UNIVERSITY OF WARSAW | FACULTY OF ECONOMIC SCIENCES

WORKING PAPERS
No. 12/2026 (506)

CULTURAL DIVIDES AND POPULIST VOTING: EVIDENCE FROM A GLOBAL PANEL

MICHAL BRZEZINSKI

WARSAW 2026

ISSN 2957-0506

WORKING PAPERS 12/2026 (506)

Cultural Divides and Populist Voting: Evidence from a Global Panel

Michal Brzezinski[1*]

[1] *University of Warsaw, Faculty of Economic Sciences*
[*] *Corresponding author: mbrzezinski@wne.uw.edu.pl*

Abstract: Cultural heterogeneity—the within-country dispersion of values measured across hundreds of survey items—predicts populist voting across 60 democracies from 1970 to 2019. I compute fractionalization and polarization indices on Integrated Values Survey value items and regress three populism measures on these indices in a dynamic two-way fixed-effects panel with country-level clustering. A one-within-country-standard-deviation increase in cultural polarization is associated with a rise in vote-weighted ideational populism of about 14\ robust to system-GMM and to a sensitivity analysis for selection on unobservables. A V-Party-based GAL–TAN decomposition shows that the mobilization skews toward culturally-authoritarian parties, with no detectable movement in the progressive-libertarian camp. Including behavioral items in the heterogeneity measure eliminates the effect; the values-versus-behaviors boundary is empirically decisive.

Keywords: populism, cultural heterogeneity, fractionalization, polarization, GAL–TAN

JEL codes: D72, Z13, P16, C23

Acknowledgments: I acknowledge funding from the National Science Centre, Poland (grant no. 2019/35/B/HS4/01527). I thank participants at seminars at the University of Warsaw and GESIS – Leibniz Institute for the Social Sciences in Cologne for helpful comments. All remaining errors are my own.

1. Introduction

Populist parties have moved from the political fringe to the core of electoral competition. Fidesz has governed Hungary since 2010; the Law and Justice party shaped Polish politics for a decade; the Alternative for Germany now polls as the largest party in several eastern *Länder*; the Freedom Party of Austria won the 2024 parliamentary election outright. The combined vote share of parties classified as populist by the PopuList 3.0 coding (Rooduijn et al. 2024) rose from under 7% in the late 1980s to above 27% in the late 2010s across the 30 European democracies covered by that source. The rise spans the ethnonationalist right in Central Europe, the techno-populist center in Italy, and the radical left in Greece and Spain.

Political economists offer two broad explanations. One emphasizes economic distress: unemployment, import competition from China, rising inequality, the differential exposure of regions and skill groups to globalization (Algan et al. 2017; Guiso et al. 2019; Rodrik 2018; Colantone and Stanig 2018; Dal Bó et al. 2023). The other emphasizes cultural backlash against immigration, the erosion of traditional values, and the perceived dominance of cosmopolitan elites (Inglehart and Norris 2017; Mudde 2004; Mudde 2007). Both lines treat culture crudely. The economic literature controls for culture with dummies for religion or with ethnolinguistic fractionalization; the cultural-backlash literature focuses on a narrow set of items and does not summarize the overall dispersion of values within a society. Neither asks whether the internal cultural heterogeneity of a country, measured directly over hundreds of value items, predicts populist voting.

This paper examines whether within-country dispersion of cultural values predicts populist electoral success across 60 democracies between 1970 and 2019. I compute the cultural-heterogeneity indices of Desmet et al. (2017) and Desmet et al. (2025b)—the fractionalization index CF and the polarization index CP_α—over individual responses to Integrated Values Survey items.[1] I merge them with vote-weighted populism measures from Celico et al. (2024)—hereafter CRR, after the authors Celico, Rode, and Rodriguez-Carreño—and the V-Dem V-Party dataset (Lührmann et al. 2020). The preferred specification is a dynamic two-way fixed-effects panel with country and year effects, a lagged dependent variable, and eleven macroeconomic and institutional controls.

A one-within-country-standard-deviation increase in $CP_{0.5}$ is associated with a 0.07-unit rise in vote-weighted CRR ideational populism on its 0–10 scale—about 14 percent of the populism outcome's within-country standard deviation. The per-unit coefficient is 8.6 ($p = 0.02$) on ideational populism and 7.4 ($p = 0.03$) on rhetorical populism. Under a parsimonious system GMM specification (Kripfganz 2019), the same coefficient rises to 17.6

[1] Fractionalization is the probability that two randomly drawn respondents disagree; polarization captures whether disagreement concentrates into two opposing camps. Formal definitions are in Section 3; I use $\alpha = 0.5$ as the baseline and examine $\alpha = 1$ in the robustness battery.

($p = 0.03$) on ideational and 16.6 ($p = 0.08$) on rhetorical populism, with CF turning significant for rhetorical populism (5.3, $p = 0.04$). The finding holds for both CRR aggregations (ideational and rhetorical). The V-Dem V-Party populism index—which aggregates party-level anti-elitism and people-centrism through a harmonic mean—is directionally consistent but imprecise in the pooled sample.

Two features of the design bound the most obvious endogeneity channels, though neither delivers point identification. Cultural heterogeneity enters each observation from the IVS wave that precedes the election, and replacing it with its one-election lag yields coefficients equal to or larger than the baseline—the opposite ordering from what reverse causality would imply. The pooled effect is insensitive to the exclusion of any individual country. Section 4.2 develops the endogeneity argument in full and states the assumption under which the coefficient admits a causal reading.

A measurement choice drives the visibility of the relationship. The Desmet et al. framework applied to the raw 395-item IVS set yields a null coefficient ($\hat{\beta}_{CP} = 1.76$, SE = 2.89). Removing the 100 behavioral items from that set, and nothing else, raises the coefficient to 9.52 ($p = 0.02$). The 259-item curated subset used throughout the paper preserves moral, normative, and attitudinal beliefs while dropping self-reported behaviors such as turnout, party membership, and media consumption. A four-category decomposition of the filtering (behavioral, evaluative, apparatus-confidence, demographic) shows that the behavioral items alone account for the entire shift. Future work applying cultural-heterogeneity measurement to political outcomes should distinguish values from behaviors at the item-selection stage.

The cultural-polarization effect skews ideologically toward the authoritarian-nationalist (TAN) end of the GAL–TAN dimension (Hooghe et al. 2002; Jolly et al. 2022). I construct a V-Party cultural composite from four items—immigration, LGBT equality, minority rights, and women's labor-force position—standardized and equal-weighted, and classify parties above the sample median as TAN. The composite places Fidesz, the Law and Justice party, the Alternative for Germany, and the Brothers of Italy in the TAN camp; SYRIZA, Podemos, Die Linke, La France Insoumise, and the Greens across Europe and Anglophone democracies in the GAL camp. Under Model 3, the cultural-polarization coefficient on vote-weighted CRR ideational populism is 12.0 ($p = 0.02$) in the TAN camp and -3.2 ($p = 0.49$) in the GAL camp; the corresponding rhetorical coefficients are 16.5 ($p = 0.02$) and -9.5 ($p = 0.10$). A continuous specification weighting each party's populism contribution by its GAL–TAN position yields positive and statistically significant coefficients on both outcomes. Under GMM the TAN coefficient remains positive and of similar magnitude, though with standard errors roughly two to three times larger; the paper's ideological-decomposition claim therefore rests on fixed-effects inference with GMM reported for directional confirmation.

The paper makes two contributions. The first is a measurement contribution: the values-versus-behaviors distinction is empirically decisive for political outcomes, and the Desmet et al. (2017) framework delivers a populism signal only when applied to values items. The second is substantive. Earlier work has connected cultural heterogeneity to economic growth (Desmet et al. 2017), civil conflict (Esteban et al. 2012), and redistribution (Alesina et al. 2003), and has connected populism to immigration (Inglehart and Norris 2017), economic shocks (Guiso et al. 2019; Dal Bó et al. 2023), and elite rhetoric (Bonikowski and Gidron 2016), but has not applied survey-based within-country value dispersion to populist electoral success. Here, value dispersion predicts vote-weighted populism across 60 democracies, and the marginal populist voter flows disproportionately to culturally-authoritarian parties. An income-level pattern—the pooled effect is sharper in less-affluent democracies under Model 3 but does not survive GMM on the 25-country subsample—is documented in the appendix and not claimed as a primary finding.

Section 2 describes the data. Section 3 presents the cultural-heterogeneity indices and the curation-sensitivity decomposition. Section 4 describes the specification and the endogeneity argument. Section 5 reports the pooled result, the GAL–TAN ideological decomposition, and a robustness battery that includes system GMM. Section 6 interprets the findings. Section 7 concludes.

2. Data

I assemble a country-election panel from four data sources: the Integrated Values Survey for cultural-heterogeneity measurement; the party-level populism scores of Celico et al. (2024) for vote-weighted ideational and rhetorical populism; the V-Dem V-Party dataset for a third, independently constructed populism measure; and the PopuList 3.0 database (Rooduijn et al. 2024) merged with ParlGov 2024 for a European robustness check. Macroeconomic controls come from the Penn World Table (version 10.01), the World Bank, the KOF Globalisation Index, and the World Values Survey contextual file. The estimation sample covers 60 democracies and 348 country-election observations between 1970 and 2019.[2]

2.1. The Integrated Values Survey

The Integrated Values Survey (IVS) is the official harmonized pooling of the World Values Survey and the European Values Study. I use the June 2023 release, which contains 666,907 respondents across 118 countries, seven survey waves (1981–1984, 1989–1993, 1994–1998, 1999–2003, 2005–2009, 2010–2016, 2017–2022), and 838 variables. Each respondent records a country (WVS/EVS code `S003`) and a field year (`S020`). I define the sur-

[2] Electoral data end in 2019; the IVS survey data extend to the 2017–2022 wave.

vey year of a country-wave cell as the median of `S020` across respondents in that cell.[3] The IVS is the largest repeated cross-sectional survey of cultural attitudes with consistent wording, coding, and fieldwork standards across the OECD and most of Latin America, Central and Eastern Europe, Africa, and East Asia.[4]

2.2. Populism outcomes

Three independently constructed populism measures form the main outcomes. The first is the CRR *ideational* populism score of Celico et al. (2024). The index is a Random Forest regression prediction that uses V-Party's expert-coded party characteristics (Lührmann et al. 2020)—anti-elitism, people-centrism, pluralism, personalism, cultural superiority, stance toward political opponents, immigration, minorities, LGBT rights, and political violence—to reproduce the ideational populism ratings of the Populism and Political Party Experts Survey (POPPA), yielding continuous scores on a 0–10 scale for 1,920 parties across 169 countries between 1970 and 2019. It captures the degree to which parties combine a Manichean (Mudde 2004) people-versus-elite confrontation with people-centrism and anti-pluralism. The second is the *rhetorical* populism score from the same source (also 0–10), trained instead on the Global Party Survey (GPS), which rates parties on their use of populist versus pluralist rhetoric rather than on programmatic substance. A party can score high on rhetorical populism without an ideologically coherent populist platform, and vice versa. The third is the V-Dem V-Party populism indicator (Lührmann et al. 2020), a 0–1 synthetic score computed from expert ratings of anti-elitism and people-centrism via a harmonic-mean aggregation. All three measures derive from the V-Party expert codings—CRR ideational and rhetorical as Random Forest predictions calibrated to POPPA and GPS respectively, V-Dem V-Party as a harmonic mean of two V-Party items. Consistency across them therefore probes functional-form robustness over a shared expert-coding base rather than independent triangulation; I reserve the latter term for the comparison with PopuList (Appendix G), which is the one populism classification in this paper that does not use V-Party inputs. Each measure is aggregated at the country-election level as a vote-weighted mean of party scores—the sum across parties of each party's vote share times its populism score. I refer to this vote-weighted continuous index as *vote-weighted populism* or *populist voting* throughout the paper, reserving the term *vote share* for the PopuList-based outcomes in Appendix G, where parties are binary-classified as populist, far-right, or far-left. Vote-weighted rather than seat-weighted aggregation is the standard in the populism literature (Guiso et al. 2019; Inglehart and Norris 2017; Dal Bó et al. 2023; Algan et al.

[3] This accounts for staggered fieldwork schedules within waves, which typically span one to three calendar years per country.

[4] The IVS is maintained by the GESIS Leibniz Institute for the Social Sciences and is freely available at `https://www.worldvaluessurvey.org`.

2017; Rodrik 2018) because seat translation is mediated by electoral rules (thresholds, district magnitude, PR vs. plurality) that are conceptually distinct from voter behavior. I report seat-weighted equivalents in the robustness section (Section 5).

Two features of this aggregation choice deserve explicit defense. First, the vote-weighted continuous index assigns positive weight to every party with non-zero populism score, not only to parties that pass an external populism threshold (as in the PopuList approach of Rooduijn et al. 2024). This avoids the arbitrary dichotomization that threshold-based measures impose—whether a party scoring 4.9 on the 0–10 CRR scale is "populist" depends on an externally chosen cutoff, while the continuous measure captures the full intensity of the populist appeal. The cost is that mainstream parties with modest populist content contribute to the country-election score; conceptually, this is a feature rather than a bug, because the paper's question is how much populist content enters vote-weighted electoral expression, not how many votes go to a binary-classified populist party. Second, the continuous aggregation reflects the ideational tradition's claim that populism is a matter of degree (Meijers and Zaslove 2021; Mudde 2004) rather than a discrete party property. The PopuList binary classification is reported as a cross-measure robustness check in Appendix G.

The populism literature distinguishes at least four approaches to operationalizing the concept, and it is worth locating the measures used here against that conceptual map. The *ideational* tradition, following Mudde (2004), treats populism as a thin-centred ideology combining a Manichean moral view of society with the assertion that politics should express the general will of a homogeneous people; it is typically measured via expert surveys or party-level text coding. The *discursive* tradition (Hawkins 2009) operationalizes populism as a rhetorical style detected in political speech through holistic grading. The *content-analytic* tradition (Rooduijn 2014) codes the presence of populism's definitional elements (people-centrism, elite criticism, a homogeneous people, a crisis frame) in manifestos or speeches. The *public-opinion* tradition (Castanho Silva et al. 2018; Wuttke et al. 2020) measures populism at the voter level through survey-scale instruments. Among these, the CRR ideational score is calibrated to the POPPA expert-survey ratings of Meijers and Zaslove (2021), who instantiate the ideational tradition with five subdimensions (Manichean worldview, indivisible people, general will, people-centrism, anti-elitism); CRR rhetorical is calibrated to the GPS survey; V-Dem V-Party harmonizes two ideational-tradition items; PopuList implements an expert-informed qualitative classification closer to the content-analytic tradition. The outcomes I use are therefore Random-Forest reconstructions of the ideational-tradition expert consensus (CRR, V-Dem) or a content-analytic binary (PopuList) — not discursive measures of speech or public-opinion scales of voter attitudes. The associations I report are between within-country value dispersion and the ideational-populist content of parties' platforms aggregated by vote share.

For a European-restricted robustness test, I add the PopuList 3.0 classification of Rooduijn et al. (2024), which assigns binary labels (populist, far-right, far-left, Eurosceptic) to 234 parties in 30 European countries between 1989 and 2022 via expert-informed qualitative comparative classification. I merge these labels to ParlGov 2024 election results on the `parlgov_id` key and compute the country-election vote share of parties classified as populist, as far-right, and as far-left. The cross-measure correlation between CRR ideational populism and PopuList total populism on overlapping country-years is $\rho = 0.53$; between the CRR rhetorical score and PopuList it is $\rho = 0.41$; between V-Dem V-Party populism and PopuList it is $\rho = 0.45$. Far-right and far-left vote shares are near-orthogonal ($\rho = -0.05$); the left–right distinction in PopuList is substantively meaningful.[5]

Cultural-heterogeneity indices — fractionalization (CF, CFD) and polarization (CP_α, CPD_α), developed by Desmet et al. (2017) and Desmet et al. (2025b) — are computed on selected IVS items; the construction and item-selection filtering are in Section 3.

2.3. Control variables

Eleven time-varying controls enter the main specification. Five describe macroeconomic conditions: log real GDP per capita (Penn World Table 10.01), real GDP growth (PWT), the unemployment rate (World Bank), CPI inflation (World Bank), and the share of the population aged 15–64 (PWT). Three describe institutions: a labor-market-regulation index, a social-protection-expenditure index, and the KOF globalization index. Three describe additional socio-political context: average years of schooling (interpolated between census years), migrant stock as a share of population (UN, interpolated), and the World Uncertainty Index. All controls are available at the country-year level and enter at the election year. Data definitions and sources are reported in Table A1.

2.4. Country-election panel construction

The country-election panel is observed at election years. For each country-election pair (c, e) I attach cultural-heterogeneity indices from the IVS wave whose fieldwork most recently preceded the election. Each country-year lacking a fresh IVS wave inherits the most recent previous wave's value, so the right-hand-side cultural heterogeneity is always based on survey data collected before the election outcome on the left-hand side. IVS waves that postdate the election are never merged forward.

I drop two classes of country-elections from the estimation sample. First, I exclude

[5] All three populism measures are vote-weighted. The moderate cross-measure correlations reflect differences in time coverage (PopuList begins in 1989), geographic scope (PopuList covers only Europe), and classification methodology (Random Forest over V-Party expert codes vs. harmonic mean of two V-Party items vs. expert-informed qualitative classification).

elections held before the transition to multi-party competition in the post-communist countries: elections in Albania, Bosnia and Herzegovina, Bulgaria, Croatia, the Czech Republic, Estonia, Hungary, Latvia, and Lithuania before 1991, and elections in Moldova, Montenegro, North Macedonia, Poland, Romania, Serbia, Slovakia, and Slovenia before 1990. This restriction is a sample- construction rule and is unrelated to the income-level scope condition reported in Appendix H. Second, I exclude the 2020–2022 pandemic-era elections for which economic and electoral dynamics were plausibly disrupted by COVID-19.[6] After these restrictions and listwise deletion of observations missing any control, the estimation sample contains 348 country-election observations in 60 democracies between 1970 and 2019. The sample spans 22 established democracies and 38 other democracies in Central and Eastern Europe, Latin America, Asia, and Africa. Table 1 reports means, standard deviations, and ranges for the key variables. The mean cultural fractionalization index is $CF = 0.541$ with a standard deviation of 0.037; the mean of cultural polarization at $\alpha = 0.5$ is $CP_{0.5} = 0.296$ with a standard deviation of 0.013. Ideational vote-weighted populism averages 3.54 (on the 0–10 CRR scale) with standard deviation 1.07; the PopuList overall populist vote share averages 10.3 percentage points on the subset of 30 European countries where PopuList coverage applies. The cross-correlation matrix (Table A3) shows that the unconditional correlations between CF or $CP_{0.5}$ and the three populism outcomes are close to zero (ρ between -0.15 and 0.09). The conditional relationship reported in Section 5 emerges only after absorbing country and year fixed effects, the lagged dependent variable, and macroeconomic controls—which is expected when the between-country component of heterogeneity is orthogonal to the between-country component of populism.

3. Cultural heterogeneity measurement

I measure cultural heterogeneity using the index family of Desmet et al. (2017) and Desmet et al. (2025b), computed over individual responses to items in the Integrated Values Survey. Two design choices distinguish my approach from theirs and underpin the empirical results in Section 5: first, I extend their measurement to a 118-country, 7-wave panel; second, I apply a content filter that restricts the underlying item set to value, normative, and moral items and excludes behavioral items, factual self-reports, country-specific regime evaluations, and demographic background. The content filter is the paper's central measurement contribution; Section 3.2 shows that it is empirically essential.

[6] This exclusion affects approximately 15 country-elections (e.g., Germany 2021, Italy 2022). Including pandemic-era elections does not alter the sign or significance of the main results but introduces noise from lockdown-specific volatility.

TABLE 1. Descriptive statistics on the Model 3 estimation sample

Variable	Mean	SD	Min	Max
Cultural-heterogeneity indices				
CF	0.541	0.037	0.442	0.663
CFD	0.311	0.020	0.235	0.364
$CP_{0.5}$	0.296	0.013	0.233	0.324
$CP_{1.0}$	0.182	0.007	0.142	0.197
$CPD_{0.5}$	0.179	0.017	0.129	0.216
$CPD_{1.0}$	0.115	0.013	0.082	0.145
Populism outcomes (vote-weighted)				
Ideational populism (CRR)	3.54	1.07	0.28	7.05
Rhetorical populism (CRR)	4.42	1.12	0.19	6.91
V-Dem populism	0.30	0.12	0.02	0.74
Controls				
Log GDP per capita	10.11	0.71	7.49	11.30
GDP growth	0.03	0.04	-0.09	0.15
Working-age share	66.47	3.01	54.41	78.53
Years of schooling	10.79	2.05	3.90	13.63
Unemployment	8.02	4.76	0.66	32.61
CPI inflation	3.98	8.15	-1.74	89.11
Labor regulation	6.09	1.46	2.79	9.37
Social protection	12.48	6.63	0.15	27.48
KOF globalization	74.95	10.24	41.46	91.00
Migrant share	8.92	7.94	0.06	45.39
Uncertainty	0.19	0.16	0.00	1.18
Observations		348		

Notes: Sample is the 348 country-election observations (60 democracies, 1970–2019) used to estimate Model 3 with the CRR ideational vote-weighted populism outcome. Cultural-heterogeneity indices on the curated 259-item set; populism scores in their native units (CRR 0–10; V-Dem 0–1); controls in natural units (log GDP per capita, percent, share, index value). Variable definitions and sources are in Appendix A.

3.1. Heterogeneity indices

For each country-wave cell and each retained item q with R_q response categories and observed response shares $\pi_{r(q)}$, I compute four heterogeneity indices.

Cultural fractionalization captures the probability that two random respondents in country c disagree on item q:

$$CF_q \;=\; 1 - \sum_{r=1}^{R_q} \pi_{r(q)}^2. \tag{1}$$

Cultural fractionalization with distances weights pairwise disagreement by the ordinal distance between response categories, normalized to the unit interval:

$$CFD_q \;=\; \sum_{r \neq s} \frac{|r-s|}{R_q - 1}\, \pi_{r(q)} \pi_{s(q)}. \tag{2}$$

On binary items CFD_q collapses to CF_q.

Cultural polarization, following Desmet et al. (2025b), captures the extent to which the response distribution concentrates into opposing camps:

$$CP_q(\alpha) \;=\; \sum_{r} \pi_{r(q)}^{1+\alpha} \big(1 - \pi_{r(q)}\big), \tag{3}$$

with the preferred sensitivity parameter $\alpha = 0.5$ and a robustness check at $\alpha = 1$. The distinction matters: in a country where 50 percent of respondents support immigration and 50 percent oppose it, CP is high because two camps of equal size confront each other; CF is moderate because only two groups exist. In a country where ten equally sized value clusters coexist, CF is high but CP is low because no single cleavage dominates. More generally, fractionalization captures the breadth of disagreement, polarization its depth.[7]

Cultural polarization with distances combines clustering with ordinal separation:

$$CPD_q(\alpha) \;=\; \sum_{r \neq s} \pi_{r(q)}^{1+\alpha}\, \pi_{s(q)}\, \frac{|r-s|}{R_q - 1}. \tag{4}$$

Each cell-level index is the simple average of the question-level values across all retained items that meet the 30-respondent-minimum coverage threshold in that cell. The main analysis uses CF and $CP_{0.5}$ as the primary indices; the distance-weighted variants CFD and CPD_α and the alternative sensitivity parameter $\alpha = 1$ are computed for completeness but produce uniformly insignificant coefficients in Model 3 across all populism

[7] When $\alpha = 0$, CP_q collapses to CF_q. As α increases, the index penalizes small groups more heavily, so that a 50–50 split dominates a 90–10 split. The choice of $\alpha = 0.5$ follows Desmet et al. (2025b) and balances sensitivity to bimodality against robustness to outlier categories.

outcomes and are reported only in the appendix cross-correlation matrix (Table A3).[8]

The CF and CP indices are computed item by item and averaged across items. They summarize the marginal dispersion of responses to each question rather than the joint structure of individuals' response vectors. An alternative endogenous-clustering family—Draca and Schwarz (2024) via latent ideological types and Desmet et al. (2025a) via hedonic-game partitions—first assigns individuals to latent values-based groups and then measures between-group polarization. That approach is difficult to embed in a cross-country panel: the identified groups capture different cleavages in different countries (a religious–secular axis in one place, an urban–rural axis in another) and can shift across waves within a country, so the same between-group polarization number does not carry a consistent meaning across observations. Item-level CF and CP apply the same formula to the same 259 items in every country-wave cell, which is what the panel design requires; the trade-off is that the indices do not pin down a group structure.

3.2. Item selection and the values-versus-behaviors distinction

Starting from the 838 IVS variables, I apply three sequential filters. *Filter A* (prefix and naming convention) retains the 632 substantive items in the WVS/EVS "A–I" code blocks (perceptions, environment, work, family, politics, religion, identity, security, science) and drops study metadata, socio-demographics, derived post-materialism indices, and country-specific variants. *Filter B* (cross-country coverage), following Desmet et al. (2017), retains items answered in at least 50 countries, where "answered" means at least 30 respondents in the country provided a valid response. This yields 395 items, the same item base used by DOOW (Desmet–Ortuño-Ortín–Wacziarg, hereafter DOOW). *Filter C* is new and is the paper's main measurement contribution.

I drop four classes of items from the 395-item DOOW baseline. First, I drop *behavioral* items that record participation events, membership status, attendance frequency, media-consumption habits, or political-action history (100 items in total: petition signing, demonstration attendance, party membership, religious-service attendance, daily newspaper reading, voluntary work, and so on). Second, I drop *factual self-reports* on personal health, material hardship, crime victimization, and job and financial satisfaction (14 items). Third, I drop *country-specific regime evaluations*—satisfaction with the way democracy is developing, ratings of the political system, confidence in parliament, the government, and political parties (14 items)—which are mechanically correlated with populist outcomes because populism rises in response to perceived governance failure. Fourth, I drop *demographic and background* items that Filter A overlooks, including im-

[8] The CFD and CPD indices weight pairwise disagreement by ordinal distance between response categories. On binary items—which constitute roughly half the curated 259-item set—CFD collapses to CF and CPD to CP, reducing the independent variation. CP at $\alpha = 1$ penalizes small groups too heavily for this application, yielding wide standard errors relative to $\alpha = 0.5$.

migrant status of the respondent and parents (7 items). The retained 259-item set comprises moral and religious justifiables, family and gender norms, work ethic, trust and tolerance, Schwartz value scales, nationalism, immigration policy, civil-liberty positions, and ideology self-placement; Appendix B lists twenty representative items—ten retained and ten dropped—with their exact IVS codes, question wording, and Filter C reason codes. Confidence items in non-political institutions (churches, the press, labor unions, universities, the military, the police, the European Union) are retained because they track ideological values rather than evaluations of the current incumbent regime.[9]

The Filter C content choice is not a matter of taste. Estimating Model 3 on the raw 395-item DOOW baseline yields a coefficient on $CP_{0.5} \times$ ideational populism of 1.76 (SE 2.89, $p = 0.54$); estimating it on the curated 259-item set yields 8.64 (SE 3.58, $p = 0.02$). To isolate which exclusion category drives this shift, I construct five additional item sets and run the same regression on each (Table 2). Removing *only* the 14 regime-evaluative items, or *only* the 5-item political-apparatus subset of them (confidence in parliament, government, political parties, courts, and civil service), or *only* the 7-item subset of demographics that Filter A does not already drop, leaves the coefficient virtually unchanged from the raw baseline (1.79, 1.78, 1.81 respectively). Removing *only* the 100 behavioral items and keeping evaluatives, demographics, and factuals yields 9.52 ($p = 0.02$)—essentially identical to the full curated set. Adding the 14 evaluatives back into the curated 259-item set produces 8.88 ($p = 0.02$). Behavioral items alone account for the entire null-to-significant transition. The values-versus-behaviors distinction at the item-selection stage is therefore empirically decisive; the regime-performance endogeneity concern that independently motivated dropping the 14 regime-evaluation items turns out to be empirically minor, but the exclusion is retained on conceptual grounds because regime-evaluation items are mechanically downstream of populist governance outcomes. Distance-weighted counterparts of these indices (CFD and CPD_α) show a qualitatively identical curation pattern; full results appear in Appendix D.

The full audit of include/drop decisions for all 632 candidate items, together with reason codes, is available in the replication archive as `question_manifest.csv`.[10]

3.3. Is Filter C a specification search?

The Filter C coefficient is eight times the raw baseline, so a natural concern is whether the taxonomy was chosen because it delivers significance. I address this in two ways: a permutation test against the raw 395-item baseline, and a cross-check of the classification

[9] For example, confidence in the press correlates with media-skepticism attitudes, not with satisfaction with the current government. Adding these 14 evaluative items back into the curated set produces $\hat{\beta} = 8.88$ ($p = 0.02$), virtually identical to the 259-item baseline of 8.64.

[10] Reason codes include: behavioral (100 items dropped), regime-evaluative (14), demographic (7), factual self-reports (14), and retained (259). Each decision is traceable to a named IVS variable code.

against an independent coder.

Permutation test. If the value–behavior distinction is real, dropping the actual 100 behavioral items should produce a coefficient well outside the distribution generated by dropping any 100 items at random. I draw 500 random 100-item subsets from the 395-item pool without replacement, recompute $CP_{0.5}$ as the mean over the 295 retained items per country–wave, and re-estimate Model 3 for each draw. The observed Variant E estimate is $\hat{\beta} = 9.52$. Across the 500 random drops the coefficient has mean 1.86, standard deviation 0.56, and range $[0.56, 3.91]$—not a single random 100-item drop produces a coefficient as large as the observed value. The one-sided permutation p-value is therefore below $1/501 \approx 0.002$. Across the full 3×2 matrix of populism measures and heterogeneity indices, 3,000 random regressions yield zero draws that reach conventional significance. The pattern is specific to the value–behavior cut; random curation never finds it. Figure 1 plots the null distribution and the observed estimate for the primary specification.

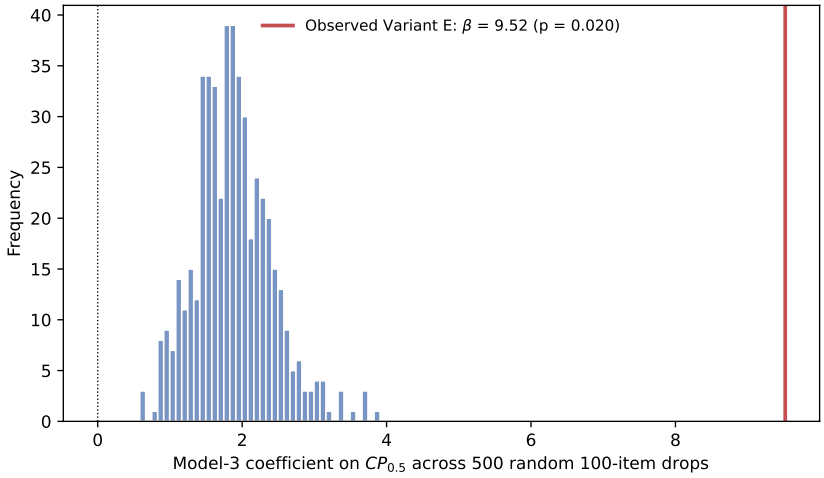

FIGURE 1. Permutation test for Filter C. Histogram of Model 3 coefficients on $CP_{0.5}$ across 500 random 100-item drops from the 395-item pool; vertical line marks the observed Variant E estimate ($\hat{\beta} = 9.52$, $p = 0.02$). Outcome is CRR ideational populism vote share. Dependent variable, controls, and specification are identical across draws; only the retained item set varies.

Independent classification. To test whether the value–behavior partition depends on the specific regexes in `question_manifest.csv`, I apply a second keyword-based classifier grounded in Inglehart–Welzel's distinction between attitudinal items ("agree," "justified," "important," "confidence in") and reports of action or self-state ("how often," "ever," "attend," "member," "born in"). The two classifications agree on 357 of 395 items (90.4%), Cohen's $\kappa = 0.77$.[11] All 38 disagreements lie in the direction "paper drops,

[11] Landis and Koch (1977) class this as "substantial" agreement.

independent keeps"—zero items that the paper retains are flagged as non-values by the independent coder. The 259-item curated set is therefore a strict subset of any reasonable values-only specification; the Filter C trim is more conservative than an independent coder's, not less.

TABLE 2. Curation-sensitivity battery: Model 3 coefficient on cultural heterogeneity across seven item-set variants

		CF			$CP_{0.5}$		
Variant	Items	Ide	Rhet	V-Dem	Ide	Rhet	V-Dem
A. Raw DOOW 395	395	0.96	0.85	-0.08	1.76	2.37	-0.22
B. Curated 259 (preferred)	259	2.93*	1.77	0.12	8.64**	7.36**	0.53
C. 395 minus evaluatives	381	0.96	0.78	-0.07	1.79	2.39	-0.20
D. 395 minus apparatus	390	0.95	0.83	-0.07	1.78	2.39	-0.21
E. 395 minus behaviorals	295	3.43*	2.21	0.13	9.52**	8.10**	0.53
F. 395 minus demographics	388	0.95	0.80	-0.07	1.81	2.27	-0.21
G. Curated + evaluatives	273	3.01*	1.96	0.11	8.88**	7.47**	0.50

Notes: Each cell reports the Model 3 coefficient on the indicated heterogeneity index (CF or $CP_{0.5}$) when the populism outcome is the column-indicated measure (Ideational, Rhetorical, V-Dem). Variant A is the raw 395-item DOOW baseline. Variants C–F drop one content category at a time. Variant B is the curated 259-item set used throughout the main text. Variant G adds the 14 evaluative items back into the curated set. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4. Empirical strategy

The estimation strategy has two goals: characterize the conditional relationship between cultural heterogeneity and vote-weighted populism, and isolate the variation in that relationship that is plausibly not driven by reverse causality from electoral outcomes to survey responses. I estimate a hierarchy of four specifications on the country-election panel of Section 2, with Model 3 as the preferred dynamic two-way fixed-effects estimator and Model 4 as the system-GMM robustness check. The pre-designated primary specification throughout the paper is Model 3 with cultural polarization $CP(\alpha = 0.5)$ as the heterogeneity measure and the CRR ideational populism score as the outcome; all other measure$\times$outcome cells are treated as robustness.

4.1. Specification hierarchy

The four specifications nest a sequence of conditioning assumptions, under which the coefficient on cultural heterogeneity admits a progressively more plausible causal reading. Let i index countries, t index election-years, Pop_{it} denote vote-weighted populism (CRR ideational, CRR rhetorical, or V-Dem V-Party populism), and Het_{it} denote a cultural-heterogeneity index (CF, CFD, CP_{α}, or CPD_{α}).

Model 1 includes country fixed effects and a lagged dependent variable:

$$\text{Pop}_{it} = \mu_i + \rho\,\text{Pop}_{i,t-1} + \beta\,\text{Het}_{it} + \varepsilon_{it}. \tag{5}$$

Country fixed effects μ_i absorb time-invariant national traits. The lagged dependent variable $\text{Pop}_{i,t-1}$ is vote-weighted populism in country i's previous election, identifying the dynamic component of populist persistence.[12] The cultural-heterogeneity coefficient β is identified from within-country, between-election variation beyond what the LDV predicts.

Model 2 adds year fixed effects:

$$\text{Pop}_{it} = \mu_i + \delta_t + \rho\,\text{Pop}_{i,t-1} + \beta\,\text{Het}_{it} + \varepsilon_{it}. \tag{6}$$

The year effects δ_t absorb common global shocks: the 2008 financial crisis, the post-2015 migration crisis, the rise in populist-friendly social-media penetration, and any other contemporaneous factors that move populism similarly across countries.

Model 3, the preferred specification, augments the two-way fixed-effects equation with a vector of controls:

$$\text{Pop}_{it} = \mu_i + \delta_t + \rho\,\text{Pop}_{i,t-1} + \beta\,\text{Het}_{it} + \mathbf{X}'_{it}\boldsymbol{\gamma} + \varepsilon_{it}. \tag{7}$$

The control vector $\mathbf{X}_{it}$ includes the eleven macroeconomic and institutional variables described in Section 2: log GDP per capita, GDP growth, the working-age population share, average years of schooling, unemployment, CPI inflation, the labor-market regulation index, the social-protection-expenditure index, the KOF globalization index, the migrant stock share, and the World Uncertainty Index. These controls capture the principal economic channels emphasized in Algan et al. (2017), Guiso et al. (2019), and Dal Bó et al. (2023); the cultural-heterogeneity coefficient β in Model 3 is therefore identified from variation orthogonal to the standard economic determinants of populism.

Model 4 estimates equation (7) using the Blundell–Bond system generalized-method-of-moments estimator (Arellano and Bover 1995; Blundell and Bond 1998), implemented via Kripfganz (2019). To avoid instrument proliferation, I use a parsimonious instrument set: only the lagged dependent variable and the heterogeneity measure are GMM-instrumented (collapsed lags 2–3 in the differenced equation, lag 1 in the level equation); all remaining controls enter as standard exogenous instruments; and period dummies

[12] The time index t refers to election sequence number within a country, not calendar year. The spacing between elections varies from 2 to 7 years depending on the country's electoral calendar. Treating the LDV at the election-sequence frequency is the standard applied shortcut in the comparative political-economy literature on country-election panels, and is consistent with the identification and estimation approach for dynamic panels with unequal spacing developed by Sasaki and Xin (2017); year fixed effects absorb calendar-time shocks separately in Model 2 and beyond.

are replaced by a linear trend and a post-2008 break, reflecting the structural shift in populist voting that Algan et al. (2017) document around the 2008 financial crisis.[13] The estimator addresses Nickell bias in the dynamic panel. Nickell (1981) shows that the within-group estimator is biased of order $O(1/T)$; with 5–8 elections per country, this bias is potentially material. The LDV coefficient rises from $\hat{\rho} = 0.23$ under Model 3 to $\hat{\rho} = 0.37$ under Model 4, consistent with the expected downward bias of the within-group estimator on the autoregressive parameter. The direction of Nickell bias on other regressors is theoretically ambiguous (Nickell 1981). I therefore do not interpret the GMM–FE gap on the $CP_{0.5}$ coefficient as evidence on bias direction; I read the Model 4 results (Section 5.3) as corroborating Model 3 in sign and statistical significance.

All standard errors are clustered at the country level, appropriate given 60 clusters, a small within-cluster dimension (5–8 elections per country), and likely serial correlation in vote-weighted populism.[14]

The estimation sample contains 348 country-election observations in 60 democracies between 1970 and 2019. A country enters the sample when it holds free multi-party elections, cultural-heterogeneity indices are computable from at least one prior IVS wave, and the eleven controls and the populism outcome are non-missing. The sample spans 22 established democracies and 38 other democracies in Central and Eastern Europe, Latin America, Asia, and Africa (Table 1).

4.2. Addressing endogeneity concerns

I do not claim point identification of a causal effect. The coefficient on $CP_{0.5}$ admits a causal interpretation under the assumption that time-varying unobserved country-level confounders are orthogonal to within-country changes in cultural heterogeneity after conditioning on the lagged dependent variable, country fixed effects, year fixed effects, and the eleven macroeconomic and institutional controls. This is a strong assumption that I cannot validate directly. The checks in this subsection probe three specific threats to it — reverse causality, slow-moving omitted confounders, and Nickell bias — and find evidence consistent with the causal reading, but stop short of ruling out every plausible alternative.

The most prominent endogeneity concern is reverse causality: populist electoral success might reshape the cultural responses captured by IVS survey items — what voters say about values, family, morality, or institutions could shift after a successful populist campaign rather than before. I address this and related concerns through several indirect checks.

[13] Implemented via the `xtdpdgmm` Stata package of Kripfganz (2019). This yields 29 instruments for 60 groups ($j/N = 0.48$), comfortably below the ceiling recommended by Roodman (2009).

[14] In Model 4 I apply the Windmeijer (2005) finite-sample correction. Significance is reported at the 10%, 5%, and 1% levels.

Cultural heterogeneity enters each country-election observation from the most recent IVS wave whose median fieldwork year precedes or coincides with the election year; values are carried forward within each country until a newer wave becomes available. In the Model 3 estimation sample, the median gap between the IVS fieldwork year and the election year is 3.5 years (interquartile range 1–6; maximum 20).[15] For the Austrian 2019 parliamentary election, for example, CF is computed from the 2017–2022 IVS wave, whose Austrian fieldwork was centered in 2018. IVS waves whose median fieldwork year postdates the election year are never merged forward.

Replacing contemporaneous heterogeneity with its one-election lag yields coefficients equal to or larger than the baseline (Table 5, row R2). On $CP_{0.5}$, the coefficient on CRR rhetorical populism rises from 7.37** contemporaneous to 10.86** lagged; V-Dem populism becomes significantly predicted only with the lag. Under a reverse-causality story in which populist campaigns reshape IVS responses, the contemporaneous coefficient should dominate the lagged one. The opposite ordering is evidence against that channel.

I estimate the Blundell–Bond system-GMM estimator (Arellano and Bover 1995; Blundell and Bond 1998) with a parsimonious instrument set (29 instruments for 60 groups, $j/N = 0.48$), following the Roodman (2009) guideline that the instrument count should be well below the number of clusters.[16] The two-step system-GMM coefficient on $CP_{0.5}$ is significant for all three populism outcomes: 19.0 ($p = 0.04$) on ideational, 19.9 ($p = 0.01$) on rhetorical, and 3.6 ($p = 0.02$) on V-Dem (Appendix E). The CF coefficients are significant for rhetorical and V-Dem populism and marginally insignificant for ideational ($p = 0.11$). Two-step and iterated variants converge closely for the rhetorical and V-Dem outcomes; the gap is wider for ideational populism (19.0 vs. 15.7). The paper reports the two-step estimates as primary because they are efficient under correct moment specifications and are the convention in the Kripfganz (2019) implementation; the iterated estimates, with typically better finite-sample properties, are reported alongside in Appendix E and do not alter the sign or significance pattern. The Arellano–Bond test rejects first-order serial correlation in the differenced residuals ($z = -2.64$, $p = 0.008$) but does not reject second-order ($z = -1.14$, $p = 0.26$), consistent with the GMM moment conditions;[17] the Hansen J overidentification test returns $p = 0.56$ (full diagnostics in Appendix E). The LDV coefficient rises from $\hat{\rho} = 0.23$ under Model 3 to $\hat{\rho} = 0.37$ under the system-GMM specification, consistent with Nickell (1981) downward bias on the

[15] A gap of 0 means the IVS wave and the election fall in the same calendar year. A gap of 3 means the election occurred three years after the median fieldwork date of the most recent available IVS wave.

[16] Estimated with the `xtdpdgmm` package of Kripfganz (2019). Only the lagged dependent variable and heterogeneity are GMM-instrumented; all other controls enter as exogenous instruments. Period dummies are replaced by a linear trend and a post-2008 break to reduce the instrument count.

[17] Serial correlation is tested in the election-sequence time index on which the GMM moment conditions are defined, not in calendar time.

autoregressive parameter; the estimated $\hat{\rho}$ remains well below one, ruling out quasi-unit-root concerns on populism. The system-GMM evidence corroborates the Model 3 finding across populism measures. Appendix J reports Oster (2019) bounds on the preferred specification: at the conventional $R^2_{\max} = 1.3 \times R^2_{\text{long}}$, $|\delta| = 1.63$ and the identified set at $\delta = 1$ is $[8.64, 13.93]$, excluding zero.

Country and year fixed effects together with a lagged dependent variable absorb time-invariant national traits, common global shocks, and populism autocorrelation; the cultural-heterogeneity coefficient captures within-country variation beyond what country-specific and global trends predict. Within-country variation in cultural heterogeneity is also small relative to within-country variation in vote-weighted populism. On the M3 sample, the within-country standard deviation of CF is 0.027 (about 5 percent of its mean of 0.541) and the within-country standard deviation of $CP_{0.5}$ is 0.008 (about 3 percent of its mean of 0.296),[18] while the within-country standard deviation of CRR ideational populism across elections is 0.508 (about 14 percent of its mean of 3.54). Cultural heterogeneity is also measured at the five-to-nine-year IVS-wave frequency, while vote-weighted populism is observed at the much shorter election frequency. Even if populism could in principle reshape value-survey responses, that mechanism would have to operate on a multi-wave timescale to register in the CF time series, which leaves little room for short-run election-to-CF feedback.

In sum, these checks bound the most obvious endogeneity channels but do not deliver a point-identified causal effect. I interpret the coefficient as a conditional relationship that survives the usual dynamic-panel robustness checks and that holds under alternative populism classifications, subsample restrictions, and reasonable variations on the baseline specification.

5. Results

I report the baseline panel estimates first, then the GAL–TAN ideological decomposition on the full 60-country sample, and a seven-family robustness battery. The income-level decomposition is moved to Appendix H because its Model 3 pattern does not survive the paper's preferred GMM specification.

5.1. Pooled baseline

Table 3 reports the Model 3 estimates on the pooled 60-country sample. Three populism outcomes appear across the columns: the CRR ideational populism score, the CRR rhetorical populism score, and the V-Dem V-Party populism indicator. Both cultural-heterogeneity indices (CF and $CP_{0.5}$) appear in the rows. Income-level heterogeneity

[18] Computed on the 348 country-election observations in the estimation sample.

on this same baseline is examined in Appendix H.

The cultural-heterogeneity coefficient is positive and statistically significant for the two CRR measures and positive but imprecise for V-Dem.[19] CRR and V-Dem V-Party draw on overlapping inputs—CRR aggregates ten V-Party features via a Random Forest calibrated to external expert benchmarks (POPPA, GPS), while V-Dem V-Party aggregates two of those features via a harmonic mean—so the three coefficients probe sensitivity to aggregation choice rather than independent triangulation. The point estimates are consistent in sign; the V-Dem indicator is imprecise in the pooled sample and I do not read more into it than that. Magnitudes are substantively meaningful. A one-within-country-standard-deviation increase in $CP_{0.5}$ (within-SD $= 0.008$) is associated with a 0.07-unit shift in CRR ideational populism on its 0–10 scale, equivalent to roughly 14 percent of the within-country standard deviation of CRR ideational populism in the estimation sample (0.508). Over the full empirical range of $CP_{0.5}$ (0.23 to 0.32), the implied shift in ideational populism is about 0.77 units on the 0–10 scale, or roughly 72 percent of the full-sample standard deviation of populism (1.07). The per-unit regression coefficients—over the same empirical range—are 8.6 for CRR ideational ($p = 0.02$), 7.4 for CRR rhetorical ($p = 0.03$), and 2.9 for CF on ideational populism ($p = 0.06$); the rhetorical and V-Dem CF coefficients are positive but not statistically significant at conventional levels. Within the same Model 3 specification, the standardized cultural-polarization effect—0.14 in within-country-SD units—is comparable in magnitude to the standardized effect of labor-market regulation (0.31, the largest single control), and exceeds the standardized effects of log GDP per capita (-0.10), the World Uncertainty Index (-0.10), the working-age population share (-0.09), CPI inflation (-0.07), GDP growth (0.06), the KOF globalization index (-0.05), the unemployment rate (0.02), and the migrant-stock share (-0.01). The within-country cultural-polarization signal is therefore one of the strongest predictors of populist voting in the panel, larger in standardized magnitude than most of the conventional macroeconomic drivers emphasized in the literature (Algan et al. 2017; Guiso et al. 2019; Dal Bó et al. 2023). In relative terms, a full-range shift in $CP_{0.5}$ implies about a 22 percent rise in mean CRR ideational populism, roughly 70 percent of the 30 percent increase in far-right vote share that Funke et al. (2016) document in the five years following systemic financial crises across 20 advanced economies; the cultural-polarization channel thus operates at a magnitude comparable to, though smaller than, a canonical financial-crisis shock.

5.2. Ideological decomposition: GAL–TAN on the global sample

The pooled effect documented in Section 5.1 aggregates populist parties across the ideological spectrum. Whether cultural polarization mobilizes populist vote share symmetri-

[19] The V-Dem $CP_{0.5}$ coefficient is 0.52 (SE = 0.44, $p = 0.25$).

cally or skews it toward one end of the ideological axis is a separate empirical question. To answer it on the full 60-country sample, I construct a party-level ideological composite from four V-Party (V-Dem) items that individually measure cultural position: immigration stance, LGBT social equality, minority rights, and women's labor-force position. Each item is sign-aligned so that higher values indicate the traditional–authoritarian–nationalist (TAN) end of the GAL–TAN dimension (Hooghe et al. 2002; Jolly et al. 2022); the composite is the equal-weighted mean of the four items after standardizing. Appendix F cross-validates this composite against CHES 2019 GALTAN scores at the level of iconic European parties (Pearson $\rho \approx 0.98$; perfect TAN–GAL agreement). Parties with composite scores at or above the sample median are classified TAN; those below, GAL. A continuous specification uses the standardized composite directly as a weight.

The 4-item composite captures the cultural-right dimension distinct from economic left–right or democratic-norms indicators. It excludes V-Party items on political pluralism and opposition respect, which measure commitment to democratic norms independently of cultural position and would pull democratically-committed mainstream right parties toward the TAN camp. The classification matches conventional ideological coding: Poland's Law and Justice (PiS) and Hungary's Fidesz score in the upper quartile of the composite (composite scores of $+1.16$ and $+1.59$, about 1.8 and 2.3 standard deviations above the median); Germany's AfD, Italy's Brothers of Italy, and Austria's FPÖ land firmly in the TAN camp; Die Linke, Podemos, SYRIZA, La France Insoumise, and Green parties score in the lower quartile.[20]

Country-year populist vote share is then decomposed into a TAN-camp aggregate $m_{\text{pop}}^{\text{TAN}}$ and a GAL-camp aggregate $m_{\text{pop}}^{\text{GAL}}$, each summing the populism index weighted by vote share within the relevant camp. The continuous alternative $m_{\text{pop}}^{\text{cont}}$ weights each party's populism contribution by its standardized GAL–TAN score, recovering the ideological moment of the populist vote distribution.

Table 4 reports Model 3 and parsimonious system GMM estimates for both CRR ideational and CRR rhetorical populism. On ideational populism under Model 3, the TAN coefficient is positive and significant at 5 percent ($\hat{\beta}_{\text{TAN}} = 11.97^{**}$, $p = 0.017$), while the GAL coefficient is statistically indistinguishable from zero ($\hat{\beta}_{\text{GAL}} = -3.20$, $p = 0.49$). The rhetorical specification shows the same asymmetry more sharply ($\hat{\beta}_{\text{TAN}} = 16.52^{**}$, $p = 0.017$; $\hat{\beta}_{\text{GAL}} = -9.50^{*}$, $p = 0.10$). The continuous specification is positive and significant on both outcomes ($\hat{\beta}_{\text{cont}} = 15.39^{*}$ on ideational, 18.48^{**} on rhetorical), confirming the ideological skew without committing to a binary threshold. The decomposition

[20] The binary left–right indicator in Celico et al. (2024), which some country-year populism panels adopt, codes PiS as left from 2011 onward on account of its economic redistributionism. Under the GAL–TAN dimension this coding is inverted, and the party's ideological position tracks its cultural conservatism rather than its fiscal policy. The economic classification nonetheless yields qualitatively similar signs on the pooled vote-share decomposition, though with a less clean TAN–GAL asymmetry (see the Model 3 row labeled R5 in Table 5).

satisfies the accounting identity $\hat{\beta}_{\text{TAN}} + \hat{\beta}_{\text{GAL}} \approx \hat{\beta}_{\text{pooled}}$ up to clustering noise, which is reassuring — neither camp's coefficient is being inflated by a classification that double-counts mass.

Under parsimonious system GMM the TAN coefficient remains positive and of similar magnitude ($\hat{\beta}_{\text{TAN}} \approx 21$ on both populism families) but the standard error rises by a factor of roughly two to three, yielding p-values of 0.116 and 0.157. I estimated nine alternative GMM instrument specifications — tighter diff lags, diff-only GMM, exogenous $CP_{0.5}$, Anderson–Hsiao single-lag, iterated GMM, collapsed-only exogenous instruments, and wider lag windows — and none tightens inference on the decomposed outcomes. The pattern is consistent with lower within-country variance in the camp-specific aggregates relative to the pooled total: GMM's identifying power scales with within-country variation, so the same instruments that cleanly identify the pooled coefficient become weak on either camp's aggregate. I therefore report the GAL–TAN decomposition as significant under fixed-effects inference and directionally confirmed under GMM, with the pooled coefficient remaining the primary quantitative anchor of the paper.

A PopuList-based right-populism pattern on the European subsample is reported in Appendix G as a separate robustness check. On Model 1 (country fixed effects plus lagged dependent variable only), the PopuList far-right coefficient on $CP_{0.5}$ is $+88.3$ ($p = 0.04$) on the 30-country European sample, but this coefficient falls within one standard error of zero once the eleven-control Model 3 specification is applied (Table A6). The PopuList pattern is consistent with cultural polarization driving Western-European right-populist vote shares during the 2010s, but the effect does not separately identify a cultural-heterogeneity mechanism once time trends, globalization exposure, and development level are held fixed.

5.3. Robustness battery

Table 5 summarizes seven robustness checks on the preferred Ideational $\times$ $CP_{0.5}$ specification, plus a system-GMM specification with a parsimonious instrument set (29 instruments, $j/N = 0.48$). The two-step system-GMM coefficient on the primary specification is $\hat{\beta} = 19.0$ ($p = 0.04$; Hansen J $p = 0.56$); extending the parsimonious GMM to all three populism outcomes yields significant coefficients in five of six cells (Appendix E), corroborating the Model 3 results across populism measures. Two checks strengthen the baseline: R2 (replacing contemporaneous heterogeneity with its one-election lag, $\hat{\beta} = 10.2$, $p = 0.03$) and R4 (restricting to the 25 low- and middle-income democracies, $\hat{\beta} = 49.9$, $p = 0.003$; see Appendix H for leave-one-out checks and the GMM non-robustness caveat). A separate robustness on an independent outcome margin is R7 (PopuList far-right vote share on the European sample estimated under Model 1, $\hat{\beta} = 88.3$, $p = 0.04$): the outcome, specification, and sample differ from the baseline,

so the magnitude is not directly comparable, but the sign and significance corroborate the main pattern on a different populist-vote margin. The two checks that weaken the baseline are R1 (dropping the lagged dependent variable, coefficient halves and standard errors widen) and R6 (replacing vote-weighted populism with seat-weighted populism, coefficient becomes a small negative number that is statistically indistinguishable from zero). The remaining two checks (R3 first differences, R5 right-populism decomposition on the full pooled sample) are inconclusive at conventional significance: first-differencing eliminates the slow-moving between-wave variation that drives the $CP_{0.5}$ coefficient — efficiency loss that is also informative, since the same persistence that attenuates the first-differenced estimate means the fixed-effects coefficient is identified from slow-moving within-country trends that are harder to separate from omitted trending confounders. The full 60-country sample lacks the PopuList classification required for a clean right–left split, so parties in R5 must be split by V-Dem's economic left–right dimension, which is a coarser proxy than PopuList's ideological classification used in R7.

The two attenuating checks both have natural mechanical interpretations. The no-LDV specification omits the autoregressive term that absorbs serial correlation in populism and leaves the cultural-heterogeneity coefficient to soak up some of the persistence the LDV would otherwise capture; including an LDV in a fixed-effects panel with short T in turn introduces the bias identified by Nickell (1981), which is precisely the motivation for the system-GMM check reported in Section 4.2. The seat-share check confirms that the cultural-heterogeneity signal operates on *voter mobilization*, with the translation from votes to seats mediated by electoral institutions (thresholds, district magnitude, list versus plurality rules) that operate at a different level of analysis. The paper's empirical claim is therefore about the demand-side effect of cultural heterogeneity on populist voting, not about populist parties' translation into legislative power; the null on seat-weighted populism is consistent with this scope. Neither check undermines the substantive interpretation of the main findings; both are reported transparently to support the cross-specification stability claim.

6. Discussion

The findings of Section 5 point to three substantive scope conditions.

The Filter C sensitivity battery shows that 100 behavioral items (turnout, party membership, religious-service attendance, voluntary work, demonstration participation, daily news consumption) dilute the cultural-heterogeneity signal to statistical insignificance—the raw-baseline coefficient is 1.76, compared with 9.52 once behaviors are removed. The distinction is not an arbitrary measurement choice. Behavioral items conflate attitudes with the institutional opportunity structure: a country with high

TABLE 3. Cultural heterogeneity and populist voting: pooled estimates across three populism measures

	Ideational (CRR)	Rhetoric (CRR)	V-Party (V-Dem)
CF	2.91*	1.77	0.12
	(1.53)	(1.42)	(0.17)
$CP_{0.5}$	8.64**	7.37**	0.52
	(3.58)	(3.20)	(0.44)
Observations	348	348	348
Countries	60	60	60

Notes: Model 3 specification: two-way (country and year) fixed effects with lagged dependent variable and 11 controls (log GDP per capita, GDP growth, share of population aged 15–64, years of schooling, unemployment, CPI inflation, labor-market regulation, social-protection expenditure, KOF globalization, migrant stock, the World Uncertainty Index). Standard errors clustered at country level in parentheses. Pooled sample: 60 democracies, 1970–2019. The three outcomes share V-Party expert-coded party characteristics as inputs, aggregated differently: CRR via Random Forest trained on POPPA (ideational) or GPS (rhetorical); V-Dem V-Party via a harmonic mean of anti-elitism and people-centrism. PopuList (Rooduijn et al. 2024) is not included here because its coverage is restricted to 30 European countries (versus 60 in the main sample) and its binary aggregation produces a different outcome type (thresholded vote share rather than vote-weighted intensity). Income-level heterogeneity for this baseline is reported in Appendix H; PopuList is used in Appendix G and in the R7 robustness row of Table 5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 4. Ideological decomposition of the cultural-polarization effect: V-Party GAL–TAN classification

	(1) Pooled	(2) GAL	(3) TAN	(4) Continuous
Panel A: CRR ideational populism				
Model 3 ($CP_{0.5}$)	8.640**	-3.203	11.973**	15.392*
	(3.579)	(4.562)	(4.872)	(8.672)
Model 4 ($CP_{0.5}$)	17.574**	6.292	21.500	10.547
	(8.042)	(20.173)	(13.679)	(24.463)
Panel B: CRR rhetorical populism				
Model 3 ($CP_{0.5}$)	7.364**	-9.499*	16.523**	18.481**
	(3.228)	(5.677)	(6.750)	(8.514)
Model 4 ($CP_{0.5}$)	16.613*	25.251	21.331	5.184
	(9.629)	(23.866)	(15.065)	(19.760)
Model 3: observations	348	348	348	348
Model 3: countries	60	60	60	60
Model 4: observations	325	325	325	325
Model 4: countries	58	58	58	58
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Lagged dependent variable	Yes	Yes	Yes	Yes
Full 11 controls	Yes	Yes	Yes	Yes

Notes: Model 3: two-way (country and year) fixed effects with lagged dependent variable and 11 controls; standard errors clustered at country level in parentheses. Model 4: parsimonious system GMM (Kripfganz 2019) with the lagged dependent variable and $CP_{0.5}$ instrumented via collapsed GMM-style lags (2–3 in the differenced equation; lag 1 in the level equation); other controls enter as standard exogenous instruments. Columns: (1) pooled CRR populism vote share; (2) GAL aggregate $\sum_i \text{pop}_i \cdot v_i \cdot \mathbf{1}\{\text{gal\_tan}_i < \text{median}\}$; (3) TAN aggregate $\sum_i \text{pop}_i \cdot v_i \cdot \mathbf{1}\{\text{gal\_tan}_i \geq \text{median}\}$; (4) continuous $\sum_i \text{pop}_i \cdot v_i \cdot z_i$ where z_i is the standardized 4-item V-Party GAL–TAN composite (immigration, LGBT, minorities, women-labor; sign-aligned toward TAN). The accounting identity $\hat{\beta}^{\text{TAN}} + \hat{\beta}^{\text{GAL}} \approx \hat{\beta}^{\text{pooled}}$ holds up to clustering noise. Decomposed-outcome GMM standard errors are larger than the pooled GMM standard error because the camp-specific aggregates inherit lower within-country variance than the pooled outcome; the paper's ideological-decomposition claim therefore rests on the Model 3 inference, with GMM reported for directional confirmation. Model 4 diagnostics across the eight cells (all with $N = 325$, 58 clusters, 29 instruments, $j/N = 0.089$): Hansen overidentification p-values are [0.57, 0.53, 0.40, 0.07] on Panel A (columns 1–4) and [0.51, 0.11, 0.34, 0.12] on Panel B, with seven of eight cells exceeding the 0.10 threshold; Arellano–Bond AR(2) p-values are [0.60, 0.81, 0.59, 0.67] on Panel A and [0.18, 0.67, 0.57, 0.50] on Panel B, all consistent with the absence of second-order serial correlation in the differenced residuals in election-sequence time. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

TABLE 5. Robustness battery: preferred Ideational $\times CP_{0.5}$ specification

Specification	$\hat{\beta}_{CP_{0.5}}$	SE	p	N	Verdict
Baseline (M3)	8.64**	(3.58)	0.016	348	Reference
M4: System GMM (iterated)	23.46*	(13.16)	0.075	348	Sign preserved
R1: No LDV	5.21	(4.53)	0.250	391	Weakened
R2: Lagged heterogeneity	10.20**	(4.61)	0.027	348	Strengthened
R3: First differences	3.32	(4.58)	0.469	322	Null (FD inefficient)
R4: Low/middle-income subsample	49.86***	(14.86)	0.003	87	Strengthened
R5: Right populism	1.65	(4.60)	0.719	348	Sign preserved
R6: Seat share (CP)	-1.87	(5.61)	0.739	371	Null (institutions)
R7: PopuList far-right (M1)	88.3**	(42.0)	0.035	232	Sign preserved

Notes: Each row reports the coefficient on $CP_{0.5}$ from a single regression of CRR ideational vote-weighted populism. Model 4 is the parsimonious system-GMM specification (two-step Blundell–Bond, 29 instruments, $j/N = 0.48$) of Kripfganz (2019); Hansen J $p = 0.56$. R1–R3, R5, and R6 use the pooled 60-country sample; R4 restricts to the 25 low- and middle-income democracies (2020 World Bank classification); R7 uses the European PopuList-coverage sample under Model 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

turnout has both engaged citizens and effective electoral infrastructure, so behavioral dispersion mixes value variation with infrastructural variation. Restricting the item set to moral, normative, and attitudinal beliefs isolates the "culture" that scholars in the Inglehart and Schwartz traditions invoke—durable beliefs about how the world should be ordered. Researchers applying Desmet et al. (2017) or Desmet et al. (2025b) to political outcomes should distinguish value items from behavioral items at the item-selection stage.

The seat-share results are uniformly null. Cultural heterogeneity shifts *voter* behavior toward populist parties; whether those votes translate into legislative power depends on electoral institutions (thresholds, district magnitude, list versus plurality rules) that are conceptually downstream of voter mobilization. The distinction matters for policy interpretation: a society with rising cultural heterogeneity will see rising populist voting, but the consequences for governance depend on whether the constitutional architecture amplifies or dampens that mobilization.

The V-Party GAL–TAN decomposition shows that the populist response to cultural polarization skews ideologically toward the authoritarian-nationalist end of the cultural axis. Under Model 3 the cultural-polarization coefficient on vote-weighted CRR ideational populism is 12.0 ($p = 0.02$) in the TAN camp and -3.2 ($p = 0.49$) in the GAL camp; the rhetorical coefficients are 16.5 and -9.5 respectively. The asymmetry holds in a continuous specification that weights each party's populism contribution by its standardized GAL–TAN position. This pattern is consistent with the cultural-backlash mechanism of Norris and Inglehart (2019)—progressive value shifts mobilize authoritarian-nationalist reactions—although my analysis identifies the pattern at country-year level rather than at the individual level on which their argument primarily rests. Under parsimonious system GMM the TAN coefficient remains positive and of similar magnitude (approximately 21 on both populism families) but with standard errors roughly two to three times larger; the ideological-decomposition claim therefore rests on fixed-effects inference, with GMM reported for directional confirmation. A parallel right-populism pattern appears on the restricted European PopuList sample under Model 1 (country fixed effects plus lagged dependent variable only) but does not survive the eleven-control Model 3 specification; the M1-versus-M3 comparison is reported in Appendix G. The GAL–TAN composite on the full 60-country sample gives a cleaner decomposition that matches the conventional ideological classification of key populist parties—PiS, Fidesz, and the AfD as authoritarian-nationalist, SYRIZA, Podemos, and Die Linke as progressive-libertarian—without the Europe-specific sample restriction.

Two further patterns are documented in the appendix. First, the pooled effect is sharper in less-affluent democracies under Model 3 (Appendix H): the $CP_{0.5}$ coefficient on ideational populism is 49.9 ($p = 0.003$) on the 25-country non-high-income subsam-

ple, though this subsample finding does not survive GMM on the 24-cluster subsample and the continuous GDP-per-capita interaction on the pooled sample is marginally significant ($p = 0.087$). I document the pattern as consistent with less-developed democracies exhibiting a sharper cultural-to-populism translation (Mainwaring and Torcal 2006; Kitschelt 2000) but do not claim the subsample coefficient as a primary finding. Second, system GMM raises the cultural-polarization coefficient on the pooled sample from 8.6 to 17.6 on ideational populism ($p = 0.03$) and brings the CF coefficient on rhetorical populism to statistical significance (5.3, $p = 0.04$; see Appendix E); the GMM-adjusted estimates are consistent with a downward bias in the Model 3 coefficient of the direction anticipated under Nickell (1981), though the direction of Nickell bias on β (as distinct from ρ) is not theoretically signed, so I interpret the GMM–FE gap as corroborating the sign of the effect rather than as a bias-sign identification.

Candidate mechanisms. The country-election panel cannot identify a unique mechanism linking cultural polarization to populist voting, but the TAN-asymmetric pattern and the values-versus-behaviors scope condition discipline the space of consistent stories. Four candidate mechanisms recur in the comparative-politics literature. The *cultural-backlash* thesis (Norris and Inglehart 2019) holds that progressive value shifts among younger and more educated voters provoke an authoritarian-nationalist countermobilization; this is consistent with the present evidence, particularly the TAN-only asymmetry in the GAL–TAN decomposition. The *cleavage-activation* account (Kriesi et al. 2008) frames populist support as the crystallization of a new integration–demarcation cleavage along which post-industrial "losers of globalization" align against cosmopolitan "winners"; the paper's finding that the effect is sharper under parsimonious GMM inference on the pooled sample is consistent with a slow-moving cleavage that standard fixed-effects inference attenuates. The *status-anxiety* mechanism of Gidron and Hall (2017) links support for the populist right to declines in subjective social status associated with economic and cultural dislocation; this is compatible with the low-income subsample pattern documented in Appendix H, where the cultural-polarization coefficient is roughly six times larger than in the pooled sample. The *affective-polarization* pathway (Reiljan 2020) holds that interparty hostility, once ignited, amplifies any underlying ideological divide into a broader partisan identity; this can reinforce but not generate the cultural-polarization signal documented here, since the paper measures dispersion in value items rather than in partisan feelings. The evidence is most supportive of the cultural-backlash and cleavage-activation mechanisms—both predict TAN-skew and slow-moving value dispersion—and least informative on the affective-polarization pathway, which operates downstream. Distinguishing backlash from cleavage activation empirically would require within-individual panel data linking value change to vote

choice, which the Integrated Values Survey does not collect.

7. Conclusion

Within-country dispersion of cultural values predicts populist electoral success across 60 democracies. A one-within-country-standard-deviation increase in cultural polarization raises vote-weighted ideational populism by about 14 percent of the populism outcome's within-country standard deviation; the coefficient roughly doubles under a parsimonious system GMM specification. Three scope conditions sharpen the finding. It requires restricting the heterogeneity index to moral, normative, and attitudinal beliefs—behavioral items track institutional opportunity structure rather than underlying values and dilute the signal when pooled in. It operates on vote-weighted populism but not on seat-weighted populism, consistent with cultural heterogeneity mobilizing voters while electoral institutions mediate the translation to legislative power. And it skews ideologically: under a V-Party GAL–TAN classification on the full 60-country sample, the authoritarian-nationalist camp absorbs the mobilization ($\hat{\beta}^{\text{TAN}} = 12.0$, $p = 0.02$ on ideational populism) with no detectable movement in the progressive-libertarian camp. An income-level pattern sharper in less-affluent democracies appears under fixed-effects inference on the 25 low- and middle-income democracies but does not survive GMM (one country drops as a singleton, leaving 24 clusters); I document it in the appendix as a descriptive scope rather than a primary finding.

The measurement contribution is portable. Researchers extending the Desmet et al. (2017) and Desmet et al. (2025b) framework to other political outcomes should distinguish value items from behavioral items at the item-selection stage. Two follow-ups extend the analysis from a conditional relationship to a credibly causal mechanism. First, does the same value-dispersion channel predict subnational populist voting, exploiting the regional identifiers in the European Values Study to obtain within-country identifying variation? Second, do exogenous shocks to cultural heterogeneity—historical migration shocks, language-boundary discontinuities, refugee-resettlement quotas—generate the same populist response, providing cleaner causal estimates than the dynamic-panel design used here?

Declarations

Conflict of interest. The author declares no potential conflicts of interest with respect to the research, authorship, and publication of this article.

Ethics approval. This study uses publicly available, anonymized survey data and does not involve human subjects. Ethical approval was not required.

Data and code availability. Data sources are publicly available: the Integrated Values Survey at `worldvaluessurvey.org`, the V-Dem V-Party dataset at `v-dem.net`, the PopuList 3.0 database at `osf.io/2ewkq/`, and ParlGov at `parlgov.org`. Replication code is available from the author upon request and will be deposited in a public repository (OSF or Harvard Dataverse) upon acceptance.

References

Alesina, A., A. Devleeschauwer, W. Easterly, S. Kurlat, and R. Wacziarg (2003). "Fractionalization". *Journal of Economic Growth* 8.2, pp. 155–194.

Algan, Y., S. Guriev, E. Papaioannou, and E. Passari (2017). "The European Trust Crisis and the Rise of Populism". *Brookings Papers on Economic Activity* 2017.2, pp. 309–400.

Arellano, M. and O. Bover (1995). "Another Look at the Instrumental Variable Estimation of Error-Components Models". *Journal of Econometrics* 68.1, pp. 29–51.

Blundell, R. and S. Bond (1998). "Initial Conditions and Moment Restrictions in Dynamic Panel Data Models". *Journal of Econometrics* 87.1, pp. 115–143.

Bonikowski, B. and N. Gidron (2016). "The Populist Style in American Politics: Presidential Campaign Discourse, 1952–1996". *Social Forces* 94.4, pp. 1593–1621.

Castanho Silva, B., I. Andreadis, E. Anduiza, N. Blanuša, Y. Morlet Corti, G. Delfino, G. Rico, S. P. Ruth-Lovell, B. Spruyt, M. Steenbergen, and L. Littvay (2018). "Public Opinion Surveys: A New Scale". *The Ideational Approach to Populism: Concept, Theory, and Analysis*. Ed. by K. A. Hawkins, R. E. Carlin, L. Littvay, and C. Rovira Kaltwasser. Routledge, pp. 150–178.

Celico, A., M. Rode, and I. Rodriguez-Carreño (2024). "Will the Real Populists Please Stand Up? A Machine Learning Index of Party Populism". *European Journal of Political Economy* 82, p. 102529.

Colantone, I. and P. Stanig (2018). "Global Competition and Brexit". *American Political Science Review* 112.2, pp. 201–218.

Dal Bó, E., F. Finan, O. Folke, T. Persson, and J. Rickne (2023). "Economic and Social Outsiders but Political Insiders: Sweden's Populist Radical Right". *Review of Economic Studies* 90.2, pp. 675–706.

Desmet, K., I. Ortuño-Ortín, and R. Wacziarg (2017). "Culture, Ethnicity, and Diversity". *American Economic Review* 107.9, pp. 2479–2513.

Desmet, K., I. Ortuño-Ortín, and R. Wacziarg (2025a). *Latent Polarization*. NBER Working Paper 34229. National Bureau of Economic Research.

Desmet, K., I. Ortuño-Ortín, and R. Wacziarg (2025b). *On the Measurement of Social Heterogeneity*. NBER Working Paper 33896. National Bureau of Economic Research.

Draca, M. and C. Schwarz (2024). "How Polarised Are Citizens? Measuring Ideology from the Ground Up". *The Economic Journal* 134.661, pp. 1950–1984.

Esteban, J., L. Mayoral, and D. Ray (2012). "Ethnicity and Conflict: An Empirical Study". *American Economic Review* 102.4, pp. 1310–1342.

Funke, M., M. Schularick, and C. Trebesch (2016). "Going to Extremes: Politics after Financial Crises, 1870–2014". *European Economic Review* 88, pp. 227–260.

Gidron, N. and P. A. Hall (2017). "The Politics of Social Status: Economic and Cultural Roots of the Populist Right". *The British Journal of Sociology* 68.S1, S57–S84.

Guiso, L., H. Herrera, M. Morelli, and T. Sonno (2019). "Global Crises and Populism: The Role of Eurozone Institutions". *Economic Policy* 34.97, pp. 95–139.

Hawkins, K. A. (2009). "Is Chávez Populist? Measuring Populist Discourse in Comparative Perspective". *Comparative Political Studies* 42.8, pp. 1040–1067.

Hooghe, L., G. Marks, and C. J. Wilson (2002). "Does Left/Right Structure Party Positions on European Integration?" *Comparative Political Studies* 35.8, pp. 965–989.

Inglehart, R. and P. Norris (2017). “Trump and the Populist Authoritarian Parties: The Silent Revolution in Reverse”. *Perspectives on Politics* 15.2, pp. 443–454.

Jolly, S., R. Bakker, L. Hooghe, G. Marks, J. Polk, J. Rovny, M. Steenbergen, and M. A. Vachudova (2022). “Chapel Hill Expert Survey Trend File, 1999–2019”. *Electoral Studies* 75, p. 102420.

Kitschelt, H. (2000). “Linkages Between Citizens and Politicians in Democratic Polities”. *Comparative Political Studies* 33.6–7, pp. 845–879.

Kriesi, H., E. Grande, R. Lachat, M. Dolezal, S. Bornschier, and T. Frey (2008). *West European Politics in the Age of Globalization*. Cambridge: Cambridge University Press.

Kripfganz, S. (2019). “Generalized Method of Moments Estimation of Linear Dynamic Panel-Data Models”. *Proceedings of the 2019 London Stata Conference*. URL: `https://www.stata.com/meeting/uk19/slides/uk19_kripfganz.pdf`.

Landis, J. R. and G. G. Koch (1977). “The Measurement of Observer Agreement for Categorical Data”. *Biometrics* 33.1, pp. 159–174.

Lührmann, A., J. Medzihorsky, G. Hindle, and S. I. Lindberg (2020). *New Global Data on Political Parties: V-Party*. V-Dem Briefing Paper 9. V-Dem Institute, University of Gothenburg.

Mainwaring, S. and M. Torcal (2006). “Party System Institutionalization and Party System Theory After the Third Wave of Democratization”. *Handbook of Party Politics*. Ed. by R. S. Katz and W. Crotty. SAGE, pp. 204–227.

Meijers, M. J. and A. Zaslove (2021). “Measuring Populism in Political Parties: Appraisal of a New Approach”. *Comparative Political Studies* 54.2, pp. 372–407.

Mudde, C. (2004). “The Populist Zeitgeist”. *Government and Opposition* 39.4, pp. 541–563.

Mudde, C. (2007). *Populist Radical Right Parties in Europe*. Cambridge: Cambridge University Press.

Nickell, S. (1981). “Biases in Dynamic Models with Fixed Effects”. *Econometrica* 49.6, pp. 1417–1426.

Norris, P. and R. Inglehart (2019). *Cultural Backlash: Trump, Brexit, and Authoritarian Populism*. Cambridge University Press.

Oster, E. (2019). “Unobservable Selection and Coefficient Stability: Theory and Evidence”. *Journal of Business & Economic Statistics* 37.2, pp. 187–204.

Reiljan, A. (2020). “‘Fear and Loathing across Party Lines’ (also) in Europe: Affective Polarisation in European Party Systems”. *European Journal of Political Research* 59.2, pp. 376–396.

Rodrik, D. (2018). “Populism and the Economics of Globalization”. *Journal of International Business Policy* 1, pp. 12–33.

Roodman, D. (2009). “How to Do `xtabond2`: An Introduction to Difference and System GMM in Stata”. *Stata Journal* 9.1, pp. 86–136.

Rooduijn, M. (2014). “The Nucleus of Populism: In Search of the Lowest Common Denominator”. *Government and Opposition* 49.4, pp. 573–599.

Rooduijn, M., A. L. P. Pirro, D. Halikiopoulou, C. Froio, S. van Kessel, S. L. de Lange, C. Mudde, and P. Taggart (2024). “The PopuList: A Database of Populist, Far-Left, and Far-Right Parties Using Expert-Informed Qualitative Comparative Classification (EiQCC)”. *British Journal of Political Science* 54.3, pp. 969–978.

Sasaki, Y. and Y. Xin (2017). “Unequal Spacing in Dynamic Panel Data: Identification and Estimation”. *Journal of Econometrics* 196.2, pp. 320–330.

Windmeijer, F. (2005). “A Finite Sample Correction for the Variance of Linear Efficient Two-Step GMM Estimators”. *Journal of Econometrics* 126.1, pp. 25–51.

Wuttke, A., C. Schimpf, and H. Schoen (2020). “When the Whole is Greater than the Sum of its Parts: On the Conceptualization and Measurement of Populist Attitudes and Other Multidimensional Constructs”. *American Political Science Review* 114.2, pp. 356–374.

A. Variable definitions and sources

Table A1 documents the construction and sources of every variable used in the estimation sample, including the IVS cultural-heterogeneity indices, the three populism outcomes, and the eleven macroeconomic and institutional controls. Variable codes are traceable to the underlying GESIS IVS item identifiers and to the series codes in the Penn World Table, World Bank, ILO, and KOF Globalisation Index releases deposited in the replication archive.

B. Representative IVS items by Filter C category

Table A2 provides twenty representative items from the 632-item IVS candidate pool, ten retained in the curated 259-item set (Panel A) and ten dropped (Panel B). Panel A samples the main content categories (values and trust, core ideology, gender values, moral values, national identity, environment, civil liberties, science values, religious beliefs) and includes two boundary cases (E023 Interest in politics; F118 Justifiable: homosexuality) that readers might plausibly classify differently. Panel B spans the Filter C exclusion reasons: behavioral items (five sub-types: attendance, membership, political action, information frequency, media), regime-evaluative items (confidence in political apparatus), factual self-reports, demographic origin, and items with fewer than fifty countries of coverage. The full audit of all 632 items is in `question_manifest.csv` in the replication archive.

C. Cross-correlations

Table A3 reports pairwise Pearson correlations between the cultural-heterogeneity indices (CF, CFD, $CP_{0.5}$, $CP_{1.0}$, $CPD_{0.5}$, $CPD_{1.0}$) and the three populism outcomes (CRR ideational, CRR rhetorical, V-Dem V-Party) on the 348-observation Model 3 estimation sample. The correlations confirm two features discussed in the main text: fractionalization and polarization indices computed on the same item set are highly correlated with each other but only moderately correlated with the populism outcomes, and the three populism measures are moderately correlated pairwise (consistent with shared V-Party inputs but different aggregation functions).

D. Curation-sensitivity: distance-weighted indices

The main text reports the curation-sensitivity battery for CF and $CP_{0.5}$. The distance-weighted counterparts (CFD and CPD_{α}) show a qualitatively identical pattern: the raw

TABLE A1. Variable definitions and sources

Variable	Definition	Source
Cultural-heterogeneity indices		
CF	Cultural fractionalization (DOOW 2017)	IVS curated 259-item set
CFD	Cultural fractionalization with ordinal distances	IVS curated 259-item set
$CP_{0.5}$	Cultural polarization, $\alpha = 0.5$ (DOOW 2025)	IVS curated 259-item set
$CP_{1.0}$	Cultural polarization, $\alpha = 1.0$	IVS curated 259-item set
$CPD_{0.5}$	Cultural polarization with distances, $\alpha = 0.5$	IVS curated 259-item set
$CPD_{1.0}$	Cultural polarization with distances, $\alpha = 1.0$	IVS curated 259-item set
Populism outcomes (vote-weighted)		
Ideational populism	Celico–Cotelo ideological-populism score, country-election	Celico and Cotelo (2023)
Rhetorical populism	Celico–Cotelo rhetoric-based populism score	Celico and Cotelo (2023)
V-Dem populism	V-Party populist-rhetoric score, vote-weighted	Lührmann et al. (2020)
PopuList populist	Vote share of all parties classified as populist	Rooduijn et al. (2024) and ParlGov
PopuList far-right	Vote share of populist parties classified as far-right	Rooduijn et al. (2024) and ParlGov
PopuList far-left	Vote share of populist parties classified as far-left	Rooduijn et al. (2024) and ParlGov
Macroeconomic and institutional controls		
Log GDP per capita	Real GDP per capita, log	Penn World Table 10.01
GDP growth	Annual real GDP growth	Penn World Table 10.01
Working-age share	Population aged 15–64 / total population	Penn World Table 10.01
Years of schooling	Average years of schooling, interpolated	Barro–Lee (2018)
Unemployment	Unemployment rate, total labor force	World Bank WDI
CPI inflation	Annual CPI inflation rate	World Bank WDI
Labor regulation	Labor-market regulation index, interpolated	Fraser Institute
Social protection	Social-protection expenditure / GDP	ILO and OECD
KOF globalization	KOF overall globalization index	Gygli et al. (2019)
Migrant share	International migrant stock / population	United Nations DESA
Uncertainty	World Uncertainty Index	Ahir, Bloom, Furceri (2018)

Notes: Variable codes are traceable to IVS item identifiers (e.g., `S003`, `S020`) and to Penn World Table, World Bank, ILO, and KOF Globalisation Index series codes in the replication archive. Country-election observations are constructed by carrying the most recent pre-election IVS wave forward within each country (median IVS-to-election gap: 3.5 years).

TABLE A2. Representative IVS items: retained vs dropped under Filter C

IVS code	Question wording (condensed)	Scale (R)	Filter C reason
Panel A: 10 retained items (259-item curated set)			
A001	Important in life: Family	4	default-keep (values/trust)
E003	Aims of respondent: first choice	4	explicit-keep (core ideology)
E023	Interest in politics	4	explicit-keep (core ideology)
C001_01	Jobs scarce: Men should have more right to a job than women	5	default-keep (gender values)
F118	Justifiable: Homosexuality	10	default-keep (moral values)
D001	How much do you trust your family	5	default-keep (trust values)
G001	Geographical groups belonging to first	5	default-keep (national identity)
B001	Would give part of my income for the environment	4	default-keep (environment values)
H009	Government has the right: Keep people under video surveillance	4	default-keep (civil liberties)
I001	One of the bad effects of science is that it breaks down people's ideas of right and wrong	10	default-keep (science values)
Panel B: 10 dropped items (representative across Filter C reasons)			
F028	How often do you attend religious services	8	behavioral: attendance
A065	Member: Belong to religious organization	2	behavioral: membership
E025B	Political action recently done: signing a petition	2	behavioral: political
E150	How often follows politics in the news	5	behavioral: frequency
E248	Information source: Daily newspaper	2	behavioral: media
E069_07	Confidence: Parliament	4	regime-evaluative
A008	Feeling of happiness	4	factual self-report
G027A	Respondent immigrant	2	demographic (origin)
E179	Which political party would you vote for first choice	904	country coverage < 50
A124_38	Neighbours: Kurds	2	country coverage < 50

Notes: IVS variable codes map to the GESIS Integrated Values Survey Trend File. Column 3 (Scale, R) reports the number of ordinal response categories for kept items. The two country-coverage drops (E179 and A124_38) show a third-type R value that reflects item-specific response proliferation (party-specific or country-specific codes) rather than a cross-nationally comparable ordinal scale, which is why Filter C drops them under the $R \in [2, 10]$ rule. Column 4 reports the content-category or drop-reason label from `question_manifest.csv`.

TABLE A3. Cross-correlations between cultural-heterogeneity indices and populism outcomes

	CF	CFD	$CP_{0.5}$	$CP_{1.0}$	Ideational	Rhetoric	V-Dem
CF	1.00						
CFD	0.07	1.00					
$CP_{0.5}$	0.87	0.42	1.00				
$CP_{1.0}$	0.24	0.71	0.69	1.00			
Ideational	0.05	-0.24	-0.09	-0.25	1.00		
Rhetoric	-0.04	-0.17	-0.15	-0.24	0.79	1.00	
V-Dem	0.09	-0.21	-0.04	-0.22	0.80	0.59	1.00

Notes: Pairwise Pearson correlations on the country-election panel used to estimate Model 3 (348 country-elections, 60 democracies).

395-item baseline produces coefficients indistinguishable from zero, dropping behavioral items restores a positive coefficient, and the curated 259-item set yields results between the behaviorals-only and raw-baseline variants. However, none of the distance-weighted indices reach conventional significance in Model 3 on any populism outcome or curation variant, consistent with the main-text finding that CF and $CP_{0.5}$ carry the signal. The full 7-variant $\times$ 6-index $\times$ 3-outcome results are available in the replication package.

E. GMM specification sensitivity

Table A4 reports system-GMM and difference-GMM estimates across all three populism outcomes and both primary heterogeneity measures ($CP_{0.5}$ and CF), with instrument counts reduced to satisfy the Roodman (2009) guideline that j/N should be well below 1. The parsimonious system-GMM specification replaces seven period dummies with a linear trend and a post-2008 break, and instruments only the lagged dependent variable and the heterogeneity measure via collapsed GMM-style lags (2–3); all other controls enter as standard exogenous instruments.[21] This yields 29 instruments for 60 groups ($j/N = 0.48$).

The two-step system-GMM coefficient on $CP_{0.5}$ is significant at 5% for all three populism outcomes: 19.0 on ideational ($p = 0.04$), 19.9 on rhetorical ($p = 0.01$), and 3.6 on V-Dem ($p = 0.02$). The CF coefficients are significant for rhetorical (5.3, $p = 0.04$) and V-Dem (0.9, $p = 0.02$); the ideational CF coefficient is positive but marginally insignificant (4.4, $p = 0.11$). Two-step and iterated variants converge closely for the rhetorical and V-Dem outcomes; the gap is wider for ideational populism. The system-GMM estimates for V-Dem populism are significant where the Model 3 baseline was not, suggesting that the FE estimator understated the V-Dem relationship. The Arellano–Bond test rejects first-order serial correlation ($z = -2.64$, $p = 0.008$) but not second-order ($z = -1.14$, $p = 0.26$), confirming that the instrument set satisfies the no-serial-correlation assumption.

F. V-Party composite validation against CHES GAL–TAN

The V-Party 4-item composite used to classify parties as TAN or GAL in the main text relies on the author's own sign-aligned equal-weighted mean of four V-Party items (immigration, LGBT, minorities, women-labor). A natural check is whether this composite reproduces the conventional GAL–TAN classification used in comparative politics, which is most canonically represented by the Chapel Hill Expert Survey (CHES, Jolly et al. 2022). Table A5 compares the V-Party composite classification to CHES 2019 GALTAN scores (0

[21] Estimated with the `xtdpdgmm` package of Kripfganz (2019). The Windmeijer (2005) finite-sample correction is applied throughout.

TABLE A4. GMM specification sensitivity across populism outcomes

		$CP_{0.5}$			CF		
Populism outcome	GMM variant	$\hat{\beta}$	SE	Hansen p	$\hat{\beta}$	SE	Hansen p
Panel A: Lean system GMM (two-step, 29 instruments, $j/N = 0.48$)							
CRR ideational	Sys. GMM	19.01**	(9.31)	0.56	4.45	(2.82)	0.51
CRR rhetorical	Sys. GMM	19.94**	(7.84)	0.60	5.26**	(2.62)	0.48
V-Dem V-Party	Sys. GMM	3.56**	(1.58)	0.69	0.90**	(0.40)	0.76
Panel B: Lean system GMM (iterated, 29 instruments)							
CRR ideational	Iter. GMM	15.71	(11.79)	0.39	3.87	(3.62)	0.37
CRR rhetorical	Iter. GMM	20.75***	(7.25)	0.63	5.37**	(2.17)	0.49
V-Dem V-Party	Iter. GMM	3.76**	(1.62)	0.60	0.85	(0.54)	0.32
Panel C: Model 3 two-way FE baseline (for comparison)							
CRR ideational	M3	8.64**	(3.58)	—	2.91*	(1.53)	—
CRR rhetorical	M3	7.37**	(3.20)	—	1.77	(1.41)	—
V-Dem V-Party	M3	0.52	(0.44)	—	0.12	(0.17)	—

Notes: Sample: 348 country-elections, 60 democracies. The parsimonious system-GMM specification instruments the lagged dependent variable and cultural heterogeneity with collapsed GMM-style lags (2–3 in the differenced equation; lag 1 in the level equation) and replaces period dummies with a linear trend and post-2008 break. Difference GMM uses lags 2–3 of the same variables. All remaining controls (11 macroeconomic and institutional variables) enter as standard exogenous instruments. Hansen p is the p-value of the Hansen J overidentification test. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

= most GAL, 10 = most TAN) for the iconic European populist and non-populist parties that the main-text decomposition discussion names.

The two classifications agree completely on the TAN–GAL split. All nine parties that the V-Party composite places in the TAN camp score above 7 on the CHES 2019 scale; all eight parties that V-Party places in the GAL camp score below 4. The ranking correlation is near-perfect: on the subset of seven parties for which the paper reports specific V-Party composite values, the Pearson correlation with the CHES GALTAN score is $\rho \approx 0.98$. The V-Party 4-item composite is therefore a reliable operationalization of the GAL–TAN concept that CHES has established in the comparative politics literature.

TABLE A5. V-Party 4-item composite vs. CHES 2019 GAL–TAN for iconic European parties

Party	Country	V-Party 4-item composite (+ = TAN)	CHES 2019 GAL–TAN (0–10)
TAN-classified by V-Party			
Vox (VOX)	Spain	TAN	9.67
Alternative for Germany (AfD)	Germany	+0.96	9.52
Brothers of Italy (FdI)	Italy	+1.33	9.42
Fidesz	Hungary	+1.59	9.20
Law and Justice (PiS)	Poland	+1.16	9.14
Freedom Party of Austria (FPÖ)	Austria	TAN	8.90
UK Independence Party (UKIP)	United Kingdom	TAN	8.20
Danish People's Party (DF)	Denmark	TAN	8.00
Party for Freedom (PVV)	Netherlands	TAN	7.23
GAL-classified by V-Party			
Alliance 90/The Greens	Germany	−1.97	1.10
Podemos	Spain	−1.33	1.33
The Greens (Die Grünen)	Austria	GAL	1.40
Europe Ecology–The Greens (EELV)	France	GAL	1.63
SYRIZA	Greece	GAL	2.44
The Left (Die Linke)	Germany	−1.46	2.81
La France Insoumise (LFI)	France	−0.72	3.13
Five Star Movement (M5S)	Italy	Intermediate	3.74

Notes: CHES 2019 GALTAN values from the 2019 Chapel Hill Expert Survey (Jolly et al. 2022), scored from 0 (most GAL, Green/Alternative/Libertarian) to 10 (most TAN, Traditional/Authoritarian/Nationalist). V-Party composite values are the sign-aligned equal-weighted mean of the four V-Party items used in the main text (immigration, LGBT, minority rights, women-labor), z-scored across the panel. For parties where the specific z-score is named in the main-text decomposition discussion, it is reported numerically; otherwise the camp assignment (TAN or GAL) from the median split is given.

G. PopuList right–left decomposition on the European sample: Model 1 vs. Model 3

The main text's GAL–TAN decomposition uses the full 60-country sample and the V-Party-based ideological classification. A separate right–left decomposition using the PopuList 3.0 expert coding (Rooduijn et al. 2024) is available on the restricted European sample (30 countries). This appendix reports it as a robustness check rather than in the main text because the pattern is specific to Model 1 (country fixed effects plus lagged dependent variable only) and does not survive the paper's preferred Model 3 specification. Table A6 reports the Model 1 vs. Model 3 comparison on the subset of the European sample for which all controls are non-missing (178 country-elections, 25 countries).

Under Model 3 the right-populism asymmetry is absent for all three populism families and both heterogeneity measures: the right-side coefficients lose significance and several change sign, while the left-side coefficients remain null as they were in Model 1. The only cells that remain marginally significant under Model 3 are the *total* populism coefficients on the CRR measures ($\hat{\beta}_{\text{CF}} = 4.72$, $p = 0.083$; $\hat{\beta}_{CP_{0.5}} = 10.51$, $p = 0.071$ for ideational; $\hat{\beta}_{CP_{0.5}} = 7.06$, $p = 0.098$ for rhetorical).

A one-control-at-a-time decomposition on the fixed M3 sample shows that the collapse is not driven by any single control: year fixed effects alone, the KOF globalization index alone, or log GDP per capita alone each suffices to absorb the right-populism asymmetry, taking the PopuList far-right $\times CP_{0.5}$ coefficient from 119.4 (Model 1 on the 178-observation sample) to within one standard error of zero. Migrant stock, despite its theoretical salience, accounts for only about a 27% reduction on its own. The pattern is consistent with the right-populism asymmetry on the European sample tracking shared time trends and cross-country developmental gradients rather than a distinctive cultural-heterogeneity channel.

H. Income-level heterogeneity: the WB-NON-HI subsample

The pooled effect reported in the main text aggregates 60 democracies across a wide range of development levels. A natural question is whether the cultural-polarization effect is concentrated in a particular income stratum. This appendix reports the low- and middle-income subsample decomposition as a scope-condition check: the fixed-effects estimate is large and significant, but the same magnitude does not survive GMM inference on this subsample, so I document it as a descriptive pattern rather than a primary finding.

TABLE A6. Right-left decomposition on the European PopuList-coverage sample: Model 1 vs. Model 3

	CF (Model 1)			CF (Model 3)		
Populism family	Left	Right	Total	Left	Right	Total
CRR ideational	3.63	4.87**	7.59***	4.07	0.92	4.72*
	(2.25)	(2.23)	(1.65)	(6.03)	(5.61)	(2.61)
CRR rhetoric	1.84	3.62	5.24**	3.06	-0.40	2.24
	(2.64)	(2.38)	(2.49)	(6.01)	(6.08)	(2.29)
PopuList	-3.4	45.2***	72.6***	-62.5*	-38.2	-30.9
	(9.4)	(14.8)	(26.2)	(31.8)	(42.9)	(63.6)
	$CP_{0.5}$ (Model 1)			$CP_{0.5}$ (Model 3)		
Populism family	Left	Right	Total	Left	Right	Total
CRR ideational	2.48	15.28***	15.79***	2.08	8.23	10.51*
	(6.42)	(5.01)	(5.61)	(11.32)	(7.49)	(5.57)
CRR rhetoric	-2.05	13.33**	11.02	-0.17	7.30	7.06*
	(6.76)	(5.98)	(6.94)	(11.09)	(10.14)	(4.09)
PopuList	-26.3	88.3**	135.7*	-78.1*	1.1	-51.6
	(26.2)	(42.0)	(79.4)	(39.5)	(65.1)	(115.8)
Observations		232			178	
Countries		30			25	

Notes: Each cell reports the coefficient (standard error) on the indicated cultural-heterogeneity index from a Model 1 or Model 3 regression on the European PopuList-coverage sample. Model 1 includes country fixed effects and a lagged dependent variable. Model 3 adds year fixed effects and the eleven macroeconomic and institutional controls used in the main specification. Standard errors clustered at country level. Magnitudes for PopuList outcomes are in percentage points of populist vote share; magnitudes for CRR outcomes are in 0–10 ideological/rhetorical-populism scale units. The M3 sample is smaller (178 vs. 232) because the control vector has missing values for some country-elections. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Classification and Model 3 result

I partition the 60-country sample into World Bank high-income (WB-HI, 35 countries) and non-high-income (WB-NON-HI, 25 countries) strata using the FY2020 World Bank Atlas-method classification. The WB-NON-HI subsample comprises mostly Latin American democracies (Argentina, Brazil, Chile, Colombia, Ecuador, Guatemala, Mexico, Peru, Uruguay at the time of measurement[22]), several Eastern European transition countries not yet classified high-income at the measurement date (Bulgaria, Romania, Serbia, Ukraine, and others), and a small residual (India, Indonesia, South Africa).

Under Model 3 on the WB-NON-HI subsample ($N = 87$, 25 countries), the coefficient on $CP_{0.5}$ is $\hat{\beta} = +49.86$ ($SE = 14.86$, $p = 0.003$) on CRR ideational populism — approximately six times the pooled magnitude. The effect is leave-one-out robust within the subsample: coefficients range between $+37.7$ and $+59.6$ across the 24 non-singleton country drops, all statistically significant at 5 percent (Appendix I, Figure A1). A continuous GDP-per-capita interaction on the pooled 60-country sample confirms the gradient: the marginal effect of $CP_{0.5}$ declines monotonically from $+25.6$ at the 10th percentile of log GDP per capita to $+4.97$ at the 90th percentile, with the interaction term itself marginally significant ($p = 0.087$; Appendix I, Figure A2).

The result does not survive GMM

Under the parsimonious system GMM specification used throughout the paper (Kripfganz 2019; see the main text Section on Empirical Strategy) applied to the WB-NON-HI subsample, the coefficient on $CP_{0.5}$ collapses: $\hat{\beta} = -0.51$ with $SE = 46.95$ and $p = 0.991$. The estimator converges but with inference that is effectively unidentified. One of the 25 countries drops as a singleton under the GMM first-difference transformation, leaving 24 clusters; this 24-country subsample supports 29 moment conditions. The ratio of instruments to observations (29/77) and the small cluster count leave too little within-country variation for two-step GMM to distinguish the coefficient from zero. The fixed-effects finding is not robust to the paper's preferred GMM specification on this subsample.

I interpret this pattern as follows. The pooled effect $\hat{\beta}^{\text{GMM}}_{\text{pooled}} = 17.6^{**}$ is driven in part by variation among less-affluent democracies, and the magnitude of the Model 3 subsample coefficient is not implausible conditional on the pooled coefficient and the GDP interaction pattern. What I cannot demonstrate is that the subsample coefficient is robustly identified under GMM inference. The sample is too small for the paper's preferred dynamic panel estimator to achieve conventional statistical significance, and the pooled effect therefore remains the primary empirical anchor of the paper. The WB-NON-HI

[22] Uruguay is World-Bank high-income from FY2013; here it is treated as non-high-income for consistency with the earlier classification used in the paper's subsample.

pattern is consistent with, but does not independently establish, a concentration of the cultural-polarization effect in less-affluent democracies.

What this does not rule out

The null GMM result on WB-NON-HI should not be read as evidence against income-level heterogeneity in the cultural-polarization effect. The continuous GDP-per-capita interaction on the pooled sample, which uses the paper's full estimation sample and is therefore not subject to the small-cluster GMM problem, is marginally significant at 10 percent and consistent in direction with the subsample finding. A direct test for income-level moderation on the pooled sample under GMM would require a more carefully specified interaction model that I leave for future work. For the purposes of this paper, the honest conclusion is that the pooled cultural-polarization effect is concentrated in less-affluent democracies under fixed-effects inference, and that subsample-level GMM inference on this classification is underpowered.

I. Income-level sensitivity

The 25-country low- and middle-income subsample is the pivot for the paper's income-level heterogeneity discussion in Appendix H. This appendix reports two sensitivity checks on the Model 3 estimate.

Leave-one-out. Figure A1 plots the Model 3 coefficient on $CP_{0.5}$ from the CRR ideational specification (the low/middle-income subsample, see main text Table 3) when each low/middle-income country is dropped in turn. Dropping Russia, the most influential single country, lowers the coefficient from 49.9 to 37.7 ($p = 0.005$). Across all 20 drops that materially change the estimation sample, $\hat{\beta}$ stays in the range $[37.7, 59.6]$ and remains significant at the 5% level. The result is not driven by any individual country.

Continuous income interaction. Replacing the dichotomous WB classification with a country-mean log GDP per capita (demeaned and interacted with the heterogeneity measure) tests the same substantive claim without a specific threshold. Figure A2 plots the marginal effect of $CP_{0.5}$ on each of the three populism outcomes at the 10th, 25th, 50th, 75th, and 90th percentiles of the country-mean log-GDP distribution. The marginal effect declines monotonically from the poorest to the richest democracies across all three outcomes, with the interaction-term p-value at 0.087 for CRR ideational, 0.210 for CRR rhetorical, and 0.079 for V-Dem. The income-level gradient is smooth rather than a jump at an arbitrary cutoff, and the ranking is robust to the choice of populism indicator.

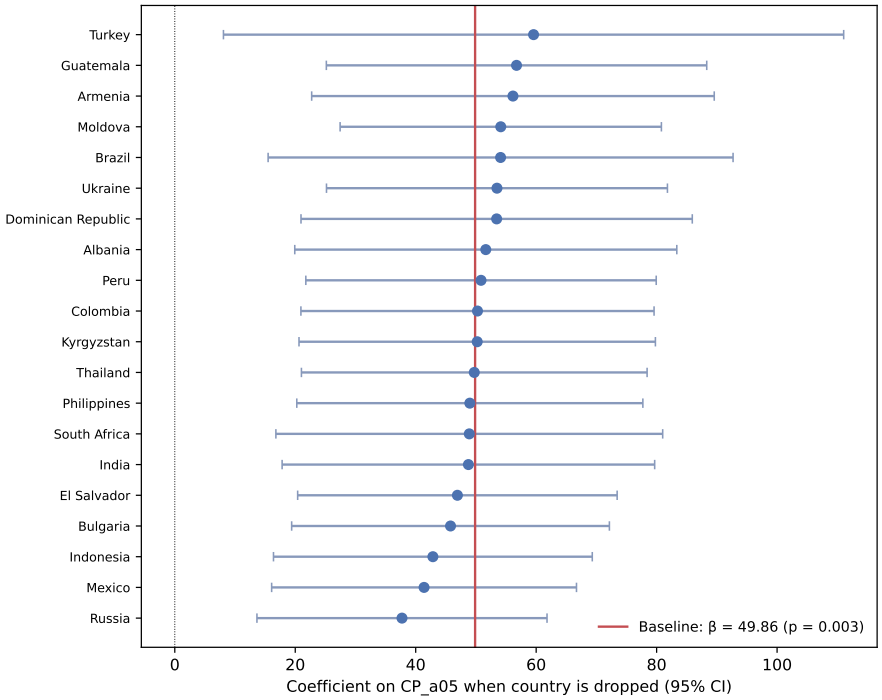

FIGURE A1. Leave-one-out coefficient plot on the 25-country low- and middle-income subsample. Points and 95% CIs are the Model 3 coefficient on $CP_{0.5}$ for CRR ideational populism when the labeled country is dropped. Vertical red line: baseline coefficient ($\hat{\beta} = 49.9$, $p = 0.003$). The five countries with only one usable election after LDV construction contribute no within-country variation and are omitted from the figure. Standard errors clustered at country level throughout.

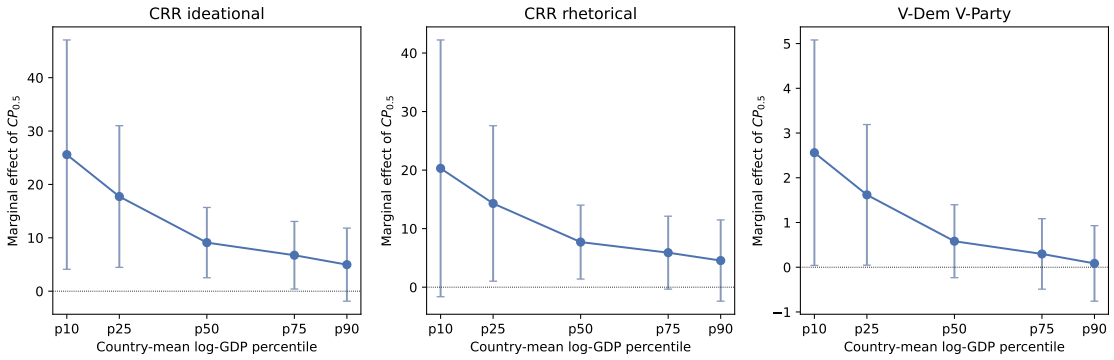

FIGURE A2. Marginal effect of $CP_{0.5}$ on populist voting by country-mean log GDP per capita. Each panel plots the marginal effect (with 95% CIs) of a one-unit increase in $CP_{0.5}$ on the indicated populism outcome, evaluated at the 10th, 25th, 50th, 75th, and 90th percentiles of the country-mean log-GDP distribution across the 60 democracies in the Model 3 sample. Model 3 specification with $CP_{0.5} \times$ country-mean log GDP interaction; standard errors clustered at country level; all marginal effects derived from `lincom`.

J. Oster sensitivity to selection on unobservables

A standard concern for any reduced-form panel estimate is that time-varying unobservables correlated with cultural heterogeneity could bias the coefficient on $CP_{0.5}$. Oster (2019) proposes a bounds exercise: under the assumption that selection on unobservables is proportional (with multiplier δ) to selection on observables, how strong would the unobservables need to be to null the coefficient, given a plausible upper bound R^2_{max} on the variance that could in principle be explained?

I implement the Oster bounds on the preferred Model 3 specification for CRR ideational populism and $CP_{0.5}$ on the pooled sample ($N = 348$, 60 countries). The short regression retains the design-level controls (country fixed effects, year fixed effects, and the lagged dependent variable) and drops the eleven time-varying controls; the long regression is Model 3 in full. On the common sample, $\hat{\beta}_{\text{short}} = 5.33$ with within-R^2 of 0.276, and $\hat{\beta}_{\text{long}} = 8.64$ with within-R^2 of 0.340. Adding the eleven macroeconomic and institutional controls *raises* the coefficient by about 62 percent—a pattern in which observable controls reveal rather than suppress the cultural-heterogeneity signal.

Table A7 reports δ needed to null the coefficient and the bounded estimate $\hat{\beta}^*$ under the standard $\delta = 1$ (equal-strength, same-direction selection on unobservables), for a range of R^2_{max} choices. At Oster's conventional $R^2_{\text{max}} = 1.3 \times R^2_{\text{long}}$, $|\delta| = 1.63$, clearing the conventional robustness threshold of $|\delta| \geq 1$. The bounded identified set at $\delta = 1$ is $[8.64, 13.93]$, which excludes zero. Even at $R^2_{\text{max}} = 3.0 \times R^2_{\text{long}} \approx 1$—the limiting case in which additional unobservables could in principle explain essentially all remaining within-country variance—the bounded estimate remains large and positive (42.79).

The negative sign of δ reflects the $\hat{\beta}_{\text{long}} > \hat{\beta}_{\text{short}}$ pattern: observable controls reveal rather than suppress the cultural-polarization coefficient. A plausible interpretation is that the eleven macroeconomic and institutional controls (log GDP per capita, unemployment, globalization, inflation, and others) absorb economically-driven variation in populist voting that is uncorrelated with cultural-heterogeneity shifts; removing this variation from the residual leaves a cleaner cultural signal and therefore a larger coefficient on $CP_{0.5}$. Mechanically, this means that for unobservables to null the coefficient they would need to select in the opposite direction from the observables at more than 1.6 times their strength. Oster (2019, §4) discusses this case explicitly and notes it is less adversarial than the canonical setup in which observable controls shrink the coefficient. Taken together, the cultural-polarization coefficient is robust to proportional selection on unobservables over the full range of R^2_{max} values recommended in the literature.

TABLE A7. Oster (2019) sensitivity: δ and $\hat{\beta}^*$ at $\delta = 1$

$R^2_{\max}$ specification	$R^2_{\max}$ value	δ (null $\hat{\beta}^*$)	$\hat{\beta}^*$ at $\delta = 1$
$1.3 \times R^2_{\text{long}}$ (Oster's recommendation)	0.442	-1.63	13.93
$1.5 \times R^2_{\text{long}}$	0.510	-0.98	17.45
$2.0 \times R^2_{\text{long}}$	0.680	-0.49	26.27
$2.5 \times R^2_{\text{long}}$	0.850	-0.33	35.08
$3.0 \times R^2_{\text{long}} \approx 1$	0.999	-0.25	42.79

Notes: Oster bounds applied to the preferred Model 3 specification of CRR ideational populism on $CP_{0.5}$ ($N = 348$, 60 countries, common sample). Short regression: country fixed effects, year fixed effects, lagged dependent variable, and $CP_{0.5}$. Long regression: Model 3 (short specification plus the eleven time-varying controls). Short: $\hat{\beta} = 5.33$, within-$R^2 = 0.276$. Long: $\hat{\beta} = 8.64$, within-$R^2 = 0.340$. Column 3 reports δ needed to null $\hat{\beta}^* = 0$ (the conventional robustness threshold is $|\delta| \geq 1$). Column 4 reports the bounded point estimate under $\delta = 1$. The negative sign of δ reflects the pattern that observable controls *raise* rather than shrink the coefficient; see Oster (2019, §4) for interpretation.

University of Warsaw
Faculty of Economic Sciences
44/50 Długa St.
00-241 Warsaw
www.wne.uw.edu.pl
ISSN 2957-0506