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Abstract

Populists govern more of the world's countries than at any point in the past century, on both the right and the left. What they share is not an economic programme but a worldview that casts politics as a struggle between ordinary citizens and a self-serving establishment, a conflict that is zero-sum at its core. We show that exposure to this political environment leaves a durable imprint on citizens' beliefs. Combining data on populist leaders with surveys of more than 260,000 respondents in 53 countries, we exploit within-country-survey-year variation across birth cohorts and find that greater exposure during early adulthood is associated with stronger zero-sum beliefs later in life. The result is replicated in two independent surveys and across multiple measures of zero-sum thinking and perceived economic conflict and persists into respondents' mid-forties. The associations extend to voting intentions: exposure does not predict support for populist parties overall, but it does predict support for ideologically extreme parties aligned with the populist rule experienced in early adulthood. The mechanism evidence provides little support for an economic-scarcity explanation and instead points towards a rhetorical channel, as populist rule features substantially more anti-elite rhetoric in two independent measures and more domestic zero-sum framing in United Nations speeches.

JEL Classification: D72, P16

Keywords: Populism, zero-sum beliefs

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Populist Rule and the Formation of Zero-Sum Beliefs*

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September 9, 2026

Abstract

Populists govern more of the world's countries than at any point in the past century, on both the right and the left. What they share is not an economic programme but a worldview that casts politics as a struggle between ordinary citizens and a self-serving establishment, a conflict that is zero-sum at its core. We show that exposure to this political environment leaves a durable imprint on citizens' beliefs. Combining data on populist leaders with surveys of more than 260,000 respondents in 53 countries, we exploit within-country-survey-year variation across birth cohorts and find that greater exposure during early adulthood is associated with stronger zero-sum beliefs later in life. The result is replicated in two independent surveys and across multiple measures of zero-sum thinking and perceived economic conflict and persists into respondents' mid-forties. The associations extend to voting intentions: exposure does not predict support for populist parties overall, but it does predict support for ideologically extreme parties aligned with the populist rule experienced in early adulthood. The mechanism evidence provides little support for an economic-scarcity explanation and instead points towards a rhetorical channel, as populist rule features substantially more anti-elite rhetoric in two independent measures and more domestic zero-sum framing in United Nations speeches.

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1 Introduction

Populist governments have become a prominent feature of contemporary politics on both the right and the left. A growing literature documents substantial economic costs of populist rule (Funke et al., 2023; Bellodi et al., 2024). Yet populism is defined not by a common economic programme but by a particular worldview: a thin-centred ideology organised around an antagonism between “the people” and “a corrupt elite” (Mudde, 2007). That antagonism is zero-sum at its core.

Whether exposure to this political worldview leaves a lasting imprint on citizens’ own beliefs is unclear. On the one hand, repeated exposure to a narrative that portrays gains for one group as losses for another may lead citizens to interpret economic and social relations in more zero-sum terms. On the other hand, populist rhetoric may primarily reflect beliefs and grievances that voters already hold rather than reshape them. Indeed, existing work has largely treated pre-existing grievances, including zero-sum beliefs, as a source of support for populist leaders (Guriev and Papaioannou, 2022). Even if populist rule does influence beliefs, it is not clear whether such effects persist once the period of exposure has passed.

Here we provide large-scale evidence on this question. We combine data on populist leaders from Funke et al. (2023) with individual-level survey data from the World Values Survey (WVS) and the International Social Survey Programme (ISSP), which provide complementary measures of zero-sum thinking and perceived economic conflict. Together, the surveys cover more than 260,000 respondents in 53 countries, whom we link to political histories spanning more than seven decades. We focus on exposure between ages 18 and 25, the period in which political attitudes are thought to be especially malleable (Krosnick and Alwin, 1989; Giuliano and Spilimbergo, 2025). For each respondent, we measure the share of these years spent under a populist government.

Our empirical strategy exploits between-cohort variation within country-survey-year cells. We ask whether cohorts that experienced more populist rule during ages 18 to 25 report stronger zero-sum beliefs than other cohorts surveyed in the same country and the same year. Our preferred specification includes country-by-survey-year fixed effects, birth-cohort fixed effects, age fixed effects, and individual controls. The country-by-survey-year fixed effects ensure that the comparison is between respondents facing the same political, economic, and social environment at the time of the survey, while the cohort and age fixed effects account for common generational and life-cycle patterns.

We find that greater exposure to populist rule during early adulthood is associated with stronger zero-sum thinking later in life. In our preferred WVS specification, full exposure between ages 18 and 25, relative to no exposure, is associated with a 0.092-point increase in zero-sum thinking on the 1 to 10 scale. This corresponds to roughly one-fifth of the gap between the United States and Türkiye. We obtain the same qualitative pattern in the ISSP using three complementary measures of perceived conflict between economic groups. The relationship is persistent, remaining detectable among respondents observed into their mid-forties.

We further address identification concerns in four ways. First, we show that populist rule has no comparable association with zero-sum thinking when exposure occurs before birth, in childhood, in adolescence, or later in adulthood. Rolling-window estimates become larger as the exposure window approaches ages 18 to 25, and the pre-birth coefficient is close to zero. Second, we examine whether the estimates are driven by the economic and political conditions under which populists come to

power. We control for democracy, GDP per capita, and a broad set of political and social upheavals experienced during the exposure window, including assassinations, general strikes, terrorism, government crises, purges, riots, revolutions, and anti-government demonstrations. We also account for observable characteristics of the populist leaders themselves, including their background, prior career, political experience, and ideology. The estimates remain similar, indicating that the results are not driven by other conditions experienced during the exposure window or by observable differences across populist leaders.

Third, we address concerns about selection and reverse causality. Selective migration could matter if exposure to populist rule affects who remains in, or leaves, a country, thereby changing the composition of the cohorts we observe. Using respondents' country of birth, however, we find no evidence that cohorts with greater exposure to populist rule during ages 18 to 25 are more likely to appear as migrants in the survey data. Reverse causality could also arise if young people with stronger pre-existing zero-sum beliefs help elect populist leaders and are then classified as exposed to them. To address this concern, we separate exposure to populists who were already in office when a respondent turned 18 from exposure to populists who came to power during ages 19 to 25. The coefficient on exposure to populists already in office remains positive. Because these governments came to power before the respondent entered the formative period, their election cannot be explained by the political choices of that cohort during ages 18 to 25. This mitigates the concern that our main result simply captures young people with stronger pre-existing zero-sum beliefs helping bring populists to power.

Finally, we examine whether formative exposure to populist rule is associated with broader differences in attitudes later in life, rather than specifically with zero-sum thinking. Placebo tests show no systematic relationship with a range of other economic, social, and cultural attitudes, suggesting that the main result is specific to the zero-sum outcomes we study. The estimates are also stable across alternative covariate sets, controls for parental influence, a post-1945 cohort sample, alternative weighting schemes, leave-one-out exercises, and permutation-based inference.

The second part of the paper examines why exposure to populist rule might shape zero-sum beliefs. We consider two broad channels. The first is economic. Populist governments often underperform their non-populist counterparts (Funke et al., 2023), and economic hardship and perceived scarcity may themselves foster zero-sum thinking. Early-life economic success, conversely, has been shown to reduce zero-sum thinking (Chinoy et al., 2026). If this channel were central, the relationship we document could reflect the economic conditions associated with populist rule rather than populism itself.

We find little support for this mechanism. The estimate remains similar when we account for economic conditions during the same exposure window and for individual correlates of scarcity and competition. More directly, exposure to recessions during ages 18 to 25 when no populist is in office does not predict stronger zero-sum thinking. Nor is the relationship concentrated in periods when populist rule coincides with economic hardship. Instead, the positive estimate is concentrated outside recessions and during years of positive GDP-per-capita growth. Formative populist exposure is also not systematically associated with respondents' financial satisfaction at the time of the survey. Moreover, the point estimates are larger where populist leaders face stronger institutional constraints. Since these constraints limit the scope for populist governments to alter policy and economic outcomes, the combined pattern is difficult to reconcile with a mechanism

operating primarily through economic hardship.

We next investigate a rhetorical channel with three linked implications. Populist rule should expose citizens to more anti-elite and zero-sum rhetoric. Because citizens often rely on experts to judge whether these claims are credible, populist exposure may also reduce trust in experts whose views contradict them. And if political communication carries these narratives, the association could be stronger where media were freer and reached more citizens, although the direction is not obvious *ex ante*: freer media give citizens access to competing information, but they also make criticism from journalists and experts visible, and populists can portray that criticism as further evidence of elite obstruction.

The first prediction is strongly supported. Using two independent measures, we find large shifts in political rhetoric under populist rule. In V-Party, governing-party anti-elitism is 1.14 standard deviations higher. In a separate text-based analysis of more than 11,000 United Nations speeches, anti-elite rhetoric is 0.58 standard deviations higher and domestic zero-sum framing is 0.67 standard deviations higher under populist leaders. Populist rule is therefore associated with a political environment in which people-versus-elite and zero-sum narratives are substantially more prominent.

The second prediction concerns trust in experts. Populists may have incentives to discredit those who challenge their claims (Ali et al., 2025; Szeidl and Szucs, 2025), which is particularly relevant when citizens cannot directly assess whether complex economic and social relationships are truly zero-sum. Consistent with this mechanism, formative exposure is associated with lower trust across six measures of science and scientists, with a summary effect of about 0.31 standard deviations. We find no comparable decline in trust across a broader set of institutions and groups, indicating that the pattern is concentrated in sources of expertise that can provide competing interpretations.

The third prediction concerns the reach of political communication. The results are stronger where media freedom is greater and where radio and television reach a greater share of the population; that is, where the confrontation between populists and the actors they cast as elites is most visible. The media gradient also survives allowing the effect of populist exposure to vary flexibly with institutional quality, reducing the concern that it simply reflects broader differences across political systems. Overall, these results are more consistent with a rhetorical channel than with an economic-scarcity explanation.

We then ask whether the imprint of formative populist exposure extends beyond beliefs to later political preferences. We find no evidence that exposed respondents are more likely to support populist parties in general. However, they are more likely to support parties at the ideological extremes, and the pattern lines up with the ideology of the populist government experienced during early adulthood: exposure to left-wing populists is associated with greater far-left support, while exposure to right-wing populists is associated with greater far-right support.

Related literature and contributions. Our analysis connects to several literatures. First, it broadens the literature on the consequences of populism from economic and institutional outcomes to belief formation and political culture. Existing work shows that populist governments weaken checks and balances and democratic institutions (Guriev and Papaioannou, 2022), underperform non-populist governments across several economic dimensions (Funke et al., 2023), and degrade bureaucratic quality (Bellodi et al., 2024). A related strand examines particular regimes or political

events, documenting effects on trade, investment, risky behaviour, and other outcomes (Breinlich et al., 2020; Ajzenman et al., 2023; Costa et al., 2024; Bloom et al., 2025). We show that exposure to populist rule is associated with durable differences in how citizens understand social and economic life, including a more zero-sum worldview.

Second, we contribute to the literature on the origins of zero-sum thinking. Existing work links zero-sum beliefs to deep historical experience, economic development, social mobility, and informational conflict. Voigtländer and Voth (2012) show how historical persecution can leave persistent cultural and political legacies. Carvalho et al. (2023) develop an evolutionary account in which rivalrous economic environments sustain effort-suppressing and zero-sum beliefs. Chinoy et al. (2026) show that individual and ancestral economic experiences and historical legacies shape zero-sum thinking, and Ali et al. (2025) show that such beliefs can persist when citizens perceive experts or other groups as having opposed interests. We add a political origin: exposure to populist rule. The distinction matters because populism is often understood as a response to citizens' pre-existing grievances and beliefs. Our results suggest that exposure to populist rule may in turn reinforce the zero-sum worldview with which populism is closely associated.

Third, our findings speak to a growing literature showing that political rhetoric and communication have real effects. Political messages can shift candidate support even when voters are exposed to fact-checking (Barrera et al., 2020), redirect attention towards politically salient issues (Jetter and Molina, 2022), raise hate crimes against minorities (Müller and Schwarz, 2021, 2023), and affect discriminatory behaviour by law enforcement (Barilari and Zambiasi, 2026). We extend this literature by showing that exposure to a political environment characterised by populist rhetoric during early adulthood is associated with differences in beliefs observed much later. Our mechanism evidence also connects to recent theories in which populist rhetoric operates partly by discrediting the experts who could otherwise challenge zero-sum narratives (Ali et al., 2025; Szeidl and Szucs, 2025).

Fourth, we contribute to the literature on political socialisation and formative experiences. Attitudes are especially malleable in young adulthood and become more stable thereafter (Krosnick and Alwin, 1989; Giuliano and Spilimbergo, 2025). A growing literature shows that experiences during these years can leave lasting effects on adult beliefs and political attitudes, including exposure to epidemics and corruption (Eichengreen et al., 2021, 2024; Daniele et al., 2023; Aksoy et al., 2025).¹ We add populist rule as a formative political experience and show that its associations extend beyond institutional trust to how citizens interpret social and economic conflict. We also provide evidence that these associations extend to later political preferences.

The rest of the paper is organised as follows. Section 2 describes the data and construction of the populism exposure measures. Section 3 presents the empirical strategy. Section 4 reports the main results and robustness checks. Section 5 explores the mechanisms. Section 6 presents evidence on political preferences. Section 7 concludes.

¹Related work studies cumulative or lifetime exposure to political and institutional environments, including experience with democracy (Fuchs-Schündeln and Schündeln, 2015) and exposure to successful democratic performance (Acemoglu et al., 2025). A separate strand shows that early-life economic experiences have persistent effects on adult preferences and beliefs, including exposure to downturns, inflation, broader macroeconomic conditions, inequality, and economic growth (Malmendier and Nagel, 2011, 2016; Roth and Wohlfart, 2018; Cotofan et al., 2023; Besley et al., 2026).

2 Data

This section describes our main data sources, the construction of our treatment measure, and the additional datasets used for robustness checks and mechanism tests.

2.1 Survey data on zero-sum thinking

World Values Survey. Our main individual-level data come from the World Values Survey (WVS). We focus on the waves in which our key outcome, zero-sum thinking, is available: Wave 2 (1989 to 1993), Wave 3 (1994 to 1998), Wave 5 (2005 to 2009), and Wave 6 (2010 to 2014).² The WVS provides nationally representative samples from more than 90 countries, with interviews conducted in respondents' local languages.

Our main outcome measures whether respondents view economic gains as zero-sum. Specifically, we use responses to the following survey item:

Now I'd like you to tell me your views on various issues. How would you place your views on this scale? 1 means you agree completely with the statement on the left; 10 means you agree completely with the statement on the right; and if your views fall somewhere in between, you can choose any number in between.

1 = *People can only get rich at the expense of others*

⋮

10 = *Wealth can grow so there's enough for everyone*

Because the original scale runs from 1 at the zero-sum pole to 10 at the non-zero-sum pole, we reverse-code it as $11 - x$, so that higher values indicate stronger zero-sum beliefs. This single-item measure has been shown to capture much of the same variation as a multi-question index, making it a useful cross-country measure of zero-sum thinking (Chinoy et al., 2026).

We use respondents' country of residence and year of birth to link them to the exposure measure described below. We also use individual characteristics, including gender, education, having children, and income, as controls.

International Social Survey Programme. We also use the International Social Survey Programme (ISSP) as a complementary source of evidence. We focus on the Social Inequality modules fielded in 1987, 1992, 1999, 2009, and 2019. The ISSP is a long-running cross-national survey project covering more than 30 countries, with nationally representative samples and harmonised questionnaires administered in respondents' local languages.

The ISSP does not include the same zero-sum item as the WVS. We therefore use perceived economic and class conflict as a complementary proxy for a zero-sum view of economic relations. In each Social Inequality wave, respondents are asked to evaluate the level of conflict in their country between:

... *poor people and rich people?*

... *the working class and the middle class?*

... *management and workers?*

²The number of respondents from our baseline specification in each country-wave and year is reported in Appendix Table A.1.

Each item is measured on a four-point scale: *very strong conflicts*, *strong conflicts*, *not very strong conflicts*, and *there are no conflicts*. We use these questions to construct three versions of a conflict-based index: (i) the simple average of the three raw items, reverse-coded so that higher values indicate greater perceived conflict; (ii) a binary index equal to one for each dyad in which the respondent reports very strong or strong conflict, averaged across the three dyads; and (iii) the first principal component of the three items, rescaled to lie between 0 and 1.

To assess whether the ISSP measures capture a similar perception of economic relations to the WVS zero-sum measure, we examine how each is related to political ideology. This provides a common benchmark across the two surveys. If the ISSP questions on conflict between economic groups are capturing a concept similar to zero-sum thinking, respondents who score higher on the ISSP measures should display the same ideological pattern as respondents who score higher on the WVS measure. Appendix Figure A.1 shows that this is the case. In the WVS, respondents with stronger zero-sum beliefs place themselves further to the left on the 10-point left-right ideological scale. In the ISSP, respondents who perceive greater conflict between economic groups are also more likely to support parties further to the left. The fact that both measures are related to political ideology in the same direction supports our interpretation of the ISSP conflict questions as complementary measures of a zero-sum view of economic relations.

Appendix Figure A.2 further shows that stronger *zero-sum thinking* predicts greater support for income equality and government responsibility, as well as more restrictive attitudes towards immigration and immigrant neighbours. These relationships are positive, precisely estimated, and stable across the four specifications used in the main analysis, consistent with Chinoy et al. (2026).

As in the WVS, we use respondents’ country of residence and year of birth to link them to the exposure measure, together with the available demographic variables as individual-level controls.

2.2 Populism data, treatment construction, and identifying variation

We measure exposure to populism using annual country-level data from Funke et al. (2023). Their dataset covers 60 countries from 1900 to 2020, together accounting for more than 95% of world GDP. We use the updated version of the data, which codes each country-year with an indicator equal to one if the head of the central government is classified as populist and zero otherwise.

Leaders are classified as populist using the widely accepted political-science definition in Mudde (2007). A leader is coded as populist only when anti-elite, people-centred rhetoric clearly dominates both the campaign and the time in office. The coding therefore applies a deliberately high threshold to limit false positives. In the version of these data we use, a populist headed the central government in 28 of the 60 countries at some point between 1900 and 2020, across 58 distinct spells of populist rule that together cover 463 country-years.

We match the country-year populism data to individual survey records using respondents’ country of residence and year of birth. Following a vast literature on political socialisation, we focus on exposure during the “impressionable years” of early adulthood when individuals are especially likely to form durable political attitudes and worldviews (Newcomb, 1943; Krosnick and Alwin, 1989; Giuliano and Spilimbergo, 2025). For each respondent, we calculate the share of years between ages 18 and 25 spent under populist leadership in the survey country. The treatment variable ranges from 0, for individuals with no exposure during this window, to 1, for individuals exposed throughout all

eight years, and therefore varies at the country-by-birth-cohort level.³

We use the same approach to construct placebo and robustness measures for other life stages: pre-birth years, defined as ages -8 to -1 ; early childhood, ages 2 to 9 ; adolescence, ages 10 to 17 ; post-formative adulthood, ages 26 to 33 ; and mid-adulthood, ages 34 to 41 . These equal-length windows allow us to test whether the relationship is concentrated in the impressionable years of early adulthood or instead reflects exposure to populist rule at other points in the life cycle.⁴

Appendix Figure A.3 shows the timing and duration of populist rule across countries in the WVS estimation sample. The figure highlights substantial variation both across countries and over time. Some countries, such as Argentina, Ecuador, and Mexico, experienced multiple populist spells, while others, such as Japan, Poland, and South Korea, experienced only a single relatively short spell. WVS countries not shown in the figure have no populist spell that generates formative exposure for respondents in our estimation sample.⁵ Appendix Figure A.4 presents the analogous figure for the ISSP sample.

The treatment varies across birth cohorts within a country because different cohorts pass through ages 18 to 25 in different calendar years. Cohorts whose early adulthood years overlap more closely with a populist spell therefore receive greater exposure than cohorts whose early adulthood years fall before or after that spell. Appendix Figure A.5 shows the country-year variation underlying this measure. Panel A plots the timing of populist rule in each country. Panel B removes country and year fixed effects, isolating changes in populist rule within countries after removing persistent country differences and common year shocks. These changes in the timing of populist rule generate differences in formative exposure across birth cohorts.

Appendix Table A.2 summarises the resulting treatment variation across the 3,933 country-cohort cells in the baseline WVS sample. For overall populist exposure, 87.1% of cells have no exposure during ages 18 to 25 , 10.5% have partial exposure, and 2.4% have full exposure throughout the eight-year window. Thus, 12.9% of country-cohort cells have some exposure to populist rule, spread across 25 countries. Among these exposed cells, more than four-fifths are partially rather than fully exposed. The treatment therefore contains substantial variation in the intensity of exposure and is not simply a comparison between fully exposed and unexposed cohorts.

Appendix Figure A.6 shows how the identifying variation is distributed across countries after imposing the fixed effects and controls from our fully saturated specification. We residualise formative exposure with respect to country-by-survey-year, cohort, and age fixed effects and individual controls, and calculate each country's contribution to the remaining treatment variance. No single country accounts for more than 17% of this variation. The estimate therefore draws on differences in exposure across birth cohorts in a broad set of countries rather than being driven by one country or populist episode.

³Because we do not observe respondents' residence histories, we restrict the sample to individuals born in the country in which they were interviewed. This reduces measurement error from international migration, although some respondents may still have lived abroad during part of their early adulthood exposure window.

⁴Holding each window to the same eight-year length leaves ages 0 to 1 between the pre-birth and early-childhood windows. As a robustness check, assigning ages 0 to 1 to the early-childhood window, so that it spans ages 0 to 9 , leaves the timing pattern unchanged: the early-childhood coefficient remains small and statistically insignificant, while the coefficient on ages 18 to 25 is essentially unaffected.

⁵For example, France is included in the WVS sample but has no populist leader spell. The United States and the United Kingdom experienced populist rule in the late 2010s, but the timing of WVS interviews means that no surveyed cohorts were exposed during ages 18 to 25 in our WVS estimation sample.

2.3 Additional datasets

We use several additional datasets for robustness checks and mechanism analysis. To account for conditions prevailing during respondents’ impressionable years (ages 18 to 25), we use measures of democracy (Boix et al., 2013), political instability (Banks and Wilson, 2024), and GDP per capita from the Maddison Project (Bolt and Van Zanden, 2025). The political-instability measures include assassinations, general strikes, terrorism and guerrilla warfare, major government crises, purges, riots, revolutions, and anti-government demonstrations. We also use leader-level data on biographical, educational, career, and ideological characteristics (Baturu, 2016; Herre, 2023).

For the economic-scarcity channel, we use the Maddison data to measure economic conditions during the exposure years of early adulthood. We also examine other correlates of zero-sum thinking highlighted in the literature, including income concentration, immigration background, and race.

To study the rhetorical channel, we use two measures of populist rhetoric. First, the V-Party dataset (Lindberg et al., 2022) provides expert-coded measures of party rhetoric at the party-election level. Its populism index combines two questions: *“How important is anti-elite rhetoric for this party?”* and *“Do leaders of this party glorify the ordinary people and identify themselves as part of them?”* We use the overall index and each component separately, assigning to each country-year the scores of the party of the head of government. Second, we construct text-based measures from speeches delivered at the annual United Nations General Debate. We score all 11,140 speeches delivered between 1946 and 2025, covering roughly 200 states, using a large language model that applies the holistic grading rubric of Team Populism (Hawkins, 2009). The measures capture anti-elitism, people-centrism, and zero-sum framing, while distinguishing rhetoric directed at a domestic elite from grievance directed at foreign powers. We aggregate the scores to the country-year level and standardise them within calendar year.⁶

We examine two further implications of the rhetorical channel. First, we ask whether formative exposure is associated with lower trust in epistemic authorities. For this analysis, we use the 2018 Wellcome Global Monitor, which measures trust in scientists, science, and university scientists, alongside trust in national government, doctors and nurses, hospitals, non-governmental organisations, traditional healers, and neighbours. Second, we examine whether the association is stronger where political messages can reach a wider audience or are more publicly contested. We measure media reach using radio and television receivers per capita and newspaper circulation per capita (Banks and Wilson, 2024). We measure institutional constraints and media freedom using V-Dem indices of legislative and judicial constraints on the executive, liberal democracy, media bias against the opposition, and government censorship of the media (Coppedge et al., 2025). We average each of these measures over ages 10 to 17, the eight years immediately preceding the formative exposure window. Measuring the moderators before ages 18 to 25 avoids conditioning on institutional or media changes occurring during the measured exposure period.

Finally, to study later political preferences, we match respondents’ stated vote intentions to party classifications from the PopuList (Rooduijn et al., 2023, 2024), which identifies populist, far-left, and far-right parties in Europe from 1989 onwards.

⁶Appendix B documents the corpus, scoring model and rubric, and validation of the resulting measures. It also reports a conventional dictionary-based benchmark, which recovers people-centrism but cannot separate domestic anti-elite rhetoric from grievance against foreign powers and colonial legacies, and which we therefore use only for comparison.

3 Empirical strategy

3.1 Estimating equation

To examine the relationship between exposure to populism and zero-sum thinking, we estimate the following specification for respondents aged 26 or older, who have completed the formative-years window:

$$\text{ZeroSum}_{icbt} = \beta \text{Exposure to populism}_{cb}^{18-25} + \theta_{ct} + \delta_b + \lambda_{a(i)} + X_i' \Gamma + \varepsilon_{icbt}. \quad (1)$$

Here, ZeroSum_{icbt} is our measure of zero-sum thinking for individual i , residing in country c , born in cohort b , and surveyed in year t . As described above, the measure captures the belief that economic success comes at the expense of others and is coded so that higher values indicate stronger zero-sum beliefs.

Our treatment variable, $\text{Exposure to populism}_{cb}^{18-25}$, measures the share of ages 18 to 25 that respondent i , from country c and birth cohort b , spent under populist rule. Using the annual country-level data from [Funke et al. \(2023\)](#), we calculate the fraction of these eight years in which the survey country was governed by a populist leader. The measure therefore varies at the country-cohort level and ranges from zero, for no exposure, to one, for exposure throughout all eight years. The coefficient β therefore measures the difference in zero-sum thinking later in life associated with moving from no exposure to full exposure to populist rule during ages 18 to 25.

We include country-by-survey-year fixed effects (θ_{ct}), which absorb all factors common to respondents interviewed in the same country and year. These include persistent country characteristics, such as historical legacies and social norms, as well as country-specific conditions at the time of the survey, including economic conditions, political events, institutions, and whether a populist is currently in power. In the less restrictive specifications, where country and survey-year fixed effects are included separately, we therefore additionally control for populist rule at the time of the survey.

We include birth-cohort fixed effects (δ_b) to absorb differences across cohorts and single-year-of-age fixed effects ($\lambda_{a(i)}$) to account for life-cycle patterns in zero-sum beliefs.⁷ The vector of individual controls, X_i , includes gender, education, a dummy variable for having children, and within-country-year income deciles.⁸

Unless otherwise noted, we restrict the sample to respondents born in the country in which they were interviewed and cluster standard errors at the country-cohort level. We use the same fixed-effect structure across datasets, together with the available individual-level controls and survey weights.

3.2 Identification

Identification comes from differences in formative exposure across birth cohorts interviewed in the same country and survey year. The timing of populist spells means that cohorts born only a few

⁷Age, birth cohort, and survey year are mechanically related, so their fixed effects are subject to the usual age-period-cohort normalisation. This normalisation does not affect the coefficient of interest: β is identified from country-specific differences in cohort exposure after partialling out common age and cohort patterns and country-by-survey-year effects.

⁸Education, having children, and current income may themselves be affected by earlier political exposure. Appendix Table A.3 therefore shows that the estimate is similar when these potentially endogenous controls are excluded and only gender is retained, as well as when alternative individual controls are added.

years apart can experience different amounts of populist rule between ages 18 and 25. For example, in the 2019 ISSP, US respondents born in 1993 were partly exposed to Donald Trump’s presidency during ages 18 to 25, whereas respondents born in 1985 were not exposed to a populist government during the same formative window. The comparison is therefore between cohorts observed in the United States in the same survey year, net of common cohort and age patterns.

The identifying assumption is that, conditional on the fixed effects and observed controls, country-specific differences across cohorts in exposure to populist rule during ages 18 to 25 are unrelated to other unobserved factors that independently shape later zero-sum beliefs:

$$E [\varepsilon_{icbt} \mid \text{Exposure to populism}_{cb}^{18-25}, \theta_{ct}, \delta_b, \lambda_{a(i)}, X_i] = 0. \quad (2)$$

This assumption would be violated if cohorts with greater exposure to populist rule during ages 18 to 25 also experienced other shocks that independently affected their later zero-sum beliefs. We address this possibility in several ways.

First, birth-cohort fixed effects absorb differences that are common to people born in the same year across countries. For example, people born in 1950 may have been exposed to global events, technological change, or broader generational shifts that differ from those experienced by people born in 1960. These common cohort differences are absorbed by δ_b . Age fixed effects separately account for systematic differences associated with respondents’ age at the time of the survey. For example, respondents interviewed at age 35 may differ from those interviewed at age 55 in their political attitudes or economic beliefs simply because they are at different stages of the life cycle.

Second, country-by-survey-year fixed effects absorb all country-level conditions at the time of the survey. These include, for example, inflation, unemployment, GDP growth, national political events, institutional changes, and the government in office. Thus, respondents interviewed in the same country and survey year are compared under the same national economic and political environment. Identification then comes from differences across birth cohorts within that country and survey year, arising from their different exposure to populist rule between ages 18 and 25.

Third, formative exposure to populist rule may be correlated with other economic, political, or social conditions experienced during the same years that independently shape later zero-sum beliefs. To account for potential confounders, we include a rich set of conditions measured over ages 18 to 25, including GDP per capita, democracy, and several measures of political instability. The results remain largely unchanged when these factors are included.

Fourth, ages 18 to 25 also coincide with entry into the electorate, raising a separate reverse-causality concern: cohorts with unusually strong pre-existing grievances could help elect the populist leader whose tenure subsequently contributes to their measured exposure. To address this concern, we split formative exposure into two components. The first captures exposure to populists who were already in office when the respondent turned 18. The second captures exposure to populists who took office during ages 19 to 25. Exposure to leaders already in office at age 18 cannot be explained by the cohort’s own political choices during ages 18 to 25. The inherited component remains positive and statistically significant across specifications, and equality between the inherited and acceded coefficients is never rejected.

Finally, the impressionable-years interpretation implies that the relationship should be concentrated around early adulthood rather than appearing similarly throughout the life cycle. We there-

fore construct comparable exposure measures before birth, during childhood and adolescence, and later in adulthood. We also estimate a sequence of rolling eight-year exposure windows approaching ages 18 to 25. The estimates are small and statistically insignificant for the earlier placebo windows, become larger as the window approaches early adulthood, and peak at ages 18 to 25. Exposure in the later adult windows shows no comparable precisely estimated association. These exercises help distinguish formative exposure from a more general relationship between lifetime exposure to populist rule and zero-sum beliefs.

4 Results

4.1 Main results

We begin with graphical evidence. Figure 1 plots the relationship between formative exposure to populism and zero-sum thinking under the same sequence of specifications reported in Table 1. In each panel, both variables are residualised using the fixed effects and controls included in the corresponding regression. Panel (a) controls for country, survey-year, and age fixed effects. Panel (b) adds individual controls, and Panel (c) adds birth-cohort fixed effects. Panel (d) corresponds to our preferred specification and replaces the separate country and survey-year fixed effects with country-by-survey-year fixed effects. In panels (a) through (c), we also control for whether a populist is in power at the time of the survey, as in columns (1) through (3) of Table 1. Across all four panels, cohorts with greater exposure to populist rule between ages 18 and 25 display stronger zero-sum thinking later in life.

Table 1 reports the corresponding regression estimates using the World Values Survey. The four columns follow the same progression as Figure 1, with column (4) representing our fully saturated specification. The coefficient on exposure to populism is positive and statistically significant across all four specifications. In column (4), full exposure during ages 18 to 25, relative to no exposure, is associated with a 0.092-point increase in zero-sum thinking on the 1 to 10 scale, or 0.034 standard deviations.

To put the magnitude of the estimate in perspective, we compare it with differences in average zero-sum thinking across countries. In the WVS, countries at the 25th and 75th percentiles differ by 0.96 points on the 1 to 10 scale. Full formative exposure is associated with a 0.092-point increase, equal to about 10% of this difference. As a second benchmark, Chinoy et al. (2026) report a gap between the United States and Türkiye equivalent to 0.468 points on our scale. Our estimate is about 20% of that gap. These benchmarks show that the estimated shift in zero-sum thinking is meaningful relative to observed differences across countries.⁹

4.2 Evidence from the ISSP

We next test whether the main result also appears in a different survey using alternative measures of zero-sum economic conflict. The WVS measure is based on a single question asking whether one person’s economic gains come at the expense of others. The International Social Survey Programme

⁹Chinoy et al. (2026) report country means of 0.401 for the United States and 0.453 for Türkiye on a 0 to 1 scale. The difference of 0.052 corresponds to $0.052 \times 9 = 0.468$ points on our 1 to 10 scale, so $0.092/0.468 = 0.197$, or about 20% of the US–Türkiye gap. In our WVS sample, the 25th and 75th percentiles of country-average zero-sum thinking are 4.17 and 5.13, respectively, a difference of 0.96 points; $0.092/0.96 = 0.096$, or about 10%.

(ISSP) instead asks respondents about the extent of conflict between different economic groups. This provides an independent test using different questions and a different survey.

Table 2 reports estimates of Equation 1 for three measures constructed from questions about conflict between poor and rich people, the working and middle classes, and management and workers. Panel A uses the average response across the three questions. Panel B measures the share of the three pairs for which respondents report strong or very strong conflict. Panel C uses the first principal component of the three questions, rescaled to range from 0 to 1. In all three cases, higher values indicate greater perceived conflict between economic groups.

The four columns follow the same sequence of specifications as Table 1. Column (1) includes country, survey-year, and age fixed effects; column (2) adds individual controls; column (3) adds birth-cohort fixed effects; and column (4) replaces the separate country and survey-year fixed effects with country $\times$ survey-year fixed effects.

The results show the same qualitative pattern as the WVS findings. Formative exposure to populism is positively related to perceived economic conflict across all three measures and all four specifications. In column (4), full exposure during ages 18 to 25 is associated with a 0.056-point increase in the mean conflict measure, a 0.029-point increase in the binary conflict measure, and a 0.017-point increase in the PCA measure. Relative to the standard deviation of each outcome, these estimates are very similar in magnitude, ranging from 0.076 to 0.083 standard deviations, and all are statistically significant at the 1 percent level.¹⁰ The consistent pattern across the ISSP and WVS provides independent validation of our main finding and shows that the relationship extends beyond a single survey item to a broader measure of perceived economic conflict.

For the remainder of the analysis, we return to the WVS because it provides broader country coverage for our main outcome and is better suited to the robustness and mechanism exercises that follow.

4.3 Persistence and timing of exposure

Persistence across respondent age. We next ask whether the relationship between formative populist exposure and zero-sum thinking remains visible as respondents get older. This provides a simple way to assess how long the estimated difference persists after the exposure period. We interact formative exposure with respondents' age at the time of the survey and plot the implied effect at each age in Figure 2. The estimated effect is largest for respondents observed soon after age 25. At age 26, full formative exposure is associated with an increase in zero-sum thinking of about 0.20 points. The estimate then declines with age and remains statistically distinguishable from zero until roughly the mid-forties. Beyond that point, the confidence intervals include zero and become progressively wider. Because the data are repeated cross-sections rather than a panel, this exercise does not track the same individuals over time. It instead shows that the relationship remains detectable among respondents surveyed up to roughly two decades after the formative exposure period.

¹⁰ Appendix Table A.4 reports results for each of the three underlying conflict questions separately. The coefficient on formative populist exposure is positive for perceived conflict between poor and rich people, between the working and middle classes, and between management and workers, and is statistically significant at the 5 percent level or better in all but one specification. The aggregate results are therefore not driven by any single component.

Timing of exposure over the life cycle. We next examine whether ages 18 to 25 are a distinct period for the relationship between populist exposure and later zero-sum thinking. This exercise addresses two concerns. First, cohorts with greater exposure during ages 18 to 25 may already have differed from other cohorts before the exposure period. If so, exposure measured before the respondent was born could also predict later zero-sum thinking. Second, populist exposure may matter similarly at any age rather than specifically during early adulthood. If that were the case, we should observe comparable estimates for exposure earlier or later in life.

To test these possibilities, we construct exposure to populist rule over six eight-year windows spanning different stages of the life cycle: pre-birth (ages -8 to -1), early childhood (ages 2 to 9), adolescence (ages 10 to 17), the impressionable years (ages 18 to 25), post-formative adulthood (ages 26 to 33), and mid-adulthood (ages 34 to 41). Figure 3 Panel (a) includes all six exposure measures in the same regression.¹¹ Exposure before birth, during childhood, and during adolescence has little relationship with later zero-sum thinking. The estimate is largest and most precisely estimated for exposure during ages 18 to 25, while the estimates for later adult exposure are smaller and not statistically significant. The near-zero pre-birth estimate is particularly informative because it shows that populist political history preceding the respondent’s own life does not predict later zero-sum thinking in the same way.

Appendix Figure A.7 estimates each life-cycle window separately and yields the same pattern. Exposure during ages 18 to 25 again produces the largest and only statistically significant estimate. For the later windows, we restrict the sample to respondents old enough to have completed the relevant exposure period, using respondents aged 34 or older for ages 26 to 33 and aged 42 or older for ages 34 to 41.

Figure 3 Panel (b) provides a finer test of timing. We estimate exposure over rolling eight-year windows, beginning with ages 10 to 17 and moving the window forward one year at a time until ages 18 to 25. The estimate is close to zero for the earliest window and rises steadily as the window moves towards early adulthood, reaching its largest value at ages 18 to 25. Taken together, the two exercises show that the relationship is concentrated around early adulthood rather than reflecting pre-existing cohort differences or exposure to populist rule at other stages of life.

4.4 Robustness

Placebo tests using unrelated attitudes. A first concern is that exposure to populism during early adulthood may proxy for broader generational or cultural shifts rather than a specific change in zero-sum thinking. To assess this, we re-estimate Equation 1 using a battery of economic, social, and cultural attitudes as dependent variables. If the baseline estimates were capturing broad changes in attitudes among more exposed cohorts, we would expect systematic relationships with these

¹¹Because a populist spell can span more than one of these windows, the six exposure measures are not orthogonal, and we therefore report collinearity diagnostics for the Panel (a) estimating sample. Raw pairwise correlations between adjacent windows range from 0.32 to 0.42 and are close to zero for non-adjacent pairs. After partialling out the baseline country-by-survey-year, cohort, and age fixed effects and individual controls, no residual pairwise correlation exceeds 0.22 in absolute value. Variance inflation factors range from 1.06 to 1.20 when computed among the residualised windows and from 1.69 to 1.92 under the conventional definition that includes the fixed effects in the auxiliary regression, well below standard thresholds. Each window retains between 73 and 81 percent of its raw standard deviation after residualisation. These diagnostics indicate that the timing pattern in Panel (a) is not driven by severe collinearity across adjacent exposure windows.

placebo outcomes as well. Figure 4 shows no such pattern. Formative exposure to populism is not systematically associated with this broader set of attitudes, providing evidence that the baseline result reflects a more specific change in zero-sum beliefs rather than a general shift in respondents' reported attitudes.

Other formative-year shocks and leader characteristics. Populist leaders often come to power during periods of institutional stress, economic disruption, or political upheaval, and they may differ from other leaders in ways unrelated to populism. We address these concerns in three ways. First, Appendix Table A.5 augments the baseline specification with a battery of country-level conditions measured during each respondent's exposure window (ages 18 to 25): exposure to democracy (Boix et al., 2013); assassinations, general strikes, acts of terrorism, major government crises, purges, riots, revolutions, and anti-government demonstrations (Banks and Wilson, 2024); and GDP per capita from the Maddison Project (Bolt and Van Zanden, 2025). Second, drawing on the Cursus Honorum database (Baturu, 2016), Appendix Table A.6 controls for formative-years exposure to thirteen leader attributes, including education, career background, prior imprisonment, revolutionary experience, and interim status, entered both individually and jointly.¹² Third, using the Global Leader Ideologies dataset (Herre, 2023), Appendix Table A.7 controls jointly for the fraction of ages 18 to 25 spent under left-wing, right-wing, and centrist leaders.

Across these exercises, the coefficient on formative exposure to populism remains positive and close to the baseline estimate, while none of the leader-ideology indicators enters significantly. The results therefore provide little evidence that the main result is explained by the broader environment in which populists come to power, the observable characteristics of the leaders to whom cohorts were exposed, or the general left-right orientation of the government.

Sample, covariates, migration, and reverse causality. A second set of checks concerns the estimation sample and individual controls. The estimate remains similar when potentially endogenous individual controls are dropped and when town size, religion, and marital status are added (Appendix Table A.3); when the sample is restricted to post-1945 birth cohorts, where it is slightly larger (Appendix Table A.8); under unweighted estimation and the WVS post-stratification weight (Appendix Table A.9); and when we control for proxies of parental influence, namely living with one's parents and valuing parental approval (Appendix Table A.10).

We also examine selective migration. Assigning formative exposure using respondents' countries of birth, exposure to populism does not predict whether an individual appears as a migrant in the WVS sample in any specification (Appendix Table A.11). This provides little evidence that selective migration explains the results.

Finally, ages 18 to 25 are also when cohorts first enter the electorate, raising a possible reverse-causality concern: cohorts with unusually strong pre-existing grievances could help elect the populist whose subsequent tenure contributes to their measured exposure. We therefore split formative exposure into an *inherited* component, accumulated under a populist already in office when the respondent turned 18, and an *acceded* component, accumulated under a populist who took office during ages 19 to 25. The inherited component cannot reflect the cohort's own role in initially

¹²We collapse the leader-level data to a country-year panel by retaining, in each country-year, the leader who served the greatest number of months during that year, and construct exposure using the same ages 18 to 25 averaging as for the baseline treatment.

bringing that populist to power. Its coefficient is positive and significant in all four specifications, reaching 0.104 in the preferred specification, while equality between the inherited and acceded coefficients is never rejected (Appendix Table A.12). This pattern reduces the concern that the baseline association is driven by cohorts helping to elect the populists to whom they are subsequently exposed.

Inference and influential observations. A final set of checks examines whether the results depend on particular cohorts, country-waves, or inference choices. Re-estimating the four baseline specifications while sequentially dropping each birth cohort and each country-wave leaves the point estimates positive and tightly clustered around their baseline values (Appendix Figures A.8 and A.9). The conclusions are also unchanged under alternative clustering schemes, including clustering by country $\times$ year, age $\times$ cohort, cohort, and country $\times$ age (Appendix Figure A.10).

We additionally conduct permutation inference that respects the country-cohort structure of the treatment. We permute exposure at the country $\times$ birth-year block level, first across the full sample and then within countries, using 500 permutations for each scheme and specification. In both exercises, the observed t-statistic is extreme relative to the permutation distribution. The resulting permutation p-values, defined as the share of permuted t-statistics at least as extreme as the observed estimate, are close to zero in the three less demanding specifications and remain below 5 percent in the preferred country-by-survey-year specification (Appendix Figures A.11 and A.12). Taken together, these exercises indicate that the main inference is not sensitive to influential observations, alternative clustering schemes, or permutation-based inference.

5 Mechanisms

The main results show that formative exposure to populist rule is associated with stronger zero-sum thinking later in life. This section asks why. We organise the evidence around two broad channels.

The first is an *economic-scarcity* channel. Populist governments often underperform non-populist governments across a range of economic outcomes (Funke et al., 2023). Economic conditions experienced early in life can also leave durable marks on beliefs and preferences (Cotofan et al., 2023), including zero-sum thinking (Chinoy et al., 2026). If populist rule worsens material conditions during early adulthood, exposed cohorts may come to see society as more competitive and zero-sum, even if populist rhetoric plays no independent role.

The second is a *rhetorical* channel. Populists depict public life as a struggle between “the people” and a self-serving elite. In that frame, one side’s gains come at the expense of the other. Repeated exposure to this message during the impressionable years may lead individuals to interpret social and economic life through a more zero-sum lens, above and beyond the economic conditions they experience. The evidence below is not designed to identify a unique mechanism. Instead, we ask whether the patterns are more consistent with an economic-scarcity account or with a rhetorical channel.

5.1 Economic scarcity

We begin with the most direct alternative explanation. If formative exposure to populist rule affects zero-sum beliefs mainly through economic hardship, then the relationship should weaken

once economic conditions are held fixed, and exposed respondents should also report worse economic well-being. We find little support for either prediction.

Figure 5 summarises the main evidence. We first control for macroeconomic conditions during respondents' impressionable years. If worsening economic conditions were driving the result, the coefficient on populist exposure should fall once these conditions are taken into account. It does not. Controlling for GDP per capita and a broader set of political and economic conditions experienced during ages 18 to 25 leaves the coefficient positive and similar in magnitude (Appendix Table A.5).

We next examine subjective economic well-being. If perceived scarcity were an important channel, exposed respondents should report lower satisfaction with their financial situation. We find no such pattern. Financial satisfaction is one of the placebo outcomes in Figure 4 and is not systematically related to formative populist exposure. This evidence is necessarily suggestive because financial satisfaction is measured at the time of the survey rather than during early adulthood, but it provides little indication of persistent differences in perceived economic well-being.

Finally, we examine whether populist exposure is capturing broader economic conditions already known to be related to zero-sum beliefs. Chinoy et al. (2026) highlight income inequality, immigration, and the historical legacy of enslavement as important correlates of zero-sum thinking. We therefore control for the country's pre-tax top 1% income share during ages 18 to 25, indicators for whether the respondent's mother or father was born abroad, and, in the United States, an indicator for Black respondents. Across these exercises, the coefficient on formative populist exposure remains positive, statistically significant, and similar in magnitude to the baseline estimate (Appendix Table A.13).

These initial tests show that observable economic conditions and individual economic circumstances do not explain the main result. We next use more direct tests of whether economic hardship itself can account for the relationship.

5.1.1 Economic hardship without populist rule

We next ask whether economic hardship on its own predicts stronger zero-sum thinking. To do so, we construct a measure of the share of ages 18 to 25 in which GDP per capita declined (Bolt and Van Zanden, 2025) and no populist was in office. This measure is constructed in the same way as our populist-exposure variable. If economic hardship were the main mechanism, greater exposure to these non-populist downturns should also be associated with stronger zero-sum beliefs later in life.

We find no evidence of this. In column (4), the coefficient on non-populist recession exposure is -0.039 and statistically insignificant (Figure 5; Appendix Table A.14, Panel A). When we include populist exposure and non-populist recession exposure together, the coefficient on populist exposure remains positive at 0.083 , while the coefficient on non-populist recession exposure is close to zero at -0.023 (Panel B). Thus, exposure to economic downturns without populist rule does not produce the same relationship with later zero-sum thinking.

5.1.2 Populist rule in good and bad economic conditions

We also ask whether the main result is driven specifically by exposure to populist rule during periods of economic hardship. We split formative populist exposure according to whether the

populist governed during a recession or outside one. If economic scarcity were the main channel, exposure during recessions should have the larger effect on later zero-sum thinking.

The point estimates show the opposite pattern. In column (4), exposure to populist rule outside recessions is associated with a 0.134-point increase in zero-sum thinking, while the estimate for exposure during recessions is negative and imprecisely estimated at -0.056 . We obtain the same pattern when we classify economic conditions using positive versus negative GDP-per-capita growth. The corresponding estimates are 0.137 for exposure during positive-growth years and -0.046 for exposure during negative-growth years (Figure 5; Appendix Tables A.14, Panel C, and A.15). The same ordering also appears in the less restrictive specifications, where the estimates for exposure during good economic conditions range from 0.268 to 0.329 and are statistically significant at the 1 percent level.

This pattern is difficult to reconcile with a simple scarcity mechanism. If worsening economic conditions were driving the result, the effect should be strongest when populist rule coincides with economic downturns. Instead, the positive association is concentrated in periods when the economy is not contracting.

5.1.3 Institutional constraints and scope for policy implementation

We then use institutional constraints to test whether the economic consequences of populist rule can explain the main result. The logic is straightforward. Populist leaders who face fewer institutional constraints have greater scope to implement their preferred policies and influence economic outcomes. If changes in policy or economic performance were the main channel, the relationship between populist exposure and zero-sum thinking should therefore be stronger when institutional constraints are weak.

We measure the institutional environment using three V-Dem indices: legislative constraints on the executive, judicial constraints on the executive, and the liberal democracy index (Coppedge et al., 2025). For each respondent, we average each moderator over ages 10 to 17, the eight years immediately preceding the formative exposure window, and distinguish between values above and below the sample median.

To formalise this comparison, let EtP_{cb}^{18-25} denote formative exposure to populism and let Con_{cb} equal one when the average level of institutional constraints experienced by cohort b in country c over ages 10 to 17 is above the sample median. We estimate:

$$\begin{aligned} \text{ZeroSum}_{icbt} = & \beta_1 \text{EtP}_{cb}^{18-25} + \beta_2 \text{Con}_{cb} + \beta_3 (\text{EtP}_{cb}^{18-25} \times \text{Con}_{cb}) \\ & + \theta_{ct} + \delta_b + \lambda_{a(i)} + X_i' \Gamma + \varepsilon_{icbt}. \end{aligned} \quad (3)$$

The fixed effects and controls are the same as in the baseline specification. β_1 gives the estimate when constraints are low, while $\beta_1 + \beta_3$ gives the estimate when constraints are high.

The top row of Figure 6 shows a clear pattern. Across all three measures, the estimates are larger when populist leaders face stronger institutional constraints. In high-constraint settings, full formative exposure is associated with an increase in zero-sum thinking of roughly 0.14 to 0.16 points, with all estimates statistically distinguishable from zero. In low-constraint settings, where populist leaders have greater freedom to implement their policies, the estimates are small, negative, and statistically insignificant.

This is the opposite of what a simple economic-policy channel would predict. If populist exposure shaped beliefs mainly through changes in policy or economic performance, the relationship should be strongest where leaders have the greatest freedom to implement their agenda. Instead, it is strongest where their ability to do so is more constrained.

This pattern also points towards the rhetorical channel examined next. Strong institutional constraints can increase conflict between populist leaders and the courts, legislatures, experts, and other institutions that limit their agenda. Populists may portray these actors as obstructing the will of the people (Acemoglu et al., 2013), making people-versus-elite rhetoric more salient. We turn to direct evidence on this rhetorical channel below.

5.2 Rhetorical channel

5.2.1 Anti-elite narrative

We now turn to a rhetorical channel. The key idea is that populist leaders repeatedly frame politics as a conflict between “the people” and a self-serving elite. If exposure to this rhetoric shapes how citizens interpret economic and social relations, populist periods should feature more anti-elite and zero-sum language. We examine this prediction using two complementary measures of political rhetoric.

Populism has two central components in the definition we use. People-centrism presents “the people” as a homogeneous and virtuous group, while anti-elitism portrays a corrupt elite as acting against their interests (Mudde, 2007). Anti-elitism is especially relevant for our mechanism because it presents politics as a conflict between opposing groups, in which the power or gains of the elite come at the expense of the people. We therefore examine whether political rhetoric becomes more anti-elite and people-versus-elite during periods of populist rule.

Our first measure comes from V-Party. Figure 7 (upper panel) compares the rhetoric of governing parties in populist and non-populist country-years, controlling for country and year fixed effects. V-Party provides expert-coded measures of governing-party populism and its two components, anti-elitism and people-centrism. Populist country-years are associated with a large shift in rhetoric. The governing-party populism score is more than one standard deviation higher, while anti-elitism rises by 1.14 standard deviations and people-centrism by 0.82 standard deviations (Appendix Table A.16). The larger increase in anti-elitism is particularly relevant for our argument because this component most directly frames politics as a conflict between the people and an opposing elite (Hawkins, 2009).

Our second measure uses the actual language of political leaders. We analyse every speech delivered at the annual United Nations General Debate between 1946 and 2025, covering more than 11,000 addresses and over 32 million words from roughly 200 states. We score each speech with a large language model using a rubric based on the Team Populism framework, which separately measures anti-elitism, people-centrism, domestic zero-sum framing, and grievances directed at foreign powers. The distinction between domestic and international conflict is central to our mechanism. Our argument concerns rhetoric that portrays politics as a struggle between ordinary citizens and a domestic elite, not conflict between countries.

The UN corpus is especially useful for this purpose because it provides a common institutional setting with broad country coverage over a long period, allowing us to compare the rhetoric used under populist and non-populist rule on a consistent basis. We do not interpret these speeches

as messages that survey respondents necessarily observed. Rather, we use them as a standardised measure of the rhetorical environment associated with populist rule. Appendix B describes the corpus, scoring procedure, and validation exercises in detail.

The lower panel of Figure 7 shows that, among the country-years for which the Funke et al. (2023) classification is available, speeches delivered by populist leaders contain substantially more of this language. Anti-elite content is 0.58 standard deviations higher, people-centrism is 0.51 standard deviations higher, and domestic zero-sum framing is 0.67 standard deviations higher. All three estimates are statistically significant at the 1 percent level. Grievances directed at foreign powers also increase, but by a smaller amount: 0.37 standard deviations (Appendix Figure A.13 plots the full distributions, and Appendix Table A.17 reports the regression estimates).

Taken together, the two measures show that periods of populist rule are characterised by a distinctly more conflictual rhetorical environment. Governing parties use substantially more anti-elite language, and populist leaders themselves use more domestic zero-sum framing. This is the first piece of evidence for the rhetorical channel: the periods in which respondents are exposed to populist rule are also periods in which political discourse more strongly presents society as a conflict between the people and an opposing elite.

5.2.2 Competing narratives and the information environment

The evidence above shows that populist periods feature more anti-elite and zero-sum rhetoric. We now examine why this rhetoric may shape beliefs. Zero-sum claims are often difficult for citizens to evaluate directly because questions such as whether trade, immigration, or economic growth benefit one group at the expense of another depend on information and expertise beyond personal experience. Citizens may therefore rely on scientists, academics, journalists, and other experts to assess these claims and provide competing interpretations. Recent theory argues that populists have incentives to discredit such actors, both because this weakens an important source of opposition to their narrative and because criticism from experts can itself be portrayed as evidence that an elite is acting against the people (Szeidl and Szucs, 2025; Ali et al., 2025). This mechanism gives us two empirical predictions, which organise the analysis that follows. We first ask whether formative exposure to populist rule is associated with lower trust in the experts best placed to challenge zero-sum claims. We then examine whether the relationship with zero-sum thinking is stronger where political messages and the public contestation surrounding them are more visible, using variation in media freedom and broadcast reach.

Trust in experts. We first test whether formative exposure to populist rule is associated with lower trust in scientific experts. This speaks directly to the rhetorical channel because experts can provide information that challenges zero-sum claims. If populist rhetoric undermines their credibility, cohorts exposed during their impressionable years should place less trust in them later in life. We test this prediction using the 2018 Wellcome Global Monitor, focusing on countries represented in our WVS estimation sample for comparability with the main analysis.¹³ We construct formative exposure in the same way as in the main analysis and compare birth cohorts within countries.

¹³Because the Wellcome Global Monitor was fielded in a single year, age and birth cohort are mechanically collinear. We therefore include country and birth-cohort fixed effects together with the individual controls, cluster standard errors at the country-cohort level, and use the survey’s population-projection weights.

The top panel of Figure 8 shows that full formative exposure is associated with lower trust across all six measures related to science and scientists. Trust that scientists produce accurate information falls by about 0.31 standard deviations, trust that scientists’ work benefits many people by 0.28, general trust in science by 0.24, trust that university scientists benefit the public by 0.22, trust in scientists by 0.19, and trust that university scientists are honest by 0.15. A summary index of the six measures falls by about 0.31 standard deviations, and every estimate is statistically significant at conventional levels.

Importantly, the pattern is specific to scientific expertise rather than reflecting a general decline in trust. The lower panel of Figure 8 shows no comparable relationship with trust in national government, doctors and nurses, hospitals, non-governmental organisations, traditional healers, or neighbours. This distinction is important for the mechanism. Citizens often cannot directly assess whether economic and social interactions are truly zero-sum and may rely on experts to explain why gains for one group need not imply losses for another. If populist rhetoric weakens trust in these sources, such counterarguments become less persuasive, leaving more room for zero-sum interpretations to take hold.

This pattern is consistent with the mechanism underlying our main result. If formative exposure to populist rule reduces trust in experts who can challenge zero-sum claims, those claims may carry greater weight, helping explain why exposed cohorts report stronger zero-sum beliefs later in life.

Media freedom. We next examine whether the relationship between formative populist exposure and later zero-sum thinking depends on the pre-formative media environment for each cohort. This provides a further test of the rhetorical channel. If populist rhetoric matters because it repeatedly frames politics as a conflict between the people and an opposing elite, its effect on beliefs should depend on how visible that political conflict is to citizens. Freer media make both populist claims and the responses of journalists, experts, opposition parties, and civil society more salient.

The direction of this relationship is not obvious *ex ante*. If greater access to competing information mainly helps citizens correct misleading or exaggerated populist claims, greater media freedom should weaken the relationship between populist exposure and later zero-sum thinking. But populists often present criticism from journalists, experts, and other institutions as further evidence that an entrenched elite is acting against the people. In that case, greater exposure to public disagreement may reinforce the very people-versus-elite conflict at the centre of populist rhetoric. The relationship with later zero-sum thinking could therefore be stronger, rather than weaker, where media are freer.

We measure the pre-formative media environment using two V-Dem indicators, media bias against the opposition and government censorship of print and broadcast media, averaged over ages 10 to 17. Lower values indicate greater media freedom.

The bottom row of Figure 6 shows a pronounced media gradient. When media bias or censorship is low, full formative exposure to populist rule is associated with an increase in zero-sum thinking of roughly 0.11 to 0.13 points, and the estimates are statistically significant. When media bias or censorship is high, the corresponding estimates are close to zero and imprecisely estimated. The main relationship is therefore concentrated in settings where citizens are more likely to observe both populist claims and the public conflict surrounding them. This pattern is consistent with a rhetorical mechanism in which visible confrontation between populists and the actors they portray as elites makes a conflict-based interpretation of politics more salient and, in turn, is associated

with stronger zero-sum beliefs later in life.

One concern is that media freedom may simply capture broader differences in institutional quality. We therefore allow the relationship between formative populist exposure and zero-sum thinking to vary jointly with media freedom and our measures of institutional constraints. We interact exposure with each constraint measure separately, with all three constraint measures simultaneously, and with quartiles of the liberal democracy index. Across these specifications, the media gradient remains. Where media are freer, the estimate for full formative exposure ranges from roughly 0.11 to 0.14 points, while under more biased or censored media it remains close to zero (Appendix Figures A.14 and A.15). The stronger relationship in freer media environments therefore does not appear to be explained simply by their broader institutional characteristics.

Broadcast reach. Finally, we ask whether the relationship between formative populist exposure and later zero-sum thinking is stronger where political messages can reach more people. This provides a complementary test of the rhetorical channel. If repeated exposure to populist narratives contributes to the formation of zero-sum beliefs, the relationship should be stronger in settings where mass communication gives those narratives a wider potential audience. Radio and television are particularly relevant because they allow political messages, as well as the public debate surrounding them, to reach large parts of the population repeatedly.

We measure pre-formative broadcast reach by averaging radio and television receivers per capita over ages 10 to 17, using data from the Cross-National Time-Series archive (Banks and Wilson, 2024). We combine these measures into a standardised index. The interaction between formative populist exposure and media reach is positive for radio, television, and the combined broadcast index. By contrast, the corresponding estimate for newspaper circulation is small and statistically insignificant (Appendix Table A.18).

Figure 9 shows that the relationship with zero-sum thinking changes little over much of the broadcast-reach distribution but becomes substantially larger where reach is highest. A tercile specification shows the same pattern. Full formative exposure is associated with an increase of about 0.21 points in zero-sum thinking in the top tercile of broadcast reach, compared with essentially zero in the lower two terciles (Appendix Figure A.16). The continuous interaction also remains positive in the country-by-survey-year specification (Appendix Table A.18).

This pattern is consistent with the idea that the reach of political communication matters for how formative populist exposure translates into later beliefs. Where radio and television reach a larger share of the population, populist messages and the political conflict they generate have greater scope to shape how citizens interpret economic and social relations. The substantially stronger relationship with zero-sum thinking in these settings therefore provides additional evidence consistent with a rhetorical channel.

5.3 Taking stock

Taken together, the evidence provides little support for a simple economic-scarcity channel. The main result remains similar after controlling for observable formative-period economic conditions; formative exposure is not associated with lower financial satisfaction at the time of the survey; and we find no positive association between exposure to non-populist recessions and later zero-sum thinking. The point estimates are also larger for populist exposure outside economic downturns

and in settings with stronger institutional constraints. None of these exercises alone identifies the mechanism, but their combined pattern is difficult to reconcile with economic hardship as the primary explanation.

The evidence is more consistent with a rhetorical channel. Years of populist rule feature substantially more anti-elite rhetoric in two independent measures, and speeches delivered by populist leaders contain more domestic zero-sum framing. This rhetoric presents economic and political relations as a conflict in which the gains of an elite come at the expense of ordinary people. At the same time, formative populist exposure is associated with lower trust specifically in science and scientists, who can provide information that challenges such zero-sum claims. If citizens place less weight on these competing sources of information, the zero-sum interpretation promoted by populist leaders may become more persuasive. Consistent with this mechanism, the relationship with later zero-sum thinking is also stronger where media freedom is greater and where radio and television reach a larger share of the population, giving populist narratives and the political conflict surrounding them greater scope to reach citizens. Overall, these results are consistent with a rhetorical and information channel in which repeated anti-elite and zero-sum rhetoric, weaker trust in sources that challenge such claims, and broader exposure to political communication contribute to stronger zero-sum beliefs later in life.

6 Political preferences

We finally ask whether the imprint of formative populist exposure extends beyond beliefs to later political preferences. In particular, we examine whether respondents who experienced populist rule during their impressionable years are more likely to support populist or ideologically extreme parties later in life. If formative political environments leave a persistent mark on how individuals view political conflict, they may also shape the types of parties they support. The analysis is limited to 21 European countries covered by the PopuList from 1989 onward. This substantially smaller sample reduces statistical power and therefore, if anything, makes persistent relationships with later political preferences harder to detect.

We use self-reported vote intentions from the WVS. Respondents are asked which party they would vote for if a national election were held tomorrow. We match these stated choices to expert-coded party classifications from the PopuList (Rooduijn et al., 2023, 2024). We distinguish four non-mutually exclusive groups: (i) populist parties, regardless of ideology; (ii) far-left parties; (iii) far-right parties; and (iv) parties on either the far left or far right.¹⁴ This distinction allows us to ask whether formative exposure predicts support for populism in general or instead leaves an ideological imprint reflecting the type of populist rule respondents experienced.

We estimate the fully saturated specification in column (4) of Table 1, replacing zero-sum thinking with each binary vote-choice outcome. We then separate formative exposure to left-wing and right-wing populist leaders to examine whether later party support follows the ideology of the populist government experienced during the impressionable years. Table 3 reports the results.

¹⁴The PopuList classifies European parties from 1989 onward. We can therefore match only WVS respondents who report a party they would vote for and whose stated party appears in the PopuList. Appendix Table A.19 reports the full country, survey-wave, and birth-cohort composition of the matched sample.

The first two columns show little evidence of a general shift towards populist parties, but some evidence of greater support for the ideological extremes. In column (1), the coefficient on support for a populist party is small, negative, and statistically insignificant. In column (2), by contrast, full formative exposure is associated with a 4.0 percentage-point increase in stated support for a far-left or far-right party. The estimate is statistically significant at the 10 percent level and is sizeable relative to the sample mean of 7.9 percent.

The ideology-specific estimates provide a more informative pattern. In column (3), formative exposure to a left-wing populist is associated with a 2.6 percentage-point increase in support for a far-left party. In column (4), exposure to a right-wing populist is associated with a 3.5 percentage-point increase in support for a far-right party. The corresponding cross-ideology estimates are small and statistically insignificant. Thus, the positive estimates are concentrated on the same side of the ideological spectrum as the populist government to which respondents were exposed during early adulthood.

Overall, the results do not suggest that formative populist exposure simply increases support for populist parties as a broad category. Instead, later support for parties at the ideological extremes aligns with the ideology of the populist rule experienced during early adulthood. This is consistent with formative political environments leaving a lasting imprint not only on how individuals interpret economic and social conflict, but also on their later political preferences.

7 Conclusion

We study whether exposure to populist rule during early adulthood, ages 18 to 25, is associated with stronger zero-sum beliefs later in life. Across more than 260,000 respondents in 53 countries, we find a clear positive relationship. In the WVS, full formative exposure is associated with a 0.092-point increase in zero-sum thinking, roughly one-fifth of the gap between the United States and Türkiye. The same pattern appears in the ISSP using three alternative measures of perceived economic conflict. The relationship is concentrated around early adulthood and remains detectable into respondents' mid-forties.

Several results make alternative explanations less likely. Exposure before birth, during childhood or adolescence, or later in adulthood shows no comparable relationship with zero-sum thinking. The main estimate is largely unchanged when we account for economic, political, and social conditions during the formative years and observable differences across populist leaders. We find no evidence that selective migration drives the result, and exposure to populists already in office when respondents turned 18 remains positively associated with zero-sum thinking, reducing concerns about reverse causality. Placebo tests also show no systematic relationship with a broader set of economic, social, and cultural attitudes.

The mechanism evidence points away from a simple economic-scarcity explanation and towards a rhetorical and information channel. Non-populist recessions do not predict stronger zero-sum thinking, while the association with populist rule is larger outside economic downturns and where leaders face stronger institutional constraints. Populist periods also feature substantially more anti-elite rhetoric and domestic zero-sum framing. Formative exposure is associated with lower trust in science and scientists, and the relationship with zero-sum thinking is stronger where media freedom is greater and broadcast reach is greater. This pattern is more consistent with the rhetorical and

information environment surrounding populist rule than with economic hardship alone.

We also find evidence that formative exposure extends to later political preferences. Exposure does not predict support for populist parties in general, but support for parties at the ideological extremes aligns with the ideology of the populist rule experienced during early adulthood.

More broadly, our findings suggest that political environments may leave lasting marks on beliefs. Governments influence policies and institutions, and exposure to the political environments they create is also associated with how citizens interpret economic and social relations. These differences can persist well beyond a government's time in office.

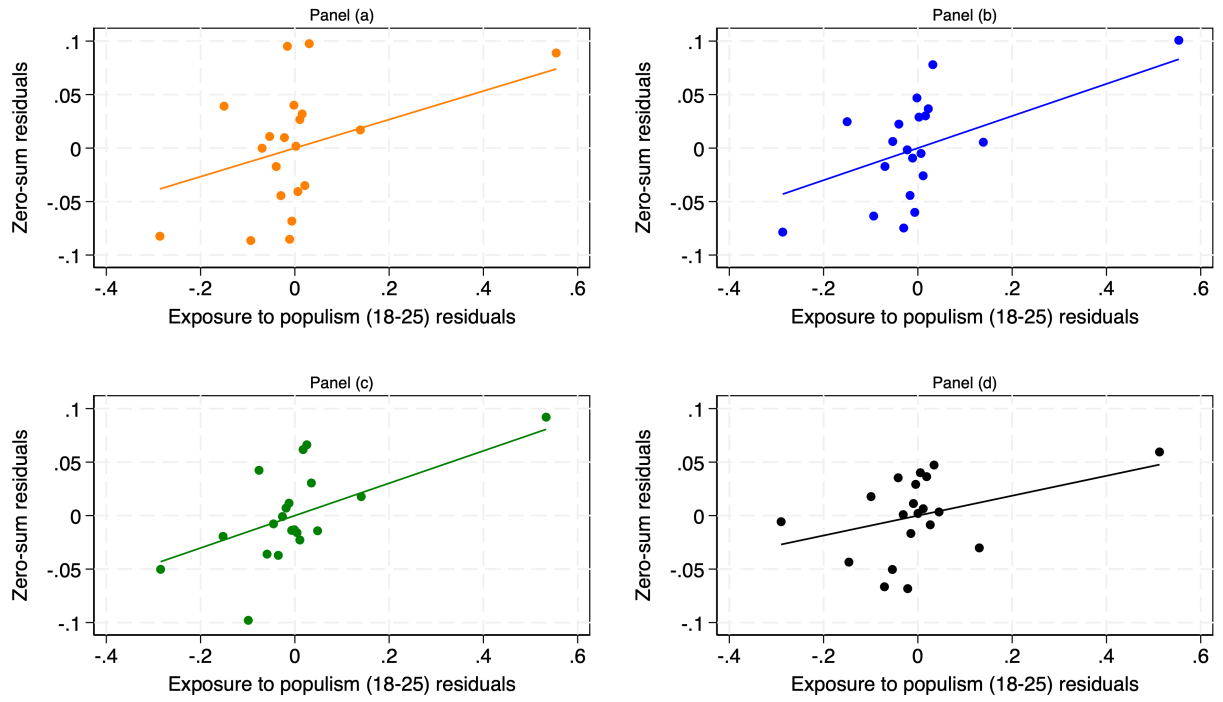
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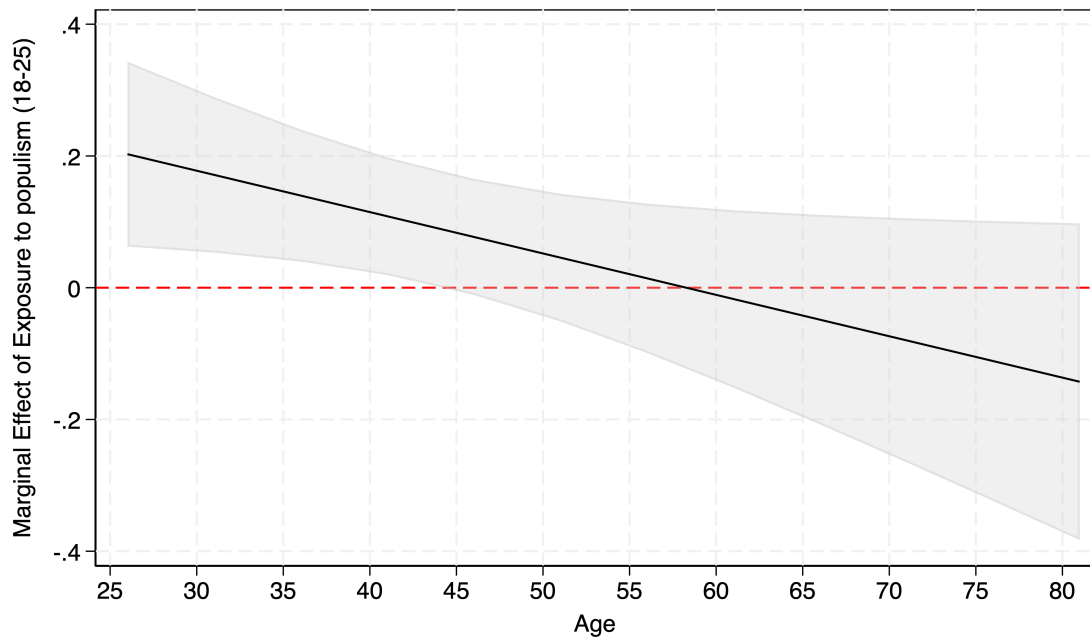
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Figure 1: Exposure to populism and zero-sum thinking



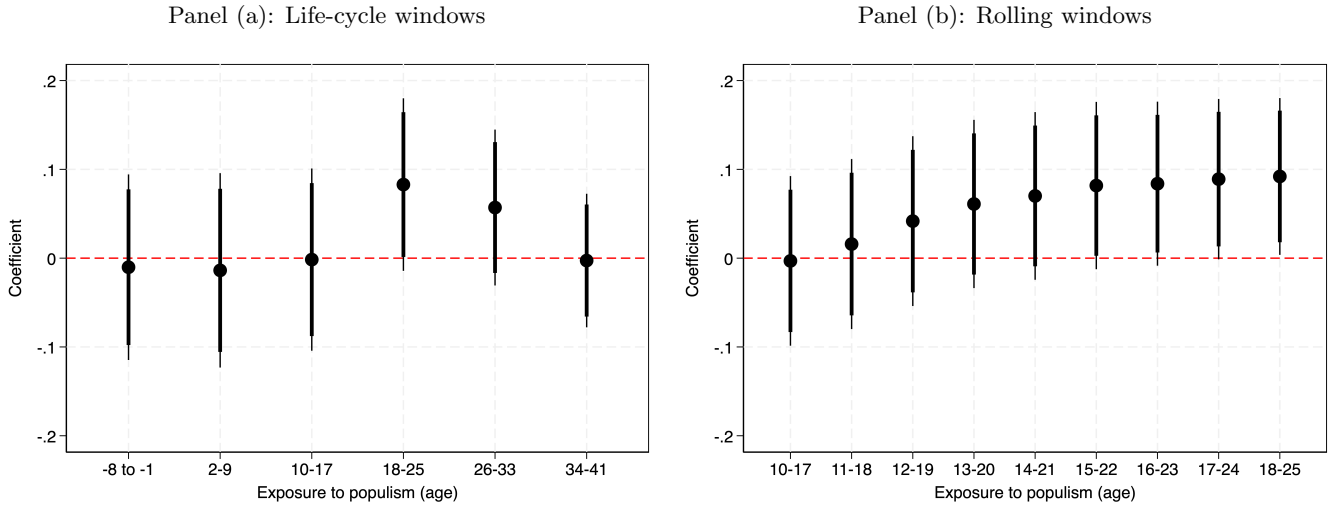
Notes: This figure shows binned scatterplots of *Exposure to populism (18-25)* against *Zero-sum thinking*. Plotted values are residualised using the fixed effects and controls corresponding to each column of Table 1. Panel (a) residualises on country, year, and age fixed effects. Panel (b) adds individual controls. Panel (c) adds cohort fixed effects. Panels (a) through (c) additionally partial out contemporaneous populism, consistent with columns (1) to (3) of Table 1. Panel (d) includes age, cohort, and country $\times$ year fixed effects, as well as individual controls, which already absorb contemporaneous populism.

Figure 2: Formative populist exposure and zero-sum thinking, by respondent age



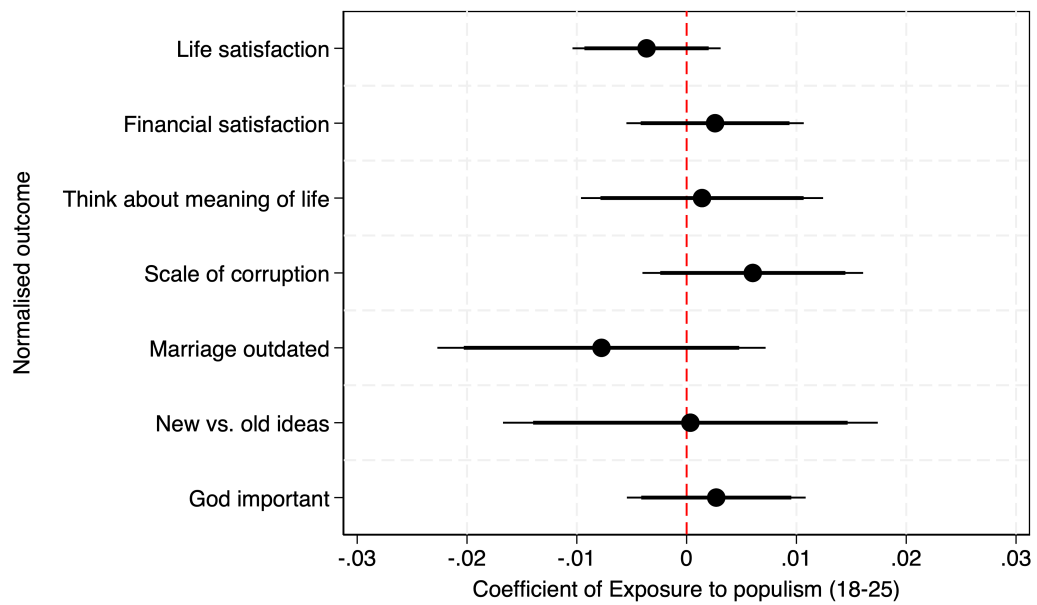
Notes: This figure shows the marginal effect of *Exposure to populism (18-25)* on *Zero-sum thinking* at each respondent age. Estimates are obtained by interacting exposure with respondent age in the specification of column (4) of Table 1, which includes age, cohort, and country $\times$ year fixed effects, as well as individual controls. The figure describes how the estimated association varies with the age at which respondents are observed in repeated cross-sections; it does not follow the same individuals over time. Standard errors are clustered at the country-cohort level. Shaded areas denote 95% confidence intervals.

Figure 3: Timing of exposure across the life cycle



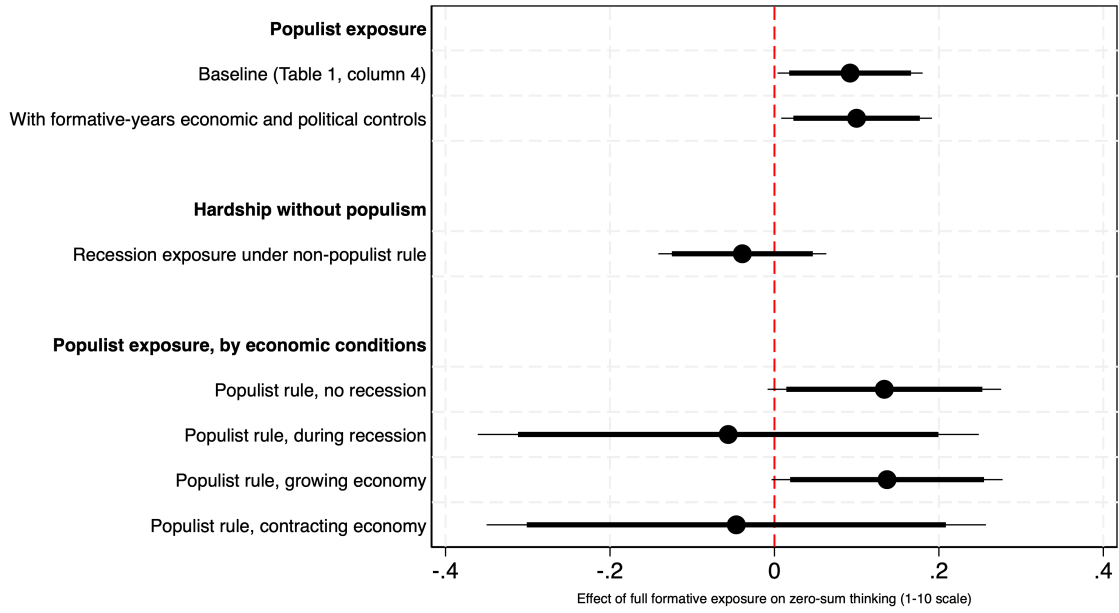
Notes: This figure shows OLS coefficients on *Exposure to populism* across age windows. Panel (a) enters six eight-year windows in the same regression: pre-birth (ages -8 to -1), early childhood (ages 2 to 9), adolescence (ages 10 to 17), impressionable years (ages 18 to 25), post-formative adulthood (ages 26 to 33), and mid-adulthood (ages 34 to 41). Panel (b) estimates nine rolling eight-year windows separately, starting with ages 10 to 17 and shifting forward one year at a time until ages 18 to 25. Both panels use the specification of column (4) of Table 1, with age, cohort, and country $\times$ year fixed effects and individual controls; standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals. Appendix Figure A.7 reports the six life-cycle windows estimated separately.

Figure 4: Placebo outcomes



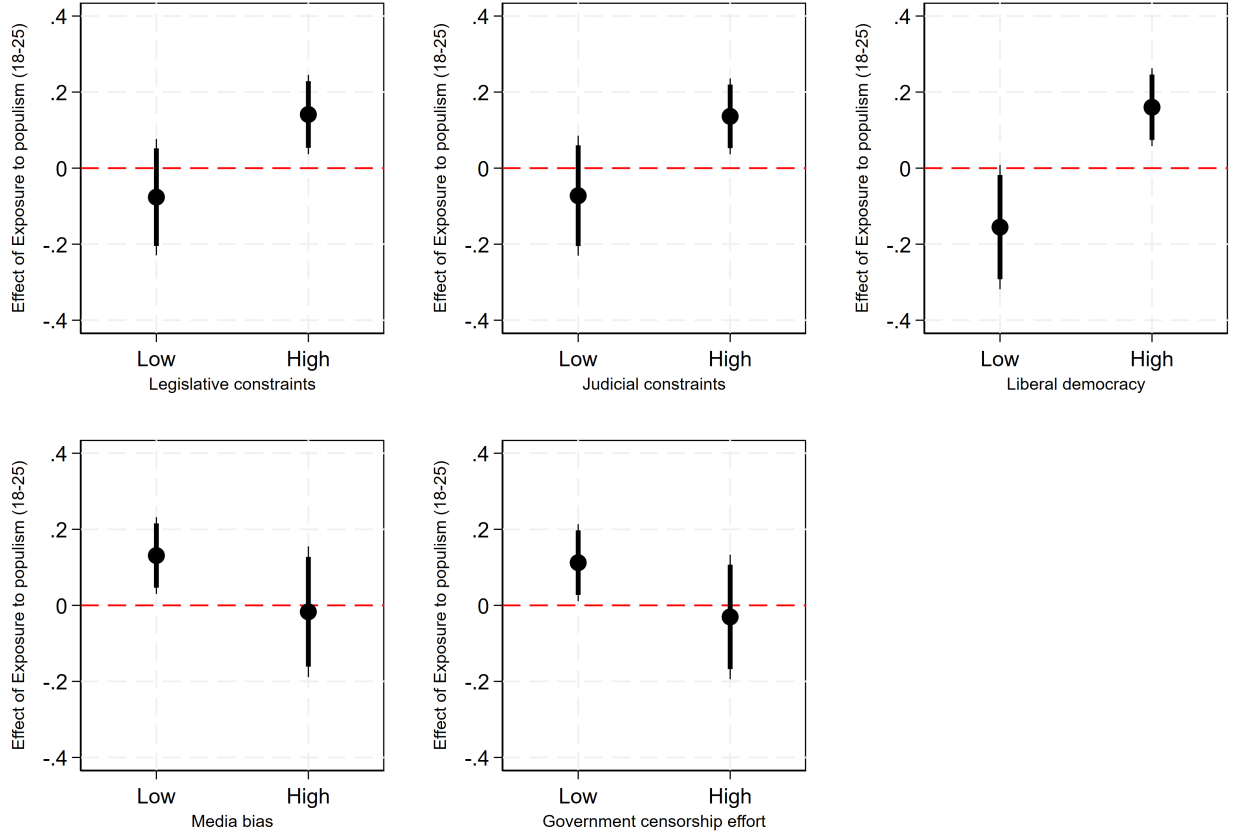
Notes: This figure shows OLS coefficients on *Exposure to populism (18-25)* for seven socio-economic and cultural attitudes. Each outcome is normalised to the unit interval. Each estimate uses the specification of column (4) of Table 1, with age, cohort, and country $\times$ year fixed effects and individual controls. Standard errors are clustered at the country-cohort level. Thick and thin intervals denote 90% and 95% confidence intervals, respectively.

Figure 5: Economic scarcity tests



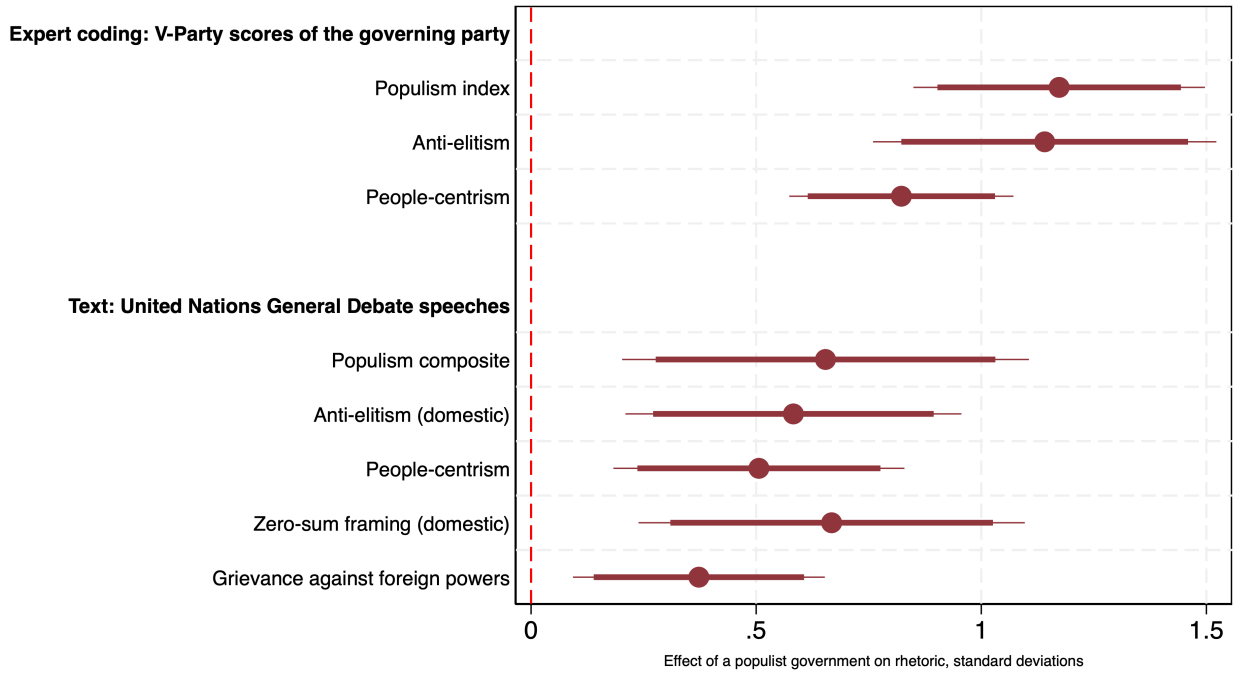
Notes: This figure summarises the economic-scarcity tests. Each marker reports a coefficient on the stated exposure measure from the indicated regression of zero-sum thinking, all using the fixed-effect and control structure of the preferred specification, column (4) of Table 1, with age, cohort, and country $\times$ year fixed effects and individual controls. The specification labelled “With formative-years economic and political controls” adds GDP per capita and the political-shock battery during ages 18 to 25 (Appendix Table A.5). *Recession exposure under non-populist rule* is the share of ages 18 to 25 in which GDP per capita declined while no populist held office (Appendix Table A.14). The bottom group splits populist exposure by whether a recession, or negative GDP-per-capita growth, coincided with the populist spell (Appendix Tables A.14 and A.15). Standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals; the dashed red vertical line marks zero.

Figure 6: Institutional constraints and media freedom



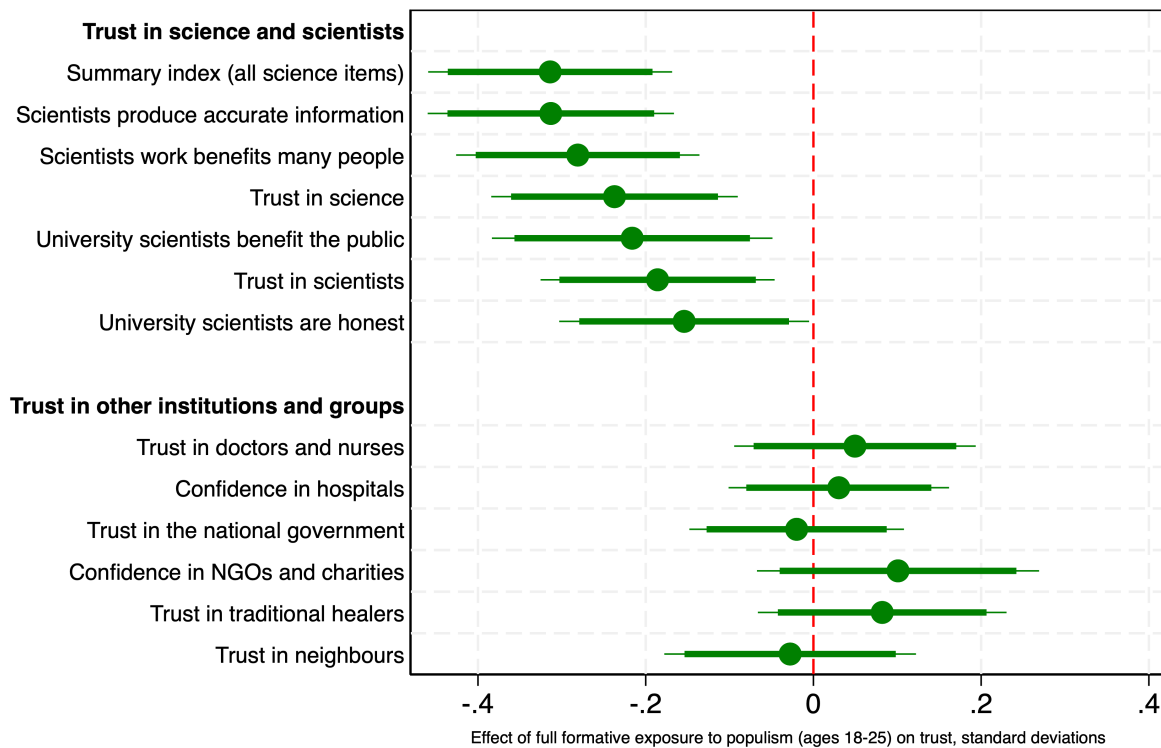
Notes: This figure shows the effect of *Exposure to populism (18-25)* on *Zero-sum thinking* separately for respondents facing low and high values of five moderators averaged over ages 10 to 17, before the formative exposure window. The top row uses three V-Dem measures of executive constraints: legislative constraints on the executive, judicial constraints on the executive, and the liberal democracy index. The bottom row uses two V-Dem measures of the media environment: media bias against the opposition and government censorship effort, where lower values denote greater media freedom. Estimates are based on Equation 3, which augments the specification of column (4) of Table 1 with the moderator and its interaction with populism exposure. Standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals; the dashed red horizontal line marks zero.

Figure 7: Populist rule and political rhetoric



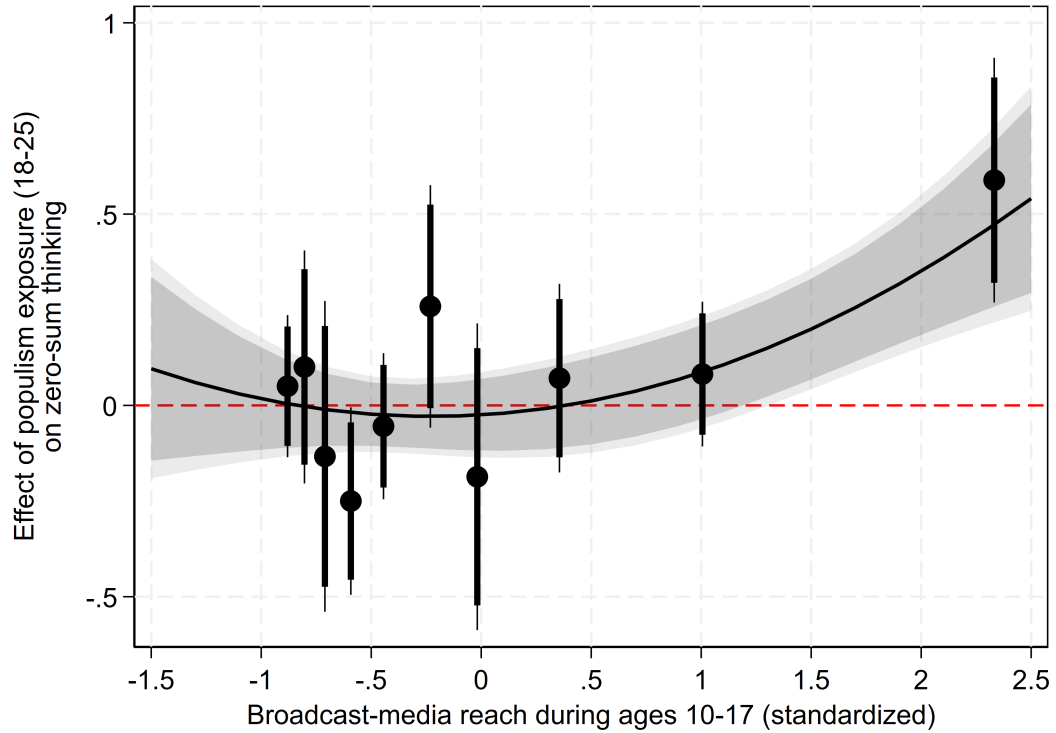
Notes: This figure shows the coefficient on the [Funke et al. \(2023\)](#) populist-government indicator from country-year panel regressions of rhetoric measures, each estimated with country and year fixed effects and standard errors clustered by country. The upper panel uses V-Party expert codings of the head of government's party, expressed in standard deviations of each outcome ($N = 2,695$ country-years; Appendix Table [A.16](#)). The lower panel uses scores of United Nations General Debate speeches, graded by a large language model applying the Team Populism rubric and standardised within calendar year ($N = 3,614$ country-years; Appendix Table [A.17](#); Appendix [B](#) describes the corpus and scoring, and Appendix Figure [A.13](#) plots the full distributions). Thick and thin bars denote 90% and 95% confidence intervals; the dashed red vertical line marks zero.

Figure 8: Formative exposure and trust in scientists



Notes: This figure shows coefficients on *Exposure to populism (18-25)* from separate regressions of trust outcomes, estimated using respondents in 2018 Wellcome Global Monitor countries represented in our WVS estimation sample (about 44,000 respondents in 47 countries, fielded before the COVID-19 pandemic). Exposure is constructed exactly as in the main analysis. Each regression includes country and birth-cohort fixed effects and individual controls, uses the survey's population-projection weights, and clusters standard errors at the country-cohort level. Outcomes are coded so that higher values indicate more trust and are standardised to unit variance, so coefficients are in standard deviations. The upper panel reports trust in science and scientists, with a summary index of the six items first; the lower panel reports trust in other institutions and groups. Thick and thin bars denote 90% and 95% confidence intervals; the dashed red vertical line marks zero.

Figure 9: Formative exposure and zero-sum thinking, by broadcast reach



Notes: This figure plots the marginal effect of *Exposure to populism (18-25)* on *Zero-sum thinking* as a function of pre-formative broadcast-media reach, averaged over ages 10 to 17 and measured as a standardised composite of radio and television receivers per capita from the Cross-National Time-Series archive (Banks and Wilson, 2024). The solid line and shaded band are the effect and its 95% confidence interval from a single regression that interacts populism exposure with a quadratic in standardised broadcast reach. The points and whiskers show the effects estimated for each decile of broadcast reach, from a single pooled model that interacts exposure with decile indicators; for the decile estimates, thick and thin whiskers denote 90% and 95% confidence intervals, respectively. Both use the specification of column (4) of Table 1. Standard errors are clustered at the country-cohort level. The dashed red horizontal line marks zero. Appendix Figure A.16 reports a coarser tercile specification.

Table 1: Exposure to populism and zero-sum thinking, main results

	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.135*** (0.050)	0.151*** (0.049)	0.149*** (0.049)	0.092** (0.045)
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country $\times$ Year FEs				✓
R-squared	0.069	0.073	0.074	0.088
Mean of outcome	4.567	4.567	4.567	4.567
SD of outcome	2.709	2.709	2.709	2.709
Clusters	3,367	3,367	3,365	3,365
Observations	134,302	134,302	134,300	134,300

Notes: This table reports OLS coefficient estimates using the baseline WVS sample. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. Columns (1) to (3) include a control for contemporaneous exposure to populism. All specifications use the WVS survey probability weights. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2: Exposure to populism and perceived economic conflict, ISSP results

Panel A	Perceived conflict (mean)			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.062*** (0.018)	0.061*** (0.018)	0.051*** (0.019)	0.056*** (0.018)
R-squared	0.123	0.134	0.135	0.155
Mean of outcome	2.322	2.322	2.322	2.322
SD of outcome	0.672	0.672	0.672	0.672
Clusters	2,791	2,791	2,791	2,791
Observations	129,352	129,352	129,352	129,352
Panel B	Perceived conflict (mean of binarised comps.)			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.036*** (0.009)	0.035*** (0.010)	0.027*** (0.010)	0.029*** (0.009)
R-squared	0.138	0.152	0.153	0.168
Mean of outcome	0.379	0.379	0.379	0.379
SD of outcome	0.372	0.372	0.372	0.372
Clusters	2,791	2,791	2,791	2,791
Observations	129,352	129,352	129,352	129,352
Panel C	Perceived conflict (PCA, 0–1)			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.020*** (0.006)	0.020*** (0.006)	0.016** (0.006)	0.017*** (0.006)
R-squared	0.131	0.143	0.144	0.164
Mean of outcome	0.440	0.440	0.440	0.440
SD of outcome	0.223	0.223	0.223	0.223
Clusters	2,686	2,686	2,686	2,686
Observations	118,657	118,657	118,657	118,657
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓

Notes: This table reports OLS coefficient estimates using the baseline ISSP sample. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. In Panel A, the dependent variable is the mean of raw responses across three perceived conflict items (between poor and rich people, the working class and the middle class, and management and workers), each rated on a four-point scale and reverse-coded so that higher values indicate greater perceived conflict. In Panel B, the dependent variable is the mean of binarised versions of the same three items, where each item equals one if the respondent reports strong or very strong conflict. In Panel C, the dependent variable is the first principal component of the three conflict items, rescaled to the unit interval using a min–max transformation. Columns (1) to (3) additionally control for contemporaneous populist rule. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: Exposure to populism and stated vote intentions

Stated vote intention	Populist	Far-left/right	Far-left	Far-right
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	-0.010 (0.019)	0.040* (0.023)		
Exposure to right-wing populism (18-25)			0.006 (0.011)	0.035* (0.021)
Exposure to left-wing populism (18-25)			0.026*** (0.009)	0.012 (0.021)
Age FEs	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓
Country $\times$ Year FEs	✓	✓	✓	✓
R-squared	0.164	0.160	0.086	0.195
Mean of outcome	0.063	0.079	0.033	0.046
SD of outcome	0.243	0.270	0.178	0.210
Clusters	1,533	1,533	1,533	1,533
Observations	46,523	46,523	46,523	46,523

Notes: This table reports OLS coefficient estimates using the WVS sample matched to the PopuList. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in each column is a binary indicator for whether the respondent would vote for (1) a populist party, (2) a far-left or far-right party, (3) a far-left party, or (4) a far-right party, if a national election were held tomorrow. Columns (3) and (4) replace overall exposure with separate measures of exposure to right-wing and left-wing populist governments. Parties are classified using the PopuList (Rooduijn et al., 2023, 2024). All columns use the same fixed-effect and control structure as column (4) of Table 1: single-year age, birth-cohort, and country $\times$ year fixed effects and individual-level controls. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Populist Rule and the Formation of Zero-Sum Beliefs

Supplementary Information (SI) Appendix

For Online Publication

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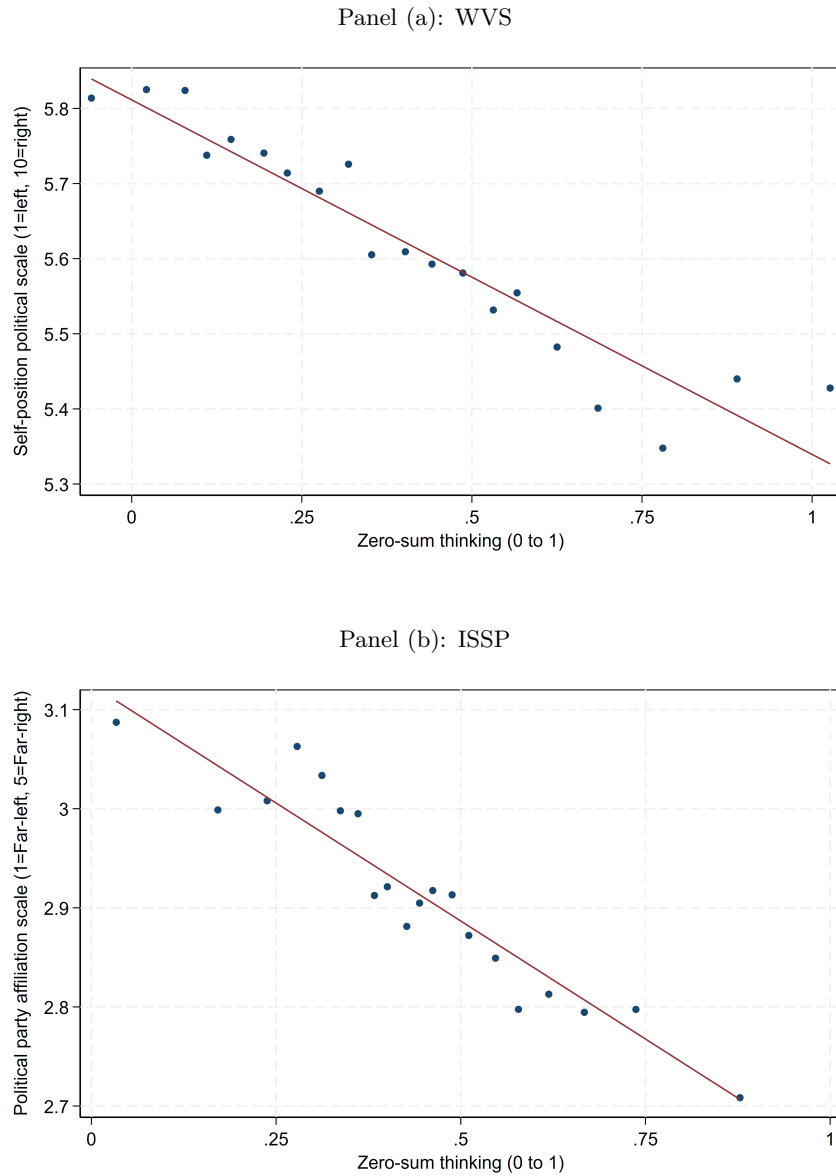
EBRD, King's College London and CEPR

Harry Pickard

Newcastle University

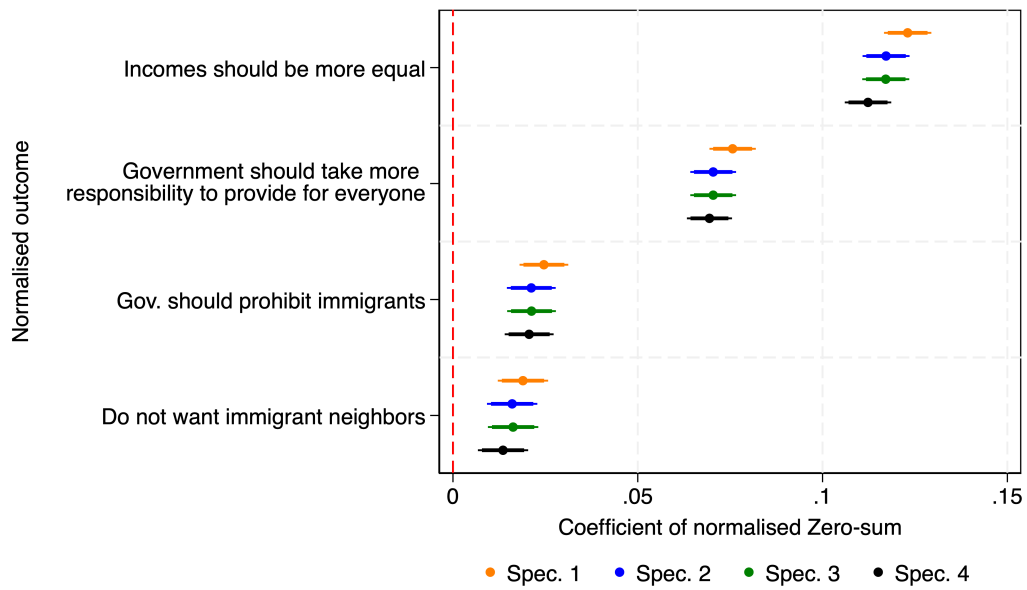
A Supplementary Figures and Tables

Figure A.1: Ideology and zero-sum thinking



Notes: This figure shows binned scatterplots. Panel (a) uses WVS data: the x-axis is *Zero-sum thinking*, normalised to the unit interval, and the y-axis is self-reported political orientation from 1 (left) to 10 (right). Panel (b) uses ISSP data: the x-axis is the mean of the three perceived-conflict items, normalised to the unit interval, and the y-axis is the left-right orientation of the respondent's affiliated political party from 1 (far left) to 5 (far right). Both panels plot residualised values after removing country $\times$ year fixed effects.

Figure A.2: Zero-sum thinking and policy preferences



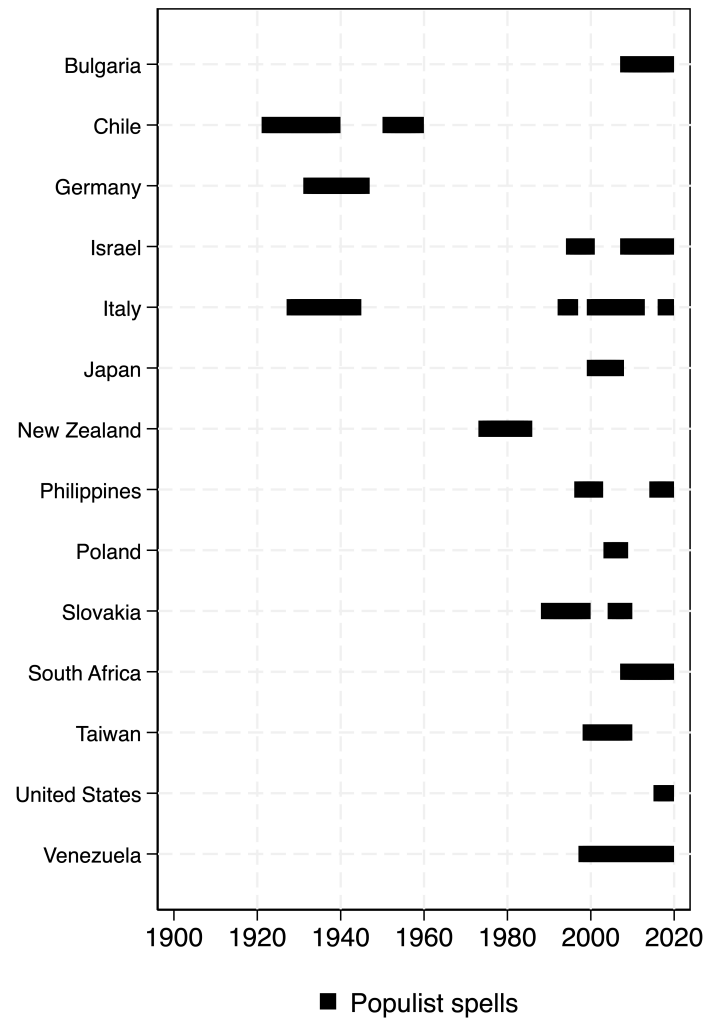
Notes: This figure shows OLS coefficients on *Zero-sum thinking*, normalised to the unit interval, for four policy outcomes, also normalised to the unit interval. The outcomes are support for income equalisation, support for government responsibility to provide for citizens, support for government prohibition of immigration, and opposition to having immigrant neighbours. Each colour corresponds to a specification from Table 1: Spec. 1 includes country, year, and age fixed effects; Spec. 2 adds individual controls; Spec. 3 adds cohort fixed effects; and Spec. 4 replaces country and year fixed effects with country $\times$ year fixed effects. Standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals, respectively.

Figure A.3: Populist spells by country (WVS)



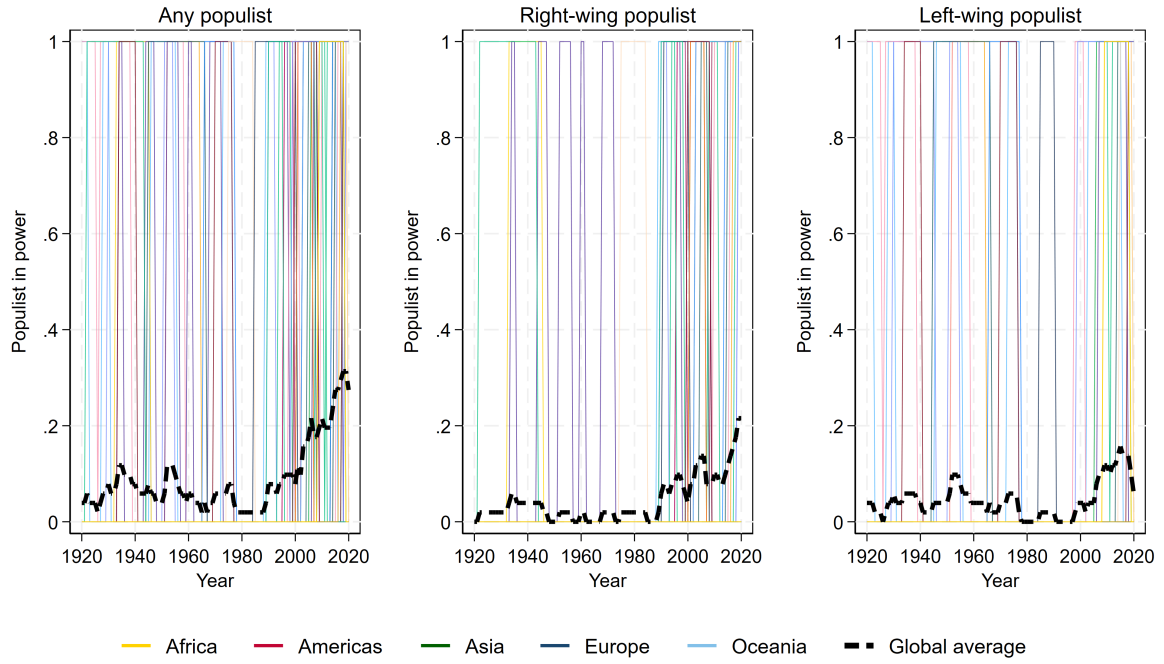
Notes: This figure shows populist spells in WVS countries for which those spells generate formative exposure for at least one cohort in the estimation sample.

Figure A.4: Populist spells by country (ISSP)

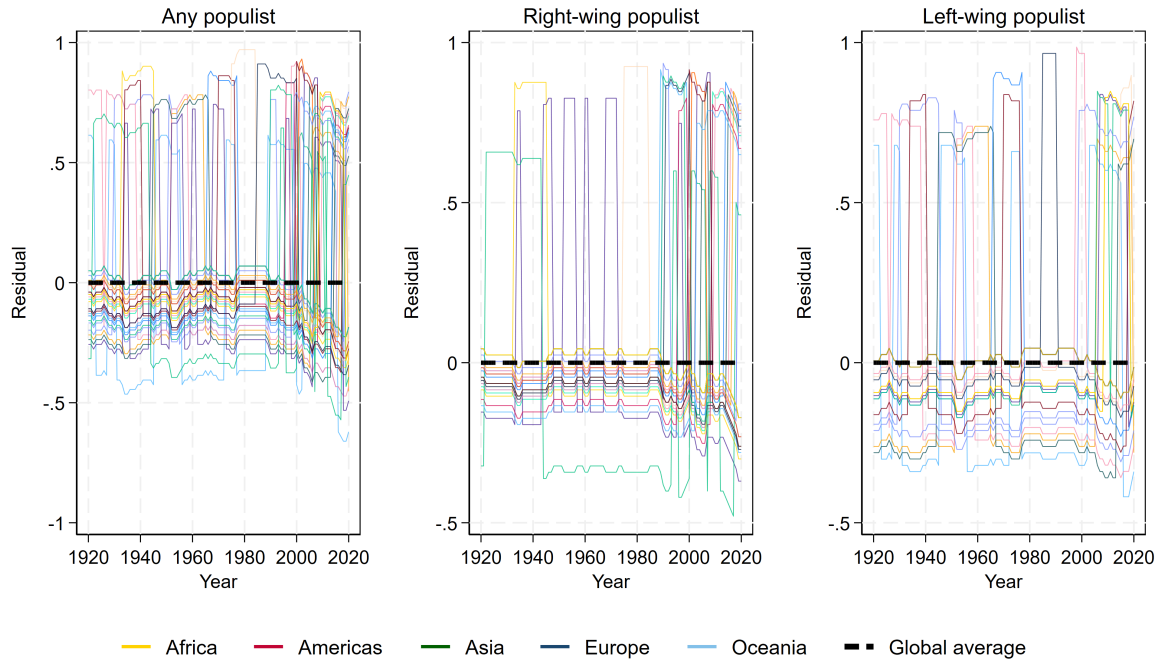


Notes: This figure shows populist spells in ISSP countries for which those spells generate formative exposure for at least one cohort in the estimation sample.

Figure A.5: Populist leaders over time
Panel (a): Raw values

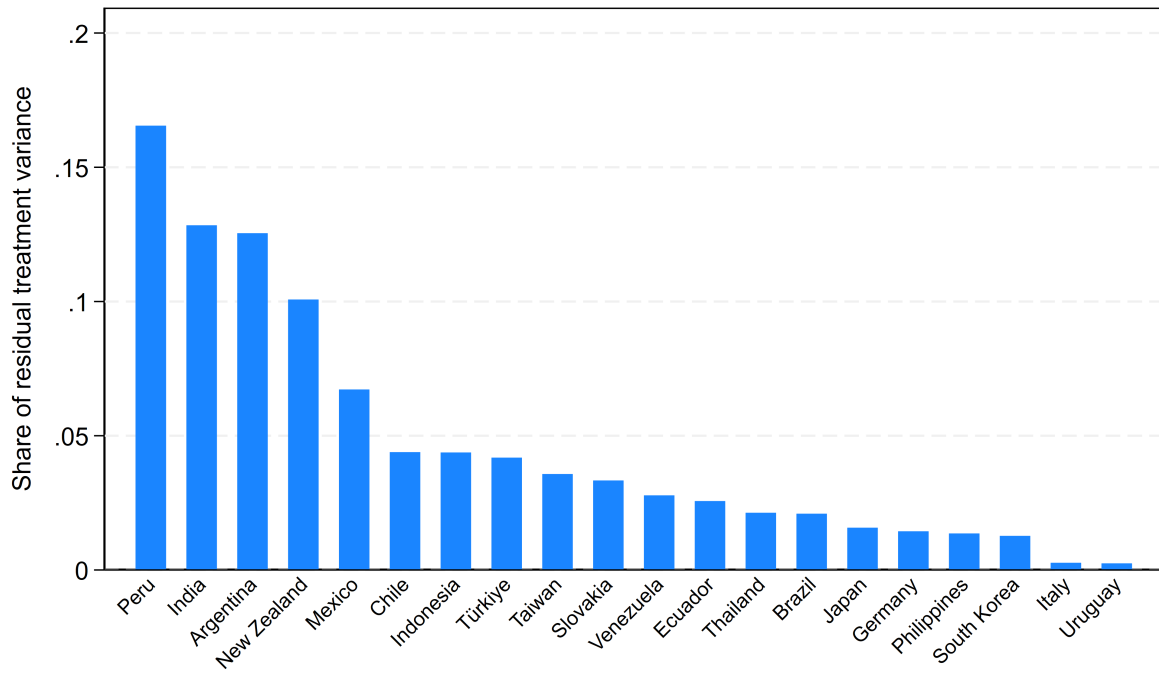


Panel (b): Residuals after partialling out country and year fixed effects



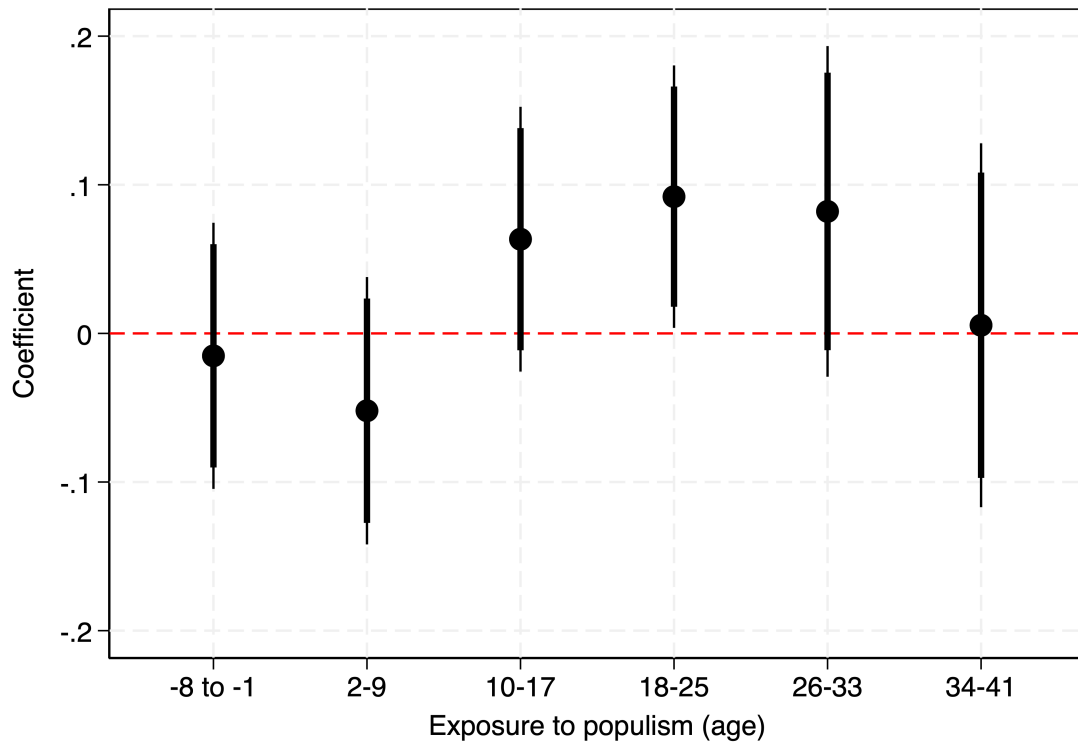
Notes: Panel (a) plots the raw country-year indicator for populist rule for each country, separately for any populist (left), right-wing populist (centre), and left-wing populist (right). The dashed black line shows the unweighted share of countries governed by a populist leader in each calendar year. Panel (b) plots the residuals after partialling out country and year fixed effects. All plots are colour-coded by continent: gold for African countries, red for American countries, green for Asian countries, navy for European countries, and light blue for Oceanian countries.

Figure A.6: Share of residual (identifying) treatment variation



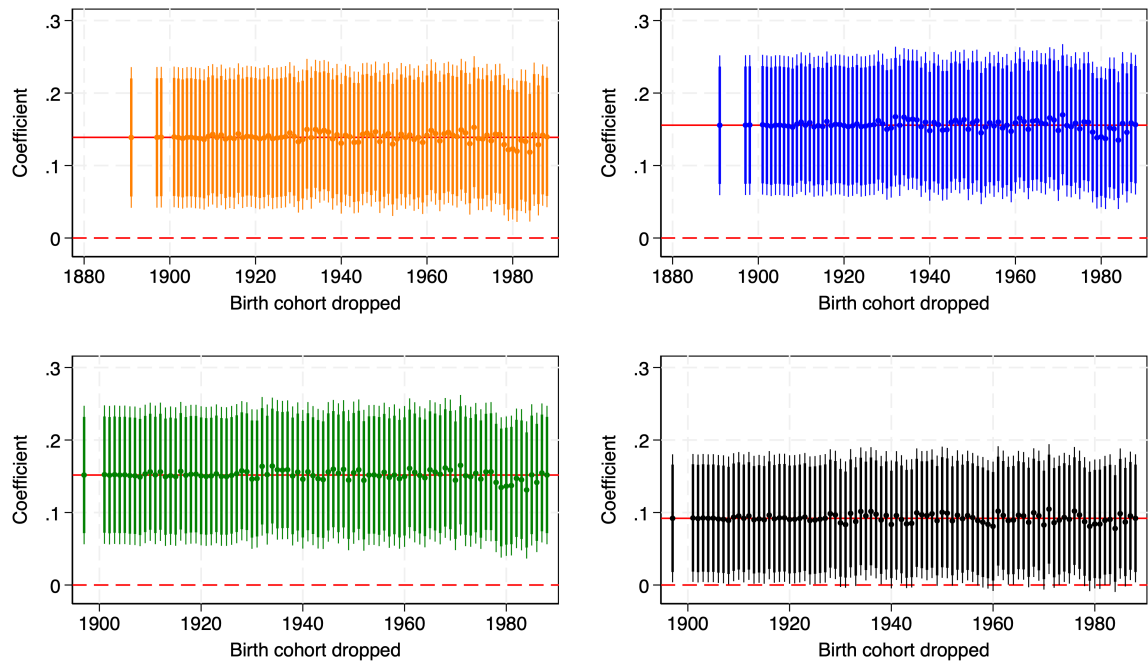
Notes: This figure shows the share of residual treatment variance contributed by each of the 20 countries with the largest shares. *Exposure to populism (18-25)* is residualised against country $\times$ year, cohort, age, and individual controls (gender, education, within-country-year income deciles, and an indicator for having children) using survey probability weights, matching the absorption set of column (4) of Table 1. Shares are computed from the weighted sum of squared residuals and sum to one across all countries in the estimation sample.

Figure A.7: Life-cycle exposure windows, estimated separately



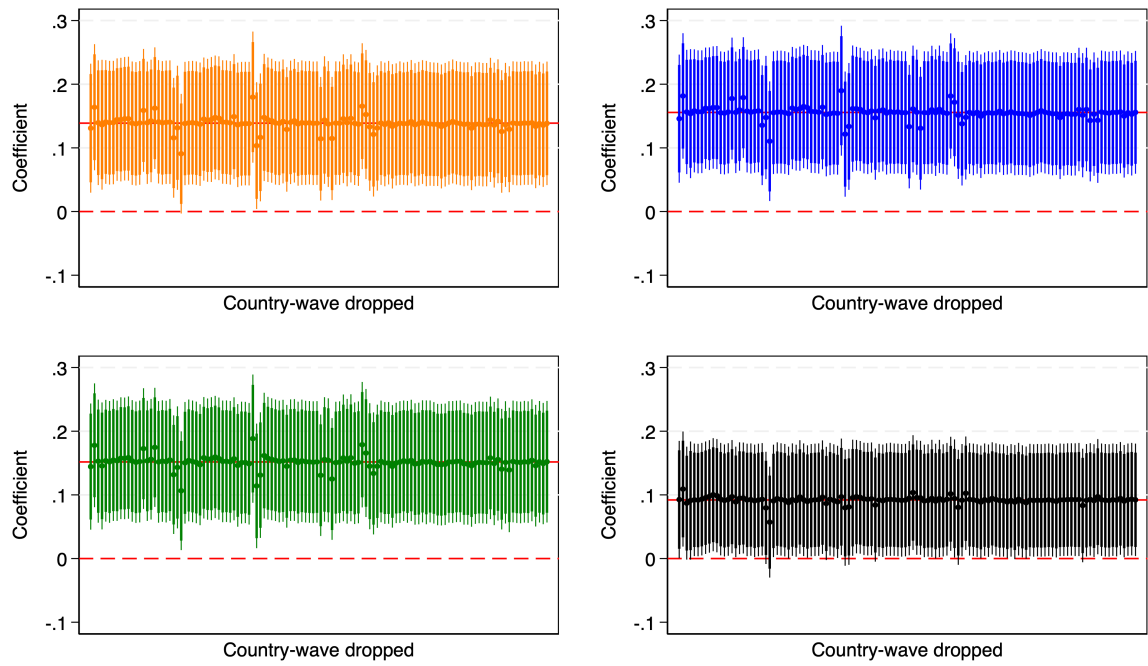
Notes: This figure shows OLS coefficients on *Exposure to populism* across six eight-year age windows: pre-birth (ages –8 to –1), early childhood (ages 2 to 9), adolescence (ages 10 to 17), impressionable years (ages 18 to 25), post-formative adulthood (ages 26 to 33), and mid-adulthood (ages 34 to 41). Each window is estimated in a separate regression using the specification of column (4) of Table 1, with age, cohort, and country $\times$ year fixed effects and individual controls; Figure 3 Panel (a) enters all six windows jointly. Standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals, respectively.

Figure A.8: Leave-one-out robustness across birth cohorts



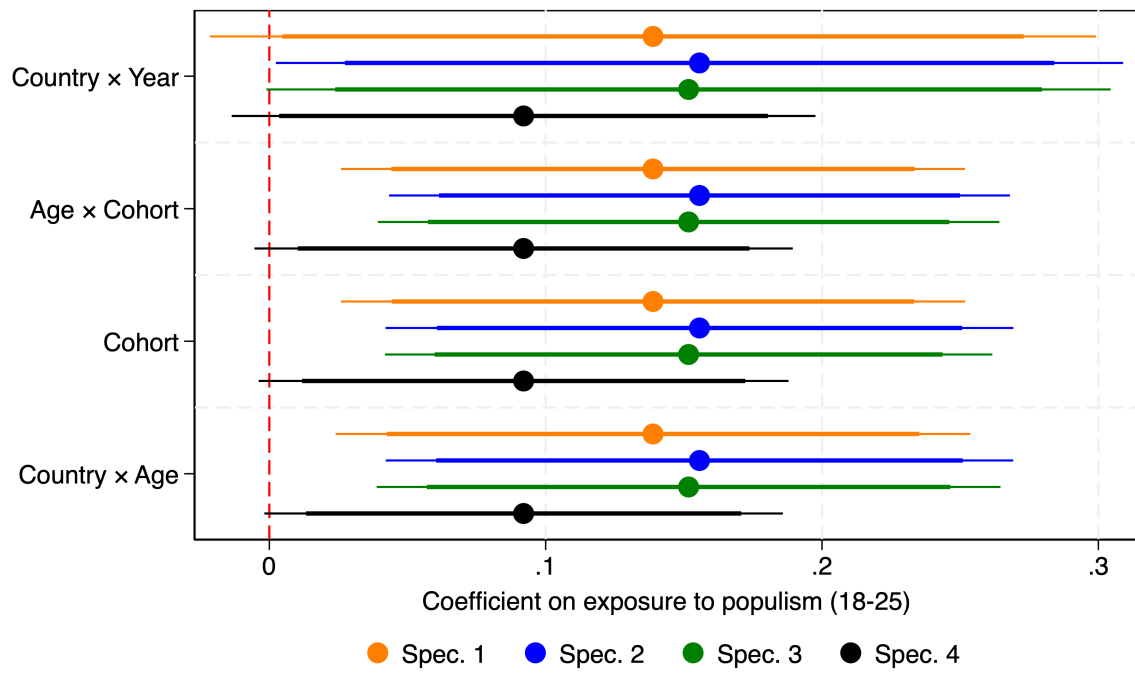
Notes: This figure shows OLS coefficients on *Exposure to populism (18-25)* from a leave-one-out exercise in which each birth cohort is sequentially excluded from the estimation sample. Each panel corresponds to one of the four baseline specifications from Table 1: orange is column (1), blue is column (2), green is column (3) and black is column (4). The horizontal line marks the baseline estimate. Standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals, respectively.

Figure A.9: Leave-one-out robustness across country-waves



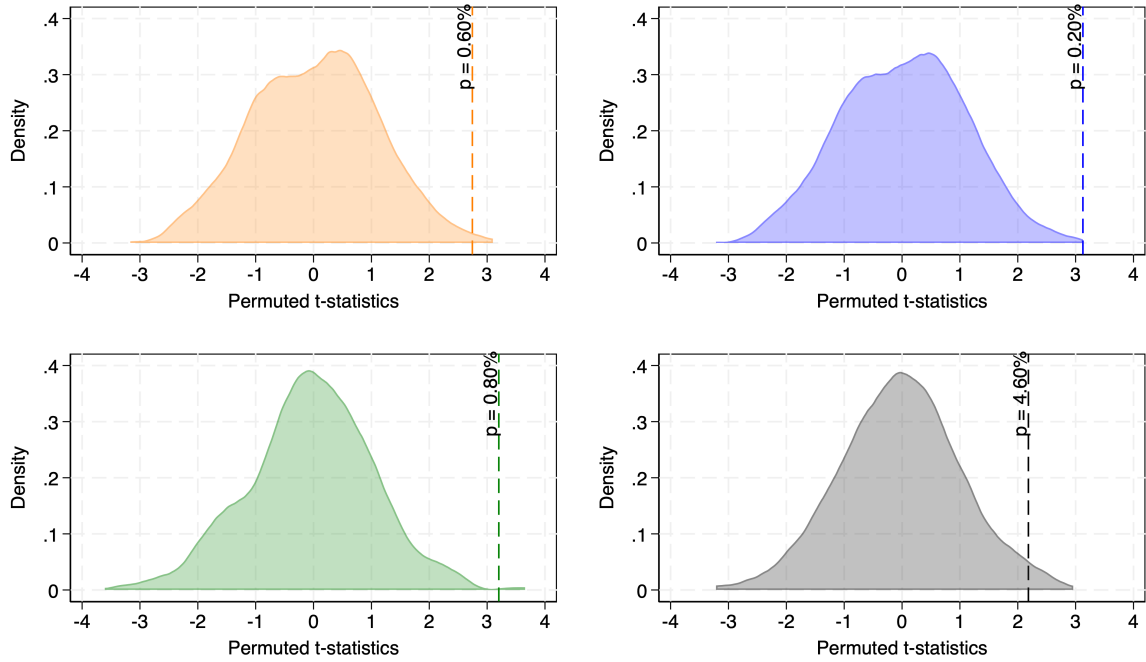
Notes: This figure shows OLS coefficients on *Exposure to populism (18-25)* from a leave-one-out exercise in which each of the 122 country-waves is sequentially excluded from the estimation sample. Each panel corresponds to one of the four baseline specifications from Table 1: orange is column (1), blue is column (2), green is column (3) and black is column (4). The horizontal line marks the baseline estimate. Standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals, respectively.

Figure A.10: Alternate clustering of standard errors



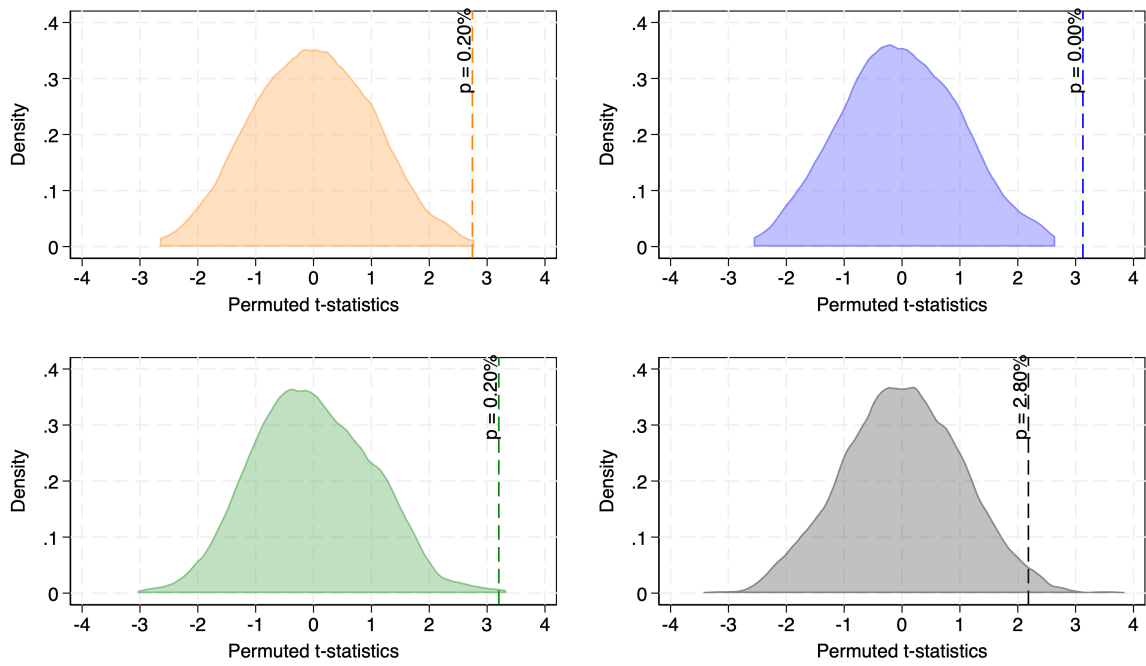
Notes: This figure shows OLS coefficients on *Exposure to populism (18-25)* across four alternative clustering schemes: country × year, age × cohort, cohort, and country × age. Each colour corresponds to one of the four baseline specifications from Table 1: Spec. 1 includes country, year, and age fixed effects; Spec. 2 adds individual controls; Spec. 3 adds cohort fixed effects; and Spec. 4 replaces country and year fixed effects with country × year fixed effects. Thick and thin bars denote 90% and 95% confidence intervals, respectively.

Figure A.11: Permutation inference, country-cohort block permutation



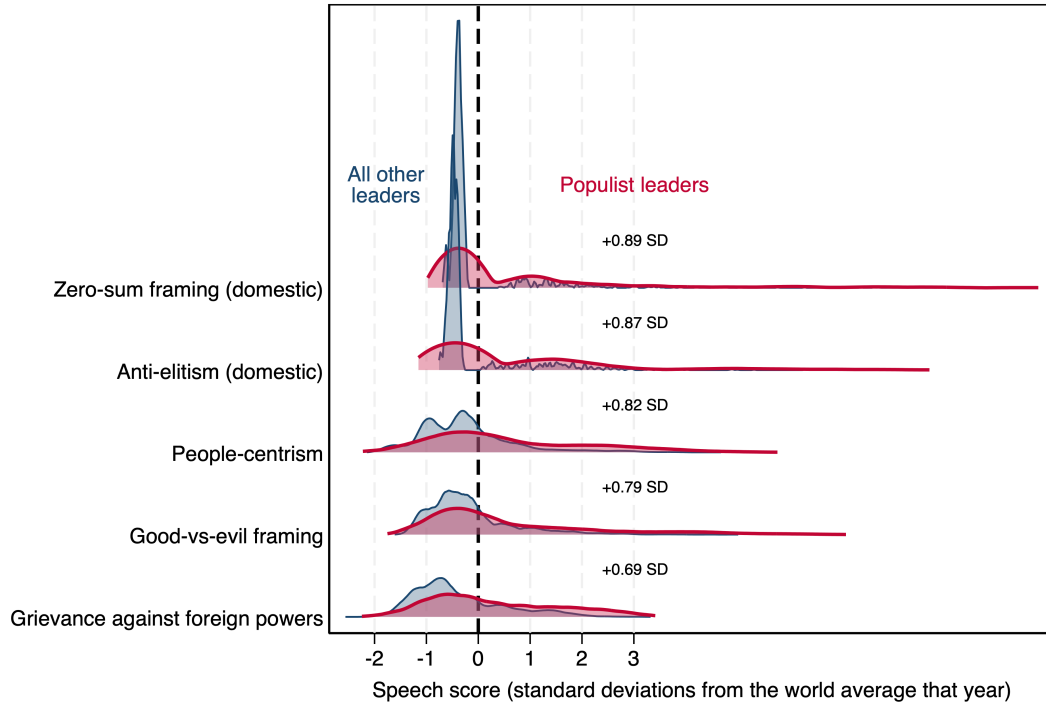
Notes: This figure shows the distribution of t-statistics on *Exposure to populism (18-25)* from 500 permutations in which each country $\times$ birth-year cell is permuted as a single block across the full sample, so that all respondents in a given (country, birth-year) cell receive the same permuted exposure value. Panels correspond to the four specifications of Table 1: column (1), top-left; column (2), top-right; column (3), bottom-left; and column (4), bottom-right. The vertical dashed line marks the t-statistic from the baseline, unpermuted estimate, and the percentage reported in each panel is the permutation p -value: the share of permuted t-statistics at least as extreme as the observed estimate. Standard errors are clustered at the country-cohort level.

Figure A.12: Permutation inference, within-country cohort block permutation



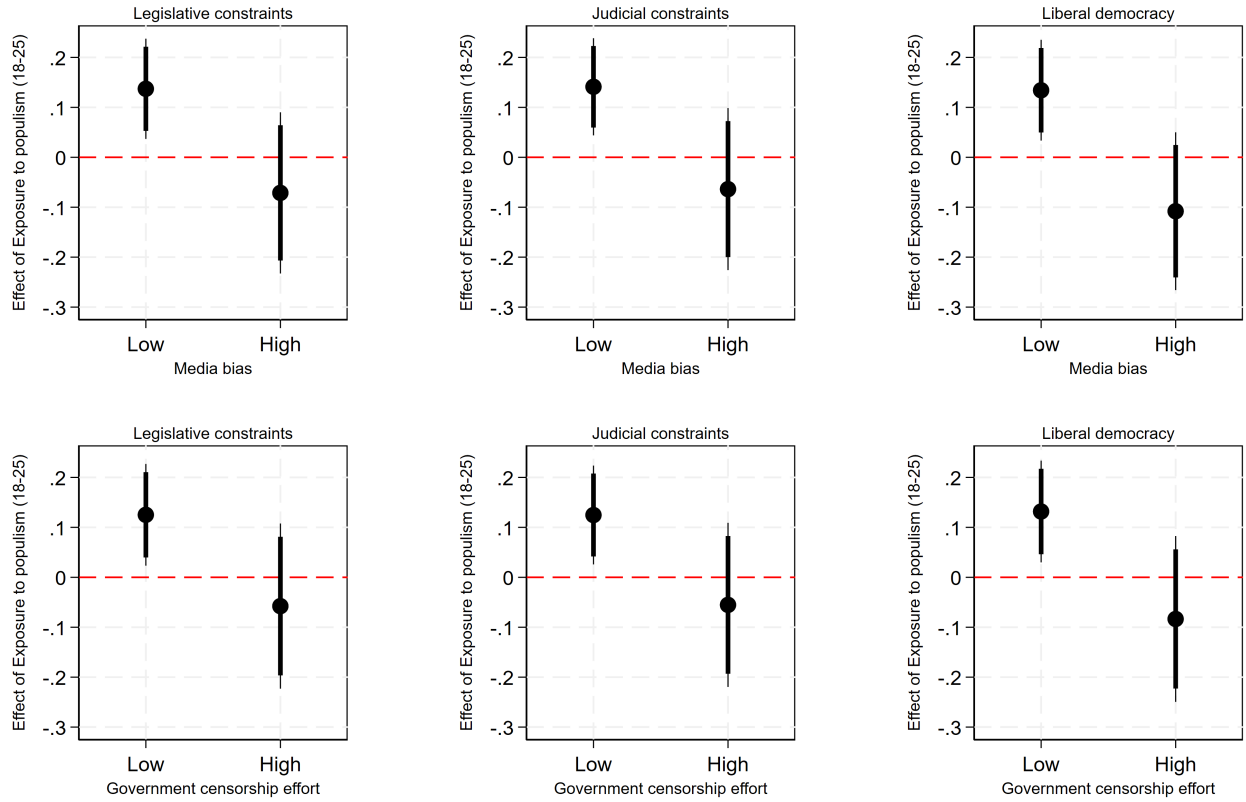
Notes: This figure shows the distribution of t-statistics on *Exposure to populism (18-25)* from 500 permutations in which country $\times$ birth-year exposure blocks are reshuffled within each country, preserving the cross-country structure. Panels correspond to the four specifications of Table 1: column (1), top-left; column (2), top-right; column (3), bottom-left; and column (4), bottom-right. The vertical dashed line marks the t-statistic from the baseline, unpermuted estimate, and the percentage reported in each panel is the permutation p -value: the share of permuted t-statistics at least as extreme as the observed estimate. Standard errors are clustered at the country-cohort level.

Figure A.13: Populist rule and rhetoric at the United Nations



Notes: Each row plots the distribution of one component of populist rhetoric across the country-years for which the [Funke et al. \(2023\)](#) populist-government classification is available and a United Nations General Debate speech is scored, one speech per country and year. Red curves are country-years in which the head of government is classified as populist; blue curves are the remaining country-years in that sample. The speech corpus itself is broader; this figure and Appendix Table A.17 use the subset for which the populist-government treatment is defined. Each component is scored from 0 to 10 by a large language model applying the Team Populism holistic rubric, and is then standardised within calendar year, so the horizontal axis measures standard deviations from that year's world average (Appendix B describes the corpus, the scoring, and the domestic–international distinction). The value printed on each row is the unconditional difference in mean scores between populist and other country-years; these raw distributional gaps are larger than the fixed-effects-adjusted regression coefficients reported in Appendix Table A.17 and Figure 7. Under a populist leader, every component shifts towards more populist speech, and the shift is largest for the domestic components and smallest for grievance against foreign powers.

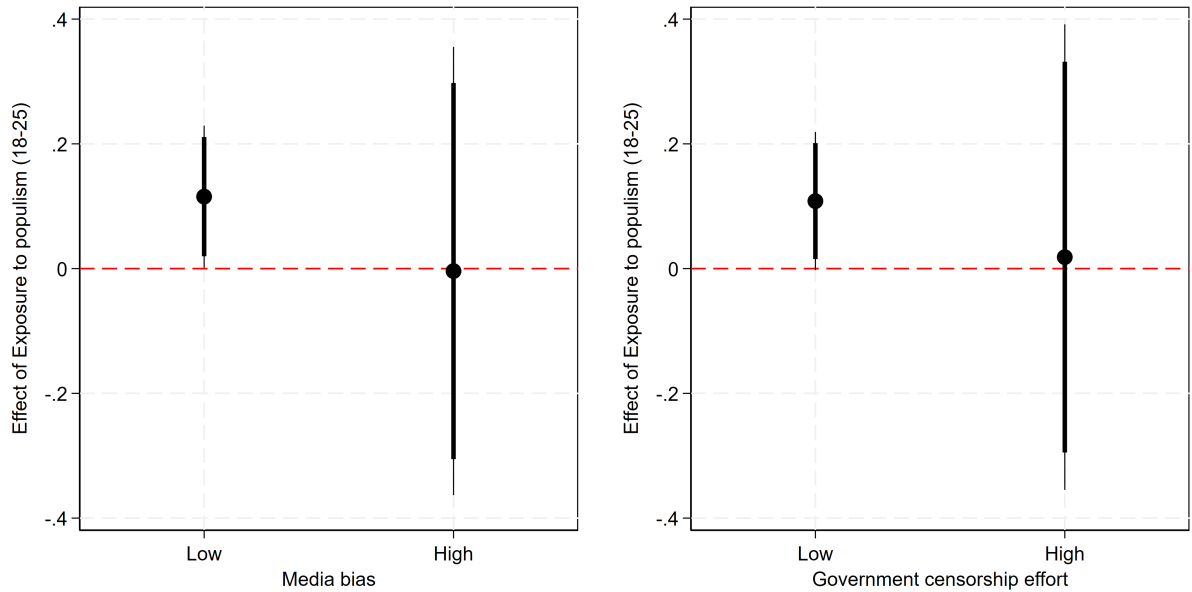
Figure A.14: Media gradient controlling jointly for institutional constraints



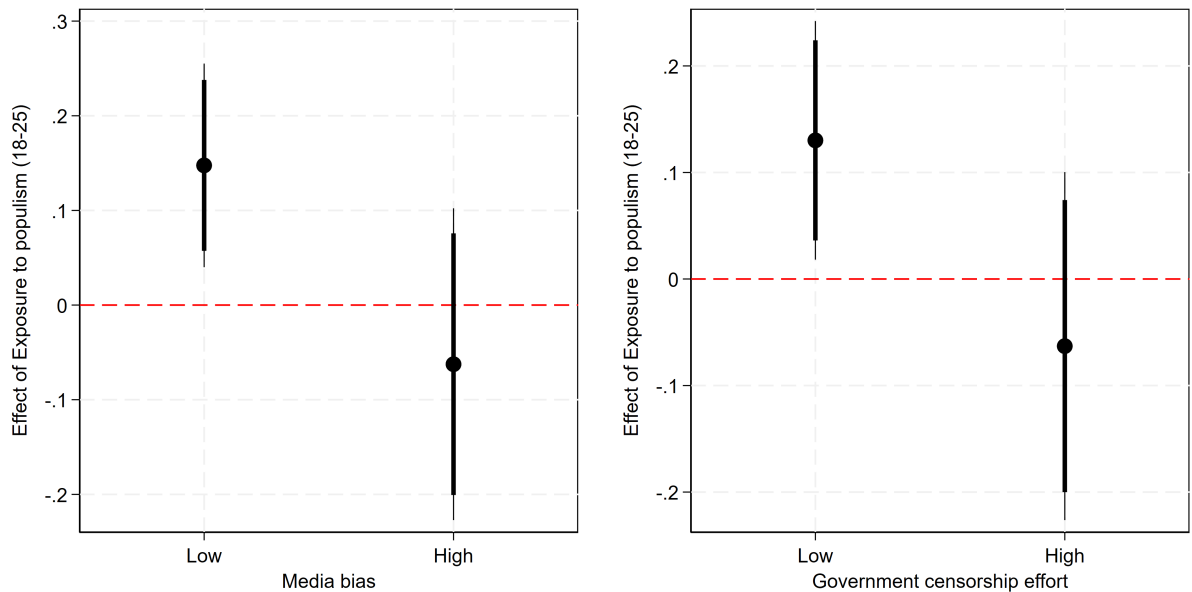
Notes: This figure shows the marginal effect of formative (ages 18 to 25) exposure to populism on zero-sum thinking, evaluated at low (below-median) and high (above-median) values of media bias (top panel) and government censorship effort (bottom panel), with each media moderator averaged over ages 10 to 17 for the relevant cohort. In each panel, populist exposure is additionally interacted with one institutional-constraint moderator: legislative constraints on the executive (left), judicial constraints (centre), and the liberal democracy index (right), each also averaged over ages 10 to 17 for the relevant cohort. The sample and controls are as in the bottom row of Figure 6. Thick and thin whiskers denote 90% and 95% confidence intervals; standard errors are clustered at the country-cohort level.

Figure A.15: Media gradient controlling for institutional quality

Panel (a): Flexible control for democratic quality

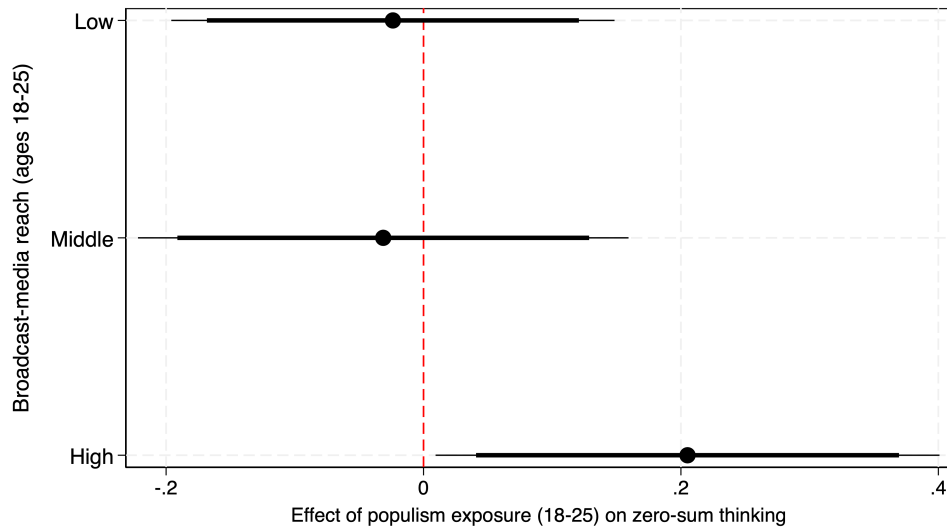


Panel (b): Joint control for all institutional constraints



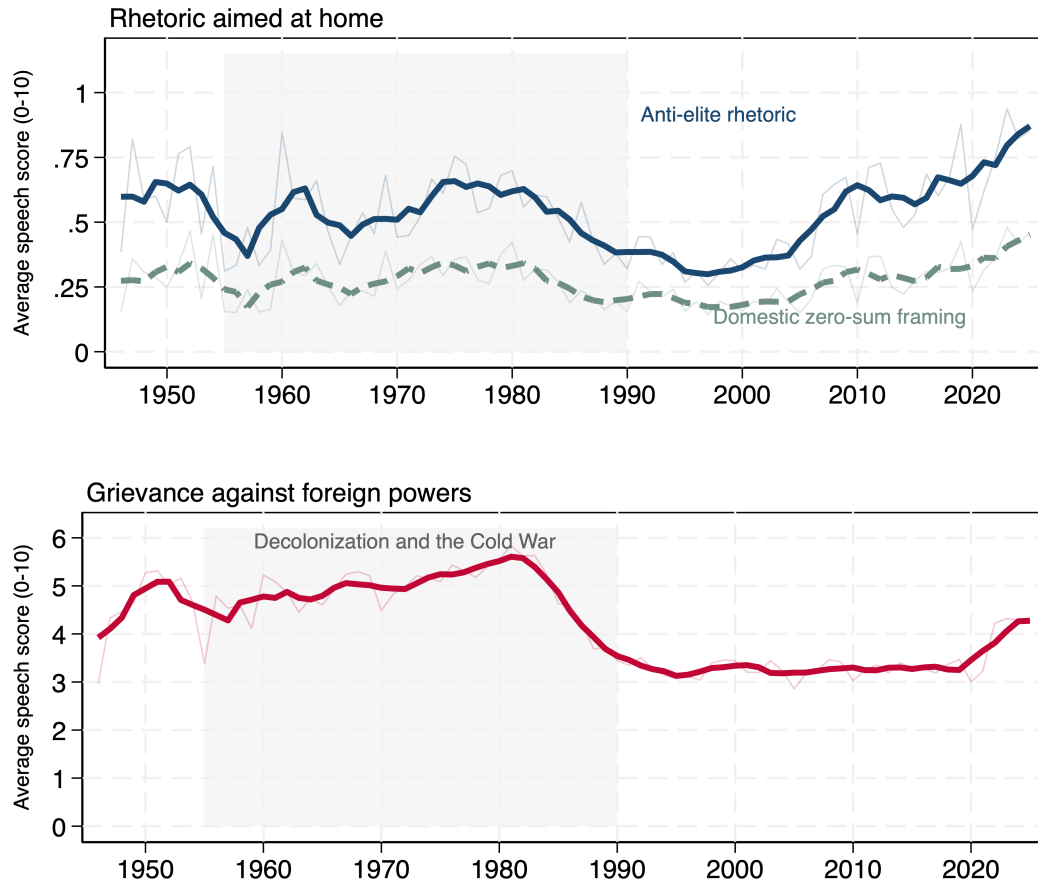
Notes: This figure shows the marginal effect of formative (ages 18 to 25) exposure to populism on zero-sum thinking at low and high values of media bias (left) and government censorship effort (right), with the exposure effect additionally allowed to vary with institutional quality. In Panel (a), populist exposure is interacted with dummies for the four quartiles of the liberal democracy index, with liberal democracy averaged over ages 10 to 17 for each cohort, so the exposure effect varies flexibly with democratic quality. In Panel (b), populist exposure is interacted simultaneously with all three institutional-constraint indicators (legislative constraints, judicial constraints, and liberal democracy), each averaged over ages 10 to 17 for the relevant cohort. The media moderators are likewise averaged over ages 10 to 17, before the formative exposure window. The sample and controls are as in the bottom row of Figure 6. Thick and thin whiskers denote 90% and 95% confidence intervals; standard errors are clustered at the country-cohort level.

Figure A.16: Broadcast reach: tercile specification



Notes: This figure shows the effect of *Exposure to populism (18-25)* on *Zero-sum thinking* separately for cohorts in the bottom, middle, and top tercile of pre-exposure broadcast-media reach (averaged over ages 10 to 17 for each cohort). Broadcast reach is a standardised composite of radio and television receivers per capita, from the Cross-National Time-Series archive (Banks and Wilson, 2024). Each estimate uses the specification of column (4) of Table 1, estimated on the tercile subsample. Standard errors are clustered at the country-cohort level. Thick and thin bars denote 90% and 95% confidence intervals. The continuous version appears as Figure 9.

Figure A.17: Domestic and international conflict rhetoric at the United Nations, 1946–2025



Notes: Each line is the world average of a speech-score component across every United Nations General Debate speech in a given year, 1946–2025; thin lines are annual means and thick lines five-year moving averages. Components are scored from 0 to 10 by a large language model applying the Team Populism rubric (Appendix B). The two panels use different vertical scales. The top panel shows rhetoric aimed at home: anti-elite framing (navy) and domestic zero-sum framing (dashed green), the speech analogue of the zero-sum belief this paper studies. The bottom panel shows grievance against foreign powers (red). Grievance against foreign powers dominates speech at the general debate and falls after the end of the Cold War, whereas domestic anti-elite and zero-sum framing stay low throughout. The shaded band marks the decolonisation and Cold War period, 1955–1990. This contrast is why a keyword measure that cannot separate the two overstates populism in this setting.

Table A.1: Sample composition across countries and survey waves

Country	ISO	Wave	Year	<i>N</i>	Country	ISO	Wave	Year	<i>N</i>	Country	ISO	Wave	Year	<i>N</i>
Argentina	ARG	1989–93	1991	744	Hungary	HUN	2005–09	2009	837	Russia	RUS	2010–14	2011	1,775
Argentina	ARG	1994–98	1995	819	India	IND	1989–93	1990	1,611	Slovakia	SVK	1989–93	1990	376
Argentina	ARG	2005–09	2006	732	India	IND	1994–98	1995	1,183	Slovakia	SVK	1994–98	1998	915
Argentina	ARG	2010–14	2013	723	India	IND	2005–09	2006	1,242	Slovenia	SVN	1994–98	1995	770
Australia	AUS	1994–98	1995	1,615	India	IND	2010–14	2012	2,899	Slovenia	SVN	2005–09	2005	821
Australia	AUS	2005–09	2005	1,241	Indonesia	IDN	2005–09	2006	1,151	Slovenia	SVN	2010–14	2011	820
Australia	AUS	2010–14	2012	1,350	Italy	ITA	2005–09	2005	844	South Africa	ZAF	1989–93	1990	2,049
Brazil	BRA	1989–93	1991	1,253	Japan	JPN	1989–93	1990	602	South Africa	ZAF	1994–98	1996	2,232
Brazil	BRA	1994–98	1997	808	Japan	JPN	1994–98	1995	707	South Africa	ZAF	2005–09	2006	2,136
Brazil	BRA	2005–09	2006	1,142	Japan	JPN	2005–09	2005	818	South Africa	ZAF	2010–14	2013	2,526
Brazil	BRA	2010–14	2014	1,175	Japan	JPN	2010–14	2010	1,758	South Korea	KOR	1989–93	1990	980
Bulgaria	BGR	1994–98	1997	820	Latvia	LVA	1994–98	1996	966	South Korea	KOR	1994–98	1996	1,005
Bulgaria	BGR	2005–09	2006	872	Lithuania	LTU	1994–98	1997	764	South Korea	KOR	2005–09	2005	1,007
Canada	CAN	2005–09	2006	1,825	Malaysia	MYS	2005–09	2006	752	South Korea	KOR	2010–14	2010	1,010
Chile	CHL	1989–93	1990	1,091	Malaysia	MYS	2010–14	2012	1,035	Spain	ESP	1989–93	1990	1,076
Chile	CHL	1994–98	1996	761	Mexico	MEX	1989–93	1990	987	Spain	ESP	1994–98	1995	918
Chile	CHL	2005–09	2006	763	Mexico	MEX	1994–98	1996	1,007	Spain	ESP	2005–09	2007	984
Chile	CHL	2010–14	2012	788	Mexico	MEX	2005–09	2005	1,174	Spain	ESP	2010–14	2011	988
China	CHN	1989–93	1990	766	Mexico	MEX	2010–14	2012	1,471	Sweden	SWE	1994–98	1996	807
China	CHN	1994–98	1995	1,124	Netherlands	NLD	2005–09	2006	830	Sweden	SWE	2005–09	2006	852
China	CHN	2005–09	2007	1,291	Netherlands	NLD	2010–14	2012	1,427	Sweden	SWE	2010–14	2011	819
China	CHN	2010–14	2013	1,678	New Zealand	NZL	1994–98	1998	956	Switzerland	CHE	1994–98	1996	973
Colombia	COL	2005–09	2005	2,296	New Zealand	NZL	2005–09	2004	799	Switzerland	CHE	2005–09	2007	1,140
Colombia	COL	2010–14	2012	1,164	New Zealand	NZL	2010–14	2011	558	Taiwan ROC	TWN	1994–98	1998	704
Croatia	HRV	1994–98	1996	942	Norway	NOR	1994–98	1996	946	Taiwan ROC	TWN	2005–09	2006	1,035
Cyprus	CYP	2005–09	2006	817	Norway	NOR	2005–09	2007	897	Taiwan ROC	TWN	2010–14	2012	938
Cyprus	CYP	2010–14	2011	693	Peru	PER	1994–98	1996	762	Thailand	THA	2005–09	2007	1,352
Czechia	CZE	1989–93	1991	770	Peru	PER	2005–09	2006	1,015	Thailand	THA	2010–14	2013	1,081
Czechia	CZE	1994–98	1998	964	Peru	PER	2010–14	2012	837	Türkiye	TUR	1989–93	1990	708
Ecuador	ECU	2010–14	2013	901	Philippines	PHL	1994–98	1996	865	Türkiye	TUR	1994–98	1996	1,310
Egypt	EGY	2005–09	2008	2,551	Philippines	PHL	2010–14	2012	1,022	Türkiye	TUR	2005–09	2007	957
Egypt	EGY	2010–14	2013	1,228	Poland	POL	1989–93	1989	772	Türkiye	TUR	2010–14	2011	1,211
Estonia	EST	1994–98	1996	862	Poland	POL	1994–98	1997	921	United Kingdom	GBR	2005–09	2005	837
Estonia	EST	2010–14	2011	1,078	Poland	POL	2005–09	2005	758	United States	USA	1994–98	1995	1,315
Finland	FIN	1994–98	1996	771	Poland	POL	2010–14	2012	774	United States	USA	2005–09	2006	1,098
Finland	FIN	2005–09	2005	874	Romania	ROU	1994–98	1998	953	United States	USA	2010–14	2011	1,720
France	FRA	2005–09	2006	856	Romania	ROU	2005–09	2005	1,465	Uruguay	URY	1994–98	1996	814
Germany	DEU	1994–98	1997	1,740	Romania	ROU	2010–14	2012	1,273	Uruguay	URY	2005–09	2006	760
Germany	DEU	2005–09	2006	1,789	Russia	RUS	1989–93	1990	1,423	Uruguay	URY	2010–14	2011	729
Germany	DEU	2010–14	2013	1,582	Russia	RUS	1994–98	1995	1,715	Venezuela	VEN	1994–98	1996	795
Hungary	HUN	1994–98	1998	516	Russia	RUS	2005–09	2006	1,408					

Total: 48 countries, 122 country-waves, 134,302 respondents

Notes: This table shows the composition of the estimation sample. It includes country-waves from WVS waves 2 (1989–1993), 3 (1994–1998), 5 (2005–2009), and 6 (2010–2014) in which the zero-sum thinking question (E041) was asked. *Wave* reports the abbreviated WVS wave date range; *Year* is the year the survey was fielded in that country. *N* is the number of respondents in each country-wave in the estimation sample; the reported total of 134,302 matches columns (1) and (2) of Table 1. The count falls to 134,300 in columns (3) and (4) because `reghdfe` drops two singleton observations, units that are the sole member of a fixed-effect cell and hence contribute no identifying variation, once the cohort fixed effects are absorbed.

Table A.2: Distribution of Exposure to populism (18-25) across country-cohort cells

Exposure (18-25)	Cells	Mean	S.D.	Share = 0	Share partial	Share = 1	Share > 0	Countries
Any populist	3,933	0.074	0.219	87.1%	10.5%	2.4%	12.9%	25
Right-wing populist	3,933	0.030	0.138	93.9%	5.4%	0.7%	6.1%	18
Left-wing populist	3,933	0.043	0.176	92.8%	5.6%	1.6%	7.2%	14

Notes: This table shows the distribution of the treatment variable across the country-cohort cells in the baseline WVS sample over which formative exposure is defined, one observation per (country, birth-cohort) cell. The count exceeds the cluster count reported in Table 1 because estimation drops cells with missing covariates or that are absorbed as singletons by the fixed effects. “Share partial” is the share of cells with $0 < x < 1$ and “Share = 1” is full eight-year exposure. “Countries” counts countries with any cell with positive exposure values.

Table A.3: Exposure to populism and zero-sum thinking, alternate sets of controls

	Zero-sum thinking				
	(1)	(2)	(3)	(4)	(5)
Exposure to populism (18-25)	0.083* (0.045)	0.092** (0.045)	0.083* (0.046)	0.084* (0.045)	0.077* (0.046)
Controls	Male only	Town size	Religion	Marital status	All
Age FEs	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓
Country $\times$ Year FEs	✓	✓	✓	✓	✓
R-squared	0.085	0.088	0.086	0.088	0.087
Mean of outcome	4.567	4.567	4.581	4.566	4.581
SD of outcome	2.709	2.709	2.703	2.709	2.703
Clusters	3,365	3,365	3,352	3,365	3,352
Observations	134,300	134,300	129,685	134,000	129,413

Notes: This table reports OLS coefficient estimates using the baseline WVS sample. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. All columns use the specification of column (4) of Table 1 (single-year age, cohort, and country $\times$ year fixed effects). Column (1) includes a male dummy only, replacing the baseline individual-level controls. Columns (2) to (4) augment the baseline controls with town size, religious affiliation, and marital status, respectively. Column (5) includes all three additional controls jointly. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.4: Exposure to populism and perceived economic conflict, ISSP items separately

Panel A	Poor vs. rich			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.081*** (0.020)	0.079*** (0.020)	0.060*** (0.020)	0.061*** (0.019)
R-squared	0.118	0.127	0.128	0.148
Mean of outcome	2.440	2.440	2.440	2.440
SD of outcome	0.859	0.859	0.859	0.859
Clusters	2,788	2,788	2,788	2,788
Observations	127,201	127,201	127,201	127,201
Panel B	Working class vs. middle class			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.050** (0.022)	0.050** (0.023)	0.035 (0.023)	0.054** (0.021)
R-squared	0.100	0.110	0.110	0.120
Mean of outcome	2.041	2.041	2.041	2.041
SD of outcome	0.775	0.775	0.775	0.775
Clusters	2,705	2,705	2,705	2,705
Observations	124,378	124,378	124,378	124,378
Panel C	Management vs. workers			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.055*** (0.021)	0.054** (0.022)	0.057** (0.022)	0.051** (0.022)
R-squared	0.094	0.099	0.100	0.123
Mean of outcome	2.487	2.487	2.487	2.487
SD of outcome	0.809	0.809	0.809	0.809
Clusters	2,778	2,778	2,778	2,778
Observations	125,782	125,782	125,782	125,782
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓

Notes: This table reports OLS coefficient estimates using the baseline ISSP sample, replacing the combined conflict indices of Table 2 with each of the three underlying items estimated separately. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in Panel A is perceived conflict between poor and rich people, in Panel B between the working class and the middle class, and in Panel C between management and workers, each measured on the original four-point ISSP scale and reverse-coded so that higher values indicate stronger perceived conflict. Columns (1) to (3) additionally control for contemporaneous populist rule. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.5: Controlling for other impressionable-year shocks

	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.142*** (0.050)	0.158*** (0.049)	0.148*** (0.050)	0.100** (0.047)
Exposure to populism (t)	0.437*** (0.050)	0.448*** (0.050)	0.441*** (0.049)	
Exposure to democracy (18-25)	-0.144*** (0.038)	-0.151*** (0.039)	-0.134*** (0.039)	-0.105*** (0.037)
Assassinations (18-25)	-0.032** (0.015)	-0.030** (0.014)	-0.031** (0.014)	-0.015 (0.014)
General strikes (18-25)	0.096*** (0.036)	0.088** (0.035)	0.066** (0.034)	0.036 (0.032)
Terrorism/guerrilla warfare (18-25)	-0.034 (0.032)	-0.041 (0.032)	-0.032 (0.032)	-0.039 (0.031)
Major government crises (18-25)	0.008 (0.036)	0.004 (0.036)	0.000 (0.035)	0.011 (0.034)
Purges (18-25)	-0.003 (0.021)	0.005 (0.022)	0.005 (0.021)	0.011 (0.019)
Riots (18-25)	0.000 (0.009)	0.003 (0.009)	0.002 (0.009)	-0.010 (0.009)
Revolutions (18-25)	-0.101** (0.040)	-0.094** (0.039)	-0.097** (0.040)	-0.030 (0.037)
Anti-government demonstrations (18-25)	-0.016* (0.009)	-0.016* (0.009)	-0.015* (0.009)	-0.003 (0.008)
Domestic shock (missing)	-0.088** (0.045)	-0.085* (0.045)	-0.072 (0.056)	-0.014 (0.054)
ln GDP per capita (18-25)	-0.004 (0.040)	0.005 (0.040)	0.005 (0.040)	0.056 (0.040)
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country $\times$ Year FEs				✓
R-squared	0.069	0.073	0.074	0.085
Mean of outcome	4.496	4.496	4.496	4.496
SD of outcome	2.681	2.681	2.681	2.681
Clusters	2,747	2,747	2,745	2,744
Observations	114,497	114,497	114,495	114,494

Notes: This table reports OLS coefficient estimates using the WVS sample for which shock data are available. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. Each shock variable measures the average annual value of the shock experienced between ages 18 and 25. Exposure to democracy is drawn from [Boix et al. \(2013\)](#); GDP is obtained from [Bolt and Van Zanden \(2025\)](#); all other shocks (assassinations, general strikes, terrorism/guerrilla warfare, major government crises, purges, riots, revolutions, and anti-government demonstrations) are from [Banks and Wilson \(2024\)](#). *Domestic shock (missing)* is an indicator equal to one if any shock variable is missing, with missing values set to zero. Columns (1) to (3) include a control for contemporaneous exposure to populism. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.6: Controlling for leader characteristics during the impressionable years

	Zero-sum thinking													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Exposure to populism (18-25)	0.113** (0.051)	0.107** (0.051)	0.126** (0.050)	0.110** (0.051)	0.118** (0.051)	0.124** (0.051)	0.120** (0.050)	0.122** (0.050)	0.095* (0.052)	0.119** (0.050)	0.119** (0.050)	0.120** (0.050)	0.120** (0.050)	0.093* (0.054)
Male	0.080 (0.072)													0.102 (0.075)
Ever been imprisoned		0.052 (0.035)												0.023 (0.040)
Revolutionary movement			0.077** (0.038)											0.033 (0.046)
Law degree				0.041 (0.031)										0.028 (0.034)
PhD degree					-0.025 (0.047)									0.018 (0.055)
University education						-0.072* (0.041)								-0.062 (0.046)
Western university education							-0.013 (0.037)							0.000 (0.041)
Prior business career								-0.063 (0.055)						-0.046 (0.059)
Prior political career									0.070** (0.031)					0.041 (0.037)
Prior international development career										-0.180 (0.156)				-0.198 (0.162)
Prior public servant career											-0.051 (0.075)			-0.026 (0.080)
Prior academic career												-0.017 (0.060)		0.009 (0.072)
Interim leader													-0.132 (0.094)	-0.095 (0.095)
Age FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country × Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R-squared	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088
Mean of outcome	4.559	4.559	4.559	4.559	4.559	4.559	4.559	4.559	4.559	4.559	4.559	4.559	4.559	4.559
SD of outcome	2.688	2.688	2.688	2.688	2.688	2.688	2.688	2.688	2.688	2.688	2.688	2.688	2.688	2.688
Clusters	1,797	1,797	1,797	1,795	1,795	1,797	1,797	1,797	1,797	1,797	1,797	1,797	1,797	1,795
Observations	102,177	102,177	102,177	102,167	102,167	102,177	102,177	102,177	102,177	102,177	102,177	102,177	102,177	102,167

Notes: This table reports OLS coefficient estimates using the WVS sample for which leader-characteristics data are available. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. Each leader-characteristic variable measures the average annual value of the characteristic experienced between ages 18 and 25. Columns (1) to (13) add one leader characteristic at a time; column (14) includes all characteristics jointly. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.7: Controlling for leader ideology

	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.142*** (0.050)	0.158*** (0.050)	0.158*** (0.050)	0.100** (0.046)
Left-wing leader (18-25)	0.002 (0.133)	0.062 (0.134)	0.120 (0.130)	0.107 (0.124)
Right-wing leader (18-25)	-0.028 (0.131)	0.040 (0.131)	0.096 (0.129)	0.093 (0.123)
Centrist leader (18-25)	0.029 (0.138)	0.086 (0.139)	0.160 (0.137)	0.172 (0.129)
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓
R-squared	0.070	0.075	0.075	0.089
Mean of outcome	4.565	4.565	4.565	4.565
SD of outcome	2.711	2.711	2.711	2.711
Clusters	2,934	2,934	2,934	2,934
Observations	129,232	129,232	129,232	129,232

Notes: This table reports OLS coefficient estimates augmenting the baseline specification with controls for the ideological character of leaders during respondents' impressionable years. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. *Left-wing leader (18-25)*, *Right-wing leader (18-25)*, and *Centrist leader (18-25)* measure the fraction of years aged 18 to 25 spent under a leader of each ideological type, as classified by the Global Leader Ideologies dataset (Herre, 2023). The dataset assigns each leader-year to one of four categories, left-wing, right-wing, centrist, or missing or unclassified; the three ideology shares are entered jointly with the missing or unclassified share as the omitted category, so they do not sum to one, and respondents whose entire ages 18 to 25 window is missing or unclassified are excluded. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.8: Exposure to populism and zero-sum thinking, post-1945 birth cohorts

	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.201*** (0.056)	0.212*** (0.055)	0.215*** (0.054)	0.113** (0.050)
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country $\times$ Year FEs				✓
R-squared	0.071	0.076	0.077	0.091
Mean of outcome	4.592	4.592	4.592	4.592
SD of outcome	2.695	2.695	2.695	2.695
Clusters	1,814	1,814	1,814	1,814
Observations	100,669	100,669	100,669	100,669

Notes: This table reports OLS coefficient estimates replicating Table 1 on the subsample of WVS respondents born after 1945. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. Columns (1) to (3) include a control for contemporaneous exposure to populism. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.9: Exposure to populism and zero-sum thinking, alternate weighting approaches

Panel A	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.133*** (0.049)	0.150*** (0.048)	0.151*** (0.047)	0.093** (0.043)
R-squared	0.069	0.074	0.075	0.089
Mean of outcome	4.567	4.567	4.567	4.567
SD of outcome	2.709	2.709	2.709	2.709
Clusters	3,367	3,367	3,365	3,365
Observations	134,302	134,302	134,300	134,300
Panel B	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.132*** (0.049)	0.150*** (0.049)	0.153*** (0.048)	0.084** (0.043)
R-squared	0.070	0.074	0.075	0.090
Mean of outcome	4.567	4.567	4.567	4.567
SD of outcome	2.709	2.709	2.709	2.709
Clusters	3,367	3,367	3,365	3,365
Observations	134,302	134,302	134,300	134,300
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓

Notes: This table reports OLS coefficient estimates replicating Table 1 under alternative weighting schemes. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. Panel A uses unweighted regressions. Panel B applies the WVS post-stratification weight S017. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.10: Exposure to populism and zero-sum thinking, controlling for proxies of parental influence

Panel A	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.136*** (0.049)	0.153*** (0.049)	0.147*** (0.049)	0.087* (0.045)
Live with parents	0.025 (0.024)	0.001 (0.024)	0.003 (0.025)	0.005 (0.025)
R-squared	0.070	0.075	0.076	0.088
Mean of outcome	4.555	4.555	4.555	4.555
SD of outcome	2.715	2.715	2.715	2.715
Clusters	3,147	3,147	3,145	3,145
Observations	124,937	124,937	124,935	124,935
Panel B	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.177*** (0.055)	0.193*** (0.054)	0.191*** (0.054)	0.098* (0.051)
Make parents proud	-0.183*** (0.020)	-0.204*** (0.020)	-0.201*** (0.020)	-0.209*** (0.020)
R-squared	0.072	0.076	0.077	0.090
Mean of outcome	4.600	4.600	4.600	4.600
SD of outcome	2.662	2.662	2.662	2.662
Clusters	3,288	3,288	3,286	3,286
Observations	111,649	111,649	111,647	111,647
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓

Notes: This table reports OLS coefficient estimates using the baseline WVS sample, augmenting the specifications of Table 1 with controls for parental influence. *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. As in Table 1, columns (1) to (3) include a control for contemporaneous exposure to populism. Panel A adds a binary control for whether the respondent currently lives with their parents. Panel B adds a binary control for whether the respondent reports that making their parents proud is an important personal value. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.11: Selective migration

	Migrant			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25) [birth country]	0.000 (0.001)	0.000 (0.001)	−0.000 (0.001)	0.000 (0.001)
Origin-country FEs	✓	✓	✓	✓
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓
R-squared	0.873	0.873	0.873	0.873
Mean of outcome	0.040	0.040	0.040	0.040
SD of outcome	0.195	0.195	0.195	0.195
Clusters	2,820	2,820	2,815	2,815
Observations	83,504	83,504	83,499	83,499

Notes: This table examines whether formative populist exposure in the respondent's origin country predicts being observed as a migrant in the WVS. The dependent variable is a binary indicator equal to one if the respondent was surveyed outside their country of birth. *Exposure to populism (18-25) [birth country]* is the proportion of years aged 18 to 25 that the respondent's origin country was governed by a populist leader. All specifications include origin-country and single-year age fixed effects. Survey-country, survey-year, cohort, and survey-country × survey-year fixed effects are included as indicated. Standard errors are clustered at the origin-country × cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.12: Inherited versus acceded formative exposure

	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Inherited exposure (18-25)	0.122** (0.057)	0.145*** (0.056)	0.147*** (0.056)	0.104** (0.050)
Acceded exposure (18-25)	0.159** (0.078)	0.165** (0.078)	0.154** (0.077)	0.068 (0.075)
<i>p</i> -value: inherited = acceded	0.671	0.815	0.936	0.665
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country $\times$ Year FEs				✓
R-squared	0.069	0.073	0.074	0.088
Mean of outcome	4.567	4.567	4.567	4.567
SD of outcome	2.709	2.709	2.709	2.709
Clusters	3,367	3,367	3,365	3,365
Observations	134,302	134,302	134,300	134,300

Notes: This table splits formative exposure to populism into two components. *Inherited exposure (18-25)* is the share of ages 18 to 25 spent under a populist who was already in office when the respondent turned 18; *acceded exposure (18-25)* is the share spent under a populist who first took office during ages 19 to 25. The two components sum to total formative exposure. Because an inherited populist was already governing before the cohort was old enough to vote, inherited exposure cannot reflect the cohort's own part in bringing that leader to power, which addresses the concern that reverse causality operates through the cohort's first-time voting. The dependent variable is zero-sum thinking on a 1 to 10 scale. Columns (1) to (3) additionally control for contemporaneous populist rule; column (4) absorbs it through country-by-survey-year fixed effects. Column specifications match Table 1. Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.13: Exposure to populism and zero-sum thinking, controlling for other correlates

Panel A	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.155*** (0.052)	0.172*** (0.051)	0.193*** (0.051)	0.094** (0.047)
Pre-tax top 1% income share (18-25)	1.006** (0.412)	0.980** (0.410)	1.445*** (0.415)	-0.466 (0.396)
R-squared	0.068	0.072	0.073	0.088
Mean of outcome	4.554	4.554	4.554	4.554
SD of outcome	2.689	2.689	2.689	2.689
Clusters	2,391	2,391	2,388	2,388
Observations	110,594	110,594	110,590	110,590
Panel B	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.134*** (0.050)	0.150*** (0.049)	0.148*** (0.049)	0.092** (0.045)
Mother born abroad	-0.085 (0.064)	-0.083 (0.064)	-0.084 (0.064)	-0.038 (0.063)
Father born abroad	-0.002 (0.062)	-0.004 (0.062)	-0.006 (0.062)	0.009 (0.061)
R-squared	0.069	0.073	0.074	0.088
Mean of outcome	4.567	4.567	4.567	4.567
SD of outcome	2.709	2.709	2.709	2.709
Clusters	3,367	3,367	3,365	3,365
Observations	134,302	134,302	134,300	134,300
Panel C	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Exposure to populism (18-25)	0.135*** (0.050)	0.151*** (0.049)	0.149*** (0.049)	0.092** (0.045)
Black (US)	0.024 (0.117)	-0.016 (0.117)	-0.019 (0.118)	-0.024 (0.117)
R-squared	0.069	0.073	0.074	0.088
Mean of outcome	4.567	4.567	4.567	4.567
SD of outcome	2.709	2.709	2.709	2.709
Clusters	3,367	3,367	3,365	3,365
Observations	134,302	134,302	134,300	134,300
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓

Notes: This table reports OLS coefficient estimates using the baseline WVS sample, augmenting the specifications of Table 1 with other correlates of zero-sum thinking highlighted by Chinoy et al. (2026). *Exposure to populism (18-25)* is the proportion of years aged 18 to 25 that the individual spent living under a populist leader. The dependent variable in all columns is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. As in Table 1, columns (1) to (3) include a control for contemporaneous exposure to populism. Panel A controls for the country's pre-tax top 1% income share, averaged over the respondent's impressionable years (ages 18 to 25). Panel B controls for whether the respondent's mother and father were each born outside the country of residence. Panel C controls for an indicator for Black respondents in the United States, following the historical-determinants analysis in Chinoy et al. (2026). Standard errors are clustered at the country-cohort level and are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.14: Formative exposure to economic hardship without populist rule

Panel A	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Recession under non-populist (18-25)	0.061 (0.054)	0.049 (0.054)	0.048 (0.056)	−0.039 (0.052)
Panel B	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Populist exposure (18-25)	0.155*** (0.052)	0.170*** (0.052)	0.164*** (0.051)	0.083* (0.047)
Recession under non-populist (18-25)	0.091 (0.056)	0.082 (0.056)	0.079 (0.057)	−0.023 (0.054)
<i>p</i> -value: $\beta_{pop} = \beta_{rec}$	0.321	0.172	0.199	0.090
Panel C	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Populist, no recession (18-25)	0.286*** (0.078)	0.306*** (0.077)	0.329*** (0.077)	0.134* (0.072)
Populist + recession (18-25)	−0.210 (0.168)	−0.211 (0.170)	−0.293* (0.166)	−0.056 (0.155)
Non-populist recession (18-25)	0.090 (0.056)	0.081 (0.056)	0.070 (0.057)	−0.025 (0.054)
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓
Mean of outcome	4.548	4.548	4.548	4.548
SD of outcome	2.701	2.701	2.701	2.701
Clusters	3,120	3,120	3,118	3,118
Observations	130,360	130,360	130,358	130,358

Notes: This table reports OLS estimates using the WVS sample. The dependent variable is *Zero-sum thinking*, which takes values between 1 and 10, with higher values indicating more zero-sum thinking. *Recession under non-populist (18-25)* is the fraction of years aged 18 to 25 with declining GDP per capita, from the Maddison Project (Bolt and Van Zanden, 2025), while no populist held office; *Populist exposure (18-25)* is formative populist exposure. Panel B enters both measures jointly and reports the *p*-value of a test of equality of the two coefficients. Panel C partitions formative exposure into populist without recession, populist with recession, and non-populist recession, with non-populist and no recession as the omitted category. Columns (1) to (3) include a control for contemporaneous exposure to populism. All specifications use survey probability weights and cluster standard errors at the country-cohort level. The sample is smaller than the main specification because six post-communist countries lack Maddison coverage over the relevant formative years. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.15: Formative populist exposure by economic conditions

	Zero-sum thinking			
	(1)	(2)	(3)	(4)
Populist, good economy (18-25)	0.268*** (0.076)	0.289*** (0.075)	0.321*** (0.076)	0.137* (0.072)
Populist, bad economy (18-25)	-0.233 (0.168)	-0.231 (0.170)	-0.320* (0.166)	-0.046 (0.155)
Country FEs	✓	✓	✓	
Year FEs	✓	✓	✓	
Age FEs	✓	✓	✓	✓
Controls		✓	✓	✓
Cohort FEs			✓	✓
Country × Year FEs				✓
R-squared	0.066	0.071	0.072	0.086
Mean of outcome	4.548	4.548	4.548	4.548
SD of outcome	2.701	2.701	2.701	2.701
Clusters	3,120	3,120	3,118	3,118
Observations	130,360	130,360	130,358	130,358

Notes: This table reports OLS estimates from the WVS. Formative populist exposure (ages 18 to 25) is split into two mutually exclusive components based on the contemporaneous state of the economy: the fraction of impressionable years spent under a populist during positive GDP-per-capita growth (good economy) and during negative growth (bad economy), classified using the Maddison Project ([Bolt and Van Zanden, 2025](#)). The dependent variable is *Zero-sum thinking*. Columns (1) to (3) include a control for contemporaneous exposure to populism. Standard errors are clustered at the country-cohort level and reported in parentheses; survey probability weights are applied. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.16: Populist government and the content of governing rhetoric

	V-Party score of the head-of-government party		
	Populism index (1)	Anti-elitism (2)	People-centrism (3)
Populist government (Funke et al.)	0.273*** (0.038)	1.562*** (0.261)	1.181*** (0.179)
Effect (SD units)	1.17	1.14	0.82
Country FEs	✓	✓	✓
Year FEs	✓	✓	✓
Observations	2,695	2,695	2,695

Notes: This table shows OLS estimates from the country-year panel for which both V-Party measures and the [Funke et al. \(2023\)](#) populist-government classification are available. The dependent variable in each column is a V-Party score for the head-of-government party: the overall populism index (column 1), anti-elitism, or people-versus-elite antagonism (column 2), and people-centrism (column 3). The regressor is an indicator equal to one if a populist headed the government that year, as classified by [Funke et al. \(2023\)](#). Each column is a separate regression with country and year fixed effects, and standard errors clustered by country are reported in parentheses. The row ‘Effect (SD units)’ expresses each coefficient in standard-deviation units of the dependent variable. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.17: Populist government and rhetoric at the United Nations

	Populism composite (1)	Anti- elitism (2)	People- centrism (3)	Zero-sum (domestic) (4)	Zero-sum (int'l) (5)
Populist government	0.654*** (0.226)	0.583*** (0.186)	0.506*** (0.161)	0.668*** (0.214)	0.373** (0.140)
Country FEs	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓
R-squared	0.304	0.270	0.307	0.248	0.390
Clusters	59	59	59	59	59
Observations	3,614	3,614	3,614	3,614	3,614

Notes: This table reports OLS estimates from a country-year panel of speeches delivered at the annual general debate of the United Nations, 1946 to 2025 (Baturio et al., 2017), each scored with a large language model applying the Team Populism holistic rubric (Appendix B). The dependent variable in each column is a component of populist rhetoric, standardised within calendar year to have mean zero and unit standard deviation, so that coefficients are expressed in standard-deviation units. *Populist government* is the Funke et al. (2023) indicator for a populist head of government. The populism composite averages domestic anti-elitism, Manichaeian framing, and domestic zero-sum framing. *Zero-sum (domestic)* captures framing of domestic groups as gaining at one another's expense; *Zero-sum (int'l)* captures nation-versus-nation framing. The regression sample is restricted to country-years that can be matched to the Funke et al. (2023) populist-government classification. All columns include country and year fixed effects, with standard errors clustered by country reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.18: Populism exposure and zero-sum thinking, by media reach

	Zero-sum thinking				
	Radio (1)	TV (2)	Newspapers (3)	Broadcast (4)	Composite (5)
Populism exposure (18-25) $\times$ Reach	0.092** (0.043)	0.125** (0.050)	0.018 (0.049)	0.124*** (0.045)	0.094** (0.044)
Populism exposure (18-25)	0.049 (0.045)	0.075 (0.047)	0.075 (0.048)	0.045 (0.044)	0.048 (0.045)
Country $\times$ Year FEs	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓
Age FEs	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	102,228	94,789	98,810	104,371	104,510

Notes: This table reports OLS estimates of the association between impressionable-years populism exposure and zero-sum thinking, allowing the association to vary with the reach of different media over ages 10 to 17, before the formative exposure window. Media reach is measured by per-capita receivers (radio, television) or per-capita circulation (newspapers) from the Cross-National Time-Series archive (Banks and Wilson, 2024), averaged over ages 10 to 17 and standardised to have unit standard deviation within the estimation sample. Columns (1) to (3) use each medium separately; *Broadcast* (4) is a standardised composite of radio and television, the mass channels through which a leader can address citizens directly, and *Composite* (5) adds newspapers. Each column is a separate regression using the specification of column (4) of Table 1: country $\times$ year, cohort, and age fixed effects and the individual controls, with standard errors clustered at the country-cohort level and reported in parentheses. The interaction is positive and statistically significant for radio, television, and the broadcast composite, but small and statistically insignificant for newspaper circulation. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.19: Composition of the matched voting sample

Country	ISO	Survey years	Birth cohorts	<i>N</i>
Bulgaria	BGR	1997–2006	1905–1980	1,391
Croatia	HRV	1996	1907–1970	885
Cyprus	CYP	2006–2019	1915–1993	1,793
Czechia	CZE	1991–2022	1908–1995	2,566
Estonia	EST	1996–2011	1918–1985	1,734
Finland	FIN	1996–2005	1911–1979	1,438
Germany	DEU	1997–2018	1907–1992	5,359
Greece	GRC	2017	1927–1991	640
Hungary	HUN	1998–2009	1911–1983	1,237
Latvia	LVA	1996	1911–1970	805
Lithuania	LTU	1997	1912–1971	616
Netherlands	NLD	2012–2022	1922–1995	3,156
Norway	NOR	1996–2007	1917–1981	1,777
Poland	POL	1989–2012	1906–1986	2,694
Romania	ROU	1998–2018	1905–1992	4,026
Slovakia	SVK	1990–2022	1908–1995	2,265
Slovenia	SVN	1995–2011	1912–1985	1,952
Spain	ESP	1990–2011	1902–1985	3,698
Sweden	SWE	1996–2011	1920–1985	3,235
Switzerland	CHE	1989–2007	1905–1980	2,728
United Kingdom	GBR	1998–2022	1906–1995	2,528

Total: 21 countries, 46,523 respondents

Notes: This table shows the composition of the WVS sample matched to the PopuList that underlies Table 3. All columns of Table 3 use the same fixed-effect and control structure as column (4) of Table 1; the estimation sample differs from the main zero-sum sample because it is restricted to respondents whose stated party is matched in the PopuList. For each of the 21 countries we report the ISO code, the range of WVS survey waves in the matched sample, the range of birth cohorts, and the number of matched respondents. The matched sample covers 21 European countries and 46,523 respondents surveyed between 1989 and 2022.

B Measuring populist rhetoric at the United Nations

Our mechanism argument is that exposure to populist rule also means exposure to a political environment in which leaders more strongly frame politics as a conflict between “the people” and an opposing elite. This appendix provides an independent, text-based test of that premise. We use speeches from the annual United Nations General Debate to ask whether country-years governed by populist leaders are characterised by systematically more anti-elite and zero-sum rhetoric. The UN setting is useful because it provides a common corpus covering a large number of countries over a long period. We do not interpret these speeches as messages that respondents in our surveys necessarily observed. Instead, we use them as a standardised sample of political rhetoric with which to compare the language used under populist and non-populist rule. We describe the corpus, the measurement challenge created by the international setting, our scoring procedure, and the validation exercises below.

The corpus. We use the United Nations General Debate Corpus ([Baturu et al., 2017](#)), which collects the statement delivered by each member state at the opening general debate of every session of the UN General Assembly. We use the full corpus from 1946 to 2025, comprising 11,141 speeches and approximately 32 million words from roughly 200 states. We successfully score 11,140 speeches; the sole exception is an empty placeholder file for a 2025 session. Each speech is distributed as a plain-text file identified by the speaker’s country, session, and year. We parse these identifiers to recover the country and year, read the complete speech text, and match each speech to our country-year panel. No speech is excluded on the basis of its content or resulting score.

The General Debate is particularly useful for our purpose because it is annual, has near-universal country coverage, and places governments in a broadly common institutional setting. The speeches are delivered by heads of state or government or by their representatives under the same general diplomatic conventions. This reduces differences in setting that would arise from comparing heterogeneous domestic speeches and allows us to ask whether rhetoric systematically changes when a country is governed by a populist leader.

The measurement problem. The international setting also creates an important measurement challenge. Populism is normally identified from domestic political communication, where anti-elite rhetoric is directed at a national establishment. At the United Nations, however, governments frequently criticise foreign powers, former colonial powers, wealthy countries, or international institutions. The same words can therefore describe two conceptually different forms of conflict.

In our corpus, a dictionary-based measure constructed from standard populism word lists ([Rooduijn and Pauwels, 2011](#)) captures people-centric language reasonably well but does not cleanly distinguish domestic anti-elitism from international grievance. Terms such as “exploitation,” “powerful,” or “elite” may appear in a domestic people-versus-elite argument, but they may also describe conflict between countries. A speech criticising former colonial powers or international institutions can therefore receive a high anti-elite score even when it contains no domestic appeal pitting ordinary citizens against a national elite.

This distinction matters directly for our mechanism. Our argument concerns the domestic antagonism between “the people” and “the elite” that is central to populist rhetoric, not grievances between nations. A useful measure must therefore distinguish domestic anti-elite and zero-sum

framing from international conflict rather than treating the two as equivalent. Appendix Figure A.17 illustrates the importance of this distinction. International grievances account for a substantial share of conflictual language in the General Debate and vary strongly with episodes such as decolonisation and the Cold War, whereas domestic anti-elite rhetoric follows a different pattern.

Scoring procedure. To make this distinction, we score each speech with a large language model using a fixed prompt based on the holistic-grading approach developed in the Team Populism literature (Hawkins, 2009). We adapt this approach to the specific measurement problem posed by UN speeches by requiring the model to distinguish domestic people-versus-elite rhetoric from grievances directed at foreign actors. The complete prompt is reproduced at the end of this appendix.

The scoring rubric defines populism as a Manichaeian discourse that presents a virtuous “people” in opposition to a corrupt or self-serving domestic “elite.” For each speech, the model returns a structured JSON object containing an overall populism grade from 0 to 2 and separate 0-to-10 scores for anti-elitism, people-centrism, Manichaeian framing, domestic zero-sum framing, and international zero-sum framing. It also records the identity of the domestic elite named in the speech, if any, and a model-reported confidence score.

The key feature of the procedure is that domestic and international conflict are coded separately. Anti-elitism and domestic zero-sum framing receive positive scores only when the speech targets a domestic elite or presents conflict between groups within the country. Criticism of foreign governments, former colonisers, or international institutions is instead assigned to the international zero-sum measure. The advantage over a fixed dictionary is therefore contextual. The model can identify who is portrayed as benefiting at whose expense and distinguish an attack on a domestic establishment from a grievance against a foreign power even when the language used is similar.

Concretely, we use Anthropic’s Claude Haiku 4.5 model, snapshot `claude-haiku-4-5-20251001`, through the Messages API (API version 2023-06-01) in July 2026. Each of the 11,140 speeches is scored in a separate call. The fixed rubric is supplied as the system prompt and the raw speech text as the sole user message. We allow up to 60,000 characters of input, which exceeds the length of every non-empty speech in the corpus, so no scored speech is truncated. We set the sampling temperature to zero to reduce sampling variation and use a maximum of 400 output tokens.

Importantly, the scoring procedure is blind to our treatment classification. The model is never told whether the country-year is classified as populist and is not shown the Funke et al. (2023) populist coding or any other information about the country beyond the text of the speech itself. The returned JSON object is parsed automatically. Calls that fail for transient reasons are retried up to four times with exponential backoff, while outputs that cannot be parsed as valid JSON are re-queued rather than imputed. The empty 2025 placeholder is the only file for which no score is produced.

We then match the scores to the country-year panel and standardise each rhetorical measure within calendar year. This puts the outcomes in standard-deviation units relative to other speeches delivered in the same year and removes common year-specific shifts in the level or dispersion of the scores.

Validation. We use two exercises to assess whether the resulting measures capture the rhetorical differences relevant to our mechanism. First, we compare rhetoric under populist and non-populist rule in country-year regressions with country and year fixed effects. The scored corpus covers

roughly 200 states, whereas this comparison necessarily uses the subset of country-years for which the [Funke et al. \(2023\)](#) populist-government classification is available, and therefore for which the treatment can be defined. Because the speeches were scored without information on the [Funke et al. \(2023\)](#) classification, this comparison is independent of the text-scoring procedure itself. Country-years governed by populist leaders display substantially more anti-elite, people-centric, and domestic zero-sum rhetoric. Anti-elitism is approximately 0.58 standard deviations higher, people-centrism 0.51 standard deviations higher, and domestic zero-sum framing 0.67 standard deviations higher. International zero-sum grievances also increase, but by a smaller amount: 0.37 standard deviations ([Appendix Table A.17](#)). Thus, the largest rhetorical differences occur precisely in the domestic dimensions most closely connected to the people-versus-elite conflict underlying our mechanism.

Second, we compare the text-based scores with an independent expert-coded measure. Across the more than 6,400 country-years for which both measures are available, the UN-based anti-elitism score is positively correlated with V-Party’s expert-coded anti-elitism measure for the head of government’s party, with a correlation coefficient of 0.24. The corresponding dictionary-based measure does not reproduce this relationship. This comparison provides additional evidence that separating domestic anti-elite rhetoric from international grievance improves the measurement of the concept we seek to capture.

These exercises support the interpretation underlying the mechanism analysis. Country-years classified as populist are characterised not simply by more conflictual language in general, but specifically by more anti-elite and domestic zero-sum rhetoric. The UN evidence therefore provides an independent measure of the rhetorical environment associated with populist rule, rather than a direct measure of the political messages observed by the respondents in our surveys.

The scoring prompt. For replicability, the complete system prompt passed to the model is reproduced below, verbatim.

You are an expert political-science coder applying the Team Populism holistic-grading rubric to a speech given by a country's representative at the United Nations General Debate.

Populism is a Manichaeian political discourse that (1) exalts "the people" as a homogeneous, virtuous community, (2) vilifies a conspiring, self-serving "elite" or "establishment", and (3) frames politics as a moral struggle between them.

Read the speech and grade it. CRUCIAL DISTINCTION: score anti_elite and zerosum_domestic ONLY for framing that targets a DOMESTIC elite/establishment or pits domestic groups (people vs elites, class vs class, workers vs owners) against each other as antagonists. Grievances against FOREIGN powers, former colonizers, other nations, or international institutions are NOT anti-elite; put nation-vs-nation "they gain at our expense" framing in zerosum_international instead.

Return ONLY a JSON object, no prose:

```
{
  "populism_grade": 0|1|2,      // 0 = pluralist/not populist,
                                // 1 = mixed, 2 = strongly populist
  "anti_elite": 0-10,          // vilification of a DOMESTIC elite/
                                // establishment as corrupt/self-serving
  "people_centric": 0-10,      // exaltation of "the people"/ordinary
                                // citizens as virtuous
  "manichaeian": 0-10,         // framing politics as a good-vs-evil
                                // struggle between antagonistic groups
  "zerosum_domestic": 0-10,    // DOMESTIC relations framed as one group
                                // gaining at another's expense
  "zerosum_international": 0-10, // relations between NATIONS framed as
                                // zero-sum (they gain, we lose)
  "elite_named": "short string", // who the domestic elite is, or "none"
  "confidence": 0.0-1.0
}
```