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Dependencies and Their Weaponization: The Economic Impact of Trade Sanctions

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Abstract

Do trade dependencies leave countries vulnerable to geoeconomic coercion? We study the economic costs of trade sanctions from the 1960s to the present. In contrast to previous studies we identify the specific traded goods affected by the imposed trade sanctions, and use this to develop a measure of the “intensity” of these sanctions. While a majority of sanction policies have been enacted against an economically trivial share of a country’s trade, larger sanctions do appear to come with economic costs. Specifically, if we consider sanctions imposed between 1962 and 2020 targeting trade equivalent to at least 0.1 percent of GDP, we find that sanctioning trade worth one percentage point of a country’s GDP leads to a GDP loss of 1-2 percentage points in the 1-3 years following the sanction. We also find that sanctions of similar intensities appear more costly for lower-income countries, and countries whose trade is highly concentrated among a small number of trading partners. Low income and developing countries thus appear most vulnerable to trade sanctions.

Keywords: Trade sanctions, Trade dependencies, Geoeconomics

JEL classification: F13, F14, F41, F51

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1 Introduction

To what extent do trade dependencies leave countries vulnerable to geoeconomic coercion? Does diversifying one’s trading partners provide meaningful insurance against such threats? Are all trade sanctions the same? Since Russia’s invasion of Ukraine in 2022, policymakers have been increasingly concerned with such questions. This paper attempts to quantify the impact of trade sanctions using bilateral, product-level trade data and evidence from more than a half-century of sanctions.

We contribute to the existing literature in several ways. First, existing empirical analyses of the impact of economic sanctions study these sanctions as binary events ([Eichengreen et al., 2023](#); [Bianchi and Sosa-Padilla, 2023](#); [Ghomi, 2022](#); [Larch et al., 2022](#); [Gutmann et al., 2023](#); [Larch et al., 2024](#); [Felbermayr et al., 2025](#))¹. We add to this literature by identifying the specific product-level bilateral trade flows that are targeted by trade sanctions; this allows us more accurately to measure the severity of different trade sanction policies. Specifically, we measure a sanction’s severity as the share of sanctioned trade in a country’s GDP. We find that a majority of historical sanctions considered in the literature target trade flows corresponding to only a trivial share of the sanctioned country’s GDP. Indeed, in several cases they target goods that are not traded at all between the sanctioning and sanctioned countries. For these sanctions, we are unable to detect an impact on the sanctioned country’s economic outcomes. Above a threshold “intensity,” however, sanctions appear to have an impact: sanctioning trade worth one percentage point of a country’s GDP leads to 1-2 percentage points in lost GDP in the 1-3 years following the sanction. To our knowledge, our paper is the first to measure trade sanction intensity based on product-level, bilateral trade data, and to use this to estimate the macroeconomic impact of sanctions.

Second, a parallel literature uses quantitative trade models to estimate the actual and potential impact of sanctions in specific cases ([Haidar, 2017](#); [Hernández and Leone, 2019](#); [Bachmann et al., 2022](#); [Flach et al., 2024](#); [Hausmann et al., 2024](#)). Such exercises typically rely on general equilibrium trade models with multiple countries, multiple industries, and/or input-output linkages across industries ([Caliendo and Parro, 2014](#); [Costinot and Rodríguez-Clare, 2014](#); [Baqaei and Farhi, 2019, 2024](#)). Many of these studies estimate losses in real GDP due to recent historical sanction cases – for instance, those imposed on Iran, Venezuela, and Russia.² Our approach is a

¹For a literature review, see [Morgan et al. \(2023\)](#), [Felbermayr et al. \(2021\)](#), [van Bergeijk \(2021\)](#). Historians and political scientists have also studied the effects of historical sanctions; their research typically asks whether sanctions are effective in achieving their declared political aims, and most often focuses on case studies ([Miller, 2014](#); [Ghomi, 2022](#); [Mulder, 2022](#); [Hufbauer et al., 2009](#); [Gutmann et al., 2020](#)).

²See for example [Haidar \(2017\)](#) for Iran or [Hernández and Leone \(2019\)](#) for Venezuela. [Bachmann et al. \(2022\)](#)

complementary empirical one, based on a panel of sanction events. In section 4.3, we compare predictions from our approach to those from these quantitative models.

Third, we compile a dataset of product-level sanctions from 1962 to 2020. We start from the “Global Sanctions Data Base” (GSDB) (Felbermayr et al., 2020), which has been used in several recent studies (Gutmann et al., 2024; Kwon et al., 2024; Larch et al., 2024; Kwon et al., 2022). Using historical policy documents, to the extent possible we then identify the specific goods to which each trade sanction in the GSDB applied.

Because countries are sanctioned for reasons that may be related to their economic conditions, there is a potential endogeneity issue when studying the economic costs of sanctions. Recent efforts to address this issue include reducing the control sample (Neuenkirch and Neumeier, 2015) and employing a nearest neighbor matching approach (Neuenkirch and Neumeier, 2016). Kwon et al. (2022) count the number of binary sanctions imposed by senders in a given year to account for different levels of sender aggressiveness, and use this as an instrument. In our view, the main threat to identification is that sanctions often coincide with other events such as war that may come with economic costs. To mitigate this concern, we first include controls for ex-ante geopolitical conditions - including whether a country is involved in a military conflict or experiencing political instability.³ Second, we match sanctioned countries to control countries that are comparable across observable variables, and implement an estimator that allows us to estimate placebo coefficients for differential “pretrends” between our sanctioned and control countries.

Previous studies that are closely related to ours are Kwon et al. (2022), and Gutmann et al. (2023), who use an event study design and find quantitatively significant impacts of sanctions on GDP.⁴ Our main contribution is to determine the specific goods under sanction, in order to get a sense of the relative importance of these sanctions. This gives us a more precise sense of their economic significance, and allows us to distinguish between sanctions of greater and lesser intensity.

Ultimately, we find that sanctions inflict costs that depend on their intensity. This is in line with previous work showing that the average effects of sanctions depend crucially on the type of sanctions considered, their duration, objectives, and sender types (Felbermayr et al., 2025;

among others uses quantitative trade models to simulate different trade policy scenarios after the inception of the Russian-Ukraine War in 2022.

³We define and classify military conflicts using data from the Uppsala Conflict Data Program (Gleditsch et al., 2002; Davies et al., 2025), and classify political instability using the Polity5 database (Marshall and Gurr, 2020).

⁴Gutmann et al. (2023) find that during a sanction episode, the GDP per capita of an average target country decreases by 2.8 percent within the initial two years. This decline persists even three years after the sanctions are lifted.

Bista and Sheridan, 2025). For the many economically small sanctions that affect less than 0.1% of a country’s GDP in trade, we find no detectable effect. For those sanctions above this threshold, we find that sanctioning 1% of GDP worth of trade causes 1-2 percentage points of lost GDP in the 1-3 years following the sanction.

In addition, we also find that trade vulnerabilities can render sanctions more costly. Sanctions of the same intensity appear more costly for 1) lower-income countries and 2) countries whose trade is highly concentrated with a small number of partners.

The remainder of the paper is structured as follows. Section 2 introduces our sanction data and provides descriptive evidence. Section 3 gives our formal empirical analysis, and section 4 studies the vulnerabilities that render sanctions more potent. Section 5 concludes.

2 Sanction policies and intensities

This section describes the sanction policies we consider, how we define treated observations, and how we construct our measure of sanction intensity – our main explanatory variable.

2.1 Trade sanction policies, 1962-2020

As a starting point we use the fourth edition of the Global Sanctions Database (GSDB) (Feldmayr et al., 2020; Yalcin et al., 2025), which provides information on 1,547 sanction policies, including arms embargoes, travel restrictions, and military, financial, and trade sanctions between 1950 and 2023. Observations in the GSDB are “policies.” One policy can cover multiple countries, and multiple policies can simultaneously be applied to the same country. As a result, we will transform this list of sanction policies into a dataset with one observation per sanctioned country per year. In this section, we describe how we move from observing sanction *policies* that are defined in binary terms to constructing one continuous observation *per sanctioned country per year* indicating not only the presence of a sanction, but its intensity.

We first define the set of policies we consider. Out of the 1,547 sanction policies in the GSDB, we restrict our attention to the database’s 588 trade sanction policies. These sanction policies are explicit trade sanctions targeting the sanctioned country’s imports and/or exports.⁵

Throughout the paper, we follow a convention in the literature that refers to trade sanctions

⁵That is, we do not consider tariffs or other conventional trade policy measures as “sanctions.” In particular, US tariffs and other conventional trade policy measures imposed since 2018 are frequently viewed as constituting sanctions. We will not adopt this view for two reasons: first, to maintain a consistent definition throughout our sample, including the long period in which it arguably does not make sense to consider tariffs as economic sanctions; and second since their frequent use falls mostly beyond our sample period regardless. (We only consider sanction onsets up to 2020, in order to observe economic outcomes over a five-year horizon.)

from the perspective of the country imposing the sanction. An “import sanction” thus refers to a policy in which the sanctioning country stops importing from the sanctioned country, thereby affecting the sanctioned country’s *exports*. Similarly, an “export sanction” refers to a policy in which a sanctioning country stops exporting to the sanctioned country, affecting the sanctioned country’s imports. Finally, a “complete” sanction is one that explicitly targets *all* bilateral trade with the sanctioned country, while a “partial” sanction targets only some goods.

Because bilateral product-level trade data from UN Comtrade ([United Nation, 2025](#)) only begin in 1962, and because we need these data to construct our trade intensity measure, we use the 415 trade sanction policies that begin during our sample period of 1962 to 2020.⁶ Note that these trade sanction policies, as coded in the GSDB, already exclude sanctions that are pure arms embargoes. From these 415 trade sanction policies, we exclude sanctions that are not official state policies. These include consumer boycotts (one case); trade restrictions imposed for health, food-safety or animal-welfare reasons, such as the beef import bans that followed the mad cow disease outbreaks in Europe (14 cases); one case of aid conditionality with no trade instrument; and one set of non-state entity designations (17 policies in all). This leaves 398 policies. In addition, we drop four policies from the GSDB when it appears that the listed target was not in fact embargoed based on our review of policy documents (the United Kingdom in the 1973 Arab oil embargo; Turkey in 1974, against which the United States enacted only an arms embargo; and Canada in 1979, which the Arab League only threatened to boycott), or when the sanctioning country was not yet a sovereign state (Lithuania’s 1990 embargo of the Soviet Union). This leaves us with 394 valid trade sanction policies.

We will estimate the impact of export and import sanctions separately, as sanctioning different trade flows may have different effects, and these sanctions may operate through different transmission channels ([Itskhoki and Ribakova, 2024](#)). Table 1 presents an overview of the 394 sanction policies we consider, splitting sanctions by their direction. Out of the 394 trade sanction policies we consider, there are ten complete export, 14 complete import, 134 partial export, and 70 partial import sanctions. In addition, 53 policies were simultaneous complete export and import sanctions, three were simultaneously complete export and partial import sanctions, one was a simultaneous partial export and complete import sanction, and 109 were bi-directional

⁶Our measure of sanction intensity requires observing trade at least one year before a sanction starts. However, the only sanctions that begin in 1962 are complete trade sanctions against Cuba; we complement our data with Cuba’s complete bilateral trade flows in 1961 from the “trahist” database described below, allowing us to start our sample period in 1962. We choose to end our sample of sanction policies in 2020, in order to study economic performance in a five-year window after the sanction. Note that when we impose restrictions for identification below, we will end up dropping all of the sanctions from 2017-2020, and so the sanctions that enter our main analyses only cover up to 2016. We flag this again when it comes up below.

partial sanctions. In table 1, the average bilateral trade sanction policy lasts for 7.6 years, with a median length of six years.

Table 1: Trade sanction policies, 1962-2020

	Total number of export & import sanctions	Sanction duration in years		Number of sender countries		Number of observed target sectors (3-digit SITC)					
						Export sanctions on a country's imports			Import sanctions on a country's exports		
						N	Mean	Median	N	Mean	Median
Full sample	394	7.6	6	17.4	1	225	34.0	4	157	31.4	10
By decade											
1962–1969	24	13.7	12	22.3	1	12	73.5	82	13	68.0	34
1970–1979	44	6.5	2	8.5	1	38	26.1	3	10	30.2	15.5
1980–1989	50	7.1	6	6.7	1	28	48.1	9	30	49.8	39
1990–1999	74	6.2	3	33.6	1	45	50.8	5	27	39.3	16
2000–2009	62	9.0	8	28.0	1	40	34.5	2	23	23.5	3
2010–2020	140	7.2	6.5	9.9	1	62	12.3	4	54	12.0	3.5

Source: Felbermayr et al. (2020). Here, the unit of observation is a policy, which may involve multiple sanctioning countries and may apply to multiple sanctioned countries. See text.

2.2 Trade sanction intensity

Since our purpose is to measure the intensity of sanctions, and see how this matters for their effectiveness, we need to move beyond simple binary indicators of trade sanction policies. Our measure of the “intensity” of a trade sanction takes into account the importance to the sanctioned country of all sanctioned trade. We will compute the following measure:

$$\widetilde{SANC}_{at}^d = \frac{\sum_{b \in \mathcal{B}_{at}^d} \sum_{k \in \mathcal{K}_{at}^{bd}} x_{at-1}^{bkd}}{GDP_{a,t-1}} \quad (1)$$

$\widetilde{SANC}_{at}^d$ represents the intensity of sanctions in direction d (where d can be either an import or an export sanction) faced by sanctioned country a in year t , over all sanctioned goods and sanctioning partners (where goods are indexed by k and partners by b). The set $\mathcal{B}_{at}^d$ includes all countries that sanction country a at time t in direction d . For each sanctioning country b , the set $\mathcal{K}_{at}^{bd}$ denotes the set of goods that country b is sanctioning against country a in direction d in year t . Then, the flow x_{at-1}^{bkd} is the bilateral trade flow in commodity k , between countries a and b , in direction d , in the year before the sanction, $t - 1$. x_{at-1}^{bkd} is thus the value of pre-existing trade between a and b , in a commodity which will become sanctioned at time t . By summing over all such goods and all such partners, we get the total value of country a ’s trade (in direction d) in time $t - 1$ that will become sanctioned in year t . Taking the share of this

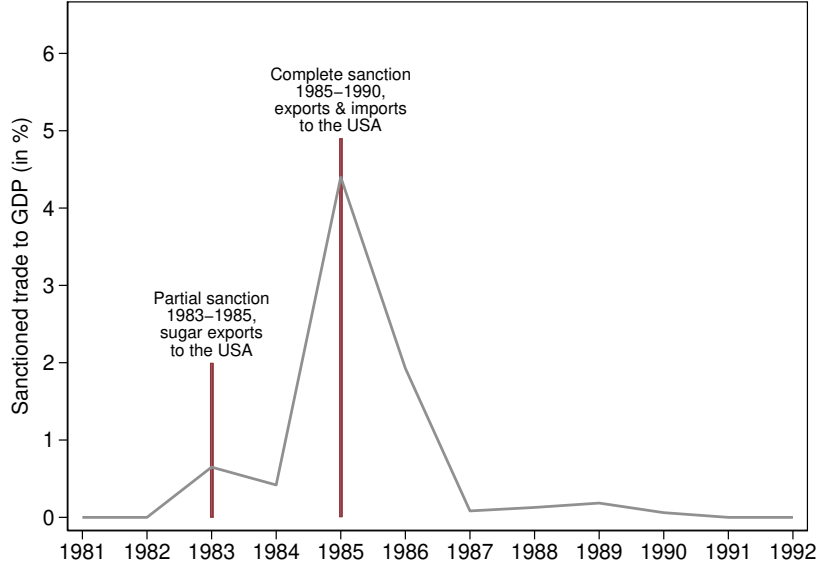
trade in sanctioned country a 's GDP gives us a measure of how important the sanctioned trade is to country a .

Figure 1 illustrates an example of our sanction intensity measure. It plots the path of sanctions enacted by the US against Nicaragua in the 1980s, as the US government tried to remove the communist Sandinista party from power. After the Sandinistas expelled three US diplomats in 1983, Ronald Reagan's administration retaliated by reducing the quota on US imports of sugar from Nicaragua by 90%. Nicaraguan sugar exports to the US totaled \$21.4 million in 1982, the year before the sanction, amounting to about 0.65% of Nicaragua's GDP; they fell to \$6.7 million by 1984.⁷ Ignoring a GATT ruling that the sanctions were illegal, in 1985 the White House invoked emergency national security powers to bypass Congress and enact a full trade embargo against Nicaragua (Leogrande, 1996). As Figure 1 shows, this produces a spike in our sanction intensity measure: gross trade with Nicaragua totaled \$160.3 million in 1984, which was about 4.4% of Nicaragua's GDP. Total trade fell to \$69.5 million in 1985 as the sanction began to take effect, and finally fell all the way to \$3.0 million in 1986. In figure 1, this corresponds to the intensities of the sanction in 1985, 1986, and 1987. The US finally lifted sanctions in 1990.

Figure 1 also helps motivate our empirical design, which we will discuss in section 3. Our baseline design will be to analyze the effect of the initial onsets of a sanction of a certain intensity – here, the initial sanction enacted in 1983, with an intensity of about 0.65% – including any future intensification of the sanction as part of the treatment effect. In appendix F we also study the effects of these intensifications themselves – in this case, asking about the additional effect of the increase from 0.65% to 4.4% in 1985 – under a more restrictive set of identifying assumptions.

⁷In the notation of (1), for this example $\mathcal{B}_{\text{NIC},1983}^{\text{imp}} = \{\text{US}\}$ and $\mathcal{K}_{\text{NIC},1983}^{\text{US,imp}} = \{\text{sugar}\}$.

Figure 1: US trade sanctions against Nicaragua, 1983-1990



Note: This figure shows our “sanction intensity” measure over the course of sanctions enacted by the US against Nicaragua in the 1980s. The US sanctioned about 0.65% of Nicaragua’s GDP worth in trade beginning in 1983, when it reduced imports of sugar from Nicaragua. It then enacted a more intense complete sanction worth about 4.4% of GDP in 1985.

2.3 Sanction onsets and observed sanction intensities and onsets

As noted earlier, one sanction policy can cover several goods and countries, and multiple policies can apply to the same sanctioned country. To construct our sanction intensity measure in practice, we need to split up and recombine the policies presented in table 1 into a total sanction intensity for each sanctioned country over all policies, commodities, and sanctioners. Our first step will thus be to determine how many sanctioned *country-years* our 394 *policies* actually apply to.

Our full list of 394 sanction policies correspond to 1,665 sanctioned country-years between 1962 and 2023. As we discuss below, our baseline strategy will be to consider a country as “treated” by a sanction only at the “clean onset” of an entirely new sanction: that is, when a country faces a trade sanction after having faced *no* trade sanction in any of the three preceding years.⁸ Many of the 1,665 sanctioned-country-by-year observations are continuations of existing sanctions. There are just 181 sanction *onsets*, of which 113 are the start of an export sanction and 133 are the start of an import sanction (65 are the simultaneous onset of sanctions in both directions).

How “intense” were each of these onsets? To answer, we need to know which commodities were

⁸We discuss the econometric rationale for this restriction in section 3

sanctioned in all of the policies underlying an onset ($\mathcal{K}_{at}^{bd}$ from (1)) as well as the importance of trade in these sanctioned commodities for the sanctioned country (x_{at-1}^{bkd}). We start by determining $\mathcal{K}_{at}^{bd}$, the set of goods to which each sanction policy applies. This information is not included in the GSDB. “Complete” sanction policies apply to every traded good by definition. To determine the goods affected by a given “partial” sanction policy, we examine historical news sources and policy documents. Note that our measure of sanction intensity focuses on a policy action. If a product is targeted by a sanction policy, we code it accordingly. Our results should thus be interpreted as the effects of *enacting* a sanction of a certainty intensity, which will *inter alia* depend on how well the intended policy is enforced. We assign to each targeted good the closest-matching SITC code, at the highest level of precision possible.⁹

Figure 2 shows an example of the type of document we consult. It is an excerpt from the White House announcement from May 10th, 1983 that we use to code the first US sanction against Nicaragua, discussed above (Figure 1). The announcement cut Nicaragua’s quota for sugar exports to the United States from 58,800 to 6,000 short tons. We therefore code this policy as an import sanction on a single good, sugar (3-digit SITC code 061, “Sugars, molasses and honey”).¹⁰

Figure 2: Example of a policy document used to identify sanctioned goods



Source: Ronald Reagan Presidential Library & Museum, “Announcement of Revised United States Sugar Import Quotas for Nicaragua, Honduras, Costa Rica, and El Salvador,” 10 May 1983 (excerpt), <https://www.reaganlibrary.gov/archives/speech/announcement-revised-united-states-sugar-import-quotas-nicaragua-honduras-costa>.

Ultimately, we are able to determine the goods to which a sanction policy applies for 310 of the 394 policies. This includes the 77 “complete” sanctions, for which the relevant goods are trivially known.¹¹ Of the 317 partial sanction policies, we are able to determine the goods

⁹This is discussed further in appendix C.

¹⁰This case does reveal an imperfection in our measure: here, we treat the sanction as affecting all trade in sugar, when in reality the US did still allow a small amount of sugar imports. We view this as a simplification that is unlikely to matter quantitatively.

¹¹“Complete” and “partial” follow the GSDB’s own classification of each policy. However, our reading of the policy documents shows that six of the policies the GSDB lists as partial did cover total bilateral trade. four

affected in 233 of them. Table 2 shows the categories of goods most often under sanction across these 310 policies. Export sanctions are most often enacted to prevent countries from importing war-related goods (arms, ammunition, explosives, and nuclear materials); they also frequently seek to prevent countries from importing energy-related goods or other machinery. Import sanctions, in contrast, often embargo countries’ exports of raw commodities, such as agricultural products, crude materials, energy-related goods, or diamonds and gold.

Table 2: Sanctioned goods across 310 policies

	All	Export sanctions (target’s imports)	Import sanctions (target’s exports)
Policies	310	245	181
Complete (all goods)	83	71	77
Partial, goods identified	227	166	95
Partial, goods not identified	—	8	9
<i>Partial sanctions with identified goods that cover (% of partial):</i>			
Arms, ammunition and explosives (891, 593)	91 (40)	89 (54)	14 (15)
Crude oil and petroleum products (33)	58 (26)	38 (23)	22 (23)
Machinery and transport equipment (7 excl. 718.7)	46 (20)	42 (25)	10 (11)
Other manufactured goods (6 excl. 67, 667.2; 8 excl. 891)	41 (18)	27 (16)	17 (18)
Nuclear materials and reactors (286, 525, 718.7)	39 (17)	39 (23)	5 (5)
Food and live animals (0)	39 (17)	7 (4)	33 (35)
Coal, natural gas and electricity (32, 34, 35)	36 (16)	25 (15)	12 (13)
Crude materials, oils and fats (2 excl. 286; 4)	36 (16)	10 (6)	27 (28)
Iron and steel (67)	31 (14)	18 (11)	16 (17)
Chemicals (5 excl. 525, 593)	24 (11)	23 (14)	4 (4)
Diamonds and gold (667.2, 97)	24 (11)	8 (5)	19 (20)
Beverages and tobacco (1)	12 (5)	6 (4)	7 (7)
Other (9 excl. 97)	4 (2)	4 (2)	1 (1)

Notes: This table shows the 310 trade sanction policies of Table 1 for which sanctioned goods could be determined in at least one direction. A policy that sanctions trade in both directions counts in both direction columns. In the first column, a policy is complete if every direction it sanctions is “complete,” and partial otherwise. We show the number of partial sanctions that name at least one good in a given row (with the % share of total partial sanctions in the column in parentheses). Since a policy can cover several categories, the rows do not sum to the total. We list the relevant SITC Rev. 4 codes in the far left hand column in parentheses: a sanctioned good counts toward the category of its longest matching code prefix, and a one-digit code counts toward every category in that section.

We next aggregate the contributions of each policy, sanctioner, and commodity, into the total sanction intensity each country faces. This requires observing the relevant bilateral flows, x_{at-1}^{bkd} .

We use bilateral, product-level trade data from the UN Comtrade database (United Nation, 2025). Any given trade flow - for example, from country a to country b - is in principle observable twice: as either exports from a , or imports into b . Where possible, our default is to measure all trade flows as exports, rather than as imports. We do this because exports are reported as “Free on Board” (FOB) meaning that exporters report the value of goods loaded at the border. Importers in contrast usually report the value of imports including cost, insurance,

sanctions against Rhodesia in 1966–67, which began as partial sanctions and were later extended to complete bilateral trade, and the two US sanctions on Kuwait in 1990 and on Yugoslavia in 1999, which covered complete bilateral trade from the start. Table 2 counts these as complete, and so lists 83 complete and 227 partial policies.

and freight (CIF). In our preferred measure of intensity, we measure an affected trade flow as an export. When trade exists but is not reported in this direction, we fall back on measuring it as an import.¹²

After observing all relevant bilateral flows and computing $\widetilde{SANC}_{at}^d$ where possible, we end up with 122 onsets (i.e., out of our original 181; for 48 onsets the sanctioned goods of at least one policy are not identified, and for another 11 the trade data do not measure the sanctioned flows). We also drop onsets whose real GDP data carry a data-quality flag in any year from three years before to five years after the onset,¹³ leaving 118 onsets. We drop onsets whose GDP growth lies outside the 1st to 99th percentile at any horizon, which leaves us a final sample of 108 clean onsets: 50 of these are onsets of export sanctions and 74 are import sanctions (16 onsets are in both directions at once).¹⁴ Table B1 in the appendix documents each of these steps. While this is a relatively small sample, it is the best that can be done given the evidence we have at present regarding the goods that were sanctioned when partial sanctions were imposed, and the pre-existing trade flows involving these goods. Despite the small sample size, we will show below that there are useful lessons that can be learned with the data at our disposal.

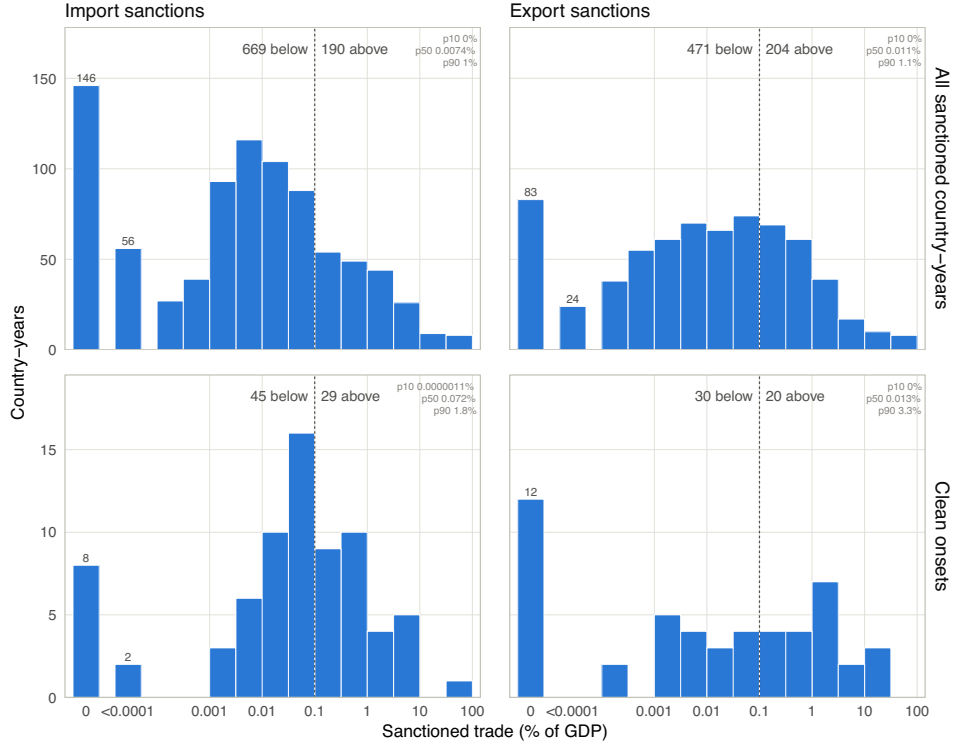
Figure C1 in the appendix shows the full set of unique onset histories in our data – it clarifies that we see onsets across most years from 1962-2016. Table C1 in the appendix shows the breakdown of sanctioned commodities across these 108 onsets, following the same format as table 2. The takeaway is similar to that from the latter table. In our data, export sanctions usually seek to prevent countries from importing critical energy and manufactures. Import sanctions usually seek to stop countries from exporting their raw materials and commodities. While our analysis will only cover those sanction onsets for which we have adequate data, these onsets thus cover a wide temporal range (figure C1) and cover a range of often sanctioned goods (comparing table C1 and table 2).

¹²In appendix figure F1, we attempt to correct for the discrepancy between CIF and FOB in these import measures, by estimating the average wedge between the two in observations that report flows with both methods; our results are robust to this correction. In figure F1 we also explore alternative ways of measuring bilateral trade and show that our results are robust to alternative choices.

¹³We describe GDP data in more detail in appendix A.

¹⁴This final sample of retained sanction onsets covers 1962-2016.

Figure 3: Distribution of sanction intensities



Note: Sanctioned trade in $t - 1$ as a share of GDP in $t - 1$ (log scale), for import sanctions (left) and export sanctions (right). Top row: every sanctioned country-year with an observed intensity, counted separately for each direction of sanction the country faces. Bottom row: the initial intensity of the 108 clean onsets in our estimation sample. The first bar counts sanctions with zero intensity and the second those below 0.0001% of GDP; the dashed line marks our threshold of 0.1% of GDP.

Figure 3 shows the resulting distribution of sanction intensities. The top panel shows the intensities of sanctions across all sanctioned country-years in our data. The bottom panel considers only the initial intensity of “clean onsets” of sanctions. The takeaway from both is similar: many of the sanctions in the GSDB are very minor. Around 74% of sanctions affect less than 0.1% of GDP. A large mass of sanctions (309 of the 1,534 sanctioned country-years) affect less than 0.0001% of GDP, and 229 of these are sanctions in which countries enact sanction policies covering goods currently not traded (i.e., sanctions with zero intensity). In contrast, a minority of sanctions do affect a portion of trade that is potentially more economically meaningful.

Treating sanctions as binary occurrences collapses this heterogeneity in sanction intensity, which makes it more difficult to get an empirical sense of their importance.

3 Empirical strategy and results

3.1 Impact of bilateral trade sanctions on bilateral trade volumes

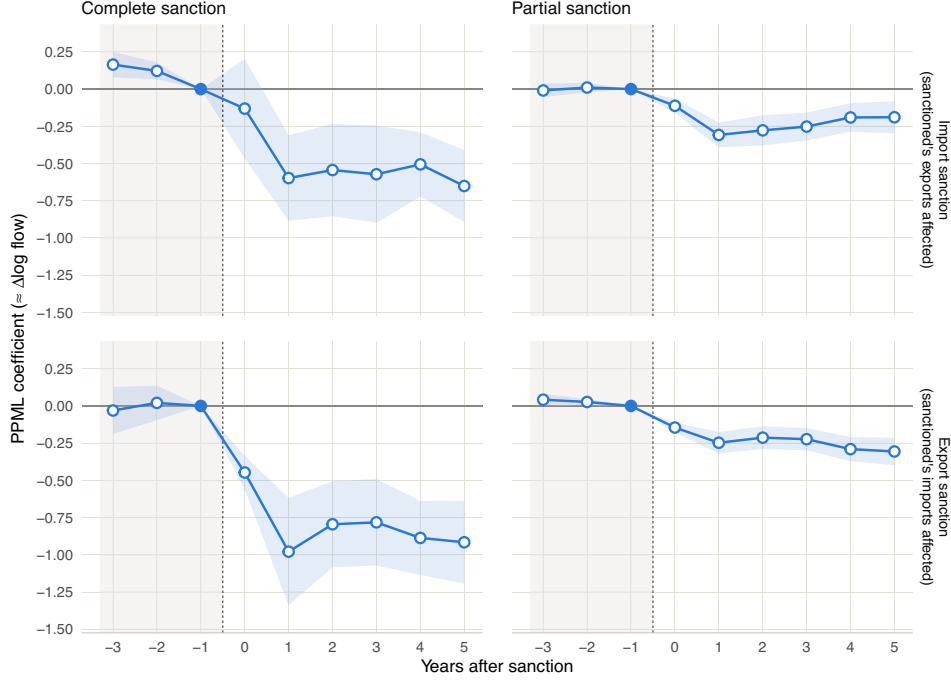
Our aim is to study whether more intense enacted sanctions have larger economic effects. As a first step, we confirm that enacted sanction policies do indeed have their intended effect on trade: that is, that bilateral trade between the sanctioning and sanctioned countries falls following a sanction policy.

To test for this effect, equation (2) measures the extent to which bilateral trade flows fall following the imposition of a bilateral sanction. We study the average path of trade between country a and country b , after b imposes a sanction on the trade flow, via the following equation:

$$\Delta_h \log \left(\text{trade volume}_{a,t+h}^{bd} \right) = \alpha_t^{dch} + \beta^{dch} \text{SANC}_{abt}^{dc} + \mathbf{X}_{at} \boldsymbol{\Gamma}^{dch} + \varepsilon_{abt}^h \quad (2)$$

The outcome variable is the change in the log nominal volume of the flow between countries a and b , between year $t - 1$ and year $t + h$, in direction d . The variable SANC_{abt}^{dc} is a dummy variable for the clean onset of a bilateral sanction in direction d (import or export), of type c (partial or complete). This equation is thus estimated separately for each (d, c) pair: for export sanctions, in which b stops exporting to a , trade flow $\text{flow}_{a,t+h}^{b,exp}$ is trade from b to a , and vice versa for import sanctions. Our coefficients of interest are β^{dch} , which track the average path of sanctioned trade following the sanction's imposition. Note that we estimate the above equation for every value between $h = -3, \dots, 5$; the negative values of h are “placebo” coefficients that test the hypothesis that bilateral trade between sanctioned and unsanctioned countries were on different trajectories on average before the sanction.

Figure 4: Direct effect of bilateral sanctions on bilateral trade



Note: Average change in a bilateral trade flow after it falls under sanction. This figure plots the estimates of β^h in equation (2), obtained with Poisson Pseudo-Maximum Likelihood. The y-axis is the approximate log-change in value of the affected flow: that is, of trade from a sanctioned to a sanctioning country, following an import sanction (top); or in trade from a sanctioning to a sanctioned country, following an export sanction (bottom). The average complete sanction spell length is 8.5 years, and the average partial sanction spell length is 7.4 years (policy spells, as in Table 1). 90% confidence intervals; standard errors clustered by directed dyad.

Figure 4 plots the coefficients $\hat{\beta}^h$ from estimating equation (2), using a Poisson Pseudo-Maximum Likelihood (PPML) estimator.¹⁵ We find the expected direct effect. Trade does indeed fall following the imposition of a sanction. Following a complete import or export sanction, the relevant bilateral trade flow between the two countries falls by between 0.5 and one log point (left panel). In our sample, this corresponds on average to a reduction in the nominal value of bilateral flows of about 51%. These estimates are somewhat smaller than previous results. For instance, [Felbermayr et al. \(2025\)](#) find a 70% reduction of trade after complete sanctions. When the sanction is only ‘partial,’ trade following a sanction falls by 0.25 log points on average, which corresponds approximately to a 22% reduction. There is however some evidence that bilateral trade before a complete import sanction may already be declining prior to the sanction’s imposition: the pre-period coefficients are slightly positive and significant. In the other three panels in figure 4, pre-trends appear flat prior to the imposition of a sanction.

¹⁵ As noted in [Head and Mayer \(2014\)](#), PPML allows us to include observations with zero bilateral trade which are prevalent, especially following complete sanctions, without needing to transform the data or introduce a separate extensive margin estimation.

Finally, note that we can adjust equation (2) to produce an equation that would come out of a gravity model. Appendix D discusses the nuances involved with this approach and figure D1 in the appendix shows the resulting estimates.

3.2 Discussion of sanction endogeneity

To test for the economic costs of sanctions, we use a Local Projections-based difference in differences strategy, designed to capture the economic costs of trade sanctions on GDP per capita. In this design, a country is “treated” if they experienced no sanction for three consecutive years, followed by a new sanction. We call this a “clean onset” of a sanction. The treatment dosage is precisely the intensity of this new sanction.

A primary concern with this design is that sanctions may be systematically correlated with other events that are responsible for economic slowdowns. Specifically, we worry that sanctions are often passed against countries at war. If wars systematically coincide with sanctions, we would risk attributing the economic damages inflicted by war to the costs of the sanction. Our best effort to address this possibility, discussed in more detail when we present our estimation, will be to add controls for countries that are at war in a window prior to the imposition of the sanction.¹⁶ Relatedly, we add controls for “political turmoil” in the three years preceding the sanction, using the regime codes of the *POLITY* measure from the Polity database (Marshall and Gurr, 2020) (see appendix A).

3.3 Discussion of treatment and control groups

Treatment group We form treatment groups for import and export sanctions separately. In each case, the treatment group consists of countries that experience the import and/or export sanction in year t , after having experienced no sanction in *either* direction in years $t - 3$ through $t - 1$. We impose this restriction to mitigate the possibility that a previous sanction is continuing to affect economic conditions. The assumption we are making here is that the effects of a sanction subside within three years of its lifting. Dube et al. (2025) show that under such an assumption, the estimator we implement below can identify treatment effects.¹⁷

¹⁶There is a remaining concern that sanctions may work by triggering wars, in which case our design would attribute the effect of the sanction to the war that it triggers. In our design, this would be part of the treatment effect; this is admittedly not the mechanism we hope to study. We cannot rule out this possibility, and cannot control for wars occurring after a sanction’s imposition because conditioning on an ex-post outcome would introduce selection bias. Despite this, we show in appendix figure F2 that our results below are robust to adding controls for wars that occur in the five years following the sanction onset, and discuss the interpretation of this result.

¹⁷Admitting only “clean onsets” and “clean controls” (defined below) into our sample ensures that we are indeed only comparing countries whose sanction intensity changed in year t , with countries whose sanction intensity remained unchanged throughout the entire relevant horizon. This avoids contaminating our results

Defined this way, a country that experiences a clean onset of both an import and an export sanction at the same time will end up in both of our treatment groups. If we are testing for the effect of an import sanction, we thus need to take into account whether or not an export sanction was also imposed. Below, we show our strategy to control for this potential simultaneity. In total, we observe 74 clean onsets of import sanctions and 50 of export sanctions; 16 of these are onsets in which both begin simultaneously.

Control group We next build a matched control group. The sample of admissible controls for year t includes all countries that faced no sanction between years $t - 3$ and $t + 5$, and for which we observe non quality-flagged outcome variables over this entire horizon $t - 3$ to $t + 5$.

We use these countries to build a matched control sample. We use matching with two goals in mind. First, it allows us to construct a control group that is more comparable to our treated countries on some observable dimensions. This renders the parallel trends assumption more plausible. Second, matching allows us to include as controls countries that may be less directly affected by rerouted trade due to the sanction. For a country treated in year t , we look at the control group for year t , and consider similarity across three covariates: log real GDP per capita in $t - 1$, the cumulative two-year change in log-real GDP per capita (from $t - 3$ to $t - 1$), and log nominal GDP (in USD) at $t - 1$, to capture overall country size. We also prefer controls that trade little with the sanctioned country, with the goal of reducing the chance that our control group is itself affected by the sanction. We use the Mahalanobis distance to pick the five best controls for each treated observation over these four measures. We explain the details of this procedure in appendix [E](#).

3.4 The impact of country-level trade sanctions on GDP per capita

We now turn to estimating the impact of trade sanctions on GDP per capita. As described in the previous section, we define the “intensity” of a trade sanction as a continuous measure equal to the share in GDP of previously-unsanctioned trade that falls under sanction. Because many sanctions are minor, as shown in figure [3](#), we will test for nonlinearities in their economic effects.

We use local projections that are able to make difference-in-differences comparisons between

with the type of ‘unclean’ difference-in-difference comparisons discussed in many recent papers ([de Chaisemartin and D’Haultfoeuille, 2022](#)). The ability of a local projection specification like equation [3](#) to identify a causal effect is discussed in [Dube et al. \(2025\)](#). Our estimator is not robust to ‘treatment heterogeneity’ in the sense that it will not pick up nonlinearities in treatment (this is the case with any linear regression). To address this, we could define different treatment bins (e.g., low, medium, and high intensity) and estimate a clean discrete LP-DID for each bin, to test for any nonlinearities in the treatment effect. However, our sample is too small for this to give reliable estimates.

sanctioned and unsanctioned countries, to study the effect of sanctions on per-capita growth. We will account for nonlinearities between very small and more economically meaningful sanctions by splitting sanctions into intensity groups. A clean onset of a sanction in direction d against country a in year t belongs to the high-intensity group $g = H$ if $\widetilde{SANC}_{at}^d \geq \kappa$, and to the low-intensity group $g = L$ otherwise; we set $\kappa = 0.1\%$ of GDP. For each group we construct its own matched control sample (as described in section 3.3), and we denote by $\mathcal{S}_g^d$ the group- g onsets together with their matched controls, restricted to observations whose outcome lies within the 1st to 99th percentiles of the group’s distribution at every horizon $h = 0, \dots, 5$. We then jointly estimate the following on the two samples together:

$$\Delta_{t-1}^{t+h} y_i = \sum_{k \in \{L, H\}} \mathbb{1}\{g = k\} \left[\alpha_{k,t}^h + \beta_k^h \widetilde{SANC}_{it}^d + \delta_k^h \widetilde{SANC}_{it}^{-d} + \mathbf{\Gamma}_k^{h'} \mathbf{X}_{i,t-1}^h \right] + \varepsilon_{i,g,t}^h, \quad (i, t, g) \in \mathcal{S}_L^d \cup \mathcal{S}_H^d \quad (3)$$

where an observation is a country-year-group triple (i, t, g) (a country-year that serves as a control in both groups enters once for each). The outcome variable $\Delta_{t-1}^{t+h} y_i$ is the cumulative change in log real GDP per capita of country i from year $t - 1$ to year $t + h$, and $\widetilde{SANC}_{it}^d$ is the intensity of sanctions in direction d faced by country i in year t , which is zero for every control. We estimate equation (3) by weighted least squares (weights as in appendix E), in one regression for each pair (h, d) : that is, for each horizon $h = -3, \dots, 5$, where $h = 0$ is the sanction year and $h = -1$ is the reference year, and for each direction $d \in \{\text{exp}, \text{imp}\}$. Newey-West standard errors are computed on the stacked sample, with country $\times$ group as the panel unit.

We control for any simultaneous sanctions that the country may have been facing in the opposite direction by including $\widetilde{SANC}_{at}^{-d}$. As a result, β_g^h captures the marginal effect of sanctioning an additional amount of GDP worth of trade, holding fixed all other sanctions. We include year fixed effects $\alpha_{g,t}^h$.¹⁸ In $\mathbf{X}_{a,t-1}$ we include two lags of the log of real GDP per capita, as well as three dummy variables for: whether the country faced a financial sanction; was in “political turmoil” as defined in appendix A; or was at war, where each dummy variable is equal to one if the country faced the given condition in any of the years $t - 3$ to $t - 1$. As Dube et al. (2025) discuss, this amounts to implementing a conditional parallel trends assumption: that countries with similar GDP levels and with similar geopolitical situations (as crudely proxied through our war and policy variables) would have continued with comparable GDP growth, were it not for the sanction.¹⁹ By estimating this equation for $h \leq -1$, we test whether sanctioned

¹⁸Time-invariant country fixed effects are removed because the outcome variable is a difference; the year-fixed effect captures the average h -year growth at time t .

¹⁹Including lags of the outcome variable is unlikely to introduce problematic Nickell bias because of the long time dimension of our data (Alvarez and Arellano, 2003).

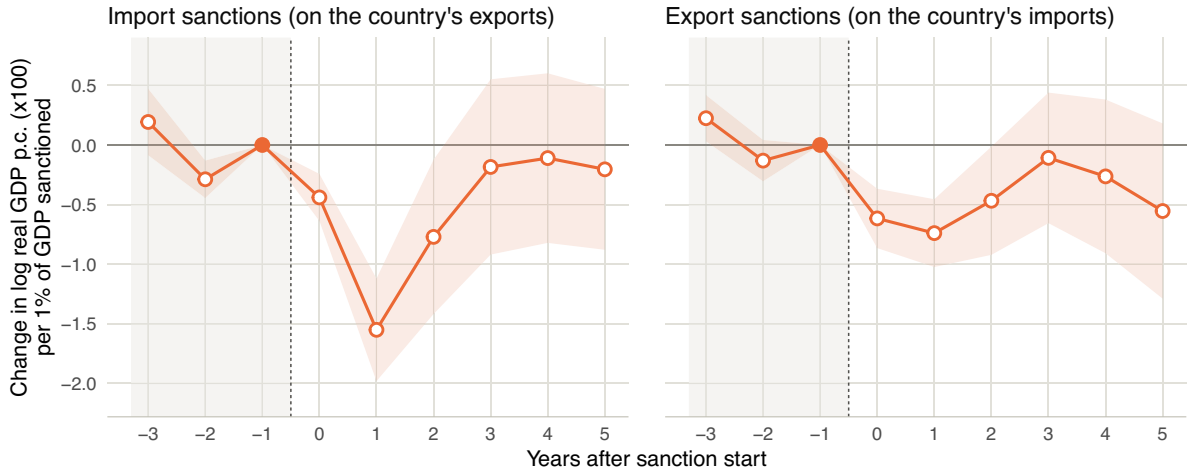
and unsanctioned countries were on differential trends prior to the imposition of the sanction; this provides a heuristic check for whether the parallel trends assumption is plausible. β_g^h thus measures the horizon- h effect of initiating an average sanction above or below the threshold.

Discussion of results

Figure 5 plots our estimates of β_H^h . These reflect the cumulative percentage-point change in GDP per capita for each percentage-point of GDP worth of trade that falls under sanction which we interpret as “elasticity”.

Figure 5 restricts attention to sanctions whose intensities are above 0.1% of GDP. It suggests that for these economically meaningful sanctions, higher-intensity policies come with larger GDP costs. Each sanctioned percentage point of GDP costs somewhere between 0.5 to 1.5 log points in GDP in the following 1-2 years, with some evidence of catch-up in years 3-5. The pre-sanction coefficients move around somewhat, but they do not appear to exhibit an obvious pre-trend. We want to emphasize that these results are somewhat noisy and estimated off only 74 and 50 sanction onsets respectively; at the same time, the data do appear to indicate a relationship between more intense sanctions and lower subsequent growth.

Figure 5: Impact of sanctioned trade to GDP per capita



Note: Estimates of β_H^h from equation (3). Sanction intensity is defined based on the share in GDP of trade in $t - 1$ that becomes sanctioned in t . 90% confidence intervals using Newey-West standard errors; Other details in text.

The impact of sanctions on a target country’s exports (left panel) are potentially more pronounced, which would suggest a deviation from Lerner symmetry. The ability of a sanctioned country to substitute consumption and production intertemporally may lead to different welfare effects of temporary import and export sanctions (Itskhoki and Mukhin, 2023).

The results presented here should be interpreted as the impact of passing a policy *intended* to sanction an additional percentage point of GDP worth in trade. This includes the effects of forces that may reduce the impact of enacted sanctions. For instance, sanctioned countries may compensate through domestic trade (Larch et al., 2024); enacted sanctions may be imperfectly enforceable, and multiple sanctioners may not sufficiently coordinate; third countries may interfere; sanctioned countries may rapidly shift trade to new trading partners and/or unsanctioned products (Biersteker and van Bergeijk, 2015; Petrevski, 2025). In addition, there could be also potential confounds. For example, sanctions may be anticipated by target countries, leading to trade shifts prior to the imposition of sanctions (Itskhoki and Ribakova, 2024).²⁰

As we show in the discussion in appendix F, the relationship in figure 5 does *not* meaningfully appear when looking at sanctions worth less than 0.1% of GDP – indeed, these sanctions are so small so as to render uninformative an estimate of their effects. In 20 cases, they are sanctions where trade in the affected commodity is already nonexistent. It is perhaps unsurprising that we cannot detect an effect of such very small sanctions. But by distinguishing between these sanctions and more economically meaningful onsets above a certain intensity threshold, we can identify an effect of the latter. Our results hold for a variety of thresholds, ranging from 0.05% of GDP to 0.3% of GDP, as Figure F4 in the appendix shows.

4 Heterogeneity in sanction effects on GDP per capita

The literature stresses the heterogeneous impact of sanctions (Felbermayr et al., 2025), and we have already shown that small sanctions have no distinguishable impact on GDP. In this section we group sanctioned countries according to potential vulnerabilities, which might lead to sanctions having a more or less harmful impact. In particular, we consider a country’s overall level of development, as well as measures of their trading partner concentration. We find some suggestive if inconclusive evidence that lower-income countries, and countries that rely on a

²⁰One reason that sanctioning a country’s exports might come with higher costs is as follows. These sanctions reduce foreign demand for those exports, assuming a high substitution elasticity. If this is the case, to remain competitive a target country may lower the cost of production with an accompanied exchange rate depreciation, to offset the increased trade costs. The exchange rate depreciation would induce a reduction of the price at which exports are sold which in turn lowers export revenues and leads to a lower budget constraint. It would also reduce the sanctioned country’s real prices and thus its purchasing power for foreign imports. Faced by a lower budget constraint the sanctioning country will tend to shift demand from foreign imports to domestic products. (Itskhoki and Mukhin, 2023; Itskhoki and Ribakova, 2024). Despite such factors, Figure 5 shows that trade sanctions above a certain intensity do have an economically significant impact on GDP.

Because trade sanctions are most imposed on developing and emerging market economies whose currency is often linked to the US dollar and debt denominated in foreign currency, an exchange rate depreciation would lead to an increase of borrowing costs, thus higher interest rates. This would also make repayment costs of foreign currency debt more expensive which in turn could increase the sovereign default risk, but also adversely impacts the business cycle of such countries.

small number of partners, may be more vulnerable to trade sanctions.

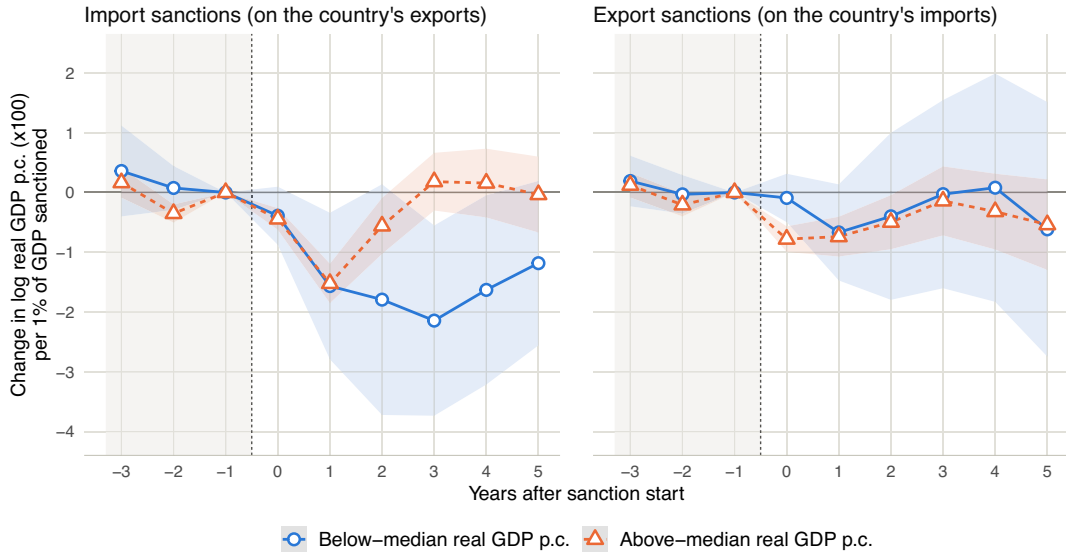
4.1 Vulnerability by GDP per capita

To examine whether lower-income countries are more vulnerable to trade sanctions, we extend equation (3) by interacting sanction intensity with an indicator for whether a sanctioned country is higher-income:

$$\Delta_{t-1}^{t+h} y_a = \alpha_{g,t}^h + \beta_g^h \widetilde{SANC}_{at}^d + \gamma_g^h \widetilde{SANC}_{at}^d R_{a,t-1} + \theta_g^h R_{a,t-1} + \delta_g^h \widetilde{SANC}_{at}^{-d} + \mathbf{\Gamma}_g^{h'} \mathbf{X}_{a,t-1} + \varepsilon_{a,g,t}^h \quad (4)$$

The dummy variable $R_{a,t-1}$ equals one if country a 's real GDP per capita in $t-1$ is above the median across all countries in our data in that year, and zero otherwise. The samples, matched controls, and all other terms are exactly as in equation (3). Because $R_{a,t-1}$ is also defined for the control countries, we include its level $\theta_g^h R_{a,t-1}$ alongside the interaction. The effect of a sanction worth one percent of GDP is then β_g^h for countries at or below the median income and $\beta_g^h + \gamma_g^h$ for countries above it.

Figure 6: Heterogeneity by country income



Note: Estimates of β_H^h (countries at or below the median) and $\beta_H^h + \gamma_H^h$ (countries above the median) from equation (4), for sanctions above 0.1% of GDP. The median of real GDP per capita in $t-1$ is taken over all countries in our data in each year. Sample, matched controls and weights as in figure 5. 90% confidence intervals using Newey-West standard errors.

Figure 6 plots β_H^h and $\beta_H^h + \gamma_H^h$ for the high-intensity group. It shows some evidence that lower-income countries may be somewhat more vulnerable to import sanctions, although with only 40 total “clean onsets,” estimates are somewhat noisy. Specifically, an extra percentage point of GDP under sanction appears to cost about 1-2 percentage points in lost GDP. In

our data, lower-income countries appear to experience a more persistent decline with a slower recovery following an import sanction. This is imperfect but suggestive evidence that lower-income countries may be less quickly able to substitute for sanctioned trade than higher-income ones.

4.2 Concentration of trading partners

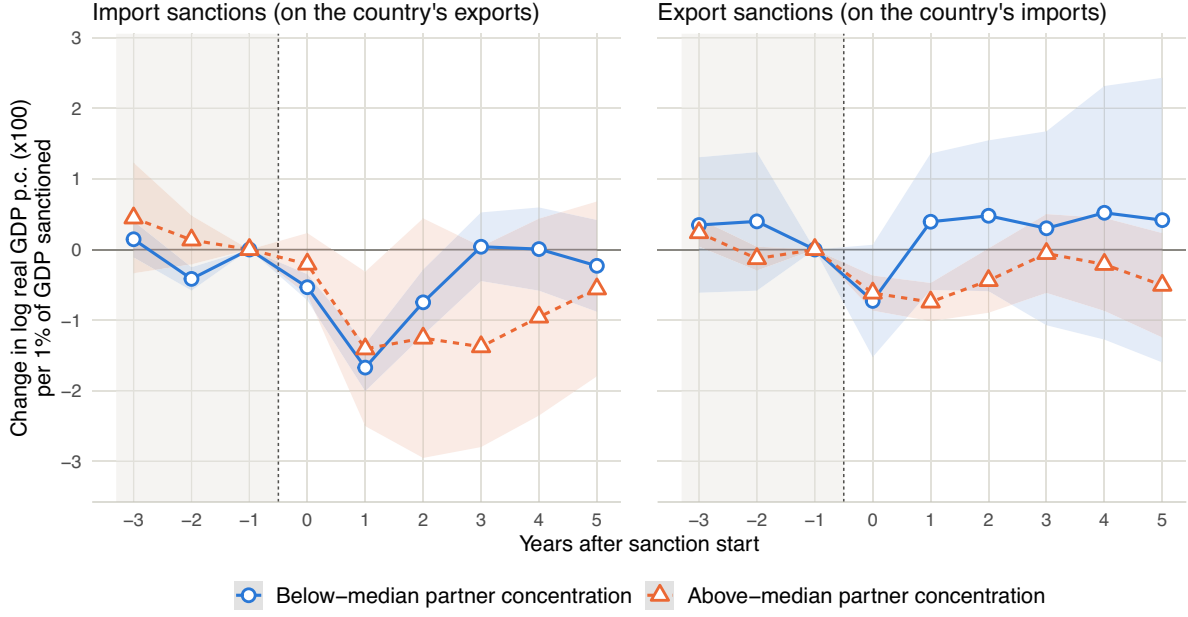
Beyond the overall level of income, sanctions may be especially costly for countries that rely on a small number of trading partners, as these countries may have fewer opportunities to substitute away from partners imposing sanctions.

A standard measure of trade concentration is the Herfindahl-Hirschman index. For a given direction (imports or exports), let $HHI_{at}^d \equiv \sum_{b=1}^N \left(\frac{x_{at}^{bd}}{X_{at}} \right)^2$ be the sum of the squared import (or export) volumes over all of a 's N sanctioning and non-sanctioning trading partners. HHI_{at}^d is thus measured on a scale from 0 to 1, where higher HHI_{at}^d means that country a relies on a smaller number of trading partners for a larger share of its trade.

We define a dummy variable for whether a country's HHI is above or below the median value, and interact this dummy with our sanction intensity measure. That is, we reestimate equation (4), where $R_{a,t-1}^d$ is now an indicator for whether country a 's partner concentration (in the sanctioned direction) is above the sample's median.²¹ Again, the path of β_g^h is the effect of a sanction worth one percent of GDP on countries with below-median HHI, and $\beta_g^h + \gamma_g^h$ is the same effect for countries with above-median HHI.

²¹Note that in contrast to equation (4), this means that $R_{a,t-1}^d$ is indeed direction-specific.

Figure 7: Interaction of sanction intensity with trading partner concentration



Note: This figure shows the response of GDP to sanctions when a sanctioned country's trading partners are more or less concentrated. The y-axis thus measures the percentage-point loss in GDP from sanctioning an additional percentage point worth of GDP in trade, for countries with above- or below-median partner concentration. Partner concentration is measured in $t - 1$ in the sanctioned direction (exports for import sanctions, imports for export sanctions), and the median is taken over all countries in our data in each year. 90% confidence intervals using Newey-West standard errors.

Figure 7 plots these two paths, for sanctions above our intensity threshold ($g = H$). In the point estimates, there is suggestive but imperfect evidence that countries with higher partner concentration experience greater losses in GDP for the same intensity import sanction. Low-HHI countries (blue) appear on average to recover faster, while high-HHI partners (orange) take longer; however, with only 40 sanction onsets worth more than 0.1% of GDP, these estimates are noisy and not statistically significant.

4.3 Countries' vulnerability to sanctions

In a last step, we use our results to present a very rough ranking of countries by their potential vulnerabilities to trade sanctions. Figures 6 and 7 suggest that countries with lower incomes per capita and a higher concentration of trading partners may face greater costs for the same intensity sanction. Based on this, figure 8 gives a very simple presentation of countries, ranked on these two measures in 2024 and split at the median of each (as in figures 6 and 7). Countries plotted in the top left quadrant are those which, according to our interaction estimates, appear more vulnerable to sanctions of the same intensities. Countries are sized according to the share of total exports in GDP (for import sanctions, top) or total imports in GDP (for export

sanctions, bottom). We exclude countries with less than \$10 billion in nominal GDP.

Finally, Table 3 uses our results to generate reduced-form predictions for GDP losses following some major, salient sanction events. We calculate the cumulative percent loss in GDP per capita after five years by applying our $h = 5$ coefficient from Figures 5 6 and 7 to each episode’s observed sanction intensity. This allows us to compare our simple reduced form estimates to estimates that come out of the structural trade models discussed in the introduction. For instance, we can compare our cumulative 5-year loss to the permanent level effects on Russian GDP following the 2014 sanctions, as estimated in Flach et al. (2024). The trade sanctioned in 2014 was worth only 0.06 percent of Russia’s GDP, and our estimates imply a cumulative loss of 0.03 percent of GDP per capita. This is an order of magnitude below the 0.3 percent loss in real income that Flach et al. (2024) estimate for the same episode.

Table 3: Reduced form estimates of historical sanction episodes

Country	Year	Dir.	Intensity (% GDP)	HHI	HHI group	rGDPpc (2021 \$)	GDPpc group	Cum. % GDPpc, $h=5$ (slope b)		
								Fig 5 slope	$\times$ GDPpc group	$\times$ HHI group
Iran	1995	exp	0.303	0.072	low	8,742	above	-0.17 (-0.55)	-0.16 (-0.54)	0.13 (0.42)
Iran	2012	imp	6.859	0.137	high	14,554	above	-1.40 (-0.20)	-0.24 (-0.03)	-3.83 (-0.56)
Iran	2018	exp	0.000 [†]	0.111	high	14,717	above	0.00 (-0.55)	0.00 (-0.54)	0.00 (-0.51)
Iran	2018	imp	1.639	0.129	high	14,717	above	-0.33 (-0.20)	-0.06 (-0.03)	-0.91 (-0.56)
Libya	2011	imp	0.000 [†]	0.253	high	38,662	above	0.00 (-0.20)	0.00 (-0.03)	0.00 (-0.56)
Myanmar	2021	exp	0.001 [†]	0.178	high	7,902	below	0.00 (-0.55)	0.00 (-0.62)	0.00 (-0.51)
Russia	2014	exp	0.062 [†]	0.062	low	34,921	above	-0.03 (-0.55)	-0.03 (-0.54)	0.03 (0.42)
Russia	2022	imp	7.379	0.048	low	37,816	above	-1.51 (-0.20)	-0.26 (-0.03)	-1.70 (-0.23)
Venezuela	2019	imp	6.507	0.217	high	11,692	below	-1.33 (-0.20)	-7.70 (-1.18)	-3.63 (-0.56)

Notes: this table applies the estimated coefficients from figure 5 to salient historical sanction episodes. See text. Items with a [†] are those with a below-threshold sanction, for which we extrapolate the above-threshold coefficient. Unlike our estimation sample, intensities here count every sanction policy in force in the episode’s year, including policies that began before 1962 or after 2020, such as the 2022 measures against Russia. Appendix table G3 lists the episodes we omit because their intensity cannot be measured.

We come closer to replicating existing estimates of the impact of Venezuela’s 2019 oil sanctions.

Figure 8: Countries by HHI and income per capita (2024), as proxies for sanction vulnerability



Note: Countries with nominal GDP of at least \$10 billion in 2024. Horizontal axis: real GDP per capita in 2024 (2021 international dollars, log scale). Vertical axis: Herfindahl-Hirschman index of trading-partner concentration in 2024 (log scale), for exports (top panel, relevant for import sanctions) and for imports (bottom panel, relevant for export sanctions). Dots are sized by exports (top) or imports (bottom) as a share of GDP. Dashed lines mark the medians across the plotted countries.

[Hernández and Leone \(2019\)](#) use a multi-region, multi-sector computable general equilibrium model to simulate a US embargo on Venezuelan oil, similar to the one the United States put in place in January 2019. They model the embargo as a 98 percent cut in Venezuela’s exports of crude and refined petroleum to the United States, together with cuts in US exports of crude oil and other intermediate inputs to Venezuela. Their scenario covers only these trade restrictions, not the financial sanctions imposed from 2017. In their simulation, the embargo alone lowers Venezuelan GDP by 3.2 percent. In our data, the trade sanctioned in 2019 was worth 6.5 percent of Venezuela’s GDP. Our figure 5 would convert this to a 1.33 percentage point loss in GDP over five years; letting the effect vary with income or with partner concentration, as in Figures 6 and 7, gives losses of 7.7 and 3.6 percent respectively, since Venezuela’s exports were concentrated on few partners and its income was below the median. While the range of estimates compatible with our data is thus rather wide, it is consistent with the estimate reported in [Hernández and Leone \(2019\)](#).

5 Conclusion

Trade sanctions inflict costs, but not all sanctions are the same: some target economically meaningful amounts of trade while others do not. The majority of historical sanction policies have impacted only economically trivial quantities of trade, and have had no detectable effects on GDP. However, those sanctions that have been sizable have lowered GDP, and their impact has increased along with their intensity. Measuring not only the intensity of sanctions, but also the concentration of trade, leads to a clearer sense of how costly sanctions are, as well as who has the most to fear from them. Sanctioning trade worth 1% of a country’s GDP may end up costing 1-2 percentage points in lost GDP per capita in the 1-3 years following the sanction, and the losses are greater for lower-income countries and countries with undiversified trading partners.

Appendix

A Macroeconomic variables

GDP and GDP per capita: Penn World Tables, Version 11.0 provided by [Groningen Growth and Development Centre \(2025\)](#) is our baseline GDP measure, which refers to national-accounts based GDP, making it the most comparable across countries. It is reported in 2021 USD. To compute GDP per capita we use GDP and population provided by Penn World Tables.

We then supplement the GDP per capita in order of decreasing priority with GDP per capita data from the Maddison Growth project ([Bolt and Van Zanden, 2024](#)), [World Bank \(2026\)](#), [International Monetary Fund \(2026\)](#) and [United Nations \(2026\)](#). To extend GDP per capita further and backwards in time, we apply the implied growth rates of GDP per capita from the Maddison Growth project to GDP per capita levels in the Penn World Tables. For countries not in the Penn World Tables, we use the full series from the other source

Finally, we add GDP per capita from a couple of other sources for specific countries not included in the above sources. To measure GDP separately in North and South Vietnam, we rely on data provided by [Bassino \(2000\)](#). To measure GDP separately in East and West Germany before reunification, we use data from [Total Economy Database \(2026\)](#).

Trade: All trade values are in current US dollars. Our sanction intensities, equation (1), divide sanctioned trade by nominal GDP. Sanctioned trade is measured with bilateral, product-level trade flows from UN Comtrade ([United Nation, 2025](#)) (SITC Rev. 1 for the years before 2000, Rev. 3 for 2000–2006 and Rev. 4 from 2007), taken from the exporter’s report where it exists and from the importer’s report otherwise (appendix G lists the exceptions). For complete sanctions, which cover all bilateral trade, we use the bilateral total; where Comtrade records no such total, we take it from CEPII’s TRADHIST database ([Fouquin and Hugot, 2016](#)) and, failing that, from the CEPII Gravity database ([Conte et al., 2022](#)). Nominal GDP in current US dollars comes from the UN National Accounts ([United Nations, 2026](#)), extended forward and backward with the growth of the World Bank’s nominal GDP series ([World Bank, 2026](#)); where no UN data exist, we use the World Bank series, extended backward with TRADHIST, and IMF data ([International Monetary Fund, 2026](#)).

The bilateral trade flows in section 3.1 and the trade exposures we use to choose matched controls (appendix E) come from the CEPII Gravity database, which reports each flow as recorded by the exporter and by the importer in Comtrade and in the IMF’s Direction of Trade

Statistics. We measure the flows of section 3.1 with the exporter’s Comtrade report, and trade exposures with the exporter’s report or, where it is missing, the importer’s. The trading-partner concentration indices of section 4.2 are computed from the Gravity database (the average of its Comtrade and IMF records), with missing country-years filled from TRADHIST and then from Comtrade.

Scope of regime authority: We use the Polity5 (Marshall and Gurr, 2020) variable that classifies regimes on an ordinal scale from autocratic to democratic regimes.

Political turmoil: We code a country as being in political turmoil in a year if, in the Polity5 regime (case-format) data (Marshall and Gurr, 2020), it was in a special regime period that year (a foreign interruption, an interregnum - a collapse of central authority - or a transition (*POLITY* codes -66 , -77 and -88); or if a new regime began that year whose *POLITY* score differs by three or more points from that of the regime before it (Polity’s own threshold for a regime change). Countries that Polity5 does not cover (those with fewer than 500,000 inhabitants) are coded as not in turmoil. Our control in equation (3) is a dummy for political turmoil in any of the years $t - 3$ to $t - 1$.

Wars: We identify wars with the UCDP/PRIO Armed Conflict Dataset, version 26.1 (Gleditsch et al., 2002; Davies et al., 2025). The dataset records every armed conflict – a contested incompatibility over government or territory in which armed force is used between two parties, at least one of them the government of a state – and classifies a conflict as a war in each calendar year in which it causes at least 1,000 battle-related deaths (Pettersson, 2025). We code a country as at war in a year if it is a primary party to a conflict of any type (interstate, internal, internationalized internal or extrasystemic) that reaches this threshold in that year. Both the country-level regressions of equation (3) and the bilateral regressions of section 3.1 control for a dummy for whether the sanctioned country was at war in any of the years $t - 3$ to $t - 1$.

B Sample construction

Table B1 documents each step from the sanction policies of the GSDB to the clean onsets in our estimation sample, as described in sections 2.1 and 2.3.

Table B1: Building our estimation sample from the Global Sanctions Database

Restriction	Remaining	Removed
<i>A. Sanction policies</i>		
1 GSDB v4 sanction policies	1,547	
2 Trade sanction policies	588	959
3 Policy begins in 1962–2020	415	173
4 Official trade sanctions only (no consumer boycotts, health, food-safety or animal-welfare bans, aid conditionality or entity designations)	398	17
5 Trade sanction imposed on the listed target by a sovereign sender (appendix G)	394	4
6 Sanctioned goods identified in at least one sanctioned direction of which identified in every sanctioned direction	310 293	84
<i>B. Sanctioned countries and clean onsets (country-years, the 394 policies of step 5)</i>		
7 Sanctioned country-years, 1962–2020 (1,665 in 1962–2023)	1,425	
8 Clean onsets: a trade sanction after three years without one facing an export sanction / an import sanction / both	181 113 / 133 / 65	
9 Sanction intensity observed in every direction faced removed because the goods of a policy are unidentified / the trade data do not measure the flow	122	59 48 / 11
10 Real GDP p.c. observed, with no source break in $t - 3$ to $t + 5$	118	4
11 Inside the 1st–99th percentile band of GDP growth (the estimation sample)	108	10
<i>C. Clean onsets by direction</i>		
	Export sanctions	Import sanctions
Clean onsets facing the direction	113	133
Sanction intensity observed	69	85
Intensity of the other direction observed (zero if none)	62	81
Real GDP p.c. requirements	59	78
Inside the band: the estimation sample	50	74
of which at or above 0.1% of GDP / below	20 / 30	29 / 45
Matched control country-years in the estimation sample	249	254

Notes: Panel A counts GSDB policies (case identifiers); step 4 removes 17 policies that are not geopolitical trade sanctions, and step 5 the four policies whose listed trade sanction was never imposed on the target or whose sender was not a state (appendix G). Panel B counts sanctioned country-years under the step-5 policies. A clean onset is a country-year with a trade sanction in either direction after none in the three preceding years; an onset is kept while it is in the estimation sample of at least one direction. Panel C follows the estimator’s own order of restrictions for each direction; a country facing both directions at its onset enters both columns. The band is computed separately for each direction and intensity group (above and below 0.1% of GDP), over the onsets and their matched controls.

C Coding sanctioned goods

SITC codes give more precise designations via higher numbers of digits. For descriptive statistics, we will aggregated sanctioned goods cateerogires, but because more specific goods are sometimes sanctioned, we collect our data for higher-digit SITC codes where necessary. In addition, different “revisions” of the SITC system have taken effect in different years. Revision 1 was in use from 1961–1975; rev. 2 from 1975–1985; rev. 3 from 1985–2007; and rev. 4 since 2007. We assign goods using revision 4, and then convert this into the code system used at the time of reported trade (i.e., in order to pull relevant trade data) using a crosswalk.

Table C1: Sanctioned goods across sample of clean onsets

	Export sanctions (target's imports)			Import sanctions (target's exports)		
	All	$\geq 0.1\%$ of GDP	$< 0.1\%$	All	$\geq 0.1\%$ of GDP	$< 0.1\%$
Clean onsets	50	20	30	74	29	45
Complete (all goods)	15	12	3	28	13	15
Partial	35	8	27	46	16	30
<i>Partial onsets that cover (% of partial):</i>						
Arms, ammunition and explosives (891, 593)	13 (37)	0	13 (48)	0	0	0
Crude oil and petroleum products (33)	10 (29)	6 (75)	4 (15)	0	0	0
Machinery and transport equipment (7 excl. 718.7)	2 (6)	0	2 (7)	0	0	0
Other manufactured goods (6 excl. 67, 667.2; 8 excl. 891)	1 (3)	1 (12)	0	1 (2)	0	1 (3)
Nuclear materials and reactors (286, 525, 718.7)	7 (20)	0	7 (26)	0	0	0
Food and live animals (0)	0	0	0	42 (91)	13 (81)	29 (97)
Coal, natural gas and electricity (32, 34, 35)	4 (11)	3 (38)	1 (4)	0	0	0
Crude materials, oils and fats (2 excl. 286; 4)	0	0	0	3 (7)	1 (6)	2 (7)
Iron and steel (67)	1 (3)	0	1 (4)	0	0	0
Chemicals (5 excl. 525, 593)	2 (6)	1 (12)	1 (4)	0	0	0
Diamonds and gold (667.2, 97)	0	0	0	1 (2)	0	1 (3)
Beverages and tobacco (1)	0	0	0	4 (9)	4 (25)	0

Notes: This table shows the 108 clean onsets in the estimation sample of Figure 5, by the direction of the sanction and by whether its intensity is at least 0.1% of the sanctioned country's GDP. Of these, 16 are onsets in both directions and count in both. An onset is "complete" if any policy in force against the target in that direction in the onset year is a complete sanction; the goods of a "partial" onset are the union of the goods across all applicable policies. In the second half of the table, we show the number of partial sanction onsets that sanction at least one good in a row's category (with the % share of total partial onsets in the column in parentheses). As in Table 2, the rows do not sum to the total. We list the relevant SITC Rev. 4 codes in the far left hand column in parentheses: a sanctioned good counts toward the category of its longest matching code prefix, and a one-digit code counts toward every category in that section.

D Appendix to section 3.1

In section 3.1, we track the average path of bilateral trade following a sanction. This is meant primarily to describe and validate the sanction policies in our data. An alternative is to estimate

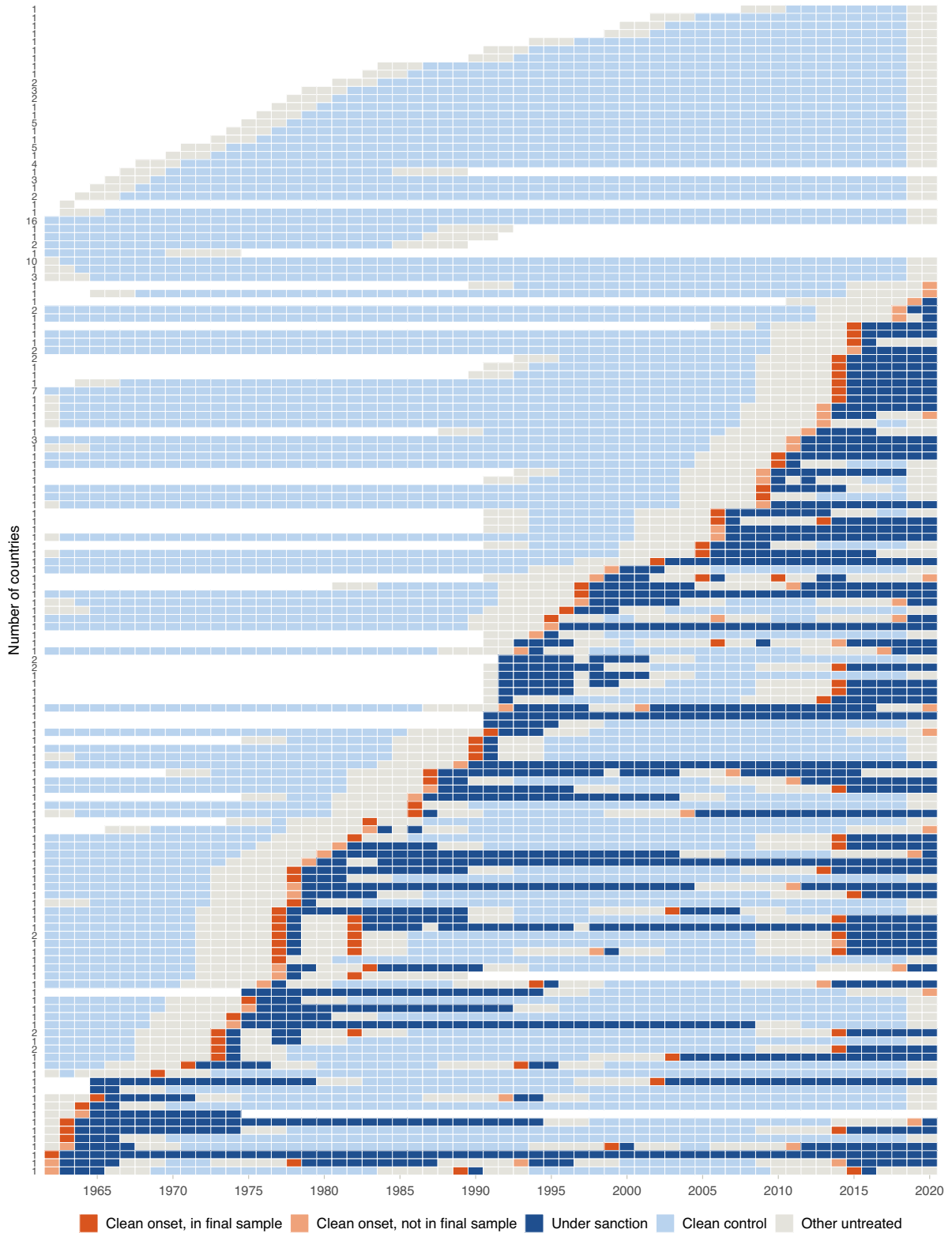
$$\Delta_h \log \left(\text{trade volume}_{a,t+h}^{bd} \right) = \alpha_{at}^{dh} + \alpha_{bt}^{dh} + \beta^h \text{SANC}_{abt}^d + \gamma^h \text{SANC}_{abt}^{-d} + \varepsilon_{abt}^h \quad (5)$$

As discussed by Head and Mayer (2014), a wide variety of international trade models yield predictions about nominal bilateral flows that can be estimated by structural gravity equations similar to equation (5). When working with panel data, structural gravity models estimate nominal bilateral trade flows as a function of importer and exporter time-varying fixed effects as well as bilateral fixed effects. Since equation (5) is the difference of two gravity equations, the bilateral fixed effects, capturing time-invariant determinants of trade between two countries a and b , drop out, while the time-varying exporter and importer fixed effects remain in the estimation.²² Sanctions are the key time-varying trade cost that equation (5) seeks to investigate.

²²To see this, observe that our equation can be written directly as the difference between two gravity equations:

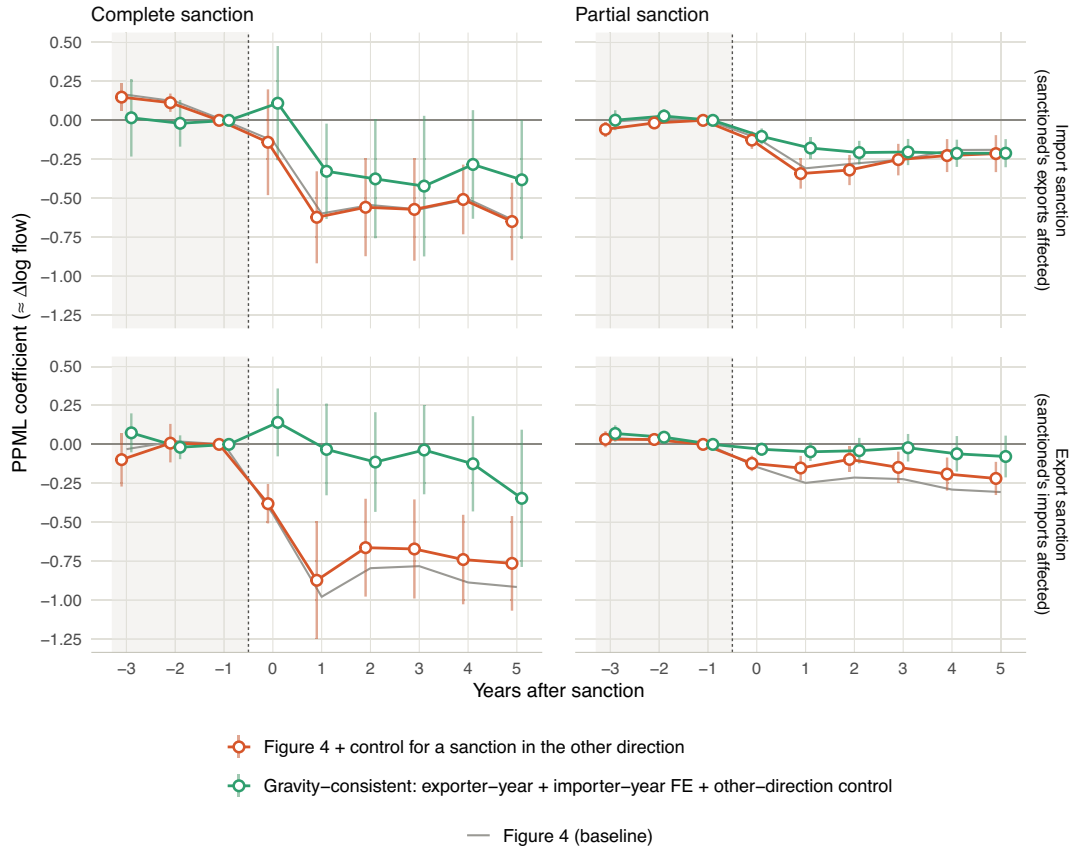
$$x_{ab,t+h} - x_{ab,t-1} = \underbrace{(\gamma_{at+h} - \gamma_{at-1})}_{\alpha_{at}^h} + \underbrace{(\gamma_{bt+h} - \gamma_{bt-1})}_{\alpha_{bt}^h} + \underbrace{(\beta_1 \tau_{abt+h} - \beta_2 \tau_{abt-1})}_{\beta^h \text{SANC}_{abt}^d + \gamma^h \text{SANC}_{abt}^{-d}} + \underbrace{(\nu_{abt+h} - \nu_{abt-1})}_{\varepsilon_{abt}^h} \quad (6)$$

Figure C1: Unique treatment histories, 1962-2020



Note: This figure shows unique treatment histories in our data. A “treatment history” is a specific sequence of treatment statuses from 1962-2020, where “status” can be either a clean onset; a sanction in place; a clean control; or “other untreated” (i.e., from missing data, or in the five years preceding or the three years following a sanction, in which a country is untreated but is also disqualified from being used as a control). Dark orange marks the 108 clean onsets in our final sample; light orange marks clean onsets that do not enter it because their sanction intensity is not observed, their GDP data are missing or flagged, or their GDP growth lies outside the 1st to 99th percentile (table B1).

Figure D1: Bilateral trade following sanctions: alternative specifications



Note: Alternatives to figure 4, on the same sample and estimated with PPML. Orange: the specification of figure 4 with an additional dummy for a sanction onset by the same sanctioning country in the opposite direction in the same year. Green: equation (5), with exporter-year and importer-year fixed effects and the same dummy. Grey line: figure 4. 90% confidence intervals; standard errors clustered by directed dyad.

Finally, we cluster standard errors by directed dyad to account for serial correlation of the error terms. $\hat{\beta}^h$ thus gives an estimate of the causal effect of sanctions on bilateral trade, in a way that is consistent with structural gravity. Figure D1 plots these estimates and compares them to those in the main text's figure 4.

Equation (5) identifies β^h only from differences between sanctioning and non-sanctioning partners of the same target in the same year, since it includes target-by-year fixed effects. For the most meaningful complete sanctions, sanctioning countries account for a very large share of the sanctioned country's trading partners. This means that in practice, the comparison group may be very small. As a result, while equation (5) is theoretically appealing, our data do not provide enough of the variation it relies on; as such, we prefer to use the reduced-form specification (2) to study the path of bilateral trade following sanctions.

Any time-invariant trade frictions are taken out by differencing.

E Appendix to section 3.3

First, we describe the way in which we match controls while prioritizing those that are less directly involved with the sanctioned country. Let X_{at} denote the vector of three covariates for country a (log real GDP per capita, its two-year growth, and log nominal GDP). Let e_{ab} denote country b 's total trade (exports plus imports) with a in $t - 1$ as a percentage of b 's nominal GDP in $t - 1$. We transform it to $z_{ab} = \log_{10} \max(e_{ab}, 10^{-4}) + 4$, which is zero when b 's trade with a is below one millionth of b 's GDP and rises by one with every tenfold increase. Then, total “distance” between a and b is

$$d(a, b)^2 = (X_a - X_b)' \hat{\Sigma}^{-1} (X_a - X_b) + z_{ab}^2 / \hat{\sigma}_z^2,$$

where $\hat{\Sigma}$ is the covariance matrix of X over all onsets and clean controls in the group, pooled across years, and $\hat{\sigma}_z^2$ is the variance of z_{ab} over all of the group's onset-candidate pairs with reported trade. The first term is the Mahalanobis distance on the three covariates (Rubin, 1980; Abadie and Imbens, 2006). The second adds trade exposure as a fourth dimension whose target value is zero: a candidate one standard deviation more exposed to the sanctioned country is penalized as much as one that is one standard deviation further away in income, growth or size. Where the bilateral flow is not reported, z_{ab} is set to the median over a 's candidates. Each onset is matched to the five candidates with the smallest $d(a, b)$, with replacement. In the regressions, each onset is given a weight of one and each control a weight equal to the number of onsets it is matched to, divided by five, so that every onset and its matched set carry equal weight.

While this matching strategy is employed less often than methods such as inverse propensity weighting, we think it is better suited to this setting for two reasons. First and primarily, matching explicitly allows us to select controls that are less likely to themselves be affected by the sanction. Second, sanction onsets are difficult to predict in our data, meaning that countries have similar propensities and IPW is not that helpful in choosing similar controls; we prefer our more transparent matching strategy.

As a suggestive test of whether control countries' macroeconomies may themselves be affected by sanction onsets, figure E1 plots event-study coefficients for these countries' per capita GDP across sanction onsets. If sanctions had obvious effects on these unsanctioned controls via spillovers, this might show up as a trend break at sanction onsets. No such trend break is visible in figure E1. While spillovers may still be occurring, we view this as additional encouragement that they are perhaps not large enough to bias our results.

Figure E1: Event study coefficients of matched controls' real GDP per capita



△ Figure 5's matched controls ○ All other clean controls of the same year

Note: Average change in log real GDP per capita (x100) since $t - 1$, around the onsets above 0.1% of GDP in figure 5, for the five matched controls of each onset (orange) and for all other clean controls of the same year (blue). Each onset is weighted equally. 90% confidence intervals using standard errors clustered by country.

F Alternate specifications and robustness

Below-threshold sanctions These sanctions have such small intensities that estimates of their effects are unreliable. Below-threshold sanctions have doses which average 0.03% of GDP; so the slope divides growth differences by dose differences of a few hundredths of a percent. Any small difference will thus get magnified into a very large coefficient. To demonstrate this, we can reassign doses at random among below-threshold onsets; all of these reshuffled samples reproduce a very similar estimate, demonstrating that the difference in intensity among these observations is not predictive of changes in GDP. At the same time, the same exercise applied to the above-threshold onsets eliminates the above-threshold slope (Figure F3), suggesting that the above-threshold slope is in fact informative. Against a per-unit effect the size of β_H , the below-threshold design has 5% power and would exaggerate any significant estimate 24–1,000-fold (Gelman and Carlin, 2014).

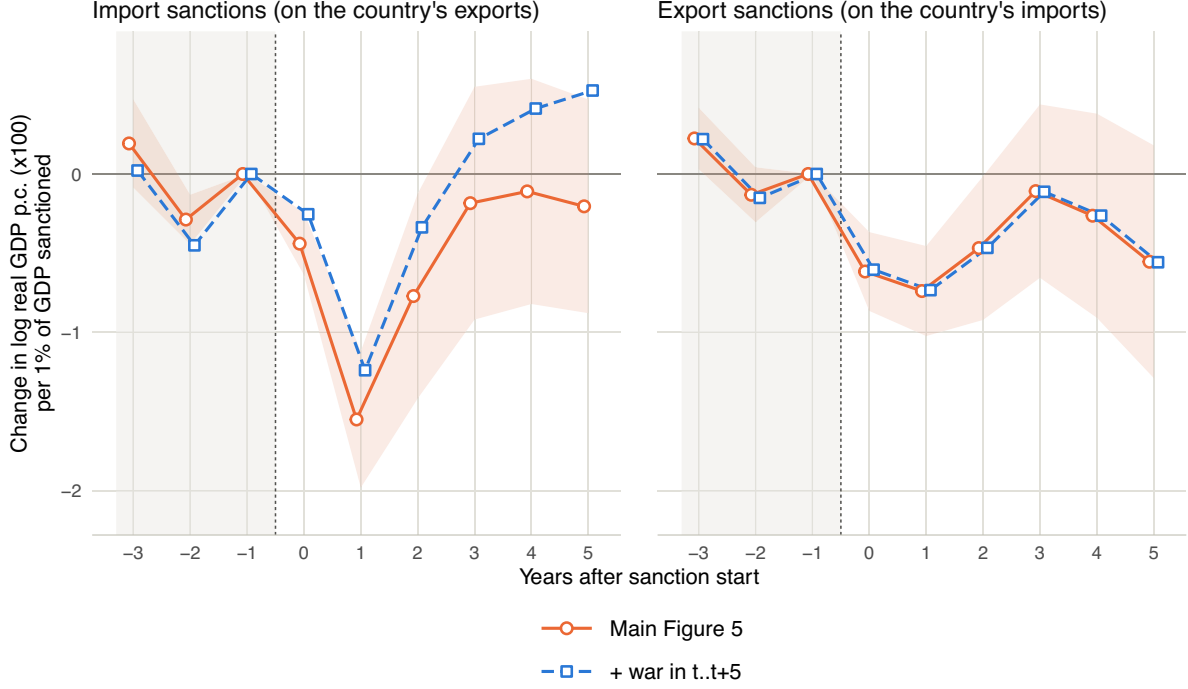
Threshold definition In the main text we picked $\kappa = 0.1\%$ of GDP as our threshold for “economically meaningful” sanctions. This is admittedly arbitrary; in figure F4 we show that the results in figure 5 are robust to varying the threshold from 0.05% of GDP up to 0.3% of GDP. These figures do show that are results are somewhat sensitive to excluding certain observations, however: at the most and least extreme thresholds, estimates at horizons 2 through 5 change

Figure F1: Alternative measures of the sanctioned trade flow



Note: Estimates of β_H^h as in figure 5, with sanction intensities computed from different reports of the same trade flows. Panel (a): whose report measures the sanctioned flow; our baseline uses the exporter's report, with the importer's report as a fallback. Panel (b): importers' reports converted from CIF to FOB values, using estimated (BACI-style) margins or a flat 10% margin. The shaded band is the 90% confidence interval of the baseline (Newey-West).

Figure F2: Controlling for wars after the sanction onset



Note: Estimates of β_H^h from equation (3) with an additional control for whether the sanctioned country is at war in any of the years t to $t+5$ (blue), compared with figure 5 (orange, with its 90% confidence interval using Newey-West standard errors).

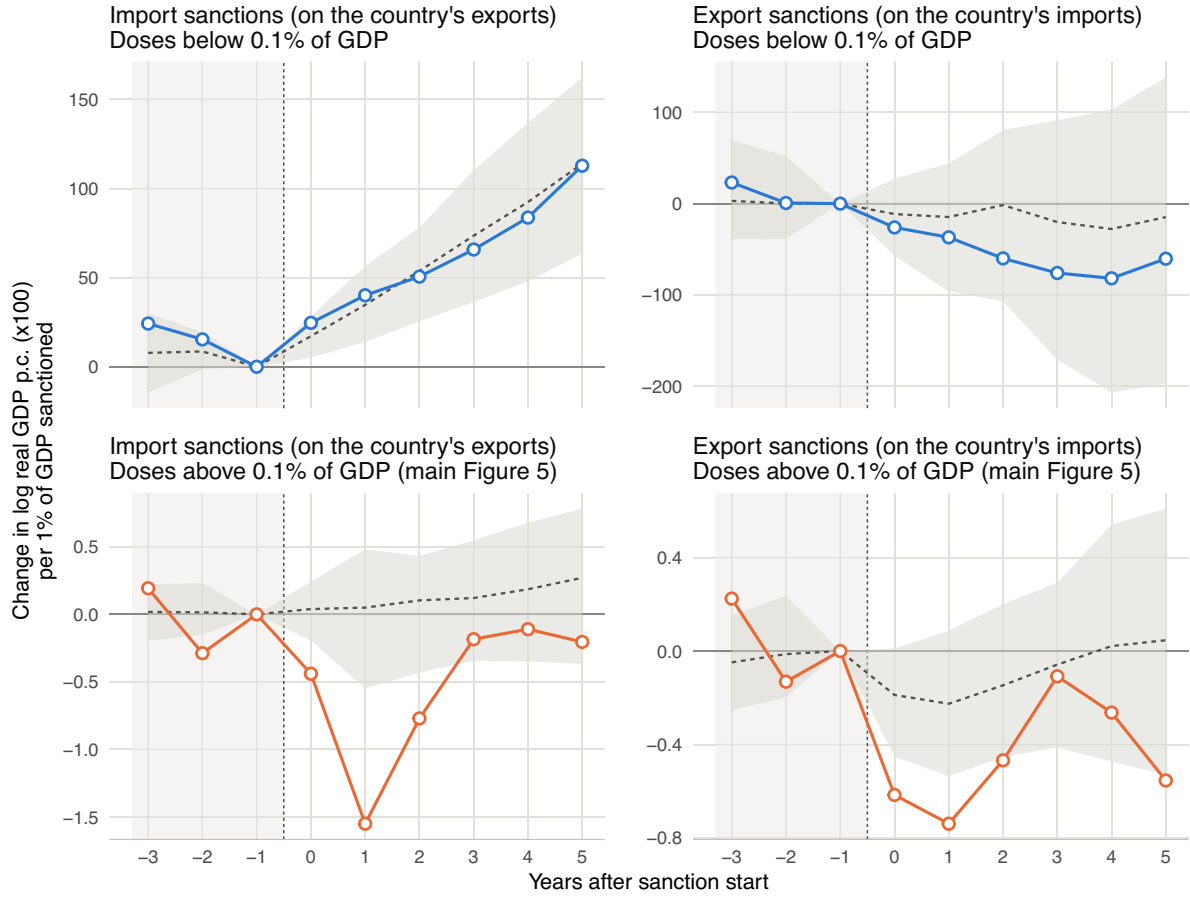
because of the inclusion/exclusion of additional observations.

Intensification of existing sanctions In section 2, we observe that many sanction policies are intensifications of existing sanctions. If we are willing to accept additional assumptions about the effect of sanctions, we can also examine the effect of intensifying an existing sanction. To do so, we estimate standard local projections on *every* country-year in our panel, without restricting to the “clean onsets” of our main design. We estimate the following equation:

$$\begin{aligned} \Delta_{t-1}^{t+h} y_i = & \sum_{d \in \{\text{imp}, \text{exp}\}} \left[\beta_d^h \widetilde{SANC}_{it}^d + \sum_{j=1}^3 \gamma_{d,j}^h \widetilde{SANC}_{i,t-j}^d \right] \\ & + \sum_{j=1}^3 \rho_j^h y_{i,t-j} + \sum_{j=1}^3 \left[\omega_j^h W_{i,t-j} + \phi_j^h F_{i,t-j} \right] + \mu_i^h + \tau_t^h + \varepsilon_{it}^h, \end{aligned}$$

Outcomes and $\widetilde{SANC}_{it}^d$ are as in the main text. Here, we include three lags of prior sanction intensity; this equation thus covers new and continuing sanctions. Alongside the three lags of sanction intensity, we also include three lags of log real GDP per capita, indicators for being at war ($W_{i,t-j}$) and for facing a financial sanction ($F_{i,t-j}$). We also include country and year fixed effects. Identification here relies not only the standard exogeneity assumption we’ve been using

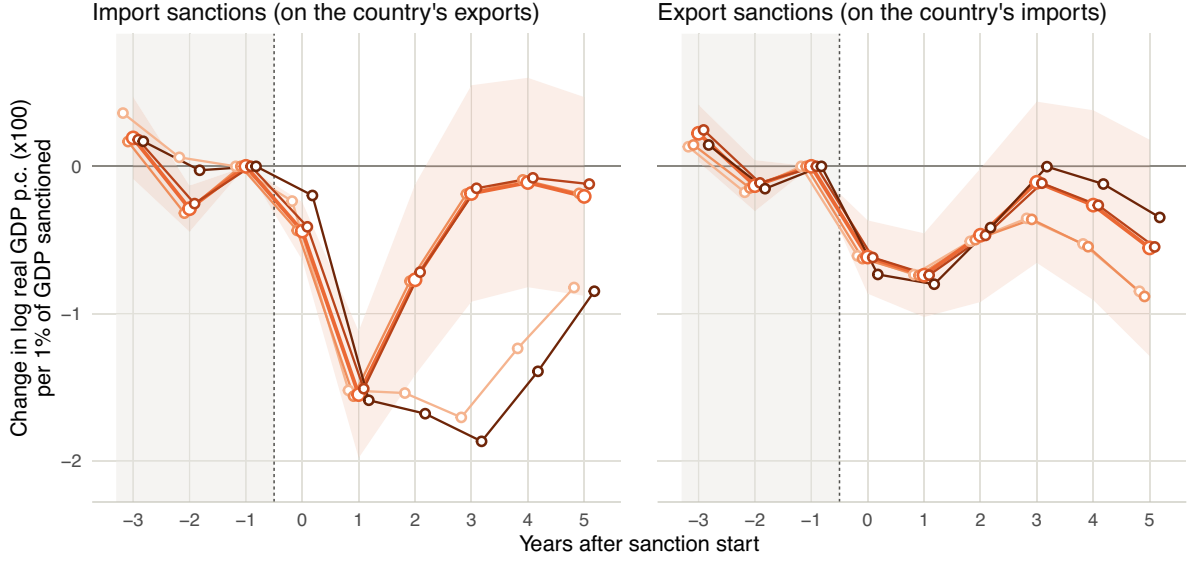
Figure F3: Reshuffling test for sanctions above and below the intensity threshold



—○— Slope with the actual doses, below 0.1%
 —○— Slope with the actual doses, above 0.1%
--- Doses reassigned at random among the stack's onsets: median and 5–95% of 2,000 draws

Note: Slopes of GDP per capita on sanction intensity among onsets below 0.1% of GDP (β_L^h , top row) and above it (β_H^h , bottom row, as in figure 5). Dashed lines and shaded bands: the median and the 5th to 95th percentiles of the slopes over 2,000 samples in which the intensities are reassigned at random among the onsets of the same group.

Figure F4: Robustness to threshold definition



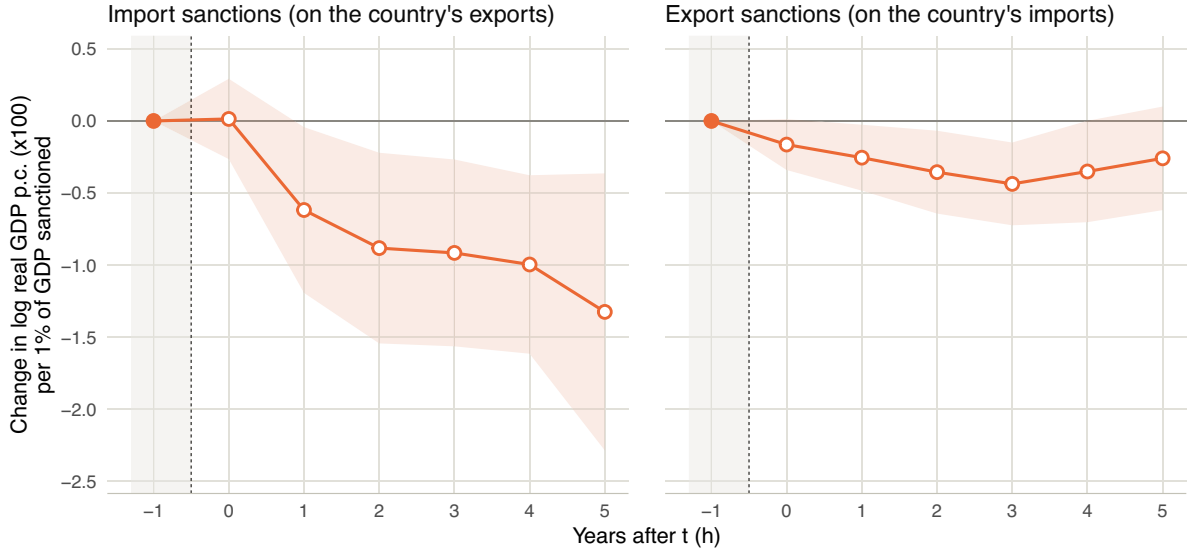
Threshold (share of GDP sanctioned): ○ 0.05% ○ 0.075% ○ 0.1% (baseline) ○ 0.2% ○ 0.3%

Note: Estimates of β_H^h from equation (3) with the intensity threshold κ set at 0.05%, 0.075%, 0.1% (baseline), 0.2% and 0.3% of GDP. Each threshold re-forms the two groups, their matched controls and their percentile bands. The shaded band is the 90% confidence interval of the baseline (Newey-West).

– that conditional on recent sanctions, growth, political conditions, and war, the imposition of new sanctions in t is unrelated to other determinants of future growth. In addition, this specification requires that the effects of sanctions be linear and additive, so that the lagged intensity controls do indeed remove the effect of preexisting sanctions.

Because this specification uses every country-year rather than only clean onsets, two GSDB codings of long-running sanctions that we consider errors carry much of its variation in import-sanction intensity. We therefore recode them in this specification (see appendix G). First, the GSDB codes the 1992–1996 UN embargo on the Federal Republic of Yugoslavia (UN Security Council Resolution 757; GSDB case 470) against all successor states of the former Yugoslavia, although only Serbia and Montenegro were embargoed; we set the sanction intensities of Croatia, Slovenia, North Macedonia, and Bosnia and Herzegovina in the years in which this policy is coded against them to zero. Second, the GSDB codes the UN’s call to boycott Portuguese goods in 1966–1974 (GSDB case 118) as a complete import sanction by every UN member, including Portugal’s main trading partners such as France, the United Kingdom and the United States, which did not boycott Portugal; we set Portugal’s import-sanction intensity in these years to zero. Without these two recodings, the estimated effects of import sanctions in this specification are close to zero and statistically insignificant.

Figure F5: Response of GDP to sanction intensification



Note: Estimates of β_d^h from the equation above, on all country-years up to 2020 with observed sanction intensities, after the recoding of two GSDB policies described in the text. 90% confidence intervals using Newey-West standard errors.

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