

Who pays for rearmament? Budget re-compositing and fiscal expansion in the European Union

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Abstract. Russia's full-scale invasion of Ukraine altered European security priorities, but a higher defense share does not by itself establish cuts elsewhere. This study examines all ten functions of government in the EU-27 using Eurostat data for 2005–2024. Exact budget shares are paired with percentage-of-GDP outcomes, a post-2022 geographic-exposure design, country and year fixed effects, few-treated-cluster inference, false-discovery-rate control, and equivalence tests.

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Across the full panel, a one-percentage-point higher defense share is most strongly associated with a 0.871-point lower economic-affairs share; smaller associations for environmental protection, housing, and recreation are statistically detectable but fall within a ± 0.25 -point equivalence bound. Health, education, and public order associations are equally small, whereas social protection re-mains inconclusive. After 2022, the eight more-exposed countries do not show a statistically robust differential increase in the defense budget share (0.362 points; wild-bootstrap $p = .267$). They do, however, expand total expenditure by 3.034 percentage points of GDP ($p < .001$). The early rearmament episode, therefore, reflects fiscal expansion more clearly than broad program displacement.

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1. INTRODUCTION

Russia's full-scale invasion of Ukraine transformed the fiscal context in which European governments make security decisions. Comparative and institutional assessments now describe the post-2022 build-up as a structural shift in both the level and composition of European defense expenditure (Cepparulo et al., 2025; Checherita-Westphal et al., 2025). Defense planning moved from a gradual burden-sharing debate to an immediate question of readiness, procurement, and strategic autonomy (Fiott, 2022, 2024; Michaels & Sus, 2024; Tuna, 2026). NATO burden-sharing had already varied markedly across members (Kim & Sandler, 2023; Conyon, 2025), but the post-2022 environment made the domestic budget consequences of higher military commitments more salient. The political question is commonly framed as “guns versus butter”: if defense receives more, which civilian programs pay?

That framing is incomplete unless the scale of the budget is separated from its composition. A rising defense share can coincide with a real cut in a civilian function, with slower civilian growth, or with an expanding expenditure envelope in which both defense and civilian spending increase. Historical evidence shows that armed conflict can alter fiscal balances and expenditure composition simultaneously (Gupta et al., 2004), while recent European evidence treats military expenditure as an endogenous part of fiscal policy rather than a mechanically fixed claim on a constant budget (Olejnik & Kuna, 2025). Government revenue capacity, debt, and fiscal institutions may therefore determine whether rearmament reallocates an existing envelope or enlarges it (Lyeonov et al., 2026; Toth et al., 2025). Recent EU and OECD assessments likewise find that short-run output effects vary with fiscal space, import intensity, procurement composition, and financing, so a defense expansion need not map one-for-one into civilian retrenchment (Furceri et al., 2026; OECD, 2026).

Existing studies usually estimate bilateral relationships between military spending and health, education, growth, or the environment. Reviews and meta-analyses emphasize substantial heterogeneity across samples, periods, and specifications (Alptekin & Levine, 2012; Dunne et al., 2005; Dunne & Tian, 2015). The results are also mixed for social expenditure: OECD evidence allows defense and welfare spending to rise together (Lin et al., 2015), whereas identification based on military-spending shocks finds medium-run crowding out of health and education in more fiscally constrained settings (David et al., 2026). Country and category-specific studies similarly show that short-run stimulus can coexist with adverse longer-run or compositional effects (Tsitouras & Tsounis, 2026). Crowding out has been observed in some health and

education settings (Elish et al., 2023; Ikegami & Wang, 2022; Badmus et al., 2025), and the composition and productivity of military budgets also matter (Becker & Dunne, 2021). Bilateral designs cannot show where a displaced percentage point travels, and a non-significant coefficient does not establish that an association is economically negligible. These limitations are especially important in a compositional system whose exhaustive shares sum to 100.

This study asks two linked questions. First, how are movements in the defense share associated with the complete functional composition of EU-27 expenditure over 2005–2024? Second, did geographically exposed member states follow a distinct post-2022 path in budget shares or expenditure as a percentage of GDP? The design contributes by imposing an exact ten-function budget system, estimating the post-2022 contrast over a common 2016–2024 event window, and aligning inference with the small number of exposed countries. Multiple-testing control, equivalence tests, randomization inference, alternative windows, country trends, and leave-one-exposed-country-out estimates make the strength and limits of the evidence explicit.

The main result is a distinction between long-run budget recomposition and the early rearmament episode. In the full panel, economic affairs show the strongest negative association with the defense share. However, the exposed-country contrast after 2022 is not statistically robust for the defense share, whereas total expenditure rises by about three percentage points of GDP. The post-2022 evidence consequently supports fiscal expansion more clearly than general civilian retrenchment. This qualification matters for climate and investment strategies, which depend on sustained public commitment (Meckling & Strecker, 2022; Ciot, 2023; Raudla et al., 2025). It also matters for energy-transition delivery, where policy coherence and system-flexibility investment condition renewable penetration (Tutar & Štreimikienė, 2026), and for territorial equity, because national averages can conceal persistent subnational SDG disparities (Tutar & Kyriakopoulos, 2026). The public legitimacy of security-welfare choices remains an additional constraint (Bremer & Bürgisser, 2022; Sacchi et al., 2025).

2. LITERATURE REVIEW AND THEORETICAL FRAMEWORK

2.1. Rearmament, burden sharing, and bilateral trade-offs

Security threats create a collective-action problem across allies and a domestic allocation problem within each state. Burden-sharing research explains cross-national military-spending differences through threat perceptions, alliance incentives, signaling, neighboring military burdens, and domestic political conditions (Kim & Sandler, 2023; Conyon, 2025; Clements et al., 2021). In Europe, the invasion accelerated debates over capabilities and procurement, yet higher nominal commitments do not reveal how national budgets absorb them (Fiott, 2022, 2024). Political accountability can alter military allocations, and domestic institutions mediate the translation of security demand into expenditure (Lopes da Silva, 2022).

The empirical guns-versus-butter literature frequently tests one civilian category at a time. Cross-country evidence on health is not uniform: estimated substitution depends on institutions, income, and model specification (Ikegami & Wang, 2022; Badmus et al., 2025). Country evidence can show clearer trade-offs, as in the health-and-education case examined by Elish et al. (2023), but it does not necessarily generalize to the EU's fiscal structure. The recent EU study by Waszkiewicz et al. (2025) likewise shows why social and economic conditions must be considered when interpreting military expenditure. The relevant counterfactual is therefore not automatically a fixed public budget.

2.2. Budget composition, rigidity, and scale

Expenditure functions differ in political and legal adjustability. Social protection and health contain recurrent commitments, statutory entitlements, and organized constituencies. Their determinants include demographic and labor-market conditions that change slowly (Karaalp-Orhan et al., 2024; Yurchyk et al., 2024), while research on austerity shows that voters react to the design and justification of welfare recalibration (Bremer & Bürgisser, 2022). War-related migration can also generate persistent sectoral labor-market mismatches whose severity depends on integration capacity, adding a distinct implementation constraint to post-2022 budgeting (Tutar et al., 2025; Karolyi et al., 2025; Pankova et al., 2025). Economic affairs are broader and more heterogeneous: they include transport, sectoral support, and other interventions, so a negative coefficient cannot be interpreted as a direct measure of public investment. Environmental protection is also a broad COFOG function, not a complete measure of climate policy.

Fiscal institutions can protect aggregate sustainability while shifting adjustment toward less legally committed items. The composition of consolidation affects macroeconomic outcomes (Tutar et al., 2026; Alesina & Perotti, 1997), fiscal rules can improve expenditure efficiency (Apeti et al., 2025), and expenditure benchmarks may have distinct implications for the share of investment in the budget (Cronin & McNerney, 2026). Debt may constrain the ability to expand the total envelope (Dudzevičiūtė et al., 2021; Toth et al., 2025), whereas fiscal resilience depends in part on the stock and organization of social infrastructure (Neuhuber et al., 2025; Piekut & Rybaltowicz, 2024).

Climate-related programs illustrate the interpretive risk. Security shocks may complicate the implementation of the Green Deal (Ciot, 2023; Streimikiene et al., 2024), but environmental outcomes depend on regulation, finance, and administrative capacity, as well as the COFOG environmental-protection line (Meckling & Strecker, 2022; Niu, 2024; Raudla et al., 2025). Military activity may also have direct climate consequences (Elveren, 2025). A budget-share association is thus evidence about relative fiscal priority, not a complete welfare or environmental effect.

2.3. Hypotheses

The compositional logic gives a precise but limited prediction. If the defense share rises while all ten functions exhaust the budget, the remaining coefficients must sum to minus one. Rigidity theory predicts that the largest negative association should occur in comparatively adjustable functions, but it does not imply that every negative coefficient is a real cut.

H1a: A higher defense share is associated with lower shares of comparatively adjustable government functions, with the largest negative association expected in economic affairs.

H1b: Associations between the defense share and the recurrent service or entitlement functions—health, education, public order, and social protection—are smaller than ± 0.25 percentage points.

Geographic exposure provides a separate test of the post-2022 episode. Estonia, Finland, Hungary, Latvia, Lithuania, Poland, Romania, and Slovakia border Russia, Belarus, or Ukraine and faced a sharper change in the immediate security environment.

H2: After 2022, geographically exposed states experience a larger increase in the defense share than other EU members.

Finally, inherited commitments and debt may limit the ability to increase the defense share of exposure.

H3: A larger prewar health-plus-social-protection share and a higher prewar debt ratio attenuate the post-2022 exposure association.

3. METHODOLOGY

3.1. Data and sample

The balanced target population is the EU-27 for the period 2005 to 2024. Eurostat's annual COFOG data provide expenditure in national currency and as a percentage of GDP for the first ten functions (Eurostat, 2026a). Government debt and net lending are taken from the deficit and debt table (Eurostat, 2026b), and real GDP per capita is taken from the annual national accounts (Eurostat, 2026c). The World Bank's SIPRI-sourced military expenditure indicator is used only for convergent validation (World Bank, 2026). The resulting panel contains 535 country-years; Slovakia has 15 observations, and the other countries have 20. No country-year used in the main models is missing any functional share, total expenditure, debt, balance, or real income measure. Under Eurostat's ESA 2010 national accounting framework (gov_10a_exp), total general government expenditure by function includes transactions such as gross fixed capital formation and net acquisitions of non-produced non-financial assets. Consequently, a negative expenditure entry—such as the observed minimum of -0.64% for environmental protection—arises in national accounts when asset sales/disposals (e.g., revenues from the sale of pollution allowances/emission rights, land, or capital equipment) or retrospective capital receipts in a specific year exceed current operational outlays, yielding a negative net capital transaction recorded in the COFOG division.

For each country-year, expenditure in the ten functions is normalized by their observed sum. This construction produces exact shares that total 100 and avoids the rounding discrepancy in the earlier specification. Because these shares lie in a simplex and carry relative rather than independent information, interpretation follows the core principles of compositional data analysis (Pawlowsky-Glahn et al., 2015; Greenacre, 2021). The COFOG defense share correlates at .888 with the independent military-expenditure-to-GDP series, supporting convergent validity without treating the two concepts as identical. Table 1 reports the distribution and a descriptive cross-sectional dependence diagnostic.

Table 1

Descriptive statistics and cross-sectional dependence

Variable	Mean	SD	Min	Max	CD
General public services	13.21	3.10	7.49	24.72	38.77
Defence	2.79	1.25	0.55	8.51	20.23
Public order and safety	4.00	1.26	1.76	7.66	8.35
Economic affairs	12.36	3.67	5.58	38.63	15.15
Environmental protection	1.74	0.73	-0.64	4.77	3.09
Housing and community	1.72	1.17	0.04	8.27	17.18
Health	13.89	2.67	5.11	24.29	26.40
Recreation and culture	2.83	0.97	1.15	7.61	7.98
Education	11.25	2.05	7.10	18.18	20.95
Social protection	36.21	4.85	22.89	46.96	18.62
Total expenditure (% GDP)	45.51	6.91	20.70	64.90	34.27
Military expenditure (% GDP)	1.42	0.60	0.21	4.15	35.91
Gross debt (% GDP)	63.52	36.84	3.90	209.40	38.71
Net lending (% GDP)	-2.62	3.61	-32.10	5.30	42.46
Real GDP per capita (2010 EUR)	26148	17126	4170	88130	58.35

Note: Budget functions are percentages of total functional expenditure unless otherwise stated. N = 535. CD is the Pesaran (2015) cross-sectional dependence test statistic, testing the null of cross-sectional independence across units; all statistics are distributed asymptotically as standard normal and are significant at $p < .01$.

Source: Eurostat; World Bank; authors' calculations.

Specifically, the Pesaran (2015) cross-sectional dependence (CD) test evaluates the null hypothesis of cross-sectional independence (or weak dependence) against the alternative hypothesis of strong cross-sectional dependence. The test statistic is calculated as follows:

$$CD = \sqrt{[2T / N(N-1)] \sum_{i=1}^{N-1} \sum_{j=i+1}^N \hat{\rho}_{ij}}$$

where N denotes the cross-sectional dimension (i.e., the number of countries), T denotes the time dimension (i.e., the length of the sample period), and $\hat{\rho}_{ij}$ represents the pairwise sample correlation coefficient of the residuals (or variables) between cross-sectional units i and j . Under the null hypothesis of cross-sectional independence, the CD statistic is asymptotically standard normally distributed, $N(0, 1)$, as $N \rightarrow \infty$, provided that T is sufficiently large. In Table 1, the test is applied to the panel series to assess the presence of cross-sectional dependence and, consequently, to establish the empirical necessity of employing Driscoll–Kraay standard errors, which account for general forms of unobserved cross-sectional correlation.

3.2. Variables and empirical strategy

Let S_{kit} be the percentage share of non-defence function k in country i and year t , and let D_{it} be the defence share. The full-panel profile is estimated separately for each of the nine non-defence functions:

$$S_{kit} = \alpha_i + \lambda_t + \beta_k D_{it} + \gamma_k' X_{it} + \varepsilon_{kit} \quad (1)$$

Equation (1) includes country and year fixed effects. The descriptive controls *are total expenditure as a percentage of GDP, debt, net lending, and log of real GDP per capita*. Because the dependent variables and defense exhaust the same budget, $\sum_{k \neq D} \beta_k = -1$ is an accounting identity, not an independent empirical finding (Aitchison, 1982). Driscoll–Kraay standard errors allow general cross-sectional dependence and serial correlation (Driscoll & Kraay, 1998). First differences provide a sensitivity check less dependent on persistent within-country levels.

The post-2022 design is deliberately narrower. For 2016–2024, $Exposed_i$ identifies the eight geographically exposed states and $Post_t$ equals one in 2022–2024. Separate models are estimated for each budget share, each function as a percentage of GDP, and total expenditure as a percentage of GDP:

$$Y_{it} = \alpha_i + \lambda_t + \theta(Exposed_i \times Post_t) + \varepsilon_{it} \quad (2)$$

Equation (2) excludes contemporaneous fiscal controls because they may themselves respond to the invasion and therefore be post-treatment variables. The coefficient θ is an exposed-versus-other change contrast, conditional on common year shocks and time-invariant country characteristics. It is not labeled a causal effect: exposure is fixed, only eight countries are treated, and country-specific shocks can still differ after 2022. The limitations of two-way fixed-effects interpretations are considered explicitly (de Chaisemartin & D'Haultfœuille, 2022).

Exposure is fixed, and the post-period begins in the same year for every exposed country, so the staggered-adoption weighting problem does not arise. Nevertheless, recent difference-in-differences research underscores that dynamic coefficients require cautious interpretation when parallel trends are uncertain, or effects are heterogeneous (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021; Roth et al., 2023).

The event specification uses 2021 as the omitted year and retains every available pre-period coefficient:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\tau \neq 2021} \theta_{\tau} (Exposed_i \times 1[t = \tau]) + \varepsilon_{it} \quad (3)$$

Equation (3) jointly tests the five pre-2021 exposure leads and visualizes the timing of the defense-share contrast. The moderation model uses the 2021 health-plus-social-protection share, R_i , and 2021 debt, B_i , both mean-centered. The full hierarchy is

$$D_{it} = \alpha_i + \lambda_t + \theta E_i P_t + \pi_R(P_t R_i) + \pi_B(P_t B_i) + \delta_R(E_i P_t R_i) + \delta_B(E_i P_t B_i) + \varepsilon_{it} \quad (4)$$

Equation (4) contains the lower-order post-by-moderator terms required to interpret the triple interactions. Country fixed effects absorb the time-invariant main effects of exposure, rigidity, and debt.

3.3. Inference and robustness

Country-clustered standard errors are reported for Equations (2)–(4). Cluster-robust inference is asymptotic in the number of independent clusters and can be unreliable when treatment is concentrated in only a few groups (Cameron & Miller, 2015; Ferman & Pinto, 2019). Since eight treated clusters make conventional asymptotics fragile, the preferred p-values use a restricted wild-cluster bootstrap with Webb's six-point weights and 3,999 repetitions (MacKinnon & Webb, 2018). Randomization inference permutes the eight-country exposure label 1,999 times. Benjamini–Hochberg q-values control the false discovery rate within each family of functional outcomes (Benjamini & Hochberg, 1995).

For H1b, two one-sided tests assess whether the full 95% confidence interval lies inside a prespecified ± 0.25 percentage-point interval (Lakens, 2017). The choice of the ± 0.25 percentage-point equivalence band follows Lakens' (2017) smallest effect size of interest (SESOI) framework and is grounded in the institutional dynamics of EU public budgeting. First, in a normalized 100-point functional budget where median civilian shares range between 1.7% and 13.9%, a shift of less than a quarter of a percentage point falls within routine annual budgetary drift, technical reclassifications, and baseline inflation adjustments rather than intentional political reprioritization. Second, given an average EU government expenditure envelope of approximately 45.5% of GDP (Table 1), a 0.25-percentage-point displacement in the functional budget corresponds to roughly 0.11% of national GDP (i.e., $0.0025 \times 0.455 \approx 0.00114$), which is widely recognized as economically negligible in macro-fiscal consolidation and rebalancing benchmarks. Thus, associations constrained strictly within $[-0.25, +0.25]$ reflect negligible displacement rather than economically meaningful structural substitution. A non-significant difference test is otherwise described as inconclusive rather than as evidence of zero. Robustness checks extend Equation (2) to 2005–2024, exclude 2020–2021, add country-specific linear trends, classify Sweden as exposed, and omit each exposed country in turn. These specifications address window choice, pandemic contamination, differential trends, geographic coding, and single-country leverage.

4. EMPIRICAL RESULTS AND DISCUSSION

4.1. Descriptive patterns

Social protection is the largest average function at 36.21% of total expenditure, followed by health (13.89%), general public services (13.21%), economic affairs (12.36%), and education (11.25%). Defense averages 2.79% of the functional budget. Figure 1 shows that the exposed and other-country means moved broadly together before 2022, followed by a visibly wider but heterogeneous gap in 2023–2024.

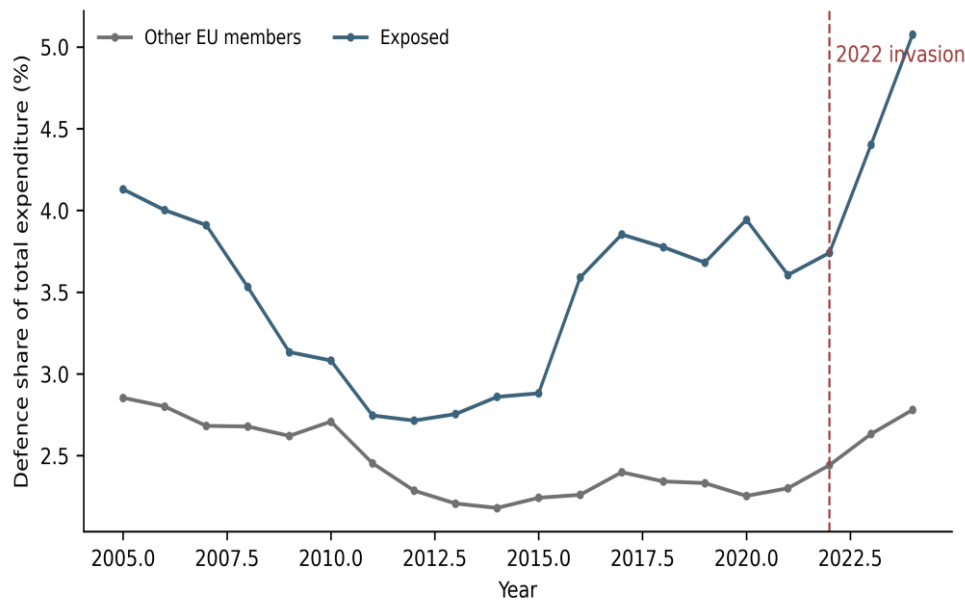


Figure 1. Mean defense share of total expenditure by geographic exposure, 2005–2024
Source: Authors' calculations from Eurostat data.

Figure 1 is descriptive and should not be read as a treatment-effect plot. Among exposed countries, the change from 2021 to 2024 ranges from a slight decline in Slovakia to increases of more than two percentage points in Estonia, Lithuania, Hungary, and Poland. This variation is why the analysis reports leave-one-country-out results and few-treated-cluster inference.

4.2. The full-panel budget-share profile

Table 2 presents Equation (1). The sum of coefficients is -1.000 to numerical precision, as required by the normalized budget identity. The largest association is in economic affairs: a one-point increase in the defense share is associated with a 0.871-point decrease in the economic-affairs share (SE = 0.305, 95% CI [-1.468, -0.273], $p = .004$, $q = .019$). Environmental protection, housing, and recreation also have negative q -values below .05, but their entire confidence intervals fall inside the ± 0.25 equivalence band. Their statistical detectability should therefore not be confused with large substantive displacement.

Table 2

Full-panel budget-share profile, 2005–2024

Non-defence function	b	DK SE	95% CI	p	BH q	$\pm .25$ equiv.
General public services	0.177	0.108	[-0.035, 0.389]	.102	.184	No
Public order and safety	-0.022	0.063	[-0.146, 0.102]	.725	.929	Yes
Economic affairs	-0.871	0.305	[-1.468, -0.273]	.004	.019	No
Environmental protection	-0.118	0.047	[-0.209, -0.026]	.011	.027	Yes
Housing and community	-0.071	0.028	[-0.127, -0.016]	.012	.027	Yes
Health	-0.007	0.074	[-0.153, 0.138]	.921	.929	Yes
Recreation and culture	-0.092	0.032	[-0.154, -0.029]	.004	.019	Yes
Education	-0.014	0.064	[-0.139, 0.112]	.831	.929	Yes
Social protection	0.018	0.199	[-0.373, 0.408]	.929	.929	No

Note: Equation (1), country and year fixed effects, Driscoll–Kraay (DK) standard errors. BH q is the Benjamini–Hochberg adjusted p -value across nine functions. Equivalence means the 95% CI is within ± 0.25 points. $N = 535$.
Source: Authors' calculations.

For public order, health, and education, Table 2 supports equivalently small associations. Social protection is different: its estimate is near zero, but the confidence interval is too wide to establish equivalence. General public services are similarly inconclusive. H1a is supported only in a qualified sense, because economic affairs is the dominant negative association, while the first-difference estimate remains negative (-0.807 , $SE = 0.302$) but misses the 5% false-discovery threshold ($q = .069$). H1b is supported for public order, health, and education, but not for social protection.

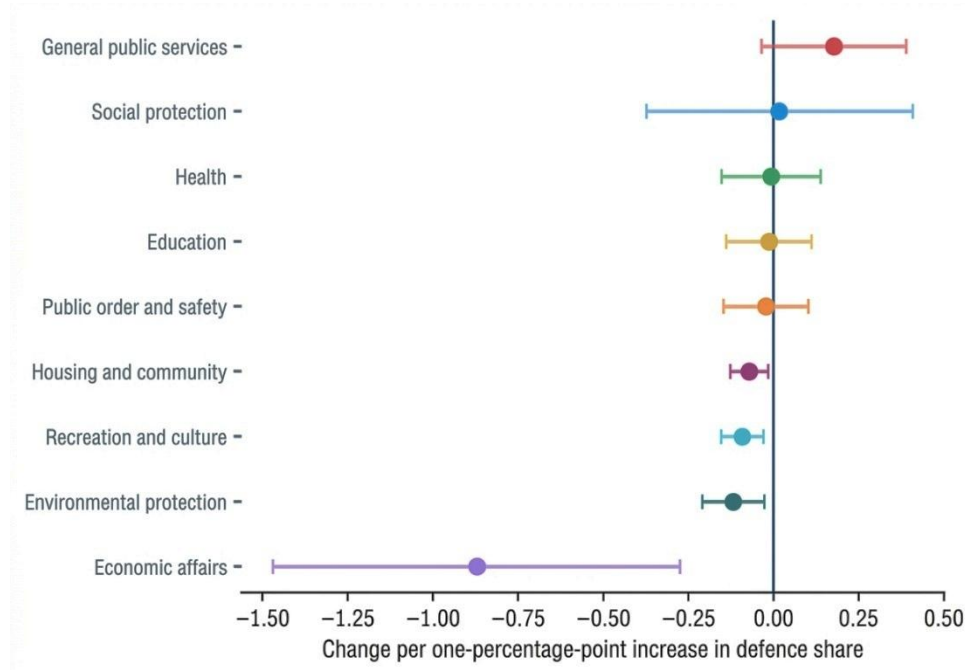


Figure 2. Estimated budget-share profile and 95% confidence intervals

Source: Authors' calculations.

Figure 2 makes the distinction visible: only economic affairs cross the zero line and extend beyond the equivalence region. The coefficient does not identify a euro-for-euro cut in transport or public investment. COFOG economic affairs combines investment, subsidies, operating expenditure, and sectoral interventions, and the share can fall even when its level rises more slowly than the total budget.

4.3. Post-2022 exposure, composition, and spending scale

Table 3 reports Equation (2) on two scales. In Panel A, the exposed-country post-2022 coefficient for the defense share is 0.362 percentage points (clustered $SE = 0.313$; wild-bootstrap $p = .267$; $q = .445$). No budget-share outcome survives false-discovery control. H2 is therefore not supported in the preferred specification. This conclusion differs from one based solely on conventional or cross-sectional standard errors and illustrates why inference must reflect the number of treated clusters.

Table 3

Post-2022 exposed-country contrasts, 2016–2024

Outcome	b	Cluster SE	95% CI	Wild p	BH q
Panel A. Share of total expenditure					
General public services	1.050	0.516	[0.038, 2.062]	.037	.370
Defence	0.361	0.313	[-0.252, 0.975]	.267	.445
Public order and safety	-0.184	0.158	[-0.493, 0.126]	.258	.445
Economic affairs	0.199	0.650	[-1.075, 1.473]	.762	.847
Environmental protection	-0.142	0.090	[-0.319, 0.036]	.130	.432
Housing and community	-0.215	0.268	[-0.739, 0.310]	.440	.550
Health	-0.634	0.528	[-1.669, 0.401]	.218	.445
Recreation and culture	-0.350	0.211	[-0.764, 0.063]	.086	.432
Education	-0.175	0.211	[-0.588, 0.238]	.389	.550
Social protection	0.089	0.568	[-1.024, 1.202]	.876	.876
Panel B. Expenditure as % of GDP					
General public services	0.808	0.250	[0.318, 1.298]	.001	.010
Defence	0.275	0.133	[0.014, 0.537]	.075	.188
Public order and safety	0.077	0.078	[-0.076, 0.230]	.309	.516
Economic affairs	0.466	0.321	[-0.163, 1.095]	.138	.275
Environmental protection	-0.022	0.047	[-0.114, 0.071]	.632	.704
Housing and community	-0.057	0.133	[-0.317, 0.203]	.725	.725
Health	0.171	0.208	[-0.237, 0.578]	.409	.584
Recreation and culture	-0.067	0.095	[-0.252, 0.119]	.633	.704
Education	0.280	0.120	[0.045, 0.514]	.040	.132
Social protection	1.115	0.405	[0.321, 1.910]	.008	.040
Total expenditure envelope	3.034	0.751	[1.562, 4.506]	<.001	—

Note: Equation (2), country and year fixed effects. Wild p uses 3,999 restricted wild-cluster bootstrap repetitions; BH q adjusts within each ten-function panel. N = 243. The envelope row is outside the functional test family—source: Authors' calculations.

Panel B changes the interpretation. The defence-to-GDP contrast is 0.275 points (wild p = .075; q = .188), which is suggestive but not robust at the 5% level. General public services and social protection rise significantly as shares of GDP after adjustment, while the total expenditure envelope rises by 3.034 percentage points of GDP (SE = 0.751, 95% CI [1.562, 4.506], wild p < .001). The randomization p-value for the envelope is .002, compared with .123 for the defense share. Thus, the exposed group's early fiscal response is better characterized by a larger budget than by broad substitution away from civilian functions.

The event coefficients in Figure 3 do not indicate a single significant trend: $\chi^2(5) = 5.65$, $p = .342$. The 2024 coefficient is 0.991 points (95% CI [-0.007, 1.989], $p = .052$), suggesting that the relative defense-share change may have emerged late and remains imprecisely estimated. The event study reduces concern about a simple pre-existing divergence, but it cannot rule out post-2022 shocks correlated with geography.

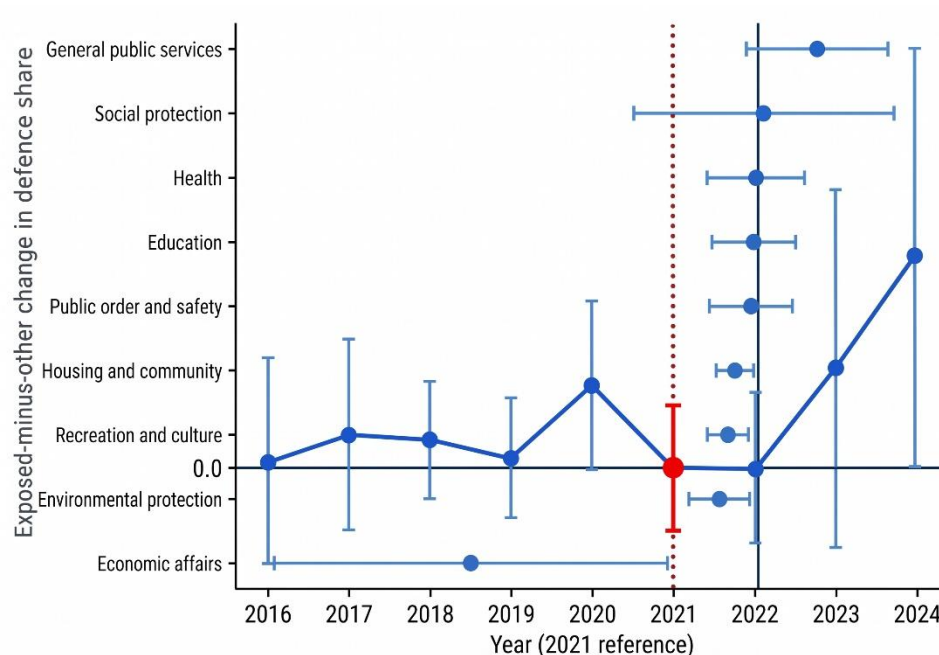


Figure 3. Event-time exposed-country differences in the defense share, 2016–2024 (2021 omitted)

Source: Authors' calculations.

4.4. Budget rigidity and fiscal space

Table 4 applies the full hierarchy in Equation (4). In the rigidity-only model, the triple interaction is -0.064 (SE = 0.030) under conventional clustered inference, but its wild-bootstrap p-value is .112. In the joint model, the rigidity interaction is -0.059 (SE = 0.042; wild p = .490), and the debt interaction is -0.010 (SE = 0.014; wild p = .713). Neither inherited commitments nor debt provides robust evidence of moderation.

Table 4

Moderation of the post-2022 defense-share contrast by budget rigidity and pre-existing debt

Specification	Exposure × Post [95% CI] (wild p)	× Rigidity [95% CI] (wild p)	× Debt [95% CI] (wild p)	Within R ²
Exposure only	0.361 [-0.252, 0.975] (.267)	—	—	0.041
Rigidity hierarchy	0.281 [-0.342, 0.904] (.339)	-0.064 [-0.126, -0.002] (.112)	—	0.081
Debt hierarchy	0.039 [-0.684, 0.762] (.951)	—	-0.011 [-0.040, 0.018] (.626)	0.102
Both hierarchies	-0.014 [-0.755, 0.727] (.978)	-0.059 [-0.145, 0.027] (.489)	-0.010 [-0.039, 0.019] (.713)	0.136

Note: All models estimate Equation (4) with country and year fixed effects. Cells report point estimates, [95% cluster-robust confidence intervals], and (wild-bootstrap p-values with 3,999 repetitions). Rigidity is the sample-centered 2021 health-plus-social-protection share; debt is the sample-centered 2021 gross government debt-to-GDP ratio. N = 243. Source: Authors' calculations.

The implied exposure contrast remains statistically indistinguishable from zero across the observed range of treated-country rigidity (39.27%–59.72%). H3 is not supported. This does not show that fiscal constraints are irrelevant; it shows that the available cross-sectional variation and short post-period do not identify the predicted attenuation with adequate precision.

4.5. Robustness and interpretation

Table 5 summarizes the defense-share robustness checks. The full 2005–2024 window produces a larger coefficient, but the preferred wild-bootstrap p-value is .101. Excluding the pandemic years, adding country trends, and including Sweden as exposed do not generate 5% evidence. Leave-one-exposed-country-out coefficients range from 0.207 to 0.527, with wild p-values from .123 to .524.

Table 5

Robustness of the post-2022 defense-share contrast

Check	b	Cluster SE	95% CI	Wild p
Baseline 2016–2024	0.361	0.313	[-0.252, 0.975]	.257
Full 2005–2024	0.839	0.426	[0.004, 1.674]	.101
Exclude 2020–2021	0.397	0.316	[-0.223, 1.016]	.233
Country-specific linear trends	0.074	0.313	[-0.540, 0.688]	.816
Sweden is classified as exposed	0.465	0.285	[-0.093, 1.023]	.118
Leave one exposed country out	0.207– 0.527	—	—	.123–.524

Note: All models include country and year fixed effects. Leave-one-out entries report the summary range across the eight individual exposed-country exclusions; detailed country-by-country results confirming that no single state drives the baseline estimate are provided in Appendix Table A1

Source: Authors' calculations.

The robust result is consequently the scale response, not a post-2022 compositional cut. The long-panel profile still indicates that defense-intensive country-years tend to allocate relatively less to economic affairs. However, this association may combine policy substitution, reverse adjustment, and common shocks. Fiscal consolidation studies show that composition matters (Alesina & Perotti, 1997), while fiscal rules and revenue capacity shape the feasible envelope (Apeti et al., 2025; Lyeonov et al., 2026). Recent macroeconomic assessments similarly stress that the effects of rearmament depend on procurement content, import leakages, fiscal space, and cross-border spillovers (Checherita-Westphal et al., 2025; Furceri et al., 2026; OECD, 2026). The evidence here fits a two-margin account: governments can adjust both the division of expenditure and its total size.

The result also changes the environmental interpretation. Environmental protection has a small negative share association in Table 2, yet the post-2022 exposed-country coefficient as a percentage of GDP is -0.022 and statistically weak (wild p = .632). Claims that rearmament already displaced climate expenditure would therefore exceed the evidence. Climate delivery may still face opportunity costs due to economic-affairs programs, financing constraints, or administrative capacity (Ciot, 2023; Meckling & Strecker, 2022; Niu, 2024), and these pressures may be spatially uneven where governance capacity and SDG performance diverge across regions (Tutar & Kyriakopoulos, 2026). Renewable deployment also depends on technology-specific policy mixes and flexibility investments rather than on the environmental protection budget line alone (Tutar & Štreimikienė, 2026). COFOG aggregates cannot isolate those channels.

4.6. Policy implications and limitations

The policy implication is not that rearmament is costless. A three-point expansion of total expenditure relative to GDP must ultimately be financed through revenue, borrowing, or later adjustment, and debt conditions differ considerably across the EU (Dudzevičiūtė et al., 2021; Toth et al., 2025). Comparative EU evidence likewise emphasizes that the growth and debt consequences depend on the allocation between personnel, equipment, and investment and on the division of financing responsibilities (Cepparulo et al., 2025). The finding instead identifies the timing of the trade-off: during 2022–2024, exposed governments expanded the fiscal envelope sufficiently that widespread civilian cuts are not visible in aggregate COFOG

data. If security commitments persist, future constraints may appear after the short post-period examined here.

Four limitations bound the conclusions. First, the exposure design has eight treated countries and only three post years. Wild-bootstrap and randomization inference improve calibration but cannot create information. Second, COFOG functions are broad: economic affairs are not synonymous with public investment, and environmental protection does not capture all green expenditure. Third, annual data cannot identify within-year appropriations, off-budget instruments, EU-level financing, or procurement backlogs. Fourth, Equation (1) is both associational and compositional; the exact sum of coefficients reflects accounting closure. The results are therefore evidence about relative budget structure, not a welfare ranking or a structural marginal propensity to cut.

5. CONCLUSION

This study revises the guns-versus-butter question by separating budget composition from expenditure scale. In the EU-27 over 2005–2024, economic affairs show the largest negative association with the defense share. Health, education, and public order are equivalently small within a ± 0.25 -point bound, while the evidence for social protection is inconclusive. These results provide partial support for H1a and H1b, with important qualifications regarding category and robustness.

The post-2022 analysis does not support H2 or H3 under inference appropriate for eight exposed clusters and multiple outcomes. The defense-share contrast is positive but imprecise; moderation by prewar commitments or debt is not robust. By contrast, exposed states increase total expenditure by approximately 3.0 percentage points of GDP. The early rearmament episode is therefore characterized more clearly by fiscal expansion than by broad civilian displacement. Longer post-periods and more disaggregated expenditure data will be needed to determine whether this financing pattern endures or merely postpones the need for harder budget choices.

DATA AND CODE AVAILABILITY

The analysis uses public Eurostat and World Bank series identified in Section 3.1. The accompanying replication package contains the derived panel, source metadata, analysis scripts, coefficient tables, and figure files needed to reproduce the reported results.

CONFLICT OF INTEREST

The authors declare no conflict of interest.

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APPENDIX

Table A1

Post-2022 defense-share contrast: Leave-one-exposed-country-out sensitivity (2016–2024)

Excluded Country	b	Cluster SE	95% CI	Wild Bootstrap p-value
<i>Baseline (All exposed)</i>	<i>0.361</i>	<i>0.313</i>	<i>[-0.252, 0.975]</i>	<i>.257</i>
Excluding Estonia	0.334	0.318	[-0.290, 0.958]	.284
Excluding Finland	0.382	0.320	[-0.245, 1.009]	.241
Excluding Hungary	0.312	0.301	[-0.278, 0.902]	.310
Excluding Latvia	0.355	0.322	[-0.276, 0.986]	.265
Excluding Lithuania	0.329	0.315	[-0.288, 0.946]	.298
Excluding Poland	0.207	0.274	[-0.330, 0.744]	.524
Excluding Romania	0.398	0.325	[-0.239, 1.035]	.219
Excluding Slovakia	0.527	0.311	[-0.083, 1.137]	.123

Note: Models estimate Equation (2) over 2016–2024 with country and year fixed effects, dropping each geographically exposed country sequentially. Wild bootstrap p-values are calculated using 3,999 repetitions with Webb weights. Across all iterations, the exposure contrast remains statistically indistinguishable from zero at the 5% level, verifying that the empirical findings do not depend on the leverage of any single member state

Source: Authors' calculations.