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Political polarization, public debt, and sovereign rating downgrades

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"The repeated debt-limit political standoffs and last-minute resolutions have eroded confidence in fiscal management." —Fitch Ratings (2023), on downgrading the United States' long-term rating to 'AA+' from 'AAA'.

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ABSTRACT

We examine whether political polarization changes the credit consequences of public debt. Using sovereign ratings from Moody's, Fitch, and DBRS merged with fiscal data and a measure of political polarization from the V-Dem Country–Year dataset for 1996–2025, we find that debt is more strongly associated with downgrade risk when polarization is high. The pattern is most visible in rating actions rather than rating levels, which change infrequently. In pooled discrete-time hazard models, the debt × polarization interaction is positive and statistically significant; its magnitude remains similar across alternative event definitions, timing checks, and controls for persistent country characteristics, although stricter event definitions and within-country specifications are less precise. The data also show that combinations of very high debt and very high polarization are sparsely observed, so we focus interpretation on the supported range. Tests of subsequent primary-balance adjustment do not establish a fiscal-adjustment mechanism. The evidence therefore supports a conditional, predictive relationship: political polarization is associated with a steeper increase in sovereign downgrade risk as public debt rises, without identifying a unique causal channel.

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1. Introduction

Rating agencies increasingly invoke political conditions when assessing sovereign credit risk. Downgrading the United States in 2011, S&P pointed to "political brinkmanship" (Standard and Poor's, 2011); downgrading it again in 2023, Fitch gave a closely related diagnosis, quoted above (Fitch Ratings, 2023). Neither agency's own language is "polarization" in the V-Dem sense we use

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below, but both describe the same underlying phenomenon — an inability of the political system to resolve fiscal disagreement predictably — that motivates this paper. This paper asks whether political polarization changes the *marginal* credit consequence of public debt. The hypothesis is conditional: the same debt ratio may carry more downgrade risk when polarization is high. One possible interpretation is a fiscal-adjustment channel — political conflict may make consolidation harder when debt is elevated (Ghosh et al., 2013; Bi, 2012; Reinhart et al., 2003) — but our polarization measure is not itself a measure of legislative gridlock, coalition instability, or fiscal credibility. We therefore distinguish throughout between the measured political state and the fiscal mechanism that might connect that state to sovereign risk.

Testing this requires confronting how ratings behave. Ratings are attractive for the question—they exist for far more sovereigns than have a liquid benchmark bond, and agencies assess repayment capacity and willingness directly rather than bundling them with currency and liquidity risk—but they are *sticky*: revised infrequently and in discrete notches, so within a country the level barely moves from year to year. Stickiness is not a nuisance to be differenced away; it dictates the estimable margin. An estimator that identifies a debt × polarization interaction from within-country variation in the rating *level* (two-way fixed-effects linear, fixed-effects ordered logit) is starved of variation, whereas the *flow* — the discrete downgrade event — retains the cross-partial of interest. The identifying object is therefore the downgrade, not the level.

This places the paper among three literatures while differing from each. A first strand prices political characteristics of sovereigns—institutions, stability, and risk (Eichler, 2014; Bekaert et al., 2014; De Grauwe and Ji, 2013); we ask not whether politics is priced on average but whether one characteristic, polarization, changes the *slope* of creditworthiness in debt. A second relates polarization to government debt (Alesina and Tabellini, 1990; Alesina and Drazen, 1991; Persson and Svensson, 1989; Lee, 2022; Melki and Pickering, 2022); we hold debt fixed and study its credit consequence. A third models the rating *level* and its average determinants (Cantor and Packer, 1996; Afonso et al., 2011; Reusens and Croux, 2017). Recent evidence also links political polarization to credit outcomes outside the sovereign setting: Li and Tang (2026) report that greater U.S. state legislative polarization is associated with lower corporate credit ratings, with stronger effects for firms more reliant on government sales and for firms located in states with divided government. Closest to our sovereign setting, Li et al. (2024) find that U.S. state legislative polarization raises municipal yields through gridlock, and — in this journal — Ramírez-Rondán et al. (2023) interact political institutions with economic uncertainty as determinants of the rating *level*. We differ on both outcome and margin: a cross-country sovereign sample, a debt × polarization interaction rather than an institutions × uncertainty one, and the downgrade *flow* rather than the level. To our knowledge this is the first test of whether polarization steepens the debt–creditworthiness schedule estimated on actual sovereign downgrade actions.

We combine long-term sovereign rating records from Moody's, Fitch, and DBRS (S&P is unavailable under our data license) for up to 124 sovereigns over 1986–2026 with a 1996–2025 political–fiscal panel (V-Dem $v2cacamps$,[1] IMF debt, and macro controls). The ratings universe and the regression sample should not be conflated: the political–fiscal panel was assembled for 39 sovereigns, 38 of which overlap the rating file, and complete lagged primary-balance and current-account controls reduce the final hazard sample to 30 countries (26 European). We report few-cluster inference accordingly.

Three findings follow. First, in the pooled downgrade hazard the debt × polarization interaction is positive under the baseline action definition (0.055, $p = 0.040$ all-country; 0.053, $p = 0.040$ Europe), while few-cluster procedures range from approximately 0.03 under the score bootstrap to approximately 0.09 under the cluster jackknife. Second, the coefficient is economically stable across outcome definitions and persistent-country controls, although precision is not: a strict issuer-action outcome yields coefficients near 0.05 with $p \approx 0.09$, while Mundlak and country-fixed-effects estimates remain positive but imprecise. This makes the pooled association informative but leaves between-country confounding as a live concern. Third, the V-Dem measure and the proposed fiscal-adjustment mechanism are distinct. Direct regressions of subsequent primary-balance adjustment on debt, polarization, and their interaction do not yield robust evidence of that intermediate channel, so we interpret fiscal credibility as a plausible mechanism rather than an established one. Level models remain weakly informative because ratings are sticky, and upgrade and future-state placebos are flat.

2. Data

2.1. Sovereign ratings

Rating records are drawn from LSEG/Refinitiv for Moody's, Fitch, and DBRS Morningstar. The month-end issuer-rating panel is forward-filled between observed actions, mapped to a unified numeric notch (21 = AAA/Aaa down to 1 = C; investment grade ≥ 12), and averaged across available agencies to form the composite rating level. The annual composite contains 124 sovereigns over 1986–2026. S&P is excluded because it was not licensed in the extraction. The separate LSEG action file is broader than the issuer-level panel and includes long-term sovereign-related records such as Moody's country-ceiling actions. We therefore distinguish the rating *level* from the baseline action outcome and explicitly test a strict issuer-action definition below. Agency-specific estimates are positive for Moody's and Fitch, but only Moody's is significant at five percent; Fitch is less precise, and DBRS has too few events in the merged sample for a separate hazard. The Appendix provides the rating-scale key and the supplementary agency diagnostics.

[1] $v2cacamps$ is the V-Dem Country–Year dataset item for "political polarization", an expert-coded index of the extent to which society is polarized into antagonistic political camps, as opposed to consisting of a diversity of political perspectives without polarization; it is a societal/affective measure rather than a measure of legislative fragmentation or coalition gridlock specifically (see McCarty et al. (2006) for a related, U.S.-focused elite-polarization concept). Data are freely available from the V-Dem Institute at https://www.v-dem.net/data/the-v-dem-dataset/.

2.2. Political–fiscal panel and merge

Polarization, debt, and controls come from a 1996–2025 political–fiscal panel. V-Dem *v2cacamps* measures whether society is split into antagonistic political camps; we normalize the lagged raw index to POL01 $\in [0, 1]$ by min–max scaling within the relevant estimation sample,[2] so that POL01 is comparable in scale to a probability while preserving the ordering of the raw index. It is conceptually related to political conflict but is not a direct measure of legislative fragmentation or coalition instability. Debt is IMF World Economic Outlook gross general-government debt; the controls are the primary balance, real growth, CPI inflation, and the current-account balance.

The sample attrition is mostly upstream of the rating merge. The political–fiscal panel contains 39 sovereigns by design, compared with 124 in the rating universe. Thirty-eight of the 39 have some rating overlap. Requiring a rating in force at the start of year t leaves 1025 country-years; requiring lagged debt and polarization leaves 990; requiring the lagged primary balance reduces the sample to 785 country-years and 30 countries; and the lagged current account yields the final 760 country-years and 30 countries. Eight of the nine excluded political–fiscal countries are lost chiefly because a usable primary-balance history is unavailable; Luxembourg also lacks a usable start-year composite rating in the relevant window. The European sample contains 26 countries. We treat Europe as the main sample because institutions are more comparable, and use the broader sample as a robustness check.

For the baseline hazard, $D_{it} = 1$ when at least one of Moody's, Fitch, or DBRS records a long-term downgrade (a `Baja` action) in calendar year t, conditional on the sovereign being rated at the start of the year. Multiple downgrades by the same agency within a year, or downgrades by more than one agency, are collapsed to one country-year event. An upgrade by another agency does not cancel the event because the outcome is the occurrence of a downgrade, not a net-consensus rating change. Only one estimation-sample country-year contains downgrades by two distinct agencies, so a two-agency event rule has too little variation for a separate hazard. Because the action file includes some non-issuer sovereign records, we also report a strict issuer-action definition and a year-end composite-decline definition. The hazard models include the lagged composite rank as an "amount-at-risk" control, and all political–fiscal regressors are lagged one year.

2.3. Descriptive statistics

Table 1 reports the estimation sample: 26 European countries (Panel A), and those same 26 plus 4 non-European OECD/emerging economies — Canada, Chile, Mexico, and the United States — in the all-country sample of 30 (Panel B); the full list is in Appendix Table A1. Composite ratings average roughly 16.8 (between A and A+) with wide dispersion down to CCC–; downgrade actions occur in about 10% of country-years and upgrades in about 12%. Table 2 cross-tabulates downgrade frequency by debt and (normalized) polarization terciles: downgrades concentrate at higher debt and middle-to-upper polarization, while the high-debt/high-polarization cells in the upper-right of the table are visibly sparse—the same thin-support region examined formally in Section 4.4. Fig. 1 shows the joint distribution of lagged debt and polarization and marks downgrade observations. The high-debt/high-polarization corner is visibly sparse, especially beyond debt around 120% of GDP, which is why the probability interpretation below emphasizes the better-supported region around 90%.

3. Empirical strategy

3.1. Level models

The linear benchmark is a two-way fixed-effects regression of the composite rank, the rating mirror of a spread-pricing equation:

$$R_{it} = \alpha_i + \lambda_t + \beta_1 \,\mathrm{Debt}_{i,t-1} + \beta_2 \,\mathrm{POL01}_{i,t-1} + \beta_3 \left(\mathrm{Debt}_{i,t-1} \times \mathrm{POL01}_{i,t-1}\right) + \Gamma' X_{i,t-1} + \varepsilon_{it}, \tag{1}$$

where R_{it} is the composite rating rank of country i in year t, α_i and λ_t are country and year fixed effects, $X_{i,t-1}$ collects the four lagged macro controls (primary balance, real growth, inflation, current account), and ε_{it} is an idiosyncratic error term. The hypothesis is $H_1 : \beta_3 < 0$ (creditworthiness falls faster with debt under polarization). Because R is ordinal, we also estimate a pooled ordered probit with year effects and a fixed-effects ordered logit by the blow-up-and-cluster (BUC) estimator of Baetschmann et al. (2015), which dichotomizes the rating at every cutpoint, stacks, and applies conditional (within) logit. A placebo re-estimates (1) separately on low-debt (< 60) and high-debt (≥ 90) subsamples; the level mechanism, if present, should concentrate in the latter.

3.2. Downgrade-hazard models

The specification matched to rating stickiness is a discrete-time hazard for a downgrade:

[2] $\mathrm{POL01}_{i,t-1} = \left(v2cacamps_{i,t-1} - \min(v2cacamps)\right) / \left(\max(v2cacamps) - \min(v2cacamps)\right)$, with the minimum and maximum taken over the specific estimation sample used in each regression (e.g., separately for the level sample and the hazard sample), so that POL01 = 0 and POL01 = 1 always correspond to the least- and most-polarized country-year actually used in that regression. The raw *v2cacamps* range and the resulting sample-specific normalizations are reported in Table 1 and the replication package.

Table 1
Descriptive statistics, rating estimation sample.

Variable	*N*	Mean	SD	Min	Max
Panel A. European sample (26 countries, 1997–2025)					
Composite rating (rank)	665	16.83	3.47	3.0	21.0
Downgrade action (0/1)	669	0.100	0.30	0	1
Upgrade action (0/1)	669	0.124	0.33	0	1
Debt (% GDP)	669	62.0	35.1	3.9	209.9
Polarization ($v2cacamps$)	669	−0.82	1.43	−3.81	3.36
Panel B. All-country sample (30 countries, 1997–2025)					
Composite rating (rank)	754	16.92	3.42	3.0	21.0
Downgrade action (0/1)	760	0.096	0.30	0	1
Upgrade action (0/1)	760	0.120	0.33	0	1
Debt (% GDP)	760	60.7	34.7	3.9	209.9
Polarization ($v2cacamps$)	760	−0.82	1.38	−3.81	3.36

Notes: Composite of Moody's/Fitch/DBRS year-end ranks. Debt and polarization summary statistics are reported for the merged sample; macro controls (growth, inflation, primary balance, current account) are as in the political–fiscal panel. "Downgrade/upgrade action" is an indicator for at least one long-term downgrade/upgrade by a composite agency during the year, on the hazard sample (sovereigns rated at the start of the year). The all-country (Panel B) sample nests Panel A: it consists of the same 26 European countries plus 4 non-European OECD/emerging economies (Canada, Chile, Mexico, and the United States). Appendix Table A1 lists all 30 countries by sample.

Table 2
Downgrade actions by debt and polarization bin (all-country sample).

Debt (% GDP)	POL01 < 0.33	0.33 ≤ POL01 < 0.66	POL01 ≥ 0.66
< 60	10/191	20/196	1/28
60–90	4/51	12/115	5/39
90–120	1/24	12/76	1/2
≥ 120	2/8	5/30	0/0

Notes: Cells report (years with a downgrade action/country-years). Downgrades concentrate at higher debt and middle–upper polarization; the high-debt, top-polarization region remains sparse.

$$\Pr(D_{it}=1)=\Lambda\big(\lambda_t+\delta_1\,\mathrm{Debt}_{i,t-1}+\delta_2\,\mathrm{POL01}_{i,t-1}+\delta_3(\mathrm{Debt}_{i,t-1}\times\mathrm{POL01}_{i,t-1})+\Psi' X_{i,t-1}\big), \tag{2}$$

where D_{it} is the downgrade indicator defined above, Λ is the logistic CDF, λ_t are year effects, and Ψ collects the coefficients on the same four macro controls $X_{i,t-1}$ as in Eq. (1). The hypothesis is $H_2 : \delta_3 > 0$ (polarization raises downgrade risk more at high debt). We estimate (2) as a pooled logit with year dummies and country-clustered standard errors, and as a country-fixed-effects conditional logit (which discards sovereigns with no downgrade variation). Because the probability of a downgrade depends mechanically on the rating from which a sovereign can fall, we add the lagged composite rank $R_{i,t-1}$ as an amount-at-risk control, and report the interaction with and without it; we also add a duration term (years since the last rating action) and re-estimate under two alternative outcome definitions: strict issuer-rating actions only, and a decline in the year-end composite rating. As a negative control we re-estimate (2) with an *upgrade* indicator: the mechanism predicts $\delta_3 > 0$ for downgrades but $\delta_3 \approx 0$ for upgrades. As an intermediate check between pooled and conditional logit, we also estimate a Mundlak-style pooled hazard that adds country means of the time-varying regressors. This absorbs part of the between-country structure without discarding the many sovereigns that have no downgrade variation.

3.3. Timing and reverse-causality diagnostics

The reverse-causality concern in this setting is different from the one in dynamic credit-market panels. Our outcome is a rare binary rating action, not a persistent continuous financial-development variable, so a lagged-dependent-variable Arellano–Bond design is not the right tool. Instead, all political–fiscal regressors enter with a one-year lag, and we add two timing diagnostics. First, we re-estimate the pooled downgrade hazard with the debt–polarization state lagged two years, which strengthens temporal ordering. Second, we run a future-state placebo that replaces the lagged debt–polarization state with its value at $t + 1$. These exercises do not create exogenous variation and therefore do not make the estimates causal. They ask a narrower question: whether the downgrade signal is consistent with predetermined political–fiscal conditions rather than being mechanically generated by contemporaneous or subsequent political reactions to the downgrade.

We also report a dynamic-logit robustness check with initial-condition controls. A fully dynamic fixed-effects logit is not our baseline because, with rare downgrade events, adding a lagged downgrade and country effects discards substantial variation and raises the usual initial-conditions problem. Instead, we use a Wooldridge-style diagnostic (Wooldridge, 2005) that includes the lagged downgrade indicator, the initial downgrade state, country means of the time-varying covariates, the lagged rating level, and year effects. This check asks whether the debt–polarization interaction is simply proxying for downgrade persistence. The Appendix consolidates the ordinal, timing, dynamic-logit, agency-specific, and multiple-testing diagnostics.

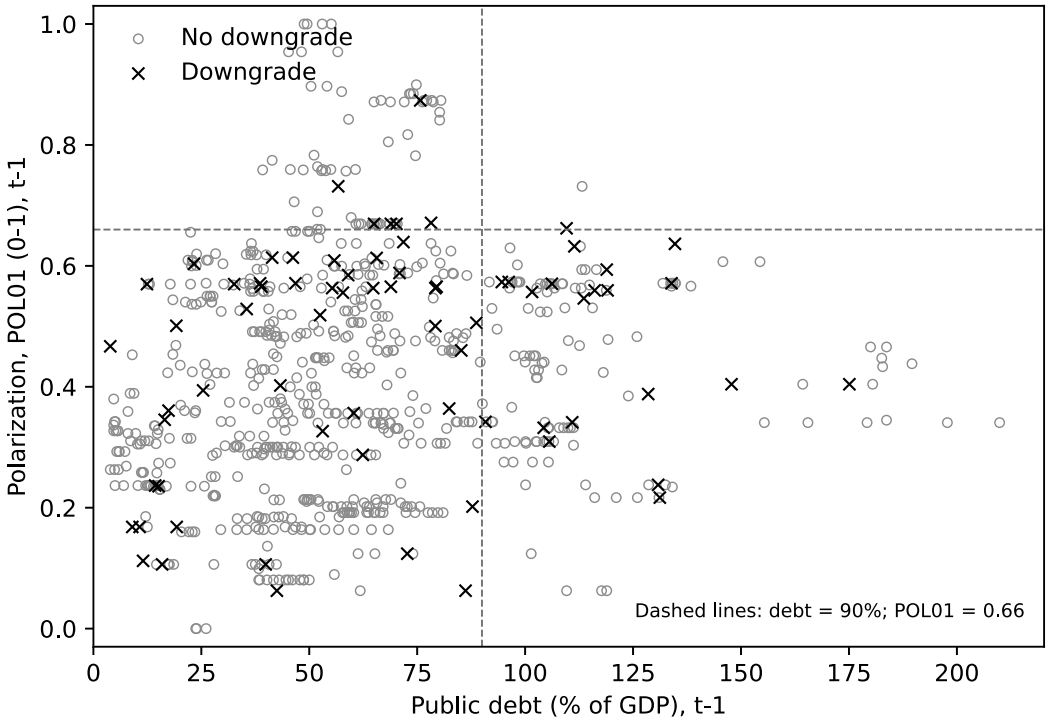


Fig. 1. Joint support of lagged public debt and polarization in the all-country hazard sample. Open circles are country-years without a downgrade action; crosses are country-years with at least one downgrade action. Dashed lines mark debt of 90% of GDP and POL01 = 0.66. The figure makes explicit that the upper-right tail is thinly populated.

3.4. Inference

With 26–30 country clusters, cluster-robust asymptotics remain unreliable, so for the linear level model we report country-clustered standard errors and, as the primary benchmark, a restricted (imposed-null) wild-cluster bootstrap with Rademacher weights and 999 replications (Cameron et al., 2008; MacKinnon and Webb, 2017; Cameron and Miller, 2015; Roodman et al., 2019). The discrete-choice models report country-clustered (pooled logit, ordered probit) or conditional-likelihood (BUC, FE logit) standard errors. Because cluster-robust logit standard errors are themselves unreliable with few clusters, we subject the pooled downgrade-hazard interaction to three complementary few-cluster procedures. First, a finite-sample-corrected cluster-robust t-test (CR1): the standard Stata-style sandwich variance with the $(G/(G-1))\times((n-1)/(n-k))$ small-sample correction, referred to a t_{G-1} rather than a standard normal distribution, following Cameron and Miller (2015). Second, a score-based wild cluster bootstrap (Kline and Santos, 2012), which perturbs the cluster-level score contributions with Rademacher weights and requires no refitting—and is therefore immune to the rare-event separation that leave-one-out methods can induce. Third, as a conservative cross-check, a cluster jackknife (CV3) variance (MacKinnon et al., 2023). We treat the score bootstrap as the primary few-cluster test for the hazard, mirroring the role of the wild-cluster bootstrap in the linear model, and report CR1 and the jackknife as complementary checks that bracket it from opposite directions. We read a non-rejection as underpowered rather than as a precise zero.

4. Results

4.1. Rating levels provide only suggestive evidence

Table 3 reports the linear notch model. The debt × polarization interaction is negative, as predicted, and nearly identical across samples (−0.073 Europe, −0.078 all-country): a unit of polarization steepens the rating's decline in debt by about 0.075 notches per point of debt-to-GDP. The wild-cluster bootstrap p-value is 0.061 (Europe) and 0.060 (all-country), so the rating-level evidence is suggestive but still short of conventional significance. The average polarization effect on the level (Table 3, lower panel) is small and insignificant in both samples, so the signal is conditional, not a uniform rating penalty. An auxiliary spread benchmark and the debt-region placebo splits, reported with the supplementary diagnostics in the Appendix, are also imprecise, foreshadowing the stickiness diagnosis below.

Table 3
Linear fixed-effects models of the composite rating.

Term	Sample	Coef.	Clust. SE	Clust. p	WCB p	(N, G)
Debt × POL01	Europe	−0.073	0.039	0.070	0.061	(673, 26)
Debt × POL01	All	−0.078	0.037	0.044	0.060	(763, 30)
POL01 (level, no int.)	Europe	−2.589	2.265	0.264	0.449	(673, 26)
POL01 (level, no int.)	All	−1.758	2.178	0.426	0.599	(763, 30)
HighDebt × POL01	Europe	−4.933	3.876	0.215	0.402	(673, 26)
HighDebt × POL01	All	−4.880	3.432	0.166	0.316	(763, 30)

Notes: Dependent variable is the composite rating rank (higher = better). Country and year fixed effects, four macro controls, regressors lagged one year. HighDebt = $\mathbf{1}\{\text{Debt} \geq 90\}$. WCB = restricted wild-cluster bootstrap (999 Rademacher reps). The predicted sign is negative.

4.2. Level and fixed-effects-ordinal models are variation-starved

The supplementary ordinal models reinforce this point. The pooled ordered probit interaction is indistinguishable from zero in both samples, and the fixed-effects ordered logit (BUC) fails to identify an informative interaction. Ratings move so rarely within country that estimators relying on within-level variation have little to work with; the debt-region placebo split tells the same story. Where a continuous price like a spread would react most — the high-debt region — ratings are unresponsive, because crisis-country ratings reach a floor and cease to track further debt increases. The level is the wrong margin.

4.3. Downgrade-hazard results and persistent-country concerns

Table 7 reports the discrete-time hazard, built on the actual long-term downgrade actions of the composite agencies. In the pooled logit with year effects, the debt × polarization interaction is positive and significant, and — critically — essentially unchanged when the rating level at risk is added: $\delta_3 = 0.055$ ($p = 0.040$) all-country and 0.053 ($p = 0.040$) Europe with the amount-at-risk control, against 0.054 ($p = 0.045$) and 0.054 ($p = 0.053$) without it. The interaction is therefore not proxying for the rating level—conditional on where a sovereign starts, polarization at high debt raises its downgrade probability. The amount-at-risk control itself enters positively (higher-rated sovereigns have more room to fall), and adding a duration term (years since the last action) leaves the interaction at $p \approx 0.04$. The upgrade placebo is flat — $\delta_3 \approx 0$ with $p = 0.869$ (Europe) and 0.734 (all-country) — and, tellingly, the amount-at-risk control *reverses sign* in the placebo (higher-rated sovereigns have less room to rise), the internal-consistency pattern that an amount-at-risk interpretation requires but the design did not impose. Fig. 2 plots the fitted downgrade probability across the debt range: flat in debt at low polarization, steeply rising at high polarization—the downgrade analogue of the steepening debt–spread schedule. The event-definition sensitivity is reported in Table 8: the strict issuer-action coefficient remains close to the baseline but loses precision ($p = 0.099$ Europe; 0.091 all-country), while the year-end composite-decline definition is somewhat larger and significant. We therefore emphasize stability of the estimated magnitude rather than claiming that significance is invariant to every reasonable event rule.

The country-fixed-effects conditional logit tells a more cautious but consistent story. The within-country interaction is positive and *strengthens* once the level at risk is controlled — from 0.027 to 0.046 (all-country) and 0.026 to 0.042 (Europe), with the p-value falling from above 0.41 to 0.18–0.23 — which argues against the reading that the pooled result is merely a between-country correlation. It does not, however, reach conventional significance. The reason is power, not sign: the conditional logit conditions out the five sovereigns that never downgrade in sample (Germany, Denmark, the Netherlands, Sweden, the United States), leaving 67–73 downgrade events across 22–25 effective clusters. We therefore treat the pooled result as the main predictive association, not as a within-country causal estimate. The positive sign survives the within-country estimator, but the loss of precision leaves open the possibility that persistent country differences contribute to the pooled association.

The extent of this power limitation can be quantified. The all-country within-country interaction is 0.046 (SE 0.034), with a 95% confidence interval of $[-0.021, 0.113]$ that contains both zero and the pooled point estimate (0.055): the conditional logit cannot discriminate between "no within-country effect" and "the pooled effect". Under the maintained conditional-logit design, the realized standard error implies only about 37% power to detect an interaction equal to the pooled estimate at the 5% level (about 32% in Europe). Reaching conventional 80% power at this effect size would require the within-country standard error to fall by roughly 40%—on the order of three times the current number of downgrade events, about 220 rather than 73. These calculations show that the within-country test has limited discriminatory power; they do not establish that low power, rather than persistent country differences, explains the pooled result.

Persistent-country controls give a deliberately mixed but informative message. Euro/non-euro group dummies and the number of agencies rating the sovereign leave the pooled coefficient near 0.05; a Mundlak specification that adds country means of the time-varying regressors yields 0.061 in Europe and 0.063 all-country but is imprecise ($p = 0.244$ and 0.220), while the country-fixed-effects conditional logit is likewise positive and imprecise. We also include two country-level proxies for persistent institutional quality and economic development — the World Bank governance-effectiveness score and log GDP per capita at purchasing power parity, both single most-recent-year cross-sections — in the pooled and Mundlak specifications (Appendix Table A2); these cannot enter the country-fixed-effects model because, being purely cross-sectional, they are absorbed by the country effects. For fiscal capacity,

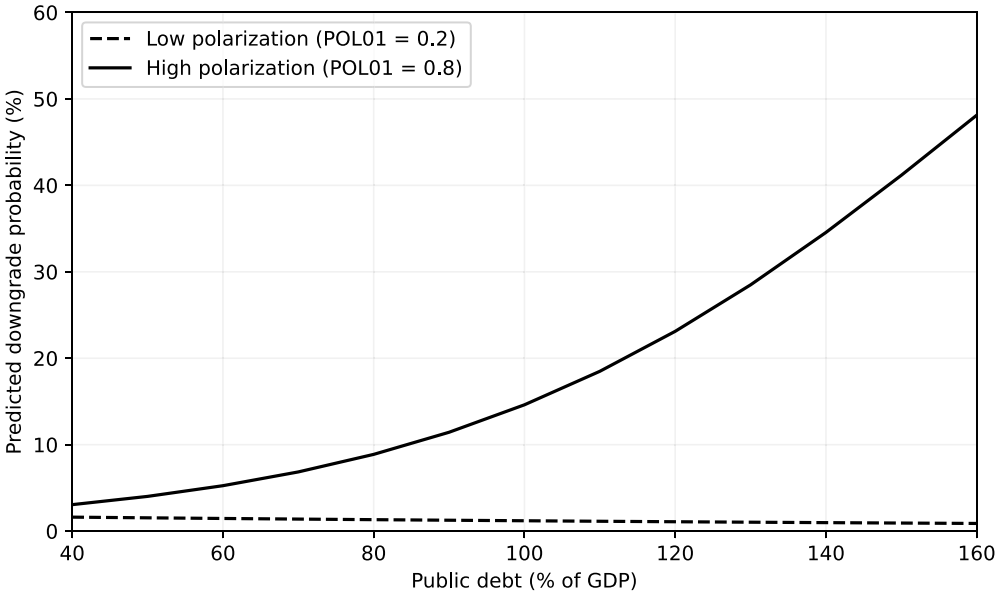


Fig. 2. Fitted probability of a sovereign downgrade across the debt range, at low and high polarization, from the all-country pooled hazard (2) including the amount-at-risk control, with other covariates at sample means. The schedule is flat in debt at low polarization and steepens sharply at high polarization. Consistent with Fig. 1, the upper-right region (POL01 = 0.8 with debt $\gtrsim$ 120) has very limited empirical support and should be read only as illustrative of fitted shape.

the baseline specification already conditions on lagged public debt and the primary balance, while the underlying panel does not contain a separate consistently populated fiscal-capacity indicator; we therefore do not introduce an additional unverified proxy. In the pooled specification the debt × polarization interaction remains similar in magnitude when the governance and income proxies are added (0.058, $p = 0.037$ Europe; 0.062, $p = 0.029$ all-country), and the corresponding Mundlak estimates are also essentially unchanged. These are robustness controls rather than a test that exhausts persistent structural differences: they show that adding these two observable proxies does not materially alter the estimated interaction, while the imprecise Mundlak and country-fixed-effects results leave broader cross-country confounding as a remaining concern.

The timing diagnostics also preserve the sign: the interaction remains positive and statistically significant when the political–fiscal state is lagged two years (0.046 in Europe and 0.048 all-country). By contrast, replacing the predetermined state with its future value gives smaller and statistically insignificant coefficients ($p = 0.456$ and $p = 0.428$). These checks are not causal identification, but they support the maintained timing interpretation: the downgrade hazard is related to pre-existing debt–polarization conditions, while the reverse-timing placebo is flat. The Wooldridge-style dynamic-logit check with initial-condition controls gives a similar-magnitude interaction (0.057 Europe, 0.056 all-country) that is not itself significant ($p \approx 0.20$–0.21), so the debt–polarization interaction is not simply proxying for downgrade persistence, though this specification shares the same power limitation as the country-fixed-effects hazard. A Benjamini–Hochberg false-discovery-rate adjustment (Benjamini and Hochberg, 1995) across the family of main, timing, and placebo tests leaves the main and two-year-lag downgrade tests near conventional thresholds, while the future-state and upgrade placebos remain far from rejection. These results suggest that the main finding is not driven solely by multiple testing.

Table 10 subjects the pooled interaction to three procedures suited to few clusters. The score-based wild cluster bootstrap — the nonlinear counterpart of the wild bootstrap used in the linear model — returns $p = 0.026$ (all-country) and 0.028 (Europe), comfortably below five percent; adding euro/non-euro group dummies, which absorb part of the time-invariant between-country structure, leaves the coefficient essentially unchanged ($0.055 \rightarrow 0.052$) with the bootstrap p at or below 0.04. The finite-sample-corrected cluster-robust t-test (CR1, t_{G-1}) is more conservative and places the baseline interaction right at the boundary of conventional significance — $p = 0.051$ (Europe) and 0.049 (all-country) — softening further to $p = 0.060$–0.062 once group dummies are added. The cluster jackknife, the most conservative of the three, places the same interaction at the ten-percent level ($p \approx 0.09$ in the baseline, 0.11 with group dummies). The three few-cluster methods therefore bracket the result rather than agreeing on a single verdict: comfortably significant under the score bootstrap, marginal under CR1, and short of conventional significance under the jackknife. We read this as evidence that the downgrade channel does not collapse under few-cluster inference, while being transparent that its significance is sensitive to which few-cluster correction is applied—a sensitivity we attribute to the modest cluster count (26–30) rather than to the sign or economic magnitude of the interaction, which is stable across all three procedures.

Table 4
Threshold sensitivity: the interaction across debt cutoffs (all-country sample).

Sample restricted to	Coef.	SE	p	N	Events
Debt < 90%	0.0403	0.0455	0.376	620	52
Debt < 100%	0.0410	0.0422	0.332	645	55
Debt < 110%	0.0650	0.0353	0.065	695	60
Debt < 120%	0.0754	0.0290	0.009	722	66
Debt < 130%	0.0759	0.0286	0.008	728	67
Debt < 140%	0.0515	0.0255	0.043	743	71
Debt < 150%	0.0502	0.0251	0.045	745	72
No cutoff (full sample)	0.0554	0.0269	0.040	760	73

Notes: Pooled downgrade hazard (year FE, amount-at-risk control, macro controls), re-estimated on the subsample with debt below each cutoff. p is the ordinary asymptotic cluster-robust p-value, on the same convention as Table 7. 120% is not a singular, cherry-picked point on this curve: significance builds and fades smoothly around it.

Table 5
Leave-one-country-out: pooled hazard re-estimated dropping each country in turn (all-country sample).

	Baseline coef.	Min	Max	Mean
Debt × POL01 (across 30 exclusions)	0.0554	0.0420	0.0719	0.0552

Notes: 30 re-estimations of the pooled downgrade hazard, each dropping one country from the full sample. Only 2 of 30 exclusions push the ordinary cluster-robust p-value above 0.10 (baseline $p = 0.040$); the coefficient never changes sign or falls outside a narrow band around the baseline. Full per-country detail is in the replication package.

4.4. Identification support: the interaction is not an artifact of a thin region

Fig. 1 shows why support deserves direct scrutiny: the interaction is identified where debt and polarization are jointly high, and that corner of the sample is thin. Rather than rest on a single restricted-sample estimate chosen after inspecting which cells oppose the hypothesis, we report three checks together.

First, Table 4 traces the pooled interaction across debt cutoffs from 90% to 150% of GDP rather than reporting one arbitrary threshold. The coefficient and its significance build smoothly from 90% ($p = 0.376$) to a plateau at 110%–130% ($p = 0.008$–0.065), then partially fade as the sparser tail beyond 130% re-enters the sample ($p = 0.043$–0.045 at 140%–150%). This is more consistent with a genuine, gradual loss of precision as the sparse tail enters and exits the sample than with an estimate that happens to peak exactly where a cutoff was chosen after the fact.

Second, Table 5 summarizes leave-one-country-out estimates: re-estimating the full-sample hazard while dropping each of the 30 countries in turn leaves the coefficient in a narrow band (0.042 to 0.072 against a baseline of 0.055), and only 2 of 30 exclusions push the ordinary cluster-robust p-value above ten percent. Excluding Italy weakens the estimate most ($p = 0.137$); excluding Portugal strengthens it most ($p = 0.005$). No single country is driving the result, though we note that excluding observations that run against a hypothesis will mechanically strengthen a coefficient under several forms of misspecification, not only under thin support, so this check rules out an obvious single-country artifact without amounting to proof of robustness on its own.

Third, Table 6 translates the logit coefficient onto the probability scale, where the interaction's practical magnitude — not just its sign and p-value — can be judged directly. At Debt = 90% of GDP, the predicted downgrade probability is 1.3% (95% CI 0.3–2.3) at low polarization versus 11.4% (5.4–17.5) at high polarization: non-overlapping intervals at a debt level well within the supported region. The average marginal effect of moving debt from 60% to 90% of GDP is essentially flat at low polarization (−0.2 percentage points, CI including zero) but +6.2 points at high polarization (CI 0.4–12.0); the difference between these two marginal effects — the probability-scale analogue of the interaction coefficient — is +6.4 points with a 95% CI of 0.3–12.5, excluding zero. We read these three checks together as evidence that the interaction reflects a genuine, if imprecisely bounded, feature of the data rather than an artifact of a single threshold, a single country, or a coefficient whose sign is easier to sustain than its magnitude is to interpret.

4.5. Intermediate outcome: fiscal adjustment

The fiscal-adjustment story is a possible interpretation of the conditional downgrade pattern, not something measured directly by $v2cacamps$. To examine it, we regress subsequent changes in the primary balance on debt, polarization, their interaction, the current primary balance, the other macro controls, and year effects; we also add country effects. Table 9 reports one-, two-, and three-year horizons. The interaction is negative at longer horizons, as the mechanism would predict, but it is not robustly different from zero. We therefore do not interpret the downgrade result as evidence that polarization works specifically through weaker realized fiscal adjustment. The data support a conditional predictive relationship; the fiscal-credibility mechanism remains a hypothesis for future work with direct measures of legislative gridlock, fiscal plans, and forecast errors.

Table 6
Predicted downgrade probabilities and marginal effects by debt and polarization.

Debt (% GDP)	Polarization	Pred. prob. (%)	SE (%)	CI low (%)	CI high (%)
60	low (0.2)	1.46	0.38	0.71	2.22
60	mid (0.5)	2.79	0.48	1.85	3.73
60	high (0.8)	5.26	1.03	3.25	7.27
90	low (0.2)	1.26	0.51	0.26	2.26
90	mid (0.5)	3.90	0.52	2.88	4.92
90	high (0.8)	11.44	3.10	5.36	17.51
Average marginal effect of debt 60% → 90% GDP, by polarization					
Low polarization (0.2)		−0.20 pp		CI [−0.67, 0.26]	
High polarization (0.8)		+6.18 pp		CI [0.37, 11.99]	
Difference (high − low)		+6.38 pp		CI [0.25, 12.52]	

Notes: Full-sample pooled hazard (amount-at-risk control, macro controls, other covariates at sample means, average year effect). Delta-method standard errors and 95% confidence intervals. We report 60% and 90% debt in the main table because they lie in the better-supported region shown in Fig. 1; predictions in the very-high-debt/high-polarization tail are treated as shape illustrations rather than precise estimates. The bottom panel's difference row is the probability-scale analogue of the logit interaction coefficient δ_3; its 95% CI excludes zero.

Table 7
Downgrade-hazard models (and upgrade placebo).

Model	Sample	Debt × POL01	SE	p	Events
Downgrade, pooled (year FE, cluster)	Europe	0.0535	0.0277	0.053	67/669
+ amount-at-risk control	Europe	0.0530	0.0259	0.040	67/669
Downgrade, pooled (year FE, cluster)	All	0.0537	0.0268	0.045	73/760
+ amount-at-risk control	All	0.0554	0.0269	0.040	73/760
Downgrade, country-FE cond. logit	Europe	0.0255	0.0348	0.464	–
+ amount-at-risk control	Europe	0.0423	0.0354	0.233	–
Downgrade, country-FE cond. logit	All	0.0273	0.0333	0.412	–
+ amount-at-risk control	All	0.0461	0.0342	0.177	–
Upgrade placebo, pooled (+ control)	Europe	−0.0062	0.0375	0.869	83/669
Upgrade placebo, pooled (+ control)	All	−0.0124	0.0365	0.734	91/760

Notes: Dependent variable is a long-term downgrade (or upgrade) action by any of Moody's, Fitch, or DBRS during year t, among sovereigns rated at the start of the year. p is the ordinary (large-G asymptotic, normal-reference) cluster-robust p-value; Table 10 reports few-cluster-corrected alternatives for the baseline specification. The amount-at-risk control is the lagged composite rank $R_{i,t-1}$. Predicted sign is positive for downgrades and zero for the upgrade placebo. The country-FE conditional logit conditions out the never-downgrading sovereigns (Germany, Denmark, the Netherlands, Sweden, and — all-country — the United States), retaining 22 (Europe) and 25 (all-country) clusters. Adding a duration control (years since the last rating action) leaves the pooled downgrade interaction at $p \approx 0.04$.

Table 8
Sensitivity to the downgrade-event definition.

Definition	Sample	Debt × POL01	SE	p	Events/N
Any long-term agency downgrade (baseline)	Europe	0.0530	0.0259	0.040	67/669
	All	0.0554	0.0269	0.040	73/760
Issuer-rating actions only	Europe	0.0480	0.0291	0.099	62/669
	All	0.0509	0.0301	0.091	68/760
Year-end composite rating declines	Europe	0.0593	0.0264	0.025	68/665
	All	0.0696	0.0286	0.015	74/754

Notes: All rows use the pooled logit with year effects, macro controls, and lagged rating level. The issuer-only rule excludes non-issuer sovereign action records, principally Moody's country-ceiling actions. The point estimate changes little but is less precise because five event-years are removed. Only one estimation-sample country-year has downgrades by two distinct agencies, so a two-agency rule does not provide enough variation for a separate hazard.

5. Conclusion

This paper asked whether political polarization conditions the credit assessment of public debt. The main empirical pattern is a positive debt–polarization interaction in pooled downgrade hazards. Its magnitude is stable to the lagged rating level, duration, broad group controls, agency coverage, timing checks, and alternative event definitions, but its statistical precision is not invariant. The baseline action outcome gives $p = 0.040$ under conventional country-clustered inference; the score bootstrap is near 0.03, CR1 is near 0.05, the cluster jackknife is near 0.09, and the strict issuer-action outcome is near 0.09. Rating levels remain weakly informative because they are sticky, while upgrade and future-state placebos are flat.

The evidence also makes clear what the result does not establish. V-Dem *v2cacamps* measures societal division into antagonistic camps, not legislative gridlock or fiscal credibility directly. Our intermediate-outcome tests do not find a robust debt × polarization

Table 9
Intermediate outcome: subsequent primary-balance adjustment.

Horizon	Specification	Debt × POL01	SE	p	N
$t+1$	Pooled + year FE	−0.00092	0.01325	0.944	850
$t+1$	Country + year FE	−0.00770	0.01596	0.630	850
$t+2$	Pooled + year FE	−0.01451	0.02350	0.537	819
$t+2$	Country + year FE	−0.03836	0.03083	0.213	819
$t+3$	Pooled + year FE	−0.02996	0.03534	0.397	788
$t+3$	Country + year FE	−0.07582	0.04413	0.086	788

Notes: Dependent variable is the change in the primary balance (percentage points of GDP) from t to the indicated future horizon. Controls are debt, POL01, their interaction, the current primary balance, real growth, inflation, and current account; standard errors are clustered by country. The absence of a stable significant interaction is treated as a null mechanism result.

Table 10
Few-cluster-robust inference on the pooled downgrade-hazard interaction.

Specification	Sample	Debt × POL01	CR1 p	Score-boot. p	Jackknife p
Baseline (+ amount-at-risk)	Europe	0.0530	0.051	0.028	0.090
Baseline (+ amount-at-risk)	All	0.0554	0.049	0.026	0.092
+ euro/non-euro group dummies	Europe	0.0519	0.060	0.032	0.106
+ euro/non-euro group dummies	All	0.0521	0.062	0.040	0.113

Notes: Pooled downgrade hazard with year fixed effects, macro controls, and the lagged rating level. CR1 p = finite-sample-corrected cluster-robust p-value: sandwich variance with the $(G/(G-1))\times((n-1)/(n-k))$ correction, referred to t_{G-1} rather than a normal distribution, following Cameron and Miller (2015); this is more conservative than the ordinary asymptotic cluster-robust p reported in Table 7. Score-boot. p = score-based wild cluster bootstrap (Kline and Santos, 2012), 9999 Rademacher replications—the nonlinear analogue of the linear wild-cluster bootstrap and the primary few-cluster test. Jackknife p = cluster jackknife CV3 (MacKinnon et al., 2023), the most conservative method. The interaction is comfortably significant under the score bootstrap, at the boundary of five percent under CR1, and at the ten percent level under the jackknife; the coefficient itself is essentially unchanged by group dummies.

effect on subsequent primary-balance adjustment, so the fiscal-adjustment mechanism should be viewed as a plausible interpretation rather than a demonstrated transmission channel. Likewise, Mundlak and country-fixed-effects hazards preserve the positive sign but are imprecise, and adding country-level governance and income controls to the pooled and Mundlak specifications leaves the interaction essentially unchanged. Persistent country differences along dimensions these two proxies do not capture remain a possible contributor to the pooled association, and with only 26–30 clusters we cannot rule this out completely.

Relative to recent finance evidence linking political polarization to corporate credit ratings and state borrowing costs (Li and Tang, 2026; Li et al., 2024), our contribution is to identify a distinct sovereign margin: polarization changes the relationship between public debt and downgrade risk rather than imposing a uniform rating-level penalty. These limitations define the scope of that contribution. The evidence is predictive rather than causal: downgrade risk rises more steeply with debt when measured polarization is high, especially in the supported part of the debt distribution, but the paper does not identify a unique institutional mechanism. Future work can sharpen that interpretation by combining the rating panel with direct measures of legislative fragmentation, coalition stability, fiscal-plan implementation, and fiscal forecast errors, and by expanding the political–fiscal cross-section so that within-country estimators contain substantially more downgrade variation.

CRediT authorship contribution statement

María J. Quinteros: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Mauricio G. Villena:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Both authors have read and approved the final manuscript and agree to be accountable for the work.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.frl.2026.110855.

Data availability

Data and supplementary material are available at: https://www.mauriciovillena.com/.

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