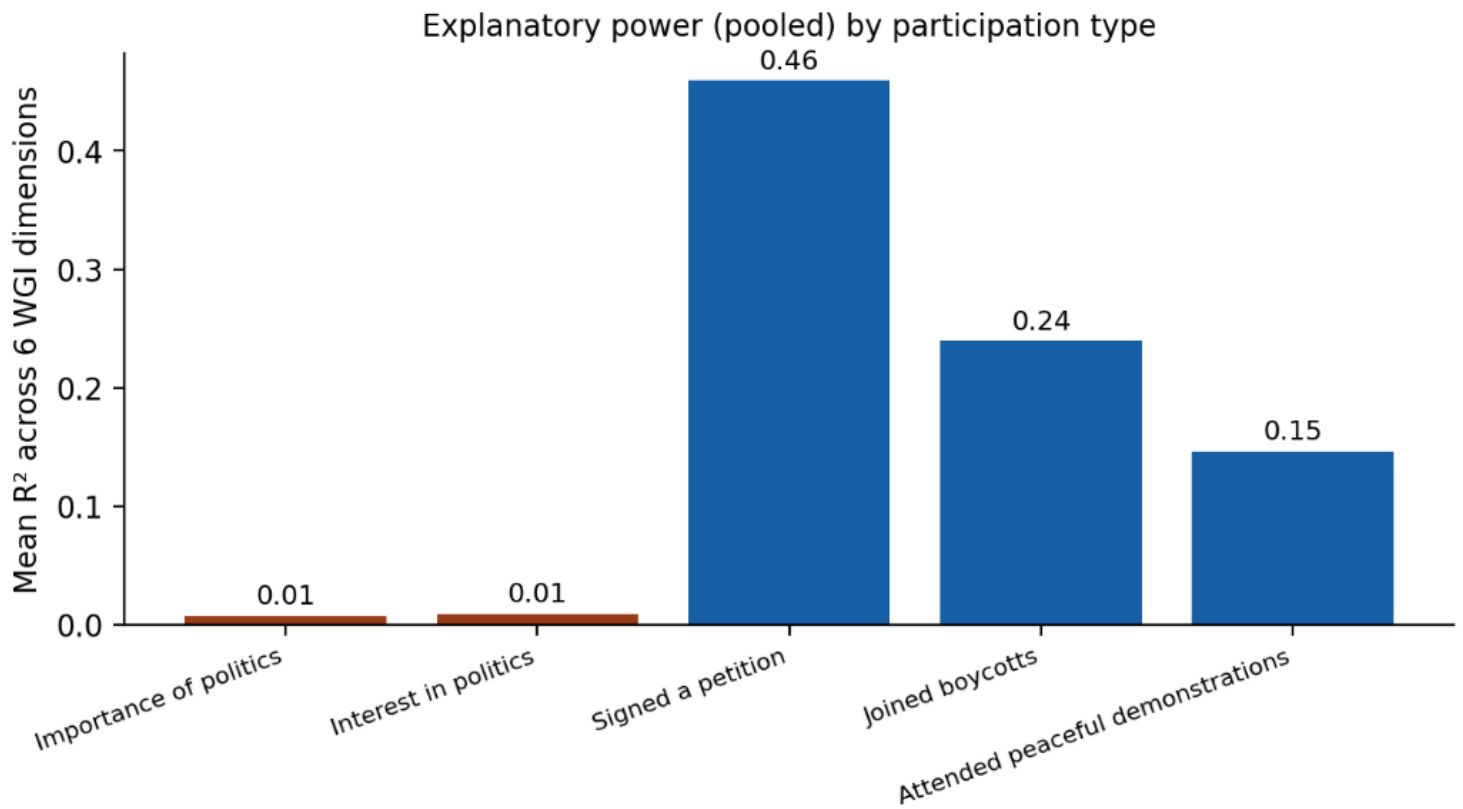


Figure 3

Mean R^2 across all six WGI dimensions, by participation item (pooled models).

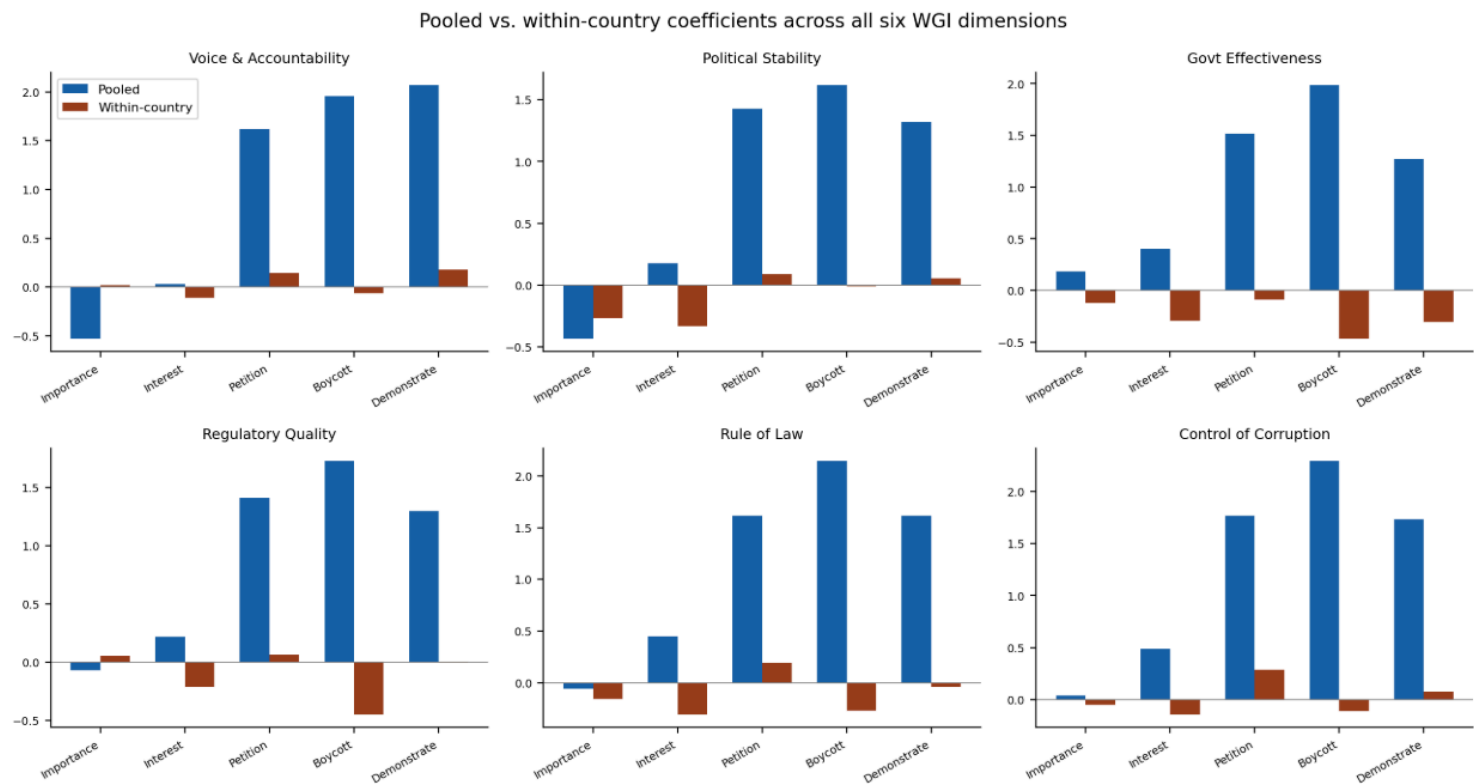


Figure 4

Pooled (blue) vs. within-country (orange) coefficients across all six WGI dimensions. The pooled effect collapses toward zero in every panel.

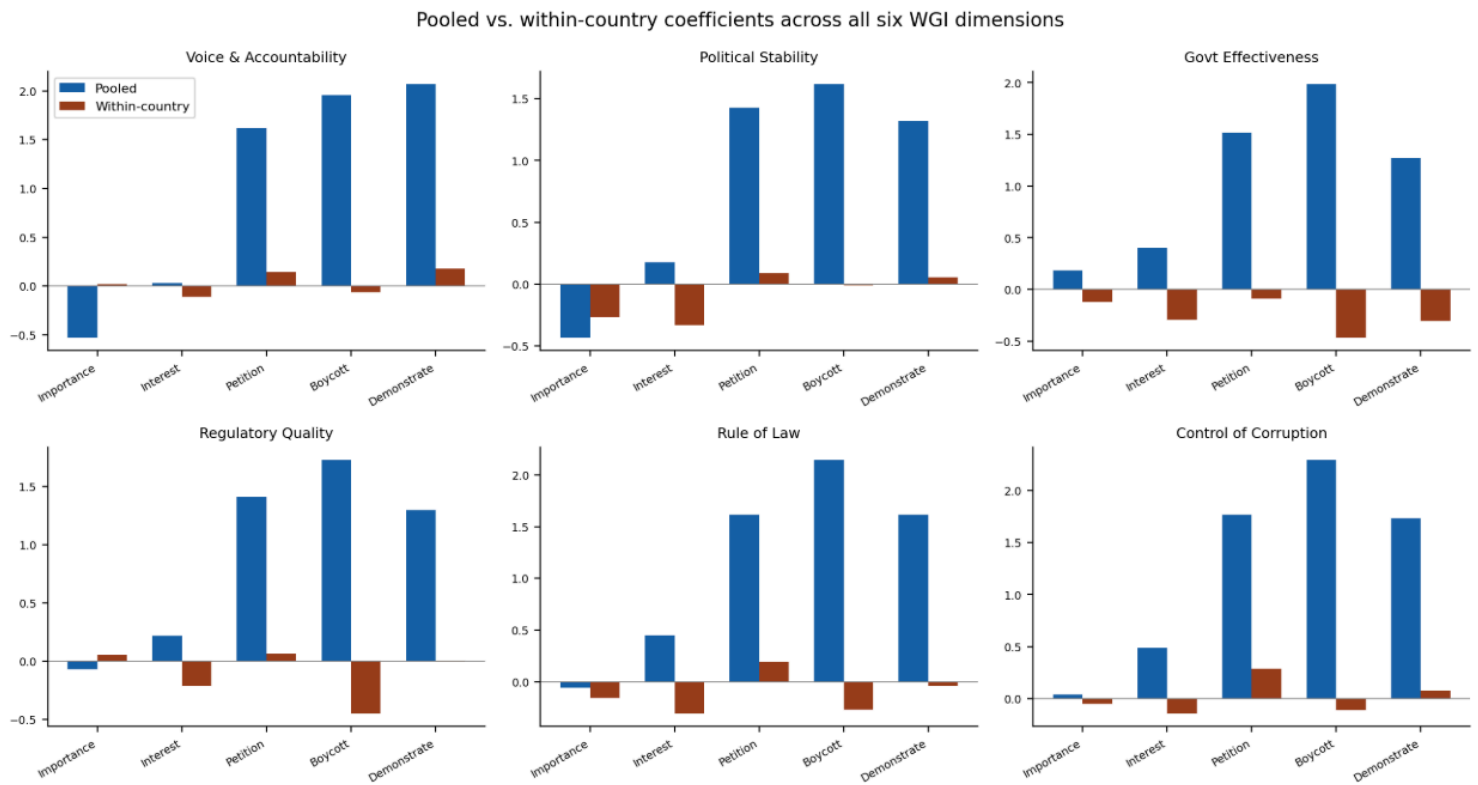


Figure 4

Pooled (blue) vs. within-country (orange) coefficients across all six WGI dimensions. The pooled effect collapses toward zero in every panel.

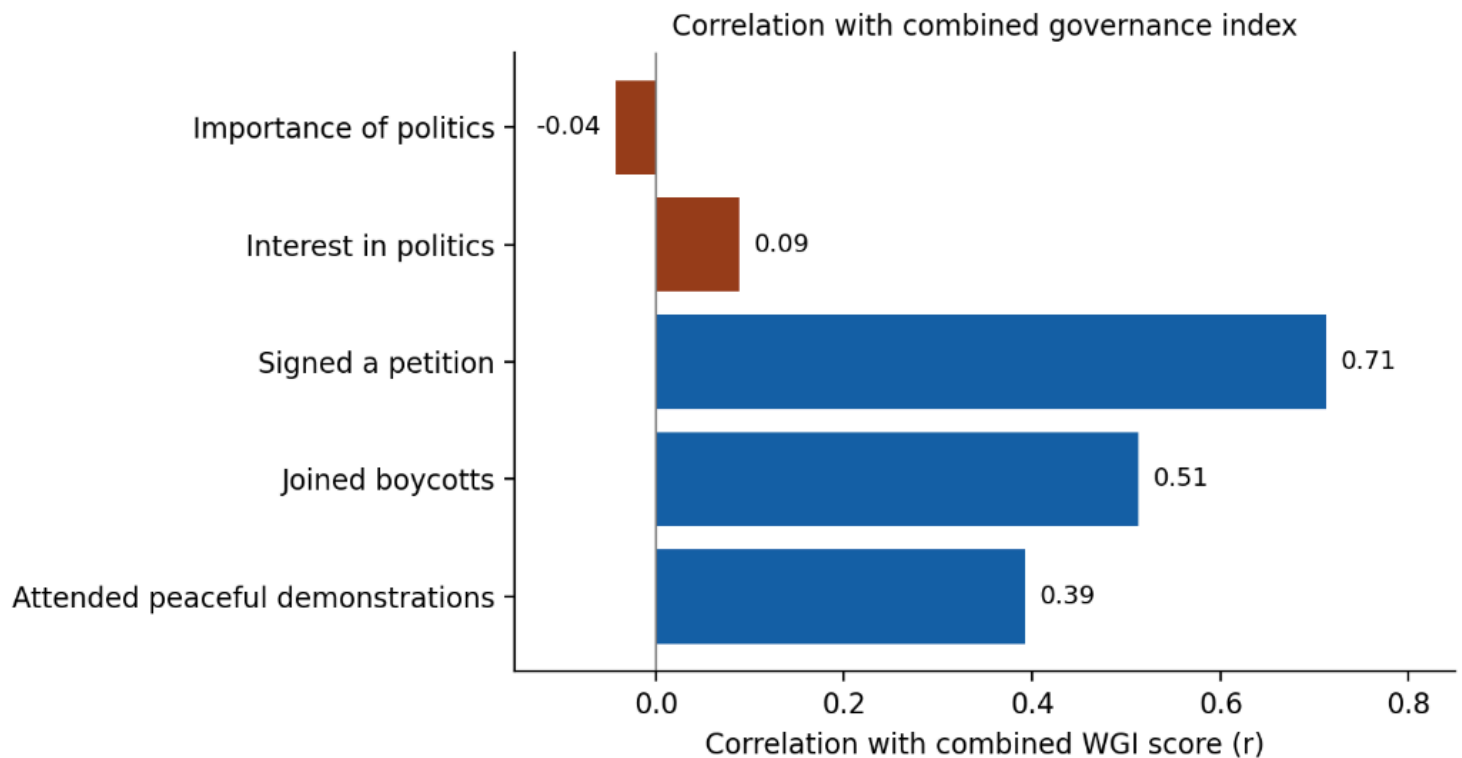


Figure 5

Correlation of each participation item with the combined governance index.

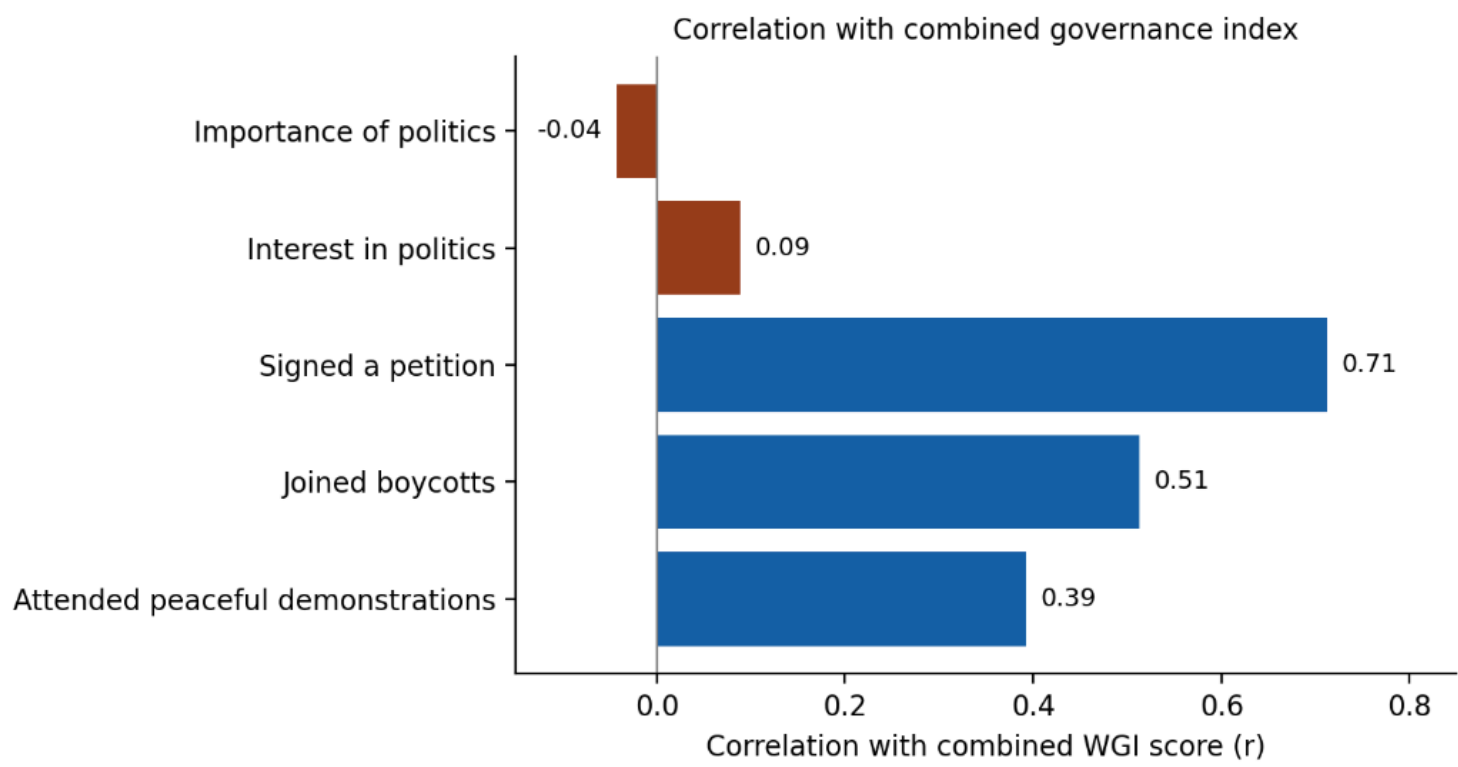


Figure 5

Correlation of each participation item with the combined governance index.

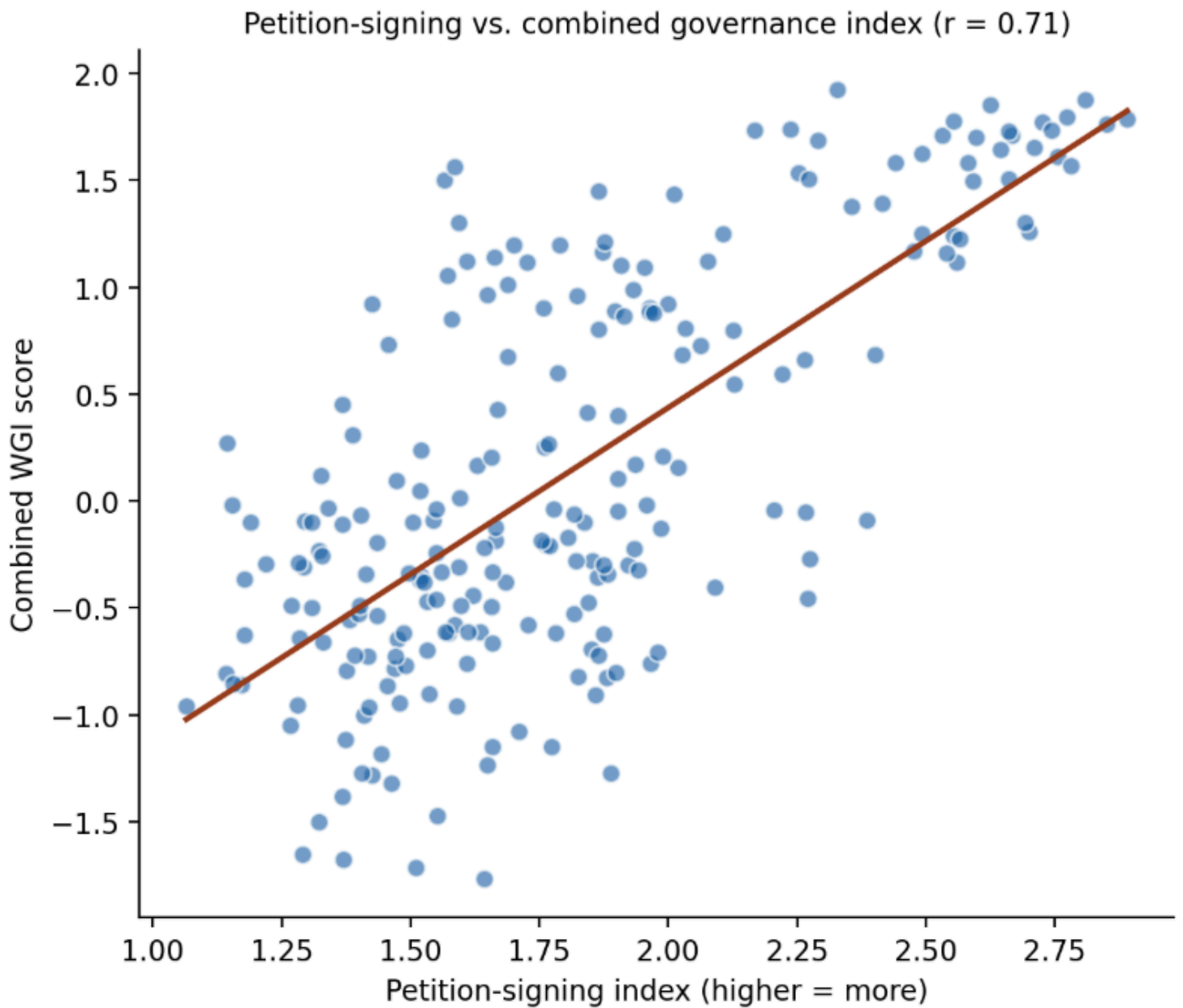


Figure 6

Petition-signing vs. combined governance index ($r = 0.71$, $n = 224$), the strongest pooled relationship using any version of the governance measure.

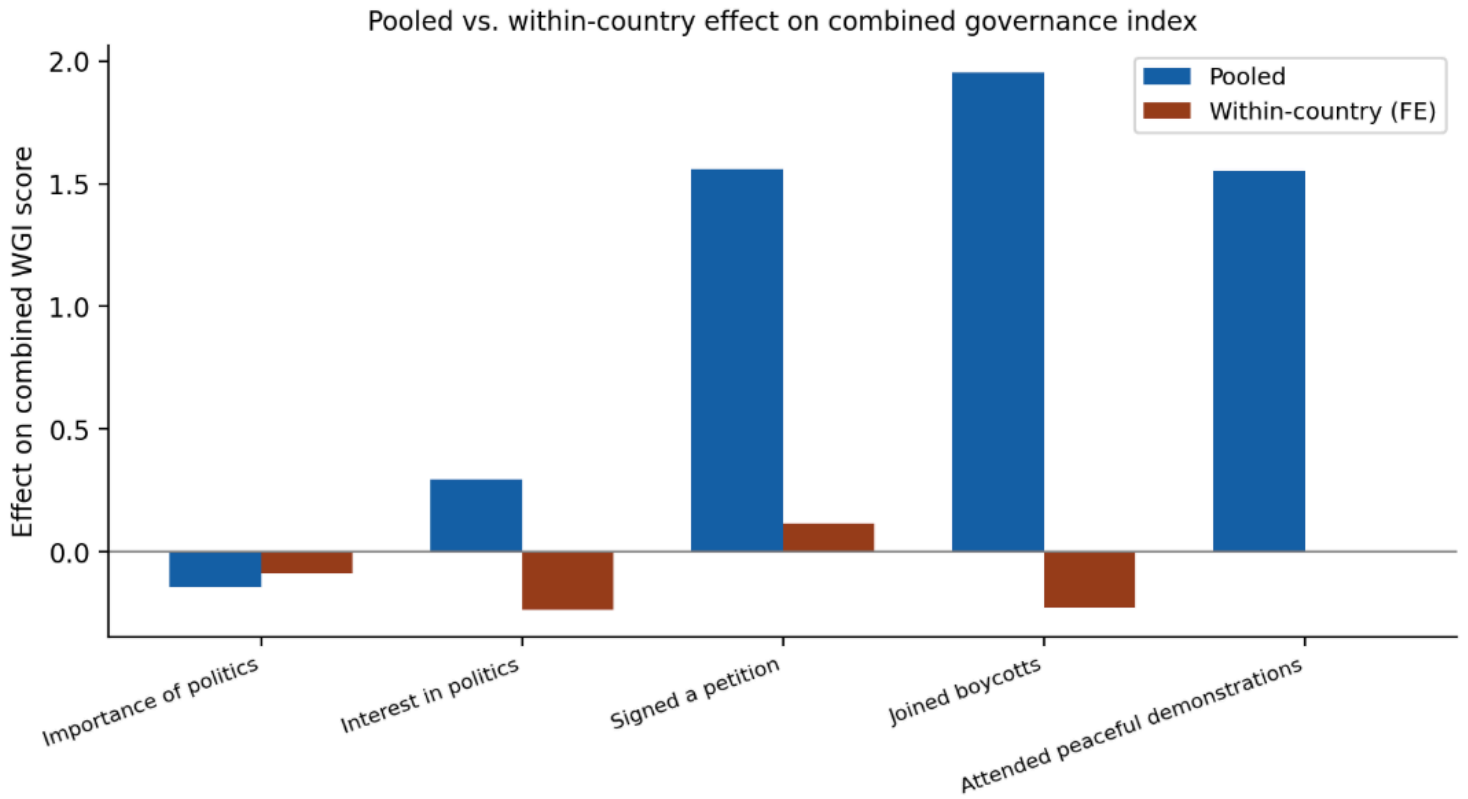


Figure 7

Pooled vs. within-country coefficients on the combined governance index. Every coefficient shrinks toward zero under fixed effects; only interest in politics remains significant, and it flips negative.

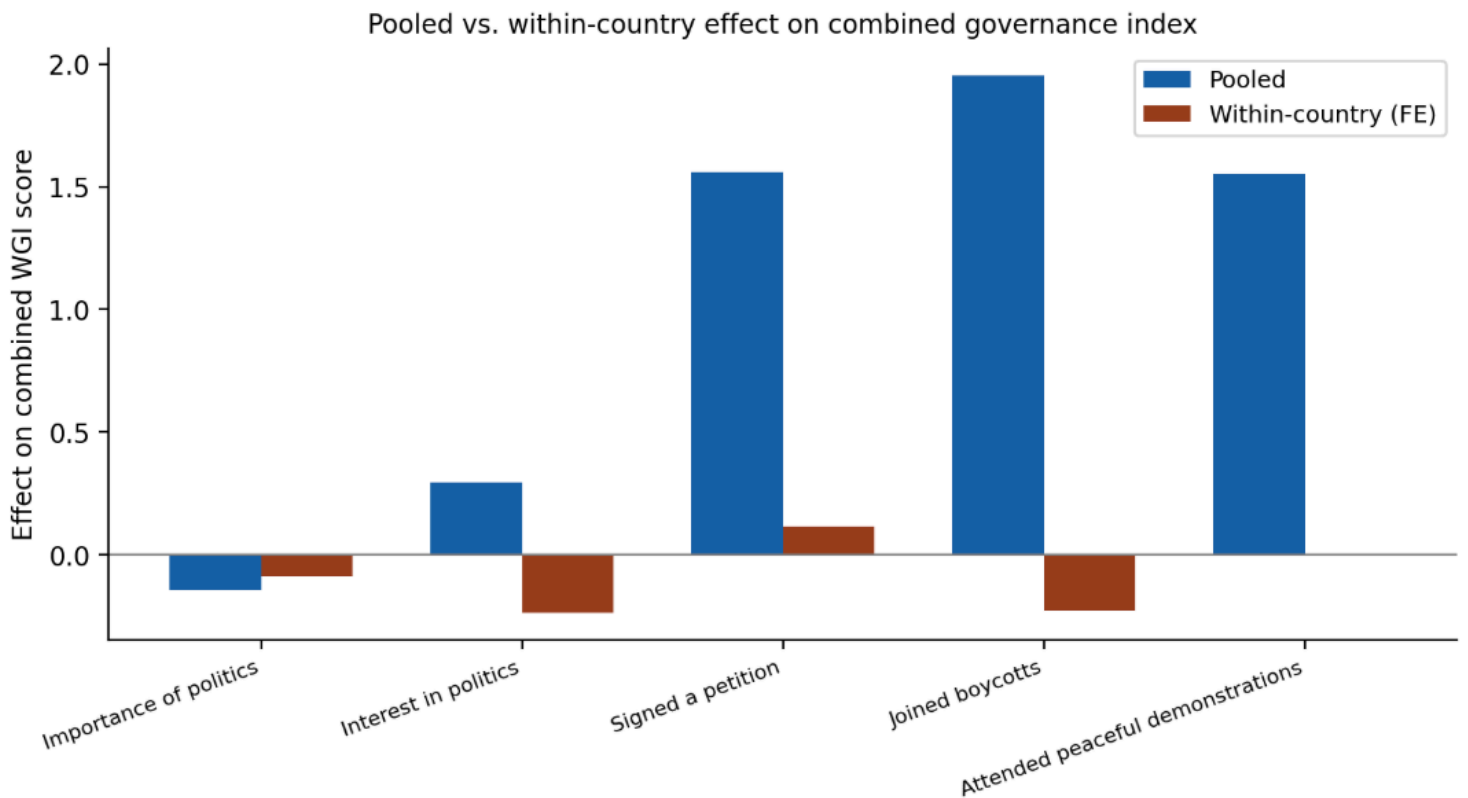


Figure 7

Pooled vs. within-country coefficients on the combined governance index. Every coefficient shrinks toward zero under fixed effects; only interest in politics remains significant, and it flips negative.