CESifo WORKING PAPERS

10086
2022

November 2022

The Effects of Heterogeneous Sanctions on Exporting Firms — Evidence from Denmark

Ina C. Jäkel, Søren Østervig, Erdal Yalcin

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 10086*

The Effects of Heterogeneous Sanctions on Exporting Firms —Evidence from Denmark

Abstract

Sanctions encompass a wide set of policy instruments restricting cross-border economic activities. In this paper, we study how different types of sanctions affect the export behaviour of firms to the targeted countries. We combine Danish register data, including information on firm-destination-specific exports, with information on sanctions imposed by Denmark from the Global Sanctions Database. Our data allow us to study firms' export behaviour in 62 sanctioned countries, amounting to a total of 453 country-years with sanctions over the period 2000-2015. Methodologically, we apply a two-stage estimation strategy to properly account for multilateral resistance terms. We find that, on average, sanctions lead to a significant reduction in firms' destination-specific exports and a significant increase in firms' probability to exit the destination. Next, we study heterogeneity in the effects of sanctions across (i) sanction types and sanction packages, (ii) the objectives of sanctions, and (iii) countries subject to sanctions. Results confirm that the effects of sanctions on firms' export behaviour vary considerably across these three dimensions.

JEL-Codes: F510, F130, F140, F520.

Keywords: sanctions, firm exports, trade margins.

Ina C. Jäkel
Aarhus University / Denmark
inaj@econ.au.dk

Søren Østervig
Aarhus University / Denmark
s.ostervig@econ.au.dk

Erdal Yalcin
Konstanz University of Applied Sciences
(HTWG), Konstanz / Germany
erdal.yalcin@htwg-konstanz.de

November 4, 2022
Søren Østervig and Ina Jäkel gratefully acknowledge financial support from the Carlsberg Foundation. Maria Løkkegaard Laursen has provided excellent research assistance.

1 Introduction

Economic sanctions have a long and popular history in serving as coercive measures to address political tensions between nations. By restricting cross-border economic activities, sanctioning states (sender countries) try to impose economic costs on the adversaries (target countries). In the last century, particularly before World War II, trade restrictions and comprehensive economic blockades represented the dominant sanction tools. Today, in a more integrated and globalized world, sanctions are imposed in various additional forms including international financial restrictions, travel bans, trade sanctions for specific goods, annulment of military assistance, and the isolation of airports and harbors, among others.

Together with the proliferation of diverse types of sanctions, the number of countries being targeted by sanctions has also increased significantly over the last decades. This increase has not only affected the countries that have been targets of sanctions, it also has implications for firms in the sender countries. In Denmark, for example, as much as 40 percent of exporters were active in at least one sanctioned country in the year 2015. How do firms in sender countries respond to sanctions in their export destinations? Are these effects heterogeneous across sanction types, political objectives, and target countries? To answer these questions, we combine firm-level register data from Denmark with information on sanctions imposed on 62 target countries over the period 2000–2015 from the Global Sanctions Database (GSDB; see Felbermayr et al., 2020a).

We start our analysis by estimating the overall effect of sanctions on firms' export behaviour. We find large negative and significant effects on firms' export to sanctioned countries: On average, firms reduce exports to sanctioned countries by 9 percent. In addition, we see a significant increase in firms' probability to exit the destination market. With these figures as a benchmark, we study heterogeneity in the effects of sanctions across *(i)* sanction types and sanction packages; *(ii)* the objectives of sanctions; and *(iii)* countries subject to sanctions.

The GSDB allows us to distinguish six different types of sanctions. We find that financial, trade, and travel sanctions affect firms' export behaviour[1], but that their estimated effects vary across estimators (OLS vs. PPML) and across firm-level outcomes considered (export values vs. firms' market exit probability). In an OLS specification for firms' destination-specific exports, financial sanctions show the strongest average trade-reducing effect, with 13 percent. The PPML estimator, instead, shows statistically significant and economically large negative effects for trade

[1] We also find some evidence that arms sanctions significantly increase firms' exit probability, but such sanctions are revealed to have a surprisingly positive effect on firms' exports in our PPML specification.

sanctions only, amounting to a reduction in firms' export of 25 percent. Finally, travel (but not trade or financial) sanctions are revealed to have a positive effect on firms' probability to exit a destination market. We offer two explanations for these somewhat contradictory results: first, OLS and PPML use different moment conditions for estimation; second, financial sanctions are often implemented jointly with trade and/or travel sanctions as a 'sanction package'.

The effects of trade, travel, and financial sanctions on firms' export behaviour are striking because in our sample period these sanctions often put only mild legal restrictions on international commerce. In particular, travel bans and financial sanctions are often designed to affect only a small group of stakeholders (e.g., travel ban for diplomatic staff, freezing of assets of specific individuals). Thus, unless an exporting firm has business contacts with this specific group of stakeholders, it is in principle possible to continue its export operation in the sanctioned countries. Similarly, almost all trade sanctions in our sample period are partial trade sanctions, only targeting very specific sectors or products. Our results suggest that such sanctions (sometimes referred to as 'smart sanctions') can still have large effects on the export behavior of the average firm. We discuss three possible mechanisms to explain this finding: first, sanctions impose additional information costs on exporting firms (e.g., to ensure compliance with the sanctions); second, even mild sanctions can lead to an increase in market uncertainty (e.g., because of the possibility of further sanctions being imposed in the future); third, some firms self-impose restrictions on their exports to sanctioned countries even where they are not legally obliged to (e.g., as part of their CSR (corporate social responsibility) strategy).[2]

As a second source of heterogeneity in the effects of sanctions, we consider the political motivation for introducing them. Sanctions with the objectives *prevent war*, or aiming at *policy change*, or *territorial conflict* have a significant negative impact on firm-level exports and increase the probability of market exit. Interestingly, sanctions with the objectives *democracy* and *human rights* – two of the most common objectives in our sample period – do, in contrast, not affect firms' export behaviour. These results indicate that policy objectives play an important role for how strong sanction policies are structured and executed.

Finally, we investigate the heterogeneity in the effects of sanctions across the countries that have been subject to sanctions. Our empirical results confirm that such heterogeneity is important. In approximately half of the country cases, sanctions lead to a significant drop in firm-level

[2]This interpretation is in line with Crozet and Hinz (2020), who find that trade sanctions against Russia had large effects even on non-targeted products, an effect they dub 'friendly fire'.

exports to the specific destination, but these estimates differ markedly in size. For the remaining countries, we estimate either insignificant or positive and significant effects. The latter cases are predominantly countries in war conditions with partial sanction policies. These results are consistent with the heterogeneity in the effects of trade sanctions across objectives and sanction types. They also highlight that one should be cautious in deriving general conclusions on the impact of sanctions on international trade from single-country case studies.

All of our estimates should be interpreted as the direct (partial equilibrium) effects of sanctions on firms' export behaviour. We obtain these estimates based on a firm-destination-level gravity equation, which highlights the need to control for the inward multilateral resistance terms (MRT) of the destination. We rely on the first stage of the gravity estimation procedure by Freeman et al. (2021) and exploit variation in global trade data to obtain estimates of the inward MRTs. In our second stage, the estimated MRTs are then included in our firm-level gravity equation to control for changes to market prices.

Methodologically, we also build on recommendations in Head and Mayer (2014) and contrast OLS and PPML estimates for the effect of sanctions on firm-level exports. As in Mayer et al. (2019), we find that estimation methods matter both for the statistical significance and the magnitude of effects: depending on the estimator, different types of sanctions are predicted to affect firms' exports. To rationalize these findings, we provide suggestive evidence that, in our sample, differences across estimators are related to the fact that PPML tends to give larger weight to larger trade flows.

We contribute to a large and growing literature on the effects of sanctions on trade. In particular, our analysis can be seen as complementary to recent studies that either *(i)* estimate the effects of heterogeneous sanctions using bilateral (country-level) trade data, or *(ii)* estimate the effects of specific sanction episodes (such as the sanctions on Russia after the invasion of Ukraine in 2014) based on firm-level export data.

The first type of study considers the effects of sanctions on total bilateral trade, often across a large number of countries and over long time spans. Using the same data source on sanctions that we employ in our work, Felbermayr et al. (2020a,b) estimate international trade effects for the world, based on a structural gravity model with aggregate bilateral trade flows. They find that sanctions have a significant and meaningful effect on exports, but only if the type of

sanction is accounted for.[3] In contrast, we focus on a single sending country (Denmark) and a shorter time horizon, but our firm-level data allows us to analyze the behaviour of individual firms and to study firms' adjustments at different margins. This type of analysis can offer important additional insights for policy makers: While estimates of the effects of sanctions at the aggregate (country) level might be driven to a large extent by the behaviour of so-called 'superstar exporters' – which account for the bulk of exports (Freund and Pierola, 2015; Ciliberto and Jäkel, 2021) – our estimates using firm-level data reflect adjustments at the average exporting firm.

The latter type of study employs firm-level data to study the effects of sanctions on firms in sender or target countries. To date, these studies mostly focus on a single (or few) sanction episode(s). In particular, Crozet and Hinz (2020) analyze the intensive margin of trade for French firms in reaction to the EU sanctions against Russia in 2014. They find that the sanctions had effects both on targeted and non-targeted products, which aligns with our finding that partial trade sanctions can have substantial effects on the exports of the average firm. Crozet et al. (2021) investigate the potential negative effects of sanctions on firms' probability of serving a given market, focusing on four different country cases (Russia, Iran, Cuba, and Myanmar). Gullstrand (2020) also considers the sanctions against Russia and finds that these sanctions had large negative effects on the exports of Swedish firms and large positive effects on the probability to stop serving the Russian market.[4]

Similar to our study, Besedeš et al. (2021) also consider a larger set of sanctioned countries, but focus on the case of financial sanctions as the most prominent type of sanctions over the last two decades. Their work differs from our study in that they focus on firms' cross-border financial activity (rather than export behaviour) as an alternative outcome of interest. Overall, they find only a limited effect of financial sanctions for the sender country.[5]

The rest of the paper is organized as follows. Section 2 describes the data and gives an overview over the types of sanctions included in our empirical analysis. Section 3 discusses our empirical approach for estimating the effects of sanctions on the export behaviour of firms.

[3]Earlier work estimating the effects of sanctions (independent of type) on international trade includes, *inter alia*, Caruso (2003), Yang et al. (2004) and Afesorgbor (2018). Other authors have also studied alternative outcomes of interest; see, e.g., Besedeš et al. (2017) for the effect of (financial) sanctions on capital flows.

[4]Notably, these studies focus on firms in the sending countries. An interesting complimentary perspective is offered in Haidar (2017), who studies the response of Iranian firms to international sanctions against Iran.

[5]In their most recent work, instead, they consider trade responses to financial sanctions, exploiting country-product level trade data for Germany; see Besedeš et al. (2022). Further studies exploiting firm-level data to study the effects of sanctions include Ahn and Ludema (2020).

Section 4 presents our results and illustrates that sanctions lead to very heterogeneous effects across the considered dimensions. Section 5 concludes.

2 Background and Data

We rely on the Global Sanctions Database (GSDB) developed by Felbermayr et al. (2020a) for information on the countries being targeted by sanctions. The GSDB defines sanctions as "binding restrictive measures applied by individual nations, country groups, the United Nations (UN), and other international organizations, to address different types of violations of international norms" (Felbermayr et al., 2020a, p.4). Importantly for our purpose, the GSDB allows us to distinguish sanctions by type (e.g., trade vs. financial sanctions) and objective (e.g., prevent war vs. policy change). While we focus on Denmark as the sanctioning country, it should be noted that the majority of the sanction cases considered in our empirical model are multilateral sanctions imposed by the EU or the UN, and correspondingly implemented by the Danish government.[6] For conciseness, we will refer to these as Danish sanctions in the following.

2.1 Sanctions of Denmark in the GSDB

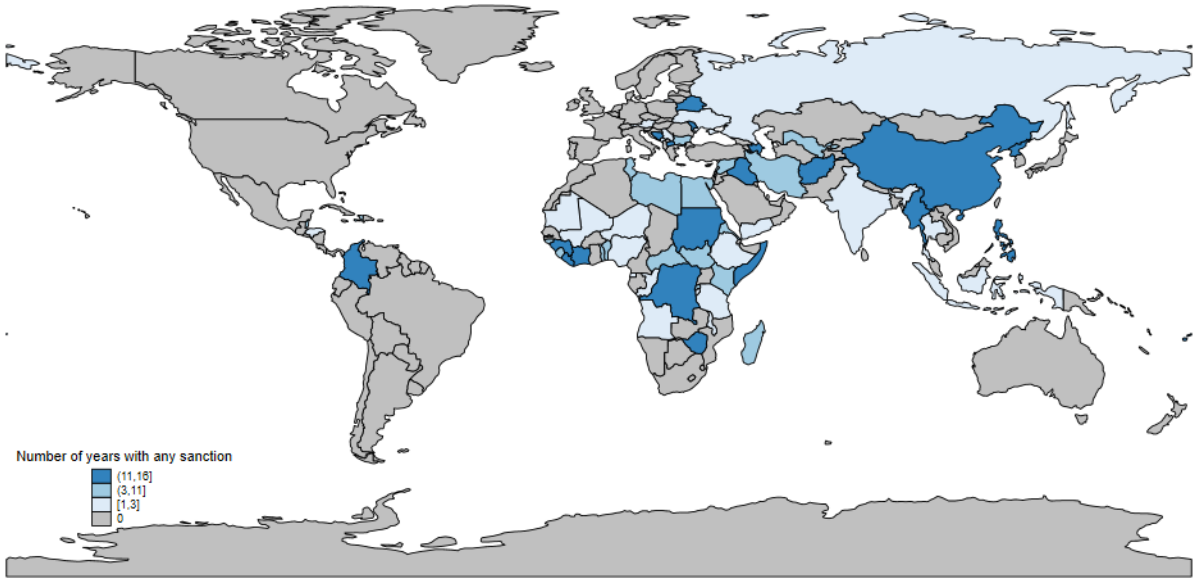
Throughout our sample period (2000–2015), we observe 64 countries being a target of Danish sanctions in at least one year.[7] We drop North Korea and Palestine due to missing information on country-level control variables (cf. Section 3), leaving us with 62 sanctioned countries in the sample. A complete list of the included sanctioned countries can be seen in Appendix A.

Some of these countries (such as Afghanistan or China) are sanctioned throughout the entire sample period. These countries will not contribute to the estimation of the effects of sanctions if sanctions are measured by a single indicator variable. However, they will affect our estimates of the heterogeneity in the effects of sanctions if there have been changes over time in the set of sanction types and/or the objectives of sanctions (which is, for example, the case for Afghanistan but not China). We nevertheless keep observations for these latter countries in the sample because they will help in estimating other parameters of our empirical model (in particular the firm-year fixed effects; cf. Section 3).

[6] In case of EU sanctions, Denmark implements all restrictive measures that are unanimously defined by all member states in Common Foreign and Security Policy (CFSP) Council decisions. In case of UN sanctions, the implementation of restrictive measures is also based on the adoption of joint EU decisions within the UN which are then implemented in the Danish legal system.

[7] In 2022, the United Nations were made up of 193 member states, illustrating the relative large number of sanctioned countries by Denmark.

Figure 1: Countries sanctioned by Denmark



Notes: This figure visualizes all countries on which Denmark imposed at least one type of sanction throughout the years 2000–2015. Darker shades indicate a longer duration of the underlying sanctions. For each country, sanction policies can contain a single or several types of sanctions (trade, financial, travel, military, arms, and other sanctions). Table A.1 in the appendix provides the list of all countries sanctioned by Denmark over the sample period.

Panel A of Table 1 provides an overview of the number of sanctioned countries exploited in the empirical analysis. Out of the 62 countries sanctioned by Denmark, 47 countries experience at least one switch in or out of being sanctioned. These countries give us 277 country-year observations subject to sanctions. Conditional on not being sanctioned in the first sample year, we observe 32 countries subject to newly imposed sanctions in the succeeding years. Moreover, we observe a further 15 countries which are sanctioned in the first sample year, but where sanctions are subsequently lifted.

Figure 1 portrays the countries that are sanctioned by Denmark at some point during our sample period, with darker shades indicating more years in which sanctions were in place. In line with the overall pattern for the EU (cf. Felbermayr et al. (2020a)), African countries have been the most frequent targets of Danish sanctions. However, we also see sanctions being imposed on various countries in the Middle East, Eastern Europe and Asia.

2.1.1 Types of Sanctions

The GSDB allows us to distinguish six types of sanctions: *(i)* trade sanctions, *(ii)* financial sanctions, *(iii)* travel sanctions, *(iv)* arms sanctions, *(v)* military sanctions, and *(vi)* other sanctions.

Trade sanctions represent restrictive measures which aim at reducing imports, exports, or both trade flows simultaneously. Trade sanctions can apply only to specific trade products with

Table 1: Number and Types of Sanctions in the Sample

Sample:	All countries[a]		Countries not continuously sanctioned[b]		First sanctioned year[c]	
	N	%	N	%	N	%
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Summary of Sample						
Number of country-years with sanctions	453		277			
Number of countries with sanctions	62		47		32	
Median number of sanctions	2		2		2	
Average number of sanctions	2.72		2.50		1.94	
Panel B: Sanction Types						
Number of country-years						
– with trade sanctions	135	30%	93	34%	6	19%
– with import sanctions	66	15%	38	14%	4	13%
– with export sanctions	106	23%	82	30%	5	16%
– with financial sanctions	343	76%	215	78%	26	81%
– with arms sanctions	257	57%	117	42%	5	16%
– with military sanctions	189	42%	97	35%	4	13%
– with travel sanctions	246	54%	123	44%	12	38%
– with other sanctions	63	14%	48	17%	9	28%

Notes: In the considered sample period (2000–2015) Denmark imposed sanctions against 64 countries. We only keep countries with information on key country-level control variables which reduces the number of sanctioned countries to 62. The unit of observation in this table is either the number of countries or country-year observations. The first two columns report summary statistics on the number of country-year observations and types of sanctions. [a] Reports summary statistics for all 62 countries with sanctions in our sample. [b] Reports summary statistics for countries with at least one switch in or out of sanctions. [c] Reports summary statistics for the first year with sanctions for countries that were not sanctioned in the first year in the sample.

target countries (partial sanctions) or to all traded goods (complete trade sanctions).[8] With the exception of Iran, Denmark has imposed only partial trade sanctions throughout the considered period. We therefore do not aim to estimate the differential effects of partial and complete trade sanctions.[9] Importantly, partial trade sanctions are typically restricting trade only for narrowly defined products or groups of items, such as dual-use products.[10] Despite many official sanction documents containing restrictions on traded products, a systematic recording of product- or sector-specific sanctions is not available at this time, so we cannot estimate the separate effects of trade sanctions on targeted vs. non-targeted products.[11]

Financial sanctions are mainly restrictive measures that freeze financial assets and investments of target countries' stakeholders located in Denmark. As a consequence, in such sanction

[8]The GSDB does not account for classical trade-restricting instruments such as anti-dumping duties. Such classical trade policy measures are used to protect domestic economic interests while sanctions are imposed to achieve broader and specific political objectives.

[9]The scarcity of complete trade bans in Denmark are in line with global trends discussed in Felbermayr et al. (2020a).

[10]Dual-use products are items that can be used for either civil or military purposes. In case of the EU sanctions against Iran (c.f. Council Regulation (EU) No 267/2012), e.g., trade related sanctions comprise restrictions on trade in dual-use goods and technology, as well as on key equipment which could be used in the petrochemical industry. Moreover, a ban was also imposed on the import of Iranian crude oil, petroleum products and petrochemical products.

[11]The GSDB and other important sanction databases (e.g., Morgan et al. (2014); Hufbauer et al. (2007)) provide information on trade sanctions only at the country-year level.

cases all funds and economic resources belonging to or owned by listed parties (people, enterprises, organizations) are frozen. Moreover, financial sanctions include restrictions on direct investments and limitations in the payment system. Direct payments to listed parties as well as indirect payments are prohibited.[12] Finally, this type of sanctions also includes the prevention of aid payments.

Travel sanctions include travel restrictions for specific people or diplomatic staff from sanctioned countries to Denmark but also in the opposite direction. In case of arms sanctions Denmark stops exporting arms or arms related material to the target country. In contrast, military sanctions refer to restrictions in monetary and personal assistance for military co-operations between Denmark and the target country. Finally, under the category "other sanctions" all types of restrictions are covered that cannot be allocated to the latter sanction types. These residual sanctions are relatively few and mainly entail diplomatic measures (e.g., the interruption of diplomatic relations with the African Union), as well as flight and harbor access restrictions.

Panel B of Table 1 provides on overview of the types of sanctions being imposed by Denmark over our sample period. In the first two columns, we report statistics for all country-year observations subject to sanctions; in the next two columns we focus on country-year observations where we observe at least one switch in or out of sanctions; and in the last two columns we zoom in on those countries with newly imposed sanctions and focus on their first year of being sanctioned.

Several interesting patterns stand out. Most strikingly, financial restrictions turn out to be the dominant type of sanction imposed by Denmark. In 76% of the country-year observations subject to sanctions, financial sanctions are in place. Arms and military sanctions are also very prevalent, but their importance declines if we exclude countries (such as China and North Korea) which are under continuous sanctions over a long period, often starting before 2000. Travel sanctions turn out to be the second largest restrictive measure. 54% of all sanction cases over the years include travel bans. In contrast, the share of sanctions including trade restrictions amounts to 30%. A separated consideration of export and import sanctions illustrates that Denmark imposed more export sanctions (share of sanctions including exports restrictions: 23%) than import sanctions (share in sanctions around 15%). However, we also see a large overlap in these two types of trade sanctions, with import sanctions often being implemented in addition to

[12] In case of the EU's sanctions against Iran, e.g., Council Regulation (EU) No 1263/2012 restricts transfers between EU financial and credit institutions and Iranian banks, including branches and subsidiaries outside Iran.

export sanctions especially in the samples of columns (3)–(6). For this reason, we will focus on trade sanctions in our empirical analysis, without distinguishing between export and/or import sanctions.

Importantly, Table 1 also illustrates that different sanctions are often implemented jointly as a 'sanction package': independent on the sample, the median number of sanctions is equal to two. In contrast, the average number of sanctions seems to increase with the duration of sanctions: for those countries that start being sanctioned during the sample period, the average number of sanctions is equal to 1.94 in the first year; cf. column (5). In contrast, it is equal to 2.50–2.72 once we consider years besides the first sanctioned year; cf. columns (1) and (3).

2.1.2 Effects of Heterogeneous Sanctions on Firms' Export Behaviour

As highlighted in Table 1, Denmark applies various sanctions in different combinations over time. Each type of sanction has a direct impact on the targeted economic activity. Financial sanctions, for example, will affect investment and financial flows, and travel bans are likely to reduce journeys of targeted groups of people. Trade sanctions will affect exports, imports or both types of trade flows with target countries. However, recall that our analysis will exploit mainly partial trade sanctions, which should affect exports only for targeted products, and thus should not be expected to have a direct impact on the average exporting firm.

In this section, instead, we discuss different channels through which all types of sanctions may have effects on firms' export behaviour even when exports are not specifically targeted by the sanctions. Independently of the sanction type, we highlight four possible channels: *(i)* the legal enforcement of sanctions; *(ii)* self-imposed restriction in light of sanctions; *(iii)* incurring information costs as a result of sanctions; and *(iv)* uncertainty about the evolution of sanctions.

In the case of trade sanctions, the legal enforcement of trade restrictions will have a direct negative effect on firms' trade flows to and/or from the target country, though only for targeted products or industries (channel *(i)*). Some firms may reduce trade with target countries despite not being legally obliged to; e.g., as part of their public relations (PR) or corporate social responsibility (CSR) strategies (channel *(ii)*). Moreover, trade sanctions can come along with additional information costs (e.g., costs for specialized law companies that ensure compliance) which can have a negative effect on firms' trade flows due to rising costs (channel *(iii)*). Finally, trade sanctions create uncertainty about the future economic conditions in target countries and, thus, firms' trade flows are likely to drop (channel *(iv)*).

Other sanctions can also have effects on firms' trade flows through the four listed channels. Given the relevance of financial and travel sanctions in Denmark, we focus on possible effects of these two sanction types on firms' export behaviour.

The legal enforcement of financial sanctions can have negative effect on trade flows. If, for example, payment restrictions are introduced, cross border trade can drop due to the unavailability of payment options (channel *(i)*). Similarly, if financial intermediaries introduce self-imposed restrictions (e.g., on the payment system for specific countries) it can entail a drop in domestic firms' trade with the target country (channel *(ii)*).[13] Due to financial sanctions, firms often incur higher costs for experts and law firms. These additional costs reduce firms' financial margins and can, as a consequence, reduce trade (channel *(iii)*). Finally, the existence of financial sanctions creates uncertainty in the target country (e.g., worsening international payment conditions) and hence, can impede firms' export behavior, particularly in the presence of sunk costs (channel *(iv)*, also see Dixit (1989)).

While travel bans aim at restricting the movement of people, they are also likely to influence firms' export behavior. The legal enforcement of travel sanctions can be a signal for exporting firms that business conditions in target countries are worsening, and thereby firms are likely to reduce trade with target countries (channel *(i)*). Similar to the case of trade sanctions, firms may also be inclined to reduce their activity in sanctioned countries for PR or CSR reasons (channel *(ii)*): Ensuring compliance with travel restrictions results in additional costs for firms and hence, can have a negative indirect effect on trade (channel *(iii)*). Finally, travel sanctions are also a signal of uncertain business conditions in target countries and hence may result in less export (channel *(iv)*).

Overall, this discussion exemplifies mechanisms through which firms' exports are affected indirectly by sanctions that do not directly aim at reducing trade with target countries. Hence, depending on the intensity of these indirect channels, we expect varying negative effects of all considered sanctions on firms' trade.

[13] An example for this channel can be seen in the case of sanctions on Iran. In 2018, German Banks stopped offering financial transaction options for Iranian businesses, after the USA tightened their financial sanctions against Iran. This is despite the fact that according to EU legislation, European banks were free to deal with Iran (cf. https://www.reuters.com/article/germany-iran-dz-bank-idUSL5N1SP5N8).

2.1.3 Objectives of Sanctions

Another important source of heterogeneity in sanctions arises from varying policy objectives. In addition to sanction *types*, the GSDB systematically groups sanctions by their *policy objective*, which can be thought of as the end goal of the sanction policy.[14]

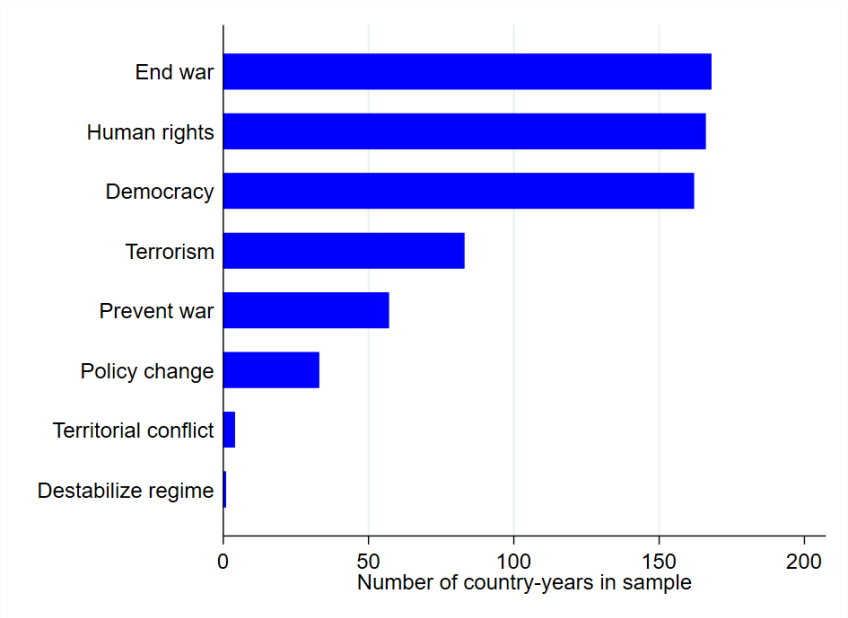


Figure 2: Objectives of Sanctions in Sample (2000–2015)

The GSDB contains information on eight distinct policy objectives, where each sanction case can have multiple objectives. Figure 2 shows the prevalence of the identified policy objectives associated with Danish sanctions across all years and countries. As can be seen, some policy objectives are more prevalent than others. The most often defined policy objective is *end war* followed by *human rights* and *democracy* related objectives. Sanctions addressing *terrorism*, *prevention of war* and *policy changes* in target countries are observed significantly less. Over the considered sample period, Denmark rarely imposed sanctions aiming at resolving territorial conflicts and destabilizing regimes in specific countries.[15]

In light of this multitude of policy objectives, we ask whether the effects of sanctions on firms' export behaviour differ depending on the proclaimed objectives. The objectives of sanctions are informative of the political environment underlying the sanction. For example, sanctions with

[14]The GSDB identifies for each sanction case policy objectives by capitalizing on the fact that in general, official sanction documents declare all targeted objectives that sanctioned countries have to fulfill before imposed sanctions are lifted. Felbermayr et al. (2020a) provide a detailed explanation of how policy objectives in sanctions are identified and how they differ from each other.

[15]Austria in the year 2000 is the only country where EU/Danish sanctions had the objective to destabilize the regime (the government of the far-right party of Jörg Haider).

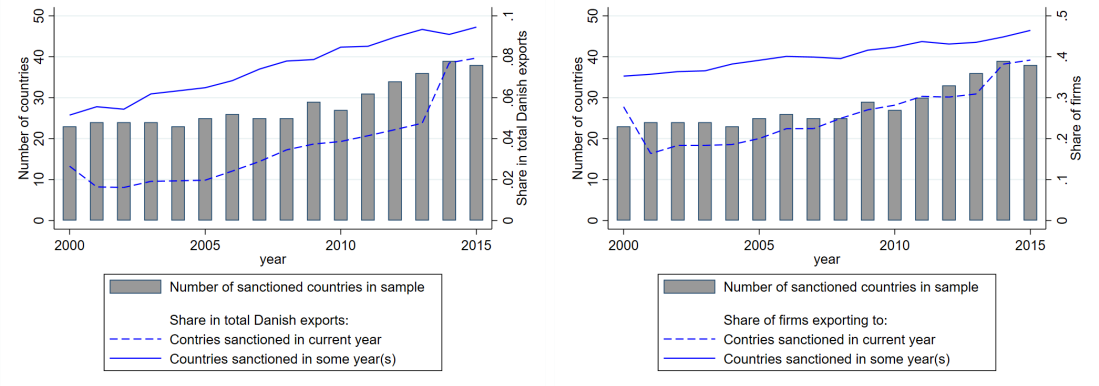
the objective *territorial conflict* signal that there is an imminent political conflict between the sender and target country, and we might expect these sanctions to be particularly detrimental to trade. Moreover, the scope of implemented sanctions (one versus several imposed sanctions) and depth of sanctions (e.g., number of product categories included in partial trade sanctions) may significantly differ across the observed policy objectives. For example, sanctions with the objective *human rights* may be designed such as to target only a very narrow group of individuals (namely, those involved in human rights violations). Finally, some of the channels through which sanctions affect firm export behaviour discussed above may be more apparent for certain objectives. For example, uncertainty about the future might be more important for sanctions related to the prevention of war or territorial conflicts.

2.2 Firm-level Data and Danish Exports to Sanctioned Countries

Our analysis builds on firm-level register data provided by Statistics Denmark for the years 2000–2015. In the External Trade Statistics, firms report their exports and imports by product and destination. We aggregate the export information up to the firm-destination-year level by summing over all products exported by the firm. We merge the trade data with the General Firm Statistics using a unique firm identifier. From the latter data source, we retrieve information on employment, industry classification, etc. We restrict the sample to firms which have a minimum of 10 employees in at least one year of the sample, and to the following broad economic sectors: *(i)* manufacturing, *(ii)* wholesale/retail, *(iii)* transport, and *(iv)* knowledge services. These sectors account for the bulk (89 percent) of overall Danish goods exports. Including sanctioned countries, our final sample contains 186 export destinations.[16]

Figures 3(a) and (b) show the importance of sanctioned countries for Danish exports. The 62 countries for which we observe sanctions being imposed in at least one sample year accounted for over 9 percent of total exports of the firms in our sample at the end of the sample period. Thus, sanctioned markets make up for a non-negligible portion of trade. Interestingly, this share has been increasing over time, reflecting the fact that some of the sanctioned countries are developing or emerging economies with high economic growth rates. The number of countries sanctioned in any specific year varies from 24 countries at the beginning of our sample to 38 countries in the mid-2010's. The share of countries sanctioned in the current year in total Danish exports

[16] We limit the sample of non-sanctioned countries to those with a minimum of five firm-export observations per year, on average.



(a) Importance of Sanctioned Countries (b) Share of Exporters to Sanctioned Countries

Figure 3: Sanctioned Countries and Danish Exports

sees a notable increase in 2014–15 when sanctions were imposed on Russia.[17] The importance of sanctioned markets for Danish exporters can also be seen by considering the share of firms that export to at least one sanctioned country; cf. Figure 3(b): in 2015, this share stood at almost 40 percent.

3 Empirical Strategy

To quantify the impact of heterogeneous sanctions on firm-level outcomes, we estimate a firm-level gravity equation using recent advances in the gravity literature (Freeman et al., 2021) which allow us to identify the direct (partial equilibrium) effect of sanctions.

3.1 Empirical Specification

Our empirical model is described by the following equation:

$$Y_{fjt} = \boldsymbol{\beta}\, \mathbf{Sanction_{jt}} + \alpha_1 \ln\left(GDP_{jt}\right) + \alpha_2 \ln\left(\hat{P}_{jt}^{1-\sigma}\right) + \alpha_3 \mathrm{RTA}_{jt} + \gamma_{ft} + \delta_{fj} + \varepsilon_{fjt}\ , \qquad (1)$$

where Y_{fjt} denotes an outcome for firm f in destination market j in year t. We consider two outcomes of interest: (the log of) firms' destination-specific exports, $\ln\left(exports_{fjt}\right)$[18], and the probability of exiting the destination in year t (conditional on serving the market in $t-1$).

[17] Figure B.1 in the Appendix shows that sanctioned countries remain important for Danish exporters even if we exclude China and Russia.

[18] The log dependent variable implies that regressions will be conditional on serving the market in year t. As discussed in Section 3.2 below, we also follow a complementary approach and implement the PPML estimator for $exports_{fjt}$, in levels.

$\mathbf{Sanction_{jt}}$ describes the sanction regime of market j at time t and $\boldsymbol{\beta}$ denotes the corresponding vector of parameters. The sanction regime is measured either by a single indicator variable assuming the value 1 if the market is sanctioned (regardless of the sanction characteristics), or a set of indicator variables that describe the particular characteristics of the sanction(s) in place (e.g., different sanction types; cf. section 2.1.1). Identification of the coefficient vector $\boldsymbol{\beta}$ relies on both the introduction and the removal of sanctions within the sample period.[19]

As the subscript indicates, $\mathbf{Sanction_{jt}}$ varies only at the destination-year level. Some sanctions may not affect all firms, at least not directly (cf. Section 2.1.2). This is, for example, the case for partial trade sanctions which target specific products. The GSDB does not allow us to exploit this type of variation in the data. Thus, our estimates of $\boldsymbol{\beta}$ should be interpreted as the average firm-level impact of sanctions through the channels discussed in Section 2.1.2.

3.1.1 Fixed Effects

Our empirical model contains two separate fixed effects, γ_{ft} and δ_{fj}, which denote firm-year and firm-destination fixed effects, respectively. The firm-year fixed effects account for any firm-specific shocks that impact a firm's export behaviour across all destinations (such as firm productivity or management quality).[20] The presence of γ_{ft} in Equation (1) implies that coefficient estimates of $\boldsymbol{\beta}$ measure the effect of sanctions on firms' export behaviour in sanctioned countries relative to their export behaviour in non-sanctioned markets.

The second fixed effect, δ_{fj}, captures any time-constant linkages between firms and destinations, and effectively allows us to compare the same firm-destination spell with and without sanctions. δ_{fj} also limits endogeneity concerns in the presence of (time-invariant) trade costs that correlate with the imposition of sanctions. Such trade costs are likely to vary across firms due to differences in their product portfolios or other inherent structures, motivating the use of firm-destination fixed effects, compared to just destination fixed effects.[21]

[19] As a robustness check, we also estimate regressions where $\boldsymbol{\beta}$ is solely identified based on the imposition of sanctions; see Section 4.5 for a discussion.

[20] In the gravity literature, controlling for outward multilateral resistance (which is a measure of time-varying country-level export capability) is crucial to obtain unbiased estimates of the effects of trade costs; see Baldwin and Taglioni (2006). By the same token, γ_{ft} can be thought of as controlling for the overall exporting capability of the firm, and thus the firm-specific outward multilateral resistance.

[21] While reverse causality is generally a concern when estimating the effect of sanctions on trade, we believe this is unlikely to be an issue in our set-up. In particular, for the case of Denmark, most sanctions are determined at the UN or EU levels. At the EU level, the Council of the EU decides by unanimity on adopting, renewing, or lifting sanctions, and Denmark would thus in principle have veto power. However, we nevertheless deem it unlikely that a single Danish firm can affect the design of EU policies.

3.1.2 Accounting for Market Size and Market Competitiveness

Our empirical model aims to estimate the direct (partial equilibrium) effect of sanctions on firms' export behaviour; i.e., the effect of sanctions which is due to an increase in trade costs. This approach closely follows previous studies estimating the effects of sanctions using bilateral trade data (e.g., Felbermayr et al., 2020a; Dai et al., 2021; Kwon et al., 2022). While we abstract from general equilibrium effects which may reinforce or mitigate the direct effect of sanctions, our approach ensures that estimates can be interpreted as causal (cf. below). To this aim, we follow the structural gravity literature and account for market size and multilateral resistance terms (MRTs) in our firm-level gravity equation.[22]

As is standard in the gravity literature, we add the log of GDP as a control for market size. Sending countries will likely hesitate to impose sanctions on larger target countries because doing so may entail larger costs for the sender. Controlling for market size is therefore important to identify the causal effect of sanctions on firms' export behaviour. However, accounting for GDP implies that our estimates of the effect of sanctions will not capture any indirect (general equilibrium) effects of sanctions on firms' export through their effect on GDP.

In addition, we want to account for changes to the price index of the destination market – reflecting the market's level of competitiveness. In Equation (1), P_{jt} denotes the destination-specific price index for imported goods.[23] Since Anderson and van Wincoop (2003), this term is often referred to as the inward multilateral resistance term (MRT), and it is inherently unobservable. Since trade costs (including sanctions) enter the importer price index P_{jt}, failure to account for P_{jt} may cause bias in the estimation of $\boldsymbol{\beta}$ (Baldwin and Taglioni, 2006). In particular, Danish sanctions often occur as part of a larger network of EU or UN sanctions. Thus, destination markets are affected by sanctions from other origins simultaneously with the Danish sanctions, leading to changes in the competitiveness of these markets.

To account for the inward MRT in our empirical model, we adapt the two-stage estimation strategy developed in Freeman et al. (2021) to our set-up. Their procedure draws upon work by Fally (2015), who shows that the properties of the Poisson pseudo-maximum likelihood (PPML) estimator ensures a link between the theoretical structure of the gravity equation's

[22]Different from previous studies exploiting bilateral trade data at the country-level, accounting for market size and MRTs through the use of importer-year fixed effects is not feasible, since these would absorb all of the effects of sanctions in Equation (1).

[23]This price index can be derived from a range of trade models when consumer preference have constant elasticity of substitution (CES); see, e.g., Armington (1969) and Melitz (2003).

MRT components and the estimated fixed effect of the empirical gravity equation. Specifically, we rely on the first stage of the gravity estimation procedure by Freeman et al. (2021) and exploit variation in global trade data to obtain estimates of the inward MRTs.[24] A detailed description of the first-stage estimation is found in Appendix B.2. We then estimate Equation (1) with the inward MRTs ($\hat{P}_{jt}^{1-\sigma}$), obtained from the first stage, as controls for changes to market prices. Again, accounting for inward MRTs is important to ensure that estimates can be interpreted as causal, while it also implies that general equilibrium effects working via changes in the market's competitiveness will not be captured by the estimates.

3.1.3 Other Control Variables

Besides the fixed effects, GDP and the estimated inward MRT, we include a dummy variable for the presence of a free-trade agreement between Denmark and the destination (RTA). The RTA dummy captures time-varying trade costs and is likely negatively correlated with the probability of being sanctioned. We obtain information on RTAs from Mario Larch's Regional Trade Agreements Database (Egger and Larch, 2008).

3.2 Accounting for Adjustments at the Extensive Margin

When taking the log of exports as the dependent variable in Equation (1), we only exploit information on firm-destination-years with positive exports. Sanctions may lead firms to stop exporting to a market and deter firms from starting to export, and ignoring these adjustments could lead to selection bias. A common strategy to avoid this source of bias is to use the PPML estimator, first advocated by Santos Silva and Tenreyro (2006) for its ability to deal with the inconsistency of log-linear OLS models in the presence of heteroskedasticity. We thus also present results based on the following model:

$$Y_{fjt} = \exp\left(\boldsymbol{\beta}\ \mathbf{Sanction_{jt}} + \alpha_1 \ln\left(GDP_{jt}\right) + \alpha_2 \ln\left(\hat{P}_{jt}^{1-\sigma}\right) + \alpha_3 \mathrm{RTA}_{jt} + \gamma_{ft} + \delta_{fj}\right) \times \varepsilon_{fjt}, \quad (2)$$

where the dependent variable now is the level of exports and the model is estimated by PPML.

Firm-level register data only contain information on trade flows that are actually realized, so the first step in implementing the PPML estimator on our sample is to generate observations

[24] We employ data on global trade from the ITDP-E (Borchert et al., 2021) and Dynamic Gravity (Gurevich and Herman, 2018) data sets. Crucially, the first-stage estimation omits Denmark as both importer and exporter to avoid the MRTs being estimated based on Danish sanctions and direct trade responses to Danish sanctions.

for zero trade flows. This task is not innocuous because it requires us to make assumptions on which firms could potentially have exported to markets where they currently are not exporting. Intuitively, some tiny and/or distant markets will not be perceived as potential export destinations by many firms, independent of sanctions. If we include observations for these firms and destinations, we might erroneously conclude that sanctions do not have an effect on firms' export behaviour.[25]

Some restrictions on the PPML estimation sample follow naturally from the econometric model. In particular, the presence of firm-year and firm-destination fixed effects implies that *(i)* for each year, only firms which export to at least one destination in that given year, and *(ii)* for each destination, only firms which export at least once to that given destination will effectively be included. Whether these sample restrictions are also plausible from an economic viewpoint is, however, unclear. For example, a fictive Danish firm that exported a single small shipment to Russia in the year 2000 will be used to infer the effects on Danish firms of the EU sanctions against Russia in the years 2014–15.

As highlighted by Head and Mayer (2014), PPML differs from OLS also in the moment conditions used to estimate the parameters. These differences imply that PPML will put more weight on observations with large levels of trade. However, we might expect large export flows to react differently to the imposition of sanctions. For example, sanctions directed towards larger markets may be inherently differently designed (e.g., because drastic sanctions on large markets may entail larger economic costs for the sender, making them politically unpopular). Similarly, firms with large exports to a given destination market (be it due to the firm's overall size as exporter or specific business links between the individual firm and the particular destination), may react differently to the imposition of sanctions compared to the average exporter. In consequence, PPML may result in estimates that are closer to the true effect of sanctions for a particular part of the sample (namely, large export flows), rather than the unweighted average.

For these reasons, we follow Mayer et al. (2019) and choose OLS estimates for our baseline, while also reporting PPML estimates and providing some evidence on the likely origins of differences in results across estimators. In addition, as discussed above, we zoom in on one specific aspect of adjustment at the extensive margin, namely, the probability of market exit.

[25]In the gravity literature, it is standard to include zero trade flows for *all* country pairs that are not trading with each other. Assuming that all country pairs could potentially trade with each other seems, however, more realistic than assuming that all firms could potentially serve all markets.

4 Results

4.1 Baseline Results: Overall Effects of Sanctions

In Table 2, we report estimates where the sanction regime is captured by a simple indicator variable for the presence of sanctions. We start with a standard OLS specification with log firm-destination-year-level exports as dependent variable. On average, across firms and countries, sanctions are predicted to reduce firms' exports by 9 percent (cf. column (1)). This effect is substantial, especially considering that many sanctions (such as financial and travel sanctions) do not specifically target trade, but instead are expected to affect firms' exports through some of the indirect channels discussed in Section 2.1.2.

Table 2: Effects of Sanctions on Danish Firms' Exports

	ln Exports OLS (1)	Exports PPML (2)	Exports>0 PPML (3)	ln Exports Weighted OLS (4)	Exit prob. LPM (5)
Any sanction	-0.0909**	-0.1293	-0.1103	-0.0392	0.0240**
	(0.040)	(0.085)	(0.070)	(0.047)	(0.010)
ln GDP	0.6348***	0.8156***	0.7326***	0.5739***	-0.0549***
	(0.066)	(0.057)	(0.063)	(0.054)	(0.007)
ln Inward MRT	-0.1962*	-0.0795	-0.1632	-0.2295	0.0947***
	(0.115)	(0.168)	(0.160)	(0.161)	(0.020)
RTA	0.0268	0.1637***	0.1113***	0.1126***	-0.0065
	(0.057)	(0.039)	(0.032)	(0.034)	(0.009)
Observations	1,035,407	2,640,375	1,035,407	1,035,407	949,085
R-squared	0.784			0.964	0.546
Firm-destination FE	Yes	Yes	Yes	Yes	Yes
Firm-year FE	Yes	Yes	Yes	Yes	Yes
Sample probability					0.1799

Notes: Robust standard errors, adjusted for clustering at the country level, in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.

In column (2), we turn to the estimation of the PPML model for firm-destination-year-level exports in levels; cf. Equation (2). While the point estimate from this model is similar to the OLS estimate, statistical significance is lost.[26] We follow Mayer et al. (2019) and investigate to what extent these differences across estimators are driven by differences in estimation samples and/or differences in the weight given to different observations. To this aim, we first apply the PPML estimator, but drop observations with no exports from the sample; cf. column (3) of Table 2. Restricting the PPML sample to those observations exploited by the OLS estimator interestingly does not have large effects on estimated coefficients, suggesting that differences between OLS and PPML are not driven by those observations. Next, we return to the OLS estimator, but mimic the PPML property of giving larger weight to larger trade flows by running a weighted

[26]Note that insignificant effects of sanctions are also reported by Felbermayr et al. (2020a) in their specification not distinguishing sanction types.

regression. Again, this estimator shows insignificant effects of sanctions on firms' exports; cf. column (4).

In sum, we take these additional regressions as suggestive evidence that differences between OLS and PPML are likely driven by differences in the weighting scheme employed by different estimators. Interestingly, this interpretation is reinforced when considering the RTA indicator, which is insignificant in the OLS specification, but turns significant and positive in the two PPML models as well as the weighted OLS specification. Below, we also show that similar patterns across estimators are found when including detailed information on different sanction types.

Do sanctions affect firms' export behaviour at the extensive margin? While it is difficult to analyze the effects of sanctions on firms' export entry due to the ambiguities in constructing a suitable sample of 'potential entrants' (cf. our discussion in Section 3.2), it is straightforward to analyze effects on market exit. The effect of sanctions is positive and statistically significant: firms' probability to exit a destination market increases by 2.4 percentage points if sanctions are in place; cf. column (5) of Table 2. This effect is also strikingly large when judged against the overall firm-destination-specific exit probability, which stands at roughly 18 percent.

Before we turn to the heterogeneity in sanctions, we also briefly discuss the effects of our other control variables. Destination GDP is estimated to have a positive effect on firms' exports, independent on the estimator employed. Furthermore, the coefficient estimate for the inward MRT is negative in columns (1)–(4), though statistically significant only in column (1). All signs are in line with expectations. Moreover, for the exit probability, estimated signs of these coefficients are reversed.

4.2 Heterogeneity by Sanction Type and Across Sanction Packages

Do firms equally react to all types of sanctions? To answer this question, we exploit information on the types of sanctions imposed and report results in Table 3. Our estimates show that, indeed, different types of sanctions have very different effects on firms' export behaviour, with some types not having any discernible effect. As can be seen at the bottom of the table, however, different sanctions are estimated to be jointly significant across all columns (including the PPML specification), reinforcing findings in Table 2.

Similar to Table 2, we start with an OLS specification for firms' destination-specific export value. Only financial sanctions are predicted to have significant negative effects on firms' ex-

Table 3: Heterogeneity Across Sanction Types

	ln Exports OLS (1)	Exports PPML (2)	Exports>0 PPML (3)	ln Exports Weighted OLS (4)	Exit prob. LPM (5)
Arms sanction	-0.0176	0.1920**	0.1504	0.1380*	0.0399**
	(0.069)	(0.097)	(0.092)	(0.074)	(0.020)
Military sanction	0.0548	-0.0393	-0.0052	0.0282	0.0063
	(0.085)	(0.084)	(0.077)	(0.057)	(0.020)
Travel sanction	0.0287	-0.0990	-0.0416	0.0115	0.0265**
	(0.054)	(0.097)	(0.099)	(0.078)	(0.012)
Trade sanction	-0.0061	-0.2547**	-0.2441**	-0.1719**	-0.0279
	(0.061)	(0.102)	(0.105)	(0.068)	(0.031)
Financial sanction	-0.1330**	-0.1086	-0.0758	-0.0630	0.0028
	(0.053)	(0.100)	(0.092)	(0.090)	(0.012)
Other sanctions	-0.0145	0.1752**	0.1266	0.0762	0.0019
	(0.045)	(0.085)	(0.102)	(0.083)	(0.015)
ln GDP	0.6360***	0.8085***	0.7280***	0.5713***	-0.0548***
	(0.066)	(0.059)	(0.065)	(0.054)	(0.008)
ln Inward MRT	-0.2005*	-0.0261	-0.1331	-0.2249	0.0912***
	(0.116)	(0.173)	(0.168)	(0.168)	(0.021)
RTA	0.0295	0.1778***	0.1241***	0.1260***	-0.0027
	(0.058)	(0.037)	(0.027)	(0.028)	(0.010)
Observations	1,035,407	2,640,375	1,035,407	1,035,407	949,085
R-squared	0.784			0.964	0.546
Firm-destination FE	Yes	Yes	Yes	Yes	Yes
Firm-year FE	Yes	Yes	Yes	Yes	Yes
Joint significance[a]	0.0656	0.0000	0.0006	0.0799	0.0000
Sample probability					0.1799

Notes: Robust standard errors, adjusted for clustering at the country level, in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.
[a] Reports the p-value for the test of joint significance of the sanctions dummies.

ports; cf. column (1) of Table 3. With –13 percent, their effect is somewhat larger than the overall effect estimated in column (1) of Table 2. Interestingly, the OLS specification suggests that trade sanctions do not significantly deter exports of Danish firms. However, we again find that estimation methods matter. In fact, the PPML estimator predicts that trade sanctions significantly reduce exports, but financial sanctions do not; cf. column (2). Once more, these differences in estimates between OLS and PPML seem to be driven by differences in their weighting scheme, rather than by the PPML model's ability to include zero trade flows: the PPML estimates are largely insensitive to dropping observations with zero exports, and a weighted OLS regression is able to replicate the PPML estimates fairly well; cf. columns (3) and (4).

Still, these differences across estimators may have interesting implications because they suggest that larger export flows are more severely affect by trade sanctions while the average export flow is affected mainly by financial sanctions. Several plausible explanations of these discrepancies come to mind. First, recall that trade sanctions in our sample are predominantly partial trade sanctions. Such sanctions may be targeted towards sectors where the typical export transaction is large (such as a certain capital goods), rationalizing the different estimates for trade sanctions in columns (1) and (2). Second, trade sanctions are typically implemented together with other sanctions as part of a sanction package (cf. below), while financial sanctions are often

imposed in isolation. Thus, when we observe trade sanctions, the overall sanction regime is also typically more severe. An alternative explanation for the differences across estimators is, thus, that firms with larger export transactions only react to sanctions that are severe.

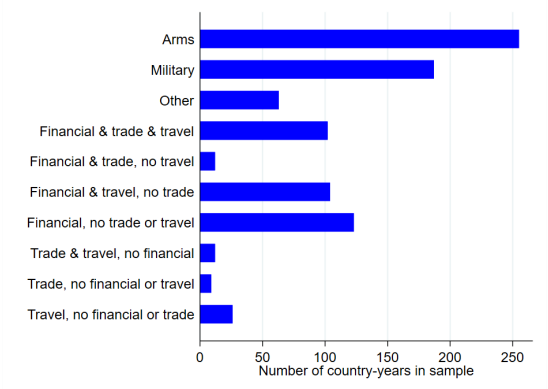
We investigate this second explanation by considering the effects of combinations of different sanctions, or 'sanction packages' (with a focus on financial, trade, and travel sanctions). Figure 4(a) reports the number of country-year observations with different combinations of these three types of sanctions. While financial sanctions are often implemented individually, trade and travel sanctions are rarely seen in isolation. In fact, trade sanctions are most often combined with *both*, financial *and* travel sanctions.

In Figures 4(b)–(c), we therefore report results from the OLS and PPML estimators for firms' destination-specific exports where we distinguish the differential effects of isolated vs. combined sanctions. Of course, estimates for sanction types or packages which are used very rarely (such as trade sanctions without financial or travel sanctions) need to be interpreted with caution.[27]

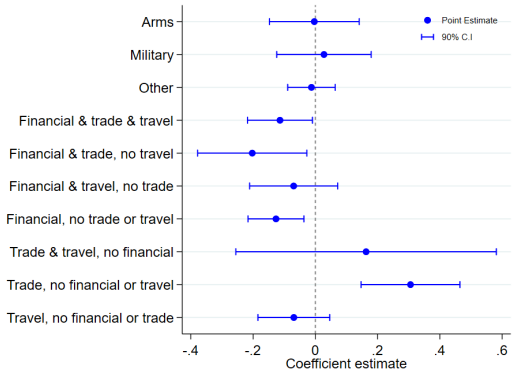
The OLS estimates reveal that financial sanctions have negative and significant effects on firms' exports independent on whether they are implemented in isolation, or jointly with trade sanctions or trade and travel sanctions. However, the estimates are very similar in magnitude, and any differences are not statistically significant. Thus, financial sanctions are enough to deter exports for the average export flow, and combining financial sanctions with trade or trade and travel sanctions does not seem to further decrease exports. This finding suggests that the different channels through which sanctions may affect firms' exports (cf. Section 2.1.2) – such as increased information costs and uncertainty – are already operative in the case of milder sanction regimes.

Instead, the PPML estimator once more shows a somewhat different pattern: here, only a combination of financial, travel, and trade sanctions is predicted to lead to significant reductions in firms' exports. This result is consistent with the negative effects of trade sanctions in the PPML model of Table 3: it shows that this negative effect of trade sanctions is driven by a combination of trade sanctions with other sanctions. A plausible interpretation of this finding is that firm-destinations with larger trade flows react to sanctions only if they are sufficiently severe, for example, because some of the indirect mechanisms through which sanctions affect

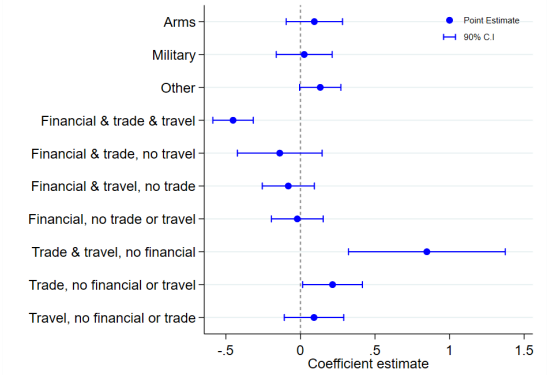
[27]For example, trade sanctions implemented in isolation show a curiously positive effect in the OLS model; cf. Figure 4(b). However, this estimate is based on very few country cases, and these cases are arguable very specific.



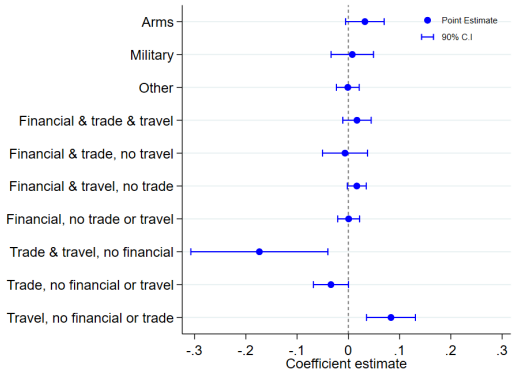
(a) Number of country-years with sanctions



(b) OLS coefficient estimates



(c) PPML coefficient estimates



(d) LPM coefficient estimates

Figure 4: Results for Sanction Packages

exports are less important for large export flows.[28]

Once more, we also report results for firms' export behaviour at the extensive margin, focusing on firms' destination-specific exit probability. Interestingly, we estimate that only travel sanctions but not trade or financial sanctions have a positive and significant effect on firms' probability to stop exporting; see column (5) of Table 3. This unexpected finding is confirmed in Figure 4(d), showing that only travel sanctions implemented in isolation have the expected positive effect on this outcome. Moreover, we note that arms sanctions also seem to increase the exit probability, but this finding should be interpreted with caution as we find a curiously positive effect of such sanctions on firms' exports in the PPML model of column (2).

4.3 Heterogeneity by Objective

As illustrated in Figure 2 above, sanctions are also imposed in connection with a variety of policy objectives. These different policy objectives may speak to the policy environment underlying the

[28]Note that an export flow might be large even firms that are small with regard to standard measures such as employment; e.g., because the considered market is large.

Table 4: Heterogeneity Across Objectives of Sanctions

	ln Exports OLS (1)	Exports PPML (2)	Exit prob. LPM (3)	ln Exports OLS (4)	Exports PPML (5)	Exit prob. LPM (6)
Objective: prevent war	-0.2557***	-0.5364***	0.0581***	-0.2294***	-0.4722***	0.0580***
	(0.055)	(0.081)	(0.019)	(0.046)	(0.063)	(0.019)
Objective: end war	0.1595**	0.1942	0.0162	0.1572**	0.1915	0.0162
	(0.076)	(0.123)	(0.013)	(0.074)	(0.123)	(0.013)
Objective: human rights	-0.0240	-0.0093	0.0233	-0.0126	0.0099	0.0232
	(0.106)	(0.128)	(0.030)	(0.104)	(0.127)	(0.030)
Objective: policy change	-0.1210**	-0.3462***	0.0282**	-0.1283**	-0.3555***	0.0282**
	(0.059)	(0.054)	(0.013)	(0.060)	(0.054)	(0.013)
Objective: terrorism	0.0810	0.2538***	-0.0006	0.0831	0.2505***	-0.0006
	(0.062)	(0.082)	(0.012)	(0.059)	(0.079)	(0.012)
Objective: territorial conflict	-0.5098***	0.1186	0.0543***	-0.5343***	0.0715	0.0544***
	(0.090)	(0.130)	(0.016)	(0.079)	(0.145)	(0.016)
Objective: democracy	-0.0519	-0.1164	-0.0027	-0.0315	-0.0734	-0.0028
	(0.100)	(0.152)	(0.027)	(0.095)	(0.166)	(0.027)
Objective: destabilize regime	0.0224	0.1489***		0.0204	0.1459***	
	(0.021)	(0.018)		(0.021)	(0.018)	
ln GDP	0.6380***	0.8100***	-0.0547***	0.6223***	0.7879***	-0.0547***
	(0.066)	(0.058)	(0.007)	(0.069)	(0.065)	(0.008)
ln Inward MRT	-0.2011*	-0.0234	0.0947***	-0.2115*	-0.0358	0.0947***
	(0.116)	(0.173)	(0.020)	(0.119)	(0.171)	(0.020)
RTA	0.0428	0.1873***	-0.0086	0.0411	0.1864***	-0.0086
	(0.054)	(0.038)	(0.009)	(0.054)	(0.037)	(0.009)
High Risk				-0.1033***	-0.1482***	0.0003
				(0.035)	(0.052)	(0.004)
Observations	1,035,407	2,640,375	949,085	1,035,407	2,640,375	949,085
R-squared	0.784		0.546	0.784		0.546
Firm-destination FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Joint significance[a]	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Sample probability			0.1799			0.1799

Notes: Robust standard errors, adjusted for clustering at the country level, in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.
[a] Reports the p-value for the test of joint significance of the sanctions dummies.

sanctions and the scope and depth of sanctions applied. Similarly, the channels through which sanctions affect firm export behaviour (cf. Section 2.1.2) may materialize to varying extents depending on the objectives. Next, we therefore investigate the potential heterogeneity in the effects of sanctions on firms' export outcomes across policy objectives.

Table 4 reports estimated effects of sanctions on Danish firms' export behaviour across the proclaimed objectives. Estimates are based on our firm-level gravity models in Equations (1) and (2). For conciseness, we do not report results for the PPML estimator for positive export flows and the weighted OLS regression; but our interpretation of the differences between OLS and PPML upholds also in the specifications of the following tables.

Our estimates show the expected signs across all three specifications only for two of the policy objectives, *prevent war* and *policy change*. Specifically, sanctions with the objective *prevent war* have a large and highly statistically significant negative effect on firms' export values both for the OLS specification in column (1) and the PPML model in column (2) of Table 4, amounting

to a reduction by 22% to 42%.[29] Moreover, the effect on the destination-specific exit probability is large and positive (column (3)). The same pattern of signs is observed for sanctions with the objective *policy change*, but the magnitudes are much smaller. Sanctions aiming at *territorial conflicts* show the largest effects (in absolute terms) on the log of exports and the exit probability, though the estimate turns insignificant in the PPML specification. We should note, however, that the effects may be imprecisely estimated because of few country cases with this policy objective over the sample period; cf. Figure 2.

The result that sanctions with the goals prevent war, policy change, and territorial conflict can be explained by the fact that target countries in those cases show a strong level of risk and often a significant drop in cross border activity. At the same time, we may be concerned that these estimated effects are not primarily driven by the underlying sanctions, but rather by market risks (e.g., in war-torn target countries and in the presence of deep policy conflicts). To investigate this possible relationship, we turn to OECD data on country risk and construct an indicator variable equal to one if the destination market is characterized as high risk (risk categories 5-7 on a 7-point scale). Indeed, most sanctioned countries are found in this high-risk category. However, we find that results are strikingly robust if we condition on this variable; cf. columns (4)–(6) of Table 4.

Interestingly, we don't find significant effects of sanctions with the objectives *human rights* or *democracy*. Our small and insignificant effects for sanction cases with these two objectives are very similar to the findings of Felbermayr et al. (2020b). To rationalize the finding, one should note that there is a high degree of complexity and heterogeneity within the two policy objectives. Human-rights-related sanctions significantly vary, from individual person related demands to larger policy demands. Thus, for at least some of these cases, we expect sanctions to be mild, targeting only a narrow set of individuals in the destination market. Equally, democracy-related policy objectives cover a broad range of topics, some of which may be rather soft objectives as compared to strong objectives such as *prevent war*. Still, our findings indicate the need for a deeper analysis into this direction of research, particularly, given the rising large number of sanction cases with these last two policy goals.

Finally, we also note that some of the estimated effects take unexpected signs, at least in subsets of the reported specifications. This is the case for *end war*, *terrorism*, and *destabilize regime*. Estimates for the latter should be interpreted with caution, as we only have a single

[29] $\exp(-0.2557) - 1 = -0.22$ and $\exp(-0.534) - 1 = -0.44$.

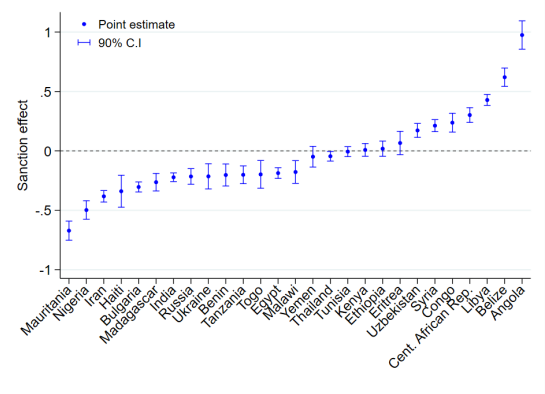


Figure 5: OLS Estimates of Country-Specific Effects of Sanctions

country case with this objective within our sample (Austria in the year 2000). Findings for the former two objectives, on the other hand, are interesting: they show that sanctions need not deter trade, and, in contrast, might spur trade in certain situations. It is likely that the positive effects on export sales is driven by trade in certain industries; such as medical goods and other consumer goods that might be exported to countries in war conditions to support civil society. Future work exploiting product level trade could bring some additional explanations for these patterns.

4.4 Heterogeneity across Sanctioned Countries

Another source of heterogeneity in the effects of sanctions comes from the individual countries that are targeted. To explore this country-level heterogeneity, we estimate the empirical specifications in Equations (1) and (2) with country-specific sanction dummies. For this purpose, we abstract from the other sources of heterogeneity discussed this far and only distinguish between a given destination, j, being sanctioned or not.

For this part of the analysis, we restrict the sample of sanctioned countries as follows. First, we exclude countries that have less than five years without sanctions throughout our sample period. This restriction ensures a sufficient number of observations in the control group for each country (i.e., a sufficient number of observations without sanctions being in place). Second, we exclude countries which are sanctioned for only one year throughout the sample, because the number of observations subject to sanctions is typically too low to ensure proper identification. These restrictions leave us with 27 country-specific sanction coefficients, presented in Figure 5 for the OLS model with export sales as dependent variable. Similar figures for the PPML estimation and the exit probability are found in Appendix B.3.

Of the 27 coefficient estimates, 15 are negative and statistically significant, indicating that sanctions had negative effects on firms' export sales in roughly half of the country cases. However, we observe a large heterogeneity in the size of these effects, which vary between -49% (Mauritania) to -4% (Thailand), but with most of the estimated effects being close to -20%. Interestingly, seven of our coefficient estimates are positive and significant at the 10% confidence level, indicating that for these countries firms' exports in the sanctioned years were, on average, larger than in the years without sanctions . Four of these countries (Angola, Cent. African Rep., The Congo and Syria) have sanctions with the objective *end war*, and the positive estimated effects for these countries thus align with our results in Table 4. Finally, we also observed five countries for which sanctions are estimated to not have any effects on firms' export sales. Again, this finding should not be surprising given that we found insignificant effects of different types of sanctions and sanctions with certain policy objectives; cf. Tables 3 and 4.

Once more, we find that estimation methods matter. Specifically, the heterogeneity in estimates is amplified in the PPML specification, with the PPML coefficients being more extreme at both ends of the distribution. Moreover, the PPML model shows a somewhat different ranking of countries with respect to the size of the estimated effects compared to Figure 5. Despite these differences, the number of countries with positive, negative and insignificant coefficient is similar across the models.

For Russia and Iran, we can compare our estimates with those in previous studies focusing on these specific country cases. For the case of sanctions on Russia, our results align well with the effects estimated for French (Crozet and Hinz, 2020; Crozet et al., 2021) and Swedish firms (Gullstrand, 2020). For Iran, the large negative estimated effect of sanctions on firms' export sales seen in Figure 5 resonates with estimates from Felbermayr et al. (2020b), who have studied these sanctions using aggregate (country-level) trade data. At the extensive margin, we also find a large positive effect of sanctions on firms' probability to exit the Iranian market (cf. Figure B.3 in the Appendix) , which broadly aligns with findings for French firms (Crozet et al., 2021). Comparing our results to those in other parts of the literature is not straightforward, since these studies have typically focused on different outcomes of interest (such as financial flows; cf. Besedeš et al. (2017)).

4.5 Remaining Empirical Concerns and Robustness

Common for all our estimations thus far is the use of dummies to identify the effects of sanctions. Identification is thus based on both the imposition and the removal of sanctions. These two types of policy changes might, however, not have symmetric effects on firms' export behaviour. Exploiting product-level trade data for the US, Kohl (2021) finds evidence that the removal of sanctions does not entail a rebound to pre-sanction export levels. Thus, the effect of sanctions might continue into post-sanction periods, potentially leading to biases in our previous estimates. To mitigate such concerns, we test whether our estimates of the effects of sanctions (ignoring any heterogeneity in effects; cf. Table 2) differ if identified based on the imposition of sanctions only.[30] Estimated by OLS, the effect of imposing sanctions is large and statistically significant: we predict that, on average, firms' destination-specific exports drop by 15% as sanctions are being imposed (see Table B.2 in Appendix B.3). This estimate is, thus, somewhat larger (in absolute size) than our previous estimate of –9.1% (cf. Table 2, column 1).

The decision to sanction markets is outside the firm's control, both when sanctions are decided by the Danish government and even more so when sanction are decided by the UN or the EU. Nevertheless, sanctions reflect political desires that may be observable long before the sanctions are agreed upon. This potentially causes firms to adjust their behavior in advance of the sanction events, such that the contemporaneous effects of sanctions do not capture the total adjustment. We conduct a test for any anticipatory (as well as lagged) effects of sanctions, by allowing for a one-year lead and lag indicators on the Any Sanction$_{jt}$ dummy (see Appendix B.3, Table B.3). In the OLS specifications, the results do not indicate any significant anticipatory or lagged effects on the intensive margin of exports or the firm's probability of exiting the destination market. In contrast, the PPML model shows *positive* effects on exports in the year prior to sanctions (which once more seems to be driven the large weight given to larger trade flows). This finding is interesting because it suggests that some firms may increase their current exports in anticipation of future disruptions to market access.

Previous literature has shown that trade intermediaries (such as wholesalers and retailers) play an important role for firms in gaining access to export markets, especially in countries with high trade barriers (Ahn et al., 2011). As we have argued before, one mechanism through

[30]Specifically, we alter the sample by excluding post-sanction observations for sanctioned countries, such that variation in the sanction dummy is always caused by the imposition of sanctions. We constrain this part of the analysis to the simple sanction dummy. Sanction types and sanction objectives may change within a sanction episode, rendering this type of constrained sample analysis infeasible.

which sanctions may affect exports is through their effects on trade costs (including information and compliance costs), implying that trade intermediation may be more important in sanctioned markets. This line of reasoning also suggests that trade intermediaries may be affected differently by sanctions. We test this hypothesis by splitting our sample by the primary sector of the firms.[31] In OLS estimations using the simple indicator for the presence of any type of sanctions, we find that firms in the manufacturing and retail/wholesale sector react similarly to sanctions, on average, although statistical significance is lower for the latter sector (see Appendix B.3, Table B.4). Thus, intermediaries do not seem to react differently than manufacturing firms.[32]

5 Conclusion

In light of rising international political tensions in recent years, countries have been increasingly implementing economic sanctions against adversaries. Sanctions are mainly imposed to enforce politically defined objectives either in target countries, specific regions (groups of countries), or to achieve international goals. While the success of sanction policies remains controversial, their economic impact can be significant depending on the composition of the sanction packages. A growing literature analyzes the latter economic effects aiming at a better understanding of how sanctions affect trade flows. Our analysis contributes and extends this empirical literature by focusing on a single sender country (Denmark) with its whole scope of sanctioned target countries for a given period. Our analysis allows a more general assessment of how sanctions may affect firms' export behaviour, in contrast to studies with a single target country which instead are able to capture very specific effects of the case in question.

Our empirical analysis carves out several new findings. When we abstract from heterogeneity of sanctions we find that sanctions policies reduce Danish firms' exports, on average, by 9 percent to 13 percent. In addition, we observe that sanctions lead to a significant increase in firms' probability to exit a target country. These estimates do, however, mark significant heterogeneity. First, we find that the types of sanctions and their composition matters. Second, the political motivations behind sanctions are important for their effect on firms' export behaviour. Finally,

[31] We focus on results for firms in the manufacturing and retail/wholesale sectors which account for the bulk of all trade observations in our sample.

[32] Once more, PPML results show somewhat different patterns, with much larger estimated effects for wholesalers/retailers compared to manufacturing firms. Turning to the heterogeneity in the effects of sanctions, we estimate no significant effect of the various sanction types for retail/wholesale firms in the OLS specification for log firm-destination-specific exports (though all sanction dummies are jointly significant). Estimates for manufacturing firms resemble those in Table 3; see Table B.5

the effects of sanctions are also largely heterogeneous across countries. Of course, these three dimensions of heterogeneity are interconnected.

Our results thus emphasize the importance of accounting for the observed heterogeneity in sanctions, particularly from a policy perspective. Moreover, the wide range in the estimated effects of sanctions for Danish firms shows that evaluating individual country cases may not be sufficiently representative for an assessment of potential effects of sanctions in other targets. In light of these findings, our analysis should be seen as complementary to existing empirical studies that account for either heterogeneous sanction effect at the aggregate level, or for heterogeneous effects in case of a single country.

References

Afesorgbor, S. (2018). The impact of economic sanctions on international trade: How do threatened sanctions compare with imposed sanctions? *European Journal of Political Economy*, 56(C):11–26.

Ahn, D. P. and Ludema, R. D. (2020). The sword and the shield: The economics of targeted sanctions. *European Economic Review*, 130:103587.

Ahn, J., Khandelwal, A. K., and Wei, S.-J. (2011). The role of intermediaries in facilitating trade. *Journal of International Economics*, 84(1):73–85.

Anderson, J. and van Wincoop, E. (2003). Gravity with gravitas: A solution to the border puzzle. *American Economic Review*, 93(1):170–192.

Armington, P. (1969). A theory of demand for products distinguished by place of production. *IMF Staff Papers*, 16:159–176.

Baldwin, R. and Taglioni, D. (2006). Gravity for dummies and dummies for gravity equations. *NBER Working Paper No. 12516*.

Besedeš, T., Goldbach, S., and Nitsch, V. (2017). You're banned! The effect of sanctions on German cross-border financial flows. *Economic Policy*, 32(90):263–318.

Besedeš, T., Goldbach, S., and Nitsch, V. (2021). Cheap talk? Financial sanctions and non-financial firms. *European Economic Review*, 134:103688.

Besedeš, T., Goldbach, S., and Nitsch, V. (2022). Smart or smash? The effect of financial sanctions on trade in goods and services. *Discussion Papers, 28/2022*, Deutsche Bundesbank.

Borchert, I., Larch, M., Shikher, S., and Yotov, Y. V. (2021). The International Trade and Production Database for Estimation (ITPD-E). *International Economics*, 166(C):140–166.

Caruso, R. (2003). The impact of international economic sanctions on trade: An empirical analysis. *Peace Economics, Peace Science, and Public Policy*, 9(2):1–36.

Ciliberto, F. and Jäkel, I. C. (2021). Superstar exporters: An empirical investigation of strategic interactions in Danish export markets. *Journal of International Economics*, 129:103405.

Crozet, M. and Hinz, J. (2020). Friendly fire: The trade impact of the Russia sanctions and counter-sanctions. *Economic Policy*, 35(101):97–146.

Crozet, M., Hinz, J., Stammann, A., and Wanner, J. (2021). Worth the pain? Firms' exporting behaviour to countries under sanctions. *European Economic Review*, 134:103683.

Dai, M., Felbermayr, G., Kirilakha, A., Syropoulos, C., Yalcin, E., and Yotov, Y. V. (2021). Timing the impact of sanctions on trade. In van Bergeijk, P. A., editor, *Research Handbook on Economic Sanctions*, chapter 22, pages 411–437. Edward Elgar Publishing.

Dixit, A. (1989). Entry and exit decisions under uncertainty. *Journal of Political Economy*, 97(3):620–638.

Egger, P. and Larch, M. (2008). Interdependent preferential trade agreement memberships: An empirical analysis. *Journal of International Economics*, 76(2):384–399.

Fally, T. (2015). Structural gravity and fixed effects. *Journal of International Economics*, 97(1):76–85.

Felbermayr, G., Kirilakha, A., Syropoulos, C., Yalcin, E., and Yotov, Y. V. (2020a). The global sanctions data base. *European Economic Review*, 129:103561.

Felbermayr, G., Syropoulos, C., Yalcin, E., and Yotov, Y. (2020b). On the heterogeneous effects of sanctions on trade and welfare: Evidence from the sanctions on Iran and a new database. *School of Economics Working Paper Series, 2020-4*, LeBow College of Business, Drexel University.

Freeman, R., Larch, M., Theodorakopoulos, A., and Yotov, Y. (2021). Unlocking new methods to estimate country-specific trade costs and trade elasticities. *School of Economics Working Paper Series, 2021-17*, LeBow College of Business, Drexel University.

Freund, C. and Pierola, M. D. (2015). Export Superstars. *The Review of Economics and Statistics*, 97(5):1023–1032.

Gullstrand, J. (2020). What goes around comes around: The effects of sanctions on Swedish firms in the wake of the Ukraine crisis. *The World Economy*, 43(9):2315–2342.

Gurevich, T. and Herman, P. (2018). The dynamic gravity dataset: 1948-2016. *USITC Working Paper, 2018-02-A*.

Haidar, J. (2017). Sanctions and export deflection: Evidence from Iran. *Economic Policy*, 32(90):319–355.

Head, K. and Mayer, T. (2014). Gravity Equations: Workhorse, toolkit, and cookbook. In Gopinath, G., Helpman, E., and Rogoff, K., editors, *Handbook of International Economics*, volume 4, chapter 3, pages 131–195. Elsevier.

Hufbauer, G. C., Schott, J. J., Elliott, K. A., and Oegg, B. (2007). Economic sanctions reconsidered. *(3rd edition). Washington, DC: Peterson Institute for International Economics.*

Kohl, T. (2021). In and out of the penalty box: US sanctions and their effects on international trade. In van Bergeijk, P. A., editor, *Research Handbook on Economic Sanctions*, chapter 21, pages 388–410. Edward Elgar Publishing.

Kwon, O., Syropoulos, C., and Yotov, Y. (2022). The extraterritorial effects of sanctions. *School of Economics Working Paper Series, 2022-3*, LeBow College of Business, Drexel University.

Mayer, T., Vicard, V., and Zignago, S. (2019). The cost of non-europe, revisited. *Economic Policy*, 34:145–199.

Melitz, M. (2003). The impact of trade on intra-industry reallocations and aggregate industry productivity. *Econometrica*, 71(6):1695–1725.

Morgan, T. C., Bapat, N., and Kobayashi, Y. (2014). Threat and imposition of economic sanctions 1945-2005: Updating the TIES dataset. *Conflict Management and Peace Science*, 31(5):541–558.

Santos Silva, J. and Tenreyro, S. (2006). The log of gravity. *Review of Economics and Statistics*, 88(4):641–658.

Yang, J., Askari, H., Forrer, J., and Teegen, H. (2004). US economic sanctions: An empirical study. *The International Trade Journal*, 18(1):23–62.

A Appendix

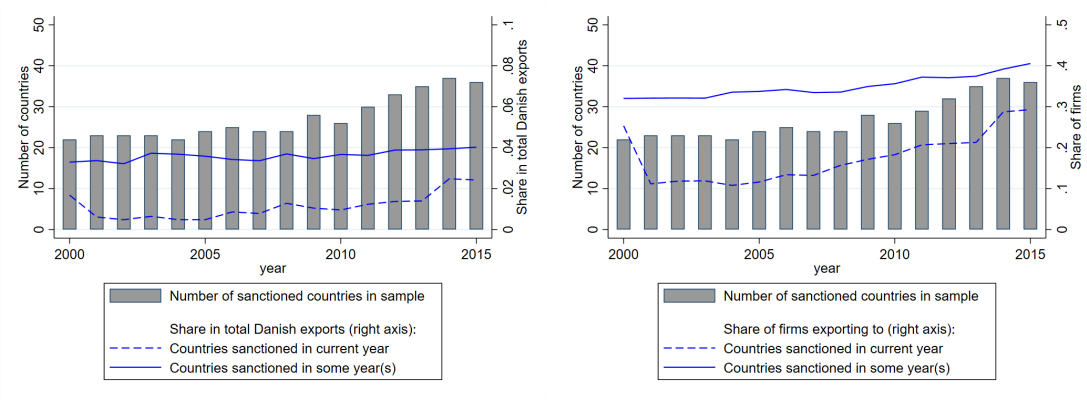
Table A.1: List of Countries with Sanctions

ISO2 code	Country name	Years with sanctions			Years in sample	
		First year	Last year	N years	N years	First year
AF	Afghanistan	2001	2015	15	15	2001
AO	Angola	2000	2002	3	16	2000
AT	Austria	2000	2000	1	16	2000
AZ	Azerbaijan	2000	2015	16	16	2000
BA	Bosnia and Herzegovina	2000	2015	16	16	2000
BG	Bulgaria	2008	2015	8	16	2000
BI	Burundi	2000	2015	3	16	2000
BJ	Benin	2009	2015	7	16	2000
BY	Belarus	2002	2015	12	16	2000
BZ	Belize	2001	2014	5	16	2000
CD	Congo (Dem. Rep. of)	2000	2013	14	14	2000
CF	Central African Republic	2003	2015	6	16	2000
CG	Congo (the)	2000	2001	2	16	2000
CI	Côte d'Ivoire	2000	2015	15	16	2000
CN	China	2000	2015	16	16	2000
CO	Colombia	2002	2015	14	16	2000
CS	Serbia and Montenegro	2000	2005	6	6	2000
EG	Egypt	2011	2015	5	16	2000
ER	Eritrea	2000	2013	7	14	2000
ET	Ethiopia	2000	2001	2	16	2000
FJ	Fiji	2000	2015	13	16	2000
GM	Gambia (the)	2014	2015	2	16	2000
GN	Guinea	2002	2015	13	16	2000
GQ	Equatorial Guinea	2000	2000	1	16	2000
GW	Guinea-Bissau	2012	2015	4	16	2000
HN	Honduras	2009	2009	1	16	2000
HT	Haiti	2001	2005	5	16	2000
ID	Indonesia	2000	2000	1	16	2000
IN	India	2000	2001	2	16	2000
IQ	Iraq	2004	2015	12	12	2004
IR	Iran	2006	2015	10	16	2000
KE	Kenya	2012	2015	4	16	2000
LB	Lebanon	2005	2015	11	16	2000
LR	Liberia	2000	2015	16	16	2000
LY	Libya	2000	2013	8	14	2000
MD	Moldova	2003	2015	13	16	2000
ME	Montenegro	2006	2015	10	10	2006
MG	Madagascar	2010	2014	5	16	2000
MK	Republic of North Macedonia	2000	2015	16	16	2000
ML	Mali	2012	2013	2	16	2000
MM	Myanmar	2000	2015	16	16	2000
MR	Mauritania	2005	2009	3	16	2000
MW	Malawi	2001	2003	3	16	2000
NE	Niger (the)	2009	2011	3	16	2000
NG	Nigeria	2014	2015	2	16	2000
PH	Philippines (the)	2002	2015	14	16	2000
RS	Serbia	2006	2015	10	10	2006
RU	Russian Federation (the)	2014	2015	2	16	2000
RW	Rwanda	2000	2013	11	16	2000
SD	Sudan (the)	2011	2015	5	5	2011
SL	Sierra Leone	2000	2010	11	16	2000
SO	Somalia	2013	2015	3	3	2013
SS	South Sudan	2011	2015	5	5	2011
SY	Syrian Arab Republic	2005	2007	3	8	2000
TG	Togo	2000	2007	8	16	2000
TH	Thailand	2014	2015	2	16	2000
TN	Tunisia	2011	2015	5	16	2000
TZ	Tanzania	2014	2015	2	16	2000
UA	Ukraine	2014	2015	2	16	2000
UZ	Uzbekistan	2005	2009	5	16	2000
YE	Yemen	2014	2015	2	16	2000
ZW	Zimbabwe	2002	2015	14	16	2000

Notes: This table lists the countries with sanctions in our sample. Some countries are not observed throughout the entire period due to political changes (e.g. Serbia and Montenegro, South Sudan) or missing data (e.g., Afghanistan in 2000).

B Online Appendix (not for publication)

B.1 Additional Figures



(a) Importance of Sanctioned Countries (b) Share of Exporters to Sanctioned Countries

Figure B.1: Sanctioned Countries and Danish Exports: excl. China and Russia

B.2 Estimation of Multilateral Resistance Terms

This appendix describes the procedure for estimating the inward multilateral resistance (cf. Freeman et al., 2021). The generalized gravity equation, as popularized by Anderson & van Wincoop (2003), expresses the value of exports from country i to country j at time t as:

$$X_{ijt} = \frac{Y_{it}E_{jt}}{Y_{wt}} \left(\frac{\tau_{ijt}}{\Pi_{it}P_{jt}} \right)^{1-\sigma}, \tag{B.1}$$

where Y_{wt} denotes world output, Y_{it} denotes output of the exporting country i, E_{jt} denotes expenditure in the importing country j, and t denotes time. These three elements make up the size-component of the gravity model. The friction – or trade cost component – consists of *(i)* τ_{ijt}, which denotes time-varying bilateral iceberg-trade costs; *(ii)* $\sigma > 1$, which is the elasticity of substitution; and *(iii)* P_{jt} and Π_{it}, which denote the inward- and outward multilateral resistance terms (MRTs) and are defined as:

$$P_{jt} \equiv \sum_{it} \left(\frac{\tau_{ijt}}{\Pi_{it}} \right)^{1-\sigma} \frac{Y_{it}}{Y_{wt}} \tag{B.2}$$

and

$$\Pi_{it} \equiv \sum_{jt} \left(\frac{\tau_{ijt}}{P_{jt}} \right)^{1-\sigma} \frac{E_{jt}}{Y_{wt}}. \tag{B.3}$$

Total trade costs are a function of bilateral sanctions s_{ijt} and other bilateral trade costs:

$\tau_{ijt}(s_{ijt}, t_{ijt})$. As discussed in the main text, sanctions (here denoted s_{ijt}) enter the price index directly, implying omitted variable bias if P_{jt} is not controlled for.

Our aim is therefore to obtain estimates of the inward MRTs, which we motivate based on Equation (B.1) above. Using the ITDP-E dataset (Borchert et al., 2021) and the Dynamic Gravity Dataset (Gurevich and Herman, 2018) from 2000–2015, we estimate the following model by PPML:

$$X_{ijt} = \exp(\gamma_{jt} + \psi_{it} + \beta_n \tau_{ijt}) \times \epsilon_{ijt}, \tag{B.4}$$

where τ_{ijt} is a set of standard bilateral trade costs variables: (log) bilateral distance, indicators for common language, colonial ties, contiguous borders, common legal systems, as well as common membership of WTO and trade agreements. γ_{jt} denotes importer-year fixed effects, ψ_{it} denotes exporter-year fixed effects, and 0 denotes a given importer chosen as the reference country, whose fixed effects are omitted from the estimation when the model is estimated without a constant. We apply Germany as reference country.[i] The vectors of fixed effects are thus estimated relative to Germany, and the estimated MRTs reflect trade resistance relative to the referenced importing country.

To recover the MRTs, our procedure draws upon the work of Fally (2015), who shows that the properties of the Poisson pseudo-maximum likelihood (PPML) estimator ensure a link between the theoretical structure of the gravity equation's MRT components and the estimated fixed effect of the empirical gravity equation. Specifically, the estimates of the MRTs raised to the power of $1-\sigma$ are shown to follow the structure:

$$\hat{P}_{jt}^{1-\sigma} = \frac{E_{jt}}{\exp(\widehat{\gamma}_{jt})} \frac{1}{E_{0t}} \tag{B.5}$$

$$\hat{\Pi}_{it}^{1-\sigma} = \frac{Y_{it}}{\exp(\widehat{\psi}_{it})} \frac{E_{0t}}{Y_{wt}}, \tag{B.6}$$

We exclude Denmark as an exporting and importing country from the sample to avoid the MRTs reflecting Danish sanctions on other countries directly. We also restrict the sample of exporting countries to those that exist continuously throughout the sample period. This restriction avoids changes in MRTs being caused by countries seizing or beginning to exist mid-sample. Thus, changes in the estimated MRTs over time will only reflect changes in trade costs. The first-

[i] One argument for this, is that neither Denmark, nor its main sanction-partners (EU and UN) impose any sanctions on Germany within- or near our sample, leaving the reference country unaffected by the direct effects of sanctions vis-à-vis Denmark.

stage sample consists of 225 exporting countries, 237 importing countries and a total of 659,448 observations[ii] of which approximately 30 pct. are zero-trade flows.

Results from the estimation of Equation (B.4) are given in column (1) of Table B.1. Combining the estimated fixed effects from this model and computing the remaining variables (E_{jt}, E_{0t}, Y_{it} and Y_{wt}) from the data-set allows us to generate vectors of $\hat{P}_{jt}^{1-\sigma}$ and $\hat{\Pi}_{it}^{1-\sigma}$ based on the expressions in (B.5) and (B.6). $\ln \hat{P}_{jt}^{1-\sigma}$ is then added as control variable to our firm-level gravity equation; cf. the main text.

To verify our procedure, we also report results from a second stage estimationg along the lines of Freeman et al. (2021). Specifically, we fit the gravity model using the estimated MRTs instead of importer-year and exporter-year fixed effect:

$$X_{ijt} = \exp \beta_n \tau_{ijt} + \alpha_1 \ln \hat{P}_{jt}^{1-\sigma} + \alpha_2 \ln \hat{\Pi}_{it}^{1-\sigma} \times \epsilon_{ijt} \tag{B.7}$$

Theory predicts that the coefficient on the inward MRT (IMR) should be equal to -1. Our estimate satisfies this theoretical prediction: -1 is within the 95% confidence interval of the estimate; cf. column (2). The trade cost coefficients also show little changes between the first and second stage in columns (1) and (2), indicating that the model fit is similar when using fixed effects and MRT estimates.

Equation B.4 does not include a bilateral sanction variable. However, to ensure that this has no implication for the estimated MRTs, we also stimate Equation B.4 with a bilateral sanction dummy and compare the backed out MRTs with estimates from column (1). Results are given in columns (3) and (4) of Table B.1. The inclusion of a sanction dummy has little impact on the estimated MRT, as the estimated α_1 coefficient in columns (2) and (4) are nearly identical. Figure B.2 plots the estimated MRTs obtained from column 1 and column 3 to further validate this result. Furthermore, the small and insignificant coefficient for *Any Sanction* in columns (3)–(4) is consistent with Felbermayr et al. (2020b).

For both sanctioned countries and non-sanctioned countries, the estimated inward MRTs are virtually identical when estimating the first stage equation with and without a bilateral sanction dummy, indicating that the bilateral trade costs within the model have little impact on the estimated country-specific indices.

[ii] We include importing countries that do not continuously exist in our sample, as some of these are also subject to Danish sanctions, such as Serbia & Montenegro.

Table B.1: Estimation of MRTs, cf Freeman et al. (2021)

	PPML		PPML	
	1st stage	2nd stage	1st stage	2nd stage
	(1)	(2)	(3)	(4)
$\ln \hat{P}_{jt}^{1-\sigma}$		-1.039***		-1.035***
		(0.0202)		(0.0204)
$\ln \hat{\Pi}_{it}^{1-\sigma}$		-1.000***		-1.000***
		(0.0185)		(0.0183)
Any sanction			-0.00848	0.0399
			(0.0236)	(0.0245)
$\ln E_{jt}$		0.960***		0.961***
		(0.00381)		(0.00377)
$\ln Y_{it}$		0.951***		0.950***
		(0.00431)		(0.00448)
ln Distance	-0.725***	-0.726***	-0.725***	-0.727***
	(0.0103)	(0.0107)	(0.0102)	(0.0106)
Contiguity	0.309***	0.374***	0.309***	0.373***
	(0.0177)	(0.0221)	(0.0177)	(0.0219)
Common language	0.185***	0.172***	0.185***	0.171***
	(0.0163)	(0.0140)	(0.0163)	(0.0140)
Common legal origin	0.408***	0.408***	0.408***	0.408***
	(0.0248)	(0.0293)	(0.0248)	(0.0289)
Colonial relationship	-0.0851***	-0.0575**	-0.0850***	-0.0569**
	(0.0220)	(0.0290)	(0.0221)	(0.0287)
Both in WTO	0.460***	0.490***	0.460***	0.493***
	(0.0623)	(0.0198)	(0.0623)	(0.0198)
Both in EU	0.185***	0.253***	0.184***	0.253***
	(0.0252)	(0.0215)	(0.0250)	(0.0214)
Free-trade agreement	0.410***	0.432***	0.410***	0.430***
	(0.0343)	(0.0358)	(0.0342)	(0.0356)
Preferential trade agreement	-0.0849**	-0.178**	-0.0853**	-0.174***
	(0.0355)	(0.0342)	(0.0355)	(0.0341)
Observations	659,448	659,448	659,448	659,448
R-squared	0.885	0.829	0.885	0.829
Exporter-time FE	Yes	No	Yes	No
Importer-time FE	Yes	No	Yes	No

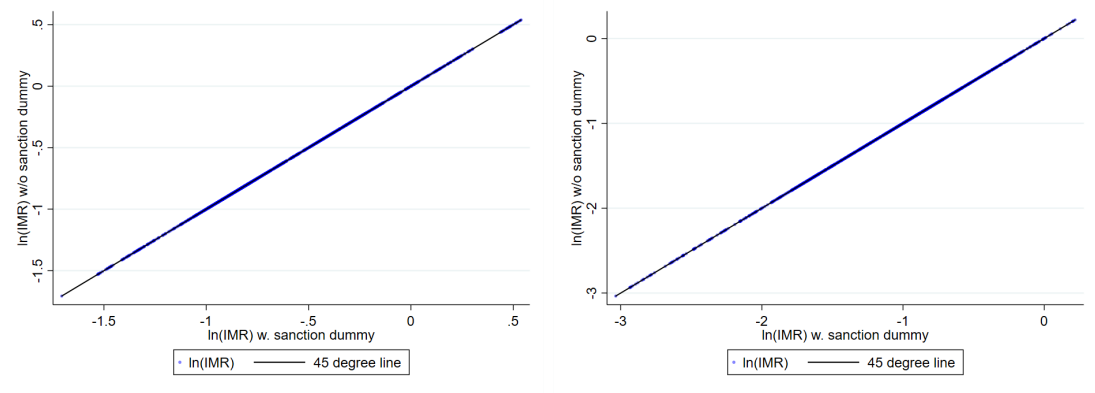
Notes: Robust standard errors in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.

B.3 Additional Results

Table B.2: Effects of Imposition of Sanctions

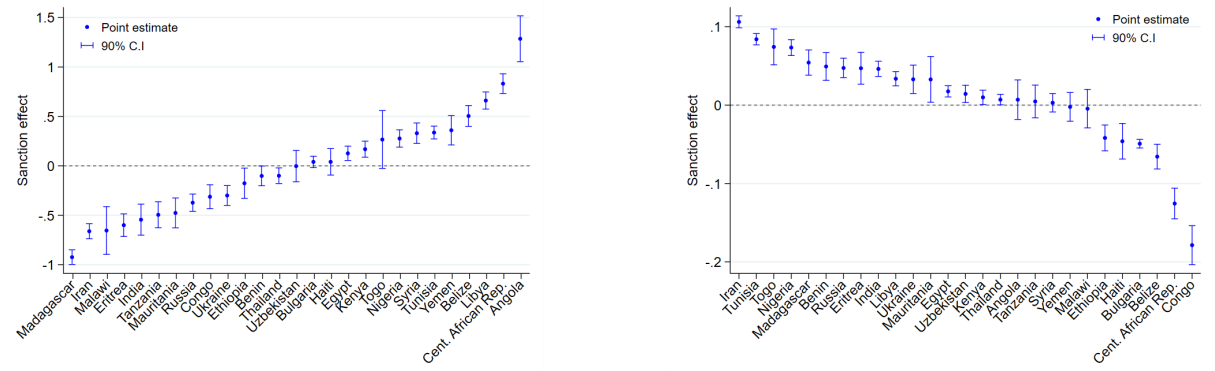
	ln Exports	Exports	Exports>0	ln Exports	Exit prob.
	OLS	PPML	PPML	Weighted OLS	LPM
	(1)	(2)	(3)	(4)	(5)
Any sanction	-0.1536***	-0.1696*	-0.1548**	-0.0541	0.0275**
	(0.041)	(0.091)	(0.071)	(0.051)	(0.012)
ln GDP	0.6480***	0.8267***	0.7439***	0.5785***	-0.0553***
	(0.067)	(0.056)	(0.062)	(0.054)	(0.008)
ln Inward MRT	-0.2132*	-0.0647	-0.1509	-0.2327	0.0977***
	(0.114)	(0.171)	(0.163)	(0.163)	(0.020)
RTA	0.0319	0.1625***	0.1124***	0.1120***	-0.0063
	(0.055)	(0.039)	(0.030)	(0.034)	(0.009)
Observations	994,455	2,507,252	994,455	994,455	910,771
R-squared	0.787			0.964	0.547
Firm-destination FE	Yes	Yes	Yes	Yes	Yes
Firm-year FE	Yes	Yes	Yes	Yes	Yes
Sample probability					0.1788

Notes: This table restricts the sample for countries which are sanctioned at some point to years with sanctions and pre-sanction years. Thus, any post-sanction observations are removed. Robust standard errors, adjusted for clustering at the country level, in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.



(a) Sanctioned countries (b) Non-sanctioned countries

Figure B.2: Estimated inward MRTs with and without sanction dummy



(a) PPML (b) Exit probability

Figure B.3: Country-Specific Effects of Sanctions

Table B.3: Anticipatory and Lagged Effects of Sanctions

	ln Exports OLS (1)	Exports PPML (2)	Exports>0 PPML (3)	ln Exports Weighted OLS (4)	Exit prob. LPM (5)
Any sanction (Lead)	-0.0496	0.0920*	0.0974**	0.1316***	0.0100
	(0.035)	(0.047)	(0.045)	(0.044)	(0.017)
Any sanction	-0.0529*	-0.1590**	-0.1604***	-0.0942**	0.0142
	(0.029)	(0.065)	(0.053)	(0.044)	(0.019)
Any sanction (Lag)	-0.0254	0.0450	0.0561	0.0216	-0.0001
	(0.046)	(0.089)	(0.078)	(0.061)	(0.007)
ln GDP	0.6588***	0.7946***	0.7203***	0.5500***	-0.0577***
	(0.073)	(0.061)	(0.065)	(0.058)	(0.008)
ln Inward MRT	-0.2135	-0.1941	-0.3018	-0.3823**	0.1011***
	(0.145)	(0.195)	(0.190)	(0.185)	(0.022)
RTA	0.0439	0.1404***	0.1025***	0.1107***	-0.0026
	(0.056)	(0.036)	(0.030)	(0.028)	(0.008)
Observations	886,159	2,164,134	886,159	886,159	864,793
R-squared	0.792			0.966	0.544
Firm-destination FE	Yes	Yes	Yes	Yes	Yes
Firm-year FE	Yes	Yes	Yes	Yes	Yes
Sample probability					0.1762

Notes: Robust standard errors, adjusted for clustering at the country level, in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.

Table B.4: Effects of Sanctions by Sector

	Manufacturing			Retail & wholesale		
	ln Exports OLS (1)	Exports PPML (2)	Exit prob. LPM (3)	ln Exports OLS (4)	Exports PPML (5)	Exit prob. LPM (6)
Any sanction	-0.0983**	-0.1022	0.0288***	-0.0812*	-0.3052***	0.0198
	(0.046)	(0.103)	(0.010)	(0.043)	(0.106)	(0.014)
ln GDP	0.7079***	0.8452***	-0.0626***	0.4703***	0.6990***	-0.0414***
	(0.073)	(0.057)	(0.008)	(0.070)	(0.144)	(0.009)
ln Inward MRT	-0.1494	-0.0883	0.0748***	-0.2654	-0.1012	0.1349***
	(0.115)	(0.192)	(0.020)	(0.161)	(0.382)	(0.033)
RTA	0.0289	0.2198***	-0.0077	0.0123	0.0156	-0.0112
	(0.049)	(0.045)	(0.008)	(0.102)	(0.056)	(0.018)
Observations	613,203	1,382,379	559,580	372,207	1,049,281	344,269
R-squared	0.785		0.524	0.795		0.571
Firm-destination FE	Yes	Yes	Yes	Yes	Yes	
Firm-year FE	Yes	Yes	Yes	Yes	Yes	
Joint significance p-value	0.0359	0.3199	0.0066	0.0599	0.0040	0.1679
Sample probability			0.1588			0.1992

Notes: Robust standard errors, adjusted for clustering at the country level, in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.

Table B.5: Effect of Sanctions by Type and Sector

	Manufacturing			Retail & wholesale		
	ln Exports OLS (1)	Exports PPML (2)	Exit prob. LPM (3)	ln Exports OLS (4)	Exports PPML (5)	Exit prob. LPM (6)
Arms sanction	0.0052	0.1904*	0.0426*	-0.0698	0.0136	0.0278
	(0.077)	(0.099)	(0.026)	(0.080)	(0.183)	(0.025)
Military sanction	0.0150	-0.0065	0.0196	0.1712	-0.0357	-0.0027
	(0.071)	(0.083)	(0.024)	(0.115)	(0.204)	(0.026)
Travel sanction	0.0509	-0.1847*	0.0325*	-0.0016	-0.1070	0.0118
	(0.061)	(0.108)	(0.017)	(0.075)	(0.210)	(0.013)
Trade sanction	-0.0369	-0.3500***	-0.0466	0.0642	0.0240	0.0150
	(0.069)	(0.112)	(0.040)	(0.086)	(0.178)	(0.030)
Financial sanction	-0.1686***	0.0385	0.0055	-0.0912	-0.3343*	-0.0050
	(0.058)	(0.096)	(0.015)	(0.073)	(0.173)	(0.018)
Other sanctions	0.0305	0.1574*	0.0069	-0.0874	0.1578*	0.0019
	(0.054)	(0.092)	(0.016)	(0.059)	(0.096)	(0.019)
ln GDP	0.7081***	0.8364***	-0.0626***	0.4754***	0.6952***	-0.0409***
	(0.073)	(0.057)	(0.008)	(0.069)	(0.147)	(0.009)
ln Inward MRT	-0.1475	-0.0279	0.0708***	-0.2863*	-0.0625	0.1315***
	(0.117)	(0.205)	(0.021)	(0.164)	(0.377)	(0.034)
RTA	0.0306	0.2348***	-0.0032	0.0187	0.0010	-0.0098
	(0.049)	(0.049)	(0.010)	(0.106)	(0.051)	(0.019)
Observations	613,203	1,382,379	559,580	372,207	1,049,281	344,269
R-squared	0.785		0.524	0.795		0.571
Firm-destination FE	Yes	Yes	Yes	Yes	Yes	
Firm-year FE	Yes	Yes	Yes	Yes	Yes	
Joint significance p-value	0.0420	0.0000	0.0000	0.0110	0.0045	0.0856
Sample probability			0.1588			0.1992

Notes: Robust standard errors, adjusted for clustering at the country level, in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.