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The Effects of Economic Sanctions on Trade: New Evidence from a Panel PPML Gravity Approach[1]

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Abstract

Economic sanctions are a popular diplomatic tool for countries to enforce political demands abroad or to punish non-complying countries. There is an ongoing debate in the literature about whether this tool is effective in reaching these goals. This paper looks at the consequences of sanctions for bilateral trade values between 1987 and 2005. In order to quantify the direct effects of sanctions on the trade flows between countries I use PPML as well as several other econometric specifications to estimate the gravity equation with country pair, sender-time, and target-time fixed effects. Following Heid et al. (2015) I include intra-national as well as international trade flows, to reduce the endogeneity bias of trade policy instruments.

The estimates reveal that there is a significant decrease in the value of trade after the introduction of sanctions, which turns out to be driven by moderate sanctions. I also check whether countries that are affected by sanctions switch to other trade partners, but here is no robust evidence for behavior like this.

JEL classification: F13, F14

Keywords: Economic Sanctions, International Trade, Panel Gravity Model, PPML

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1 Introduction

Economic sanctions and embargoes as an alternative to brute force are popular instruments of diplomatic behavior against ill-behaving states since the beginning of the 20^{th} century, and they continue to be popular today. After the annexation of the Crimea by the Russian Federation in March 2014, the European Union (EU), the United States of America (USA), and several other states were quick to implement sanctions against Russia. Russia, in turn, reacted by implementing multilateral trade sanctions on its own, specifically, a total ban on food imports from the EU, North America, Norway and Australia. These sanctions have been renewed and are still active today. Another prominent example is the case of economic sanctions of the UN against North Korea, which have been increased in number and severity numerous times as a reaction to North Korea's continuous tests of nuclear missiles. Most recently, the USA plan to reinstate their sanctions against Iran in August 2018.

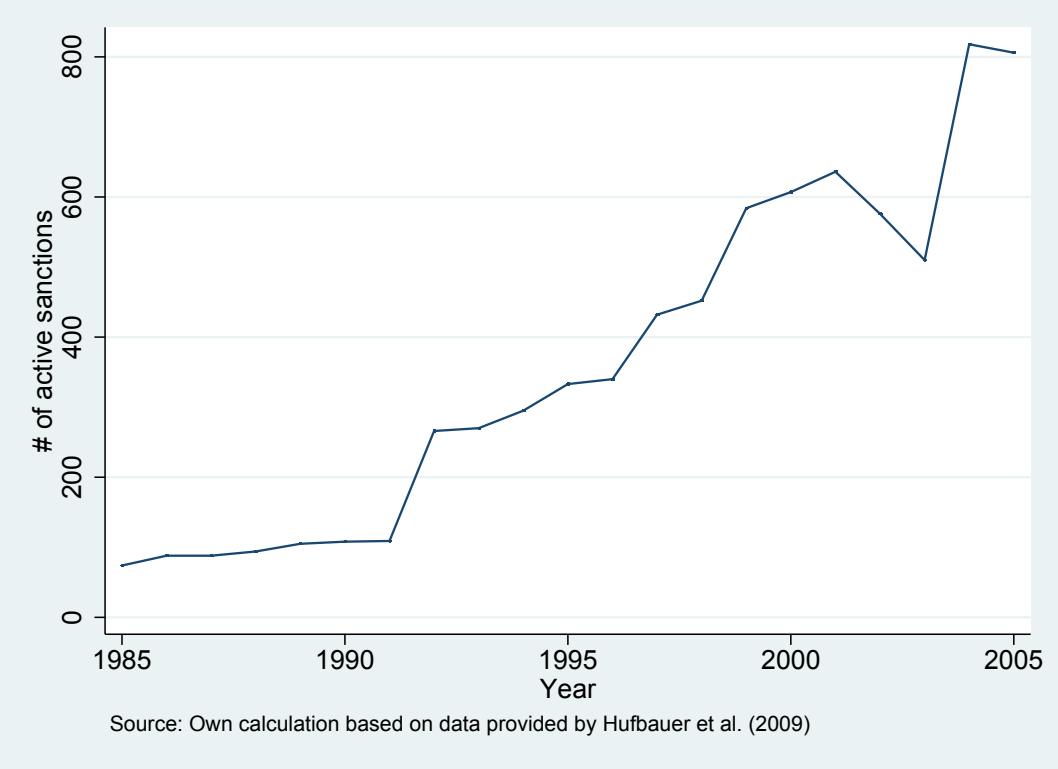
Figure 1 illustrates the number of sanctions active in a given year over the period from 1984 until 2005. It shows that the number of active economic sanctions has remained rather steady until 1990. After 1990, their usage has grown drastically, from under 100 to over 600 in just 15 years.[3]

The basic idea behind sanctions as a political instrument can be summarized by a quote of US-President Wilson from 1919: '*A nation that is boycotted is a nation that is in sight of surrender. Apply this economic, peaceful, silent, deadly remedy and there will be no need for force. It does not cost a life outside the nation boycotted, but it brings a pressure upon the nation which, in my judgment, no modern nation could resist*'.

Given the prevalence of sanctions, it is a straightforward question to ask whether they are an effective tool to enforce the goal(s) of sender countries. From an anecdotal perspective, the success rate does not seem to be overwhelming. Cuba has not abandoned its socialist regime due to pressure from the USA, Russia has not taken any steps to undo the annexation of the Crimea, and North Korea keeps testing missiles, to name just some examples. Especially North Korea has been subjected to drastic sanctions from many countries across the globe for numerous years. Hufbauer et al. (2009) show that only about one in three sanctions yields the desired political outcome.

[3] If unions like the EU or the Arab League are part of a sanction, the sanction is attributed to each member country individually.

Figure 1: Number of sanctions per year



With President Wilson's quote in mind, how is it possible for a country to resist these sanctions? Two explanations come to mind. First, it is possible that economic sanctions simply do not yield the desired punishing effect by not reducing existing trade between the sender and the targeted country. Secondly, countries that are affected by sanctions, either as a sender or a target, might switch their trade partners with little costs and therefore circumvent the expected trade reduction, which potentially offsets the negative effects of the sanctions mechanism.

In this essay I add to the sanctions literature by empirically evaluating these potential explanations. In a first step, I quantify the partial trade effect of sanctions and potential counter-sanctions on international trade by estimating a gravity equation. My preferred specification is a pseudo poisson maximum likelihood (PPML) panel estimation which includes zeros and intra-national trade flows and a comprehensive set of fixed effects. Furthermore, I use standard OLS and first difference (FD) regressions. I argue that the implementation of sanctions can be treated similarly to the formation of a regional trade agreement between two countries, but with the opposite intention, of course. Instead of abolishing tariffs and streamlining standards to facilitate trade, it is possible to interpret a sanction like the introduction of an infinitely high tariff that prevents countries from trading specific goods or from trading all together. Therefore, sanctions enter the trade costs function. Moreover, I test the

policy variables for endogeneity. The results show that the implementation of sanctions has a robust significant negative impact on bilateral trade between countries within the sample of around 9 percent when using OLS and PPML but no significant effect when using FD.

Next, I differentiate sanctions by severity types. I find that moderate sanctions are the drivers of the negative overall impact, not extensive sanctions. Limited sanctions and extensive sanctions do not influence trade significantly. I repeat this analysis for yearly data instead of using three-year intervals. The results show that the effects of sanctions become a lot more volatile and their significance depends on the choice of standard errors. To shed some light on the effectiveness of sanctions, I check for trade diversion. The results vary with the estimation method. Using OLS I find evidence for trade diversion but the result is not robust to the first differencing approach.

My data set covers the years from 1987 to 2005, making use of the Threat and Imposition of Economic Sanctions data base (TIES), the Direction of Trade Statistics data base (DOTS) and CEPII. To the best of my knowledge, nobody so far has used a data set of this magnitude to answer the questions above and has properly accounted for endogeneity, multilateral resistance, and theory consistency at the same time.

The remainder of the paper is structured as follows. The next section reviews the literature related to economic sanctions and trade. Section 3 introduces the sources and explains the composition of the data set. Section 4 provides an overview regarding the empirical specifications. Then, I present empirical results and discussions in section 4 and section 5 concludes.

2 Literature review

Several researchers have tried their hands at explaining the consequences of economic sanctions on trade from various angles, both theoretically and empirically. I here review some of the recent empirical

results based on the gravity framework.[4]

Many researchers focus on empirical effects of sanctions imposed by a *single* country, hereafter called "sender". Most chose the USA, since they are the most prominent user of economic sanctions as means of foreign policy. Hufbauer and Oegg (2003) quantify the damage of US sanctions on US trade and differentiate by severity of sanction types. The estimated negative effect of implementing an extensive sanctions in 1995 is a decrease of US exports to a sanctioned country by 99 percent and by 95 percent for 1999, while the effect of moderate and limited sanctions for the same periods is insignificant or even slightly positive. In addition, Caruso (2003) finds a large negative impact of extensive unilateral US sanctions against 49 target countries: on average, sanctions lead to a drop in US exports of 87 percent over the period from 1960 until 2000. Additionally, he offers some evidence for positive effects of trade diversion for limited and moderate sanctions by comparing US trade with countries targeted unilaterally by US sanctions to G-7 countries' trade with the same countries. Yang et al. (2004) group countries together by certain characteristics, e.g., being a former part of the Soviet Union. They cover the period from 1980 to 1998, taking 5-year intervals, and estimating each interval separately. Their results are mixed for the effects of unilateral US sanctions and their findings greatly vary with the definition of their country samples. The authors use the EU and Japan to quantify a trade diversion effect due to US sanctions but do not find evidence to support this claim.

Other authors, like Haidar (2016), explore the effects of sanctions on a single target. He focuses on sanctions targeting Iranian exporters between 2006 and 2011 and finds firm level evidence for trade diversion. According to his results, two-thirds of Iranian export value has been diverted from sanctioning to non-sanctioning countries.

The empirical results of the research mentioned above are likely to suffer from severe endogeneity bias. This is mainly due because the authors did not properly control for the multilateral resistance terms using fixed effects (see Anderson and Van Wincoop (2003)).

To shed more light on reasons for potential success or failure of economic sanctions, Early (2009) runs

[4]There are several authors who focus on effects of economic sanctions as well, but use different frameworks for their analysis: Dreger et al. (2015) focus on the depreciation of the Ruble after the Western sanctions took affect after the annexation of the Crimea and the Russian counter-sanctions that followed after 2014. Using daily exchange rate data from January 2014 to March 2015, they find that the depreciation was mainly caused by the decrease of oil prices and not so much due to economic sanctions of the West. Crozet and Hinz (2016) concentrate on the costs of imposing and maintaining sanctions on Russia for the sender countries utilizing monthly country-level trade data, from December 2013 to June 2015. Using French firm-level export data, they show that after the implementation of sanctions both, the extensive and intensive margin of exports have been strongly reduced.

a probit estimation covering the years from 1950 to 1990. He finds that close allies of a sanctioning country are most likely to increase trade with the target country, therefore helping to reduce the impact of the sanction. Using multinomial logit and data on US sanctions, Early (2011) concludes that the decision of third countries to help sanctioned countries is not driven by political but by commercial interests.

Yang et al. (2009) compare the effects of imposing sanctions on trade between the US and countries that are targeted by US sanctions with trade between the EU and those target countries between 1980 to 2003. They report that unilateral US sanctions have a negative effect on the trade value of the EU with those target countries as well. As a potential reason, the authors suggest that extensive sanctions imposed by the US may have a negative impact on a target country's total economic activity and trade.

Other authors have looked at the threat of sanctions and the optimal duration of sanctions. Afesorgbor and Mahadevan (2016) provide some evidence that the mere threat of sanctions actually boosts trade between target and sender, while imposed sanctions decrease trade. In contrast to this, Kohl and Reesink (2016) find no evidence that the threat of sanctions has any significant effect on the value of trade. Dizaji and van Bergeijk (2013) focus on the optimal duration of economic sanctions. For this, they develop a theoretical model and test it empirically via vector autoregression models by using the boycott of Iranian oil as a case study. Their key finding is that the success probability of sanctions is higher in the short run and decreases in the long run, as the economic costs reach their peak after the first two years and decrease afterwards due to economic adjustment.

Hufbauer et al. (2009) give detailed information of the goals and the success or failure of economic sanctions for the 20^{th} century. The authors find that only every third sanction is a success. Furthermore, they suggest that policy makers should use so called "smart sanctions", which target only specific sectors, instead of total embargoes because the success rate is higher.

3 Data

The information of the duration of sanctions and which countries are involved as senders and targets stems from the TIES data base by Morgan et al. (2014). It contains specific records of cases of economic sanctions, including both, their threats and impositions from 1945 until 2005. The authors differen-

tiate between 10 types of sanctions by severity. I group these sanction types into three categories, following Hufbauer and Oegg (2003), namely extensive, moderate, and limited sanctions. Extensive sanctions contain total economic embargoes and blockades, e.g., those against Cuba. Partial economic embargoes, specific import and export restrictions, and suspension of trade agreements are combined within moderate sanctions. Finally, limited sanctions refer to travel bans, termination of foreign aid, and asset freezes.

If a country has multiple sanction types in place, I only count the most severe. Sanctions that were merely threatened but never actually imposed are not included within my sample; neither is information whether sanctions ended because the goal of the sending countries was reached, or whether they were abolished because of other political reasons. Most prior empirical research of economic sanctions make use of the data set by Hufbauer et al. (2009). However, TIES offers a significant increase in the number of sanction cases.

Information of free-on-board (fob) export value on the country level is provided by the direction of trade statistics data base (DOTS) from the International Monetary Fund. To ensure theory consistent estimators of bilateral trade policy (Dai et al., 2014) and to capture the effects of globalization on international trade (Bergstrand et al., 2015), not only international but intra-national trade is included as well. Moreover, this allows to identify and estimate the effects of non-discriminatory trade policy (Heid et al., 2015). I compute intra-national trade values by taking the difference of each country's gross domestic product provided by CEPII (Head et al. (2010), Head and Mayer (2014)) and the sum of its total fob exports per year using the DOTS data.[5]

Gravity controls for distance, common language, colonial ties, contiguity, and trade agreements come from CEPII (Head et al. (2010), Head and Mayer (2014)).

Following Olivero and Yotov (2012), I use three year intervals to allow trade flows to adjust to changes in trade costs. Furthermore, I want to reduce anticipation effects of potential sanctions in the future. In conclusion, the data set covers the years 1987, 1990, 1993, 1996, 1999, 2002, and 2005 and the sample size consists of around 132,497 observations of (non-singleton) country pairs. This bilateral panel data set exceeds the data sets that have been used in the literature in time and country coverage.

[5]This shirt-sleeved approach is necessary because, so far, there is no information on aggregate intra-national trade available that covers all countries within the sample. Bergstrand et al. (2015) and Yotov (2012) use this method as well.

Table 1 provides summary statistics for the sanctions data set. Within the sample there is a total of 2,355 active trade agreements. 362 country pairs have a common colonial background, 4,096 share their primary language, and 492 countries are neighbors. Aggregate trade value varies from zero to over 300 billion USD. The closest country pair in the sample is Hongkong and Macau with a geographical distance of 60 kilometers, while the largest distance covered is from Taiwan to Paraguay.

Table 1: Summary statistics of sanctions data set

Total number of RTAs	2,355
Total number of pairs with colonial background	362
Total number of pairs with common border	492
Total number of pairs with common language	4,096
Total number of sanctions	786
Total number of limited sanctions	79
Total number of moderate sanctions	683
Total number of extensive sanctions	24

	Min	Max	Mean	Std. Dev.
Total trade (in mln USD)	0.00	302,195.4	256.16	2806.27
Distance (in km)	60.77	19,781.39	7,515.55	4,520.1
Duration of a sanction (in years)	< 1	47	8.25	9.42

The average time span of a sanction is around 8 years, but the duration varies greatly. Some only last several months, while others last up to 47 years. An example for the latter are India's sanctions against South Africa during the Apartheid.

More than 780 country pairs are affected by sanctions at least in one year over the observed period from 1987 to 2005. If sanctions are grouped due to their severity, there is a total of 24 severe, 683 moderate, and 79 limited sanctions.

4 Estimation strategy

The first specification of the gravity equation which is estimated using fixed effects OLS (FE) is given below:

$$ln(X_{ij,t}) = \beta_1 SANC_{ij,t} + \sum_{k=0}^{3} \beta_{t-k} RTA_{ij,t-k} + \rho INTL\_BRDR_{ij,t} + \mu_{i,t} + \lambda_{j,t} + \vartheta_{ij} + \epsilon_{ij,t}. \quad (1)$$

Here, $X_{ij,t}$ denotes the value of exports of sender i to target j in year t. The sanction-dummy $SANC_{ij,t}$ takes the value of 1 if country j is the target of an active sanction by country i in year t, and zero otherwise.

In order to differentiate the effects of different severity types of sanctions I classify them by groups (Hufbauer & Oegg, 2003). Furthermore, I include a dummy that captures active RTAs, $RTA_{ij,t}$, together with 3-, 6- and 9-year lags. This is done to allow for time-varying or non-linear effects of RTAs. $INTL\_BRDR_{ij,t}$ is a dummy that captures globalization effects such as technology and innovation (Bergstrand et al., 2015). It takes the value of 1 if international trade occurs, and zero otherwise. Because of perfect collinearity with the other fixed effects, the border dummy for the most recent year in the sample is dropped from the estimation.

It is possible, that shocks hit only the importer or the exporter in a given year, such as potential changes in legislature after an election within a country that could either be a boost or a hindrance to trade. To account for these multilateral resistance terms, specification (1) includes exporter-year and importer-year fixed effects denoted by $\mu_{i,t}$ and $\lambda_{j,t}$, respectively. Unobserved pair-specific characteristics affect trade flows, too (Baier & Bergstrand, 2007). To account for this, the pair fixed effect ϑ_{ij} is included.

Because of perfect collinearity, ϑ_{ij} captures all time-invariant country pair specific influences on trade, both, observable and unobservable. The drawback is that it is not possible to quantify, e.g., the effect of common language on the value of trade. The trade effects of time-varying variables, like sanctions, can still be identified.

An alternative way to control for unobserved pair-specific heterogeneity is differing the data (FD),

which is done in specification (2). It yields a difference-in-differences estimator that measures the changes on trade value if and when a country pair implements sanctions (and stops them again). The drawback is that observations are lost, if trade flows are not observed in one of the years.

$$\Delta ln(X_{ij,t}) = \beta_1 SANC_{ij,t}\Delta + \sum_{k=0}^{3} \beta_{t-k}\Delta RTA_{ij,t-k} + \rho\Delta INTL\_BRDR_{ij,t} + \mu_{i,t} + \lambda_{j,t} + \Delta\epsilon_{ij,t}. \quad (2)$$

In the presence of heteroscedasticity, however, all three specifications above are potentially biased and inconsistent due to the logarithmic form of the gravity model. The PPML approach proposed by Santos Silva and Tenreyro (2006) performs well under these circumstances, since it makes use of the multiplicative form of the gravity model. Another major advantage of the PPML method is that it allows to incorporate country pairs with zero trade flows without any manipulation of the data. Zero trade flows mostly occur for small countries. Since these countries are often the targets of sanctions, it could potentially bias the results if they are left out. This is why specification (3) given below is the preferred specification.

$$X_{ij,t} = exp\Big[\beta_1 SANC_{ij,t} + \sum_{k=0}^{3} \beta_{t-k} RTA_{ij,t-k} + \rho INTL\_BRDR_{ij,t} + \mu_{i,t} + \lambda_{j,t} + \vartheta_{ij}\Big] * \epsilon_{ij,t} \quad (3)$$

The explanatory variables are the same as in specification (1), as are the fixed effects.

5 Results

This section presents the results of the empirical estimations. In the first subsection, I show and discuss partial trade destruction effects. In the second subsection, I aim to capture trade diversion effects.

5.1 Trade destruction

Table 2 presents the estimation results of the different gravity specifications (1) to (3). For the sake of readability, only the explanatory variable of interest is shown.[6] All specifications include sender-year and target-year fixed effects. Additional controls include RTAs together with 3-, 6-, and 9-year lags and an indicator for the occurrence of international trade. In addition, specifications (1) and (3) include trade pair fixed effects. Standard errors are robust and clustered at the country-pair level, as it is common in the literature. However, in a panel gravity context, there are several other dimensions in which the errors may be correlated: at the sender, target, year, sender-year, target-year, and country-pair level, respectively (Cameron et al., 2011). Therefore, I report standard errors that are clustered at these six dimensions (multi-way) for the variables of interest as well, following Egger and Tarlea (2015). This clustering influences the size of the standard errors, and therefore, the level of significance of the reported coefficients.[7] I report the within-R^2 for the FE and FD regressions and follow the method described by Tenreyro for the PPML R^2 by computing the square of the correlation between trade and fitted values.[8]

Table 2: Trade effects of economic sanctions

	(1)	(2)	(3)	(4)
Estimation method:	FE	FD	PPML with FE sample	PPML with full sample
Sanctions	-0.074	0.003	-0.085	-0.086
	(0.033)**	(0.034)	(0.039)**	(0.038)**
	[0.037]**	[0.035]	[0.050]*	[0.050]*
	{0.060}	{0.047}	{0.062}	{0.064}
N	93828	70826	93828	132497
within R^2	0.0019	0.0003	0.0007	0.0007
Gravity controls	yes	yes	yes	yes
Pair fixed effects	yes	no	yes	yes

LHS variable columns (1) & (2): ln(export value), columns (3) & (4): export value. All estimations include sender-year and target-year fixed effects. Gravity controls include dummies for RTAs, RTA lags, and international trade. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively.
* p<0.1, ** p<0.05, *** p<0.01

The result of the FE estimation in column (1) shows that sanctions have a negative effect on the value of trade, on average of -7.1 percent ($= 100[e^{-0.074} - 1]$). The coefficient is significant at the 5 percent level.

[6] For tables with the full list of covariates, please see the Appendix
[7] If not specified otherwise, levels of significance are based on country-pair clustered errors.
[8] See her homepage for details, `http://personal.lse.ac.uk/tenreyro/LGW.html`

Column (2) shows the result for the FD approach instead of pair fixed effects. Since the first period is lost due to the estimation process, the sample size is smaller. The implementation of sanctions now seems to have no significant effect on trade.

In the last two columns the results of the preferred estimation method using PPML are presented. To show the difference between the FE and PPML estimators, column (3) shows the estimation results using the same sample size as the FE of column (1), covering only positive trade flows. At the 10 percent level of significance, the coefficient predicts an average decrease of -8.1 percent on the value of bilateral trade if sanctions are implemented. Finally, the last column makes use of the full sample including zero trade flows. The negative effect of sanctions on trade is -8.2 percent. This -8.2 percent decrease translates to a reduction of exports from the EU to Russia of about 12.9 billion USD due to active sanctions for 2016. The results of the preferred PPML estimation approaches in column (4) appear to be robust and are close to the FE result from column (1), even though the sample size differs by over 38,000 observations.

A big issue when estimating trade policy is the endogeneity of its implementation. It is not a far stretch to believe that countries are potentially reluctant to implement extensive sanctions against important trading partners but may be less so in implementing limited or moderate ones. A similar line of reasoning may hold true for RTAs. Country pair fixed effects or using the first difference should take care of this issue. To test whether strict exogeneity of the trade policy variables can be assumed, future leads are included within the preferred estimation specification (3) following Wooldridge (2010). Table 3 shows the results. Both, the future lead for RTAs as well as the future lead for sanctions are returned close to zero and insignificant when standard errors are clustered at country pairs or multi-way, allowing for the interpretation that future formation of trade agreements or future implementation of sanctions have no influence on the value of trade in the current period. These findings support the claim that there is no anticipation effect.

Table 4 offers new insights into the composition of the sanctions effect from Table 2. Here, I differentiate between the three types of sanctions, limited, moderate, and extensive, respectively. Each type is estimated individually in the columns (1) to (3) and they are estimated together in column (4). The estimation methods are the same as in Table 2. As additional controls all estimations include RTA dummies, 3-, 6-, and 9-year lags and dummies for international trade. In addition, all specifications include sender-year and target-year fixed effects. Except for the FD approach all estimations include

Table 3: Test for exogeneity of policy variables: PPML estimation

	(1)	(2)
Estimation method: PPML		
RTA	0.337	0.347
	(0.034)***	(0.034)***
	[0.046]***	[0.048]***
	{0.052}***	{0.066}***
RTA lead	-0.035	
	(0.021)*	
	[0.032]	
	{0.080}	
Sanctions	-0.088	-0.080
	(0.038)**	(0.042)*
	[0.051]*	[0.052]*
	{0.069}	{0.069}
Sanctions lead		0.037
		(0.032)
		[0.041]
		{0.060}
N	132497	132497
R^2	0.0007	0.0007
Gravity controls	yes	yes

LHS for estimation methods: export value. Gravity controls include dummies for international trade. All estimations include importer-year, exporter-year, and country pair fixed effects. The lead is three years. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively.
* p<0.10, ** p<0.05, *** p<0.01

Table 4: Trade effects of economics sanctions by severity

	(1)	(2)	(3)	(4)
Panel A Estimation method: FE				
Limited sanctions	-0.057			-0.057
	(0.146)			(0.145)
	[0.156]			(0.156)
	{0.140}			{0.141}
Moderate sanctions		-0.086		-0.086
		(0.034)**		(0.034)**
		[0.037]*		[0.037]**
		{0.058}		{0.060}
Extensive sanctions			0.312	0.312
			(0.336)	(0.336)
			[0.359]	[0.359]
			{0.182}	{0.183}
N	93828	93828	93828	93828
R^2	0.0019	0.0019	0.0019	0.0019
Panel B Estimation method: FD				
Limited sanctions	-0.043			-0.043
	(0.139)			(0.139)
	[0.143]			(0.143)
	{0.075}			{0.075}
Moderate sanctions		0.005		-0.005
		(0.035)		(0.035)**
		[0.035]		[0.035]*
		{0.047}		{0.047}
Extensive sanctions			0.074	-0.074
			(0.296)	(0.296)
			[0.310]	[0.310]
			{0.220}	{0.220}
N	70826	70826	70826	70826
R^2	0.0003	0.0003	0.0003	0.0003
Panel C.1 Estimation method: PPML (with FE sample)				
Limited sanctions	-0.011			-0.020
	(0.057)			(0.055)
	[0.057]			(0.053)
	{0.039}			{0.039}
Moderate sanctions		-0.084		-0.084
		(0.038)**		(0.038)**
		[0.050]*		[0.0510*
		{0.061}		{0.063}
Extensive sanctions			-0.458	-0.452
			(0.316)	(0.315)
			[0.388]	[0.387]
			{0.501}	{0.500}
N	93828	93828	93828	93828
R^2	0.0007	0.0007	0.0007	0.0007
Panel C.2 Estimation method: PPML (with full sample)				
Limited sanctions	-0.021			-0.030
	(0.056)			(0.057)
	[0.063]			(0.058)
	{0.032}			{0.037}
Moderate sanctions		-0.086		-0.086
		(0.038)**		(0.038)**
		[0.051]*		[0.051]*
		{0.063}		{0.074}
Extensive sanctions			-0.212	-0.212
			(0.309)	(0.399)
			[0.400]	[0.399]
			{0.181}	{0.186}
N	132497	132497	132497	132497
within R^2	0.0007	0.0007	0.0007	0.0007
Gravity controls	yes	yes	yes	yes

LHS for panel (A) & (B): ln(export value), for panel (C.1) & (C.2): export value
Gravity controls include RTA, RTA lags, and a dummy for international trade.
All estimation methods include sender-year and target-year fixed effects, methods 1, 3, & 4 include pair fixed effects. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively. * p<0.10, ** p<0.05, *** p<0.01

country-pair fixed effects as well.

Panel A provides results for the FE specification (1). The coefficient for limited sanctions is negative but does not significantly effect trade which makes economic sense, as limited sanctions do not target trade but individuals via travel bans and financial asset freezes. The coefficient for moderate sanctions predicts a decline of -8.2 percent on average for the value of trade, which is significant at the 5 percent level. The effect of extensive sanctions seems to be positive and insignificant. This result does not change, whether sanctions are included individually or together.

A different picture can be seen estimating it with FD in panel B. Like in Table 2, the FD approach leads to insignificant results for all three variables of interest, if they are estimated individually. Limited sanctions are negative, moderate sanctions are close to zero, and the coefficient for extensive sanctions is positive. However, if all three sanction types are estimated together, the coefficient for moderate sanctions returns with -0.005 and slightly significant at the 10 percent level.

The preferred PPML specification is first estimated in panel C.1 using the FE sample with positive trade flows to make it comparable with the regression from panel A. The introduction of moderate sanctions dampens trade by -8.1 percent. The effect of limited sanctions coefficient is again negative but insignificant. The coefficient for extensive sanctions is now negative and fairly large, but remains insignificant. The results remain the same, if all three sanctions dummies are included together.

Panel C.2 of the table utilizes the full sample and predicts that moderate sanctions reduce trade by -8.2 percent. In contrast to the previous PPML, the negative effect of limited sanctions increases, while the coefficient of extensive sanctions decreases. However, both remain insignificant.

The overall negative effect of sanctions seems to be driven solely by moderate sanctions within the sample. Apart from the FD approach, the coefficient remains fairly robust across all specifications. Furthermore, it makes no difference for the effects of different sanction types on trade, whether they are included individually or together in the regression.

On the first glance, it is puzzling that extensive sanctions play no significant role on the value of trade across all specifications. This counter-intuitive result may stem from the fact that these sanctions are mostly between countries that did not trade a lot with each other to begin with, like Syria and Israel.

Moreover, the number of extensive sanctions in the overall sample is very small and there is not a lot of variation within the observed time period.

These findings are quite different from previous results from the literature, where the main driver of the negative impact on trade stems from extensive sanctions. This change in results may be due to moving away from single sender or target countries and the resulting increase of the sample size and/or due to omitted variable bias in previous empirical studies.

Table 5: Trade effects of economic sanctions by severity: PPML estimation (annual data)

	(1)	(2)	(3)	(4)	(5)
Estimation method: PPML					
Sanctions	-0.055				
	(0.020)***				
	[0.037]				
	{0.042}				
Lim. sanctions		-0.116			-0.132
		(0.049)**			(0.054)**
		[0.062]*			[0.068]*
		{0.060}*			{0.064}**
Mod. sanctions			-0.054		-0.054
			(0.021)***		(0.021)***
			[0.038]		[0.038]
			{0.042}		{0.050}
Ext. sanctions				-0.216	-0.218
				(0.154)	(0.156)
				[0.314]	[0.314]
				{0.131}*	{0.128}*
N	379425	379425	379425	379425	379425
R^2	0.0008	0.0008	0.0008	0.0008	0.0008
Gravity controls	yes	yes	yes	yes	yes

LHS variable: export value. Gravity controls include dummies for RTAs, RTA lags, and for international trade. All estimations include importer-year, exporter-year, and country country pair fixed effects. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively. * p<0.1, ** p<0.05, *** p<0.01

It is possible that some sanctions began and ended between two three-year intervals. To capture those, I use yearly data instead of intervals in Table 5. This increases the number of observations from around 133,000 to nearly 380,000. In the first column, the general sanctions dummy is used. In columns (2) to (4) I distinguish once again by severity type and in column (5) I use the three types together as explanatory variables. All estimations include sender-year, target-year, and pair fixed effects. As additional controls, dummies for RTAs and international trade are added.

In the first specification, sanctions have a negative impact on the value of exports by around -5.3 percent. This effect is only significant when using heteroscedasticity-robust standard errors.

The average effect of limited sanctions presented in column 2 is given by a coefficient of -0.116 and is significant at 5 percent with robust standard errors. This effect remains statistically significant at 10 percent when clustering at country pair level and multi-way. An implementation of moderate sanctions decreases the trade value by -5.4 percent. This result is highly significant with robust standard errors and insignificant otherwise.

In the fourth column it can be shown that extensive sanctions decrease trade by around 19 percent but are they only statistically significant from zero at 10 percent when choosing multi-way clustering.

In column (5), the three severity types are once again estimated together. Like in Table 4, the results do not change and remain very robust.

The yearly effects of sanctions from Table 5 are a lot more volatile than the previous ones and their significance strongly depends on the choice of standard errors. The only persistent negative effect of sanctions stems from the implementation of limited sanctions. This seems counter-intuitive at first but it is possible that moderate and extensive sanctions can somewhat be anticipated, while travel bans and asset freezes may happen unexpectedly.

Another potential reason for the overall decrease in significance is that the data set grew in size nearly three times when using yearly data instead of intervals but the number of sanctions did not even double. This may reduce the overall impact of sanctions in this sample.

5.2 Trade diversion

In this subsection, I check for evidence of trade diversion after the imposition of a sanction within the sample. In analogy to Magee (2008) who focuses on trade diversion induced by RTAs, I capture trade diversion by means of a dummy variable. The dummy is equal to unity if either of the two countries is affected by an active sanction in year t, either as sender or as target. The dummy is zero, if i is the sender and j is the target of a sanction at time t and it is zero, if neither country is directly affected by a sanction. This means that trade diversion is defined in such a way that it only takes a positive

value if active sanctions influence one of both trade partners. Hence, the variable is not bilateral in nature but monadic. If trade diversion takes place I expect to find a positive coefficient that can offset the negative effect of a sanction. This would translate into a switch in trade away from a partner that is involved in sanctions toward one or more that are not.

In order to check for trade diversion, it is no longer possible to make use of the preferred PPML specification (3) because the trade diversion dummy would be subsumed by either the sender-time or target-time fixed effect. I use FE and FD for the estimation. The respective equations are given below:

$$ln(X_{ij,t}) = \beta_1 SANC_{ij,t} + \beta_2 TD_{it} + \beta_3 TD_{jt} + \sum_{k=0}^{3} \beta_{t-k} RTA_{ij,t-k} + \rho INTL\_BRDR_{ij,t} + \gamma MLRT_{ijt} + \vartheta_{ij} + \delta_i + \delta_j + \kappa_t + \epsilon_{ij,t} \quad (4)$$

and

$$\Delta ln(X_{ij,t}) = \beta_1 \Delta SANC_{ij,t} + \beta_2 \Delta TD_{it} + \beta_3 \Delta TD_{jt} + \sum_{k=0}^{3} \beta_{t-k} RTA_{ij,t-k} + \rho \Delta INTL\_BRDR_{ij,t} + \gamma \Delta MLRT_{ijt} + \kappa_t + \epsilon_{ij,t} \quad (5)$$

Since both, sender and targets of sanctions, can potentially divert their trade I include measures for both, TD_{it} and TD_{jt}, respectively. The explanatory variables are the same as in specification (1) but, instead of the country year fixed effects, year fixed effects κ_t, sender fixed effects δ_i, and target fixed effects δ_j, are included. Differencing again takes care of all time invariant fixed effects, therefore only the year fixed effect, κ_t, remains in the second equation.

To correct for the omission of country year fixed effects and, therefore, the omission of measures of prices, I follow the methodology of Baier and Bergstrand (2009) and use their measure to model country i's multilateral resistance to export and country j's multilateral resistance to import. $MRDIST_{ij,t}$

yields the multilateral resistance for bilateral distance between country pair ij at year t:

$$MRDIST_{ij,t} = \left[\left(\sum_{k=1}^{N} \theta_{k,t} DIST_{ik}\right) + \left(\sum_{m=1}^{N} \theta_{m,t} lnDIST_{mj}\right) - \left(\sum_{k=1}^{N}\sum_{m=1}^{N} \theta_{k,t}\theta_{m,t} lnDIST_{km}\right)\right], \quad (6)$$

with $\theta_{l,t} = \frac{GDP_{l,t}}{\sum_l^N GDP_{l,t}},\ l \in k, m.$

The coefficients for the multilateral resistance terms for border crossings of trade, RTAs, contiguity, and common language over time are defined similarly.

Table 6: Trade-diversion effects of economic sanctions

	(1)	(2)
Estimation method:	FE	FD
Sanction	0.078	-0.065
	(0.033)**	(0.037)*
	[0.038]**	[0.039]*
	{0.122}	{0.216}
Trade diversion of target	0.003	-0.004
	(0.002)*	(0.002)*
	[0.002]*	[0.002]*
	{0.007}	{0.005}
Trade diversion of sender	0.006	-0.001
	(0.001)***	(0.002)
	[0.002]***	[0.002]
	{0.006}	{0.005}
N	93869	70867
within R^2	0.0052	0.0005
Gravity controls	yes	yes
Pair fixed effects	yes	no
Year fixed effects	yes	yes
Sender, target fixed effects	yes	no

LHS variable: ln(export value). Gravity controls include dummies for RTAs, RTA lags, international trade, and controls for multilateral resistance following Baier and Bergstrand (2009). Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively.
* p<0.10, ** p<0.05, *** p<0.01

The findings of both estimations are combined in Table 6, which once again only reports the variables

of interest. The result for the FE regression in column (1) would imply that sanctions seem to have a positive impact on exports to sanctioned countries. Trade diversion seems to take place within the sample. In the presence of a sanction, trade to other countries rises on average by 0.6 percent for the sending country. Target countries seem to be able to divert 0.3 percent of their trade successfully. The coefficients are significant at the 1 percent level and 10 percent level, respectively.

The coefficients for trade diversion are positive and somewhat significant, but the point estimates are fairly small. A potential explanation is that countries that are influenced by sanctions, either as senders or as targets, split their lost trade across multiple new partners. If each of these new partners absorbs only a fraction of the total loss due to a sanction, then the changes could vanish in the aggregated value of exports.

The positive effect of trade diversion for the sending countries is twice the size of the one for target countries. This makes sense, because sending countries know about the implementation of sanctions and are able to think about potential new partners beforehand. The positive coefficient could also be a possible explanation, why some countries are very quick to implement sanctions. If the implementation of sanctions does not hurt the value of overall trade of a sending country, policy makers may not care too much whether the goal of the sanction is actually possible.

The positive effect of sanctions is puzzling and counter-intuitive. It is possible that the explanatory variables do not control for multilateral resistance as well as country-year fixed effects. Moreover, the presence of heteroscedasticity potentially affects both estimators. This may bias the results.

Using FD, the negative direct effect of sanctions re-emerges. Moreover, exports to targeted countries seem to fall. This lends support to the hypothesis that other countries reduce exports to a targeted country as well, without formally imposing a sanction (Early, 2009). Exports of sender countries to other countries do not seem to be affected. This could mean that senders only impose sanctions on targets that are not too important for their exports.

The FD-approach performs better with respect to the credibility of the sanctions dummy. The coefficient returns with -6.5 and is close to the estimated results in Tables 2 and 4. A possible interpretation for the negative coefficient for target trade diversion could be that countries that do not actively impose sanctions show solidarity with the sender and, as a consequence, additionally divert trade away from

the target. However, this approach most likely suffers from the same potential endogeneity problems as the FE.

In conclusion, the results are very volatile and depend strongly on the choice of the estimation method. Furthermore, since multilateral resistance is not controlled for by country-year fixed effects, it is possible that the results suffer from omitted variable bias. Finally, the preferred PPML method can not be applied as a robustness test with a data set of this magnitude (yet). Therefore, the results have to be treated with caution.

6 Concluding Remarks

The goal of this essay was to quantify partial trade effect of sanctions on exports using a modern estimation technique and to test its robustness against several econometric specifications commonly used in the literature. In contrast to previous research, the sample size is increased and it includes multiple senders and targets of sanctions. Furthermore, it sheds some light on the question if trade sanctions are potentially offset by the occurrence of trade diversion. For this, information containing bilateral international and intra-national trade values has been merged with gravity controls and with data regarding the imposition- and end-year as well as the severity of occurring economic sanctions between country pairs.

The evidence presented in the previous section shows that, indeed, trade sanctions have a significant and robust negative impact on the value of trade of around -8 percent when using FE and PPML across three-year intervals. If sanctions are grouped according to severity, it can be seen that the size of the negative impact is mostly due to moderate sanctions, which specifically target single sectors. The implementation of limited sanctions does not seem influence trade at all within the sample. The same holds true for extensive sanctions, which are the main drivers in related literature.

When applying yearly data, the coefficient of limited sanctions remains statistically significant and predicts a decrease of trade due to sanctions of around -11 percent. The significance of other specifications depends on the choice of standard errors. It is possible that there is an anticipation effect for moderate and extensive sanctions, but not for limited sanctions. Another possible reason is that the yearly data set includes too few active sanctions relative to the overall sample to significantly influence

trade.

The evidence for trade diversion is volatile within the sample and depends on the estimation method. If using FE, sanction-sending countries are able to divert trade away from sanctioned partners, increasing average trade value on average by around 0.06 percent. Target countries experience a positive impact of trade diversion on average trade by 0.04 percent. In addition, the coefficients predict that sanctions have a positive effect on trade.

With the FD-approach, the sanctions dummy is negative and there is no evidence for trade diversion regarding countries that are senders of economic sanctions. However, there appears to be a negative effect of trade diversion for targets of sanctions.

For future research it would be interesting to include year-sanction interactions into the estimations to see if different types of sanctions behave differently over time in order to find the optimal duration of a sanction.

New insights regarding the effect of trade diversion could come from applying a two-step estimation strategy that could allow to estimate trade diversion using PPML. Moreover, it would be interesting to analyze the effects of sanctions on sectoral trade, because sanctions typically focus on particular sectors. This would require more detailed information about sanctions, which is not available at the moment.

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A Appendix

In the following, all estimation tables are presented with all explanatory variables, except for the fixed effects dummies. *INT_BRDR* captures the effect of globalization by being 1 if trade across state borders takes place and zero otherwise. *CLNY* represents the colony dummy, *LANG* common language between country pairs, *DIST* bilateral distance, and *CNTG* contiguity. To account for multilateral resistance, all explanatory variables in Table A.8 except for sanction and trade diversion are transformed following Baier and Bergstrand (2009) and are given by *mrdis*, *mrborder*, *mrrta*, *mrcntg*, *mrlang*, and *mrclny*.

Table A.1: Trade effects of economic sanctions

Estimation method:	(1) FE	(2) FD	(3) PPML with FE sample	(4) PPML with full sample
Sanctions	-0.074	0.003	-0.085	-0.086
	(0.033)**	(0.034)	(0.039)**	(0.038)**
	[0.037]**	[0.035]	[0.050]*	[0.050]*
	{0.060}	{0.047}	{0.062}	{0.064}
RTA	0.212***	0.052	0.253***	0.270***
	(0.036)	(0.035)	(0.053)	(0.053)
RTA_LAG3	0.038	0.030	0.132***	0.133***
	(0.034)	(0.032)	(0.027)	(0.027)
RTA_LAG6	0.203***	0.164***	0.029	0.030
	(0.037)	(0.035)	(0.022)	(0.022)
RTA_LAG9	0.130***	0.018	-0.027	-0.033
	(0.038)	(0.038	(0.028)	(0.028)
INTL_BRDR_1987	-0.424***	-0.727***	-0.398***	-0.411***
	(0.125)	(0.140)	(0.043)	(0.045)
INTL_BRDR_1990	-0.437***	-0.647***	-0.400***	-0.409***
	(0.106)	(0.117)	(0.043)	(0.044)
INTL_BRDR_1993	-0.400***	-0.554***	-0.479***	-0.491***
	(0.092)	(0.099)	(0.035)	(0.036)
INTL_BRDR_1996	-0.281***	-0.367***	-0.330***	-0.332***
	(0.081)	(0.086)	(0.030)	(0.030)
INTL_BRDR_1999	-0.262***	-0.311***	-0.219***	-0.222***
	(0.068)	(0.071)	(0.028)	(0.028)
INTL_BRDR_2002	-0.155***	-0.189***	-0.158***	-0.160***
	(0.049)	(0.049)	(0.016)	(0.016)
N	93828	70826	93828	132497
within R^2	0.0019	0.0003	0.0007	0.0007
Pair fixed effects	yes	no	yes	yes
Sender-year, target-year fixed effects	yes	yes	yes	yes

LHS variable columns (1) & (2): ln(export value), columns (3) & (4): export value
Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively. * p<0.1, ** p<0.05, *** p<0.01

Table A.2: Test for exogeneity of policy variables: PPML estimation

	(1)	(2)
Estimation method: PPML		
RTA	0.337***	0.347***
	(0.034)***	(0.034)***
	[0.046]***	[0.048]***
	{0.052}***	{0.066}***
RTA lead	-0.035	
	(0.021)*	
	[0.032]	
	{0.080}	
INTL_BRDR_1987	-0.452***	-0.472***
	(0.041)	(0.032)
INTL_BRDR_1990	-0.409***	-0.431***
	(0.043)	(0.031)
INTL_BRDR_1993	-0.472***	-0.494***
	(0.037)	(0.026)
INTL_BRDR_1996	-0.325***	-0.350***
	(0.037)	(0.026)
INTL_BRDR_1999	-0.227***	-0.226***
	(0.029)	(0.029)
INTL_BRDR_2002	-0.159***	-0.158***
	(0.015)	(0.015)
Sanctions	-0.088*	-0.080
	(0.038)**	(0.042)*
	[0.051]*	[0.052]*
	{0.069}	{0.069}
Sanctions lead		0.037
		(0.032)
		[0.041]
		{0.060}
N	132497	132497
R^2	0.0007	0.0007

LHS for estimation methods: export value. All estimations include importer-year, exporter-year, and country pair fixed effects. The lead is three years. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively. * p<0.10, ** p<0.05, *** p<0.01

Table A.3: Trade effects of economic sanctions by severity: FE estimation

	(1) lim. sanctions	(2) mod. sanctions	(3) ext. sanctions
RTA	0.213***	0.212***	0.213***
	(0.036)	(0.036)	(0.036)
RTA_LAG3	0.038	0.038	0.038
	(0.034)	(0.034)	(0.034)
RTA_LAG6	0.204***	0.203***	0.204***
	(0.037)	(0.037)	(0.037)
RTA_LAG9	0.131***	0.130***	0.131***
	(0.038)	(0.038)	(0.038)
INTL_BRDR_1987	-0.424***	-0.424***	-0.424***
	(0.125)	(0.125)	(0.125)
INTL_BRDR_1990	-0.436***	-0.436***	-0.436***
	(0.106)	(0.106)	(0.106)
INTL_BRDR_1993	-0.399***	-0.399***	-0.399***
	(0.092)	(0.092)	(0.092)
INTL_BRDR_1996	-0.281***	-0.281***	-0.281***
	(0.081)	(0.081)	(0.081)
INTL_BRDR_1999	-0.261***	-0.261***	-0.261***
	(0.068)	(0.068)	(0.068)
INTL_BRDR_2002	-0.155***	-0.155***	-0.154***
	(0.049)	(0.049)	(0.049)
Sanction type	-0.057	-0.086	0.310
	(0.146)	(0.034)**	(0.336)
	[0.156]	[0.037]**	[0.359]
	{0.140}	{0.058}	{0.182}
N	93828	93828	93828
within R^2	0.0019	0.0019	0.0019

LHS for estimation methods: export value. All estimations include importer-year, exporter-year, and country pair fixed effects. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively.
* p<0.10, ** p<0.05, *** p<0.01

Table A.4: Trade effects of economic sanctions by severity: FD estimation

	(1) lim. sanctions	(2) mod. sanctions	(3) ext. sanctions
D.RTA	0.053	0.053	0.053
	(0.035)	(0.035)	(0.035)
D.RTA_LAG3	0.030	0.030	0.030
	(0.032)	(0.032)	(0.032)
D.RTA_LAG6	0.164***	0.164***	0.164***
	(0.035)	(0.035)	(0.035)
D.RTA_LAG9	0.018	0.018	0.018
	(0.038)	(0.038)	(0.038)
D.INTL_BRDR_1987	-0.728***	-0.728***	-0.728***
	(0.140)	(0.140)	(0.140)
D.INTL_BRDR_1990	-0.649***	-0.648***	-0.648***
	(0.117)	(0.117)	(0.117)
D.INTL_BRDR_1993	-0.557***	-0.557***	-0.557***
	(0.099)	(0.099)	(0.099)
D.INTL_BRDR_1996	-0.372***	-0.372***	-0.372***
	(0.086)	(0.086)	(0.086)
D.INTL_BRDR_1999	-0.310***	-0.310***	-0.310***
	(0.071)	(0.071)	(0.071)
D.INTL_BRDR_2002	-0.189***	-0.189***	-0.189***
	(0.049)	(0.049)	(0.049)
D.sanction type	-0.043	0.005	0.074
	(0.139)	(0.035)	(0.296)
	[0.143]	[0.035]	[0.310]
	{0.075}	{0.047}	{0.220}
N	70826	70826	70826
within R^2	0.0003	0.0003	0.0003

LHS for estimation methods: export value. All estimations include importer-year and exporter-year fixed effects. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively. p<0.10, ** p<0.05, *** p<0.01

Table A.5: Trade effects of economic sanctions by severity: PPML estimation (FE sample)

	(1) lim. sanctions	(2) mod. sanctions	(3) ext. sanctions
RTA	0.251***	0.253***	0.251***
	(0.051)	(0.053)	(0.051)
RTA_LAG3	0.137***	0.132***	0.137***
	(0.027)	(0.027)	(0.027)
RTA_LAG6	0.027	0.029	0.028
	(0.024)	(0.022)	(0.024)
RTA_LAG9	-0.022	-0.027	-0.022
	(0.029)	(0.028)	(0.029)
INTL_BRDR_1987	-0.377***	-0.398***	-0.377***
	(0.049)	(0.043)	(0.049)
INTL_BRDR_1990	-0.380***	-0.400***	-0.379***
	(0.047)	(0.043)	(0.047)
INTL_BRDR_1993	-0.465***	-0.479***	-0.465***
	(0.038)	(0.035)	(0.038)
INTL_BRDR_1996	-0.322***	-0.330***	-0.322***
	(0.033)	(0.030)	(0.033)
INTL_BRDR_1999	-0.215***	-0.219***	-0.215***
	(0.030)	(0.028)	(0.030)
INTL_BRDR_2002	-0.154***	-0.158***	-0.154***
	(0.017)	(0.016)	(0.017)
Sanction type	-0.011	-0.084	-0.458
	(0.057)	(0.038)**	(0.316)
	[0.057]	[0.050]*	[0.388]
	{0.039}	{0.061}	{0.501}
N	93828	93828	93828
R^2	0.0007	0.0007	0.0007

LHS for estimation methods: export value. All estimations include importer-year, exporter-year, and country pair fixed effects. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively.
* p<0.10, ** p<0.05, *** p<0.01

Table A.6: Trade effects of economic sanctions by severity: PPML estimation (full sample)

	(1) lim. sanctions	(2) mod. sanctions	(3) ext. sanctions
RTA	0.267***	0.270***	0.267***
	(0.051)	(0.053)	(0.051)
RTA_LAG3	0.139***	0.133***	0.139***
	(0.027)	(0.027)	(0.027)
RTA_LAG6	0.029	0.030	0.029
	(0.024)	(0.022)	(0.024)
RTA_LAG9	-0.028	-0.033	-0.028
	(0.029)	(0.028)	(0.029)
INTL_BRDR_1987	-0.389***	-0.411***	-0.389***
	(0.050)	(0.045)	(0.050)
INTL_BRDR_1990	-0.388***	-0.409***	-0.388***
	(0.049)	(0.044)	(0.049)
INTL_BRDR_1993	-0.478***	-0.491***	-0.478***
	(0.039)	(0.036)	(0.039)
INTL_BRDR_1996	-0.324***	-0.332***	-0.324***
	(0.033)	(0.030)	(0.033)
INTL_BRDR_1999	-0.217***	-0.222***	-0.218***
	(0.030)	(0.028)	(0.030)
INTL_BRDR_2002	-0.156***	-0.160***	-0.156***
	(0.017)	(0.016)	(0.017)
Sanction type	-0.021	-0.086	-0.212
	(0.056)	(0.038)**	(0.309)
	[0.063]	[0.051]*	[0.400]
	{0.063}	{0.051}	{0.400}
N	132497	132497	132497
R^2	0.0007	0.0007	0.0007

LHS for estimation methods: export value. All estimations include importer-year, exporter-year, and country pair fixed effects. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively.
* p<0.10, ** p<0.05, *** p<0.01

Table A.7: Trade effects of economic sanctions by severity: PPML estimation (annual data)

	(1) Sanctions	(2) Lim. sanctions	(3) Mod. sanctions	(4) Ext. sanctions
RTA	0.330***	0.330***	0.330***	0.330***
	(0.043)	(0.042)	(0.043)	(0.042)
INTL_BRDR_1987	-0.470***	-0.459***	-0.470***	-0.459***
	(0.034)	(0.038)	(0.034)	(0.038)
INTL_BRDR_1988	-0.446***	-0.436***	-0.446***	-0.436***
	(0.035)	(0.038)	(0.035)	(0.038)
INTL_BRDR_1989	-0.430***	-0.418***	-0.430***	-0.418***
	(0.036)	(0.037)	(0.036)	(0.037)
INTL_BRDR_1990	-0.428***	-0.416***	-0.427***	-0.416***
	(0.033)	(0.035)	(0.033)	(0.035)
INTL_BRDR_1991	-0.447***	-0.436***	-0.447***	-0.436***
	(0.031)	(0.032)	(0.031)	(0.032)
INTL_BRDR_1992	-0.484***	-0.475***	-0.484***	-0.475***
	(0.029)	(0.031)	(0.029)	(0.031)
INTL_BRDR_1993	-0.492***	-0.484***	-0.491***	-0.484***
	(0.029)	(0.031)	(0.029)	(0.031)
INTL_BRDR_1994	-0.434***	-0.426***	-0.434***	-0.426***
	(0.028)	(0.031)	(0.028)	(0.031)
INTL_BRDR_1995	-0.364***	-0.357***	-0.364***	-0.357***
	(0.029)	(0.031)	(0.029)	(0.031)
INTL_BRDR_1996	-0.344***	-0.340***	-0.344***	-0.340***
	(0.030)	(0.032)	(0.030)	(0.032)
INTL_BRDR_1997	-0.285***	-0.280***	-0.285***	-0.280***
	(0.033)	(0.034)	(0.033)	(0.034)
INTL_BRDR_1998	-0.280***	-0.276***	-0.280***	-0.276***
	(0.035)	(0.036)	(0.035)	(0.036)
INTL_BRDR_1999	-0.225***	-0.222***	-0.225***	-0.223***
	(0.028)	(0.030)	(0.028)	(0.030)
INTL_BRDR_2000	-0.093***	-0.089***	-0.093***	-0.090***
	(0.023)	(0.024)	(0.023)	(0.024)
INTL_BRDR_2001	-0.132***	-0.130***	-0.132***	-0.130***
	(0.020)	(0.021)	(0.020)	(0.021)
INTL_BRDR_2002	-0.157***	-0.155***	-0.157***	-0.155***
	(0.015)	(0.016)	(0.015)	(0.016)
INTL_BRDR_2003	-0.144***	-0.142***	-0.144***	-0.143***
	(0.011)	(0.011)	(0.011)	(0.011)
INTL_BRDR_2004	-0.066***	-0.064***	-0.066***	-0.064***
	(0.006)	(0.006)	(0.006)	(0.006)
Sanction type	-0.055	-0.116	-0.054	-0.216
	(0.020)***	(0.049)**	(0.021)***	(0.157)
	[0.037]	[0.062]*	[0.038]	[0.314]
	{0.042}	{0.060}*	{0.042}	{0.131}*
N	379425	379425	379425	379425
R^2	0.0008	0.0008	0.0008	0.0008

LHS for estimation methods: export value. All estimations include importer-year, exporter-year, and country pair fixed effects. Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively.
* p<0.10, ** p<0.05, *** p<0.01

Table A.8: Trade-diversion effects of economic sanctions

Estimation method:	(1) FE	(2) FD
Sanction	0.078	-0.065
	(0.033)**	(0.037)*
	[0.038]**	[0.039]*
	{0.122}	{0.216}
Trade diversion of target	0.003	-0.004
	(0.002)*	(0.002)*
	[0.002]*	[0.002]*
	{0.007}	{0.005}
Trade diversion of sender	0.006	-0.001
	(0.001)***	(0.002)
	[0.002]***	[0.002]
	{0.006}	{0.005}
mrdis	52.525***	-34.226**
	(14.419)	(16.027)
mrborder	-598.846***	225.881*
	(123.027)	(135.274)
mrrta	67.920***	-15.583
	(11.441)	(13.928)
mrcntg	1197.672***	233.236**
	(96.464)	(107.961)
mrlang	-94.444**	-75.458*
	(42.925)	(45.499)
mrclny	-58.161	151.770*
	(77.259)	(77.694)
INTL_BRDR_1987	-0.540***	-0.767***
	(0.063)	(0.138)
INTL_BRDR_1990	-0.447***	-0.642***
	(0.056)	(0.110)
INTL_BRDR_1993	-0.506***	-0.562***
	(0.052)	(0.092)
INTL_BRDR_1996	-0.345***	-0.359***
	(0.046)	(0.072)
INTL_BRDR_1999	-0.255***	-0.276***
	(0.037)	(0.053)
INTL_BRDR_2002	-0.164***	-0.177***
	(0.031)	(0.034)
RTA	0.174***	0.031
	(0.034)	(0.032)
RTA_LAG3	0.013	-0.007
	(0.029)	(0.028)
RTA_LAG6	0.093***	0.086***
	(0.030)	(0.030)
RTA_LAG9	0.048	0.090***
	(0.031)	(0.034)
N	93869	70867
R^2	0.883	0.028
Pair fixed effects	yes	no
Year fixed effects	yes	yes
Sender, target fixed effects	yes	no

LHS variable: ln(export value). Controls for multilateral resistance follow Baier and Bergstrand (2009). Standard errors in parentheses are robust, clustered at country pair level, and multi-way clustered, respectively. * p<0.10, ** p<0.05, *** p<0.01

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