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Economic sanctions and trade flows in the neighbourhood

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Abstract: We investigate the effect of economic sanctions on trade flows in countries sharing a border with the sanctioned state. On the one hand, trade models suggest that trade flows should decrease as sanctions disrupt trading routes and economic ties with suppliers and customers. On the other hand, countries can circumvent trade restrictions by clandestinely exchanging goods with sanctioned countries across the border and trading on its behalf. If this is the case, we should expect an increase in their imports and/or exports. To address this issue, we combine large-N panel data analysis and comparative case-study analysis using the synthetic control method. We find that, on average, economic sanctions tend to have a negative effect on trade patterns. Yet, case studies reveal a significant degree of heterogeneity in countries' responses. We present several cases where we detect a substantial increase in trade flows when land neighbours are targeted with economic sanctions.

Key words: economic sanctions, trade, land neighbours, synthetic control method

JEL classification: F14, F51, F53, P16

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1 Introduction

Economic sanctions—the withdrawal of customary trade and financial relations with a target country—are widely used as an instrument of coercive foreign diplomacy (Anesi and Facchini 2019; Eaton and Engers 1992, 1999; Felbermayr et al. 2020). Since 1966, the United Nations (UN) Security Council has passed a number of resolutions establishing about 30 sanctions regimes 'to support peaceful transitions, deter non-constitutional changes, constrain terrorism, protect human rights and promote non-proliferation'.[1] These sanctions regimes range from targeted measures, such as financial or commodity restrictions, to comprehensive economic and trade sanctions. Examples of the latter are sanctions imposed on Iran in 2012, which are considered exceptional in terms of severity, scope, and non-discriminatory nature.

Besides the UN, national and regional governments and organizations (e.g. the European Union) have also used economic sanctions to alter the strategic decisions of actors that threaten their own interests or violate international norms of behaviour. Sanctions are also used symbolically to stigmatize political regimes (Whang 2011). The popularity of economic sanctions has expanded over time, partially because of the increased integration of the global economy, but also given the relatively smaller cost of sanctions, compared to more invasive or riskier foreign policy tools such as military interventions (Bove et al. 2014; Felbermayr et al. 2020).

What is the effect of economic sanctions on trade? Extant research has explored the political (e.g., Marinov 2005; Gutmann et al. 2020; McLean and Whang 2021) and economic (e.g., Hufbauer et al. 1990; Caruso 2003; Kaempfer and Lowenberg 2007; Etkes and Zimring 2015; Haidar 2017; Neuenkirch and Neumeier 2016; Shin et al. 2016; Afesorgbor 2019; Moghaddasi Kelishomi and Nisticò 2022; Amodio et al. 2021) costs of sanctions on sanctioned states, hence disproportionately focusing on the behaviour and costs suffered by the target.[2] Whether and to what extent sanctions can actually harm the economy of the target country remains an open issue, particularly when targets can respond with appropriate counter-measures to mitigate their negative consequences.

As of yet, however, we know much less about how *non-target states* react to the imposition of sanctions, particularly when the target is a land neighbour, that is a country with which they share a land border. Do countries suffer economic costs when their land neighbours are confronted with economic sanctions? And do they comply with sanctions regimes? What if states have ways to avoid sanction costs without the knowledge of the international community? In this article, we explore how trade patterns in the neighbourhood change in the wake of economic sanctions, and we argue that both an increase or a decrease in the levels of trade can be expected, depending on how neighbours of targeted states respond to the sanctions.

On the one hand, trade models suggest that both the sanctioning states and the target state are made worse off by trade embargoes (Kaempfer and Lowenberg 2007). Besides senders and targets of sanction, as Slavov (2007) points out, sanctions should also hurt neighbours by disrupting trading routes and established ties with suppliers or customers. The imposition of a blockade against a state with which another nation had enjoyed trade relations can damage both countries. Economic sanctions can also increase transportation and transaction costs. Indeed, numerous countries have claimed economic damages from sanctions under article 50 of Chapter VII of the UN Charter, when measures taken by the Security Council produce 'special economic problems' which make them eligible 'to consult the Secu-

[1] Available online: https://www.un.org/securitycouncil/sanctions/information (accessed December 2021).

[2] At the micro-, firm-level, recent studies have also showed that firms are less likely to export to a market if the destination country is sanctioned (Crozet et al. 2021).

rity Council with regard to a solution of those problems'. If this is the case, we should expect a reduction in neighbours' overall trade following the imposition of economic sanctions.

On the other hand, however, although compliance with UN sanctions is mandatory for all member state, senders' compliance cannot be taken for granted, and oftentimes UN sanctions do not constrain its members from engaging in 'spoiler behaviours'. For one, the mere threat of sanctions allows economic agents in the sender as well as neighbouring states to coordinate with the target state, in order to minimize the impact of upcoming restrictions by increasing trade to stockpile resources (Afesorgbor 2019). States can also openly violate sanctions once they have been imposed by engaging in 'sanctions-busting activities', i.e. by trading with the sanctioned target (Bove and Böhmelt 2021; Early 2015).

While trade restrictions are formally adopted by states, breaches of these are not unheard of, often driven by political and economic considerations. In fact, the willingness of third parties to circumvent sanctions by trading on behalf of other countries has often undermined—at least partially—the effectiveness of the economic sanctions imposed against them. For example, sanctions-busting trade conducted by the Soviet Union, Canada, Mexico, France, and Spain helped sustaining the Cuban regime's ability to withstand US sanctions (Early 2011). And despite the broad support enjoyed by the UN's decision to impose economic sanctions against South Africa for its policy of Apartheid in 1962, numerous countries, including permanent members of the UN Security Council like the United States and the United Kingdom, busted the sanctions by trading with South Africa (Early 2015).

In addition to the direct supply of goods to sanctioned states, which is likely to be detected, states have also ways to avoid sanction costs without the knowledge of the international community and, thus, without openly violating sanctions. A recent example is China's illicit smuggling of oil into North Korea and the covert exports of coal from North Korea to China.[3] One way to circumvent trade restrictions, particularly relevant for this research, is to clandestinely exchange goods with sanctioned countries across the border. In fact, cross-border trafficking is a thriving activity in many countries, particularly in the absence of border controls (Golub 2015; Slavov 2007).[4] We may expect the porous nature of many international borders and geographic proximity to facilitate the import of goods via states neighbouring a sanctioned state. In a similar vein, neighbours of the sanctioned country can trade on its behalf by smuggling goods out of the target and exporting them to the rest of the world. Following this argument, we should expect an increase in a country's imports and/or exports after economic sanctions are imposed on its land neighbours.

Since economic sanctions can have two effects that go in opposite directions, the deterioration of trading routes and increased transportation costs on the one hand, and the opportunity to profit by engaging in 'sanctions-busting activities' on the other hand, the net effect on imports and exports is not obvious. This is something that has to be determined from the data.[5]

Against this background, this paper seeks to provide a new approach to analyze the economic costs of sanctions on non-target states and identify risks of undetected trade flows under sanctions regimes. Specifically, we focus on exports from and imports into countries neighbouring a target state and ex-

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[3] See, for instance, https://www.nytimes.com/2021/03/24/world/asia/tankers-north-korea-china.html and https://www.theguardian.com/world/2020/apr/18/north-korea-defies-sanctions-with-chinas-help-un-panel-says (accessed December 2021).

[4] There is some empirical evidence that sanctions are also followed by an increase in the shadow economy as both individuals and governments engage in illegal economic activities (Andreas 2005).

[5] Slavov (2007) similarly tests the 'smuggling hypothesis' that neighbours will trade more heavily during sanctions. He finds that overall neighbour countries are 'innocent bystanders', as UN sanctions reduce trade flows between land neighbours and the rest of the world. We use a very different research design, which distinguishes our empirical approach from his. This is the issue discussed in the next section.

amine the relation between trade patterns and embargoes using large-N panel data and synthetic control approaches. As illicit trade is notoriously difficult to detect and quantify, our panel data analysis proceeds in three steps. First, we use two-way fixed effects regressions on the entire sample of sanctioned and not-sanctioned neighbours to estimate the average effect of total economic embargoes. Second, to provide a more plausible counterfactual of the level of trade in a country had economic sanctions not been imposed on its land neighbour, we compare imports/exports in countries that share borders with sanctioned states (treatment group) with imports/exports into countries neighbouring states that were threatened with economic sanctions, which however were not imposed (control group). Third, we report event-study estimates of the effects of embargoes on trade in the neighbourhood using a 4-year bandwidth and also use the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020).

Overall, we find that on average countries experience a reduction in trade when their land neighbours are confronted with economic sanctions. As such, sanctions seem to hurt neighbours by disrupting trading routes and trading ties. However, we are able to detect a positive effect of sanctions' imposition for countries that share a high proportion of their border (more than two thirds) with target countries. We speculate this could be due to increased opportunities for smuggling. This result is consistent with our tentative test of the smuggling hypothesis presented in Appendix B, where we use night-light emissions as proxy for economic activities; we find that emissions from grid cells along the border of sanctioned countries increase in the following year compared to cells that are just off the international border.

Yet, the possibility of heterogeneous responses to economic sanctions calls for an in-depth analysis of specific cases, i.e. key neighbours of target countries. As a second approach, we move to a comparative case-study analysis using an alternative counterfactual approach—the synthetic control method—and compare the post-sanction import/export trajectories of neighbours of sanctioned states with the trajectories of combinations of otherwise similar but unexposed countries (see e.g. Abadie et al. 2010). Estimating treatment effects by comparison of a treated case with a synthetic control reveals a great degree of heterogeneity in the effect of embargoes. In three out of 15 cases, sanctions produce welfare costs; yet, in the remaining nine cases, we detect a sudden, arguably unmotivated, increase in imports or exports. As such, neighbouring embargoes have heterogeneous, country-specific effects on trade.

We proceed as follows. Section 2 presents the data. Section 3 describes the panel data analysis strategy and presents the results. Section 4 describes the synthetic control method and its main advantages in this study, and presents the implemented experiments. Section 5 provides concluding remarks.

2 Data

The unit of analysis are neighbouring states, as we seek to investigate whether, all else equal, trade increases in states neighbouring a country when economic sanctions are imposed on the latter. To this end, we employ the simplest definition of neighbouring countries, i.e. first order contiguity: states must share (part of) a border with a sanctioned state. According to this definition, only countries that are immediately contiguous to each other are considered neighbours; neighbours of neighbours (i.e. second-order contiguity) are not considered as neighbours of a sanctioned state. Not only this definition is parsimonious, it is also the most appropriate for testing the hypothesis of sanctions-busting via cross-border exchanges, which is conditional on sharing a border in the first instance. After having identified pairs of neighbouring countries, we compile information on sanctions in those states neighbouring the focal country of the monadic, country-year dataset.

For our main explanatory variable, *economic sanctions*, we rely on the EUSANCT Dataset (Weber and Schneider 2020), which contains 326 threatened and imposed sanctions by the three most important senders of sanctions, namely the European Union (EU), the United Nations (UN), and the United States

(US). The dataset contains several categories of sanctions (i.e. economic embargoes) imposed by the sender. For our main analysis we focus on *total economic embargoes*, i.e. when the sender stops the flow of all economic exchange (imports and exports) to and from the target state. One of the main strengths of the EUSANCT Dataset is the inclusion of information on both sanctions in force—which are actually imposed—as well as sanction threats. This comes at the expense of a relatively limited time frame, from 1989 to 2015. For this reason, in the second part of the analysis, using a case study, synthetic control approach, we integrate the EUSANCT Dataset with the recently released Global Sanctions Data Base (GSDB) (Felbermayr et al. 2020) which covers 729 publicly traceable multilateral, plurilateral, and purely bilateral sanctions that were enforced over the 1950–2019 period. Once again, we limit the analysis to sanctions imposed by the EU, the UN, and the US given their relative higher monitoring and enforcement capabilities, and to ensure we consider the same group of senders in the two analyses. We will focus in particular on trade sanctions, defined as 'measures that aim to restrain economic interactions with a target country by limiting international trade' (Felbermayr et al. 2020: 6).[6]

For our main dependent variables, total *trade*, *imports*, and *exports*, we use yearly data from the World Bank Development Indicators. Imports and exports of goods and services represent the value of all goods and other market services received from and provided to the rest of the world, respectively, as a share of gross domestic product (GDP). Total *trade* is the sum of exports and imports of goods and services measured as a share of GDP.

3 Panel data analysis

The unit of analysis of our panel data estimation is any country i that is not under sanctions regime in year t. Hence we compare countries that are not direct targets of sanctions, with a treated group comprising countries that are only indirectly affected by sanctions being imposed on at least one neighbouring country. We employ an empirical specification that takes the following form:

$$Y_{it} = \alpha + \beta \text{Embargo}_{it} + \delta \mathbf{X}_{it} + \mu_i + \lambda_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is the share of import or export to GDP in country i and year t. Embargo_{it} is an indicator that equals one in all years when an embargo is imposed on one of i's land neighbours, and zero otherwise. $\mathbf{X}_{it}$ is a vector of control variables that includes: the GDP per capita and growth rate of real GDP taken from the World Bank Development Indicators, to capture the level and changes in economic development; population size from the same dataset; the Polity Score taken from Marshall et al. (2018) to measure the level of democracy in a country according to a 21-point scale ranging from +10 (strongly democratic) to -10 (strongly autocratic); and a dummy for the presence of civil and international conflicts (including internationalized intra-state disputes) using data from the Uppsala Conflict Data Program (UCDP). μ_i and λ_t represent country-specific effects and year-specific effects, respectively; ε_{it} is an $i.i.d.$ error term. Table A1 in the Appendix contains the summary statistics for our sample.

We first estimate Equation 1 on the entire sample of countries with the full set of control variables and year and country fixed effects. We then use to our advantage information on similar countries—land neighbours of countries with and without actual economic sanctions—to get as close as possible to a plausible counterfactual of what would have happened in the absence of sanctions. In particular, we employ an approach similar in spirit to a differences-in-differences (DiD) analysis and run regressions where we keep only the countries sharing borders with states under actual or threatened economic sanc-

[6] The other categories are financial activity, arms, military assistance, travel, plus a residual category collecting other sanctions.

tions. Hence, the control group consists of neighbours of states in which sanctions were threatened but never imposed.[7]

Table 1 reports the estimated coefficients of the model in Equation 1 when we investigate the effects of full economic embargoes. As dependent variables, we use total trade as share of the GDP, as well as imports and exports separately. We estimate models with and without control variables. The results are in line with Slavov (2007): we detect a decline in neighbours' total trade, imports, and exports with the rest of the world following the imposition of an economic embargo. The coefficient estimate suggests that moving from 0 to 1 translates into a decrease of more than nine percentage points in total trade for the neighbour of the embargoed targets (see second column of Table 1). A reduction of almost five and four percentage points are estimated for imports and exports, respectively. As such, the substantive effects are not only statistically significant, but also economically meaningful. If this is the case, then, economic sanctions in the neighbourhood appear to disrupt trading routes and trading ties, and/or impose transportation and other transaction costs that reduce the flows between neighbours of sanctioned countries and the rest of the world.

Table 1: Embargo and trade flows in the neighbourhood

	(1)	(2)	(3)	(4)	(5)	(6)
	Total trade		Imports		Exports	
Neighbours under embargo	-12.995***	-9.076**	-6.271**	-4.971*	-6.724***	-4.105*
	(4.527)	(4.543)	(2.924)	(2.739)	(2.344)	(2.294)
Population (ml)		0.051**		0.031**		0.020
		(0.024)		(0.013)		(0.012)
GDP per capita (000)		1.386**		0.559*		0.827**
		(0.692)		(0.322)		(0.377)
GDP per capita growth (00)		2.934		-0.675		3.609
		(9.610)		(6.367)		(4.939)
Polity score		0.590**		0.391**		0.199
		(0.262)		(0.150)		(0.138)
War dummy		4.157**		2.177**		1.981**
		(1.683)		(0.863)		(0.936)
Observations	4210	3678	4210	3678	4210	3678

Note: two-way fixed-effects regressions in all specifications. Dependent variables Total trade, Imports, and Exports are expressed as % of GDP. Robust standard errors in parentheses are clustered by country. * p<0.10, ** p<0.05, *** p<0.01.
Source: authors' calculations based on data described in Section 2.

In Table 2, we restrict the sample to countries bordering targets of threatened or actually imposed sanctions. In so doing, we are using a more homogeneous sample of countries and leverage information regarding sanctions that were threatened without any explicit imposition. As a threat declares that sanctions are a possibility against the target state if, for instance, targets do not alter their behaviour, we believe that neighbours of the target state under threats of sanctions are a more plausible counterfactual than neighbours of states that are not (and maybe never will be) considered potential targets.[8]

We use both the full sample period (columns 1, 3, and 5) as well as a shorter time window of five years before and after the sanctions (columns 2, 4, and 6). The rationale behind the latter restriction is that the institutional, political, and social contexts, typically slow-moving, are more likely to be stable

[7] Note that this set-up deviates from the canonical DiD set-up in that we have multiple time periods, variation in treatment timing (staggered treatment design), and the parallel trends assumption holds potentially only after conditioning on observed covariates.

[8] Yet, it is also possible that the threat of sanctions—by affecting bilateral trade flow between the sender and its target (Afesorgbor 2019)—may also have effects on imports and exports in the neighbourhood. If this is the case, states that are only threatened with sanctions do not offer the ideal counterfactual of what would have happened in the absence of an enforced embargo, something which unfortunately can never be observed.

over a narrow window of time. As emphasized by Neuenkirch and Neumeier (2015: 118), indeed, the environment when sanctions are in place and when they are not may not be comparable, and the imposition of sanctions 'might be a consequence of an environment that is considered "bad" by the United Nations and/or the United States'.

As we can see from Table 2, we still find significant negative effects of total economic embargoes on total trade, imports, or exports in the neighbourhood of the targeted countries for the full period, as well as for the restricted period (five years around the sanctions period). The estimates in Table 2, columns 2, 4, and 6 further assuage concerns around the possibility that our main results are mostly driven by underlying differences between the treated and untreated units.

Table 2: Embargo and trade flows in the neighbourhood: restricted sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Total trade		Imports		Exports	
Neighbours under embargo	-11.447**	-10.554**	-6.107**	-5.240**	-5.340**	-5.313**
	(4.690)	(4.402)	(2.907)	(2.173)	(2.293)	(2.409)
Population (ml)	0.078***	0.075***	0.043***	0.039***	0.035***	0.036***
	(0.017)	(0.017)	(0.010)	(0.009)	(0.009)	(0.009)
GDP per capita (000)	0.509	0.088	-0.059	-0.244	0.568*	0.332
	(0.525)	(0.466)	(0.253)	(0.222)	(0.325)	(0.294)
GDP per capita growth (00)	-4.988	-1.754	-6.027	-5.889	1.039	4.135
	(9.927)	(10.786)	(6.335)	(6.815)	(5.667)	(5.228)
Polity score	0.429	0.314	0.329**	0.238	0.100	0.076
	(0.272)	(0.347)	(0.149)	(0.176)	(0.162)	(0.209)
War dummy	3.979**	3.985**	2.104**	1.856*	1.875**	2.129**
	(1.620)	(1.736)	(1.043)	(0.993)	(0.805)	(0.909)
Restricted period	No	Yes	No	Yes	No	Yes
Observations	1751	1279	1751	1279	1751	1279

Note: two-way fixed-effects regressions in all specifications. Dependent variables Total trade, Imports, and Exports are expressed as % of GDP. The sample only includes countries sharing borders with states under actual or threatened economic sanctions, thus comparing the trade flows of countries neighbouring states under threat of economic sanctions with the trade flows of countries neighbouring states under actual embargo. In columns 2, 4, and 6, the sample is restricted to the five years before and after the embargo. Robust standard errors in parentheses are clustered by country. *p<0.10, **p<0.05, ***p<0.01.
Source: authors' calculations based on data described in Section 2.

One might argue that not all embargoes have the same effect, and the way countries respond to embargoes in their neighbourhood should also depend on the 'intensity' of the relationship between the sanctioned targets and their land neighbours. To investigate whether this is the case, we construct two measures of exposure to embargo (or the intensity of the embargo). The first is based on the average share of i's trade with the embargoed states relative to its total trade during the five years before the imposition of the embargo. We thus replace the dummy Embargo$_{it}$ in Equation 1 with a term taking value zero in absence of economic embargoes in the neighbourhood and a continuous variable ranging from 0 to 1 in presence of embargoes (i.e. neighbours under embargo=1 $\times$ share of trade with embargoed states). More specifically, a country with a total trade of, for instance, 100 that completely goes to its sanctioned neighbour will get a value 1. A country that trades 100 globally but trades only five to one neighbour and five to another neighbour will get a value 0.1 if both neighbours are sanctioned.

Results are available in Table A2 in the appendix, where we can see that the coefficient is statistically significant and its magnitude is larger than the coefficient for embargoes in Table 2, highlighting that a decline in total trade is higher the larger the border shared with the targets, as one would expect given the previous round of results.

The second measure of intensity is based on the relative length of shared borders. To explore the impact of this variable, we first construct a variable that, for each country, measures the proportion of its border that is shared with a sanctioned country. For example, a country with one neighbour only who is also sanctioned will get value 1. A country with a total border of 100km that shares a 10km border with a sanctioned country will get value 0.1. Second, we create three groups based on the share of i's borders shared with countries under total economic embargoes: less than one third, between one third and two thirds, and more than two thirds. Results are reported in Table A3 in the appendix. We find a negative effect of embargoes on trade for the middle group (compared to the bottom group), whereas the effect becomes positive for the upper group. In other words, there seems to be a more moderate but positive impact of economic sanctions for countries sharing a large percentage of their borders with embargoed states. One explanation might be that larger borders provide more opportunity for smuggling, or perhaps countries find ways to take advantage of the embargo through alternative new trading routes.

Finally, and related to the previous analysis of shared borders' length, we provide an exploratory analysis that attempts to probe the smuggling mechanism in Appendix B1. Leveraging data on global night-light emissions between 1992 and 2013 using 55km x 55km grid cells from the PRIO-GRID v.2 (Tollefsen et al. 2012), we test whether emissions of cells on the border of a sanctioned country increase in the aftermath of a sanction compared to cells that are just off the border. Relying on research suggesting that night-time luminosity can be used as a proxy for economic activities and development (Bruederle and Hodler 2018; Weidmann and Schutte 2017), we expect cells on the border of sanctioned states to emit more night light if smuggling is taking place, compared to cells that are close to but not on the border. Results, while tentative, tend to confirm that this mechanism may be in place.

3.1 Event study and robustness analysis

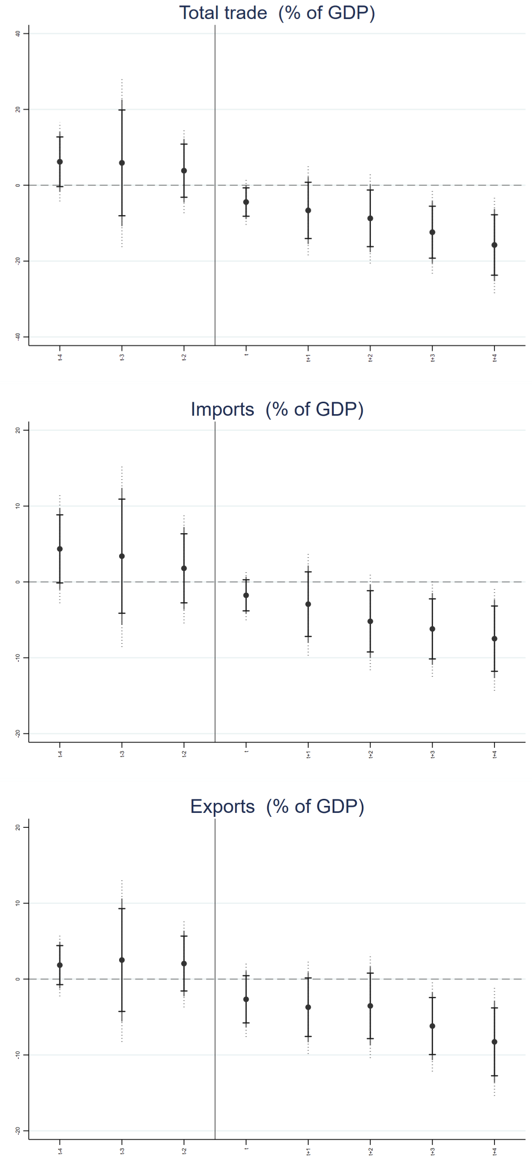
The common trend assumption behind our identification strategy requires that the trade of countries neighbouring a target state (i.e. treated countries) and those that do not (i.e. control group) behave in a similar manner in the period before the embargo. Assuming that, in the absence of economic sanctions, the units in the treatment group follow the same trend as those in the control group, the latter provides the missing potential outcome: the amount of trade in countries that share borders with sanctioned countries had economic sanctions not been imposed. This ensures that having a land neighbour under embargo is not endogenously related to trends in the outcomes. To test for the validity of this assumption, we use an event-study analysis approach and estimate the following specification:

$$Y_{it} = \alpha + \sum_{j=-4}^{-1} \beta_j Embargo_{it+j} + \sum_{k=0}^{4} \beta_k Embargo_{it+k} + \delta \mathbf{X}_{it} + \mu_i + \lambda_t + \varepsilon_{it} \qquad (2)$$

This is a modified version of Equation 1 in which the post-embargo indicator is replaced with a series of mutually exclusive lead and lag indicators. We include four pre- and four post-treatment effects, where the omitted year (i.e. the baseline year) is the year before the embargo takes place, while leads and lags are identified by the coefficients β_j and β_k, respectively. If the leads β_j are not statistically different from zero, we can assume that the parallel trends assumption holds. The β_k coefficients, instead, allow us to examine the evolution of the treatment effect over time.

Figure 1 reports event-study estimates of the effects of embargoes on neighbours' trade using the four-year bandwidth. This specification allows us to check for pre-treatment trends and to explore whether the effects of embargoes grow stronger or are abated over time. There is no evidence of pre-treatment trends (the estimated coefficients of the pre-event dummies are consistently small and statistically insignificant at conventional levels), but one year after treatment, there is a tick downward of about four percentage points in total, followed by a more modest but gradual decline over time. There is some evidence of a sustained downward trend in imports and exports in the post-embargo period as well, which we interpret as evidence against the hypothesis that countries are benefiting from embargoes imposed on their land neighbours.

Figure 1: Event-study estimates: total trade, imports, and exports (as a % of GDP)



Source: authors' calculations based on data described in Section 2.

A number of recent studies shows that the classical two-way fixed-effects models may, under certain conditions, be subject to bias, for example when previously treated units are used as controls (Callaway and Sant'Anna 2021; De Chaisemartin and d'Haultfoeuille 2020). When this is the case, one could argue that within-unit temporal heterogeneity makes the trend among early treated units a poor counterfactual for the trend among late treated units.[9]

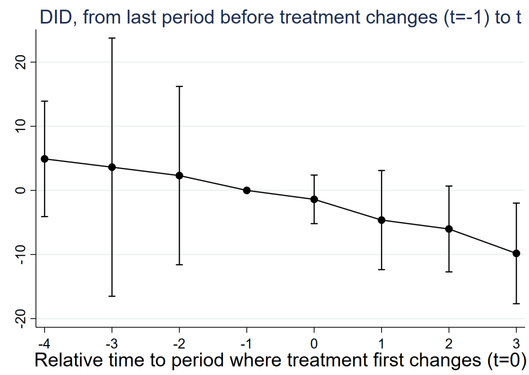
To address this issue, we resort to the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020) using Stata's *did multiplegt* module. We replicate the baseline models in columns 2, 4, and 6 of Table 2, using the restricted sample. Figure 2 reports the coefficients estimated in the event-study analysis using $t-1$ (the year before the imposition of the sanctions in the target country) as baseline year.

[9] The main point raised by De Chaisemartin and d'Haultfoeuille (2020) is that the classical two-way fixed-effects models estimate a weighted sum of several difference-in-differences (DiD), which compare the evolution of the outcome between consecutive time periods across pairs of groups, with weights that may be negative. Due to the negative weights, the linear regression coefficient may for instance be negative while all the average treatment effects (ATEs) are positive.

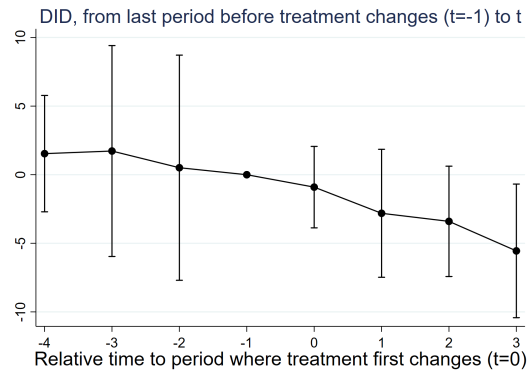
We again flexibly estimate the effects of embargoes before/after one, two, three, and four years. Overall, the results are qualitatively the same when estimated using de Chaisemartin and D'Haultfoeuille's estimator: we find a negative effect of sanctions on the trade of neighbouring countries, with dynamics similar to those we detect in the event-study analysis. As such, the evidence suggests that sanctions seem to hurt the neighbours of the embargoed targets.

Figure 2: De Chaisemartin & D'Haultfoeuille estimates: total trade, imports, and exports (as a % of GDP)

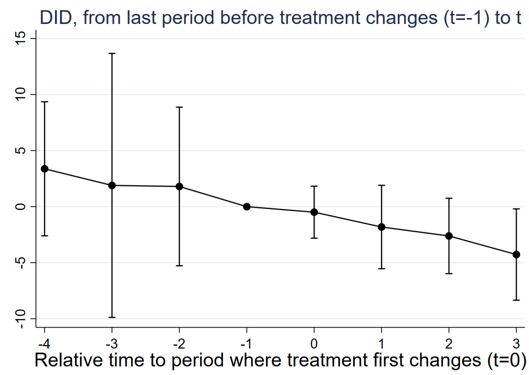
(a) Total trade



(b) Imports



(c) Exports



Source: authors' calculations based on data described in Section 2.

That being said, qualitative evidence suggests that countries could violate embargoes, and have done so, for instance by supplying specific goods to targets' neighbours so that goods can be easily transported across the border and to the sanctioned targets (see also our analysis on night-light emissions at the border in Appendix B1). At the same time, and perhaps more importantly, states' reactions to economic sanctions could vary widely and aggregate studies could conceal a deal of heterogeneity in countries' responses (Bove et al. 2017). If this is the case, heterogeneous, country-specific effects of sanctions on economic exchanges should be investigated. Given the widespread consensus that has emerged in recent years about the necessity of building bridges between the large N and case studies (Abadie et al. 2015), we now turn to a case-by-case analysis of specific embargoes. We once again unpack exports and imports to investigate whether land neighbours of sanctioned states increase their levels of trade with the rest of the world and whether they do so on behalf of the embargoed target. This analysis relies on the use of the synthetic control method, which we discuss in the next section.

4 Synthetic control method

We use the method proposed by Abadie and Gardeazabal (2003), to complement the panel data analysis on the impact of trade sanctions. We build on a simplified version of their approach, following Imbens & Wooldridge (2009)'s notation.[10]

Consider $t = 1,2,\ldots,T$ time periods and $i = 0,1,2,\ldots,G$ countries. A trade embargo occurs at time T_0, with $1 < T_0 < T$, and country 0 shares border with the embargoed target. Then, denote by $D_{0t} = 1$ the treatment status, i.e. not being under embargo but sharing a border with at least one sanctioned country. The treatment effect for country 0 at time t on the outcome of interest Y_{0t}, i.e. imports and exports in percentage of the GDP, is defined as follow:

$$\alpha_{0t} = E[Y_{0t}|D_{0t} = 1] - E[Y_{0t}|D_{0t} = 0] \qquad \text{for } t = T_0+1,\ldots,T \tag{3}$$

The potential outcome for the post-treatment period in the absence of the treatment is estimated as a weighted average of periods $t = T_0+1,\ldots,T$ outcomes in the $i = 1,2,..,G$ control groups,

$$E[Y_{0t}|D_{0t} = 0] = \sum_{i=1}^{G} \lambda_i \bar{Y}_{it} \tag{4}$$

where $\bar{Y}_{it}$ is a generic linear combination of pre-treatment outcomes and λ_i are weights, satisfying $\sum_{i=1}^{G} \lambda_i = 1$ and $\lambda_i \geq 0$, to prevent extrapolation outside the support of the data. The weights are chosen to make the weighted control country resemble the treatment country prior to the treatment. That is, the estimation problem amounts to choosing the vector of weights that minimizes the difference between the treated country and the λ-weighted average of the control countries over the period in which none of them had been exposed to the treatment, i.e.:

$$\left\| \begin{array}{c} Y_{0t} - \sum\limits_{i=1}^{G} \lambda_i \bar{Y}_{it} \\ . \\ . \\ Y_{0T_0} - \sum\limits_{i=1}^{G} \lambda_i \bar{Y}_{iT_0} \end{array} \right\|$$

where $\|\|$ denotes a measure of distance. To determine the weights, we use all the pre-intervention values of the outcome. The predictor variables can also be formed from the average of all the available pre-intervention periods, the average of a shorter pre-intervention sub-sample, or using specific years. We use all outcome lags as separate predictors to improve the pre-treatment fit of the dependent vari-

[10] Previous studies using this approach to implement a set of comparative case studies include, among others, Billmeier and Nannicini (2013) and Bove and Nisticò (2014).

able and help mitigate the endogeneity stemming from omitted variable bias.[11] In fact, as in Abadie and Gardeazabal (2003), we use an algorithm that minimizes the distance in terms of pre-treatment outcomes. Specifically, let X_1 be the $(k \times 1)$ vector of pre-intervention outcomes for the treated country and X_0 be the $(k \times i)$ matrix that includes the same variables for the control units; also, let V be a $(k \times k)$ diagonal matrix with non-negative entries measuring the relative importance of each predictor. Conditional on V, the optimal vector of weights, $\Lambda^*(V) = (\lambda_1, \ldots, \lambda_G)'$, must solve:

$$\min(X_1 - X_0\Lambda(V))'V(X_1 - X_0\Lambda(V)) \tag{5}$$

subject to $\lambda_i \geq 0$ and $\sum_{i=1}^{G} \lambda_i = 1$. The vector of weights $\Lambda^*(V)$ defines the combination of untreated control countries which best resemble the treated unit in trade before the intervention. We then select V such that the mean squared prediction error of pre-treatment outcomes is minimized, i.e.:

$$\frac{1}{T_0} \sum_{t \leq T_0} (Y_t - \sum_{i=1}^{G} \lambda_i^* Y_{it})^2 \tag{6}$$

When the number of pre-intervention periods in the data is large, as in our case, matching on pre-intervention outcomes helps control for the unobserved factors affecting the outcome of interest. Once it has been established that the unit representing the case of interest and the synthetic control unit behave similarly over *extended* periods of time prior to a trade embargo, a discrepancy in imports following the embargo is interpreted as produced by the trade embargo itself. The idea is that the future path of the synthetic control group, consisting of the λ-weighted average of all the control groups, mimics the path that would have been observed in the treatment group in the absence of the treatment.

We consider a 20-year time window so as to have ten-year pre-embargo data to calibrate the synthetic and ten-year post-embargo data to forecast the long-run effect of the embargo. The synthetic control method requires a number of comparative units, that is unexposed units that approximate the most relevant characteristics of the treated units over the same period. Therefore, we include in the donor pool countries that have never been exposed to the treatment over the entire time window analysed.

One question is whether the estimated effects are statistically significant. This is not trivial, since large sample inferential techniques are not appropriate for comparative case studies with a small number of treated and control units (Abadie et al. 2010). For each case study, we perform a Chow test to assess whether there is a statistically significant difference between the outcome of the treated unit and the outcome of the synthetic control during the post-sanctions years.

4.1 Case studies

We now identify a set of feasible case studies. We select the period 1960–2018 given the lack of information on trade for most countries before 1960. Moreover, we focus only on episodes of multilateral trade sanctions, that is sanctions imposed by the UN and/or jointly imposed by the EU and the US. We select the countries meeting the following conditions: (a) the treated country and the control group must

[11] There is a debate about the optimal choice of predictor variables, and Kaul et al. (2021) show that estimation results can vary considerably when the usage of outcome lags as predictors is restricted. As a robustness check, we also add a fairly standard set of trade predictors such as real per capita GDP, population, total trade (as a percentage of GDP), a war dummy, and the Polity IV dichotomous indicator for democracy. Keeping the pre-intervention values of the outcome makes most of these predictors less relevant, i.e. they are assigned a small weight. When we exclude them, the synthetic controls provide a poor fit. Results can be produced with our replication material.

have no missing information on the outcome variable (i.e. the share of import or export to GDP) in the 20-year-long sample period, as we require ten-year pre-sanction observations to calibrate the synthetic control and ten-year post-sanction observations to have a reasonably large span of plausible predictions of the effect of the sanctions; (b) as for some case studies the pre-treatment fit can be poor, thus undermining the credibility of our analysis, we include only countries with root mean squared prediction errors (RMSPE) smaller or equal to 2;[12] (c) because this analysis covers the period 1960–2018, the treated country must be exposed to land neighbours' sanctions at the earliest in 1970 and at the latest in 2008, as we need a ten-year period before the sanctions and at least a ten-year period after the sanctions;[13] (d) in case of multiple and subsequent sanctions, we select the first one in chronological order.

After this selection, we end up with a pool of nine episodes of multilateral economic sanctions and a final set of 15 case studies, i.e. countries that share a border with one of the nine embargoed targets. For each of the nine episodes examined, Table 3 reports the sanctioned country, the year of the sanction, the sender(s), and the specific land neighbours we use in the case-study analysis. All nine episodes entail severe international economic sanctions, ranging from general trade restrictions to total blockades.

Table 3: List of case studies

Target country	Start year	End year	Sender	Land neighbours
Iran	2011	ongoing	EU-UN-US	Turkey, Pakistan
Sierra Leone	1997	2003	EU-UN-US	Guinea
Liberia	1992	2001	UN	Côte d'Ivoire
Somalia	1992	2013	EU-UN-US	Kenya
Yugoslavia	1991	2001	EU-UN-US	Albania, Austria, Bulgaria Greece, Italy
Myanmar	1991	2013	EU-UN-US	Bangladesh, China
Haiti	1987	1994	EU-UN-US	Dominican Republic
Libya	1986	2006	EU-UN-US	Tunisia
South Africa	1977	1998	EU-UN-US	Swaziland

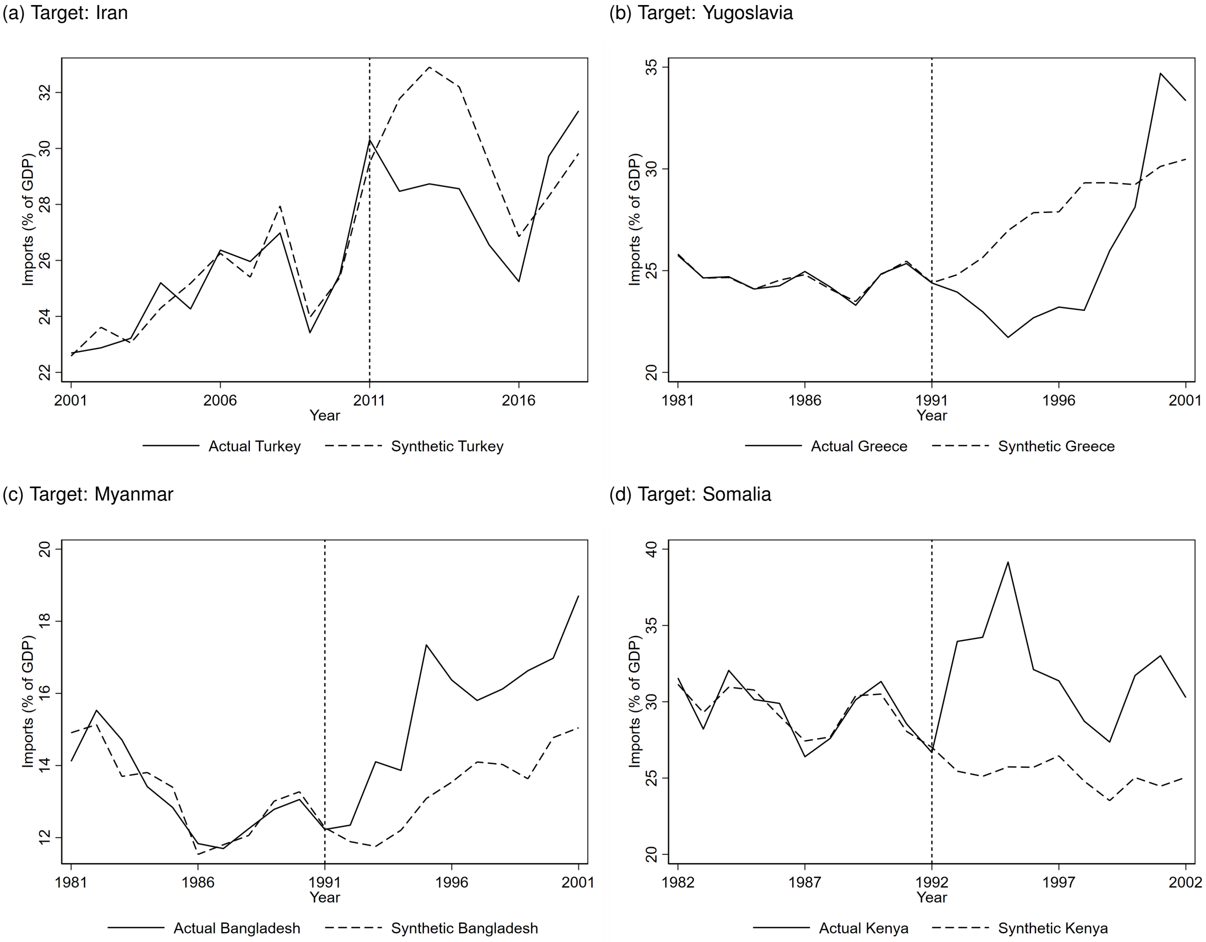
Source: authors' elaboration based on Global Sanctions Data Base (Felbermayr et al. 2020).

To illustrate the potential heterogeneity in countries' response to embargoes in their neighbourhood, Figure 3 shows four selected embargoed countries across different regions of the world, one in Middle East (i.e. Iran), one in Europe (i.e. Yugoslavia), one in Asia (i.e. Mynamar), and one in Africa (i.e. Somalia). In particular, Figure 3 plots the trends in imports for the treated country and its synthetic control for the four neighbouring countries for which we observe the best pre-treatment fit, namely Turkey, Greece, Bangladesh, and Kenya, respectively.

[12] The RMSPE is a measure of the pre-treatment fit between the path of the outcome variable for any particular country and its synthetic counterpart. The lower is the RMSPE the better is the fit.

[13] With the exception of countries neighbouring Iran (where sanctions were imposed in 2011) for which we only have seven years post-sanctions.

Figure 3: Trends in imports (as a % of GDP), treated country vs synthetic control



Source: authors' calculations based on data described in Section 2.

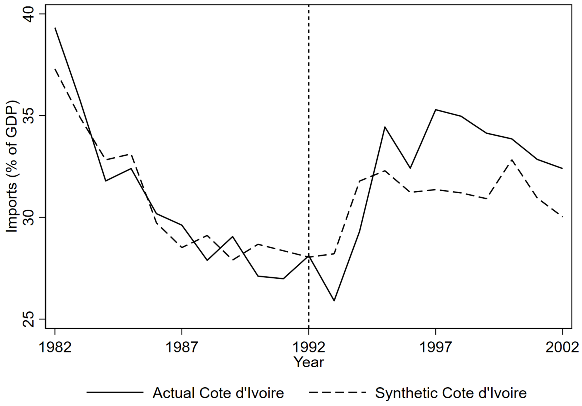
The estimated effect on imports is the difference between imports in the actual treated country (solid line) and in its synthetic version (dashed line) after the imposition of the sanctions in the neighbourhood. As shown in all case studies of Figure 3, the share of imports to GDP in the synthetic country very closely tracks the trajectory of that in the treated countries for the full ten-year period before the sanctions.[14] This indicates that the synthetic control provides a good approximation of the share of imports to GDP that we would have observed in the treated country in the post-treatment period in the absence of sanctions against their land neighbour.

A visual inspection of the discrepancies between solid and dashed lines in the four cases illustrated in Figure 3 suggests that economic sanctions do not have homogeneous effects on the trade of neighbouring states. More specifically, countries such as Turkey or Greece experience a decline in imports following the sanctions imposed on bordering countries, possibly because of the disruption in trading routes or in the established trade relations with the target. By contrast, countries like Bangladesh or Kenya increase their imports as a response to the economic sanctions imposed on one of their land neighbour, suggesting cross-border trafficking as a possible explanation. This is particularly noticeable in Figure 4, where we zoom on four alternative case studies in the African continent, where we arguably expect more porous borders. In three out of four cases, countries seem to respond to sanctions by increasing imports.

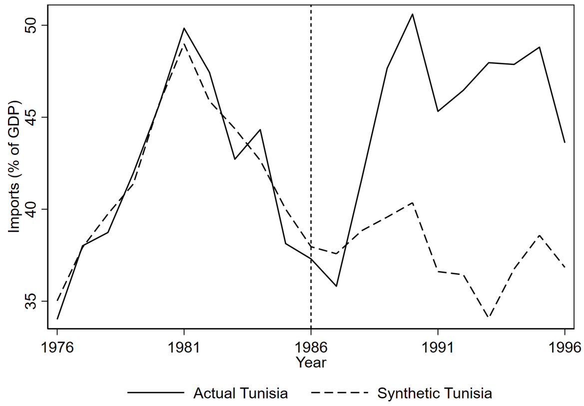
[14] Table A4 in the appendix reports comparisons of pre-treatment characteristics between synthetic and actual case studies as well as the weights of each control country in the synthetic case studies.

Figure 4: Trends in imports (as a % of GDP), treated country vs synthetic control

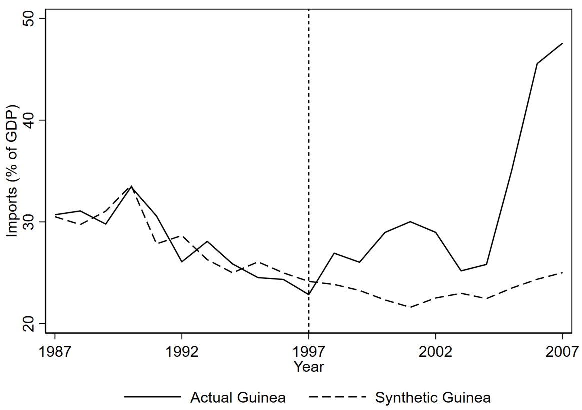
(a) Target: Liberia



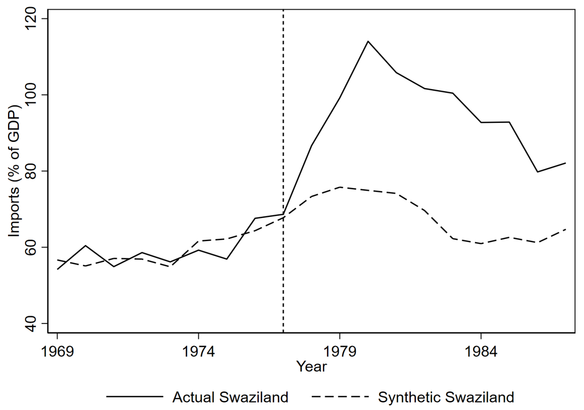
(b) Target: Libya



(c) Target: Sierra Leone



(d) Target: South Africa

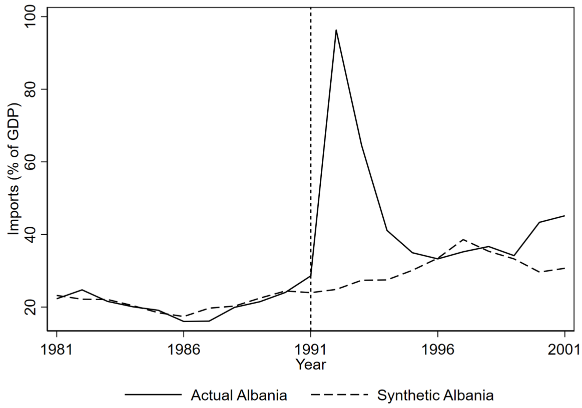


Source: authors' calculations based on data described in Section 2.

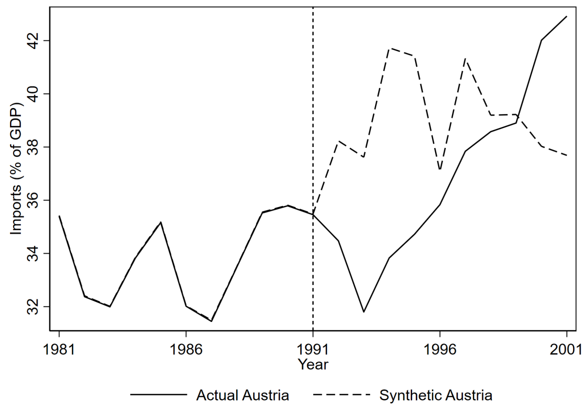
Interestingly, we detect heterogeneous effects of sanctions, even when we focus on a set of countries sharing the border with the same target state, as in Figure 5. The sanctions imposed on Yugoslavia in 1991 generated different reactions among its neighbouring countries. While Albania and Italy experienced a sharp increase in the share of imports immediately after the sanctions were imposed, the opposite is true for Austria, where imports started to decline following the sanctions. Less clear is the pattern for Bulgaria, where after an increase in the aftermath of the sanctions, imports declined for the subsequent three years and increased again over the following six years. The appendix reports additional case studies hinting at countries reacting differently to sanctions against neighbouring states (Figure A1, Appendix).

Figure 5: Trends in imports (as a % of GDP), treated country vs synthetic control

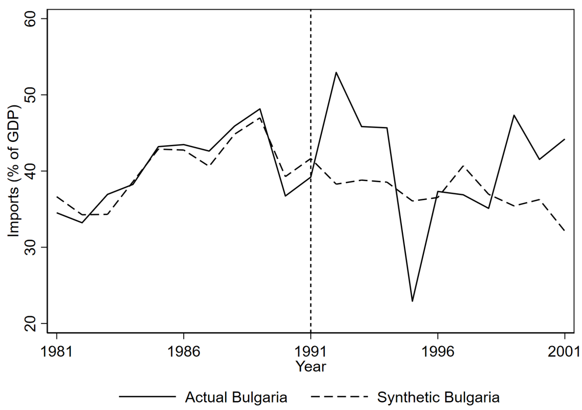
(a) Target: Yugoslavia



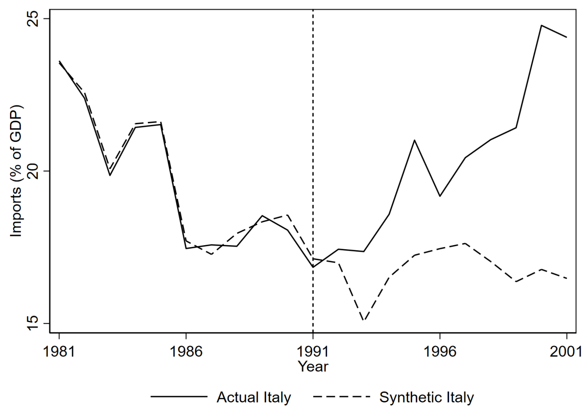
(b) Target: Yugoslavia



(c) Target: Yugoslavia



(d) Target: Yugoslavia



Source: authors' calculations based on data described in Section 2.

We now turn to the statistical significance of the results presented above. Because of space limitations, Table 4 only displays Chow tests for the four case studies presented in Figure 3 and reports the estimated gap and the p-values for each post-treatment year. Table A5 in the appendix reports these estimates for the remaining case studies. As shown in Table 4, we detect significant negative effects of the sanctions on imports in Turkey for most of the post-treatment period. These effects start materializing one year after the imposition of economic sanctions in Iran and turn to a positive effect after five years. Similarly, in Greece the estimated effect is negative for the first eight post-sanctions years, while it is positive in the final two years of the time window under analysis. The positive effect on Bangladesh's imports is significant over the full post-sanctions period except for the year after the sanctions, whereas the difference between Kenya's imports and its synthetic counterpart is significant in all post-sanctions years.

Table 4: Chow tests for Turkey, Greece, Bangladesh, and Kenya

Year	Gap	p-value		Gap	p-value
Turkey			Greece		
2012	-3.189	0.001	1992	-0.828	0.000
2013	-4.051	0.000	1993	-2.636	0.000
2014	-3.521	0.001	1994	-5.222	0.000
2015	-2.810	0.002	1995	-5.139	0.000
2016	-1.483	0.054	1996	-4.651	0.000
2017	1.555	0.045	1997	-6.234	0.000
2018	1.639	0.037	1998	-3.317	0.000
			1999	-1.077	0.000
			2000	4.605	0.000
			2001	2.907	0.000
F-test		0.000			0.000
Bangladesh			Kenya		
1992	0.499	0.391	1993	8.449	0.000
1993	2.387	0.002	1994	9.056	0.000
1994	1.697	0.013	1995	13.366	0.000
1995	4.298	0.000	1996	6.347	0.000
1996	2.870	0.001	1997	4.859	0.000
1997	1.747	0.012	1998	3.879	0.001
1998	2.132	0.004	1999	3.782	0.001
1999	3.036	0.000	2000	6.631	0.000
2000	2.243	0.003	2001	8.494	0.000
2001	3.704	0.000	2002	5.169	0.000
F-test		0.000			0.000

Note: dependent variable is share of imports to GDP.

Source: authors' calculations based on data described in Section 2.

Table 5 summarizes the results from the case-study analysis. The mean effect is the coefficient for the treatment effect (which takes on the value 1 when the country's land neighbours face economic sanctions, and 0 otherwise) in an equation where the dependent variable is the imports gap between the treated country and its artificial counterpart. We also report the p-value of this mean effect and the p-value of the Chow test, which corresponds to the one displayed at the bottom of each panel containing the individual Chow test. Finally, we report the standard error of the regression (SER), to show how well the pre-sanctions model fits the data.

Overall, the results in Table 5 indicate that the effects of sanctions on imports are insignificant for only four of the 15 case studies examined, namely Austria, Bulgaria, China, and Dominican Republic. Among the other 11 cases, we estimate a negative effect in two cases (Greece and Turkey), while a positive effect is found in nine experiments. Smuggling, by definition, is difficult to observe and very challenging to quantify. Although what happens on the border cannot be observed, we argue that, if during economic sanctions, neighbours of a target state suddenly increase their imports, we might at least suspect that the extra imports could be intended for the target. In these nine cases, the results would be consistent with this interpretation, i.e. cross-border smuggling by countries sharing the border with the target state. Although we cannot directly observe whether neighbours do import goods on behalf of the target or how much (if any) smuggling occurs, this approach can be used to help raise red flags for identifying *potential* non-compliers.

Table 5: Summary of results from case-study analysis on imports

Country	Mean effect	Mean effect (p-value)	Years	Chow test (p-value)	SER
Albania	15.443	0.038	1992-2001	0.000	2.131
Austria	-1.851	0.178	1991-2001	0.000	0.009
Bangladesh	2.237	0.000	1991-2001	0.000	0.527
Bulgaria	3.236	0.245	1991-2001	0.000	1.694
China	-0.342	0.735	1991-2001	0.066	1.578
Côte d'Ivoire	1.385	0.097	1992-2002	0.047	1.289
Dominican Rep.	0.476	0.755	1992-2002	0.000	1.100
Greece	-1.961	0.092	1991-2001	0.000	0.131
Guinea	7.832	0.005	1997-2007	0.000	1.647
Italy	3.557	0.001	1997-2007	0.000	0.255
Kenya	6.330	0.000	1992-2002	0.000	0.793
Pakistan	-3.214	0.001	2011-2018	0.013	1.307
Swaziland	25.243	0.000	1977-1987	0.000	3.544
Tunisia	7.310	0.000	1986-1996	0.000	1.272
Turkey	-1.366	0.101	2011-2018	0.013	0.638
Mean of means	4.288				

Note: SER is standard error of regression.
Source: authors' calculations based on data described in Section 2.

Figures A2 and A3 in the appendix illustrate the results from our case-study analysis using the export-to-GDP ratio. Overall, results confirm that economic sanctions have no homogeneous effects on the trade of non-target states. Table 6 shows the average effect on exports for each of the 15 case studies.[15] The effect in insignificant in three cases (Albania, Greece, and Kenya), positive and significant at conventional levels in nine cases (Austria, Bulgaria, China, Côte d'Ivoire, Dominican Republic, Guinea, Italy, Tunisia, and Turkey), and significantly negative in the remaining three cases (Niger, Pakistan, and Swaziland).

[15] Case studies for exports are the same as those for imports, with the exception of Niger which replaces Bangladesh. This is because Bangladesh displays a RMSPE larger than 2, failing to meet condition (b), while Niger reports a RMSPE smaller than 2 when the outcome variable is exports.

Table 6: Summary of results from case-study analysis on exports

Country	Mean effect	Mean effect (p-value)	Years	Chow test (p-value)	SER
Albania	0.428	0.773	1992-2001	0.000	2.024
Austria	2.724	0.048	1991-2001	0.000	0.368
Bulgaria	5.557	0.006	1991-2001	0.002	2.204
China	2.414	0.000	1991-2001	0.222	1.503
Côte d'Ivoire	9.899	0.000	1992-2002	0.001	2.567
Dominican Rep.	6.846	0.000	1992-2002	0.000	1.213
Greece	-0.325	0.726	1991-2001	0.006	1.047
Guinea	2.902	0.069	1997-2007	0.078	2.663
Italy	4.588	0.000	1997-2007	0.000	0.053
Kenya	-2.914	0.269	1992-2002	0.000	0.972
Niger	-9.360	0.000	1992-2002	0.000	1.422
Pakistan	-3.235	0.004	2011-2018	0.000	0.395
Swaziland	-8.730	0.039	1977-1987	0.003	3.620
Tunisia	4.963	0.000	1986-1996	0.004	1.666
Turkey	3.336	0.001	2011-2018	0.000	0.844
Mean of means	1.273				

Source: authors' calculations based on data described in Section 2.

Because of the clandestine nature of cross-border trafficking, estimating the impact of embargoes on trade is fraught with difficulties. Yet, the extra exports of neighbours in years of enforced embargoes do allow for some cautious inferences. If anything, we would expect an increase in exports as in the case of Dominican Republic, Kenya, or Tunisia, when neighbours of a sanctioned state can trade on its behalf by smuggling goods out of it. As with imports, the very existence and intent of smuggling would need to be further investigated.

5 Conclusions

What is the effect of economic sanctions on the trade flows of countries neighbouring the sanctioned state? Economic sanctions can be very costly to land neighbours due to the disruptions they inflict on their commerce. At the same time, anecdotal evidence suggests that economic sanctions can benefit neighbours by enabling them to engage in sanctions-busting activities as they trade on behalf of the target and smuggle goods across the border. To assess the merits of these competing arguments, we use two complementary approaches. First, we use a large-N panel data analysis on the full sample and on a restricted sample where the control group is constructed using information on countries under threats of sanctions, hence countries that could be sanctioned but are not. We also report event-study estimates of the effects of embargoes and correct for the presence of within-unit heterogeneity in the treatment effect. Second, we use the synthetic control method, a method to compare the post-event trajectory for the variable of interest with a weighted average of the values of that variable from a comparison group chosen on the basis of their pre-treatment similarity to the treated unit. This approach allows us to integrate and exploit complementarities between large-N and small-N approaches to research on the effects of economic sanctions.

Our panel data analysis reveals that on average economic sanctions regimes are followed by a decline in neighbours' imports or exports. As such, in the wake of economic sanctions, neighbouring countries appear to pay a cost. However, the only case where we detect a positive effect in the panel analysis is when we consider the intensity of the treatment in terms of shared border. Countries sharing a large proportion of their international border with sanctioned states report increasing levels of trade in the aftermath of sanctions. This is consistent with countries having more opportunities for smuggling when sharing a substantial section of their borders. Our analysis of nigh-time luminosity on the border of sanctioned states is also suggestive of a smuggling mechanism. But besides these two empirical results, on average, the panel data shows little evidence that countries will circumvent restrictions by diverting goods to neighbours of the target states.

The case studies based on the synthetic control method offer a more nuanced and intriguing picture and suggest that economic sanctions do not have an obvious unidirectional impact on trade of neighbouring countries. In three out of 15 cases, sanctions impose welfare costs on the neighbours of the target economy. In the remaining nine cases, we detect a sudden, arguably unmotivated, increase in imports or exports. In some of these cases, like Côte d'Ivoire, Guinea, or Swaziland, the increase in imports would be compatible with the presence of porous borders and lack of controls, which increase the possibility that neighbours of a sanctioned state can trade on its behalf by importing goods from the rest of the world. Again, this is in line with the previous results concerning borders discussed above.

Any research design, including those we rely on here, face difficulties in offering firm evidence of smuggling across borders or in detecting violators. In fact, our approach provides indirect evidence that can be used to raise red flags for identifying potential targets of investigation, and alternative mechanisms and dynamics could well explain why imports increase in countries sharing borders with target states. For one, the start date of the embargoes could coincide with other significant events such as the introduction of new policies that shape neighbours' trade If this is the case, a positive impact of embargoes on trade does not necessarily imply the presence of clandestine violations of embargoes through the diversion of goods to neighbours of the target states.

That being said, our findings contribute to informing the current debate on the effectiveness of control and verification systems. Ultimately, the application of more rigorous methods to more fine-grained data on trade can contribute to developing stronger monitoring and enforcement capabilities.

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Appendix

Table A1: Summary statistics

Variable	Mean	Std. dev.	Min.	Max.	N
Total trade (% of GDP)	84.29	48.21	13.75	437.33	4210
Imports (% of GDP)	45.37	25.11	0	208.33	4210
Exports (% of GDP)	38.92	26.41	3.04	228.99	4210
Neighbours under embargo	0.1	0.3	0	1	5020
Population (ml)	33.27	129.22	0.01	1371.22	4800
GDP per capita (000)	12.65	19.99	0.18	189.55	4618
GDP per capita growth (00)	0.02	0.06	-0.62	1.4	4622
Polity score	3.45	6.46	-10	10	4030
War dummy	0.28	0.45	0	1	4995

Source: authors' calculations based on data described in Section 2.

Table A2: Intensity of embargo and trade flows in the neighbourhood

	(1)	(2)	(3)	(4)
		Total trade		
Share of trade with embargoed states	-11.057***	-12.956***	-14.300***	-7.990*
	(1.866)	(4.754)	(5.352)	(4.419)
Population (ml)		0.047*	0.071***	0.075***
		(0.025)	(0.017)	(0.017)
GDP per capita (000)		1.398**	0.582	0.116
		(0.690)	(0.549)	(0.471)
GDP per capita growth (00)		5.121	-3.201	-2.888
		(9.699)	(9.992)	(10.906)
Polity score		0.615**	0.469*	0.324
		(0.262)	(0.276)	(0.348)
War dummy		4.068**	3.839**	3.719**
		(1.696)	(1.644)	(1.742)
Restricted sample	No	No	Yes	Yes
Restricted period	No	No	No	Yes
Observations	4210	3678	1751	1279

Note: two-way fixed-effects regressions in all specifications. Dependent variable 'Total trade' is expressed as % of GDP. In column 3 the sample only includes countries sharing borders with states under actual or threatened economic sanctions, thus comparing the trade flows of countries neighbouring states under threat of economic sanctions with the trade flows of countries neighbouring states under actual embargo. In column 4 the sample is further restricted to the five years before and after the embargo. The 'Share of trade with embargoed states' measures the average share of total trade with embargoed states over the five years before the embargo. Robust standard errors in parentheses are clustered by country. * p<0.10, ** p<0.05, *** p<0.01.

Source: authors' calculations based on data described in Section 2.

Table A3: Percentage of border with target states and trade Fflows in the neighbourhood

	(1)	(2)	(3)	(4)	(5)	(6)
	Total trade		Imports		Exports	
% of border in [1/3, 2/3]	-4.165	-35.061***	1.207	-15.084***	-5.372	-19.977***
	(10.436)	(2.759)	(7.596)	(1.411)	(3.640)	(1.568)
% of border $> 2/3$	3.853	8.332***	3.604	3.985***	0.249	4.346***
	(10.184)	(1.761)	(5.536)	(0.937)	(5.699)	(0.995)
Population (ml)	0.073***	0.074***	0.040***	0.039***	0.033***	0.036***
	(0.018)	(0.016)	(0.010)	(0.009)	(0.009)	(0.009)
GDP per capita (000)	0.495	0.087	-0.058	-0.242	0.553*	0.329
	(0.517)	(0.464)	(0.245)	(0.221)	(0.322)	(0.294)
GDP per capita growth (00)	-4.573	-4.720	-5.859	-7.168	1.286	2.448
	(10.444)	(10.728)	(6.727)	(6.754)	(5.744)	(5.302)
Polity score	0.511*	0.315	0.380**	0.240	0.131	0.075
	(0.272)	(0.347)	(0.147)	(0.176)	(0.163)	(0.209)
War dummy	4.389**	3.925**	2.265**	1.816*	2.124**	2.109**
	(1.708)	(1.743)	(1.097)	(0.998)	(0.823)	(0.909)
Restricted period	No	Yes	No	Yes	No	Yes
Observations	1751	1279	1751	1279	1751	1279

Note: two-way fixed-effects regressions in all specifications. Dependent variable 'Total trade' is expressed as % of GDP. The sample includes only countries sharing borders with states under actual or threatened economic sanctions, thus comparing the trade flows of countries neighbouring states under threat of economic sanctions with the trade flows of countries neighbouring states under actual embargo. In columns 2, 4, and 6 the sample is restricted to the five years before and after the embargo. Robust standard errors in parentheses are clustered by country. * p<0.10, ** p<0.05, *** p<0.01.
Source: authors' calculations based on data described in Section 2.

Table A4: Predictors and RMSPE: Turkey, Greece, Bangladesh, and Kenya

Predictor	Treated	Synthetic	Predictor	Treated	Synthetic
TURKEY			GREECE		
Imports 2001	22.689	22.585	Imports 1981	25.751	25.812
Imports 2002	22.877	23.611	Imports 1982	24.642	24.631
Imports 2003	23.219	23.051	Imports 1983	24.699	24.664
Imports 2004	25.201	24.296	Imports 1984	24.097	24.090
Imports 2005	24.266	25.182	Imports 1985	24.254	24.528
Imports 2006	26.368	26.254	Imports 1986	24.957	24.804
Imports 2007	25.961	25.403	Imports 1987	24.194	24.090
Imports 2008	26.982	27.945	Imports 1988	23.299	23.483
Imports 2009	23.417	23.966	Imports 1989	24.838	24.806
Imports 2010	25.500	25.387	Imports 1990	25.350	25.466
Imports 2011	30.310	29.502	Imports 1991	24.393	24.401
RMSPE		0.637	RMSPE		0.119
BANGLADESH			KENYA		
Imports 1981	14.116	14.909	Imports 1982	31.558	31.146
Imports 1982	15.532	15.133	Imports 1983	28.213	29.293
Imports 1983	14.713	13.699	Imports 1984	32.054	30.956
Imports 1984	13.415	13.807	Imports 1985	30.146	30.771
Imports 1985	12.838	13.400	Imports 1986	29.893	29.063
Imports 1986	11.836	11.537	Imports 1987	26.397	27.431
Imports 1987	11.698	11.804	Imports 1988	27.604	27.693
Imports 1988	12.250	12.062	Imports 1989	30.123	30.392
Imports 1989	12.785	13.013	Imports 1990	31.328	30.504
Imports 1990	13.058	13.273	Imports 1991	28.556	28.063
Imports 1991	12.227	12.276	Imports 1992	26.670	27.015
RMSPE		0.477	RMSPE		0.726

Synthetic Turkey: Cape Verde (0.003), Gabon (0.084), Comoros (0.274), Morocco (0.018), Bahrain (0.138), Japan (0.483).
Synthetic Greece: Cape Verde (0.034), Gambia (0.008), Senegal (0.093), Ghana (0.058), Bahrain (0.026), Philippines (0.096), Australia (0.677), Solomon Islands (0.009).
Synthetic Bangladesh: United States (0.096), Ecuador (0.105), Cape Verde (0.015), Japan (0.611), Philippines (0.012), Australia (0.162).
Synthetic Kenya: Jamaica (0.031), El Salvador (0.182), Gambia (0.062), Togo (0.102), Madagascar (0.04), Bahrain (0.042), Japan (0.489), Solomon Islands (0.051).

Source: authors' calculations based on data described in Section 2.

Table A5: Chow tests for remaining 11 case studies

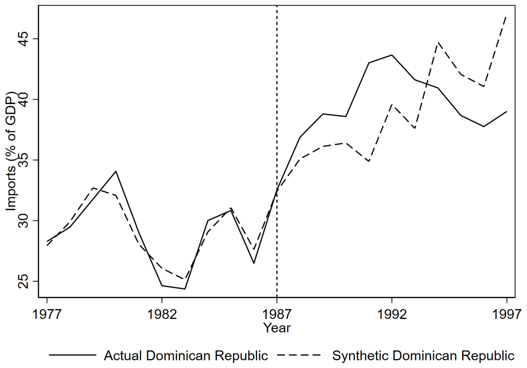
Year	Gap	p-value		Gap	p-value		Gap	p-value
Albania			Côte d'Ivoire			Pakistan		
1992	71.945	0.000	1993	-2.268	0.128	2012	-0.775	0.586
1993	37.691	0.000	1994	-2.440	0.105	2013	-3.009	0.056
1994	14.156	0.000	1995	2.189	0.140	2014	-5.528	0.003
1995	5.355	0.009	1996	1.226	0.388	2015	-5.075	0.005
1996	0.342	0.838	1997	3.962	0.017	2016	-4.475	0.010
1997	-2.842	0.113	1998	3.803	0.020	2017	-3.699	0.024
1998	1.819	0.291	1999	3.253	0.040	2018	-2.496	0.102
1999	1.418	0.404	2000	1.059	0.454			
2000	14.216	0.000	2001	1.931	0.187			
2001	14.995	0.000	2002	2.416	0.108			
F-test		0.000			0.047			0.013
Austria			Dominican Republic			Swaziland		
1992	-3.730	0.000	1988	1.859	0.142	1978	13.426	0.009
1993	-5.791	0.000	1989	2.750	0.041	1979	23.551	0.000
1994	-7.874	0.000	1990	2.251	0.083	1980	39.208	0.000
1995	-6.666	0.000	1991	8.201	0.000	1981	31.788	0.000
1996	-1.219	0.000	1992	4.148	0.006	1982	32.093	0.000
1997	-3.464	0.000	1993	4.077	0.006	1983	38.272	0.000
1998	-0.595	0.000	1994	-3.708	0.011	1984	31.887	0.000
1999	-0.294	0.000	1995	-3.315	0.018	1985	30.328	0.000
2000	4.014	0.000	1996	-3.233	0.021	1986	18.644	0.002
2001	5.258	0.000	1997	-7.987	0.000	1987	17.488	0.002
F-test		0.000			0.000			0.000
Bulgaria			Guinea			Tunisia		
1992	14.473	0.000	1998	3.000	0.116	1987	-1.681	0.239
1993	6.838	0.004	1999	2.696	0.153	1988	2.906	0.057
1994	6.946	0.004	2000	6.552	0.004	1989	8.171	0.000
1995	-13.333	0.000	2001	8.344	0.001	1990	10.336	0.000
1996	0.610	0.739	2002	6.375	0.005	1991	8.795	0.000
1997	-3.976	0.052	2003	2.129	0.249	1992	10.107	0.000
1998	-2.032	0.282	2004	3.284	0.090	1993	13.976	0.000
1999	11.716	0.000	2005	11.542	0.000	1994	11.204	0.000
2000	5.082	0.019	2006	21.125	0.000	1995	10.314	0.000
2001	11.903	0.000	2007	22.482	0.000	1996	6.855	0.001
F-test		0.000			0.000			0.000
China			Italy					
1992	1.471	0.397	1992	0.565	0.064			
1993	5.253	0.011	1993	2.425	0.000			
1994	2.501	0.165	1994	2.189	0.000			
1995	1.548	0.374	1995	3.895	0.000			
1996	0.067	0.969	1996	1.840	0.000			
1997	-2.934	0.110	1997	2.935	0.000			
1998	-3.001	0.103	1998	4.118	0.000			
1999	-2.744	0.132	1999	5.169	0.000			
2000	-3.072	0.097	2000	8.126	0.000			
2001	-1.963	0.266	2001	8.027	0.000			
F-test		0.066			0.000			

Note: dependent variable is 'Share of imports to GDP'.

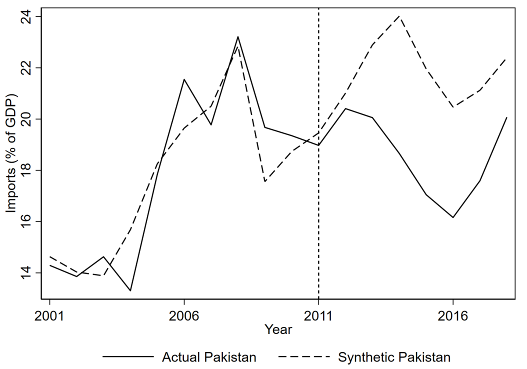
Source: authors' calculations based on data described in Section 2.

Figure A1: Trends in imports (as a % of GDP), treated country vs synthetic control

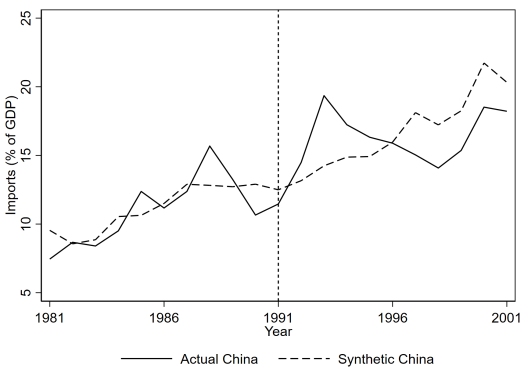
(a) Target: Haiti



(b) Target: Iran



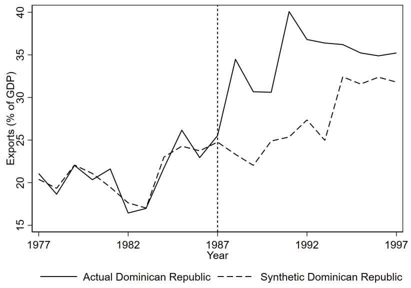
(c) Target: Myanmar



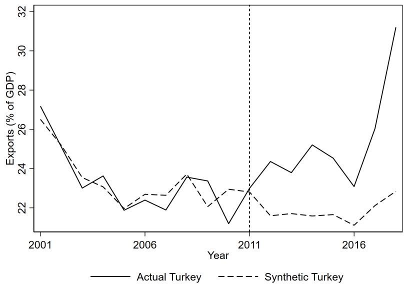
Source: authors' calculations based on data described in Section 2.

Figure A2: Trends in exports (as a % of GDP), treated country vs synthetic control

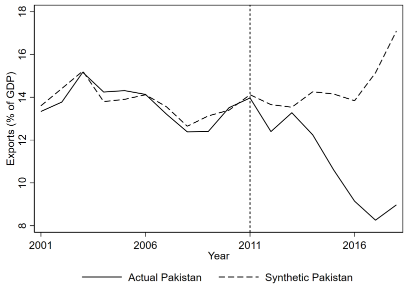
(a) Target: Haiti



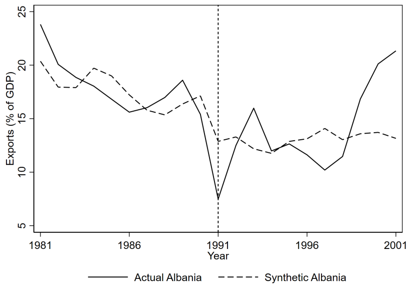
(b) Target: Iran



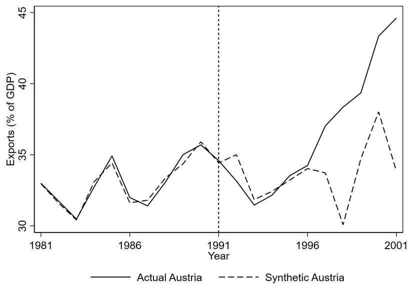
(c) Target: Iran



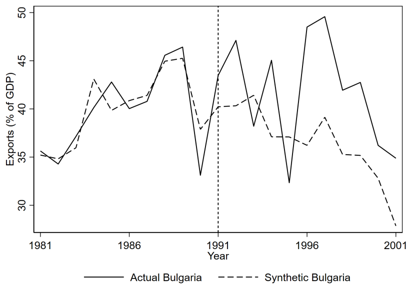
(d) Target: Yugoslavia



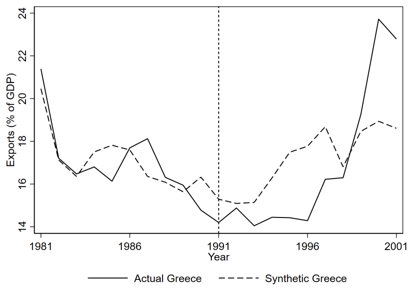
(e) Target: Yugoslavia



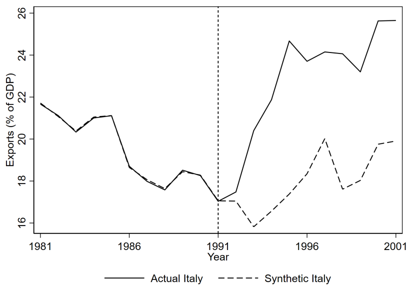
(f) Target: Yugoslavia



(g) Target: Yugoslavia



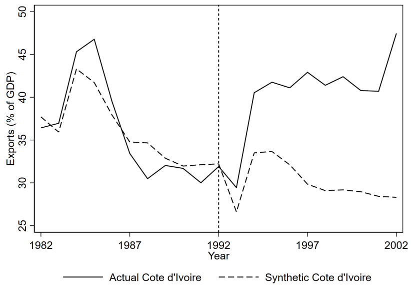
(h) Target: Yugoslavia



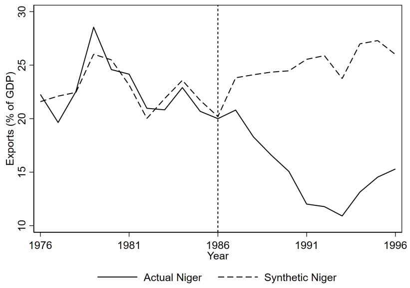
Source: authors' calculations based on data described in Section 2.

Figure A3: Trends in exports (as a % of GDP), treated country vs synthetic control

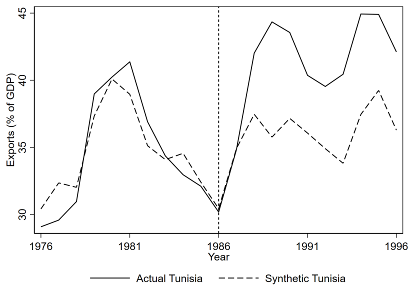
(a) Target: Liberia



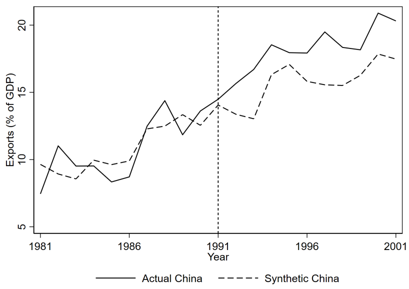
(b) Target: Libya



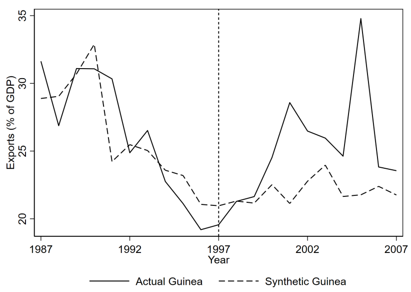
(c) Target: Libya



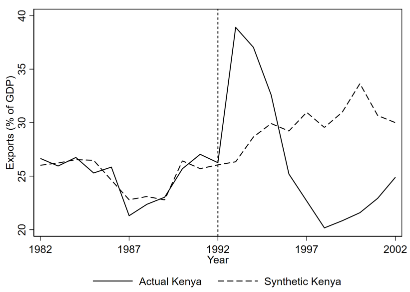
(d) Target: Myanmar



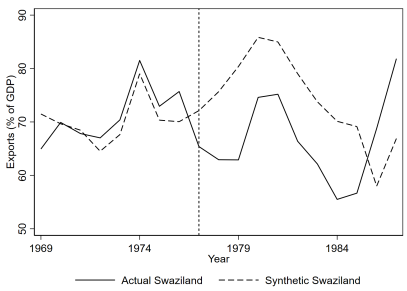
(e) Target: Sierra Leone



(f) Target: Somalia



(g) Target: South Africa



Source: authors' calculations based on data described in Section 2.

B1 Testing the smuggling hypothesis

In the paper, we propose smuggling as a mechanism through which neighbours of target countries circumvent sanctions. Neighbours can trade on behalf of sanctioned countries by smuggling goods in or out of the target's territory and exporting them to the rest of the world. The implication of our argument is that geographic contiguity enables this cross-border smuggling. While we cannot observe smuggling activities directly, we explore this mechanism using night-light emissions. Researchers have shown that night-light emissions can be used as reliable proxies for economic activity and development (Bruederle and Hodler 2018; Weidmann and Schutte 2017). If cross-border smuggling is occurring (or increasing) after the imposition of a sanction, we would expect night-light emissions to increase along the border as a consequence of increased activity in the area.

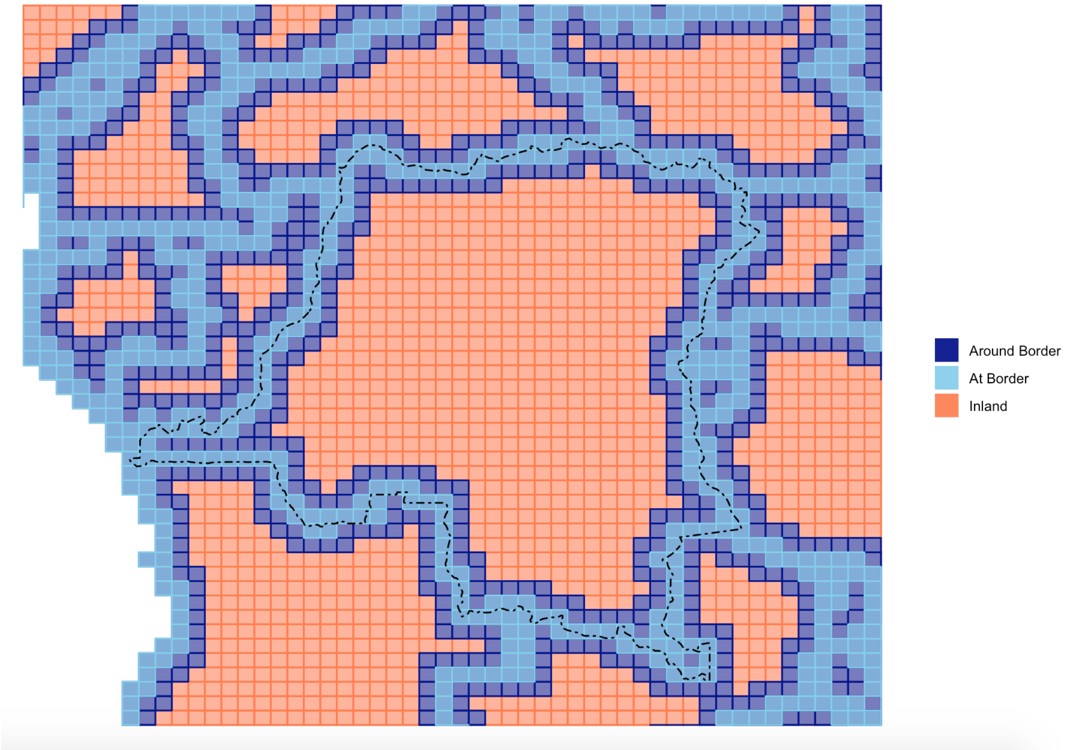
To test this mechanism, we rely on nigh-light data from the PRIO-GRID 2.0 (Tollefsen et al. 2012).[16] The PRIO data provides information on calibrated night-light emissions for 0.5x0.5 decimal degrees cells (approx. 55km x 55km at the equator) covering the entire globe from 1992 to 2013. The lights are calibrated 'to account for intersatellite differences and interannual sensor decay'.[17] Hence, each country in our sample is divided in grid cells. As a next step, we create two dummies to classify cells according to their geographic position. First, cells are classified as on the border if they intersect an international border (i.e. the border runs through the cells). Second, cells are classified as just off the border if they are adjacent to a cell that intersects the border but do not intersect the border themselves. We believe that these cells are an appropriate counterfactual, as they are likely to exhibit very similar features to cells that are just next to them, except they do not cross an international border. It follows that cells that are neither on nor just off the border are inland cells. An example of the final result based on the boundaries of the Democratic Republic of the Congo is depicted in Figure B1.

Our expectation is twofold. First, when comparing *all cells* in countries that are sanctioned with *all cells* in countries that are not sanctioned, we should expect an overall reduction in night light as a consequence of the negative effect that sanctions have on economic activities. This is consistent with what we document in our panel analysis. However, our second expectation is that, conditional on an economic sanction being imposed, luminosity of cells*on the border* of the targeted country will increase compared to those *just off the border*. We estimate models that include the same set of control variables as in our panel analysis with clustered standard errors at the cell level. We summarize our findings in Figure B2. When repeating the same analysis comparing cells on the border with all other inland cells, results are substantively similar. Each coefficient reports the effect of a different treatment, namely a total embargo being imposed in the previous year or any economic sanction being imposed in the previous year against a given country. Top panel in Figure B2 shows the results of a regression estimating changes in night-light emissions for all cells in the sample. We find that total embargoes are associated with a decrease in night-light emissions within all cells, compared to cells under no embargo. Pooling together economic sanctions, however, does not return a statistically significant coefficient. The bottom panel in Figure B2 uses cells in treated countries and compares emissions between treated cells on the border and treated cells just off the border. Consistent with our expectation, we find that cells on the border exhibit higher luminosity compared to those that are adjacent to them. It should be noted that the calibrated measure of night-light emissions ranges from 0 to 1, with a mean value of 0.05. The estimated coefficient (approx. 0.005) is thus somewhat small, although not negligible if we consider that nearby cells are unlikely to display strikingly different levels of night-light emissions.

[16] Image and data processing by National Oceanic and Atmospheric Administration (NOAA)'s National Geophysical Data Center. Data collected by US Air Force Weather Agency.

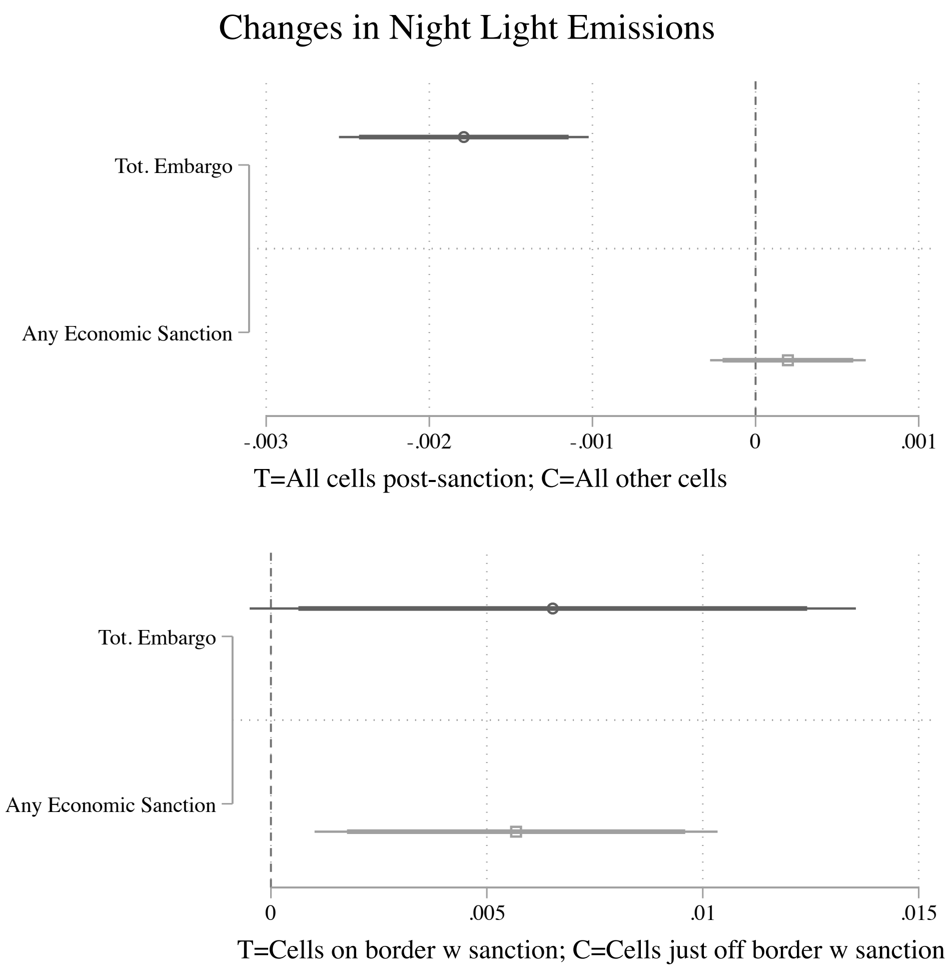
[17] See PRIO-GRID v.2 codebook available here: https://grid.prio.org/extensions/PRIO-GRID-Codebook.pdf.

Figure B1: PRIO grid cells - Democratic Republic of the Congo



Source: authors' illustration based on PRIO-GRID 2.0 (Tollefsen et al. 2012).

Figure B2: Coefficient plot for the effect of economic sanctions on night-light emissions



Note: 90% and 95% confidence intervals reported.

Source: authors' calculation based on data described in Appendix B1.