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ABSTRACT

Economic Sanctions and Trade Flows in the Neighbourhood*

We investigate the effect of economic sanctions on trade flows in countries sharing a border with sanctioned states. According to trade models, sanctions are expected to reduce trade flows as they disrupt established trading routes and economic relationships with suppliers and customers. However, there may also be instances where countries circumvent trade restrictions by clandestinely exchanging goods with sanctioned countries across the border and trading on their behalf, leading to an increase in imports and/or exports. To shed light on this issue, we employ a combination of large-N panel data analysis and comparative case studies using the synthetic control method. We find that, in the aggregate, neighbouring countries experience economic costs as sanctions disrupt trade. Yet, case studies uncover heterogeneity in countries' responses, with some cases exhibiting an increase in trade flows. We discuss possible explanations for these outcomes in the case-study analysis.

JEL Classification: F13, F14, F51, F52, K42

Keywords: economic sanctions, trade, sanctions-busting, land neighbours, smuggling, synthetic control method

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1 Introduction

When economic sanctions are imposed on a country to damage its economic activities, will these measures also hurt neighbouring countries by disrupting trade flows? Despite the extensive focus on the impact of sanctions on trade in targeted countries, we know much less about the potential unintended consequences of sanctions on countries that are not directly targeted. Economic sanctions are tool of coercive foreign diplomacy used by governments and international organisations to exert economic pressure on a target country to achieve a specific political or strategic goal (Eaton and Engers, 1992, 1999; Felbermayr et al., 2020; Anesi and Facchini, 2019).[1] Since 1966, the UN Security Council has established about 30 sanctions regimes "to support peaceful transitions, deter non-constitutional changes, constrain terrorism, protect human rights and promote non-proliferation".[2] 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 2011, which are considered exceptional in terms of severity, scope and non-discriminatory nature. In addition to the UN, the European Union (EU) and the United States (US) often impose economic sanctions as a response to violation of international norms of behaviours.

Numerous studies have explored the political (Marinov, 2005; Bapat et al., 2016; Gutmann et al., 2020; McLean and Whang, 2021, e.g,) and economic effects of sanctions on sanctioned states (e.g, Hufbauer et al., 1990; Caruso, 2003; Kaempfer and Lowenberg, 2007; Yang et al., 2009; Etkes and Zimring, 2015; Neuenkirch and Neumeier, 2016; Shin et al., 2016; Haidar, 2017; Gharehgozli, 2017; Afesorgbor, 2019; Kavaklı et al., 2020; Crozet and Hinz, 2020; Amodio et al., 2021; Moghaddasi Kelishomi and Nisticò, 2022; Ghomi, 2022), thus focusing on the behaviours and costs suffered by the target. Overall, extant studies show that, in the majority of cases, sanctions decrease the target's economic activities, although the effect largely depends on their comprehensiveness and multilateral nature.[3] While most evidence is derived from case studies, it can be challenging to extend the findings to a broader context (Özdamar and Shahin, 2021).

In this article, we study how *non-target states* react when their land neighbours are confronted with economic sanctions. Do they suffer economic costs? And do they respond with counter-measures to mitigate sanction costs? Theoretically, 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.

[1] In this article, we refer to sanctions and embargoes interchangeably.

[2] Available online: https://www.un.org/securitycouncil/sanctions/information

[3] In addition to their "intended" consequences, economic sanctions can increase infant mortality in the policy-targeted regions (Parker et al., 2016).

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). Sanctions should also hurt neighbours by disrupting trading routes and established ties with suppliers or customers (Slavov, 2007). For one, 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. 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, sanctions do not constrain states from engaging in "spoiler behaviours". States can openly violate sanctions by engaging in "sanctions-busting activities", i.e. by trading with the sanctioned country (Early, 2015; Bove and Böhmelt, 2021). In addition to the direct supply of goods to sanctioned states, countries can circumvent trade restrictions by clandestinely exchanging goods with sanctioned countries across the border. The porous nature of many international borders and geographic proximity 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. A recent example is China's illicit smuggling of oil into North Korea and the covert exports of coal from North Korean to China.[4] Cross-border trafficking is a thriving activity in many countries, especially in the absence of border controls (Slavov, 2007; Golub, 2015).[5] Crozet et al. (2021) find that exporting firms avoid the costs of economic sanctions by exporting indirectly to the target via neighbouring countries. 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 countervailing effects (trade disruption vs. the opportunity to profit from "sanctions-busting"), the net effect on imports and exports is not obvious. This is something that has to be determined from the data.[6]

Against this background, this paper seeks to provide a new approach to analyse 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

[4] Available online: https://www.nytimes.com/2021/03/24/world/asia/tankers-north-korea-china.html; https://www.theguardian.com/world/2020/apr/18/north-korea-defies-sanctions-with-chinas-help-un-panel-says

[5] 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; Moghaddasi Kelishomi and Nisticò, 2023).

[6] 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.

countries neighbouring a target state and examine the relation between trade patterns and embargoes using two-way fixed effects regressions, event-study analysis and the synthetic control method.

First, our panel data analysis reveals 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. Increased opportunities for smuggling given by the relative length of shared borders or the number of neighbouring countries seem to reduce the negative effect of sanctions on trade.

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 use the synthetic control method to compare the post-sanctions 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 degree of heterogeneity in the effect of embargoes. In three out of 13 cases, sanctions produce welfare costs. In seven cases, we detect a sudden, arguably unmotivated, increase in imports or exports. In the remaining three cases, we find no effect. In addition to the possibility of smuggling, we examine alternative reasons why trade may increase for neighbouring countries after sanctions are imposed.

This study adds to the most recent economic literature on the costs of economic sanctions (e.g., Crozet and Hinz, 2020; Moghaddasi Kelishomi and Nisticò, 2022; Ghomi, 2022; Felbermayr et al., 2021), but it also opens an avenue for research on trade policies that reduce economic integration by creating trade barriers and customs and imposing costs (Billmeier and Nannicini, 2013a; Goldberg and Pavcnik, 2016). One instance of this is Brexit, which has similarly resulted in a negative impact on the UK's trade with the EU (Dhingra and Sampson, 2022). This is all the more relevant for UK's closest neighbours, such as Ireland, which is uniquely exposed to Brexit due to a very high bilateral trade intensity (Copenhagen Economics, 2018; Arriola et al., 2018).

We proceed as follows: Section 2 gives a short description of the legal basis for sanctions carried out by the UN, EU and US. Section 3 presents the data. Section 4 describes the panel data analysis strategy and discusses the results. Section 5 outlines the synthetic control method and presents the implemented experiments. Section 6 provides concluding remarks.

2 Institutional background

Economic sanctions - the withdrawal of customary trade and financial relations with a target country - are imposed by governments or international organisations to achieve foreign policy or national security goals. Sanctions' popularity 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).

The UN, the European Union and the United States are the most important senders of sanctions. What are the legal frameworks for sanctions by these three actors? A very important sanctioning body is the UN Security Council (UNSC), which can take action to maintain or restore international peace and security by passing a resolution under Article 41 of the Charter. The latter allows for the use of non-military measures, ranging from comprehensive economic and trade sanctions to more targeted measures such as arms embargoes, travel bans, and financial or commodity restrictions. Sanctions resolutions need to receive a majority vote from the council's fifteen members in order to be approved, and none of the five permanent members – the United States, China, France, Russia, and the United Kingdom – may veto them. UN sanctions are legally binding on all UN member states, and once a resolution is adopted by the UNSC, all member states are expected to comply with and implement the economic measures imposed on the target. To monitor compliance of sanctions, the UNSC sets up special committees or monitoring groups and enlists global agencies and member states for assistance.[7] Yet, while trade restrictions are formally adopted by states, and despite the monitoring efforts, breaches of these are not unheard of, often driven by political and economic considerations. In fact, the willingness of third-parties to sanctions-bust on behalf of other countries has often undermined - at least partially - the effectiveness of the economic sanctions imposed against them. To illustrate, 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 the US and the UK - engaged in trade-based sanctions-busting on South Africa's behalf (Early, 2015).

Similar to the UN, the EU can also adopt economic and financial measures such as import and export restrictions to defend its strategic interests and protect its fundamental objectives abroad. The legal basis for economic sanctions by EU is primarily found in the EU's Common Foreign and Security Policy (CFSP). The Council makes all decisions to adopt, amend, lift, or renew sanctions after consulting with the relevant Council working

[7] Available online: https://www.un.org/securitycouncil/sanctions/information. For a detailed description of how UN sanctions work, see also Security Council Report (2013), a non-profit organisation that records the activities of the UNSC.

groups. The decisions adopted by the Council are binding on the Member States themselves.[8] Recent examples of EU economic sanctions are those imposed on Russia against its annexation of Crimea and involvement in the conflict in Eastern Ukraine, or those imposed earlier on Iran, North Korea, Syria, and Venezuela.

In the US, either the legislative branch or the executive branch may be the source of sanctions policy. Executive orders (EOs) that declare a national emergency are often issued by presidents in response to "unusual and extraordinary foreign threats", such as Russia's invasion of Ukraine.[9] The US Congress, on the other hand, has the authority to enact legislation imposing new sanctions or amending existing ones, as it has done in many cases. US economic sanctions are legally binding and enforceable within the US, and apply to foreign companies and individuals if they involve transactions with US persons or US-origin goods, or if they take place within the US. In the past, the US has punished banks and individuals that were suspected of violating its sanctions. Yet, their binding nature for other countries depends on the specific sanctions program, their direct participation and, more importantly, their willingness to implement and enforce them. In practice, once the US calls for sanctions, it often receives support from its allies. As with UN and EU sanctions, countries can invest in sanctions-busting to continue trading with the target. 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).

3 Data

The unit of analysis are neighbouring states (or neighbouring state-years) as we seek to investigate whether, all else equal, trade increases in states neighbouring a sanctioned country. 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 compiled information on sanctions in those states neighbouring the focal country of the monadic, country-year data set.

[8] Available online: https://www.eeas.europa.eu/eeas/european-union-sanctions_en#10705.

[9] Available online: https://home.treasury.gov/policy-issues/financial-sanctions/sanctions-programs-and-country-information. For the legal framework of US sanctions, see also Haass and Haass (1998).

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 GDP. *Total trade* is the sum of exports and imports of goods and services measured as a share of GDP. We also use information on trade, imports and exports in (log) levels, as well as data on merchandise trade and current account balance as a share of GDP also taken from the World Bank Development Indicators.

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 EU, UN and US. The dataset contains several categories of sanctions 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 exchanges (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 - i.e., actually imposed - as well as sanction threats. However, this comes at the expense of a relatively limited time frame ranging from 1989 to 2015. For this reason, in the second part of the analysis, we use a case-study, synthetic control approach where we integrate the EUSANCT Dataset with the recently released Global Sanctions Data Base (GSDB) (Felbermayr et al., 2020). The latter covers 729 publicly traceable multilateral, plurilateral and purely bilateral sanctions that were enforced over the 1950-2019 period. We focus on trade sanctions, defined as "measures that aim to restrain economic interactions with a target country by limiting international trade" (Felbermayr et al., 2020, p.6).[10] We restrict the sample to sanctions imposed by the EU, UN and US to ensure the comparability of the two analyses.

4 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:

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

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

where Y_{it} is trade 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 intrastate 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 A.1 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. Accordingly, Table 1 reports the estimated coefficients of the model in equation 1 for 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 9 percentage points in total trade for the neighbour of the embargoed targets (see second column of Table 1). A reduction of almost 5 and 4 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.

To get as close as possible to a plausible counterfactual of what would have happened in the absence of sanctions, we use to our advantage data on threats to implement sanctions short of the actual implementation. 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 ever under actual or threatened economic sanctions over the sample period. We thus compare imports/exports in countries that share borders with sanctioned states (treatment group) with imports/exports into neighbours of states in which sanctions were threatened but never imposed (control group).[11]

[11] Note that this setup deviates from the canonical DiD setup 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.

Table 1: Embargo and Trade Flows in the Neighborhood

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Trade		Imports		Exports	
Neighbors 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

Notes: 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

As a threat declares that sanctions are a possibility against the target state if, e.g., targets do not alter their behaviours, 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.[12]

Table 2 show the results when we restrict the sample to countries bordering targets of threatened or actually imposed sanctions. 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 over a narrow window of time. As emphasized by Neuenkirch and Neumeier (2015, p.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 sample period. The estimates in 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.[13]

[12] 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.

[13] Economic sanctions could have an impact on the international price of strategic commodities that

Table 2: Embargo and Trade Flows in the Neighborhood: restricted sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Trade		Imports		Exports	
Neighbors 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

Notes: Two-way fixed-effects regressions in all specifications. Dependent variables Total trade, Imports and Exports are 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 neighboring states under threat of economic sanctions with the trade flows of countries neighboring 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

4.1 Additional analyses

One important consideration is whether our results on the effect of sanctions on trade as a share of GDP are driven by their effect on the GDP itself. In other words, a decrease in trade flows could be attributed to an increase in the level of GDP. To counteract this, we use the actual levels of total trade, imports, and exports instead of their ratios to GDP. Our findings, presented in Appendix Table A.2, indicate that embargoes lead to a reduction in trade volumes, and this effect is statistically significant.[14]

Note also that the "smuggling hypothesis" pertain the illegal cross-border movements of goods, rather than services, but the data on trade from the World Bank include both goods and services. We thus replace the latter with data on merchandise trade - i.e., the sum of merchandise imports and exports - as a share of GDP. The results, displayed in Table A.3, column 3 and 4 indicate that embargoes cause a notable decrease in merchandise trade, which aligns with prior findings. Finally we ask whether the current account balance – i.e., a country's net trade in goods and services – as a share of GDP decreases

the embargoed country produces and exports, such as oil. This can potentially affect the terms of trade of other oil producers neighbouring the sanctioned country. The terms of trade measures the relationship between the prices of a country's exports and the prices of its imports. When controlling for terms of trade, our results are virtually identical to those reported in Table 2.

[14]Table A.3, column 1 and 2 similarly shows that there is no significant effect on GDP when it is used as the dependent variable instead of trade.

in the wake of embargoes as land neighbours may try to bypass the restrictions imposed by the sanctions and import goods on behalf of the targeted country, temporarily accumulating a current account deficit. Results, shown in Table A.3, columns 5 and 6 point towards a negative but insignificant effect of embargoes on the account balance.

One might argue that not all embargoes have the same impact, and the way countries react to embargoes in their neighbourhood also depends on the "intensity" of the relationship between the sanctioned targets and their neighbouring countries. To explore this aspect, we construct three measures of exposure to the embargo (or the intensity of the embargo). The first measure is based on the average share of a country's trade with the embargoed states relative to its total trade in the five years prior to the imposition of the embargo. More specifically, if a country has a total trade of 100 and all of it goes to a sanctioned neighbour, its value will be 1. If the country trades 100 globally but only 10 with two of its neighbours, and both neighbours are sanctioned, its value will be 0.1. The same logic applies to imports and exports. In our equation (1), we replaced the dummy variable Embargo_{it} with a dummy that takes the value 1 if the average share of trade with target states is above the mean, and zero otherwise. Results are available in Table A.4 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 share of trade with target states.

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 portion of border shared with a sanctioned country. For example, a country that only has one neighbour and that neighbour is sanctioned will have a value of 1. A country with a total border of 100 km that shares a 10 km border with a sanctioned country will have a value of 0.1. We then create two groups based on the portion of i's borders shared with countries under total economic embargoes: those below the median and those above the median. Results are reported in Table A.5 in the Appendix. We find that countries with smaller percentage of borders shared with embargoed states face a significant reduction in trade relative to countries sharing no border with embargoed states. This negative effect is also observed for countries with higher percentage of borders shared with embargoed states, but it is smaller in magnitude and statistically insignificant. This suggests that sharing a smaller portion of borders with embargoed countries may lead to more significant damage to trade. On the one hand, this could appear counterintuitive if the share of border is correlated with bilateral trade and embargoes damage economic activities. However, the correlation between the share of trade with neighbours and the relative length of shared borders is modest, around 0.16. On the other hand, one reason for this difference could be that larger shared borders provide more opportunities for

smuggling or finding alternative trade routes, thus reducing the overall economic impact of the embargo.

The third measure of intensity is based on the target's number of neighbouring countries and is used to examine to what extent it moderates the effect of sanctions on trade. If the targeted state has multiple neighbouring countries, there could be more opportunities for bypassing the sanctions and thus the effect on each neighbouring country might be smaller. We divide countries that have been subjected to embargoes into two groups based on the number of neighbouring countries. Our findings indicate that the negative effect of total embargoes on the trade of a neighbour is larger if the target's number of neighbours is below the median (i.e., 5 neighbours) than if it is above, although it is statistically significant only in the above median sub-sample (see Table A.6). This may suggest that being one of many countries sharing a border with a sanctioned state reduces the possibility of mitigating the negative effect on trade via, e.g., smuggling.

Related to the previous analysis of shared borders' length, we also provide an exploratory analysis that attempts to probe the smuggling mechanism in Appendix C.1. Leveraging data on global nightlight emissions between 1992 and 2013 using 55km x 55km grid cells from the PRIO-GRID v.2, 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 (Weidmann and Schutte, 2017; Bruederle and Hodler, 2018), 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.

4.2 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:

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

This is 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 4 pre- and 4 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 report event-study estimates of the effects of embargoes on neighbours' trade using the 4-year bandwidth. This specification allows us to check for pre-treatment trends and to explore whether the effects of embargoes become stronger or weaken 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 4 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 benefitting from embargoes imposed on their land neighbours. In line with previous results, there is no effect of sanctions on the GDP over time.

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.[15]

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. We again flexibly estimate

[15]The main point raised by De Chaisemartin and d'Haultfoeuille (2020) is that the classical twoway-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.

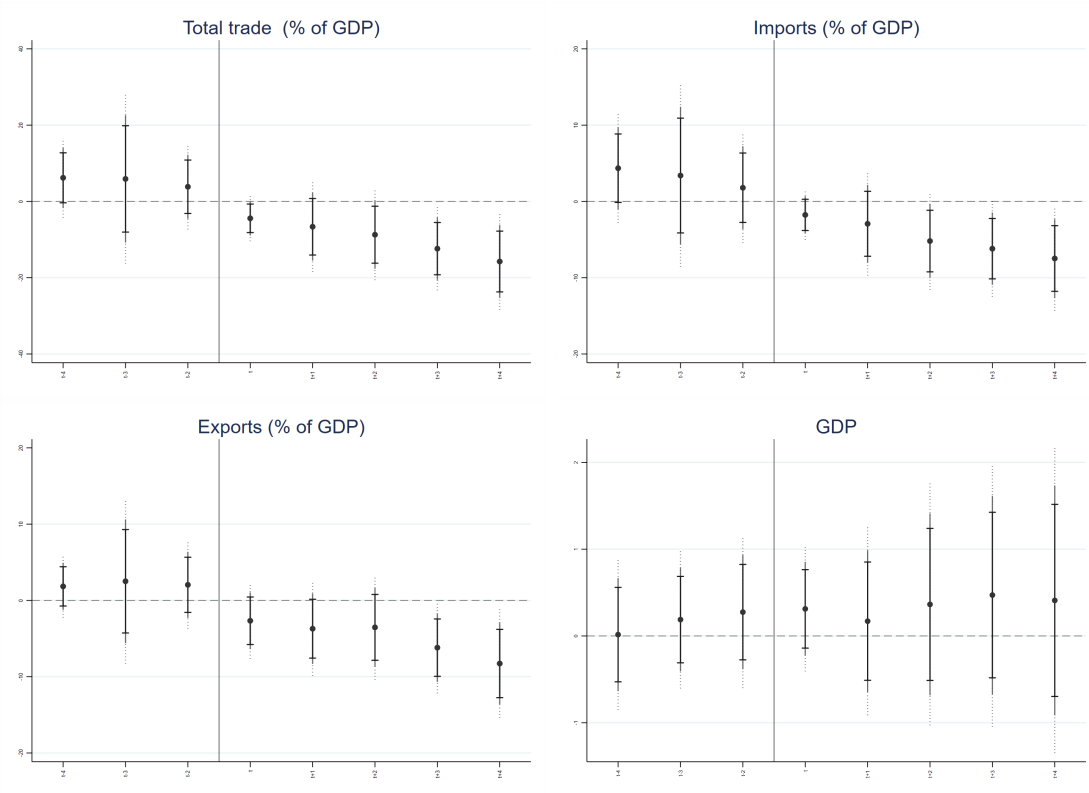
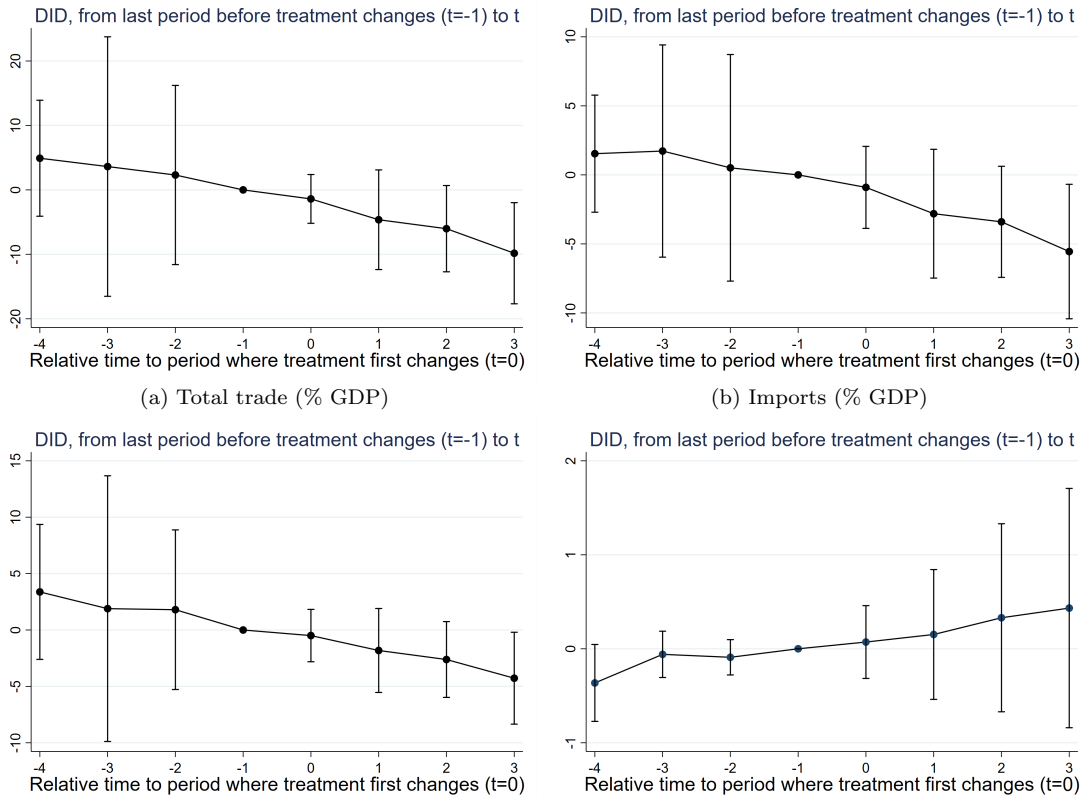


Figure 1: Event-Study estimates: Total Trade, Imports and Exports (as a % of GDP) and GDP



(a) Total trade (% GDP)

(b) Imports (% GDP)

(c) Exports (% GDP)

(d) GDP

Figure 2: De Chaisemartin & D'Haultfoeuille estimates: Total Trade, Imports and Exports (as a % of GDP) and GDP

the effects of embargoes before/after one, two, three, and 4 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 Figure 1; reassuringly, we again find no significant effects on GDP.

Taken together, the evidence in Figure 1 and 2 suggests that sanctions hurt the neighbours of the embargoed targets. Embargoes result into a decrease of at least 5 percentage points in imports (or around 11% of the average imports) and around 6 percentage points in exports (or around 14% of the average exports) for the neighbour of the embargoed targets. This negative effect of sanctions on neighbours appears weaker than on the target. Although our empirical strategy prevents us from demonstrating this difference systematically, we turn towards the current empirical scholarship for suggestive evidence of this observation. Afesorgbor (2019) finds that total economic embargoes reduce trade flows between sender and target countries by more than 80%. Our estimated coefficients would suggest a much lower average reduction in total trade of neighbouring countries with the rest of the world. To find effects of comparable scale we need to look at country-level analysis. For example, Crozet and Hinz (2020) find that the costs for the Russian Federation of the 2014 sanctions in terms of export losses amounted to around 7.4% of predicted total exports. Interestingly, our estimates are also of a comparable size to the predicted effect of Brexit on the economy of Ireland, a UK's land neighbour. By one estimate, increased trade costs will lower Irish imports and exports of goods and services by approximately 3-8% in 2030 (Copenhagen Economics, 2018).

That being said, qualitative evidence suggests that countries could violate embargoes, and have done so, by e.g., 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 nightlight emissions at the border in Appendix C.1). 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). As such, 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 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.

5 Case Studies

We use the Synthetic Control Method proposed by Abadie and Gardeazabal (2003), to complement the panel data analysis on the impact of trade sanctions. Section B.1 in the online Appendix describes the method in details. To 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 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 10-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;[16] (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 10-year period after the sanctions;[17] (d) in case of multiple and subsequent sanctions, we select the first one in chronological order; (e) as for some case studies borders can change over time, we include only countries with stable borders over the time window examined.

After this selection, we end up with a pool of 9 episodes of multilateral economic sanctions and a final set of 13 case studies, i.e., countries that share a border with one of the 9 embargoed targets. For each of the 9 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 9 episodes entail severe international economic sanctions, ranging from general trade restrictions to total blockades.

To illustrate the potential heterogeneity in countries' response to embargoes in their neighbourhood, Figure 3 shows four selected embargoed countries across different region of the world, one in Middle East (i.e. Iran), one in Europe (i.e. Yugoslavia), one in Latin America and the Caribbean (LAC) region (i.e. Haiti), and one in Africa (i.e. Somalia). Figure 3 plots the trends in imports and exports for the treated country and its synthetic control for the 4 neighbouring countries for which we observe the best pre-treatment fit, namely Turkey, Greece, Dominican Republic and Kenya, respectively.

[16] 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.

[17] With the exception of countries neighbouring Iran (where sanctions were imposed on 2011) for which we only have 7 years post-sanctions.

Table 3: List of case studies

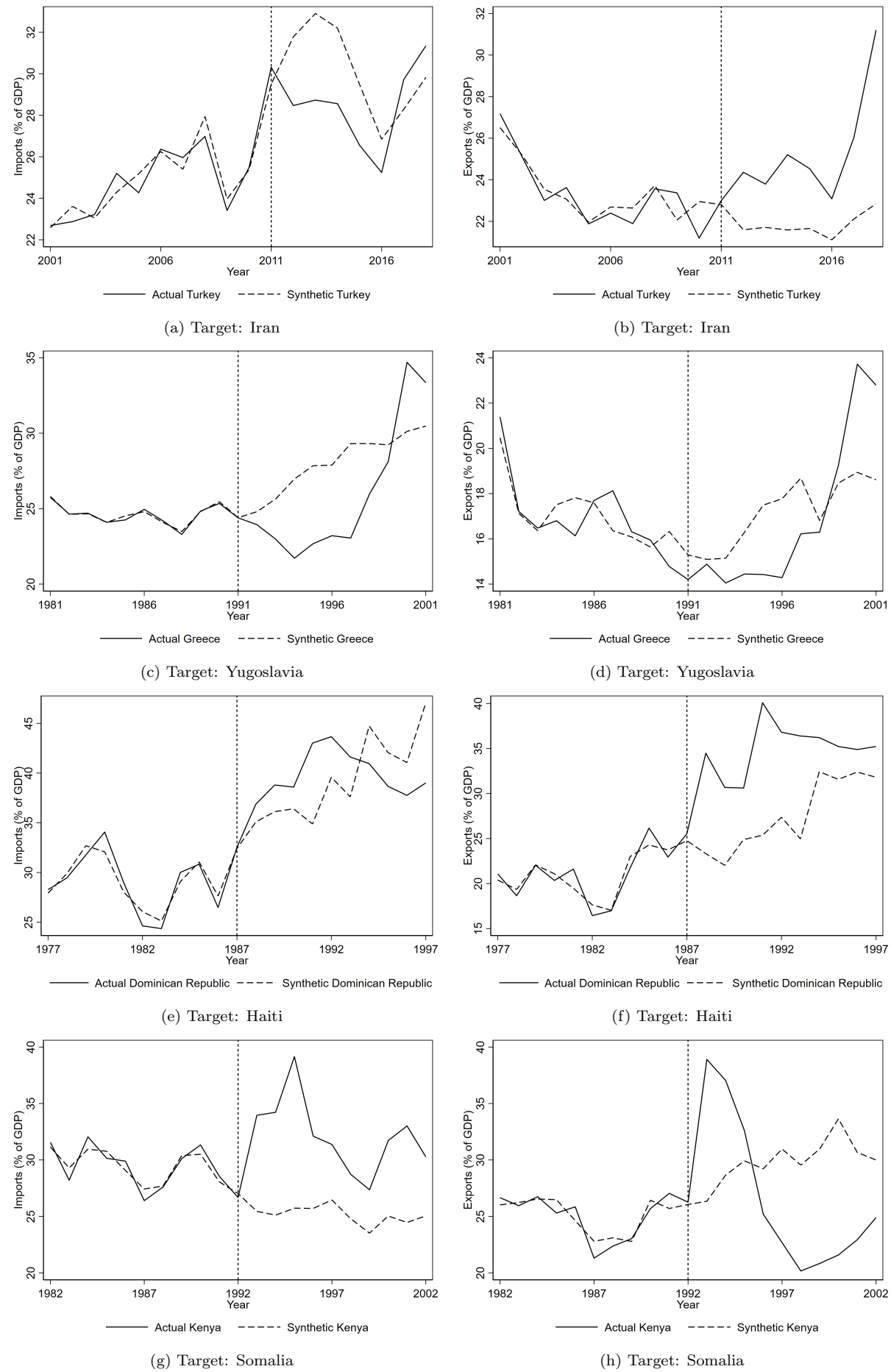
Target country	Start year	End year	Sender	Land neighbors
Iran	2011	ongoing	EU-UN-US	Turkey, Pakistan
Sierra Leone	1997	2003	EU-UN-US	Guinea
Liberia	1992	2001	UN	Cote d'Ivoire
Somalia	1992	2013	EU-UN-US	Kenya
Yugoslavia	1991	2001	EU-UN-US	Albania, Bulgaria, Greece
Myanmar	1991	2013	EU-UN-US	Bangladesh,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

The estimated effect on imports/exports is the difference between imports/exports (solid line) and in its synthetic version (dashed line) after the imposition of the sanctions in the neighbourhood. As shown in almost all case studies of Figure 3, the shares of imports and exports to GDP in the synthetic very closely track the trajectory of those in the treated countries for the full 10-year period before the sanctions.[18] This indicates that the synthetic provides a good approximation of the shares of imports and exports to GDP that we would have observed in the treated country in the post-treatment period in the absence of sanctions against its land neighbour.

A visual inspection of the discrepancies between solid and dashed lines in the 4 cases illustrated in Figures 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 and relations with the target. By contrast, countries like Dominical Republic or Kenya increase their imports as a response to the economic sanctions imposed on one of their land neighbours, suggesting cross-border trafficking as a possible explanation. This is particularly noticeable in Figures 4, where we zoom on 4 alternative case studies in the African continent, where we arguably expect more porous borders. In 3 out of 4 cases, countries seem to respond to sanctions by increasing imports or exports.

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 experienced a sharp increase in the share of imports immediately after the sanctions were imposed, in Bulgaria, after an increase in the aftermath of the sanctions, imports declined for the subsequent three years and increased again over

[18]Table B.1 in the Appendix reports comparisons of pre-treatment characteristics between synthetic and actual case study as well as the weights of each control country in the synthetic case studies.



(a) Target: Iran

(b) Target: Iran

(c) Target: Yugoslavia

(d) Target: Yugoslavia

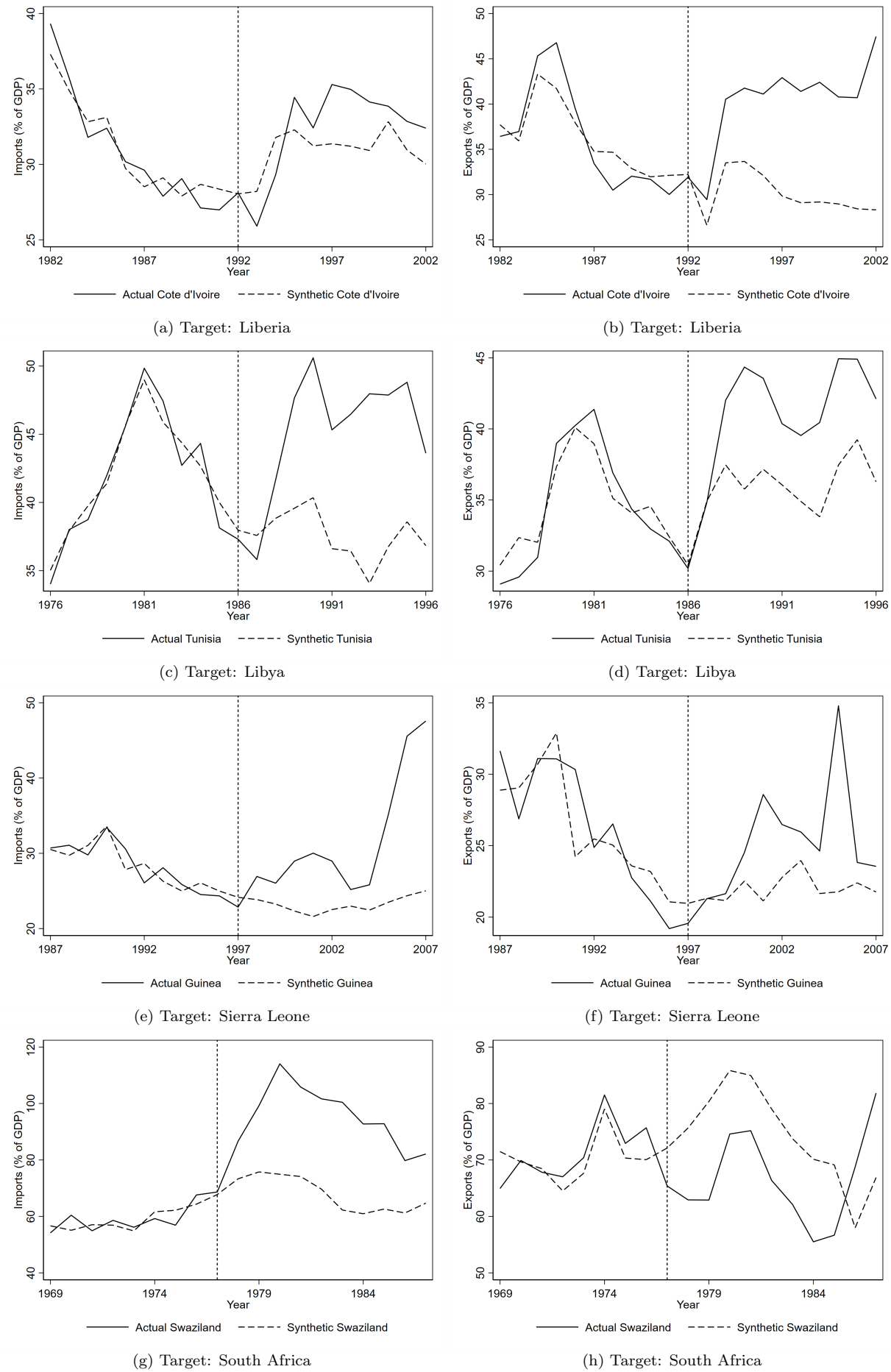
(e) Target: Haiti

(f) Target: Haiti

(g) Target: Somalia

(h) Target: Somalia

Figure 3: Trends in Imports and Exports (as a % of GDP), Treated Country vs. Synthetic Control



(a) Target: Liberia

(b) Target: Liberia

(c) Target: Libya

(d) Target: Libya

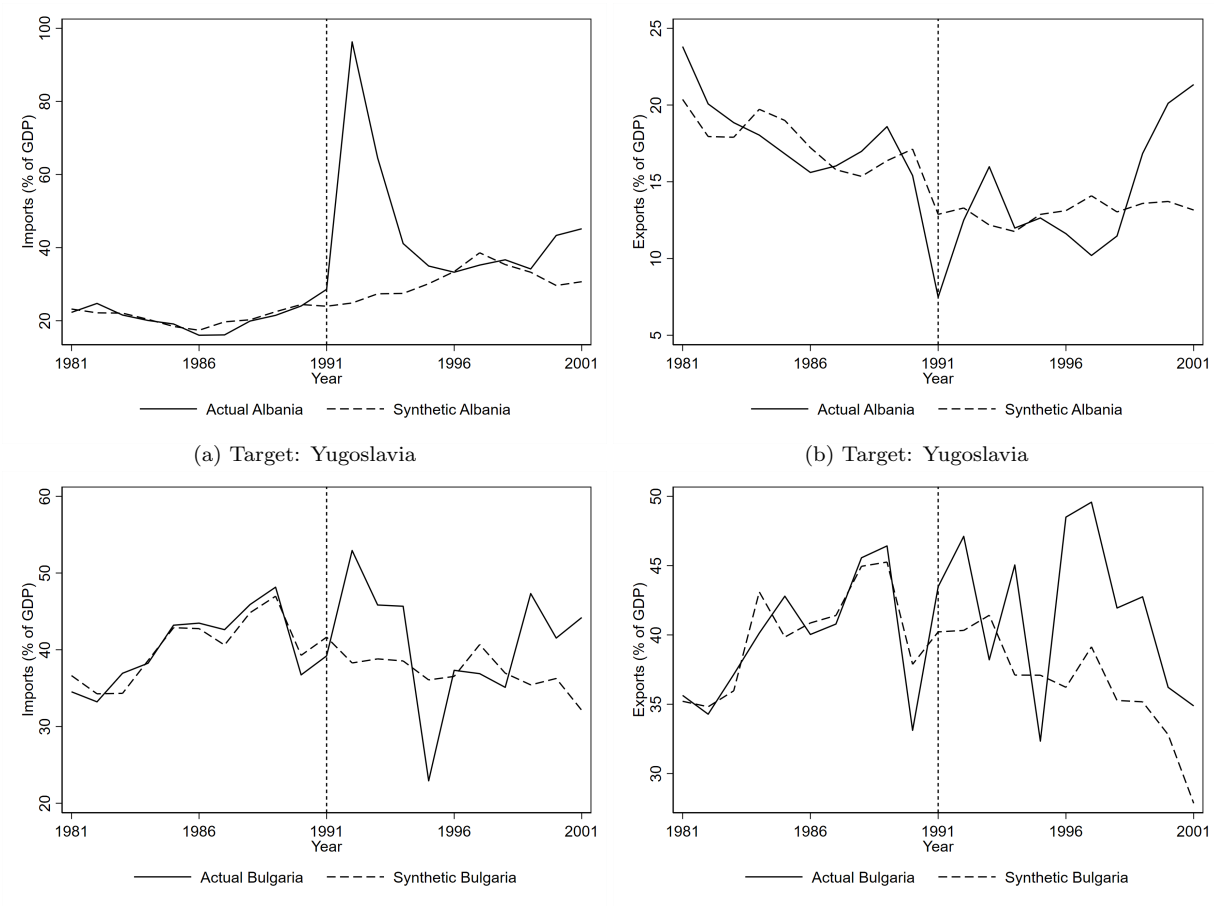
(e) Target: Sierra Leone

(f) Target: Sierra Leone

(g) Target: South Africa

(h) Target: South Africa

Figure 4: Trends in Imports and Exports (as a % of GDP), Treated Country vs. Synthetic Control



(a) Target: Yugoslavia

(b) Target: Yugoslavia

(c) Target: Yugoslavia

(d) Target: Yugoslavia

Figure 5: Trends in Imports and Exports (as a % of GDP), Treated Country vs. Synthetic Control

the following six years. However, less clear is the pattern for exports. Figure B.1 in the Appendix reports additional case studies hinting at countries reacting differently to sanctions against neighbouring states. Overall, results confirm that economic sanctions have no homogeneous effects on the trade of non-target states.

We now turn to the statistical significance of the results presented above. Because of space limitations, Table 4 displays Chow tests only for the 4 case studies presented in Figure 3 and reports the estimated gap and the p-values for each post-treatment year. Table B.2 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 materialising one year after the imposition of economic sanction in Iran, and turn to a positive effect after five years. Similarly, in Greece the estimated effect is negative for the first 8 post-sanctions years while it is positive in the final 2 years of the time window under analysis. By contrast, in Dominican Republic the effect is positive for the first 6 years (although significant only since the second year) and becomes negative over the last 4 years, whereas the difference between Kenya's imports and its synthetic counterpart is significantly positive in all post-sanctions years.

Table 5 summarises the results on imports 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 3 of the 13 case studies examined, namely Bulgaria, China, and Dominican Republic. Among the other 10 cases, we estimate a negative effect in 2 cases (Greece and Turkey), while a positive effect is found in 8 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 8 cases, the results would be consistent with this interpretation, i.e., cross-border smuggling by countries sharing the border with the target state.

Table 6 shows the average effect on exports for each of the 13 case studies.[19] The effect

[19] Case studies for exports are the same as those for imports, with the exception of Niger which replaces

Table 4: Chow tests for Turkey, Greece, Dominican Republic and Kenya. Dependent variable is Share of Imports to GDP.

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
Dominican Republic			Kenya		
1988	1.859	0.142	1993	8.449	0.000
1989	2.750	0.041	1994	9.056	0.000
1990	2.251	0.083	1995	13.366	0.000
1991	8.201	0.000	1996	6.347	0.000
1992	4.148	0.006	1997	4.859	0.000
1993	4.077	0.006	1998	3.879	0.001
1994	-3.708	0.011	1999	3.782	0.001
1995	-3.315	0.018	2000	6.631	0.000
1996	-3.233	0.021	2001	8.494	0.000
1997	-7.987	0.000	2002	5.169	0.000
F-test		0.000			0.000

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
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
Cote 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
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	4.816				

in insignificant in 3 cases (e.g., Albania, Greece and Kenya), positive and significant at conventional levels in 7 cases (e.g., Bulgaria, China, Cote d'Ivoire, Dominican Republic, Guinea, Tunisia and Turkey), and significantly negative in the remaining 3 cases (e.g., Niger, Pakistan and Swaziland).

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. Although we cannot observe directly whether neighbours do import or export goods on behalf of the target or how much (if any) smuggling occurs - the very existence and intent of smuggling would need to be investigated - this approach can be used to help raise red flags for identifying *potential* non-compliers.

5.1 Alternative explanations

Although smuggling is one strategy for sanctions avoidance, there are a number of plausible competing arguments that help explain why some neighbours of sanctioned state increase their trade in the wake of sanctions.

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
Bulgaria	5.557	0.006	1991-2001	0.002	2.204
China	2.414	0.000	1991-2001	0.222	1.503
Cote 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
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	0.906				

First, embargoes can create significant economic opportunities for neighbouring countries as global manufacturers need to relocate their production facilities out of the target. A notable example is the US-China trade war which saw some Asian countries like Vietnam and Malaysia benefiting from trade restrictions as they were already home to production facilities for big companies. These countries saw an opportunity to increase their exports to the US by ramping up production, profiting from their low labor costs and established supply chains (EUI, 2018; Kumagai et al., 2021). In a similar vein, Iraq's rivals took advantage of the disruption caused by the sanctions against Iraq as they enjoyed additional demand (Canes, 2000).

Second, the cost of imports - rather than the volume - can increase when neighbours are forced to establish new trading routes by switching suppliers after an embargo. An increase in the cost of imports can occur because of higher shipping distances, shifts in bargaining power with new suppliers and higher prices due to increased demand and limited supply. As such, the higher cost of imports would cause the prices of these goods to rise, leading to a corresponding increase in the share of GDP spent on these goods.[20]

Third, and related to the last point, when sanctioned country were supplying neighbours with inputs for local production, higher costs of inputs could make it more profitable to import the final good rather than producing it locally. This can result in a decline in

[20]Substitution tactics can incur substantial economic costs. To illustrate, South Africa lost 14 percent of its export revenue after switching trade partners in the wake of global sanctions (Kavaklı et al., 2020).

the competitiveness of the local production sector and a shift in economic activity towards import-oriented activities. If this is the case, we would observe an increase in the level of imports which is not driven by smuggling activities.[21]

Clearly, a neighbour's ability to reduce import costs is dependent on its ability to find alternative suppliers as well as the sanctioned country's comparative advantage in producing a specific good or commodity. Finding new suppliers becomes more difficult when the target has a significant comparative advantage in producing that good. Similarly, switching suppliers is easier if other suppliers around the world sell low-cost or high-quality versions of the relevant goods (Kavaklı et al., 2020). Assessing the merit of these different explanations would require fine-grained commodity-level data and information on input prices.

6 Conclusions

What is the effect of economic sanctions on the trade flows of countries neighbouring the sanctioned state? The significance of this question has increased in light of recent developments, in particular the restrictions imposed on Russia in March 2022 and the ongoing China-US trade war. Economic sanctions can be also very costly to land neighbours due to the disruptions they inflict on their trade. 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. Ultimately which effect dominates is an empirical question.

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 of countries 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, 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. This result is robust across modelling choices, alternative operationalisation of the dependent variable, alternative estimators, and more stringent definition of the counterfactual. As such, in the wake of economic sanctions, neighbouring countries appear to pay a cost. This finding aligns with recent empirical scholarship on sanctions, which finds that sanctions have a negative impact on

[21] We thank an anonymous reviewer for suggesting these alternative explanations.

the economy of the target. 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. Sanctions impose welfare costs on the neighbours of the target economy in only a few of the cases we consider (2 out of 13 for imports, 3 out of 13 for exports). In three cases, we find no significant effect. In the remaining cases (8 out of 13 for imports, 7 out of 13 for exports), we detect a sudden, arguably unmotivated, increase in imports or exports. In some of these cases, like Cote 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 (exporting) goods from (to) the rest of the world. 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. Yet, alternative mechanisms and dynamics could well explain why trade increase in countries sharing borders with target states and we discuss them in the article.

Our findings contribute to informing the current debate on the effectiveness of control and verification systems. Understanding the economic effects of these sanctions can inform future policy decisions, and provide insight into any unintended consequences of such actions. By conducting a thorough examination of the effects of economic sanctions, policymakers can make more informed decisions that balance the pursuit of foreign policy objectives with the impact on the global economy. At the same time, compliance with the sanctions requires coordination, and there is room for strategic behaviours. Ultimately, the application of more rigorous methods to fine-grained data on trade can contribute to developing stronger monitoring and enforcement capabilities.

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— Appendix —

Economic Sanctions and Trade Flows in the neighbourhood

A.1 Panel data analysis

Table A.1: 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
Log total trade	27.98	2.36	22.28	33.9	4191
Log imports	27.36	2.26	21.55	33.3	4190
Log exports	27.15	2.52	20.5	33.14	4191
Log GDP	19.71	2.57	12.77	37.42	4162
Current account balance (% of GDP)	-2.51	14.35	-240.52	311.76	3704
Mechandise trade (% of GDP)	62.28	36.52	7.81	575.61	4160
Neighbors under Embargo	0.1	0.31	0	1	4210
Population (ml)	37.1	137.5	0.01	1371.22	4207
GDP per capita (000)	11.89	17.11	0.2	111.97	4184
GDP per capita growth (00)	0.02	0.06	-0.62	1.22	4176
Polity score	3.66	6.46	-10	10	3708
War dummy	0.31	0.46	0	1	4210

Table A.2: Embargo and Trade in the Neighborhood: robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Trade		Imports		Exports	
Neighbors under Embargo	-0.185*	-0.216**	-0.178*	-0.199*	-0.203	-0.318**
	(0.105)	(0.102)	(0.096)	(0.108)	(0.135)	(0.142)
Population (ml)	0.005***	0.004***	0.005***	0.004***	0.005***	0.004***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
GDP per capita (000)	0.015	0.001	0.008	-0.005	0.022	0.006
	(0.013)	(0.009)	(0.011)	(0.007)	(0.016)	(0.012)
GDP per capita growth (00)	0.660**	0.333*	0.614**	0.310*	0.771**	0.439*
	(0.269)	(0.176)	(0.272)	(0.174)	(0.307)	(0.223)
Polity score	-0.004	-0.000	-0.002	0.001	-0.005	-0.001
	(0.006)	(0.007)	(0.006)	(0.007)	(0.008)	(0.008)
War dummy	0.055	0.068*	0.058*	0.067*	0.044	0.064
	(0.034)	(0.040)	(0.032)	(0.036)	(0.041)	(0.049)
Restricted period	No	Yes	No	Yes	No	Yes
Observations	1739	1272	1739	1272	1739	1272

Notes: Two-way fixed-effects regressions in all specifications. Dependent variables Total trade, Imports and Exports are expressed in logarithm. The sample includes only countries sharing borders with states under actual or threatened economic sanctions, thus comparing the trade flows of countries neighboring states under threat of economic sanctions with the trade flows of countries neighboring 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.3: Embargo and Trade Flows in the Neighborhood: additional results

	(1)	(2)	(3)	(4)	(5)	(6)
	GDP		Merchandise Trade		Current Account Balance	
Neighbors under Embargo	0.127	0.090	-9.897***	-7.242*	-1.558	-4.354
	(0.265)	(0.339)	(3.693)	(3.858)	(2.364)	(2.803)
Population (ml)	0.006**	0.003	0.032	0.027	0.005	0.006
	(0.003)	(0.003)	(0.022)	(0.018)	(0.006)	(0.005)
Polity score	0.041	0.054	0.262	-0.041	-0.137	-0.087
	(0.040)	(0.053)	(0.609)	(0.576)	(0.114)	(0.118)
War dummy	0.191*	0.230	3.629**	2.880*	-0.309	-0.490
	(0.114)	(0.143)	(1.592)	(1.695)	(0.811)	(0.729)
GDP per capita (000)			0.417	0.283	0.527***	0.422***
			(0.478)	(0.570)	(0.147)	(0.085)
GDP per capita growth (00)			9.967	27.959**	4.065	1.294
			(21.426)	(11.861)	(7.493)	(6.390)
Restricted period	No	Yes	No	Yes	No	Yes
Observations	1739	1272	1739	1272	1520	1121

Notes: Two-way fixed-effects regressions in all specifications. Dependent variables Current Account Balance and Merchandise Trade, are expressed as % of GDP, while GDP is expressed in logarithm. The sample includes only countries sharing borders with states under actual or threatened economic sanctions, thus comparing the trade flows of countries neighboring states under threat of economic sanctions with the trade flows of countries neighboring 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.4: Share of Trade with Target States and Trade Flows in the Neighborhood

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Trade		Imports		Exports	
Share of trade with target	-14.300***	-7.990*	-8.309**	-4.576**	-5.991**	-3.415
	(5.352)	(4.419)	(3.662)	(1.861)	(2.648)	(2.988)
Population (ml)	0.071***	0.075***	0.039***	0.039***	0.032***	0.036***
	(0.017)	(0.017)	(0.010)	(0.009)	(0.009)	(0.009)
GDP per capita (000)	0.582	0.116	-0.016	-0.230	0.598*	0.346
	(0.549)	(0.471)	(0.256)	(0.222)	(0.341)	(0.298)
GDP per capita growth (00)	-3.201	-2.888	-5.007	-6.543	1.806	3.655
	(9.992)	(10.906)	(6.968)	(6.907)	(5.143)	(5.308)
Polity score	0.469*	0.324	0.348**	0.242	0.121	0.082
	(0.276)	(0.348)	(0.149)	(0.176)	(0.164)	(0.209)
War dummy	3.839**	3.719**	2.009*	1.721*	1.830**	1.999**
	(1.644)	(1.742)	(1.026)	(0.993)	(0.822)	(0.915)
Restricted period	No	Yes	No	Yes	No	Yes
Observations	1751	1279	1751	1279	1751	1279

Notes: Two-way fixed-effects regressions in all specifications. Dependent variables Total trade, Imports and Exports are 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 neighboring states under threat of economic sanctions with the trade flows of countries neighboring 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.5: Share of Border with Target State and Trade Flows in the Neighborhood

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Trade		Imports		Exports	
Share of border below median	-14.586**	-12.661***	-7.410**	-6.412***	-7.176**	-6.250**
	(6.219)	(4.308)	(3.205)	(2.313)	(3.329)	(2.524)
Share of border above median	-7.339	-10.024	-3.668	-4.449	-3.671	-5.575
	(6.175)	(7.342)	(4.021)	(3.187)	(2.998)	(4.223)
Population (ml)	0.079***	0.075***	0.043***	0.039***	0.036***	0.036***
	(0.017)	(0.016)	(0.010)	(0.009)	(0.009)	(0.009)
GDP per capita (000)	0.582	0.099	-0.020	-0.237	0.602*	0.336
	(0.532)	(0.464)	(0.253)	(0.221)	(0.329)	(0.293)
GDP per capita growth (00)	-4.439	-2.031	-6.561	-5.971	2.123	3.941
	(10.318)	(10.693)	(6.505)	(6.777)	(5.627)	(5.194)
Polity score	0.477*	0.330	0.357**	0.246	0.121	0.084
	(0.272)	(0.346)	(0.150)	(0.175)	(0.161)	(0.209)
War dummy	3.671**	3.880**	1.951*	1.799*	1.720**	2.081**
	(1.615)	(1.742)	(1.040)	(0.996)	(0.802)	(0.911)
Restricted period	No	Yes	No	Yes	No	Yes
Observations	1746	1279	1746	1279	1746	1279

Notes: Two-way fixed-effects regressions in all specifications. Dependent variables Total trade, Imports and Exports are 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 neighboring states under threat of economic sanctions with the trade flows of countries neighboring 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. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.6: Embargo and Trade Flows in the Neighborhood: Heterogeneity by Number of Neighbours of Target States

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Trade		Imports		Exports	
Target's number of neighbours	Below median	Above median	Below median	Above median	Below median	Above median
Neighbors under Embargo	-12.674	-6.691**	-5.781	-3.399*	-6.893	-3.293**
	(8.146)	(2.799)	(3.922)	(1.787)	(4.322)	(1.467)
Population (ml)	0.622**	0.052***	0.277*	0.028***	0.345**	0.023***
	(0.294)	(0.015)	(0.156)	(0.009)	(0.155)	(0.008)
GDP per capita (000)	0.356	-0.912	-0.055	-1.649	0.411	0.737
	(0.625)	(2.244)	(0.295)	(1.367)	(0.364)	(0.931)
GDP per capita growth (00)	-14.997	13.021	-13.827	3.200	-1.171	9.821**
	(17.376)	(10.071)	(12.133)	(5.781)	(8.067)	(4.490)
Polity score	0.498	0.144	0.406	0.033	0.092	0.111
	(0.482)	(0.448)	(0.275)	(0.196)	(0.269)	(0.275)
War dummy	5.973**	0.740	2.294	0.610	3.679***	0.130
	(2.486)	(2.284)	(1.432)	(1.403)	(1.258)	(1.158)
Restricted period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	685	594	685	594	685	594

Notes: Two-way fixed-effects regressions in all specifications. Dependent variables Total trade, Imports and Exports are 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 neighboring states under threat of economic sanctions with the trade flows of countries neighboring states under actual embargo. In all columns the sample is restricted to the five years before and after the embargo. Robust standard errors in parentheses are clustered by country. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

B.1 Synthetic Control Method

We build on a simplified version of their approach, following Imbens & Wooldrige's notation.[22] 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 % 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.:

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

where $\|\|$ denotes a measure of distance. To determine the weights, we use all 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 variable and help mitigate the

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

endogeneity stemming from omitted variable bias.[23] 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 twenty-year time window so as to have ten-year pre-embargo data to calibrate the synthetic and ten-year post-embargo 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

[23] 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 fairly standard set of trade predictors such as real per capita GDP, population, total trade (as a % 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.

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.

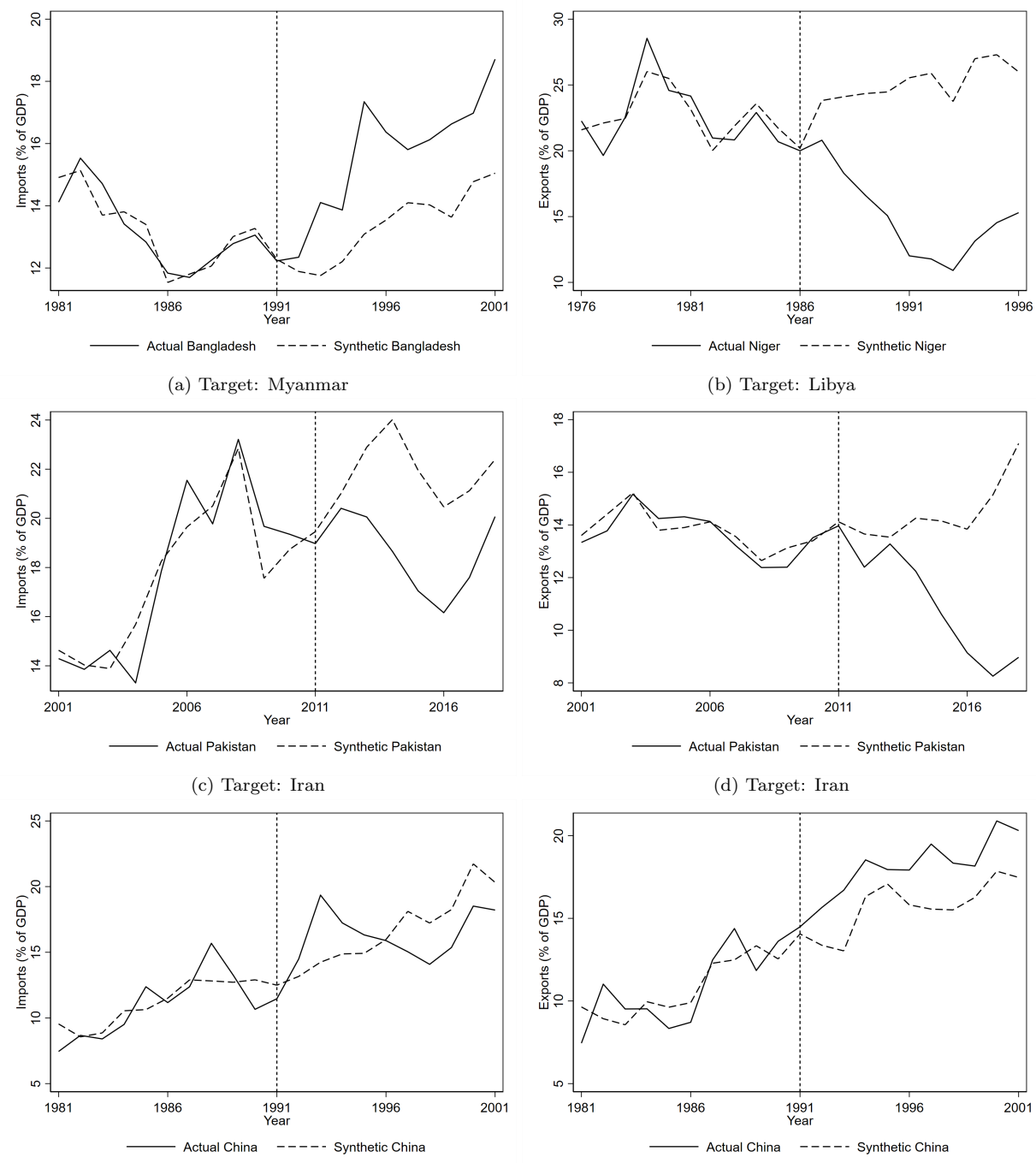
Table B.1: Predictors and RMSPE: Turkey, Greece, Bangladesh and Kenya

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
DOMINICAN REPUBLIC			KENYA		
Imports 1977	28.277	27.937	Imports 1982	31.558	31.146
Imports 1978	29.466	29.903	Imports 1983	28.213	29.293
Imports 1979	31.767	32.700	Imports 1984	32.054	30.956
Imports 1980	34.069	32.078	Imports 1985	30.146	30.771
Imports 1981	29.002	28.047	Imports 1986	29.893	29.063
Imports 1982	24.635	26.069	Imports 1987	26.397	27.431
Imports 1983	24.363	25.126	Imports 1988	27.604	27.693
Imports 1984	30.010	29.074	Imports 1989	30.123	30.392
Imports 1985	30.834	31.060	Imports 1990	31.328	30.504
Imports 1986	26.486	27.637	Imports 1991	28.556	28.063
Imports 1987	32.531	32.412	Imports 1992	26.670	27.015
RMSPE		0.998	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 Dominican Republic: Jamaica (0.072), Guinea-Bissau (0.094), Ghana (0.227), Malawi (0.166), Madagascar (0.115), Mauritius (0.118), Sri Lanka (0.02), Philippines (0.188).
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).

Table B.2: Chow tests for remaining 9 case studies. Dependent variable is Share of Imports to GDP.

Year	Gap	p-value		Gap	p-value		Gap	p-value
Albania			Cote 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
Bangladesh			China			Swaziland		
1992	0.499	0.391	1992	1.471	0.397	1978	13.426	0.009
1993	2.387	0.002	1993	5.253	0.011	1979	23.551	0.000
1994	1.697	0.013	1994	2.501	0.165	1980	39.208	0.000
1995	4.298	0.000	1995	1.548	0.374	1981	31.788	0.000
1996	2.870	0.001	1996	0.067	0.969	1982	32.093	0.000
1997	1.747	0.012	1997	-2.934	0.110	1983	38.272	0.000
1998	2.132	0.004	1998	-3.001	0.103	1984	31.887	0.000
1999	3.036	0.000	1999	-2.744	0.132	1985	30.328	0.000
2000	2.243	0.003	2000	-3.072	0.097	1986	18.644	0.002
2001	3.704	0.000	2001	-1.963	0.266	1987	17.488	0.002
F-test		0.000			0.066			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



(a) Target: Myanmar

(b) Target: Libya

(c) Target: Iran

(d) Target: Iran

(e) Target: Myanmar

(f) Target: Myanmar

Figure B.1: Trends in Imports and Exports (as a % of GDP), Treated Country vs. Synthetic Control

C.1 Night light emissions

In the paper, we propose smuggling as a mechanism through which neighbours of target countries circumvent sanctions. neighbours can trade on the 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 (Weidmann and Schutte, 2017; Bruederle and Hodler, 2018). 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 nighlight data from the PRIO-GRID 2.0 (Tollefsen et al., 2012).[24] 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".[25] 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 the border nor just off of it are inland cells. An example of the final result based on the boundaries of the Democratic Republic of the Congo is depicted in Figure C.1.

[24] Image and data processing by NOAA's National Geophysical Data Center. Data collected by US Air Force Weather Agency.

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

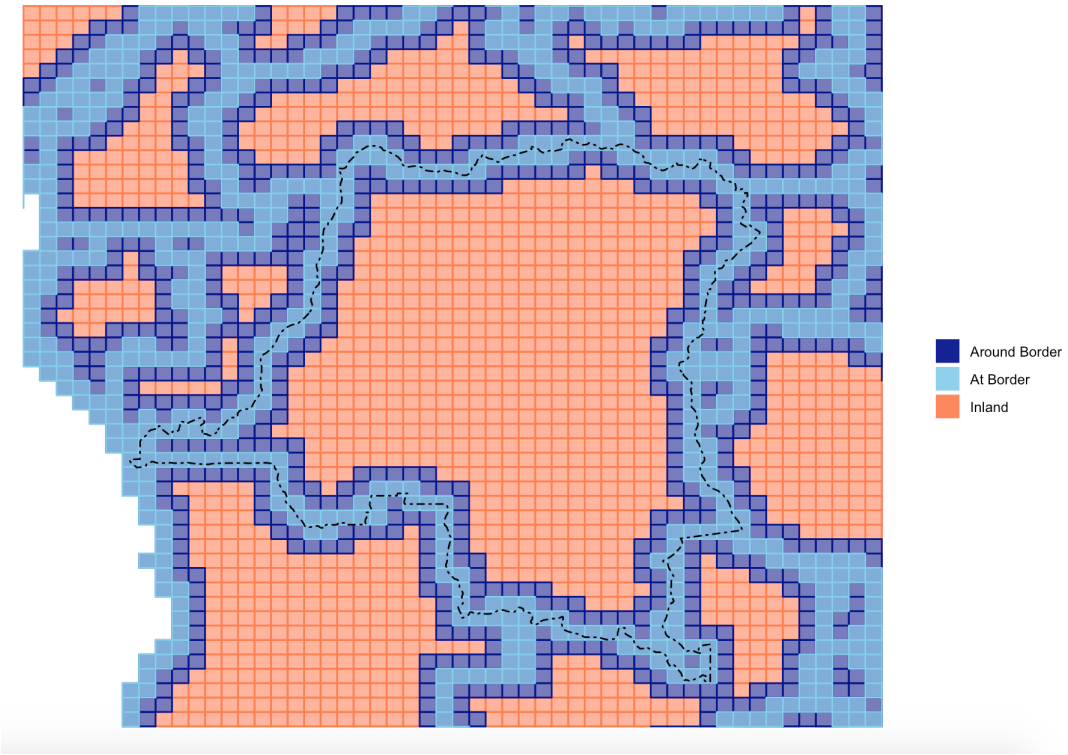


Figure C.1: PRIO grid cells - Democratic Republic of the Congo

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 C.2. 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 C.2 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 C.2 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.

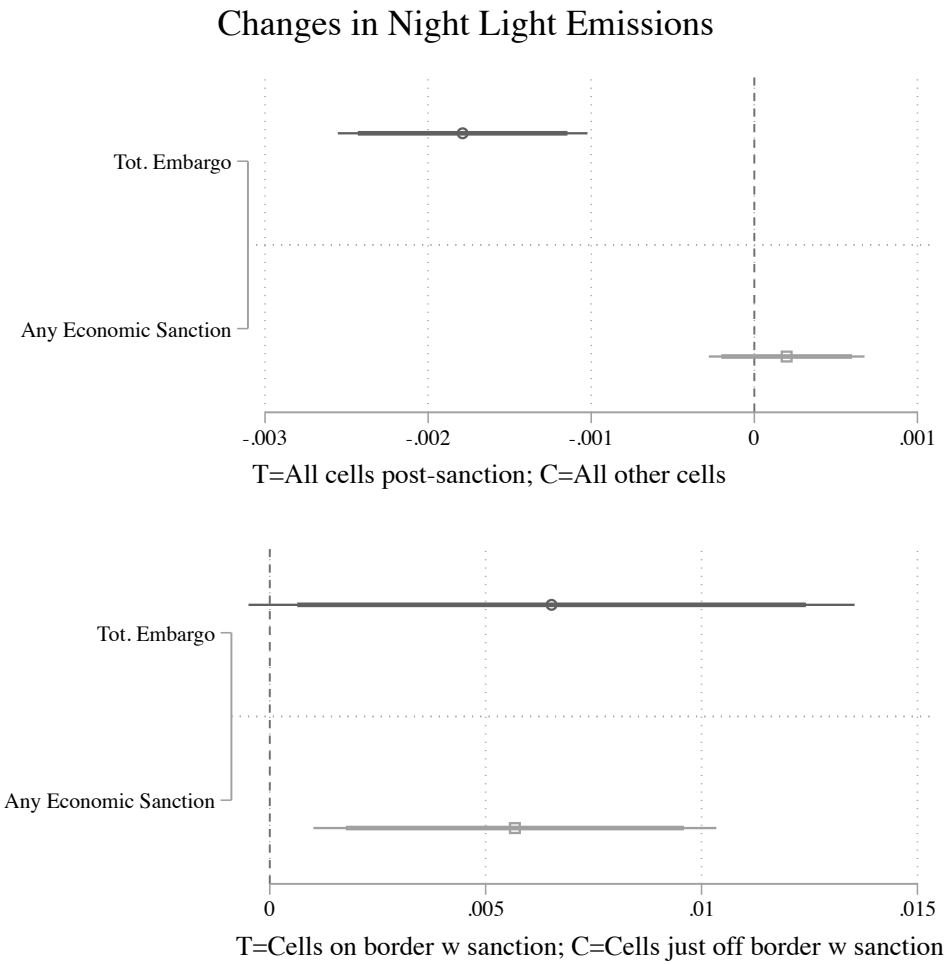


Figure C.2: Coefficient Plot for the effect of economic sanctions on night light emissions. 90% and 95% confidence intervals reported.