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Growth Costs of Economic Sanctions: Panel Evidence from European Sanctioning Economies After 2022

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Abstract

This study examines whether the 2022 tightening of sanctions against Russia and Belarus was associated with lower real GDP growth in European sanctioning economies. Most of the sanctions literature examines the impact on targeted states, whereas the macroeconomic costs for sanctioning economies have received far less systematic attention. Using annual panel data for 60 countries over 2016–2024, drawn from IMF, World Bank, and UN Comtrade databases, the analysis applies a difference-in-differences framework with country and year fixed effects, supported by parallel-trends diagnostics, placebo tests, and formal sensitivity analysis. The results indicate that, in the post-2022 period (2022–2024), real GDP growth in the sanctioning group was lower by approximately 1.47 percentage points relative to the control group; given a positive pre-treatment signal in 2021, this differential may represent an upper bound on the sanctions-related component. The negative differential was robustly stronger in more open economies. Within the sanctioning group, greater pre-2022 export dependence was associated with a more negative post-2022 growth change, whereas energy vulnerability showed no statistically significant heterogeneity. Overall, the findings indicate uneven short-run growth differentials associated with the sanctions episode, with stronger differentials in highly open economies.

Keywords: economic sanctions; real GDP growth; sanctioning countries; trade openness; export exposure; difference-in-differences

1. Introduction

International economic sanctions have become one of the most widely used instruments of foreign policy. Morgan et al. (2023) show that their use increased markedly in the post-World War II era, while Hufbauer et al. (2007) identify sanctions as a central instrument of economic pressure in international relations. Following Russia's full-scale invasion of Ukraine in 2022, the European Union significantly broadened and tightened restrictive measures against Russia and Belarus. These measures covered finance, energy, technology, transport, and trade (Council of the European Union, 2022). Because of this broad sectoral coverage, the 2022 sanctions episode provides a suitable empirical setting for examining the economic consequences for sanctioning countries.

The effects of sanctions are not confined to targeted economies. One of the most direct transmission channels is trade. Afesorgbor (2019) shows that imposed economic sanctions reduce trade flows between sanctioning and targeted countries, implying that sanctioning economies may also be adversely affected through weaker export linkages and lower trade activity. Crozet and Hinz (2020), analysing the 2014 Russia sanctions episode, estimate

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that Western sanctioning countries incurred export losses of approximately USD 42 billion, equivalent to around 0.3% of their predicted total exports in 2014–2015. Chowdhry et al. (2024) further show, in a quantitative trade model of the Iran and Russia sanctions regimes, that the welfare costs borne by sender economies depend materially on the design and composition of the sanctions coalition. This suggests that the economic burden of sanctions is not borne exclusively by the targeted state. In the context of the 2022 sanctions episode, these trade-related disruptions interacted with broader macroeconomic shocks, especially in energy markets, whose effects on investment and future competitiveness have attracted sustained attention in both policy and academic discussions. Anaya Longaric et al. (2024) show that energy shocks may significantly depress firm-level investment and weaken future competitiveness.

Systematic empirical evidence on the economic costs borne by sanctioning countries remains limited. Most of the existing literature focuses on the effects of sanctions on target states (Gutmann et al., 2023a, 2023b; Neuenkirch & Neumeier, 2015), whereas the costs for sanctioning states have been examined primarily through bilateral trade flows and firm-level adjustments (Crozet & Hinz, 2020; Yang et al., 2009). By comparison, much less systematic evidence is available on whether sanctions affect broader macroeconomic outcomes, such as aggregate economic growth, in the economies that impose them.

A further issue is that the costs of sanctions are unlikely to be distributed evenly across countries. Sanctioning economies that were more strongly integrated with sanctioned markets before 2022, or that were more exposed to external trade flows, may face more pronounced adjustment pressures. Evidence from the Baltic states and from Lithuanian firms' responses to the 2014 Russia sanctions points to heterogeneous adjustment patterns across countries, sectors, and firms (Lastauskas et al., 2023; Oja, 2015). This suggests that the economic effects of sanctions should be assessed not only in terms of their average impact but also in terms of cross-country variation in structural exposure and vulnerability.

This article examines whether the 2022 tightening of sanctions against Russia and Belarus was associated with weaker real GDP growth in European sanctioning economies during the post-2022 period (2022–2024) and whether this association varied with pre-existing exposure to sanctioned markets and trade openness. Using cross-country panel evidence, the study examines variation in the post-2022 growth differential according to pre-sanctions export exposure and trade openness. Two hypotheses guide the empirical analysis: H1 tests whether real GDP growth was lower in European sanctioning countries relative to the control group during the post-2022 period, while H2 tests whether this negative association was stronger in countries with greater pre-2022 exposure to Russian and Belarusian markets and higher trade openness. Lithuania is treated only as an illustrative small-open-economy case, not as a separate country-level causal estimate.

Methodologically, the analysis employs a difference-in-differences framework with country and year fixed effects, using annual panel data for 60 countries over the period 2016–2024. Real GDP growth and the macroeconomic control variables are taken from the IMF Data Explorer, foreign direct investment from the World Bank's World Development Indicators, and trade flows from the UN Comtrade database. Throughout the article, the pre-sanctions period is defined as 2016–2021 and the post-2022 period as 2022–2024, the three-year period beginning with the tightening of the sanctions regime in 2022 for which complete annual data were available. Accordingly, the analysis covers the sanctions regime as tightened up to the end of 2024 and does not extend to measures adopted thereafter. The credibility of the empirical design is assessed through visual and regression-based tests of the parallel-trends assumption. In addition, a placebo event-study specification is used to assess whether differential trends were present before 2022 and to qualify the interpretation of the estimated post-2022 growth differential.

The remainder of the article is organised as follows. Section 2 presents the theoretical and empirical background and develops the hypotheses. Section 3 describes the data, research design, econometric specification, and identification checks. Section 4 reports the empirical results, while Section 5 discusses their interpretation, policy implications, and limitations. Section 6 concludes.

2. Literature Review and Hypotheses

2.1. Concept, Objectives, and Classification of Economic Sanctions

International economic sanctions are generally defined as politically imposed restrictions on trade, financial flows, or other forms of economic interaction, aimed at exerting pressure on a target state or other actors without the use of military force (Hufbauer et al., 2007; Morgan et al., 2023). This definition allows sanctions to be understood not only as a formal instrument of foreign policy but also as a broader mechanism of economic pressure, whose effects extend beyond directly restricted transactions to include increased uncertainty, higher risk premia, and the reconfiguration of international economic relations.

The literature commonly distinguishes three principal functions of sanctions: coercion, constraint, and signalling (Giumelli, 2010). Sanctions may therefore aim to increase the political and economic costs faced by the target, restrict its access to financial resources, technologies, or strategic goods, and convey a political stance to the international community. Consequently, evaluating sanctions solely in terms of behavioural change in the target state is overly narrow, as it neglects their broader strategic purposes and multiple policy objectives (Drezner, 1999; Giumelli, 2010; Kaempfer & Lowenberg, 2007).

From a classification perspective, sanctions may include trade restrictions, financial sanctions, export controls on technologies and dual-use goods, sectoral measures, and travel bans. Portela (2010) shows that sanctions have become an important instrument of European Union foreign policy. In this regard, the sanctions regime imposed on Russia and Belarus in 2022 stands out because of its scale and scope, making it particularly relevant not only for analysing the effects on target states but also for examining the adjustment pressures and economic costs borne by sanctioning economies themselves.

The restrictive measures examined in this study have a clearly defined official basis. The EU sanctions framework against Russia comprises complementary sectoral and individual measures. Sectoral economic measures are based principally on Council Decision 2014/512/CFSP and Council Regulation (EU) No 833/2014, both adopted in July 2014 in response to Russia's actions destabilising the situation in Ukraine and substantially broadened after 24 February 2022 (Council of the European Union, 2014b, 2014d). Individual asset freezes and travel restrictions are based on Council Decision 2014/145/CFSP and Council Regulation (EU) No 269/2014, adopted in March 2014 and subsequently amended and extended (Council of the European Union, 2014a, 2014c). The parallel measures against Belarus are based on Council Regulation (EC) No 765/2006 and Council Decision 2012/642/CFSP, which were extended in 2022 in response to Belarus's involvement in the aggression against Ukraine (Council of the European Union, 2006, 2012).

Between February 2022 and December 2024, the EU adopted fifteen successive sanctions packages against Russia and introduced or extended related restrictive measures concerning Belarus. This period corresponds to the post-2022 period examined in this study. The analysis therefore covers the tightening of the sanctions framework up to the end of 2024, including the measures introduced through packages 1–15; packages adopted in 2025 and later fall outside the study period. Because the framework consists of several complementary legal acts, no single legal instrument covers the sanctions regime in its entirety. Consolidated versions of the relevant acts are publicly available through EUR-Lex, while the EU Sanctions Map provides a regime-by-regime overview and links to the applicable

legal instruments (European Commission, 2025a, 2025b). Table 1 summarises the principal measures adopted or extended during 2022–2024.

Table 1. EU sanctions packages against Russia and related restrictive measures concerning Belarus, 2022–2024.

Packages	Adoption Dates	Main Measures and Sectoral Coverage
1st–5th	23 February–8 April 2022	Individual asset freezes and travel bans; exclusion of major Russian banks from SWIFT; prohibition of transactions with the Central Bank of Russia; export controls on dual-use goods and advanced technology; closure of EU airspace and ports; import bans on coal, iron, and steel; restrictions on Belarusian exports of potash, wood, cement, iron, and steel.
6th–9th	3 June–16 December 2022	Embargo on seaborne imports of Russian crude oil and petroleum products; introduction of the legal basis for the oil price cap agreed by the G7 and partner countries; extension of SWIFT exclusions, including Sberbank; import ban on Russian gold; suspension of broadcasting licences of Russian state-controlled media; export restrictions on drone components and other technology that could support Russia's military capabilities.
10th–12th	25 February–18 December 2023	Tightened export controls and transit prohibitions; anti-circumvention measures targeting re-exports through third countries; import ban on Russian diamonds; strengthened enforcement of the oil price cap; introduction of contractual "no-Russia" re-export clauses for specified sensitive goods.
13th–15th	23 February–16 December 2024	Additional individual designations; prohibition concerning the use of Russia's SPFS financial messaging system; prohibition of the transshipment of Russian liquefied natural gas through EU ports; designation of vessels associated with Russia's shadow fleet; further anti-circumvention measures; parallel extension of related trade, financial, and anti-circumvention restrictions to Belarus.

Note: The table presents selected principal measures and is not an exhaustive list of all restrictions or individual designations. Packages 1–15 progressively amended the EU sanctions framework against Russia, principally Council Regulation (EU) No 833/2014 and Council Decision 2014/512/CFSP. Related measures against Belarus were introduced and extended through amendments to Council Regulation (EC) No 765/2006 and Council Decision 2012/642/CFSP. Source: Authors' compilation based on Council of the European Union (2006, 2012, 2014a, 2014b, 2014c, 2014d) and European Commission (2025a, 2025b).

The officially stated objective of these measures is to weaken Russia's economic base, restrict its access to critical technologies and markets, and significantly curtail its ability to wage war, while limiting disproportionate consequences for the EU economy and third countries (Council of the European Union, 2025). The intended effects operate primarily through restrictions on Russia's access to finance, technology, and export revenues. From the perspective of this study, however, the curtailment of trade, financial, and transport links between sanctioning economies and the targeted states may also generate adjustment pressures within the sanctioning countries themselves.

2.2. Theoretical and Empirical Background: Transmission Channels and Evidence on Economic Costs

The analysis draws on three complementary theoretical perspectives: neoclassical growth theory, endogenous growth theory, and the open economy approach. Together, these perspectives help explain the mechanisms through which sanctions may affect both target states and sanctioning countries.

Neoclassical growth theory explains long-run growth through capital accumulation, labour input, and exogenous technological progress. In the Solow (1956) and Swan (1956) models, capital accumulation increases the level of output, but diminishing returns prevent it from sustaining long-run growth on its own. Mankiw et al. (1992), by extending the

framework to include human capital, show that differences in investment and human capital accumulation help explain cross-country variation in income and growth. From this perspective, sanctions can be understood as an adverse external shock that constrains access to capital goods, external finance, and technology. Empirical evidence is broadly consistent with this mechanism. Neuenkirch and Neumeier (2015) find that United Nations sanctions reduce real GDP per capita growth in target states by 2.3 to 3.5 percentage points on average, while US sanctions reduce it by 0.5 to 0.9 percentage points. Gutmann et al. (2023a) similarly document negative effects on the growth rates of real GDP, consumption, and investment, as well as on foreign trade and foreign direct investment. Korhonen (2019) also highlights reduced access to foreign finance as an important part of the broader transmission mechanism.

Endogenous growth theory links long-run growth to knowledge creation, innovation, human capital accumulation, and institutional incentives. Romer (1990) conceptualises technological change as an endogenous process, while Lucas (1988) emphasises the role of human capital and its externalities. Aghion and Howitt (1992) relate growth to creative destruction, and Barro (1990) stresses the importance of productive public expenditure and public capital. Within this framework, sanctions may reduce knowledge diffusion, weaken innovation incentives, and slow long-run productivity growth. Wen et al. (2024) show that sanctions reduce innovation activity in target countries, with effects varying across types of sanctions and country characteristics. Simola (2023) reviews evidence indicating that Russia's adjustment through import substitution and trade reorientation mitigated some aggregate effects without eliminating pressures on the directly targeted sectors. For sanctioning countries, related adjustment pressures may arise through changes in financial and payment infrastructures (Cipriani et al., 2023) and through energy-related effects on investment and competitiveness (Anaya Longaric et al., 2024).

The open economy approach emphasises the importance of international trade, foreign direct investment, and integration into global markets for long-run growth. Frankel and Romer (1999) show that greater trade intensity is associated with higher income per capita. Borensztein et al. (1998) identify foreign direct investment as an important channel of technology transfer and growth, while Grossman and Helpman (1991) stress the link between trade and innovation. From this perspective, sanctions can be interpreted as an external shock that disrupts established trade and financial linkages, may reduce effective integration into global markets, and raises international transaction costs. Crozet and Hinz (2020) show that sanctions and counter-sanctions generated substantial export losses during the 2014 Russia sanctions episode. Imbs and Pauwels (2024) further show that the effects of trade sanctions depend not only on direct exposure but also on broader input-output linkages across countries and sectors. Related evidence from the global value chain literature illustrates a broader cross-border transmission mechanism. Bonadio et al. (2021) show that pandemic-related shocks were transmitted internationally through supply-chain linkages, while Attinasi et al. (2025) find that geopolitical decoupling of supply chains can entail substantial welfare costs, particularly in the short run, with losses varying across countries. The main theoretical mechanisms linking sanctions to economic growth are summarised in Table 2.

The 2022 sanctions episode should also be considered within the broader economic consequences of the Russia–Ukraine war and the associated process of geoeconomic fragmentation. Liadze et al. (2023) estimate that the conflict could reduce the level of global GDP by approximately 1% by 2023, with Europe being the most affected region. This indicates that the sanctions were implemented alongside substantial war-related economic shocks. Gopinath et al. (2025) document that trade and investment flows between geopolitically distant blocs have declined markedly since the onset of the war in Ukraine, while

Aiyar et al. (2024) show that geopolitical alignment has become an increasingly important determinant of foreign direct investment. Jakubik and Ruta (2023) find that, during periods of heightened trade-policy uncertainty, geopolitically aligned countries trade relatively more with one another. Baba et al. (2023) further demonstrate that the EU is exposed to the costs of geoeconomic fragmentation because of its deep integration into global value chains. This literature suggests that the sanctions episode should be viewed not only as a bilateral trade disruption but also as part of a wider reconfiguration of international economic relations, with potentially greater adjustment pressures in highly open economies.

Table 2. The impact of sanctions on economic growth across growth theories.

Growth Theory	Transmission Mechanism	Expected Effect on GDP	Main Channel
Neoclassical (Solow-Swan)	Constraints on capital accumulation and access to technology	Negative short-run effect; lower output level in the longer run	Capital, productivity
Endogenous growth (Romer, Lucas)	Reduced diffusion of technology and knowledge; weaker incentives to innovate	Possible persistent effects on productivity growth and long-run output	Innovation, human capital
Open economy and international integration	Lower trade and investment flows; weaker gains from specialisation	Negative effects likely to be stronger in more open and externally exposed economies	Trade, foreign direct investment, technology diffusion

Source: Authors' compilation based on Solow (1956), Swan (1956), Mankiw et al. (1992), Romer (1990), Lucas (1988), Frankel and Romer (1999), Borensztein et al. (1998), and Grossman and Helpman (1991).

This mechanism may be particularly relevant for small open economies, whose external exposure can be proportionally larger. Available model-based evidence suggests that the economic costs borne by sanctioning countries are generally smaller than those experienced by target states but are unevenly distributed. Crozet and Hinz (2020) document substantial export losses among Western sanctioning countries during the 2014 sanctions episode. At the European Union level, Nilsson et al. (2023) estimate that the sanctions and Russian countermeasures covered by their computable general equilibrium model may have reduced EU GDP by approximately 0.08%. Mahlkow (2023) projects a long-run decline in the EU GDP level of approximately 0.21% as a result of Western sanctions and Russian counter-sanctions.

The literature also points to substantial heterogeneity in these effects. Previous studies suggest that the economic consequences of sanctions differ across countries and sectors, reflecting variation in pre-sanctions trade links with the Russian market (Crozet & Hinz, 2020; Fritz et al., 2017). In the Baltic context, Oja (2015) shows that exposure to goods affected by Russian counter-sanctions was substantial in some Baltic economies, while the overall GDP effect remained below 0.5% once supply-chain linkages were taken into account. Firm- and sector-level evidence further indicates heterogeneous adjustment through changes in employment and investment, export reorientation, and shifts in production and market orientation, particularly in the agri-food sector (Lastauskas et al., 2023; Vitunskienė & Serva, 2017). Taken together, these studies indicate that the costs borne by sanctioning countries vary with their pre-sanctions trade exposure, sectoral composition, and capacity to redirect trade towards alternative markets.

Overall, the existing literature provides relatively strong evidence on the consequences of sanctions for target states and more limited evidence on the costs borne by sanctioning countries. It also suggests that sanctions may influence economic performance through multiple mechanisms, including trade disruption, financial constraints, weaker investment,

and slower innovation, and that these consequences differ across countries and sectors according to pre-sanctions exposure, openness, structural vulnerability, and adjustment capacity. However, systematic econometric evidence on whether the tightening of sanctions in 2022 was associated with lower real GDP growth in European sanctioning countries, and whether this association varied with pre-sanctions export dependence on Russian and Belarusian markets and trade openness, remains limited. This gap motivates the empirical analysis developed in this study.

2.3. Research Hypotheses

Based on the theoretical arguments and empirical evidence reviewed above, the study advances two hypotheses.

H1. *Following the tightening of sanctions in 2022, real GDP growth in European sanctioning countries was significantly lower relative to the control group.*

This hypothesis is grounded in earlier evidence showing that sanctioning states incurred export losses and that, although the macroeconomic impact at the European Union level appears relatively limited, available estimates still point to an overall negative effect (Crozet & Hinz, 2020; Mahlkow, 2023; Nilsson et al., 2023).

H2. *The negative post-2022 growth differential was larger in countries that, prior to 2022, were more dependent on Russian and Belarusian markets and displayed greater trade openness.*

This hypothesis follows from the heterogeneity argument: countries with stronger pre-sanctions economic linkages to sanctioned markets and greater overall openness are likely to face higher adjustment costs due to export market losses, supply-chain restructuring, and indirect effects transmitted through global value chains (Crozet & Hinz, 2020; Imbs & Pauwels, 2024).

3. Materials and Methods

3.1. Research Design

This article examines whether the 2022 tightening of sanctions against Russia and Belarus was associated with a differential change in real GDP growth in European sanctioning countries. The empirical analysis is based on a difference-in-differences (DiD) framework, which compares changes in real GDP growth between the treatment and control groups over the pre-sanctions and post-2022 periods. By including country and year fixed effects, the DiD framework controls simultaneously for time-invariant cross-country differences and common shocks affecting all countries in a given year. The interpretation of the DiD estimates is evaluated in light of the validation checks reported in Section 3.4.

The treatment group consists of 31 European sanctioning countries, while the control group includes 29 countries that did not join an equivalent sanctions regime during the period under analysis and for which sufficient data were available to construct the main macroeconomic indicators (Table 3). The analysis uses annual panel data covering the period 2016–2024. The pre-sanctions period is defined as 2016–2021, while the post-2022 period covers 2022–2024. The full sample comprises 60 countries and 540 observations.

The composition of the control group reflects a deliberate trade-off. Within Europe, almost all economies either joined the sanctions coalition or were directly targeted by it, so no sufficiently large group of European non-sanctioning countries exists. The few remaining European non-sanctioning economies are, moreover, deeply integrated with EU and Russian markets through trade and input–output linkages and could therefore have been exposed to the sanctions shock through indirect channels (Imbs & Pauwels, 2024).

Including them in the control group could contaminate the comparison and may bias the estimated differential towards zero. Selecting control countries from more distant regions reduces this risk of spillover contamination, at the cost of greater structural heterogeneity between the treatment and control groups.

Table 3. Sample composition: treatment and control groups.

Group	Countries	Notes
Treatment (N = 31)	EU-27 member states: Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, and Sweden. Other countries: the United Kingdom, Norway, Iceland, and Switzerland.	Joined the EU/UK/Swiss sanctions regime against Russia and Belarus after 24 February 2022.
Control (N = 29)	Africa (11): South Africa, Egypt, Nigeria, Algeria, Morocco, Tunisia, Ghana, Senegal, Côte d'Ivoire, Kenya, and Tanzania. Middle East (4): Saudi Arabia, the United Arab Emirates, Kuwait, and Oman. Asia (8): Pakistan, Bangladesh, Vietnam, Thailand, Malaysia, the Philippines, Sri Lanka, and Indonesia. Latin America (6): Argentina, Colombia, Chile, Peru, Uruguay, and Mexico.	Did not impose sanctions on Russia and Belarus during 2016–2024. Regional diversity helps ensure that the results are not driven by shocks specific to a single region. Parallel trends checks support comparability.

Note: The treatment group consists of European countries that participated in the sanctions regime against Russia and Belarus from February 2022 onward. The control group consists of countries that did not participate in an equivalent sanctions regime during the study period. Source: Authors' compilation.

This structural heterogeneity is addressed in three ways. First, country fixed effects absorb all time-invariant differences in economic structure, income levels, and institutions. Second, year fixed effects absorb shocks common to all countries in a given year, including world energy prices, global inflationary pressures, and the common component of the post-pandemic recovery. Third, the identification checks reported in Section 4.1 test directly whether the two groups followed comparable growth dynamics before 2022. The regional diversity of the control group further reduces the risk that the results are driven by shocks specific to a single region. Nevertheless, the estimates remain conditional on these comparability diagnostics, and the sensitivity of the results to alternative control-group compositions is acknowledged among the limitations discussed in Section 5.

The sensitivity of the results to the composition of the control group is also examined directly by excluding major energy and commodity exporters, excluding each control region in turn, and using single-region control groups. In addition, leave-one-country-out re-estimation is used to assess whether the results are driven by any individual country. These checks are reported together with the other robustness results in Section 4.2.

The study evaluates not only the average post-2022 growth differential but also heterogeneity in this association. It examines whether the negative association was stronger in countries that, prior to 2022, displayed greater export dependence on Russian and Belarusian markets and greater trade openness. Energy vulnerability is also examined as an additional heterogeneity dimension because the 2022 sanctions episode coincided with major energy-market disruptions.

3.2. Data Sources and Variables

The empirical analysis draws on data from three main international sources.

Macroeconomic indicators are taken from the IMF Data Explorer (International Monetary Fund, 2025) and include real GDP growth ($growth_{ct}$), population growth ($popGrowth_{ct}$), gross capital formation (GCF_{ct}), inflation (Inf_{ct}), and government expenditure ($GovExp_{ct}$).

Net inflows of foreign direct investment FDI_{ct}, expressed as a percentage of GDP, are obtained from the World Bank's World Development Indicators database (World Bank, 2025).

Trade data are drawn from the UN Comtrade database (United Nations, 2025). These data include country-reported bilateral trade flows in goods. Goods imports and exports are categorized using Harmonized System (HS) codes. Based on country-reported exports (rather than import data reported by Russia and Belarus) the analysis denotes exports to Russia and Belarus as $XRuBy_{ct}$ and total exports to the world as $XTotal_{ct}$. Total imports from the world are represented by $MTotal_{ct}$. Energy goods imports from Russia and the world are represented respectively by $MEnergyRu_{ct}$ and $MEnergyTotal_{ct}$. Energy goods imports are aggregated using the HS Code 27.

The main dependent variable is annual real GDP growth. The main specification includes gross capital formation, inflation, net inflows of foreign direct investment, population growth, and government expenditure as macroeconomic controls. Because inflation and foreign direct investment may also reflect adjustment channels after the sanctions shock, the interpretation of the main estimate is complemented by a reduced robustness specification that excludes all macroeconomic controls and retains only the country and year fixed effects. For the heterogeneity analysis, three vulnerability indicators are constructed: export dependence on Russian and Belarusian markets, energy vulnerability, and trade openness. Because the aim is to capture pre-sanctions vulnerability, the heterogeneity models use fixed pre-2022 values of these indicators, defined as country averages over 2016–2021.

The definitions, measurement, and sources of all variables are summarised in Table 4. The analytical country-year panel dataset and replication code are provided as Supplementary Materials.

Table 4. Variable definitions, measurement, and data sources.

Variable	Description	Formula/Construction	Unit	Source
$growth_{ct}$	Real GDP growth in country *c year t*	Annual real GDP growth rate	%	IMF
$Post2022_t$	Post-2022 period indicator	$Post2022_t = 1$ for 2022–2024; $Post2022_t = 0$ for 2016–2021	0/1	Authors' construction
$Treat_c$	Treatment-group indicator	$Treat = 1$, if the country belongs to the group of sanctioning countries; $Treat = 0$, otherwise	0/1	Authors' construction
$Sanctions2022_{ct}$	Interaction term $Post2022_t \times Treat_c$.	$Sanctions2022_{ct} = 1$, if the country belongs to the group of sanctioning countries and the time period is 2022–2024	0/1	Authors' construction
GCF_{ct}	Gross capital formation	Direct indicator	% of GDP	IMF
Inf_{ct}	Inflation	Annual change in the consumer price index	%	IMF

Table 4. *Cont.*

Variable	Description	Formula/Construction	Unit	Source
FDI_{ct}	Foreign direct investment	Net FDI inflows	% of GDP	World Bank
$PopGrowth_{ct}$	Population growth	Annual population growth rate	%	IMF
$GovExp_{ct}$	Government expenditure	Direct indicator	% of GDP	IMF
$Exposure_{ct}$	Export exposure to the Russian and Belarusian markets	$\frac{XRuBy_{ct}}{XTotal_{ct}}$	[0; 1]	UN Comtrade
$EnergyVuln_{ct}$	Energy vulnerability	$\frac{MEnergyRu_{ct}}{MEnergyTotal_{ct}}$	[0; 1]	UN Comtrade
$Tradeopen_{ct}$	Trade openness	$\frac{XTotal_{ct}+MTotal_{ct}}{GDP_{c,t}}$	ratio	UN Comtrade, IMF

Note: In the baseline model, the post-2022 differential effect for sanctioning countries is identified through the interaction term $Post2022_t \times Treat_c$. In the heterogeneity specifications, the following interaction terms are added: $(Post2022_t \times Treat_c) \times Exposure_c$; $(Post2022_t \times Treat_c) \times EnergyVuln_c$; and $(Post2022_t \times Treat_c) \times Tradeopen_c$, where the vulnerability indicators enter as fixed pre-2022 values, calculated as country averages over 2016–2021. This structure makes it possible to distinguish the baseline differential post-2022 effect from variation in that effect across countries with different levels of pre-sanctions vulnerability; a robustness check using annual indicator values is reported in Section 4.3.

3.3. Econometric Specification

To test the first hypothesis, the following DiD specification with country and year fixed effects is estimated:

$$growth_{ct} = \alpha_c + \tau_t + \delta(Post2022_t \times Treat_c) + \gamma' X_{ct} + u_{ct} \quad (1)$$

where $growth_{ct}$ denotes annual real GDP growth in country c at time t; α_c is the country fixed effect capturing time-invariant country-specific factors; τ_t is the year fixed effect capturing common shocks and trends affecting all countries in year t; δ is the coefficient on the interaction term $(Post2022_t \times Treat_c)$, interpreted as the DiD estimate of the post-2022 differential effect for sanctioning countries; $Post2022_t$ is the post-2022 period indicator (1 for 2022–2024, 0 for 2016–2021); $Treat_c$ is the treatment-group indicator (1 for a sanctioning country, 0 for a control-group country); γ' is the vector of coefficients on control variables; X_{ct} is the vector of macroeconomic control variables; and u_{ct} is the error term.

The vector of control variables is defined as:

$$X_{ct} = \{Inf_{ct}, GCF_{ct}, FDI_{ct}, PopGrowth_{ct}, GovExp_{ct}\}.$$

Accordingly, the post-2022 differential effect in this model is identified through the interaction between the post-2022 period indicator and the treatment-group indicator. This specification is consistent with the logic of the research design and with the interpretation of the DiD estimator. For brevity, the interaction term is denoted $Sanctions2022_{ct} = Post2022_t \times Treat_c$ in what follows.

To test the second hypothesis, a restricted heterogeneity specification augments the baseline post-2022 treatment indicator with its interaction with a fixed pre-sanctions vulnerability measure:

$$growth_{ct} = \alpha_c + \tau_t + \beta_1 Sanctions2022_{ct} + \beta_3(Sanctions2022_{ct} \times Z_c) + \gamma' X_{ct} + u_{ct} \quad (2)$$

Here, Z_c denotes, in turn, the fixed pre-2022 measure of export dependence, energy vulnerability, or trade openness, calculated as the country average over 2016–2021. Because Z_c is time-invariant, its level effect is absorbed by the country fixed effects. Equation (2) is restricted because it assumes that the control-group post-2022 change does not vary systematically with Z_c.

The heterogeneity analysis does not estimate a separate regression for each of the 31 sanctioning countries. A single pooled interaction model (Equation (2)) is estimated on the full panel of 540 country-year observations, and the country-level figures discussed in Section 4.3 are obtained by evaluating the estimated marginal-effect function, $\beta_1 + \beta_3 \times Z_c$, at a country's pre-2022 value of the relevant vulnerability indicator. These values are model-implied evaluations of a common estimated slope, not separate country-specific causal estimates, and they carry the statistical uncertainty of the pooled coefficients from which they are derived.

All principal models are estimated by ordinary least squares in a panel-data framework with country and year fixed effects. Standard errors are adjusted using two-way clustering by country and year, in line with current guidance on cluster-robust inference (MacKinnon et al., 2023). This approach allows for heteroskedasticity, within-country serial correlation, and dependence across countries within the same year.

3.4. Robustness and Validation Checks

Identification relies on parallel pre-2022 growth trends between the treatment and control groups. This assumption is assessed in three complementary ways: a visual inspection of pre-treatment growth trajectories, a residual-based diagnostic check, and a placebo event-study test.

First, the average trajectories of real GDP growth in the two groups are compared over the pre-treatment period.

Second, residuals are obtained from a preliminary fixed-effects model of real GDP growth, estimated on the 2016–2021 pre-treatment sample, that includes the macroeconomic controls and country and year fixed effects but excludes the post-2022 treatment interaction. These residuals, $e_{ct} = y_{ct} - \hat{y}_{ct}$, capture the component of GDP growth not explained by the controls and fixed effects. For each group, the residuals are regressed on event time over the pre-sanctions period:

$$e_{ct} = \alpha + \beta \cdot event\_time_t + \varepsilon_{ct} \tag{3}$$

where $event\_time_t$ denotes years relative to the sanctions ($t = -6, \ldots, -1$). The null hypothesis $H_0 : \beta = 0$ corresponds to no pre-2022 trend in the residuals.

Third, a placebo test is conducted using an event-study specification with relative-time indicators for the periods before and after the 2022 event. Under parallel trends, the pre-event coefficients should not differ significantly from zero; a significant pre-event coefficient is treated as a substantive limitation for causal interpretation rather than as a minor diagnostic detail. The specification is:

$$growth_{ct} = \alpha_c + \tau_t + \sum_{k \in K} \beta_k (Treat_c \times D_{t=k}) + \gamma' X_{ct} + u_{ct} \tag{4}$$

where $D_{t=k}$ denotes a relative-time indicator and $K = \{-4, -3, -2, -1, 1, 2\}$ denotes the set of included event-time indicators, corresponding to 2018–2021 and 2023–2024. The omitted category comprises 2022 together with the pre-2018 years (2016–2017), for which separate treatment-group interaction indicators are not included. The coefficient β_k measures the treatment-group differential at event time k relative to this omitted category.

These diagnostics are complemented by a joint Wald test of the pre-treatment lead coefficients from Equation (4). The null hypothesis is:

$$H_0 : \beta_{-4} = \beta_{-3} = \beta_{-2} = \beta_{-1} = 0 \quad (5)$$

The hypothesis is evaluated using a Wald χ^2 statistic with four degrees of freedom. A variant excluding the 2021 lead is evaluated with three degrees of freedom. In addition, a sensitivity analysis following Rambachan and Roth (2023) is conducted using a separately estimated event-study specification with 2021 as the reference period and standard errors clustered by country; its implementation and results are presented in Section 4.1. Finally, the stability of the baseline estimate is assessed through several robustness checks. Equation (1) is re-estimated after excluding 2021, excluding 2020–2021, restricting the estimation window to 2018–2024, and allowing for a group-specific linear trend. Sensitivity to extreme observations and individual countries is examined by winsorising real GDP growth at the 1st and 99th percentiles and by sequentially excluding each country. The model is also re-estimated using alternative control-group compositions and standard errors clustered only by country. Permutation inference randomly reassigns treatment status across countries while preserving the number of treated countries. The results are reported in Section 4.2. A separate specification allows the treatment group to follow its own linear time trend:

$$growth_{ct} = \alpha_c + \tau_t + \delta Sanctions2022_{ct} + \lambda(Treat_c \times trend_t) + \gamma' X_{ct} + u_{ct} \quad (6)$$

Here, $trend_t$ denotes a linear time trend, and λ measures the additional linear trend of the treatment group relative to the control group.

In this specification, δ measures the post-2022 differential after accounting for a smooth linear difference in trends between the treatment and control groups.

Unless stated otherwise, all robustness models retain the full set of macroeconomic controls and country and year fixed effects from Equation (1). The results are reported in Section 4.2.

4. Results

4.1. Parallel Trends and Identification Checks

The three diagnostics described in Section 3.4 are reported in turn.

Figure 1 compares average annual real GDP growth in the treatment and control groups over 2016–2021. The trajectories followed broadly similar patterns, including the 2020 contraction and subsequent recovery, and their 95% confidence intervals largely overlapped. The visual evidence is therefore broadly consistent with parallel pre-2022 trends, although it does not exclude differences in individual pre-treatment years.

As an additional diagnostic, residuals from the preliminary fixed-effects model described in Section 3.4 are examined for systematic pre-2022 trends within each group.

Figure 2 presents the mean residual GDP growth values for the treatment and control groups over 2016–2024, together with 95% confidence intervals. Before the tightening of sanctions, the average residuals in both groups fluctuate mostly around zero, and the intervals overlap substantially. This is broadly consistent with the absence of a stable and systematic divergence in the unexplained component of GDP growth across the two groups prior to 2022, after accounting for the control variables and fixed effects. At the same time, a more visible short-run deviation appears in 2021: residuals become positive in the treatment group and negative in the control group. For this reason, the visual analysis of residuals is complemented by a formal trend regression, illustrated in Figure 3.

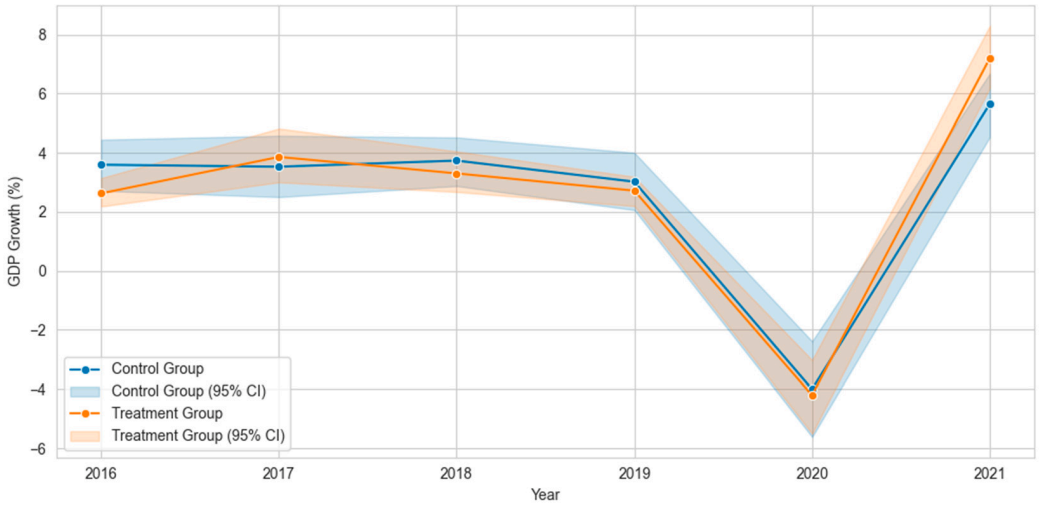

Figure 1. Average GDP growth trajectories before the 2022 sanctions (compiled by the authors based on IMF Data Explorer (International Monetary Fund, 2025)).

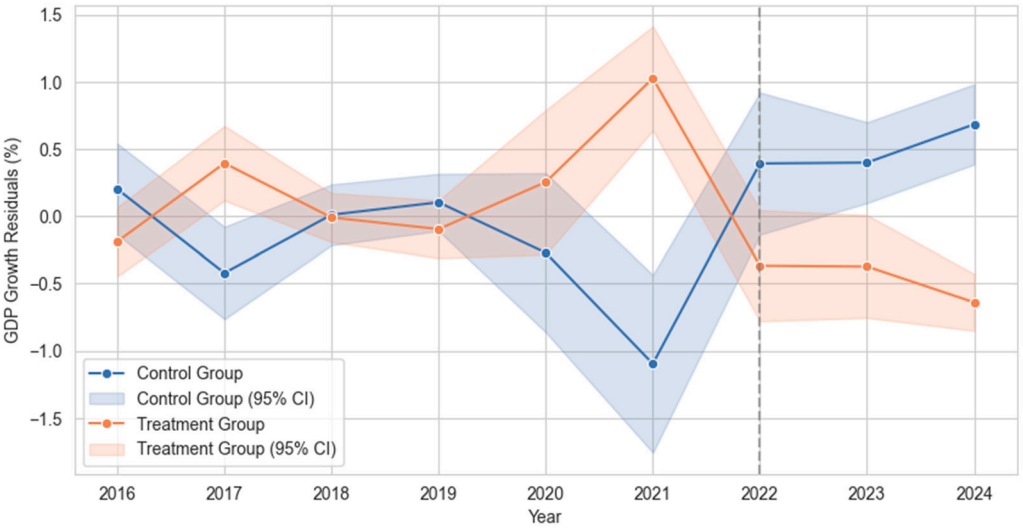

Figure 2. Diagnostic assessment of the parallel-trends assumption using GDP growth residuals (compiled by the authors based on IMF Data Explorer (International Monetary Fund, 2025)).

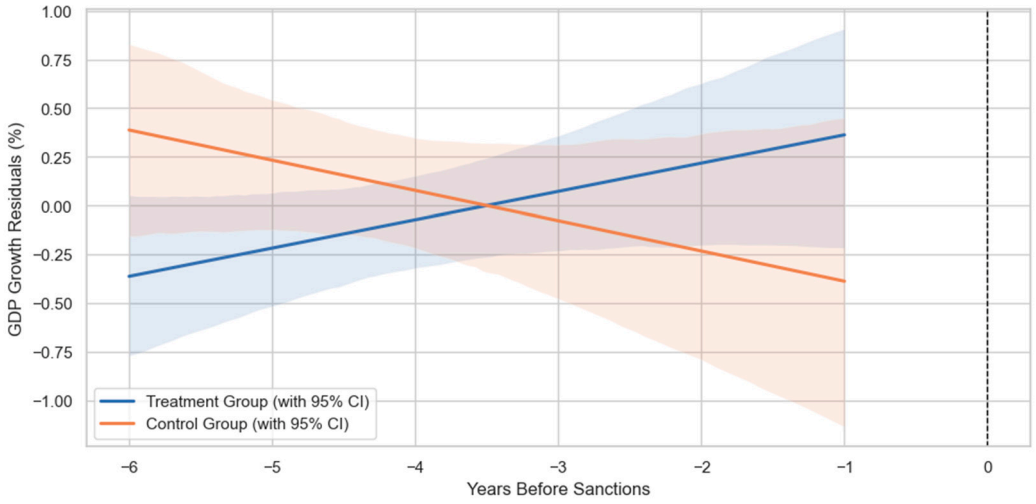

Figure 3. Trend in GDP growth residuals with 95% confidence intervals shaded (compiled by the authors based on IMF Data Explorer (International Monetary Fund, 2025)).

Formally, the association between residuals and the event-time variable in the pre-sanctions period is estimated separately for each group. The regression results are reported in Table 5.

Table 5. Parallel trends test based on event-time regression.

Result	Control Group	Treatment Group
$\hat{\beta}$ (*event_time*)	−0.155	0.145
SE	0.099	0.076
t	−1.561	1.905
p	0.120	0.058
95% CI	[−0.351; 0.041]	[−0.005; 0.295]
N	174	186

Note: The dependent variable is the residual $\hat{e}_{ct}$ from the preliminary fixed-effects model, estimated on the 2016–2021 pre-treatment sample, that includes the macroeconomic controls and country and year fixed effects but excludes the post-2022 treatment interaction. *event_time* denotes years relative to 2022 over the pre-sanctions period $t = -6, \ldots, -1$. Neither coefficient is statistically significant at the 5% level; the treatment-group coefficient is marginal ($p = 0.058$).

Table 5 shows that the null hypothesis of no directional pre-2022 trend is not rejected for either group at the 5% level. However, the positive treatment-group coefficient is marginal (p = 0.058) and warrants further attention in the placebo test. Finally, the placebo test is implemented using the event-study specification in Equation (4), with the coefficient estimates reported in Table 6.

Table 6. Placebo test.

Variable	Year	β	p	95% CI
event_pre4	2018	0.094	0.785	[−0.582; 0.770]
event_pre3	2019	−0.085	0.819	[−0.814; 0.644]
event_pre2	2020	0.666	0.188	[−0.327; 1.660]
event_pre1	2021	2.270	<0.001	[1.439; 3.101]
event_1	2023	−0.657	0.029	[−1.245; −0.068]
event_2	2024	−1.231	0.003	[−2.029; −0.433]

Note: The dependent variable is annual real GDP growth (%). The model corresponds to Equation (4) and includes country and year fixed effects and the full set of macroeconomic controls. The omitted category comprises 2022 together with the pre-2018 years (2016–2017), for which separate treatment-group interaction indicators are not included. Standard errors are two-way clustered by country and year. N = 540.

Table 6 shows that the 2018–2020 pre-event coefficients are statistically indistinguishable from zero, whereas the 2023–2024 coefficients are negative and statistically significant relative to the omitted category. The exception is the positive and highly significant 2021 coefficient, which may reflect the asymmetric post-pandemic recovery or other factors specific to that year. The pre-treatment deviation is therefore concentrated in 2021 and is examined further using the joint Wald test.

As a formal complement to Table 6, the joint Wald test defined in Equation (5) evaluates whether the pre-2022 lead coefficients are jointly equal to zero. Because two-way clustering is unstable in this test owing to the small number of year clusters, standard errors are clustered by country (MacKinnon et al., 2023). The results are reported in Table 7.

Table 7. Joint Wald test for parallel trends.

Null Hypothesis	*W*	*df*	*p*
All pre-2022 lead coefficients equal zero (2018–2021)	8.11	4	0.088
Pre-2022 lead coefficients excluding 2021 equal zero (2018–2020)	0.79	3	0.853

Note: The tests are based on the event-study specification in Equation (4) with the coefficient estimates reported in Table 6. The model includes country and year fixed effects and the full set of macroeconomic controls; standard errors are clustered by country. W denotes the Wald χ^2 statistic and *df* denotes the degrees of freedom.

Table 7 shows that the joint test for the 2018–2021 coefficients is statistically insignificant at the 5% level but marginally significant at the 10% level ($p = 0.088$) reflecting the influence of the individually significant 2021 coefficient reported in Table 6. When 2021 is excluded, the 2018–2020 coefficients are jointly insignificant ($p = 0.853$).

The 2021 coefficient can be interpreted in three ways, with different implications for the baseline estimate. If it reflected a persistent divergence in growth trends, the estimate would understate the true negative effect; however, the near-zero 2018–2020 coefficients in Table 6 are inconsistent with such a trend. If it instead reflected a transitory factor, most plausibly the earlier and stronger post-pandemic rebound in European economies, part of the post-2022 differential may capture mechanical reversion from the 2021 peak, implying that the estimate may overstate the absolute magnitude of the sanctions-related effect. Finally, the spike could reflect some anticipatory adjustment before the 2022 shock; in that case, the coefficient would represent an early response rather than a violation of parallel trends, although this interpretation cannot be tested directly within the present framework, because the isolated 2021 spike is most consistent with an asymmetric post-COVID recovery.

Given the uneven post-pandemic recovery, the post-2022 differential is interpreted cautiously and may represent an upper bound on the sanctions-related component. This interpretation is assessed using the sensitivity framework of Rambachan and Roth (2023), which examines how large post-treatment violations of parallel trends would have to be, relative to those observed before treatment, for the main conclusion to change. For this analysis, the event-study specification is re-estimated with 2021, the last pre-treatment year, as the reference period, and the parameter of interest is the average 2022–2024 coefficient. Table 8 reports 95% robust confidence sets under two assumptions. The relative-magnitude restriction $\Delta^{RM}(\overline{M})$ allows post-treatment violations to be up to $\overline{M}$ times the largest pre-treatment violation, while the smoothness restriction $\Delta^{SD}(M)$ limits how sharply the counterfactual differential trend may change over time.

Table 8. Sensitivity of the average post-2022 differential to violations of parallel trends.

Assumption on Deviations from Parallel Trends	95% Confidence Set	Excludes Zero
No additional restriction	$[-4.82; -1.40]$	Yes
$\Delta^{RM}(\overline{M})$, $\overline{M} = 0.25$	$[-6.27; -0.96]$	Yes
$\Delta^{RM}(\overline{M})$, $\overline{M} = 0.50$	$[-8.20; 0.30]$	No
$\Delta^{SD}(M)$, $M = 0$ (linear continuation of the pre-treatment trend)	$[-3.33; -0.52]$	Yes
$\Delta^{SD}(M)$, $M = 0.25$	$[-6.35; 0.03]$	No

Note: Robust confidence sets are computed following Rambachan and Roth (2023). The event-study model uses 2021 as the reference period, with five pre-treatment coefficients for 2016–2020 and three post-treatment coefficients for 2022–2024. This parametrisation differs from the placebo event-study in Table 6 because 2021 is used here as the reference point. Standard errors are clustered by country. The confidence sets refer to the average of the 2022–2024 coefficients.

Two features of Table 8 stand out. First, the average 2022–2024 differential remains statistically distinguishable from zero when post-treatment violations are restricted to 25% of the largest pre-treatment violation, but the confidence set includes zero when the permitted relative violation reaches 50%. Second, under the smoothness restriction, the confidence set excludes zero when the pre-treatment differential trend is assumed to continue linearly (Δ^{SD} with $M = 0$) but marginally includes zero when deviations from that linear path of up to $M = 0.25$ percentage points per year are allowed. The conclusion is therefore robust to limited, but not moderate, departures from parallel trends. Because the event-study model underlying Table 8 uses 2021 as the reference period, its coefficients are larger in absolute value than the baseline DiD estimate. Therefore, −1.47 percentage points remains the main estimate, while Table 8 shows how sensitive the conclusion is to possible violations of the parallel-trends assumption.

Overall, the diagnostics yield mixed results because of the significant 2021 coefficient. The baseline DiD estimate should therefore be interpreted cautiously, although the sensitivity analysis shows that the negative average 2022–2024 differential remains robust to limited departures from parallel trends.

4.2. Baseline Effect of Sanctions: Hypothesis H1

The first hypothesis tests whether, following the tightening of the sanctions regime in 2022, real GDP growth in European sanctioning countries was lower relative to the control group. To evaluate this, a panel fixed-effects model is estimated with country and year fixed effects, macroeconomic controls, and two-way clustered standard errors by country and year.

A summary of the main results is reported in Table 9. The main DiD coefficient, Sanctions2022, is negative and statistically significant ($\beta = -1.473$, $p = 0.002$, 95% CI [−2.394; −0.553]). This indicates that, after the tightening of the sanctions regime in 2022, real GDP growth in sanctioning countries was lower by approximately 1.47 percentage points on average relative to the control group, after controlling for macroeconomic covariates and netting out time-invariant cross-country differences and common annual shocks.

Table 9. Summary of DiD model regression (H1).

Variable	β	p	95% CI
Sanctions2022 (DiD)	−1.473	0.002	[−2.394; −0.553]
GCF	0.113	0.067	[−0.008; 0.233]
Inf	−0.034	0.084	[−0.072; 0.005]
FDI	0.006	0.004	[0.002; 0.010]
PopGrowth	0.415	0.210	[−0.235; 1.066]
GovExp	−0.149	0.050	[−0.299; 0.000]

Note: The dependent variable is annual real GDP growth (%). The model includes country and year fixed effects. Standard errors are two-way clustered by country and year. $N = 540$ (60 countries × 9 years). Within-$R^2 = 0.072$. Robust F-statistic = 4.770, $p < 0.001$. Poolability F-test = 13.855, $p < 0.001$.

The estimate is statistically significant and macroeconomically meaningful. In the study sample, mean real GDP growth over 2016–2024 was approximately 2.68% (2.45% in the treatment group), so the estimated differential change of −1.47 percentage points amounts to roughly one-half of the average annual growth rate. As a robustness check, a reduced specification that excludes all macroeconomic controls and retains only the country and year fixed effects yields a similar estimate ($\beta = -1.429$, $p = 0.003$, 95% CI [−2.363; −0.495]), indicating that the direction and statistical significance of the result are not sensitive to covariate adjustment. The raw group means show a similar pattern: mean

growth in the treatment group declined from 2.58% in 2016–2021 to 2.18% in 2022–2024, whereas mean growth in the control group increased from 2.59% to 3.62%. The control-group increase may partly reflect the post-2022 commodity-price environment; Table 10 therefore examines whether the result remains after major energy and commodity exporters are excluded from the control group.

Table 10. Robustness of the baseline DiD estimate.

Check	β	SE	p	95% CI/Details
Panel A. Alternative samples, specifications, and inference				
Baseline (full controls)	−1.473	0.468	0.002	[−2.394; −0.553]; N = 540
Excluding 2021	−1.124	0.450	0.013	[−2.008; −0.240]; N = 480
Excluding 2020–2021	−0.963	0.396	0.016	[−1.741; −0.184]; N = 420
Estimation window 2018–2024	−1.617	0.580	0.006	[−2.757; −0.477]; N = 420
Group-specific linear trend	−2.649	0.825	0.001	[−4.271; −1.027]; N = 540
GDP growth winsorised at the 1st and 99th percentiles	−1.456	0.457	0.002	[−2.354; −0.557]; N = 540
Leave-one-country-out (60 models)	[−1.599; −1.280]	N/A	N/A	Mean β = −1.472; each of the 60 estimates has $p < 0.05$
Permutation inference (1000 replications)	−1.473	N/A	0.001	No permuted estimate was as large in absolute value
One-way clustering by country	−1.473	0.466	0.002	[−2.389; −0.558]; N = 540
Panel B. Alternative control-group compositions				
Excluding energy/commodity exporters	−1.149	0.551	0.038	[−2.232; −0.066]; 23 controls; N = 486
Excluding Africa	−1.596	0.461	0.001	[−2.503; −0.690]; 18 controls; N = 441
Excluding the Middle East	−1.409	0.598	0.019	[−2.585; −0.234]; 25 controls; N = 504
Excluding Asia	−1.576	0.369	<0.001	[−2.301; −0.851]; 21 controls; N = 468
Excluding Latin America	−1.286	0.803	0.110	[−2.865; 0.293]; 23 controls; N = 486
Panel C. Single-region control groups				
Africa	−1.393	0.949	0.143	[−3.260; 0.475]; 11 controls; N = 378
Middle East	−1.654	1.728	0.339	[−5.056; 1.747]; 4 controls; N = 315
Asia	−1.143	1.039	0.272	[−3.189; 0.902]; 8 controls; N = 351
Latin America	−1.973	1.117	0.079	[−4.172; 0.227]; 6 controls; N = 333

Note: The dependent variable is annual real GDP growth (%). Unless stated otherwise, all specifications include country and year fixed effects, the full set of macroeconomic controls, and standard errors two-way clustered by country and year. N/A indicates that no single model-based standard error or p-value is applicable to the corresponding cell: the leave-one-country-out row summarises multiple estimates, while permutation inference does not produce a model-based standard error. Permutation inference randomly reassigns treatment status across the 60 countries while holding the number of treated countries fixed (1000 replications; seed = 42). No permuted estimate was as large in absolute value as the actual estimate; the reported p-value of 0.001 applies the finite-replication correction, $(0 + 1)/(1000 + 1)$. The excluded energy and commodity exporters are Saudi Arabia, the United Arab Emirates, Kuwait, Oman, Nigeria, and Algeria. Source: Authors' calculations.

At the same time, given the significant 2021 pre-treatment signal discussed in Section 4.1 and the marginal residual trend for the treatment group, the baseline DiD

estimate may partly capture pre-existing or contemporaneous macroeconomic dynamics and should therefore be interpreted cautiously (Roth, 2022; Roth et al., 2023). These dynamics may include broader war-related, energy-price, or post-pandemic adjustment processes.

To assess whether the negative post-2022 differential is driven by the 2021 anomaly, influential observations, or inference assumptions, a set of further robustness checks is estimated. The results are summarised in Table 10.

Four features of Table 10 deserve emphasis. First, the negative differential remains statistically significant when 2021, or both 2020 and 2021, are excluded, indicating that the result is not attributable solely to the 2021 observations. Second, the estimate remains negative and statistically significant when a group-specific linear trend is included and becomes larger in absolute value. Thus, allowing the treatment group to follow a separate linear trend does not eliminate the negative differential. Third, winsorising GDP growth at the 1st and 99th percentiles produces an estimate close to the baseline result, while all 60 leave-one-country-out estimates remain negative and statistically significant. No permuted treatment assignment produces an estimate as large in absolute value as the actual estimate, and inference remains unchanged when standard errors are clustered only by country rather than by both country and year. Fourth, the estimated differential remains negative across all five alternative control-group compositions in Panel B and is statistically significant in four of the five; the exception is the specification excluding Latin America ($p = 0.110$). In the four single-region control-group models in Panel C, all estimates remain negative, although none is statistically significant at the 5% level. Fourth, the estimated coefficient remains negative across all alternative control-group compositions, although statistical precision declines when Latin America is excluded and in the smaller single-region control-group models.

Among the control variables in Table 9, only foreign direct investment shows a statistically significant positive association with GDP growth, consistent with open-economy arguments concerning the role of capital flows and technology diffusion (Borensztein et al., 1998). Gross capital formation, government expenditure, and inflation display only marginal associations, and population growth is not statistically significant.

The empirical analysis supports H1. Following the tightening of sanctions in 2022, real GDP growth in European sanctioning countries was significantly lower relative to the control group. The estimated post-2022 differential (-1.47 percentage points) is macroeconomically meaningful and remains negative in the robustness specification that excludes all macroeconomic controls. However, in light of the diagnostic caveats identified in Section 4.1, this result should be interpreted as a post-2022 growth differential associated with the sanctions episode rather than as a precise net causal effect of sanctions alone.

4.3. Heterogeneity in the Post-2022 Growth Differential: Hypothesis H2

The second hypothesis tests whether the post-2022 growth differential was stronger in countries that, before 2022, were more dependent on Russian and Belarusian markets and exhibited greater trade openness, or faced greater energy vulnerability. Heterogeneity in this association is evaluated along three dimensions: export dependence on sanctioned markets, energy vulnerability, and trade openness. In each case, the baseline DiD model is augmented with a triple interaction term, $(Post_t \times Treat_c) \times Z_c$, where Z_c is the country's fixed pre-2022 value of the relevant vulnerability indicator (its 2016–2021 average), which allows the post-2022 differential effect to vary systematically with pre-sanctions vulnerability. The results are reported in Table 11.

Table 11. Summary of heterogeneity models (H2).

Vulnerability Indicator	Sanctions2022 (β_1)	SE	p	Interaction Coeff. (β_3)	SE	p	95% CI
Export dependence ($Exposure_c$)	−1.092	0.514	0.034	−13.980	5.581	0.013	[−24.947; −3.014]
Energy vulnerability ($EnergyVuln_c$)	−1.005	0.705	0.154	−2.111	1.619	0.193	[−5.293; 1.071]
Trade openness ($Tradeopen_c$)	0.154	0.758	0.839	−1.269	0.503	0.012	[−2.257; −0.281]

Note: The dependent variable is annual real GDP growth (%). All models include country and year fixed effects, macroeconomic controls, and two-way clustered standard errors by country and year. Because Z_c is time-invariant, its level effect is absorbed by the country fixed effects and is not separately identified. β_1 denotes the baseline post-2022 when $Z = 0$; β_3 denotes the triple-interaction coefficient capturing heterogeneity in effects. The models omit $Post2022_t \times Z_c$, thereby assuming that the control-group post-2022 change does not vary systematically with Z_c.

The interaction coefficient $Sanctions2022 \times Exposure$ is negative and statistically significant ($\beta_3 = -13.980$, SE = 5.581, $p = 0.013$, 95% CI [−24.947; −3.014]), indicating that the negative post-2022 differential on GDP growth becomes stronger as a country's pre-2022 export dependence on Russian and Belarusian markets increases. Since exposure is measured as a ratio on the [0, 1] interval, the coefficient implies that a 1-percentage-point increase in pre-2022 export dependence strengthens the negative post-2022 differential by approximately 0.14 percentage points. The baseline $Sanctions2022$ coefficient is also significant in this model ($\beta_1 = -1.092$, SE = 0.514, $p = 0.034$, 95% CI [−2.103; −0.081]), indicating that even at zero export dependence the post-2022 period is associated with slower growth.

The marginal post-2022 differential for a given country depends on its level of pre-2022 export dependence and is calculated as $\beta_1 + \beta_3 \times Exposure$. For a country with 5% export dependence ($Exposure = 0.05$), the marginal effect is approximately −1.79 percentage points, while for a country with 10% dependence it is about −2.49 percentage points. These calculations illustrate the slope of the interaction model and should not be read as precise causal estimates for individual countries. Averaged over the 31 sanctioning countries at their pre-2022 exposure values, the model-implied differentials average −1.48 percentage points, closely matching the baseline DiD estimate. Because higher exposure values lie outside the range observed in the control group, these evaluations involve extrapolation and should be interpreted cautiously.

The interaction coefficient $Sanctions2022 \times EnergyVuln$ is negative but statistically insignificant ($\beta_3 = -2.111$, SE = 1.619, $p = 0.193$, 95% CI [−5.293; 1.071]). The confidence interval, spanning both negative and positive values, does not support the existence of a systematic relationship between pre-2022 energy vulnerability and the post-2022 effect. The baseline $Sanctions2022$ coefficient remains negative but is not statistically significant in this specification ($\beta_1 = -1.005$, SE = 0.705, $p = 0.154$, 95% CI [−2.390; 0.379]); because the interaction term is likewise insignificant, the model does not provide statistically significant evidence that the post-2022 differential varied systematically with energy vulnerability.

Two considerations may help explain the lack of significance, although both remain tentative. First, the year fixed effects absorb the common component of the 2022 energy shock, so the interaction can only detect a differential effect of pre-2022 dependence on Russian energy; because energy prices are set on world markets, the differential component may simply have been small relative to its sampling variability. Second, rapid post-2022 diversification of energy supply, especially the reorientation of gas imports away from

Russia, may have reduced the short-run differences in the effective impact of energy dependence across countries.

The interaction coefficient $Sanctions2022 \times Tradeopen$ is negative and statistically significant ($\beta_3 = -1.269$, SE = 0.503, $p = 0.012$, 95% CI [−2.257; −0.281]), indicating that economies that were more open before 2022 experienced a stronger negative post-2022 differential. This is directly consistent with the open-economy argument that sanctions raise international transaction costs and that more internationally integrated economies face higher adjustment costs because of their greater dependence on cross-border flows.

The baseline $Sanctions2022$ coefficient is not statistically significant in this model ($\beta_1 = 0.154$, SE = 0.758, $p = 0.839$, 95% CI [−1.335; 1.643]). This implies that, for a hypothetically closed economy ($Tradeopen = 0$), the post-2022 differential is not statistically distinguishable from zero. The significant negative interaction indicates that the post-2022 differential becomes more negative as pre-2022 trade openness increases; it does not establish trade openness as a causal transmission channel. For a country with pre-2022 trade openness equal to 100% of GDP ($tradeopen = 1.0$), the marginal effect is approximately −1.12 percentage points ($\beta_1 + \beta_3 \times 1.0$); for a country with trade openness equal to 150% of GDP, it is about −1.75 percentage points.

As a supplementary robustness check, all three heterogeneity models were re-estimated using annual values of the vulnerability indicators instead of the fixed pre-2022 measures. The conclusions are unchanged: the export-dependence interaction remains negative and statistically significant ($\beta_3 = -44.18$, SE = 18.70, $p = 0.019$), the trade-openness interaction remains negative and significant ($\beta_3 = -1.47$, SE = 0.43, $p < 0.001$), and the energy-vulnerability interaction remains statistically insignificant. Because annual indicators may respond after 2022, these estimates are descriptive and are not the principal basis for evaluating H2. The fixed pre-2022 specification is therefore retained as the main specification.

As a robustness check for the restriction imposed in Equation (2), a fully interacted specification additionally includes $Post2022_t \times Z_c$ allowing the post-2022 change in the control group to vary with the vulnerability measure. In the fully interacted specification, β_2 captures the post-2022 exposure gradient in the control group, while β_3 captures the difference between the exposure gradients of the sanctioning and control groups. Consequently, $\beta_2 + \beta_3$ represents the exposure gradient within the sanctioning group. The statistical significance of the between-group gradient difference, β_3, is sensitive to clustering only by country ($p = 0.094$). By contrast, the exposure gradient within the sanctioning group remains negative ($\beta_2 + \beta_3 = -14.050$, $p = 0.013$). This result is corroborated by a separate model estimated only for the 31 sanctioning countries, in which the coefficient on $Post2022_t \times Exposure_c$ is also negative ($\theta = -11.552$, $p = 0.019$).

Lithuania illustrates how the restricted-model gradient may appear in a small open economy, but the calculation should be treated as descriptive. Lithuania's average pre-2022 export dependence on Russian and Belarusian markets was 0.159, the highest in the treatment group. Evaluating the restricted model at this value implies a model-based post-2022 growth differential of approximately −3.3 percentage points ($\beta_1 + \beta_3 \times 0.159$). Because Lithuania's exposure lies outside the range observed in the control group, this evaluation involves extrapolation and should be interpreted cautiously. It is an evaluation of the pooled interaction model, not a separate Lithuania-specific causal estimate. Lithuania's high trade openness further supports its interpretation as a small open economy, although gross trade statistics may overstate domestic value-added exposure in economies with substantial transit and re-export activity.

The heterogeneity analysis partially supports H2. Trade openness shows a robust negative heterogeneity pattern in both the restricted and fully interacted specifications. Export

dependence is associated with a more negative post-2022 growth change within sanctioning countries, but the corresponding gradient difference relative to the control group cannot be reliably established because of limited common support. Energy vulnerability does not show statistically significant heterogeneity.

5. Discussion

The findings indicate that the tightening of sanctions in 2022 was associated with lower real GDP growth in European sanctioning countries relative to the control group. The baseline estimate implies an average post-2022 differential of approximately −1.47 percentage points, and the differential remains negative across the robustness specifications. This result extends the analysis of sanctions beyond target states by showing that measurable macroeconomic adjustment pressures may also arise in the economies that impose them. Sanctions should therefore be understood not only as instruments of external pressure but also as policies that may entail adjustment costs for sanctioning economies.

The results are consistent with theoretical expectations and previous empirical findings concerning trade disruption, higher international transaction costs, changes in financial linkages, and broader macroeconomic adjustment. The study contributes to this literature by examining macroeconomic changes in sanctioning economies rather than focusing exclusively on target states. The findings also align with the literature on geoeconomic fragmentation, which documents declining trade and investment flows between geopolitically distant blocs and identifies potentially higher adjustment costs for deeply integrated open economies (Aiyar et al., 2024; Baba et al., 2023; Gopinath et al., 2025; Jakubik & Ruta, 2023).

The heterogeneity analysis provides further evidence that the post-2022 growth differential was not uniform across sanctioning economies. Trade openness shows a robust negative heterogeneity pattern in both the restricted and fully interacted specifications: economies that were more open before 2022 experienced a stronger negative post-2022 differential. The evidence for export dependence is more qualified. Within the sanctioning group, greater pre-2022 dependence on Russian and Belarusian markets was associated with a more negative growth change. However, the corresponding gradient difference relative to the control group cannot be reliably established because the two groups have very limited common support in export exposure. H2 is therefore supported for trade openness and receives qualified support for export dependence within sanctioning countries.

No statistically significant heterogeneity is identified with respect to energy vulnerability. This finding does not imply that energy-related factors were unimportant during the post-2022 period. Country and year fixed effects absorb the common component of the energy shock, while the interaction term captures only whether the post-2022 differential varied systematically with pre-2022 energy vulnerability. The absence of statistical significance may therefore indicate that the differential component was small relative to its sampling variability or that rapid diversification of energy supply reduced cross-country differences in effective energy exposure.

Lithuania provides an illustrative example of how the estimated heterogeneity may appear in a small open economy. Its high pre-2022 export exposure and trade openness are consistent with greater trade-adjustment pressures after 2022. However, Lithuania's export exposure lies outside the range observed in the control group, so the model-based calculation involves extrapolation and should not be interpreted as a separate country-level causal estimate. Excluding Lithuania from the sample leaves the baseline estimate essentially unchanged ($\beta = -1.420$, $p = 0.003$), confirming that the aggregate result is not driven by this illustrative case.

The main identification caveat concerns the significant positive 2021 coefficient reported in Table 6, whose implications for the baseline estimate require explicit assessment.

Several interpretations are possible. If the 2021 differential reflected a persistent divergence in underlying growth trends, the counterfactual post-2022 differential would have been positive, implying that the baseline estimate understates the negative post-2022 differential relative to that counterfactual trend. If, instead, it reflected a transitory factor, most plausibly an earlier and stronger post-pandemic rebound in European economies, part of the estimated post-2022 differential may capture mechanical reversion from the elevated 2021 base rather than the sanctions episode. Under this interpretation, the baseline estimate would overstate the absolute magnitude of the sanctions-related component. The event-study pattern in Table 6 is more consistent with the second interpretation: the 2018–2020 coefficients are close to zero and statistically insignificant, whereas the positive deviation is sharp and concentrated in 2021 rather than forming part of a persistent pre-treatment trend.

A third possibility is that the 2021 coefficient captures anticipatory adjustment before the 2022 shock. Under this interpretation, the coefficient would partly represent early responses rather than a violation of parallel trends, implying that the baseline estimate may understate the full effect. The present research design cannot distinguish directly between anticipatory adjustment and a transitory post-pandemic rebound. Nevertheless, because the 2018–2020 coefficients are close to zero and the 2021 deviation is isolated, the transitory-rebound interpretation appears more consistent with the observed pattern. Following the logic of Rambachan and Roth (2023), the baseline estimate should therefore be interpreted cautiously and may represent an upper bound on the sanctions-related component of the post-2022 differential. Consistent with this interpretation, the sensitivity analysis in Table 8 shows that the negative differential remains robust to limited, but not moderate, departures from parallel trends.

This 2021-specific caveat should be considered alongside the broader identification limitations of the study. The tightening of sanctions coincided with the war in Ukraine, energy-supply disruptions, rising food and commodity prices, refugee inflows, and uneven post-pandemic recovery. Because the treatment group largely coincides with Europe, the estimated differential cannot fully separate the sanctions regime from Europe-specific shocks during the same period. Moreover, the control group comprises countries from other regions with different economic structures, and the fixed-effects design cannot fully reproduce a European counterfactual. These concerns are mitigated, but not eliminated, by the robustness checks: excluding 2021, excluding the 2020–2021 pandemic years, and changing the control-group composition leave the negative differential intact. Examining synthetic comparison groups remains a natural direction for further research. Future work could also assess the generalizability of the findings by extending the analysis to a broader set of macroeconomic and structural variables and to a larger or updated country sample as more post-2022 data become available. The estimated differential should therefore be interpreted as a post-2022 growth differential associated with the sanctions episode rather than as a precise net causal effect of sanctions alone.

The findings suggest that sanctions policy should be evaluated not only in terms of its geopolitical objectives and effects on target states but also in terms of the adjustment burden faced by sanctioning economies. The evidence is strongest for highly open economies, while greater pre-sanctions export dependence is also associated with a more negative post-2022 growth change within the sanctioning group. Economic resilience, trade diversification, and the capacity to redirect established economic relationships should therefore be considered alongside the geopolitical objectives of sanctions.

6. Conclusions

This study examined whether the 2022 tightening of sanctions against Russia and Belarus was associated with real GDP growth in European sanctioning countries. Us-

ing a difference-in-differences framework, the results indicate that real GDP growth in sanctioning countries was lower relative to the control group during the post-2022 period (2022–2024). The baseline estimate indicates a negative post-2022 growth differential of approximately 1.47 percentage points. The estimated coefficients remain negative across all reported robustness specifications, although statistical precision declines in some alternative-control-group models. The sample-restriction estimates range from −0.96 to −1.62 percentage points, while the group-specific trend specification yields a larger negative differential. In light of the 2021 pre-treatment signal discussed in Section 4.1, the baseline estimate should be interpreted cautiously and may represent an upper bound on the sanctions-related component of the estimated differential.

The heterogeneity analysis shows that this growth differential was not uniform across countries. Trade openness displays a robust negative heterogeneity pattern in both the restricted and fully interacted specifications. Within sanctioning countries, greater pre-2022 export dependence on Russian and Belarusian markets is associated with a more negative growth change. However, the corresponding gradient difference relative to the control group cannot be reliably established because of limited common support. The country-level figures reported in this article are evaluations of a pooled model rather than separate country-specific regressions. No statistically significant heterogeneity is identified with respect to energy vulnerability. Overall, the heterogeneity analysis provides partial support for H2.

From a macroeconomic policy perspective, the results indicate that sanctions policy should be evaluated not only in terms of geopolitical objectives but also in relation to potential adjustment pressures in sanctioning economies. In highly open economies, short-run economic resilience to external shocks may be constrained by prior dependence on specific trade markets, the costs of redirecting trade flows, and limited capacity to absorb such shocks.

These conclusions should be interpreted in light of the study's limitations. The diagnostics yield mixed results: the 2018–2020 coefficients are jointly statistically insignificant, whereas the 2021 coefficient is positive and statistically significant. The formal sensitivity analysis in Section 4.1 indicates that the negative differential remains robust to limited, but not moderate, departures from parallel trends. The estimated growth differential should therefore be understood within the broader context of the sanctions period, which also included wartime, energy-market, and post-pandemic shocks. Even with these qualifications, the findings indicate that the sanctions period was associated with uneven adjustment pressures across sanctioning economies. These potential adjustment pressures should be considered alongside the geopolitical objectives of sanctions.

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Abbreviations

The following abbreviations are used in this manuscript:

CFSP	Common Foreign and Security Policy
CI	Confidence interval
COVID-19	Coronavirus disease 2019
DiD	Difference-in-differences
EU	European Union
FDI	Foreign direct investment
G7	Group of Seven
GCF	Gross capital formation
GDP	Gross domestic product
HS	Harmonized System
IMF	International Monetary Fund
LNG	Liquefied natural gas
SE	Standard error
SPFS	System for Transfer of Financial Messages (Russian financial messaging system)
SWIFT	Society for Worldwide Interbank Financial Telecommunication
UK	United Kingdom
UN	United Nations
USD	United States dollar

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