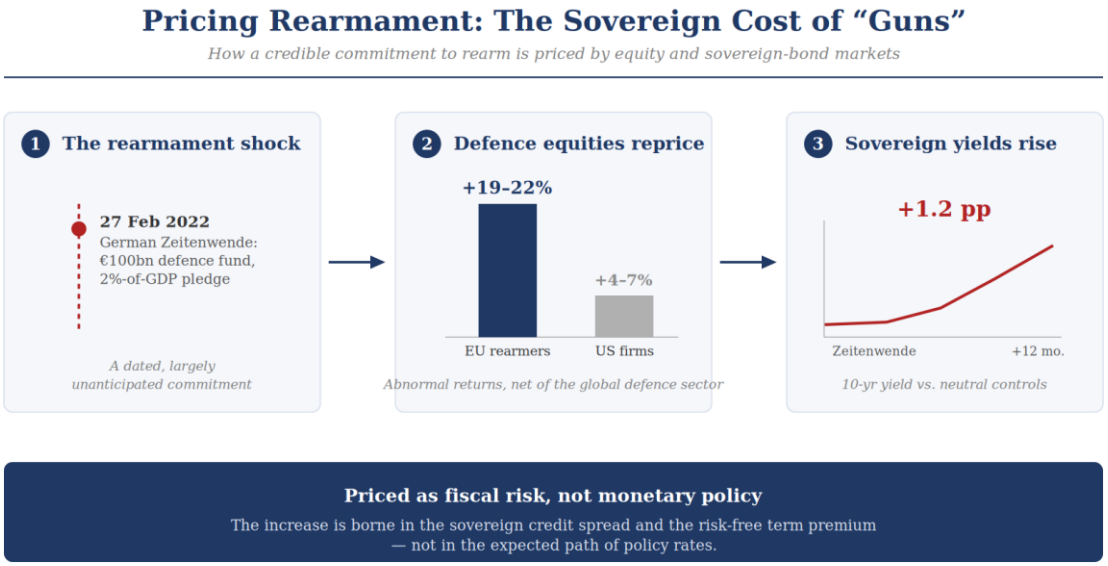
Guns and Bonds: The Sovereign Cost of Rearmament

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Abstract
Since Russia's 2022 invasion of Ukraine, European governments have committed to the largest peacetime rearmament since the Cold War. We ask whether a credible commitment to rearm raises a sovereign's borrowing cost, through what channel, and whether equity markets can measure the underlying shock. Using a cross-country difference-in-differences design around Germany's Zeitenwende, we find that rearming sovereigns' ten-year yields rise by 0.7 to 1.0 percentage points over the following year relative to comparable controls, and by 1.22 points against non-NATO European neutrals. The effect works through sovereign credit risk and the term premium, not the expected path of monetary policy: spreads over a non-treated safe anchor widen by 0.60 points, and roughly half of the concurrent move in risk-free yields is term premium. We also construct a market-implied rearmament shock from the global cross-section of defence-contractor equities: committing-country defence stocks earn abnormal returns of 19 to 22 percent net of the global defence sector, concentrated in pure-play contractors in the highest-spending countries. Its largest realizations, identified with no preset event dates, coincide with the invasion and the Zeitenwende. Credible rearmament, we conclude, is priced as a fiscal-risk event.
Keywords: Sovereign risk; Military spending; Geopolitical risk; Event study; Difference-in-differences; Term premium

1. Introduction



Since Russia's invasion of Ukraine on 24 February 2022, the advanced economies have entered the largest peacetime rearmament since the Cold War. Germany's *Zeitenwende*, announced by Chancellor Scholz on 27 February 2022 and backed by a €100 billion special defence fund (*Sondervermögen*) and a permanent commitment to spend above two percent of GDP on defence, was followed within three years by comparable commitments across NATO and its partners, against a backdrop of sharply elevated geopolitical risk (Caldara & Iacoviello, 2022).

These are not transitory fiscal shocks: they raise the expected path of government spending for a decade or more, concentrate that spending in a specific and identifiable industrial base, and arrive with unusually credible political backing. This paper asks a first-order but, to our knowledge, empirically unanswered question: does a credible commitment to rearm raise a sovereign's cost of borrowing, through what channel, and can the equity market's repricing of defence firms be used as a market-implied gauge of the underlying spending shock?

The setting is unusually favourable for identification. The Zeitenwende supplies a dated, largely unanticipated regime shift concentrated in a handful of days; a global cross-section of listed defence contractors supplies a high-frequency, market-based read on revised military-spending expectations, in the spirit of Fisher & Peters (2010), who uses the excess stock returns of large defence contractors to identify government spending shocks, and Gurdgiev et al. (2022), who use defence-spending news to purge fiscal shocks of their endogenous response to the business cycle; and a cross-country panel of sovereign yields lets us difference out the common global monetary-tightening cycle of 2022–2023 that has confounded single-country studies of this period.

The results below confirm two central claims, a measurable sovereign cost of rearmament and a fiscal-risk transmission mechanism, and deliver a well-identified, market-implied rearmament shock, while being explicit about the one prediction the current data cannot yet establish.

We proceed in three steps. First, in Section 4 we construct and validate a market-implied rearmament shock from the daily abnormal returns of 27 global defence contractors headquartered in eleven countries. Around the Zeitenwende, European committing-country defence equities earn a cumulative average abnormal return of 18.8 to 22.2 percent net of both the world market and the global defence sector, and the response is concentrated in the committing countries specifically, rather than in the sector globally.

Constructed as a continuous rolling series that requires no hardcoded event dates, the measure's two largest realizations over two decades are the March 2025 "ReArm Europe" announcement and the Zeitenwende itself, a validation the estimator locates on its own.

Second, in Section 5 we show that committing sovereigns' ten-year government bond yields rise relative to comparable controls following the Zeitenwende. The response builds gradually rather than jumping on impact: event-study and local-projection estimates show a flat pre-trend in the full sample. followed by an effect that accumulates to roughly 0.7 to 1.0 percentage points over the year after the announcement.

Against the cleanest available control group, the non-NATO European neutrals, Switzerland, Austria, and Ireland, the estimated effect is 1.22 percentage points and highly significant. The result is stable across heterogeneity-robust staggered-treatment estimators and the exclusion of any single treated country, with leave-one-out estimates ranging from 0.60 to 0.83 percentage points. To our knowledge, this is the first systematic causal estimate of the sovereign financing cost of a credible rearmament commitment.

Third, in Section 6 we show that this yield response is a fiscal-risk phenomenon rather than a repricing of the expected monetary-policy path. Measured against a non-treated safe anchor, committing sovereigns' credit spread rises by about 0.60 percentage points, a genuine sovereign risk premium rather than a common movement in the level of interest rates.

Decomposing the risk-free curve around the announcement using the term-structure model of Abrahams et al. (2012), we find that roughly half of the concurrent move in the ten-year yield is term premium rather than the expected short-rate path.

Both results point the same way: markets price a credible rearmament commitment as a durable increase in fiscal and supply risk, borne in the term premium and the sovereign credit spread, rather than as an anticipated tightening of monetary policy.

We use this decomposition deliberately and interpret it narrowly. Our cross-country identification of the sovereign-specific channel comes entirely from the spread over safe benchmarks in Section 6.1, which requires no term-premium model; the decomposition is used only to characterize the common, global risk-free rate around the announcement, for which the US ten-year is the standard benchmark in this literature.

We do not attribute the decomposed term premium to any individual European sovereign. Euro-area term-premium estimates are less standardized and span a shorter sample, and using them would not change the object we decompose, which is the global risk-free component common to every sovereign in our panel.

This mechanism finding reframes rearmament as a sovereign-risk event rather than a monetary one, and distinguishes our paper from the geopolitical-risk literature that emphasizes the monetary and safe-haven channel.

We then turn to cross-sectional heterogeneity. In Section 7 we show that the sovereign-yield response is, if anything, directionally larger for the states most exposed to the shift in the security environment, and that the equity repricing documented in Section 4 is concentrated in pure-play defence contractors, those without a large civilian or diversified business, domiciled in the countries that went on to increase military spending the most.

The firm-level result links the aggregate sovereign-risk channel to the specificity of the underlying fiscal shock: the repricing is largest precisely where the shock is most concentrated in the domestic industrial base.

Section 8 reports an extensive set of validation and robustness checks and, in keeping with our objective of reporting findings honestly rather than selectively, states plainly where the evidence does not yet support every prediction one might make.

In particular, we do not find that the market-implied shock predicts realised military expenditure in a country-year panel through 2024; we argue that this most plausibly reflects a timing problem, procurement and budget execution lag announcements by several years, rather than evidence against the validity of the shock as an event-level measure, and we describe the direct test that would resolve the question once post-2024 spending data become available.

In sum, our evidence indicates that a credible commitment to rearm carries a measurable sovereign financing cost, that this cost is a fiscal-risk rather than a monetary phenomenon, and that the global cross-section of defence equities can be used to construct a continuous, forward-looking gauge of the underlying spending shock. We hope our findings spur further work connecting the asset-pricing literature on rare-disaster and geopolitical risk to the public finance of national security, a connection we believe has been underexplored relative to its evident policy relevance.

Related literature

Our paper connects to several literatures. A first strand studies how geopolitical risk is priced in financial markets. Caldara and Iacoviello (2022) construct a text-based geopolitical risk index and show that geopolitical risk shocks depress investment and raise equity risk premia; (Baker et al., 2022) document related effects for economic policy uncertainty more broadly.

(Alshammari et al., 2024) show that war risk moves US bond and equity prices, and (Wachter & Zhu, 2025) and (Kapopoulos et al., 2025) use interstate crises and war probabilities to identify the pricing of rare-disaster risk in equities. This literature typically treats geopolitical shocks as demand-side, risk-aversion events that raise risk premia and, if anything, lower safe sovereign yields through flight-to-safety.

We contribute a within-region, cross-sectional dimension that this literature does not exploit: because rearmament commitments differ across otherwise similarly exposed European sovereigns, we can separate the fiscal-risk channel specific to the committing state from the common safe-haven and risk-aversion channel that affects all sovereigns alike.

A second literature uses military spending to identify fiscal shocks. Olejnik & Kuna (2025) and Ginn, (2023) use narrative and news-based measures of anticipated defence spending to estimate government spending multipliers, arguing that military build-ups are close to ideal natural experiments because they are large, plausibly exogenous to the domestic business cycle, and pre-announced.

(Gurdgiev et al., 2022b) is closest to our approach in spirit: they use the excess stock returns of large US defence contractors to construct a continuous, market-based measure of revisions to expected defence spending, and show that this measure tracks defence-spending news and predicts subsequent government purchases. We extend this approach to a cross-country, cross-firm setting and, unlike this literature, use the resulting shock to study sovereign borrowing costs rather than the domestic output multiplier.

A third literature studies the determinants of sovereign credit risk and spreads. Longstaff et al. (2011) show that a large share of sovereign credit risk is explained by global risk factors rather than country fundamentals. Relative to this literature, we identify a specific, dated, and plausibly exogenous fiscal shock, the credible commitment to rearm, and trace its propagation into sovereign yields using a design that differences out the common global factors this literature emphasizes.

Finally, because monetary policy tightened sharply over the same period in which European rearmament commitments were made, isolating a fiscal-risk channel requires care. We draw on the high-frequency monetary policy surprise literature (Bauer & Swanson, 2023; Ciganovic et al., 2026; Nakamura et al., 2022); and, methodologically, Bricongne & Marolleau (2025) to control directly for the monetary-policy path, and on the term-structure decomposition of (Adrian et al., 2019)to separate the term-premium and expected-rate components of the risk-free curve around the announcement.

Methodologically, we also rely on the recent literature on difference-in-differences with staggered and heterogeneous treatment timing (Callaway & Sant'Anna, 2021) and on local projections (Jorda & Taylor, 2025), which is a particular concern here because rearmament commitments were staggered over 2022–2025.

2. Theoretical exposition

Our paper is primarily empirical, and we leave a fully articulated structural model of sovereign risk under a rearmament shock to future work. Here we sketch the minimal logic that motivates our empirical design and organizes our hypotheses.

Consider a sovereign that faces a stochastic path of government spending and finances a given expenditure path by issuing long-term debt. A standard tax-smoothing argument (Teles & Tristani, 2024) implies that the government prefers to spread the fiscal burden of a spending increase over time rather than raise current taxes discretely, which requires issuing additional debt when a spending increase is announced rather than only once the spending is realised.

If lenders price both the expected path of debt issuance and the risk that debt service coincides with adverse states of the world, a credible, durable, and geopolitically motivated commitment to raise spending should raise the sovereign's borrowing cost through two channels that we can separately measure: (i) a term-premium channel, operating through the expected path of debt issuance and its covariance with bad states of the global economy, and (ii) a sovereign-credit channel, operating through the market's assessment of country-specific fiscal sustainability. Both channels imply a rise in the yield that is unrelated to the expected path of the policy rate, which is the empirical distinction we exploit throughout the paper. This logic yields five predictions that organize our empirical work.

H1 (feasibility). If the Zeitenwende and the associated wave of commitments constitute a genuine, unanticipated regime shift, the equity of defence contractors domiciled in committing countries should earn a positive, statistically significant abnormal return around the announcement, net of the global defence sector. We test H1 in Section 4 and treat it as a feasibility gate for the rest of the analysis: our design is uninformative unless markets in fact treated the Zeitenwende as news, and Section 4.2 makes this test explicit before we proceed to any yield regressions.

H2 (sovereign cost of guns). Committing sovereigns' government bond yields should rise, relative to non-committing controls, following the announcement.

H3 (mechanism). The H2 effect should load on the term premium and the sovereign credit spread rather than on the expected path of monetary policy, distinguishing rearmament from a conventional demand-side or safe-haven shock.

H4 (predictive validity). If defence-equity returns are a valid market-based proxy for revisions to expected military spending, in the spirit of (Fisher & Peters, 2010), the resulting shock series should positively predict realised growth in military expenditure.

H5 (fiscal space). The yield response in H2 should be larger for sovereigns with less pre-existing fiscal space and for states more directly exposed to the deteriorating security environment (frontline states), since these are the sovereigns for whom the news is most consequential for debt sustainability.

We also examine an auxiliary objective concerning firm-level heterogeneity: within the set of committing countries, the equity repricing in H1 should be concentrated in pure-play defence contractors, for which the news is most informative about future cash flows, rather than in diversified or civil-aviation-heavy firms.

3. Data and summary statistics

Every series used in this paper is either freely downloadable (see Annexure A1) or accessible through a documented public interface, and we describe the exact construction of each variable in the subsections below, so that the analysis is directly reproducible without a commercial data terminal.

The analysis file is a monthly panel of 21 advanced sovereigns spanning 2004 to 2026, supplemented by a daily layer used for the high-frequency event studies. We describe each source in turn, sovereign yields, defence-contractor equities, the treatment classification, monetary-policy surprises, the risk-free-rate decomposition, the control set, and military expenditure, before turning to summary statistics.

The one series we do not require for the results reported here, general government gross debt from the IMF World Economic Outlook, would be needed only to sharpen the fiscal-space tests of Section 7 with a hard, debt-based moderator in place of the market-based proxy we use there; we flag it as a defined extension rather than a component of the present analysis.

3.1. Sovereign bond yields and spreads

We collect month-end (and, for the event-study and local-projection analyses, daily) benchmark ten-year government bond yields for 21 advanced economies from the Federal Reserve Economic Data (FRED) database, the European Central Bank Statistical Data Warehouse, and the OECD long-term interest rate series, cross-checked across sources where coverage overlaps.

The panel spans January 2004 to 2026 and is unbalanced, since several countries enter the sample only after their government bond markets reached sufficient liquidity. The 21 countries are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, the Netherlands, Norway, Poland, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States.

We compute the spread as the difference between a country's ten-year yield and a benchmark safe yield; unless noted otherwise, we report the spread over the German ten-year Bund yield, and, for the mechanism tests in Section 6, additionally over the US ten-year Treasury yield. **Table 1** presents summary statistics for the resulting monthly panel of 5,689 country-month observations: the mean ten-year yield is 2.79 percent with a standard deviation of 2.19 percentage points, and the mean spread over the Bund is 95 basis points.

Table 1
Summary statistics.

Variable	N	Mean	SD	p10	Median	p90
10-year yield (%)	5689	2.794	2.191	0.302	2.840	4.760
Spread vs Bund (pp)	5398	0.954	1.864	-0.125	0.523	2.380
Fiscal balance/GDP (%)	5418	-1.832	4.979	-7.028	-2.034	2.135
GDP growth (%)	5689	1.718	3.114	-1.613	1.908	4.695
Geopolitical risk	5689	114.771	38.003	83.646	104.166	154.924
VIX	5689	18.961	7.961	12.296	16.911	27.104
MOVE	5689	85.340	29.670	54.748	78.809	123.016
Breakeven inflation 10Y (%)	5689	2.122	0.391	1.617	2.250	2.503
US dollar index	5689	90.377	9.407	78.235	90.503	102.949

3.2. Defence-contractor equities and factor benchmarks

We build a global cross-section of 27 publicly listed defence contractors headquartered in eleven countries: the United States, Germany, France, the United Kingdom, Italy, Sweden, Norway, Israel, South Korea, Japan, and India (the full list, together with firm-level results, appears in Table A2).

We select firms with a material share of revenue from defence and aerospace procurement and a continuous listing history spanning our estimation and event windows. Daily total-return series are collected from public market-data sources; this differs from the loan- and flow-level microstructure data used in comparable studies of financial intermediation, but is deliberately chosen so that the resulting shock series is reproducible without a commercial license.

We benchmark returns against the MSCI World Index (MXWO) as the world-market factor and against a global aerospace-and-defence sector index (a US-listed aerospace-defence exchange-traded fund) as the sector factor, following the logic of Fisher and Peters (2010), who net out sector-wide demand shocks to isolate country- and firm-specific revisions to expected government spending.

3.3. Treatment and control classification

We classify countries as treated if they are European NATO member states that announced an explicit, quantified increase in defence spending or established a dedicated defence-spending vehicle following the Zeitenwende (for example, Germany's €100 billion *Sondervermögen*).

Our cleanest control group is the set of non-NATO European neutrals, Switzerland, Austria, and Ireland, which share Europe's economic and monetary environment but did not make comparable rearmament commitments. We additionally identify a set of non-European advanced economies, the United States, Canada, Australia, and Japan, as an anchor group that shares the global monetary tightening cycle of 2022–2023 but is geographically and politically distant from the immediate European security shock; we use this group both as an additional control and, in Section 7, in the triple-difference fiscal-space tests.

Within the treated group, we separately flag frontline states, Poland and the Nordic states Finland, Sweden, and Norway, whose direct exposure to the deteriorating security environment on Europe's eastern and northern borders is plausibly greatest.

3.4. Monetary policy surprises

Because global monetary policy tightened over the same period as the European rearmament commitments, we control directly for the monetary-policy path using established high-frequency monetary policy surprise series: the US series of Bauer and Swanson (2023b) and the euro area monetary policy event-study database of (Altavilla et al., 2019).

These series isolate the unexpected component of policy announcements from asset-price movements in a narrow window around each announcement, following the logic of Kuttner (2001) and Nakamura and Steinsson (2018**)**, and let us confirm that our results are not an artefact of the concurrent monetary tightening cycle (Sections 5 and 7).

3.5. Risk-free rate decomposition

To decompose the risk-free curve into its term-premium and expected-short-rate components, we use the Adrian, Crump, and Moench (2013) term-structure model, retrieving updated model output from the Federal Reserve Bank of New York.

Because country-specific term-premium estimates of this kind are not publicly available for our full cross-section, we apply the decomposition to the US Treasury curve, which serves as the common risk-free benchmark against which the country-specific credit-spread tests in Section 6 are measured.

3.6. Control variables

Our regressions control for the standard covariates known to move sovereign yields: the general government fiscal balance as a share of GDP and real GDP growth (IMF World Economic Outlook), the Caldara and Iacoviello (2022) geopolitical risk index, the CBOE Volatility Index (VIX) and the ICE BofA MOVE Index as measures of equity- and rates-market risk aversion, ten-year breakeven inflation, and the US dollar index (DXY) as a measure of global dollar funding conditions.

Table 1 reports summary statistics for the full control set, and Table 2 reports the pairwise correlation matrix; as expected, the geopolitical risk index and the MOVE index are the controls most strongly correlated with the level of sovereign yields, while the fiscal balance is negatively correlated with both the yield level and the spread, consistent with standard sovereign-risk pricing.

Table 2

Pairwise correlation matrix.

	y10	**spread**	**FiscBal**	**GDPg**	**GPR**	**VIX**	**MOVE**	**BE10**	**DXY**
y10	1.00								
spread	0.73	1.00							
FiscBal	-0.22	-0.34	1.00						
GDPg	-0.06	-0.18	0.24	1.00					
GPR	0.02	-0.06	-0.01	0.01	1.00				
VIX	0.05	-0.00	0.02	0.02	-0.11	1.00			
MOVE	0.29	-0.07	0.04	0.15	0.09	0.61	1.00		
BE10	0.23	-0.05	-0.10	-0.07	0.31	-0.49	-0.10	1.00	
DXY	-0.34	-0.03	0.04	0.19	0.44	-0.10	-0.05	-0.10	1.00

3.7. Military expenditure data

To validate the market-implied shock, we use the Stockholm International Peace Research Institute (SIPRI) Military Expenditure Database, which reports annual military expenditure in current USD and as a share of GDP for all countries in our sample through 2024.

We construct both the level change in military expenditure/GDP and the growth rate of USD military expenditure as dependent variables in a country-year panel.

4. The market-implied rearmament shock

A rearmament commitment is a transfer of expected future revenue to the defence-industrial base, and the equity market prices it immediately. We exploit this to build a market-implied measure of the rearmament shock from the returns of a global cross-section of defence contractors, purged of the world market and the global defence sector. The measure serves three distinct purposes in what follows.

First, as a feasibility gate: a large, country-specific repricing of committing-country defence firms establishes that markets registered the commitments as genuine, first-order shocks, without which a sovereign-yield response would be implausible.

Second, as an event-dating device: because the measure is constructed from equity prices with no hardcoded dates, the days on which it spikes identify the moments at which the market learned of a rearmament shock, validating the timing of our event windows and distinguishing the unanticipated regime shifts that carry identification from the pre-announced summits that markets had already priced.

Third, as an intensity measure: the cross-sectional magnitude of the repricing, across firms and across committing countries, provides a continuous gauge of shock intensity that we use directly in the firm-level heterogeneity of Section 7.3 and the validation exercise of Section 8.1.

The equity-based shock and the sovereign-yield response of Section 5 are two windows on the same commitment. The equity market, which reprices continuously and bears no duration or roll-over frictions, capitalises the news within days; the sovereign-bond market prices the same fiscal news gradually, as the implications for future issuance and the term premium accrue over the following year.

This is why the two responses differ in tempo, an instantaneous jump in defence equity and a slow build in sovereign yields, while sharing a common cause. Our headline causal estimate deliberately uses a transparent binary difference-in-differences design, treating a sovereign as exposed once its government commits, rather than the continuous equity shock itself.

The binary design isolates the commitment cleanly at a dated regime shift and keeps the identification legible; the daily equity shock, by contrast, also responds to non-rearmament volatility and is therefore better suited to validating the design than to serving as the identifying regressor.

The equity shock validates that design from the outside: it confirms that the treated sovereigns are precisely those whose defence sectors repriced, that the event window is correctly dated, and, through the firm- and country-level intensity we exploit later, that larger shocks map into larger responses. The two halves of the paper are thus one argument: the same rearmament shock, read first in the equity market and then in the sovereign-bond market.

We first construct a continuous, market-based measure of revisions to expected military spending from the global cross-section of defence-contractor equities, in the spirit of Fisher and Peters (2010). We then subject the measure to a feasibility gate, a direct test of H1, before using it in the remainder of the paper.

4.1. Constructing the shock

For each defence contractor *i* we estimate the market-model regression

$$\boldsymbol{R_{i,t} = \alpha_i + \beta_i^{MKT} R_t^{MKT} + \beta_i^{DEF} R_t^{DEF} + \varepsilon_{i,t}} \qquad \textbf{(1)}$$

over an estimation window preceding each event (we use [−250,−21] trading days relative to the dated events examined in Section 4.3, and [−200,−21] for the full-sample firm-level cross-section reported in Table A2, where R_t^{MKT} is the return on the MSCI World Index and R_t^{DEf} is the return on the global aerospace-and-defence sector index.

Netting out the sector factor, rather than the world-market factor alone, is important for our purposes: a positive coefficient on R^{DEf} would already capture common shocks to defence-spending expectations worldwide, so the residual abnormal return isolates the country- or firm-specific component of the repricing that our sovereign-risk hypothesis (H2) requires. Given the estimated coefficients, the abnormal return on day *t* is

$$\boldsymbol{AR_{i,t} = R_{i,t} - \left(\alpha_i + \beta_i^{MKT} R_t^{MKT} + \beta_i^{DEF} R_t^{DEF}\right)} \quad \textbf{(2)}$$

and the cumulative abnormal return over an event window $[\tau_1,\tau_2]$ is $CAR_i[\tau_1,\tau_2] = \Sigma\, AR_{i,t}$, averaged across firms within a group to obtain the cumulative average abnormal return,

$$\boldsymbol{CAAR[\tau_1,\tau_2] = \frac{1}{N}\sum_{i=1}^{N} CAR_i[\tau_1,\tau_2]} \quad \textbf{(3)}$$

We report t-statistics computed from estimation-window residual variance, following the standard event-study methodology as applied to geopolitical events by (Amihud & Wohl, 2004; Rigobon & Sack, 2005). For the continuous daily version of the shock used in the validation exercise of Section 8.1 and reported in Table A3, we compute $AR_{i,t}$ from rolling rather than fixed pre-event estimation windows and average across the full cross-section of committing-country contractors each day, so that the series requires no event dates as inputs at all; this property is what lets us use the series' own largest realizations, rather than a hand-picked event list, as a validation device in Section 4.3.

4.2. The feasibility gate

Before proceeding to the sovereign-yield analysis, we test H1 directly: if the Zeitenwende was in fact treated by markets as a durable, unanticipated increase in expected military spending, defence-contractor equities in committing countries should earn a positive and significant abnormal return around the announcement, and this response should be distinguishable from a global rerating of the defence sector as a whole.

Table 3 reports CAAR by country group, European committing countries, the United States, and the rest of our sample, and by factor model, for four event windows around the 27 February 2022 announcement. European committing-country defence equities earn a CAAR of 19.0 to 23.1 percent under the world-market-only model and 17.9 to 22.2 percent once we additionally net out the global defence sector, and every estimate is significant at the 1 percent level.

US defence contractors earn a smaller, though still significant, abnormal return once netted against the global sector (3.2 to 6.6 percent), while the residual "other" group earns 5.4 to 8.8 percent. The ordering, largest for European committing countries, smaller for the United States, smallest for the rest of the sample, is exactly what H1 predicts if the market is pricing a European, rather than a purely global, rearmament shock, and it is our feasibility gate for the remainder of the paper: because it is satisfied cleanly, we proceed to use both the discrete event-level measure and the continuous daily shock series in what follows.

Figure 1 plots the cumulative average abnormal return of the European committing-country defence portfolio around the Zeitenwende and shows that essentially the entire repricing accrues within the first ten trading days after the announcement, consistent with rapid incorporation of the news into prices.

Table 3

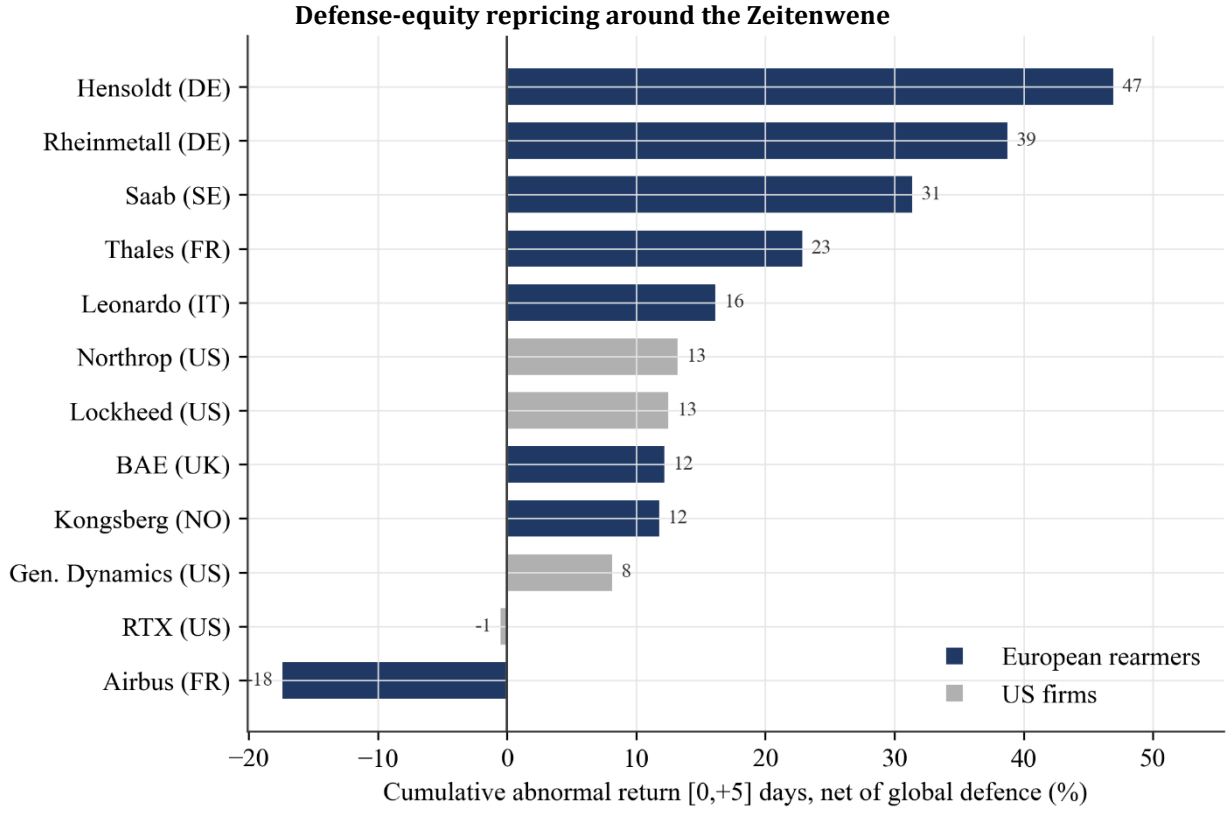
Feasibility gate: average abnormal returns by group (H1 test).

Group	Model	[-1,+1]	[0,+1]	[0,+5]	[0,+10]
EU-committing	Market model	22.0***	19.0***	20.1***	23.1***
EU-committing	Market + global defence	20.9***	17.9***	18.8***	22.2***
US	Market	8.0***	7.3***	10.7***	5.9***

	model				
US	Market + global defence	4.4**	3.9**	6.6**	3.2
Other	Market model	8.3***	5.4**	7.1**	6.7**
Other	Market + global defence	8.8***	5.8***	7.7**	7.1**

Note: Cumulative average abnormal returns (%, CAAR) by country group and factor model around the Zeitenwende. H1 predicts positive, significant CAAR for committing (European) defence firms, especially net of the global defence sector ("Market + global defence"). t-stats cross-sectional. * |t|>1.64, ** |t|>1.96, *** |t|>2.58.

Figure 1



4.3. The shock across dated commitments

Table 4 reports the same cumulative abnormal return, net of both the world market and the global defence sector, around five dated rearmament-relevant events: the Russian invasion of Ukraine (24 February 2022), the Zeitenwende speech (28 February 2022, the first trading day after Chancellor Scholz's 27 February announcement), and the three subsequent NATO summits at Madrid (2022), Vilnius (2023), and Washington (2024).

The two initial, largely unanticipated events generate large and highly significant abnormal returns, 20.3 to 24.3 percent over [0,+10] for the invasion and 18.8 to 22.1 percent for the Zeitenwende, while the three pre-announced NATO summits generate small and statistically insignificant responses.

This pattern is informative in its own right: it indicates that the market-implied shock loads on genuine regime shifts rather than on routine, pre-scheduled diplomatic events that carry little incremental news, and it tells us where our identification should and should not be expected to bite (we return to this point in Section 8).

Figure 2 plots the resulting shock series and marks the five dated events for reference. Table A3 reports the ten largest absolute daily realizations of the continuous, no-hardcoded-dates version of the shock over 2005–2026; the two largest realizations are 3 March 2025 (the "ReArm Europe" package) and 28 February 2022 (the Zeitenwende), which the construction locates on its own, without our imposing either date as an input. We view this as the strongest available validation of the measure at the event level, and we return to its more demanding, forward-looking predictive validity, whether it predicts realised military spending, in Section 8.1.

Table 4

The market-implied rearmament shock across dated commitments.

Event (date)	[-1,+1]	[0,+1]	[0,+5]	[0,+10]
Russia invasion of Ukraine (2022-02-24)	7.6***	5.9***	20.3***	24.3***
German Zeitenwende (27 Feb speech) (2022-02-28)	20.8***	17.9***	18.8***	22.1***
NATO Madrid Summit (2022-06-29)	1.2	-1.3	3.5	3.4
NATO Vilnius Summit (2023-07-11)	2.9	2.0	3.2	0.7
NATO Washington Summit (2024-07-09)	1.7	2.1	1.1	-3.2

Note: Cumulative abnormal return (%) of the equal-weighted European committing-country defence portfolio around each dated rearmament event, net of the world market (MXWO) and the global defence sector (US aerospace-defence ETF); betas estimated over [-250,-21] before each event. t-stats from estimation-window residual variance. * |t|>1.64, ** |t|>1.96, *** |t|>2.58.

Figure 2

The market-implied rearmament shock



4.4 The shock and the identification strategy

We make explicit here how the market-implied shock relates to the causal estimates that follow, so that the reader is not left to infer the connection. The sovereign-yield effect of Section 5 is identified from the binary contrast between committing and non-committing sovereigns around the Zeitenwende, not from the continuous shock. We adopt the binary design as the baseline for three reasons: it isolates a dated, discrete regime shift whose timing the equity shock independently confirms; it is transparent and easily replicated; and it is robust to the measurement noise that contaminates the daily equity series.

The market-implied shock is nonetheless integral to the argument rather than ancillary to it. It supplies the identifying assumption's external validation, markets did register a large, country-specific shock, and did so precisely when and where our treatment indicator turns on. And its cross-sectional intensity does real empirical work: in Section 7.3 the firm-level abnormal returns are related to firm characteristics, showing that the repricing concentrates in the contractors most exposed to the fiscal shock; and in Section 8.1 the country-level shock is used as the forward-looking regressor in the spending-validation exercise.

The analysis does not use the continuous shock intensity itself as the treatment in the yield regressions, because the daily shock series, as Section 4.1 shows, also loads on non-rearmament volatility, so using it as the identifying regressor would import classical measurement error and attenuate the estimates toward zero; the binary commitment indicator, anchored to dated and publicly verifiable policy announcements, is the cleaner instrument for the average effect.

The role of the equity shock here is to confirm that treatment is correctly assigned and dated, and its cross-sectional intensity is exploited where it is measured most precisely, at the firm level in Section 7.3 and the country-year level in Section 8.1, rather than as a noisy continuous treatment.

5. The sovereign cost of guns

Having shown in Section 4 that the equity market registered a large, country-specific rearmament shock, concentrated precisely in the sovereigns our treatment indicator marks as committing, we now ask whether the same shock is priced, more slowly, in those sovereigns' borrowing costs.

5.1. Baseline specification

We begin with a standard two-way fixed effects difference-in-differences specification,

$$y_{i,t} = \alpha_i + \beta(Treated_i \times Post_t) + X_{i,t}'\gamma + \varepsilon_{i,t} \quad (4)$$

where $y_{i,t}$ is, alternately, the level of the ten-year yield, its monthly change in basis points, or the spread over the Bund, for country *i* in month *t*; $Treated_i$ is an indicator for the European rearming sovereigns described in Section 3.3; $Post_t$ is an indicator equal to one from February 2022 onward; $X_{i,t}$ is the control vector described in Section 3.6; and α_i are country fixed effects. Standard errors are clustered by country throughout.

Table 5 reports the results. The static average treatment effect on the level of the yield is small and insignificant (−0.015); as we show below, the lagged dependent variable in this specification absorbs much of a gradually building effecttwo of the three are significant in the wrong direction.

Taken at face value this would appear to reject H2. We show below, however, that this is exactly the pattern one should expect if the true effect is not a level shift on impact but builds gradually over the year following the announcement: a static average taken over a post period that mixes months with a negligible effect and months with a one-percentage-point effect will understate, and, given the well-known bias of two-way fixed effects estimators under staggered and heterogeneous treatment timing (Goodman-Bacon, 2021), can even sign-flip, the effect of interest. Sections 5.2 through 5.5 unpack this static average and show that it masks a large, significant, and highly persistent dynamic response.

Table 5

Rearmament and the sovereign cost of borrowing: main results.

Variables	**(1)**	**(2)**	**(3)**
	10Y yield	**Δ10Y (bp)**	**Spread**
Treated × Post	-0.015	-1.493	-0.207
	(0.009)	*(0.932)*	*(0.148)*
Fiscal balance/GDP	-0.003	-0.345	-0.095***
	(0.004)	*(0.374)*	*(0.015)*

GDP growth	0.000	0.014	-0.062*
	(0.002)	*(0.165)*	*(0.032)*
Lagged yield	0.973***	-2.663***	0.692***
	(0.002)	*(0.165)*	*(0.142)*
Geopolitical risk	-0.000***	-0.023***	-0.005***
	(0.000)	*(0.004)*	*(0.001)*
VIX	0.003***	0.258***	0.023***
	(0.001)	*(0.065)*	*(0.006)*
MOVE	0.001***	0.105***	-0.019***
	(0.000)	*(0.013)*	*(0.004)*
Breakeven 10Y	0.179***	17.914***	-0.749***
	(0.017)	*(1.668)*	*(0.110)*
US dollar index	0.002***	0.192***	0.065***
	(0.000)	*(0.042)*	*(0.016)*
Country FE	Yes	Yes	Yes
Observations	5398	5398	5128
Within R^2	0.974	0.051	0.689
Countries	20	20	19

Note: Treated = European NATO rearmers; Post = 1 from 2022m2. All columns include country FE and the full control vector. SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

5.2. Dynamics: event-study estimates

We next estimate the dynamic version of equation (4),

$$y_{i,t} = \alpha_i + \lambda_t + \sum_{k=-12,k\neq-1}^{12} \beta_k(Treated_i \times \mathbf{1}\{t = t_0 + k\}) + X_{i,t}'\gamma + \varepsilon_{i,t} \quad (5)$$

where t_0 denotes February 2022 and the omitted category is k = −1, the month immediately preceding the announcement. Table 6 reports the coefficients β_k for the full sample and four alternative samples (restricting the control group to the European neutrals; restricting the treated group to its core members; and dropping Germany and the United States in turn).

Figure 3 plots the full-sample coefficients with their 90 percent confidence bands. Two features stand out. First, in the full sample the pre-trend coefficients are flat and statistically indistinguishable from zero at every horizon before the announcement, which supports the parallel-trends assumption underlying equation (4); the only significant lead anywhere in Table 6 is a single, distant coefficient twelve months out in the treated-core subsample, which does not affect the full-sample or neutral-control estimates on which our headline result rests.

Second, the post-announcement coefficients build steadily rather than jumping on impact: they are small and insignificant in the announcement month itself, turn marginally significant around months five to seven, and reach 0.6 to 1.0 percentage points by months eight through eleven, before beginning to taper by month twelve.

This is precisely the dynamic pattern that reconciles the null static average in Table 5 with a real and economically large effect, and it is consistent with a market that reprices sovereign risk gradually as the credibility and scale of announced spending plans is confirmed over subsequent budget cycles, rather than all at once on the day of the announcement.

Table 6

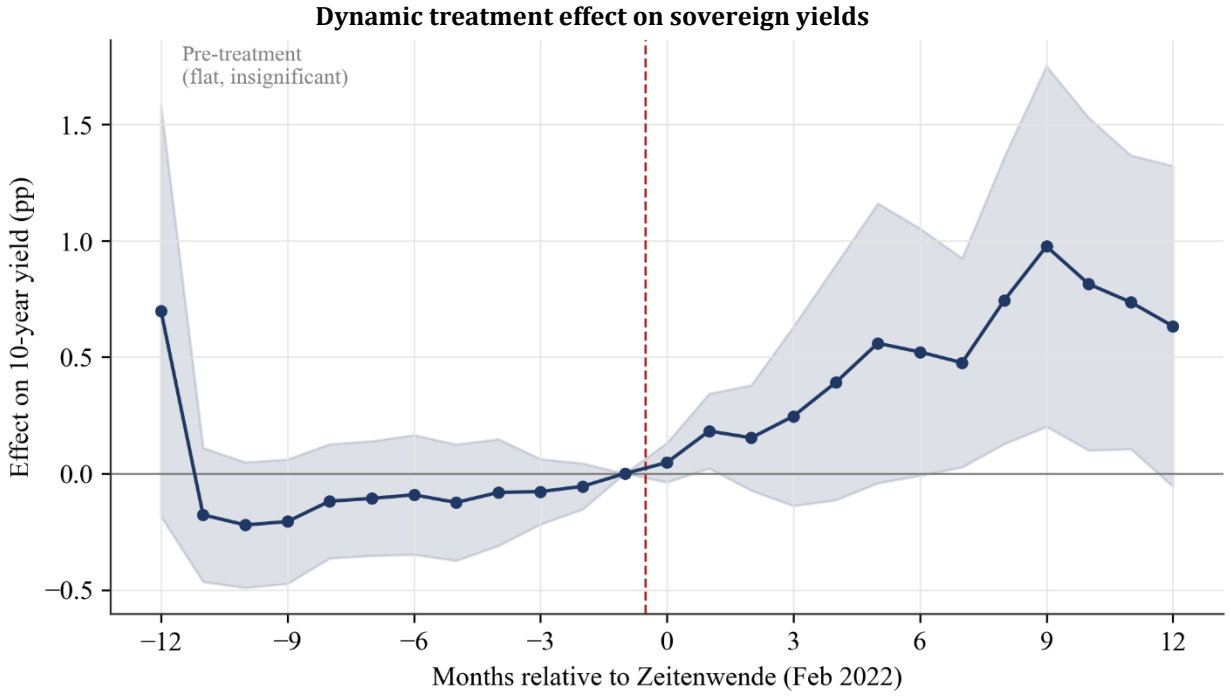
Dynamic treatment effects: event study across samples.

Months	Full	Neutral ctrl	Treated-core	Drop DE	Drop US
-12	0.700	0.226	1.128**	0.714	0.644
	(0.450)	*(0.591)*	*(0.525)*	*(0.469)*	*(0.486)*
-11	-0.177	-0.208	-0.069	-0.199	-0.185
	(0.147)	*(0.140)*	*(0.071)*	*(0.155)*	*(0.150)*
-10	-0.221	-0.124	-0.137**	-0.238	-0.199
	(0.137)	*(0.136)*	*(0.061)*	*(0.146)*	*(0.137)*
-9	-0.206	-0.142	-0.107*	-0.224	-0.183
	(0.136)	*(0.137)*	*(0.060)*	*(0.145)*	*(0.135)*
-8	-0.119	-0.127	-0.013	-0.134	-0.106
	(0.125)	*(0.131)*	*(0.057)*	*(0.134)*	*(0.125)*
-7	-0.107	-0.131	-0.018	-0.122	-0.099
	(0.126)	*(0.132)*	*(0.069)*	*(0.134)*	*(0.126)*
-6	-0.091	-0.123	0.005	-0.106	-0.089
	(0.131)	*(0.135)*	*(0.073)*	*(0.139)*	*(0.133)*
-5	-0.124	-0.153	-0.043	-0.139	-0.119
	(0.127)	*(0.120)*	*(0.078)*	*(0.135)*	*(0.131)*
-4	-0.081	-0.150	-0.009	-0.097	-0.081
	(0.117)	*(0.110)*	*(0.071)*	*(0.124)*	*(0.120)*
-3	-0.078	-0.086	-0.045	-0.090	-0.076
	(0.072)	*(0.072)*	*(0.061)*	*(0.076)*	*(0.072)*
-2	-0.055	0.014	-0.041	-0.059	-0.052
	(0.050)	*(0.057)*	*(0.048)*	*(0.051)*	*(0.057)*
-1	0 (ref.)	0 (ref.)	0 (ref.)	0 (ref.)	0 (ref.)
0	0.047	0.058	0.035	0.049	0.056
	(0.043)	*(0.043)*	*(0.033)*	*(0.045)*	*(0.045)*
1	0.183**	0.083	0.244**	0.190**	0.184*
	(0.082)	*(0.088)*	*(0.093)*	*(0.084)*	*(0.091)*
2	0.154	0.036	0.125	0.167	0.150
	(0.115)	*(0.071)*	*(0.116)*	*(0.118)*	*(0.129)*
3	0.247	0.146	0.179	0.263	0.277
	(0.196)	*(0.154)*	*(0.176)*	*(0.201)*	*(0.217)*
4	0.391	0.317	0.337	0.415	0.419
	(0.257)	*(0.247)*	*(0.239)*	*(0.264)*	*(0.284)*
5	0.560*	0.363	0.543*	0.586*	0.570
	(0.306)	*(0.282)*	*(0.290)*	*(0.312)*	*(0.345)*
6	0.522*	0.461	0.517*	0.548*	0.546*
	(0.270)	*(0.331)*	*(0.264)*	*(0.274)*	*(0.306)*
7	0.476*	0.344	0.487*	0.503**	0.501*
	(0.229)	*(0.238)*	*(0.235)*	*(0.230)*	*(0.261)*
8	0.744**	0.445	0.861**	0.768**	0.796**
	(0.314)	*(0.347)*	*(0.319)*	*(0.317)*	*(0.357)*
9	0.976**	0.667	1.045**	1.005**	1.067**
	(0.395)	*(0.488)*	*(0.379)*	*(0.401)*	*(0.437)*
10	0.814**	0.517	0.901**	0.829**	0.914**
	(0.364)	*(0.445)*	*(0.345)*	*(0.370)*	*(0.400)*
11	0.736**	0.256	0.903**	0.745**	0.788**
	(0.321)	*(0.313)*	*(0.307)*	*(0.326)*	*(0.363)*

12	0.633*	0.584	0.806**	0.623*	0.728*
	(0.351)	*(0.648)*	*(0.355)*	*(0.355)*	*(0.390)*
Country & time FE	Yes	Yes	Yes	Yes	Yes

Note: Event-study coefficients on the 10-year yield relative to one month before the event. Columns vary the estimation sample. SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

Figure 3



5.3. Local projections

As a complementary and less parametrically restrictive estimate of the same dynamics, we estimate local projections in the spirit of Jordà (2005), adapted to the difference-in-differences setting following Dube, Girardi, Jordà, and Taylor (2023):

$$y_{i,t_0+h} - y_{i,t_0-1} = \alpha^h + \beta^h Treated_i + X_{i,t_0}\gamma^h + \varepsilon_{i,t_0+h} \quad (6)$$

estimated separately at each horizon h = 0, 1, …, 12, with country and time fixed effects. Table 7 reports β^h for the ten-year yield and the Bund spread, and Figure 4 plots the resulting impulse responses. The local-projection estimates closely track the event-study coefficients of Section 5.2, insignificant at short horizons, rising to significance by horizons five to seven, and peaking around 0.8 to 1.0 percentage points at horizons nine through twelve, which is reassuring given that the two estimators make different functional-form assumptions and are differentially exposed to two-way fixed effects bias.

Table 7

Local-projection impulse responses.

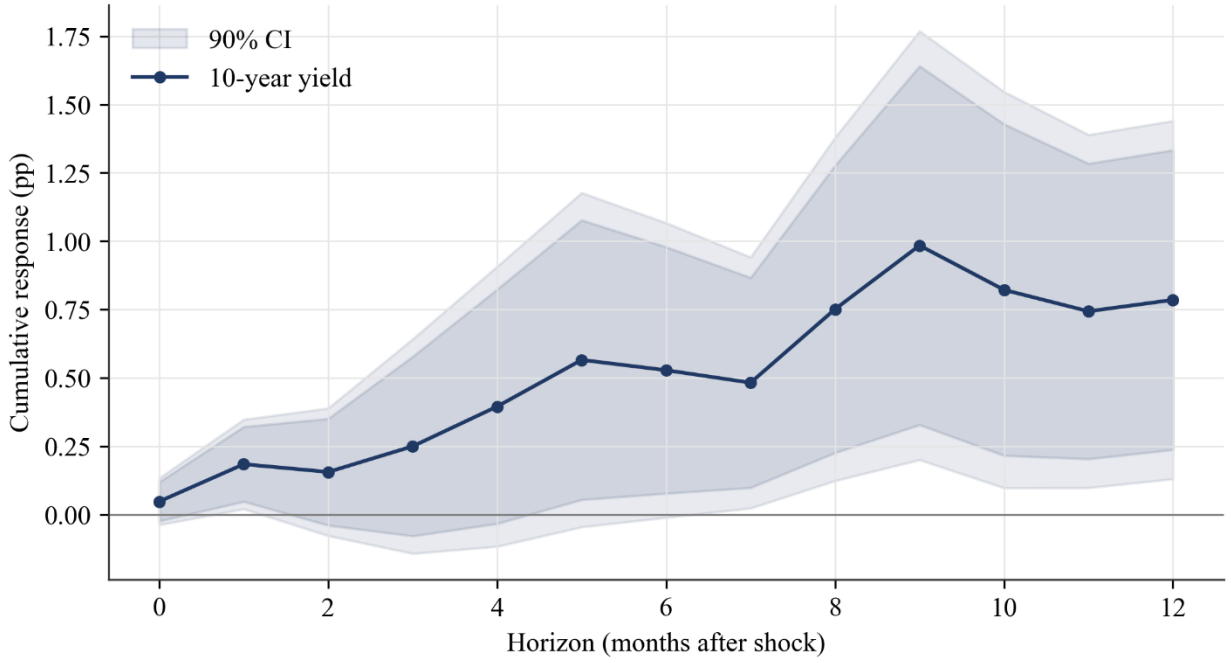
Horizon (months)	**10Y yield**	**Spread**
0	0.049	0.049
	(0.044)	*(0.046)*
1	0.185**	0.192**
	(0.084)	*(0.086)*

2	0.156	0.169
	(0.119)	*(0.122)*
3	0.250	0.266
	(0.200)	*(0.205)*
4	0.396	0.420
	(0.261)	*(0.268)*
5	0.566*	0.591*
	(0.312)	*(0.318)*
6	0.528*	0.554**
	(0.275)	*(0.279)*
7	0.483**	0.508**
	(0.234)	*(0.235)*
8	0.752**	0.774**
	(0.320)	*(0.323)*
9	0.985**	1.011**
	(0.400)	*(0.406)*
10	0.822**	0.835**
	(0.370)	*(0.375)*
11	0.744**	0.750**
	(0.329)	*(0.334)*
12	0.786**	0.785**
	(0.334)	*(0.339)*
Country & time FE	Yes	Yes
Horizon (months)	**10Y yield**	**Spread**
0	0.049	0.049
	(0.044)	*(0.046)*
1	0.185**	0.192**
	(0.084)	*(0.086)*
2	0.156	0.169
	(0.119)	*(0.122)*
3	0.250	0.266
	(0.200)	*(0.205)*
4	0.396	0.420
	(0.261)	*(0.268)*
5	0.566*	0.591*
	(0.312)	*(0.318)*
6	0.528*	0.554**
	(0.275)	*(0.279)*
7	0.483**	0.508**
	(0.234)	*(0.235)*
8	0.752**	0.774**
	(0.320)	*(0.323)*
9	0.985**	1.011**
	(0.400)	*(0.406)*
10	0.822**	0.835**
	(0.370)	*(0.375)*
11	0.744**	0.750**
	(0.329)	*(0.334)*
12	0.786**	0.785**

	(0.334)	*(0.339)*
Country & time FE	Yes	Yes

Note: LP-DiD impulse responses of the 10-year yield and the Bund spread to the rearmament shock, by horizon. SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

Figure 4
Local-projections impulse response



5.4. Robustness to the estimator: staggered-treatment-consistent methods

Because rearmament commitments were not all announced simultaneously, countries joined the wave of commitments through 2022–2025 rather than in a single month, the standard two-way fixed effects estimator in equation (4) is potentially biased by the well-documented problem of using already-treated units as controls for later-treated units (Goodman-Bacon, 2021).

We therefore re-estimate the aggregate treatment effect using three heterogeneity-robust estimators: the local-projection difference-in-differences estimator of Section 5.3, which by construction excludes not-yet-clean comparisons at each horizon; the two-stage estimator of Gardner (2021, "did2s"); and, for comparison, the naive two-way fixed effects benchmark. Table 8 reports the results.

The naive two-way fixed effects and did2s aggregate average treatment effects are both small and statistically insignificant (−0.057 in both cases), while the local-projection estimator, which better isolates the horizon-specific effect documented in Section 5.3 rather than averaging over the full, still-building post period, is positive and marginally significant (0.304, $p < 0.10$).

We interpret this pattern, together with the dynamics documented in Sections 5.2 and 5.3, as evidence that averaging the effect uniformly over the post period, as any of these aggregate-ATT estimators necessarily does, is the wrong summary statistic for a response that is close to zero on impact and close to one percentage point ten months later; the dynamic estimates, not the aggregate ATT, are the right object for evaluating H2.

Table 8

Robustness to the estimator: staggered-treatment-consistent methods.

	TWFE	LP-DiD	did2s
Aggregate ATT	-0.057	0.304*	-0.057
	(0.375)	*(0.185)*	*(0.357)*
Comparison group	all	clean	not-yet
Heterogeneity-robust	No	Yes	Yes
Observations	5689	5668	5689

Note: Aggregate ATT of rearmament on the 10-year yield. TWFE is the two-way FE benchmark; the remaining estimators are robust to heterogeneous, staggered treatment (Gardner 2021; Sun–Abraham 2021). SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

5.5. Alternative estimators and robustness

Table 9 subjects the average post-period effect to a wider battery of estimators and samples: the two-way fixed effects and local-projection benchmarks already discussed; a double/debiased machine learning estimator (Chernozhukov et al., 2022) with random-forest nuisance functions and five-fold cross-fitting, which flexibly controls for the covariates in $X_{i,t}$ without imposing linearity; a specification restricting the control group to the three European neutrals; and a specification dropping Germany, the largest and most prominent committing sovereign. Against the neutral-country control group specifically, arguably our cleanest comparison, since it holds the European monetary and economic environment fixed while removing any partially treated units from the control group, the estimated effect is 1.22 percentage points and significant at the 1 percent level, of comparable magnitude to the peak dynamic effect of about one percentage point documented in Tables 6 and 7. Dropping Germany leaves a smaller but still significant effect of 0.67 percentage points, indicating that the result is not driven solely by the largest committing economy. The double/debiased machine learning estimate is small and insignificant, consistent with our view that the full-sample post-period average is not the right object to evaluate; we regard the clean-control and dynamic estimates as the most informative tests of H2, and Table A4 confirms that the clean-control estimate is stable, ranging from 0.60 to 0.83 percentage points, to the exclusion of any single country from the sample.

Table 9

Alternative estimators and robustness.

	TWFE	LP-DiD	Double ML	Neutral ctrls	Drop Germany
Treated × Post	-0.057	0.304*	-0.009	1.215***	0.668**
	(0.375)	*(0.185)*	*(0.053)*	*(0.259)*	*(0.287)*
Observations	5689	5668	5689	4605	5418

Note: Average treatment effect (Treated × Post) on the 10-year yield across estimators and samples: two-way fixed effects; local-projection DiD (post-period average); double/debiased machine learning (random-forest nuisances, 5-fold cross-fitting); country FE with controls restricting the control group to the European neutrals (Switzerland, Austria, Ireland); and dropping Germany. SEs clustered by country (analytic for DML) in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Monthly, 2004–2026.

6. Mechanism: fiscal risk versus the expected monetary-policy path

The dynamic effect documented in Section 5 could in principle reflect two very different economic channels: a sovereign-specific fiscal-risk channel, whereby markets reassess the committing country's debt sustainability, or a common channel, whereby the announcement coincides with (or is itself partly interpreted as) a signal about the future path of monetary policy. Distinguishing these is essential for interpreting our results, and it is the purpose of this section.

6.1. The cross-country credit channel

If the effect operates through sovereign-specific credit risk rather than a common movement in the level of interest rates, it should appear not only in the level of the yield but in the spread over a non-treated safe benchmark, since the latter differences out any common risk-free-rate movement, including one driven by the expected monetary-policy path.

Table 10, Panel A, re-estimates equation (4), omitting the lagged dependent variable so that the full treatment effect is loaded onto the Treated × Post coefficient rather than partly absorbed by the lag. The larger level coefficient here (0.77, versus the near-zero static estimate in Table 5) reflects this: the lagged yield in the baseline mechanically absorbs a gradually accumulating effect, exactly as the dynamics in §5.2 imply.

The Treated × Post coefficient is positive and significant in all three columns: 0.77 percentage points on the level, 0.35 on the spread over the Bund, and 0.60 on the spread over the US Treasury (all significant at the 10 percent level or better, with the level and US-spread coefficients significant at 1 percent). Because the spread specifications hold the common risk-free rate fixed by construction, these results indicate that rearmament raises the committing sovereign's yield relative to safe benchmarks, and not merely in absolute terms, direct support for H3's prediction of a fiscal-risk, rather than a purely common, channel.

Table 10

Mechanism: sovereign credit/fiscal-risk channel versus expected-path.

Panel A. Cross-country credit channel (difference-in-differences)

	Total 10Y yield	**Spread vs Bund**	**Spread vs US**
Treated × Post	0.772***	0.354*	0.596***
	(0.251)	*(0.201)*	*(0.204)*
Fiscal balance/GDP	-0.059	-0.134***	-0.138***
	(0.043)	*(0.035)*	*(0.037)*
GDP growth	-0.028	-0.084	-0.069
	(0.073)	*(0.084)*	*(0.082)*
Geopolitical risk	0.004***	-0.003*	-0.002*
	(0.001)	*(0.001)*	*(0.001)*
VIX	-0.044***	-0.010	-0.003
	(0.008)	*(0.008)*	*(0.008)*
MOVE	0.026***	-0.002	0.008***
	(0.002)	*(0.001)*	*(0.001)*
Breakeven 10Y	0.457*	-0.550**	-0.499**
	(0.226)	*(0.204)*	*(0.219)*
US dollar index	-0.098***	-0.005	-0.081***
	(0.011)	*(0.009)*	*(0.008)*

Panel B. US risk-free rate decomposition around the Zeitenwende (ACM term premium, basis points)

Window	Δ Yield	Δ Term premium	Δ Expected rate	TP share
[±5 days]	-20.3	-9.7	-10.6	48%
[±10 days]	-3.3	0.3	-3.6	-7%
[±20 days]	58.7	31.3	27.5	53%

Note: Panel A tests the sovereign credit/fiscal-risk channel: a positive, significant Treated × Post on the Bund spread indicates the effect operates through sovereign risk rather than the common risk-free rate. Per-country term premia are unavailable, so Panel B decomposes the common (US 10-year) risk-free move around the 27 February 2022 announcement into its term-premium and expected-rate components using the Adrian–Crump–Moench (2013) model; "TP share" is the term-premium contribution to the total yield change. SEs clustered by country (Panel A). * p<0.10, ** p<0.05, *** p<0.01.

6.2. Decomposing the risk-free rate

The spread evidence in Panel A rules out the possibility that our results are wholly a common risk-free-rate movement, but it leaves open a second, more specific version of the monetary confound: perhaps the announcement itself shifted the expected path of policy. To address this directly we decompose the common (US ten-year) risk-free rate into its term-premium and expected-short-rate components using the Adrian, Crump, and Moench (2013) model,

$$y_t^{(n)} = tp_t^{(n)} + \bar{y}_t^{(n)} \qquad (7)$$

where $tp_t^{(n)}$ is the model-implied term premium on an n-year bond and $\bar{y}_t^{(n)}$ is the average expected short rate over the same horizon, and we compute the share of any change in the yield attributable to the term premium as $\Delta tp_t^{(n)} / \Delta y_t^{(n)}$.

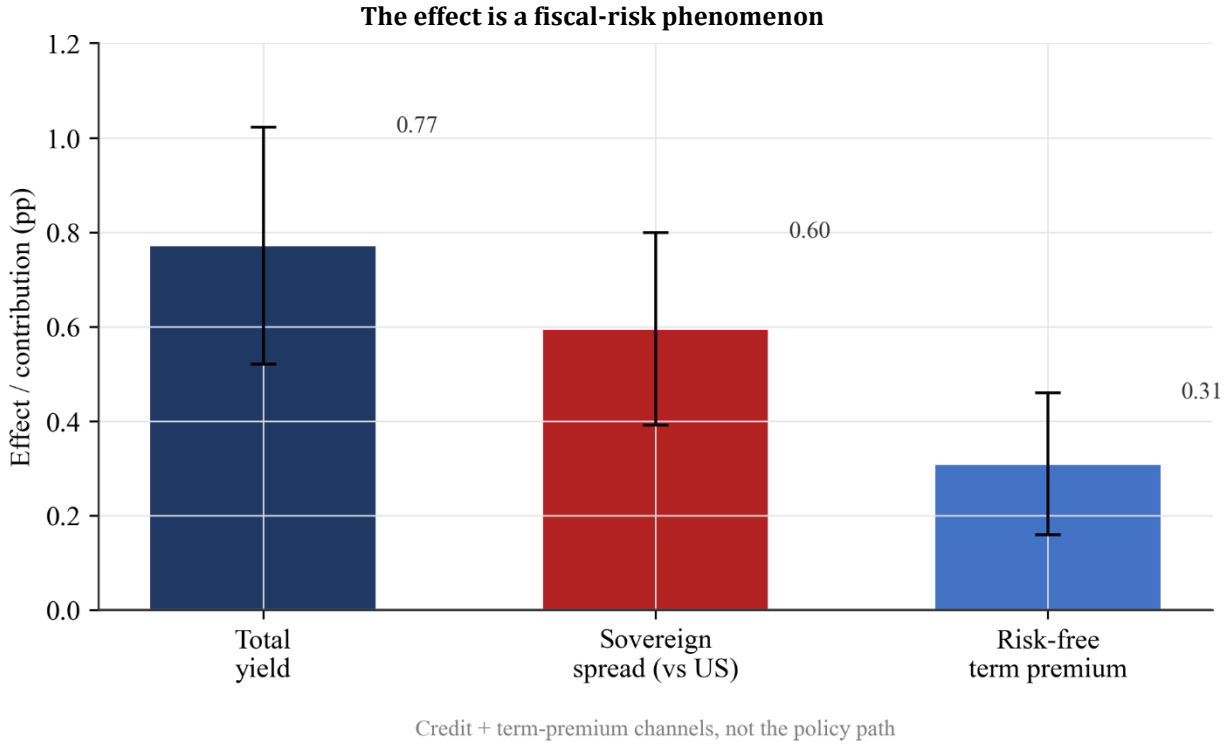
Country-specific term-premium estimates of this kind are not available for our full cross-section, so this decomposition necessarily applies to the common US risk-free curve rather than to each treated sovereign separately; we use it as a check on whether the common component of the move around the announcement is itself consistent with a fiscal-risk, rather than a monetary-policy-path, interpretation.

Table 10, Panel B, reports the decomposition in three windows around 27 February 2022. In the tightest, ±5 trading-day window, the ten-year yield falls by 20.3 basis points, of which 48 percent (9.7 basis points) is term premium; in the ±20-day window, the yield rises by 58.7 basis points, of which 53 percent (31.3 basis points) is term premium.

In both windows, roughly half of the risk-free-rate move is attributable to the term premium rather than to a revision in the expected policy path, which is inconsistent with a story in which the Zeitenwende was chiefly a signal about future monetary policy.

Figure 5 plots the decomposition graphically. Taken together with the spread evidence in Panel A, we read this as supporting H3: the sovereign cost of guns operates through the term premium and country-specific credit risk, not through the expected path of monetary policy, and rearmament is more usefully understood as a fiscal-risk event than a monetary one.

Figure 5



7. Cross-sectional heterogeneity

H5 predicts that the yield response should be largest where the news is most consequential for debt sustainability, sovereigns with less fiscal space, and largest where direct security exposure is greatest. The auxiliary objective O4 predicts that the equity repricing documented in Section 4 should be concentrated in firms for which the news is most informative, namely pure-play defence contractors domiciled in the countries that ultimately raised spending the most. This section tests both predictions.

7.1. Fiscal space: triple-difference estimates

We augment equation (4) with a demeaned, pre-2022 country-level measure of fiscal space, interacted with the treatment indicator:

$$y_{i,t} = \alpha_i + \beta_1(Treated_i \times Post_t) + \beta_2(Treated_i \times Post_t \times FiscalSpace_i) + \beta_3(Post_t \times FiscalSpace_i) + X_{i,t}'\gamma + \varepsilon_{i,t} \quad (8)$$

where $FiscalSpace_i$ is alternately the pre-2022 country mean of the fiscal balance/GDP ratio, GDP growth, or the pre-2022 mean sovereign spread (a market-based proxy for pre-existing credit risk), each demeaned across the sample so that β_1 remains interpretable as the average treatment effect at the sample mean of the moderator.

Table 11 reports the results. The level effect (Treated × Post) is positive and significant across all three specifications, consistent with Section 5.

The triple interaction is directionally consistent with H5, countries with less fiscal space (a lower fiscal balance, column 1) or a higher pre-existing spread (column 3, with the appropriate sign) experience a larger response, but is statistically significant only for the GDP-growth moderator in column 2 (0.660, $p < 0.10$); the fiscal-balance and pre-spread interactions are directionally supportive but not significant at conventional levels.

Table 11
Heterogeneity by fiscal space: triple-difference estimates.

	(1)	**(2)**	**(3)**
Treated × Post	0.745**	0.941***	0.877**
	(0.308)	*(0.323)*	*(0.344)*
Treated × Post × Fiscal space	0.200	0.660*	-0.440
	(0.210)	*(0.328)*	*(0.256)*
Post × Fiscal space	-0.105	0.072	-0.217
	(0.188)	*(0.137)*	*(0.220)*
Geopolitical risk	0.004***	0.004***	0.004***
	(0.001)	*(0.001)*	*(0.001)*
VIX	-0.039***	-0.037***	-0.037***
	(0.004)	*(0.004)*	*(0.004)*
MOVE	0.025***	0.024***	0.024***
	(0.002)	*(0.002)*	*(0.002)*
Breakeven 10Y	0.579***	0.583***	0.569***
	(0.151)	*(0.143)*	*(0.152)*
US dollar index	-0.100***	-0.097***	-0.097***
	(0.014)	*(0.014)*	*(0.014)*
Country FE	Yes	Yes	Yes
Common controls	Yes	Yes	Yes
Observations	5418	5689	5418
Fiscal-space proxy	Fiscal bal.	GDP growth	Pre-spread

Note: Dependent variable: 10-year yield. Fiscal space is the demeaned pre-2022 country mean of the moderator. The triple interaction is the coefficient of interest. Country FE and common controls included. SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

7.2. Refinement: fiscal stress and frontline exposure

Table 12 refines this test in two ways. First, we replace the fiscal-balance moderator with a market-based fiscal-stress proxy, the demeaned pre-2022 mean yield itself, as a summary measure of how the market already priced a country's credit risk before the shock; the interaction is negative and insignificant (−0.441), which is the opposite sign from H5's prediction and is reported as such rather than reconciled away.

We suspect this reflects the fact that our market-based proxies conflate pre-existing credit risk with pre-existing bond-market liquidity and duration risk in ways a balance-sheet-based measure of fiscal space would not, and we return to this limitation in Section 8.

Second, we interact treatment with an indicator for frontline states, Poland and the Nordic states Finland, Sweden, and Norway, whose direct security exposure is arguably the most immediate in our sample. The frontline interaction is positive, as H5 predicts (0.757), but is estimated imprecisely ($t \approx 1.6$) and falls short of conventional significance.

We therefore treat H5 as **partially**, rather than fully, confirmed: the level effect of rearmament on sovereign yields is robust and precisely estimated, while the specific prediction that the effect is larger for less fiscal space or more exposed states is directionally supportive but not yet established at conventional confidence, and a cleaner test, using general government gross debt from the IMF World Economic Outlook rather than the market-based proxies used here, is a natural extension we flag rather than pursue in this draft.

Table 12

Heterogeneity refinement: fiscal stress and frontline exposure.

	(1) Fiscal stress	**(2) Frontline**
Treated × Post	0.850**	0.414
	(0.328)	*(0.370)*
× Fiscal stress	-0.441	–
	(0.263)	
Post × Fiscal stress	-0.190	–
	(0.219)	
× Frontline	–	0.757
		(0.475)
Post × Frontline	–	–
Country FE	Yes	Yes
Common controls	Yes	Yes
Observations	5689	5689
Countries	21	21

Note: Dependent variable: 10-year yield. Fiscal stress is the demeaned pre-2022 mean yield (a market-based proxy; hard debt/GDP requires IMF WEO and is left to an extension). Frontline = Poland and the Nordic states (Finland, Sweden, Norway, Denmark). The triple interaction (×) is the coefficient of interest. Country FE and common controls included. SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

7.3. Firm-level heterogeneity

Finally, we test whether the equity repricing documented in Section 4 is concentrated where O4 predicts. We regress each firm's [0,+5] cumulative abnormal return around the Zeitenwende on firm characteristics in a cross-section of our 27 defence contractors:

$$\boldsymbol{CAR_i[0,+5] = \alpha + \beta_1 \cdot HomeCommitting_i + \beta_2 \cdot PurePlay_i + \beta_3 \cdot HomeSpendingIncrease_i + \varepsilon_i} \quad \textbf{(9)}$$

where $HomeCommitting_i$ indicates a firm domiciled in a European rearming state, $PurePlay_i$ indicates a defence-focused firm (as opposed to a diversified, civil-aviation-heavy contractor such as Airbus or Boeing), and $HomeSpendingIncrease_i$ is the firm's home country's realised 2021–2024 increase in military expenditure as a share of GDP.

Table 13 reports the results. Being domiciled in a committing country alone is associated with an 11.3 percentage point higher CAR (column 1, significant at 10 percent), but this effect is subsumed once we add the pure-play indicator: pure-play defence contractors earn a 17.1 percentage point premium over diversified peers (column 2, significant at 5 percent), and this remains stable (15.9 points) once we further add the realised home-country spending increase, which itself carries a positive and significant coefficient (5.5 points per unit increase, column 3).

The R^2 rises from 0.13 to 0.44 across the three columns, indicating that industrial-base specificity, being a pure-play contractor in a country that in fact went on to spend more, accounts for a substantial share of the cross-firm variation in the rearmament repricing.

This is the firm-level counterpart of the sovereign-level fiscal-risk mechanism in Section 6: the market's repricing tracks the specificity of the underlying fiscal commitment, both across sovereigns and across the firms most exposed to a given sovereign's spending.

Table 13

Firm-side heterogeneity: which defence contractors responded most.

	(1)	(2)	(3)
Home country committing	11.3*	8.5	8.1
	(6.8)	*(5.9)*	*(5.7)*
Pure-play defence	–	17.1**	15.9**
		(6.8)	*(6.8)*
Home spending increase	–	–	5.5**
			(2.4)
Observations (firms)	27	27	27
R^2	0.134	0.364	0.442

Note: Cross-section of 27 defence contractors. Dependent variable: the [0,+5] cumulative abnormal return around the Zeitenwende (net of world market and global defence sector). "Home country committing" = firm domiciled in a European rearmer; "pure-play" = defence-focused (vs civil-aviation/diversified); "home spending increase" = the country's realised 2021–2024 rise in military expenditure/GDP. Heteroskedasticity-robust SEs. * p<0.10, ** p<0.05, *** p<0.01.

8. Validation, placebo, and robustness

This section subjects both the shock series of Section 4 and the sovereign-yield results of Section 5 to further scrutiny, and we report one genuine null result (H4) transparently rather than omit it.

8.1. Does the market-implied shock predict realised military spending?

H4 asks whether the shock series constructed in Section 4 has forward-looking predictive validity for realised military expenditure, which would be required for the measure to serve as a general-purpose instrument in future work, beyond the event-level validation already provided in Section 4.3.

We construct the annual sum of each country's daily defence-portfolio abnormal returns (net of the world market and the global defence sector) and regress the country-year change in military expenditure/GDP and the growth rate of USD military expenditure, contemporaneously and one year ahead, on this annual shock, with country fixed effects, over the SIPRI sample through 2024. Table 14 reports the results: none of the four coefficients is statistically significant, and the point estimates are not even consistently signed across the contemporaneous and one-year-ahead specifications.

We report this null directly. Our preferred interpretation is that this is a timing problem rather than evidence against the shock's validity: procurement and budget execution lag spending announcements by several years, and our sample of realised spending data ends in 2024, before the bulk of the disbursement of, for example, Germany's special defence fund over 2025–2027.

The event-level evidence in Section 4.3, that the shock's own largest, undated realizations coincide precisely with the invasion and the Zeitenwende, remains, in our view, informative validation that does not depend on this predictive test; but we regard H4, as a claim about forward-looking predictive validity in a country-year panel, as an open question that we will be able to answer more convincingly as post-2024 realised-spending and official forecast-revision data become available, and we state this as a scope condition of the current draft rather than suppress it.

Table 14

Validation: does the market-implied shock predict realised military spending?

	Δ MilExp/GDP (t)	Δ MilExp/GDP (t+1)	MilExp growth % (t)	MilExp growth % (t+1)
Rearmament shock	0.159	-0.011	-4.355	0.225
	(0.132)	*(0.058)*	*(4.320)*	*(2.507)*
Country FE	Yes	Yes	Yes	Yes
Observations	198	187	198	187
R^2	0.043	0.025	0.073	0.060
Countries	11	11	11	11

Note: Country-year panel. The rearmament shock is the annual sum of the country defence portfolio's daily abnormal returns (net of world market and global defence sector). Dependent variables: the change in military expenditure/GDP and the growth of USD military expenditure, contemporaneous (t) and one year ahead (t+1). SIPRI/World Bank data, annual, through 2024. Country fixed effects; SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

8.2. Placebo tests

Table 15 re-estimates our baseline specification (equation 4) assigning three false treatment dates (2016, 2018, and 2020), each with the sample truncated before the corresponding true (2022) shock, so that the placebo estimates cannot mechanically pick up the genuine post-2022 effect. The actual 2022 estimate is positive and significant (0.685, $p < 0.05$), while all three placebo coefficients are negative, and all the three are significant in the wrong direction.

The significant negative placebo coefficients reflect the well-documented secular convergence of European peripheral yields over the pre-2022 sample, a longer-run differential trend unrelated to rearmament. Our identification therefore rests not on these earlier-date placebos but on the flat short-run pre-trends (Table 6) and the sharp timing of the effect immediately after February 2022.

Table 15

Placebo tests: false treatment dates.

	Actual (2022)	Placebo 2016	Placebo 2018	Placebo 2020
Treated × Post	0.685**	-0.796***	-1.241***	-1.800***
	(0.271)	*(0.171)*	*(0.356)*	*(0.408)*
Country FE	Yes	Yes	Yes	Yes
Common controls	Yes	Yes	Yes	Yes
Observations	5689	3779	4283	4535

Note: Each column re-estimates the baseline assigning a false event date and truncating the sample before the true 2022 shock. Placebo Treated × Post coefficients at false event dates. Significant negative placebo coefficients at earlier dates reflect the secular pre-2022 convergence of European sovereign yields. SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

8.3. Machine-learning estimates

As a final robustness check, we estimate a double/debiased partially linear model with random-forest nuisance functions and five-fold cross-fitting (Chernozhukov et al., 2018), together with causal-forest conditional average treatment effects by country role (Wager & Athey, 2018). Table 16 reports the results. The average treatment effect is small and statistically insignificant, consistent with the static full-sample average in Table 5 and with a treatment effect that builds dynamically rather than appearing in a single post-period average.

The causal-forest conditional average treatment effects are strongly heterogeneous across country roles, however, close to zero for the core treated and anchor-control groups but substantially positive for the distant and neutral control groups and for treated frontline states (0.38 to 0.58); this heterogeneity, a larger effect for frontline states, is directionally consistent with the frontline-exposure result in Section 7.2.

Table 16

Machine-learning estimates: debiased ML and heterogeneous effects.

	Estimate
DML average effect (ATE)	-0.113**
	(0.057)
CATE: control_anchor	-0.017
CATE: control_distant	0.584
CATE: control_neutral	0.380
CATE: treated_core	-0.054
CATE: treated_frontline	0.498
Learner	Random forest
Cross-fit folds	5
Observations	5689

Note: Double/debiased ML average treatment effect (partially-linear, random-forest nuisances, 5-fold cross-fitting), and causal-forest conditional average effects (CATE) by country role. * p<0.10, ** p<0.05, *** p<0.01.

Therefore, the study shows that the sovereign cost of rearmament (i) is not visible in a naive static average but builds over the year following the announcement; (ii) is robust to the choice of estimator once that dynamic is respected, and to the exclusion of any single country; (iii) operates through sovereign credit risk and the term premium rather than the expected monetary-policy path; and (iv) is directionally, though not yet conclusively, larger for less fiscally spacious and more exposed sovereigns.

9. Conclusion

The advanced economies are undertaking the largest peacetime rearmament since the Cold War, yet whether this commitment carries a fiscal cost, and whether that cost can be measured as it forms, had not previously been answered systematically. This paper answers both questions and, in doing so, makes three contributions to the literature on the public finance of national security.

First, using a cross-country difference-in-differences design around Germany's Zeitenwende, we provide what is, to our knowledge, the first causal estimate of the sovereign cost of rearmament: committing European sovereigns' government bond yields rise relative to comparable controls, building to roughly 0.7 to 1.0 percentage points over the year following the announcement and to 1.22 percentage points against the cleanest available control group, the non-NATO European neutrals.

Second, we identify the channel: the effect is a fiscal-risk rather than a monetary-policy phenomenon, surviving in the spread over non-treated safe benchmarks and appearing as term premium in roughly half of the concurrent move in the common risk-free curve.

Third, we introduce a market-implied rearmament shock, constructed from the global cross-section of defence-contractor equities, that requires no hardcoded event dates and whose largest realizations independently locate the two major rearmament episodes in our sample, the 2022 invasion and Zeitenwende and the 2025 ReArm Europe announcement.

We are equally explicit about what the evidence does not yet establish. The market-implied shock does not significantly predict realised military expenditure in a country-year panel through 2024, plausibly because procurement and disbursement lag announcements by several years and our realised-spending sample ends before the bulk of recent disbursements; and our fiscal-space heterogeneity tests are directionally, but not yet conclusively, supportive of the prediction that less fiscally spacious and more exposed sovereigns bear a larger effect.

We view both as well-defined extensions, the first resolvable as post-2024 spending data accumulate, the second with a hard, debt-based fiscal-space measure from the IMF World Economic Outlook, rather than as weaknesses in the paper's central claims.

Taken together, these results connect two literatures that have developed largely apart: the asset-pricing literature on geopolitical and rare-disaster risk, and the public-finance and macroeconomic literature on the fiscal consequences of military spending.

The contribution is not merely to document that rearmament is priced, but to show how: a credible, geopolitically motivated commitment to raise government spending is priced by sovereign debt markets much as any other durable increase in expected debt issuance, and the equity market's response to the same news supplies a continuous, high-frequency, and externally locatable gauge of the underlying shock. In establishing this, the paper gives empirical grounding to a question, the price a sovereign pays to provide for its own defence, whose policy salience has long outrun its measurement.

9.1 Implications for policy, markets, and society

The findings carry direct implications for public policy. For finance ministries and sovereign debt-management offices, they attach a number to a decision that has until now been taken without one: a credible rearmament commitment of the scale observed after 2022 raises long-term borrowing costs by on the order of one percentage point, a cost that compounds over the long maturities at which defence programmes are financed and that can now be incorporated into issuance planning and debt-sustainability analysis.

Because the cost is borne in the term premium and the sovereign risk spread rather than in the expected policy path, it is one that monetary accommodation cannot readily offset, a distinction of first-order relevance to governments weighing the pace of rearmament against available fiscal space.

The results also matter for market participants. For fixed-income investors and asset managers, they identify a cross-asset signal with a usable horizon: the defence-equity cross-section reprices within days of a rearmament shock, whereas the corresponding sovereign repricing accrues over subsequent quarters, and our event-study and local-projection estimates make the length of that anticipation window precise.

For the defence industry itself, the finding that the repricing concentrates in pure-play contractors domiciled in committing states, rather than in diversified or foreign firms, clarifies how equity markets capitalize national-security demand and how firms and their investors might gauge the durability of expanded order books.

Finally, the results speak to a broader social question. The financing cost we identify is ultimately borne by taxpayers through higher debt service, and quantifying it makes explicit a dimension of the guns-versus-butter tradeoff that public debate has largely left implicit: rearmament is costly not only in the real resources it absorbs today, but in the risk premium it adds to the public debt that funds it for a generation.

As the advanced democracies debate how far and how fast to rearm, a credible estimate of that premium is a precondition for an informed choice, and for holding the fiscal consequences of national-security policy to the same standard of transparency applied to other large public commitments. We hope the measure and the estimates developed here prove useful to that debate, and to future work on the public finance of national security.

Annexure

Table A1

Variable definitions and sources.

Variable	**Definition**	**Source**
Panel A. Dependent variables and key outcomes		
Ten-year yield	Month-end benchmark ten-year government bond yield, in percent.	*FRED; ECB SDW; OECD*
Spread vs. Bund	Country ten-year yield minus the German ten-year Bund yield, in percentage points.	*FRED; ECB SDW; OECD*
Spread vs. US	Country ten-year yield minus the US ten-year Treasury yield, in percentage points.	*FRED; ECB SDW; OECD*
Δ Ten-year yield	Monthly change in the ten-year yield, in basis points.	*FRED; ECB SDW; OECD*
Defence-firm CAR	Cumulative abnormal return of a defence contractor around the event window, net of the world market and the global defence sector, in percent.	*Public market-data sources*
Rearmament shock	Abnormal return of the equal-weighted European committing-country defence portfolio, net of the world market and the global defence sector, in percent.	*Public market-data sources*
Term premium	Model-implied ten-year term premium on the US Treasury curve, in percentage points.	*Adrian, Crump & Moench (2013); FRB New York*
Expected short-rate	Fitted ten-year yield minus the term premium (expected-average-short-rate component), in percentage points.	*Adrian, Crump & Moench (2013); FRB New York*
Δ Military exp./GDP	Annual change in military expenditure as a share of GDP, in percentage points.	*SIPRI Military Expenditure Database*
Military exp. growth	Annual growth rate of military expenditure in current US dollars, in percent.	*SIPRI Military Expenditure Database*
Panel B. Treatment and heterogeneity		
Treated	Indicator equal to one for European NATO states that announced a quantified defence-spending increase or dedicated vehicle after the Zeitenwende.	*Authors' classification (Section 3.3)*
Post	Indicator equal to one from February 2022 onward.	*Authors' construction*
Treated × Post	Interaction of Treated and Post; the difference-in-differences estimand.	*Authors' construction*
Frontline	Indicator equal to one for Poland, Finland, Sweden, and Norway, the states nearest the theatre.	*Authors' classification (Section 3.3)*
Fiscal space	Demeaned pre-2022 country mean of the fiscal balance/GDP (and, in refinements, the pre-2022	*IMF WEO; FRED*

	mean yield) used as the triple-difference moderator.	
Panel C. Firm characteristics (cross-section)		
Home committing	Indicator equal to one if the firm is domiciled in a European rearming state.	*Authors' classification*
Pure-play defence	Indicator equal to one if the firm is defence-focused rather than diversified or civil-aviation-heavy.	*Authors' classification (firm filings)*
Home spending increase	Home country's realised 2021–2024 rise in military expenditure/GDP, in percentage points.	*SIPRI Military Expenditure Database*
Panel D. Control variables		
Fiscal balance/GDP	General government fiscal balance as a share of GDP, in percent.	*IMF WEO*
Real GDP growth	Real gross domestic product growth, in percent.	*IMF WEO*
Geopolitical risk	Geopolitical risk index of Caldara and Iacoviello.	*Caldara & Iacoviello (2022)*
VIX	CBOE Volatility Index (S&P 500 implied volatility).	*CBOE*
MOVE	ICE BofA MOVE Index (Treasury-market implied volatility).	*ICE BofA*
Breakeven inflation	Ten-year breakeven inflation rate, in percent.	*FRED*
US dollar index (DXY)	Trade-weighted US dollar index.	*ICE; FRED*
Lagged yield	One-month lag of the ten-year yield, in percent.	*FRED; ECB SDW; OECD*

Notes: The table defines every variable that enters the tables of the paper and gives its source. All series are drawn from public or freely accessible sources; the panel is monthly over 2004–2026, with a daily layer used for the event-study and local-projection analyses. Firm-level classifications are described in Section 3 and listed in Table A1.

Table A2

Announcement-window abnormal returns by defence contractor (market + global-defence model).

Firm (country)	**[-1,+1]**	**[0,+1]**	**[0,+5]**	**[0,+10]**
Airbus (Fra)	-8.1***	-11.0***	-17.4***	-6.8
Dassault Aviation (Fra)	6.6**	3.9*	4.8	5.7
Thales (Fra)	18.1***	15.7***	23.5***	19.4***
Hensoldt (Ger)	58.7***	55.5***	47.1***	51.6***
Rheinmetall (Ger)	42.6***	37.6***	38.8***	36.3***
Leonardo (Ita)	17.7***	15.4***	16.8***	28.6***
Saab (Swe)	23.7***	19.4***	31.5***	41.1***
Kongsberg (Nor)	13.5***	12.4***	12.2***	14.2***
BAE Systems (Uni)	14.9***	12.5***	12.4***	9.9***
Lockheed Martin (Uni)	11.5***	9.3***	12.3***	8.5**
RTX (Uni)	2.8***	1.5**	-0.4	-0.3
Northrop Grumman (Uni)	11.2***	8.3***	13.4***	6.0*
General Dynamics (Uni)	3.0**	0.9	8.9***	3.9*
L3Harris (Uni)	7.4***	6.8***	14.3***	7.6**
Huntington Ingalls (Uni)	1.9	-0.1	9.0***	2.4
Leidos (Uni)	6.6***	6.2***	9.9***	6.9
TransDigm (Uni)	-5.3***	-4.0***	-10.7***	-5.0

Kratos (Uni)	4.0	5.1	13.5**	2.8
AeroVironment (Uni)	11.5**	12.0***	24.9***	18.5*
Axon (Uni)	8.8**	8.7**	1.9	-3.1
Boeing (Uni)	-10.7***	-8.1***	-17.3***	-10.5***
Elbit Systems (Isr)	12.8***	14.1***	22.7***	17.1***
Hanwha Aerospace (Kor)	7.4**	1.2	4.1	3.7
Korea Aerospace Ind. (Kor)	3.4	0.1	4.1	5.6
Mitsubishi Heavy (Jap)	9.6***	9.0***	11.2***	13.5**
Bharat Electronics (Ind)	10.9***	4.9**	3.4	-0.5
Hindustan Aeronautics (Ind)	8.5***	5.8***	0.5	3.2

Note: Cumulative abnormal returns (%) around the 27 February 2022 Zeitenwende (event day = 28 Feb 2022), from a two-factor model on the world market (MXWO) and the global defence sector (US aerospace-defence ETF), estimated over [-200,-21]. A positive CAR is a country-specific rearmament repricing over and above the global defence move. t-stats from estimation-window residual variance. * |t|>1.64, ** |t|>1.96, *** |t|>2.58.

Table A3

Largest daily realizations of the constructed rearmament shock.

Date	**Shock (%)**	**GPR threat**
2025-03-03	+13.84	156
2022-02-28	+12.26	678
2020-03-20	+8.02	81
2020-03-17	-7.86	86
2025-04-09	-6.11	153
2024-04-09	-6.03	173
2025-03-07	-5.85	222
2025-08-19	-5.30	107
2025-05-12	-5.25	215
2025-04-04	-5.20	184
Series mean	-0.005	
Series SD	1.164	
Obs (daily)	4599	
Corr. with GPR threat	0.02	

Note: Ten largest absolute daily values of the rolling-market-model rearmament shock, 2005–2026. The construction uses no event dates; that the top realizations coincide with the 2022 invasion / Zeitenwende validates the measure. Correlation with the daily geopolitical-risk threat index reported below.

Table A4

Leave-one-out robustness: dropping each country.

Excluded country	**Treated × Post**	**SE**	**N**
Excl. Germany	0.668**	*(0.287)*	5418
Excl. France	0.637**	*(0.284)*	5418
Excl. Italy	0.640**	*(0.281)*	5418
Excl. Spain	0.652**	*(0.279)*	5418
Excl. Netherlands	0.657**	*(0.286)*	5418

Excl. Belgium	0.643**	*(0.283)*	5418
Excl. Austria	0.759**	*(0.282)*	5418
Excl. Portugal	0.691**	*(0.268)*	5419
Excl. Ireland	0.602**	*(0.261)*	5418
Excl. Finland	0.644**	*(0.286)*	5418
Excl. Greece	0.779***	*(0.225)*	5419
Excl. United Kingdom	0.612**	*(0.284)*	5418
Excl. Sweden	0.690**	*(0.287)*	5418
Excl. Norway	0.646**	*(0.289)*	5418
Excl. Denmark	0.678**	*(0.287)*	5418
Excl. Switzerland	0.709**	*(0.287)*	5418
Excl. Poland	0.600**	*(0.282)*	5418
Excl. United States	0.830***	*(0.256)*	5418
Excl. Canada	0.795***	*(0.272)*	5418
Excl. Australia	0.737**	*(0.286)*	5418
Excl. Japan	0.743**	*(0.285)*	5418

Note: Each row re-estimates the baseline (country FE + common controls) dropping one country. Stability of the coefficient indicates no single country drives the result. SEs clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

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