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Which sanctions matter? analysis of the EU/russian sanctions of 2014

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ABSTRACT

We use a quasi-natural experiment of reciprocal imposition of trade sanctions by Russia and the EU since 2014. Using UNCTAD/BACI bilateral flows data we take this unique opportunity to analyse both sanctions. In particular, we study the effectiveness of narrow versus broadly defined sanctions, and differences in the effectiveness of sanctions imposed on exports and imports. We show that the Russian sanctions imposed on European and American food imports resulted in about an 8 times stronger decline in trade flows than those imposed by the EU and the US on exports of extraction equipment. These results do not appear to be driven by diversion of trade flows via non-sanctioning countries. Hence the difference in sanctions' effectiveness can be attributed to the limited retroactivity of Western sanctions, which allowed exemptions for exports made pursuant to contracts made prior to 2014.

1. Introduction

International trade, along with other macroeconomic phenomena, is of prime interest to policy makers, and, at the same time, poorly accessible to empirical scrutiny due to the rarity of natural experiments (or even quasi-natural experiments) that would reveal changes in trade flows in response to a change in a single variable ceteris paribus. In this paper, we utilize trade sanctions imposed by the EU and the US on (specific) trade exchanges with Russia, and broader sanctions that Russia, in response, imposed on imported EU food products. These offer a unique opportunity to analyze trade sanctions as the data have been generated by a quasi-natural experiment.

The motivation for focusing on economic sanctions and their effectiveness is the puzzlingly mixed evidence on the topic. Hufbauer et al. (2007) conclude that economic sanctions appear to be effective in compelling the target country to make concessions to the sender countries in about one third of cases (for earlier versions of this analysis see Hufbauer et al., 1985; 1990). However, these findings have been found to be sensitive to the choice of econometric specification (Drury, 1998). Furthermore, Pape (1997) pointed out that Hufbauer et al. (1990) seemed to have coded episodes as "successes" of economic sanctions in several cases which

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did not warrant it,[2] thereby compounding the uncertainty surrounding their empirical findings.

In a long stream of economic literature, especially in the field of political economy, several hypotheses have been put forward that might explain the limited effectiveness of sanctioning measures. Galtung (1967) suggests that curtailing international trade may stimulate the target country's internal markets and potentially provoke perverse political responses in the target country (see also Brooks, 2002, for a similar view). Another salient threat to the effectiveness of sanctions is the formation and enforcement of a multilateral agreement on the imposition of measures that create costs to the sender countries (e.g. Mansfield, 1995; Kaempfer and Lowenberg, 1999; Drezner, 2000). In fact, since securing the universal enforcement of sanctions appears so difficult, it has been suggested that economic sanctions ought not to be viewed as a means of punishing the target country, but rather as a way of advancing the agendas of lobbying groups within the sender countries (Kaempfer and Lowenberg, 1988). This public choice perspective is pertinent to the present application because it predicts that sanctions against exports will be more difficult to impose and sustain compared to sanctions against imports. As Kaempfer and Lowenberg (1988) themselves put it, "since producers are a more cohesive and politically effective interest group than consumers, sanctions are more likely to restrict imports from the target country than exports to the target" (p. 790).[3] Alternatively, sanctions might be seen as a form of signaling, in which the target country's government seeks to persuade its voters and allies abroad that it is willing to take a tough stance against adversaries (Lindsay, 1986).

While the importance of these political-economic considerations has been stressed for more than half a century, there is little empirical evidence of sanctions being imposed as a result of a public choice decision-making. The political economy literature predicts that sanctions will reflect the concerns of politically influential interest groups within the sender countries, while targeting politically influential groups in the target country. Sanctions are, therefore, not expected to be costly to firms (and voters) in the sender country unless there is a sufficiently powerful utility gain from sanctions that would offset these costs. In this paper, we show that these theoretical predictions map neatly on the empirical reality: we show that the Western sanctions caused minimal-to-no decline of the trade flows in equipment required for the extraction of oil and natural gas. Due to the European reliance on Russian energy imports, the costs of disrupting these trade flows could be very severe and likely to foster opposition to sanctions from European firms and voters alike. Conversely, Russian sanctions against European imports of foodstuffs create protectionist measures that benefit Russian producers, who are then motivated to lobby their government to perpetuate the sanctioning regime. Hence, the costs of sanctions to Russia are much smaller (or potentially negative) and thus Russian counter-sanctions may be enforced resolutely. More broadly, it can be said that the ban on exports removes foreign markets for the exporters in the sanctioning country, while a ban on imports creates new markets for the domestic producers in the target country, so import restrictions are more likely to garner political support than export restrictions (Brooks, 2002).

Since any effects of sanctions on economic variables (such as policy outcomes or consumer welfare) are necessarily predicated on the sanctions' effect on trade flows, we focus on trade flows themselves rather than these other variables. This approach allows us to observe the direct effect of the restrictive measures instead of an effect mediated through a host of other variables, thereby offering a clearer view of the dynamics induced by sanctions. Our identification strategy exploits the recent episode of EU and US sanctions against the Russian Federation in response to the Russian annexation of the Crimean peninsula. This situation is particularly interesting as it consists of two different sanctions packages: first the Western restrictions on exports of equipment for oil and gas extraction into Russia, and second the Russian counter-sanctions against the imports of Western foodstuffs. To our knowledge, this paper is the first to analyze a quasi-natural experiment that involves a dual sanctions episode. The significance of this pairing of the restrictive measures in this instance is the plausibility of claiming that other factors are held constant. Both of these twin sanctions packages involve the same set of countries, they occur in the same time frame, and they were both prompted by the same sequence of events. Therefore, the differences in the sanctions' effectiveness can be securely attributed to the differences in their implementation.

For identification purposes, the dramatic events of 2014 in Ukraine, which precipitated rounds of sanctions on imports into the Russian Federation, are an extraordinary opportunity to analyze the dynamics of international trade flows in response to restrictive measures. This episode has a solid claim to be a quasi-natural experiment, due to the geo-political considerations that drove the imposition of sanctions. If the sanctions had been imposed on primarily economic grounds, then there would have been strong reasons to suspect that countries selected endogenously into the sanctions regime. In this case, the alliance between the US and the EU created powerful incentives to cooperate against Russia, despite the misgivings of individual states. Thus, the selection into sanctions may be viewed as nearly-randomly assigned, opening a unique window into the effectiveness of international sanctions. The similar pre-sanction trends in related product categories, with the countries not joining the imposed sanctions, support the quasi-experimental design. Absence of selection on trends could be explained by two factors: 1) the sanction was unexpected and 2) rather than selecting on trends in trade flows, the sanctions seem to be selected on levels. In other words, sanctions targeted goods which were traded in large volumes, not goods with the fastest (or slowest) growth rate in the traded volumes.

The claim of a quasi-natural experiment is further bolstered by the timing of the imposition of sanctions. The sectoral sanctions analyzed here entered into force in August of 2014, less than six months after the Russian invasion of Crimea. This narrow window of time argues strongly that, from the economic standpoint, the sanctions were an unexpected shock to the exporters and importers alike, who were thus precluded from adapting to the new situation (see also firm-level microdata evidence from Miromanova, 2019).

[2] These concerns remain unanswered in Hufbauer et al. (2007). For a partial rebuttal, see Elliott (1998), which is addressed in Pape (1998).

[3] Specifically, Kaempfer and Lowenberg (1988) derive a public choice model of international sanctions, in which they show that a sanctioning policy in equilibrium satisfies three conditions: (i) it minimises costs of sanctions incurred by politically influential groups within the sender country, (ii) it maximises costs of sanctions incurred by politically influential groups within the target country, and (iii) maximises utility generated by participating in the sanctioning regime among politically influential groups within the sender country.

We address this issue, including formal tests, in detail in the methodology section. Obviously, the country selection into the sanctioning regime against Russia is correlated with their trade flows for the sanctioned goods. For this reason, we opted for a differences-in-differences model and conducted the standard test of differences in trends prior to the imposition of sanctions. The test did not reject the null hypothesis of trends being the same for sanctioning and not sanctioning countries. Hence, the data are consistent with the assumption of no selection on trends, which is required for quasi-natural and randomized experiments.

The findings of this paper complement several earlier works that attribute the decline of trade with Russia to a decline in oil prices and weakening of the Russian ruble, as well as finding that sanctions on their own had a rather modest impact (Dreger et al., 2016; Ahn and Ludema, 2016; Crozet and Hinz, 2016). The closest work to this paper is the analysis by Crozet and Hinz (2016), who use the same dataset but focus on the effects of sanctions on non-sanctioned trade flows. In addition, unlike Crozet and Hinz (2016), who use a gravity model of trade, we opt for a differences-in-differences specification. We select a sample of very similar goods and compare trade flows of those that are subject to sanctions with those that are not. In this way, we are estimating the effect of sanctions without general equilibrium effects, but at the same time, our estimates exploit the quasi-natural experiment that occurred in this episode. By comparing similar goods, some of which were assigned into sanctions treatment, we argue that we obtain causal estimates.[4] Furthermore, this sample construction avoids the problem of potential endogenous selection of sanctioned sectors, since the counterfactuals are created from goods that belong to the same sector as the sanctioned ones.

Finally, we note that analysis of other components of the Western sanctions package, such as restrictions imposed on specific firms and individuals is beyond the scope of this study, which focuses on aggregate trade flows. Indeed, there is evidence that entities subject to these specific sanctions were impacted significantly, even though the impact on the Russian economy as a whole was likely modest (Ahn and Ludema, 2016). These findings are consistent with the theoretical literature, which stresses the importance of aiming sanctions on politically important groups within the target countries (Galtung, 1967; Brooks, 2002). As emphasised by Ahn and Ludema (2016), the main focus of the Western sanctioning measures were highly politically connected Russian firms and individuals, while the sectoral sanctions were largely complementary to the individual-specific sanctions. In contrast, Russian sanctions target agricultural production, which is of major concern to American and European politicians.

The remainder of this paper is structured as follows: first we survey the 2014 Russian sanctions episode and describe data that are analyzed to estimate the impact of the sanctions packages. We follow the empirical analysis with robustness checks and brief concluding remarks.

2. Background

The historical and political circumstances surrounding the imposition of sanctions are well summarised in recent literature (e.g. Dreger et al., 2016; Crozet and Hinz, 2016; Moret et al., 2016) and hence the discussion here will be limited to the essential facts that are pertinent to the discussion of economic sanctions. Following the annexation of the Crimean peninsula by the Russian Federation in March of 2014, a coalition of Western countries (EU, USA, Canada and their allies) imposed a series of measures restricting trade with Russia. Initially, these measures targeted specific Russian citizens and entities, but from mid-2014 the restrictions were expanded to curtail trade in military technology and equipment for the oil and natural gas industry.[5] In response to the Western sanctions, in August of 2014, the Russian Federation imposed retaliatory measures restricting imports of foodstuffs from the EU, US, and their allies.[6]

Several features of these sanctioning measures are noteworthy. The first important point to note is that the Western sanctions were imposed at the 8-digit level of the Harmonised System (HS) for classification of goods, whereas Russian counter-sanctions were imposed at the 4-digit level, thereby covering significantly wider product categories. Thus, finding close substitutes for the sanctioned imports is arguably more difficult under the Russian sanctions. Similarly, there is a much smaller potential for re-classification of goods into non-sanctioned categories. This practice has been documented in the context of tax evasion, where products subject to higher import taxes are re-classified as similar, but less taxed products (Fisman and Wei, 2004). In a more extreme version of this scheme, the same shipment is imported under a low-tax classification and exported under a high-tax classification multiple times, each time allowing the fraudulent exporter to reclaim the tax upon export, which in reality has never been incurred (Baloun and Scheinost, 2002).

A related concern to be raised in this context is the limited retroactivity of the EU sanctions. For contracts made prior to the imposition of sanctions, the sanctioned goods may still be exported to Russia, even if sanctions are in place, provided that the exporters obtain permission from a relevant authority in their home country. Analogous provisions exist in the US sanctioning measures.[7] This discretionary element of the Western sanctions has the potential to facilitate exports of sanctioned goods into the Russian Federation, thereby reducing the effectiveness of the Western sanctions. A remarkable example of this limited retroactivity

[4] It is nevertheless encouraging that the parameters obtained here lead to a similar estimate of lost trade as the one obtained by Crozet and Hinz (2016).

[5] For the EU sanctions, see Council Regulation (EU) No 833/2014 (http://data.europa.eu/eli/reg/2014/833/oj). US trade sanctions are imposed by Directive 4 of the Office of Foreign Assets Control under Executive Order 13,662 (https://www.treasury.gov/resource-center/sanctions/Programs/Pages/ukraine.aspx).

[6] The relevant measure is the Decree of the President of the Russian Federation No. 560, English translation is available from http://en.kremlin.ru/events/president/news/46404.

[7] For the provisions granting enforcement discretion in the context of EU sanctions, cf. Council Regulation No 833, sections 2.2, 3.5, and 4.2. For the US analogue, cf. Sectoral Sanctions Identifications List of the Office of Foreign Assets Control.

was the sale of two French Mistral warships to Russia. The delivery of the warships would have been permissible despite sanctions being in place, since the deal was struck in 2010. Due to political considerations, however, the warships were not delivered and the Russian Federation was reimbursed (Tavernise, 2015). To our knowledge, there is no parallel limited retroactivity implemented in the Russian sanctions. In addition, even if some limited retroactivity provisions had been implemented on the part of the Russian Federation, it is doubtful that there would have been many pre-sanctions contracts covered by these hypothetical provisions. Contracts for the supply of foodstuffs are unlikely to be arranged long in advance due to concerns with production uncertainty and food safety (Starbird, 2005). As a result, most of the pre-sanctions contracts would have expired soon after the sanctions were imposed. Therefore, this adds another layer of difficulty for parties wishing to avoid the Russian sanctions, although anecdotal reports have been made of schemes that have managed to sidestep them (e.g. Kiselyova and Popova, 2016).

3. Data

We use data from the *Base pour l'Analyse du Commerce International* (BACI), which is constructed from the Comtrade database maintained by the United Nations Statistics Division (see Gaulier and Zignago, 2010, for a detailed description of these data). This is a panel dataset of bilateral trade flows disaggregated by 6-digit Harmonised System (HS) product categories. For each pair of countries in a given year, typically several trade flows are recorded, reflecting flows of different commodities. These trade flows are recorded both in terms of the value traded (in thousands of US dollars) and as the quantity traded. Since quantities reflect the unit of measurement for each type of good separately (tons, meters, etc.), they are not comparable across different commodities and hence we will use trade values in our models.

The sample to be analyzed in this paper consists of trade flows into the Russian Federation between 1995 and 2017. We further limit the sample to products in the same 4-digit HS category as those that are subject to sanctions. In this way, we will be comparing sanctioned and non-sanctioned products that nevertheless belong to a broadly comparable category, thereby avoiding contamination of results stemming from different dynamics of trade flows of very different product categories (Bena and Jurajda, 2011).

Having a panel dataset of trade inflows into Russia of sanctioned goods and their near-substitutes, we then construct two dummy indicators of the sanctioned status: one for the extraction equipment sanctioned by the US, the EU, and their allies, and a separate dummy for the retaliatory sanctions against foodstuffs imposed by the Russian Federation (see Appendix for further details). These dummies are equal to one if the exporter i is under the sanctioned regime and the good category j is subject to sanctions under that regime. For the main sample, only US and EU member states are classified as exporters subject to the sanction regime, while their allies who also joined the sanctions are excluded from the sample altogether.[8] The reasoning for this is twofold: Firstly, we wish to limit potential problems with sample selection by eliminating countries that joined the sanctions regime on their own accord following the move by the EU and the US. Secondly, these excluded observations contribute relatively little to the trade flows studied. Including these observations predictably leads to a minuscule change in the estimates (see robustness checks below).

Due to the data limitations, some measurement error is present in these dummies for sanctioned goods. Since the BACI data resolve product categories only at the 6-digit HS level while the sanctions are imposed at an 8-digit level, we will be falsely categorizing some products as sanctioned. The two additional digits in the product classification are the finer subdivisions used by the European Union under of the EC Regulation No 2658/87, while the standard classification used by the World Customs Organization only uses 6 digits. For this reason, neither the BACI data, nor their source, UN Comtrade data, record trade flows at the finer, 8-digit level of resolution. At the same time, however, this measurement error affects the interpretation of the results rather subtly. To the extent that there was a decrease in the imports of the sanctioned items without a contemporaneous increase in non-sanctioned imports in the same 6-digit category, this decrease will be visible in the data.

Furthermore, to address the question of measurement error explicitly, we utilize Eurostat data on exports from the EU into Russia. This dataset resolves the trade flows at the 8-digit level. However, it contains many fewer observations than BACI as it covers EU exporters only. Nevertheless, the Eurostat data allow us to calculate what percentage of the European exports at the 6-digit level are in fact sanctioned, and what percentage has been misclassified. Formally, the measurement error in year t is defined as:

$$\mathrm{ME}_t \equiv 100 \times \left(\sum_{i \in \mathbb{E}} \sum_{j \in \mathbb{C}^8} Y_{ijt} \times 1(j \notin S) \right) \Big/ \left(\sum_{i \in \mathbb{E}} \sum_{j \in \mathbb{C}^8} Y_{ijt} \right), \tag{1}$$

where $\mathbb{C}^8$ is the set of commodities disaggregated at the 8-digit classification that are classified as sanctioned at the 6-digit level, $\mathbb{E}$ is the set of exporting countries, $1(j \notin S)$ is the indicator function taking the value of one if j-th good does not belong to the set of sanctioned goods at the 8-digit classification (S) and zero otherwise, and Y_{ijt} are exports originating in country i of commodity j delivered in year t. The numerator is the value of imports that are incorrectly classified at the 6-digit level and the denominator is the total value of imports in year t. In addition, since the Eurostat data include trade flows for 2017 and 2018, we can observe the relevant trade flows over a longer horizon. Table 1 below reports the percentage of misclassified trade flows and shows that the measurement error is, in fact, minor.

[8] We code the following countries as "sanctioning exporters:" Austria, Belgium-Luxembourg, Bulgaria, Croatia, Cyprus, Czech Rep., Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, USA, and UK. Sanctioning exporters that were dropped from the main sample but are included in the sample for robustness checks are: Albania, Australia, Canada, Iceland, Montenegro, Norway, Switzerland, and Ukraine.

Table 1
Percentage of the misclassified trade flows at the 6-digit level. Measurement error (ME) is defined in Eq. (1) as the percentage of the total value of trade flows in the extraction equipment that are falsely classified as sanctioned when using data disaggregated by 6-digit HS codes.

Year	ME	Year	ME
2010	6.78%	2015	1.04%
2011	1.23%	2016	9.50%
2012	1.87%	2017	0.94%
2013	1.67%	2018	0.91%
2014	1.52%	2019	0.67%

Table 2
Differences-in-differences model of trade inflows into Russia (millions USD).

Model:	Western sanctions (Extraction equip.)		Russian sanctions (Foodstuffs)	
	(1)	(2)	(3)	(4)
Sanctions× $1(t = 2014)$	-1.867	—	-3.833**	—
	(1.227)		(1.250)	
Sanctions× $1(t = 2015)$	-3.082*	—	-6.953***	—
	(1.446)		(1.581)	
Sanctions× $1(t = 2016)$	-2.758	—	-6.254**	—
	(1.485)		(1.961)	
Sanctions× $1(t = 2017)$	-0.753	—	-6.469**	—
	(2.136)		(1.990)	
Sanctions × $1(t \geq 2014)$	—	-2.858	—	-3.963***
		(1.581)		(0.847)
Good × Exporter FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	16,384	16,384	21,179	21,179
Clusters	114	114	135	135
Tests for joint significance of Sanctions × year interactions (p-values)				
Test 2011–2013 (H_0^{PT})	0.51		0.6	
Test 2015–2017 (H_0^{S1})	0.28		< 0.01	
Test 2014–2017 (H_0^{S2})	0.18		< 0.01	

Notes: The first panel of Table 2 shows the effect of sanctions in a particular year (i.e., the interaction of sanctions with the year dummies). Columns (1) and (3) refer to models in which we consider different sanction effects in each year, while columns (2) and (4) correspond to the case when we assume the same sanction effect across all years. In all models we control for good and exporter fixed effects, as well as time fixed effects. The bottom panel presents joint tests of significance of the sanctions in the specified periods. While the effect on extraction equipment was negative, it was significant only in 2015 at the 10% significance level; when considered in joint tests it lost its significance. On the other hand, food-products sanctions are shown to be highly significant in all cases, including joint testing. Standard errors clustered at the exporter level are reported in parentheses. Significance codes: * 10%, ** 5%, *** 1%

The overwhelming majority of the exports at the 6-digit level belong to the sanctioned category, while the measurement error accounts for about 2% of the exported value on average. Furthermore, out of the two larger deviations in 2010 and 2016, only one occurs in the sanctioned period, which strongly indicates that the sanctions did not have an impact on the recorded composition of imports. The missing pattern in possible misclassification suggests it is likely random and related to the data collection or reconciliation by the trade agencies. Hence we argue that the Western sanctions can be assessed reliably using the BACI data. For the sake of completeness, we also re-estimate the differences-in-differences models on the Eurostat data as a robustness check and show that the main findings are similar to the case when BACI data are used.

Another limitation of the data is the presence of missing values. While BACI data do contain information about zero trade flows, this information is not available for all country dyads and all years. For our main specifications, we do not replace missing trade flows with zeros, but in robustness checks we show that the addition of zeros makes only a modest difference in the estimated effects.

Furthermore, it is worth noting that trade inflows of extraction equipment vary quite wildly in time. Seasonality in extraction equipment is to be expected as these goods are imported in large one-off deliveries when oil and gas producers expand their capacity (Crozet and Hinz, 2016). The pronounced, but short-lived peaks in Figure 1 indicate the presence of these dynamics. In addition, trade flows are very unequally distributed across different exporters, which further increases their variances.[9]

[9] These variances are provided in summary statistics for individual product categories reported in Tables A.1 and A.2, which are relegated to Appendix A in the interest of space.

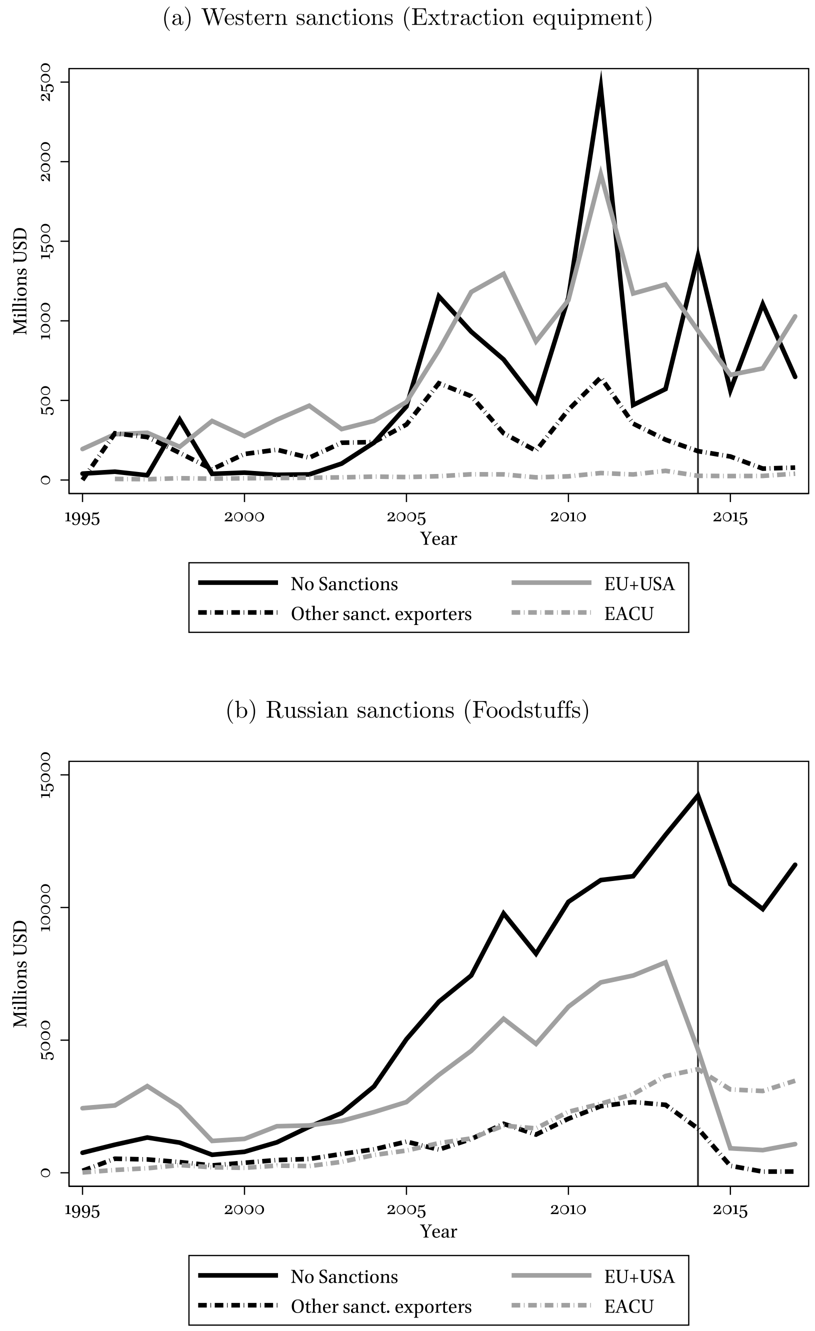

Fig. 1. Timeline of trade inflows of sanctioned goods into Russia. The first year of sanctions (2014) is indicated by a vertical line.

Note that Fig. 1 also shows that the inflows of extraction equipment do not seem to have responded to the imposition of sanctions in 2014 (indicated by the vertical line), while at the same time, imports of foodstuffs have declined sharply. Even though there is a decline of inflows from sanctioning exporters, we also observe a decline in goods (extraction equipment as well as foodstuffs) imported from non-sanctioned exporters. As documented by Dreger et al. (2016) and Crozet and Hinz (2016), a decline in imports is likely a consequence of the weakening of the Russian ruble and falling oil prices and therefore the downward trend in imports cannot be attributed to sanctions alone. Furthermore, the decline in imports from non-sanctioning exporters makes it very difficult to argue that the trade flows were diverted via non-sanctioning countries in order to bypass sanctions. While it is still possible that there were instances when sanctioned goods were indeed imported despite the restrictions (Geller et al., 2014; Kiselyova and Popova, 2016; Yeliseyeu, 2017), the general trend shows a clear decline of imports even from non-sanctioning exporters.

Fig. 1 (b) for Foodstuffs shows a visible increase for non-sanctioned exporters, especially for 2017. Some of the increase of those food exports could be attributed to widening and deepening of the Customs Union of the Eurasian Economic Union (EAEU CU).[10] However, Fig. 1 (b) makes it clear that imports from EAEU countries did not change dramatically following 2014. While we cannot fully exclude the possibility of by-passing of the Russian counter-sanctions, we do not expect strong by-passing effects. First, parties which are members of the EAEU CU must apply uniform customs tariffs and other regulatory measures when trading with third countries. The trade benefits of the large Russian market are undoubtedly too positive for participating countries to jeopardize them by detectable violation, since Russia shows its willingness and ability to apply immediate actions. For example, in 2016 the food embargo was also extended to Ukraine and since August 2015 the Russian presidential decree orders to destroy products that are under the embargo at the border.[11]

4. Empirical specification

In order to evaluate the indications from Fig. 1 rigorously, we analyze the trade data using a differences-in-differences model. The model is specified as a panel regression with fixed effects for each exporter-good pair (at 6-digit resolution) as well as time fixed effects. Our baseline model is specified as:

$$Y_{ijt} = \alpha_{ij} + \alpha_t + \sum_{s=2011}^{2017} \beta_s \text{Sanctions dummy}_{ij} \times 1(t = s) + \varepsilon_{ijt}, \tag{2}$$

where Y_{ijt} are imports originating in country i of commodity j in year t. Analogously, we estimate a simplified specification as:

$$Y_{ijt} = \alpha_{ij}^* + \alpha_t^* + \beta^* \text{Sanctions dummy}_{ij} \times 1(t \geq 2014) + \varepsilon_{ijt}^*. \tag{3}$$

In the main specification (2), time fixed effects are interacted with a Sanctions dummy taking a value of one if a given exporter-good pair is subject to a sanctioning regime and zero otherwise. For this reason, the coefficients β_s in (2) have convenient interpretations as the yearly average differences between the sanctioned and control group. The control group consists of two categories of goods: (i) products in the same 4-digit HS category as the sanctioned ones but not subject to sanctions themselves, and (ii) goods that fall into the sanctioned category by their HS classification, but are outside of the sanctioning regime because they originate from an exporter that is not subject to sanctions. Due to the rich set of fixed effects, however, much of the cross-product heterogeneity is eliminated (roughly 80% of all variation is eliminated by the exporter × good fixed effects) so there is justification to treat these collections of products as approximately homogeneous treatment and control groups.

Models (2) and (3) follow the standard differences-in-differences approach, which assumes that the conditional mean function can be written as a sum of individual-specific fixed effects and a time trend (e.g. Angrist and Krueger, 1999, pp. 1298–1299). In Robustness checks below, we verify that the results do not change much when time trends are allowed to vary across exporters (see models (7) and (8) below). As a check regarding concerns about product heterogeneity, we estimated time-series versions of this model, in which we allow model parameters between product categories (at the 6-digit HS classification). The results are also similar to the baseline model. For additional details, see Appendix B.1.

Therefore, coefficients β_{2014}, β_{2015}, and β_{2017} are interpreted as the deviations from the control group caused by sanctions, while β_{2011}, β_{2012}, and β_{2013} can serve as checks whether there were significant differences in trade flows between the two groups even before the imposition of sanctions. Formally, we will be testing three different hypotheses:

$$H_0^{\mathrm{PT}}: \beta_{2011} = \beta_{2012} = \beta_{2013} = 0, \tag{4}$$

$$H_0^{\mathrm{S1}}: \beta_{2014} = \beta_{2015} = \beta_{2016} = \beta_{2017} = 0, \tag{5}$$

$$H_0^{\mathrm{S2}}: \beta_{2015} = \beta_{2016} = \beta_{2017} = 0. \tag{6}$$

Equation (4) tests the so-called "parallel trend" assumption, which in this case states that prior to intervention, there were no systematic differences between the sanctioned group of imports and the control group consisting of imports outside the sanctioning regime. The validity of the differences-in-differences model rests on the crucial assumption that observations after the imposition of sanctions for exporter-good pairs outside the sanctions regime constitute the appropriate counterfactual for the exporter-good pairs that are subject to sanctions. For example, if the treatment group of imports exhibited pro-cyclical dynamics, while the control group was counter-cyclical, then this model would simply detect this difference in cyclical behaviour rather than the effect of sanctions. The significance of pre-treatment interactions would indicate a rejection of the crucial "parallel trend" assumption. Table 2 in the Results section shows that the parallel trend does indeed seem to hold in this dataset.

Equations (5) and (6) test whether sanctioned trade flows follow the same trend as the non-sanctioned ones in the post-intervention period. Since the sanctions were imposed in mid-2014, we conduct two separate tests: (5) tests all years in which sanctions are in place, while (6) considers only the full years of sanctions. Rejection of (5) and (6) can be interpreted as a detection of the effect

[10] From July 2010 the Customs Union comprised Belarus, Kazakhstan and Russia. Armenia and Kyrgyzstan later joined the newly created EAEU CU - (Armenia in October 2014, effective January 2015, and Kyrgyzstan in May 2015, effective August 2015).

[11] Related decrees and adjustments were retrieved on April 6, 2020 from the following sites: http://kremlin.ru/events/president/news/46404 http://government.ru/docs/14392/ https://ria.ru/20190806/1557173982.html http://publication.pravo.gov.ru/Document/View/0001201906260006?index=1&rangeSize=1

of sanctions. It should be noted that if there was an increase in imports from non-sanctioning exporters in the post 2014 period ceteris paribus then hypotheses (5) and (6) will be rejected as well. In this scenario, Russian importers are “bypassing” the sanctions by accessing exporters that are not subject to the sanctioning regime. However, in this dataset, the “bypassing” interpretation is untenable since imports of sanctioned goods into Russia have declined even from non-sanctioning exporters as documented in Fig. 1.

A simpler model in (3) was estimated which replaces the interactions with time fixed effects by interaction with a dummy indicating the post-sanctions period. In this more parsimonious model, we cannot test hypotheses (4)–(6). The advantage of estimating fewer parameters might be in increased statistical power, in other words, estimation of β^* uses a greater number of observations, which may tighten its confidence interval.

Both models were estimated for 2010–2017, even though data is available from 1995. The reason for exclusion of the older portion of the dataset is a concern for the stability of the data-generating process, which might invalidate the estimation results. For example, the crisis of 2008 would be one potential point that could have altered trade dynamics. Standard errors were clustered by each exporter yielding a sample with 114 clusters for sanctions on the extraction equipment and 135 clusters for food products.

5. Results

Russian imports that are subject to the Western sanctions, as well as imports that are subject to the Russian counter-sanctions, were modeled separately by the main differences-in-differences specification (2) and by its simplified version (3). Results from all four of these models are reported in Table 2. In the interest of space, pre-intervention interactions are omitted and the results of the joint test of their significance are presented instead. Fig. 2 plots all the coefficients, including the pre-treatment periods, for a convenient evaluation of the treatment effect and possible pre-trends. The test of the joint significance of pre-intervention interactions, and also the graphical representation indicate that the data are consistent with the parallel trends assumption. Failure to reject the significance of pre-trend dummies also suggests that either there is no endogenous selection into the sanctioning regime, or if there is such a selection, it is too weak to manifest itself in the data. It is important to emphasize that if there is selection based on the *levels* of trade flows, then it is completely eliminated in the differences-in-differences model, which includes exporter × good fixed effects. Since Russia is an important trade partner to the US and the EU, the volumes of traded goods will likely be correlated with the sanctioning regime. However, all of these differences are absorbed by the fixed effects. Therefore, the only sample selection that is of concern here is selection based on *trends*, which has not been detected by the test of the joint significance of pre-intervention interactions. Because we did not reject the null hypothesis of trends being the same, then the data are consistent with the assumption of no selection on trends. As an additional check, we also estimated a time-series version of our model (see Appendix B.1), which relaxes even the parallel-trend assumption from the differences-in-differences model. Since the time-series and differences-in-differences results are similar, we conclude that from an econometric standpoint, there is either no selection or the selection is too weak to change our results.

Post-intervention interactions show a remarkable pattern: Western sanctions against extraction equipment do not seem to have an effect on trade inflows into the Russian Federation. The interaction coefficients are negative, indicating that trade flows were lower than they would have been in the absence of sanctions, but this difference is statistically insignificant. Only at the 10% level are we able to find a single significant interaction, in 2015, but the significance disappears once the post-sanctions interactions are tested jointly. Since the interaction coefficients represent the mean change in trade flows *per exporter-good dyad*, it is not surprising that they are of somewhat limited magnitude. Scaling them up by the number of exporter-good dyads and summing up all these scaled interaction coefficients across 2014–2017 in Column (1) leads to an estimate of the overall value of the lost trade in extraction equipment, which is about 1.5 billion USD (SE = 0.9). This would mean that for 2014–2017, the lost trade value accounts for about 30% of the total trade inflows of sanctioned goods for the four-year period prior to sanctions (2010–2013). Therefore, the differences-in-differences models indicate that at most, the effect of sanctions is too small to be detected in the data.

On the other hand, the retaliatory sanctions have a very pronounced negative impact on imports, which are overwhelmingly statistically significant. Fig. 2 clearly shows the notable fall in trade inflows of foodstuffs into Russia, compared to a rather modest dip in the imports of extraction equipment. Similarly, the estimate of the overall value of lost trade is 12.6 billion USD (SE = 3.0), which is statistically significant even at the 0.1% level (constituting about a 45% reduction in trade value compared to the period before sanctions). This value may seem somewhat low[12] but it does not reflect the full effect of the trade restrictions. In particular, costs incurred by Western firms relying on Russian markets may be substantial. Nevertheless, our model indicates that the Russian sanctions resulted in about an 8 times greater loss of trade than the Western ones (SE of this ratio is 4.5).

At this point, it is worth emphasizing that the standard errors for sanctions × year interactions are actually *larger* for foodstuffs than for extraction equipment (compare columns (1) and (3) in Table 2). Therefore, the statistical insignificance of Western sanctions is not attributable to insufficient power. Even if standard errors were to be disregarded entirely, the conclusion that the value of lost trade in foodstuffs is roughly 8 times larger than the lost trade in mining equipment is unaffected. Furthermore, in both models (for foodstuffs and extraction equipment), the counterfactual outcome was constructed from goods that are similar to the sanctioned items (falling within the same 4-digit HS classification) and therefore the observed effect can be interpreted as the effect of sanctions alone rather than an artefact arising from different cyclical behaviour of foodstuffs as opposed to mining wares.

Our results are consistent with those of Dreger et al. (2016) who fail to find an effect of the Western sanctions on the exchange

[12] It is quite close to the result obtained from a gravity model estimated by Crozet and Hinz (2016) who find an effect of 10.7 billion USD for the period between 2014 to mid-2015. We found an effect of 10.8 billion USD from 2014 to the end of 2016.

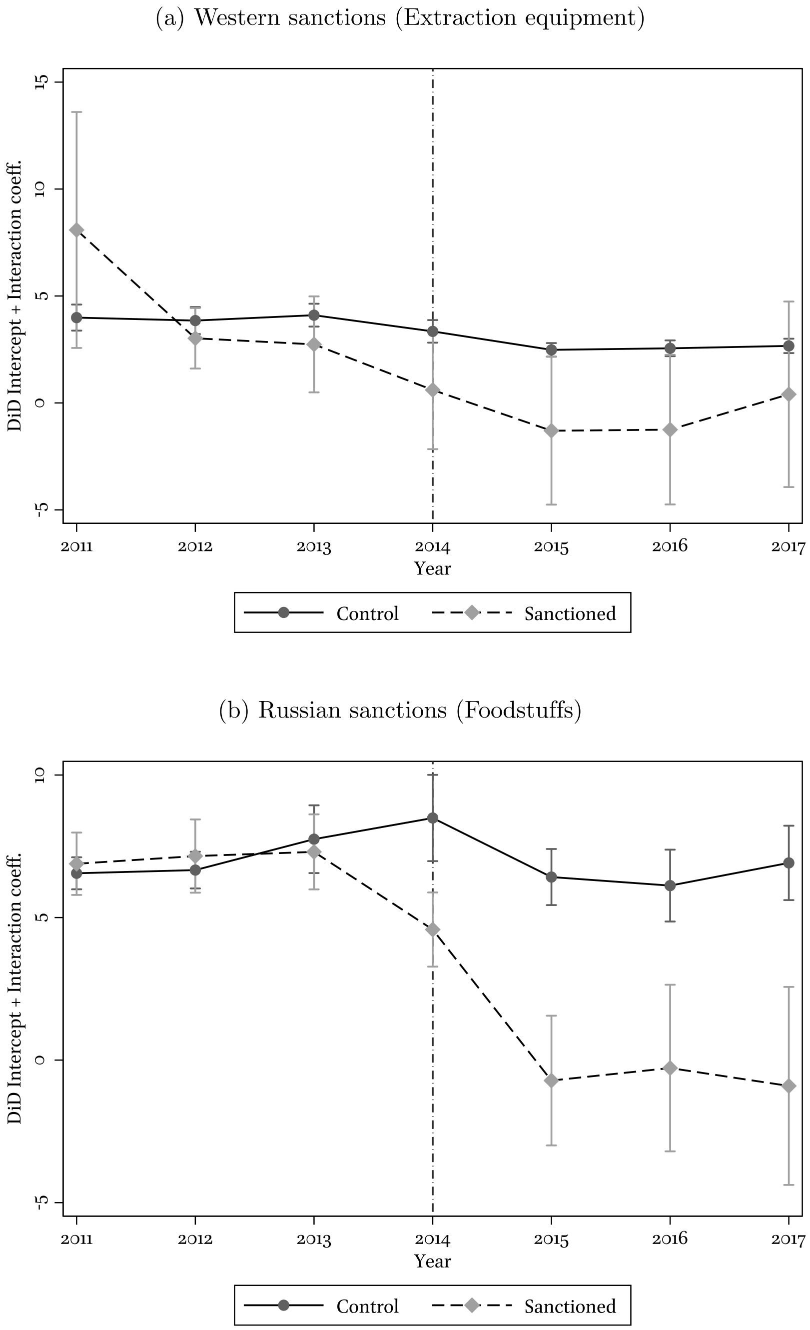

Fig. 2. Differences-in-Differences plots indicating the mean trade inflows into Russia per exporter-good dyad with 95% confidence intervals (in millions USD). The first year of sanctions, 2014, is indicated by a vertical line.

rate of the Russian ruble. A natural interpretation of the small estimated effects of Western sanctions in comparison with the Russian countermeasures is that the Western sanctions are less restrictive than the Russian ones. Indeed, our estimates point straightforwardly towards the hypothesis of grandfathered imports, i.e. the contracts made prior to the imposition of sanctions remain in force, allowing Western exporters to ship sanctioned goods into Russia. Bypassing the sanctions by re-routing the exports through non-sanctioning countries would imply an uptick of imports of sanctioned goods from non-sanctioning exporters, which would lead to significant coefficients in the differences-in-differences model and an uptick in imports of sanctioned goods into Russia from non-sanctioning exporters. Neither of these outcomes is observed (see Table 2 and Fig. 1). Attributing our results to measurement error is untenable due to the relatively small proportions of misclassification in Eurostat data (Table 1). The small degree of measurement error, therefore, undercuts potential explanations relying on Russian importers switching from sanctioned goods to non-sanctioned

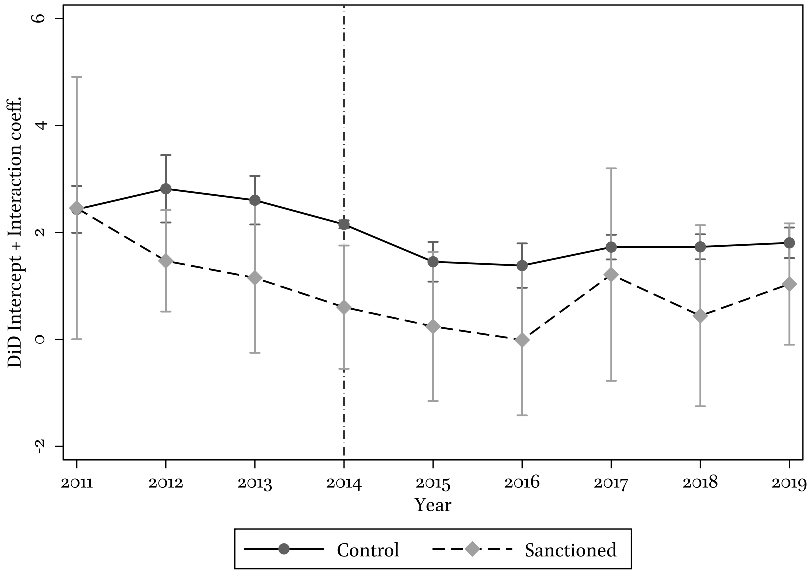

Fig. 3. Differences-in-Differences plots indicating the mean trade inflows of extraction equipment into Russia per exporter-good dyad with corresponding 95% confidence intervals (in millions EUR) using the Eurostat data. First year of sanctions, 2014, is indicated by a vertical line.

substitutes within the same 6-digit code. Analogously, there is very little room for the Western exporters to reclassify their exports in a manner similar to the cases documented in the context of tax avoidance (Fisman and Wei, 2004) and tax fraud (Baloun and Scheinost, 2002; Hignett, 2004).

6. Robustness checks

The differences-in-differences models of the Western sanctions above suffer from measurement error as the list of sanctioned goods under this regime was drawn at the 8-digit level of HS codes and such a granular resolution is not available in the BACI data. Export data from Eurostat, on the other hand, do resolve the trade flows at the 8-digit level. Unfortunately, this dataset is limited to exports originating in the EU and therefore it covers many fewer trade inflows into Russia than the BACI dataset. As shown in Table 1, the contamination by measurement error is minor in European exports, which indicates that our main results should be reliable.

As an additional verification, we re-estimate models (1) and (2) on the Eurostat dataset. The coefficients in the baseline specification are plotted in Fig. 3 and are comparable to the original results (Appendix B.2 provides further details). The Eurostat data, just like the more coarse BACI dataset, indicate that trade in the sanctioned goods followed a very similar trend as the trade in non-sanctioned goods. The value of lost trade between 2014 and 2017 according to model (1) is 1.97 billion EUR (SE = 1.00). The simplified model (2) estimates lost trade for the same period at 0.65 billion EUR (SE = 0.66). Extending the analysis up to 2019, the lost trade is at 2.8 billion EUR (SE = 1.43) in the baseline model and 0.97 billion EUR (SE = 0.97) in the simplified version. These values are very similar to the estimates from the BACI data, but due to the limited sample size are not statistically significant.

Another concern might be that in the differences-in-differences models from the preceding section, all exporting countries have been treated identically. This means that a single average change in trade flows was estimated assuming homogeneity within the whole group of countries imposing the sanctions. This raises the question whether some heterogeneity among the exporters is neglected in our baseline models.[13] In particular, since the Western sanctions afforded a significant degree of discretion to the authorities within each sanctioning country in the enforcement of sanctions (see Section 2), one might expect countries with stronger trading ties to Russia to be more reluctant to adopt strict enforcement policies.

In other words, different countries may differ in the strictness of enforcing sanctions. Therefore, to show that our baseline results are not masking parameter heterogeneity, we have partitioned the treated group of exporters into five subsamples with different characteristics: the first subsample consists of the top 5 exporters, which accounted for nearly 77% of the value of imports of sanctioned goods into Russia between 2010 and 2013 (i.e. Germany, USA, Italy, France, and Sweden). The second subsample consists of the USA plus the 3 largest European countries in terms of GDP (Germany, United Kingdom, France). These three large European exporters constitute the treatment group in the third subsample, while all the remaining European exporters are coded as the treated group in the fourth sample. The fifth sample consists of eastern European countries and the Baltic nations. In each subsample, we have kept only observations belonging to the non-sanctioning exporters (control group) and the relevant subroup of sanctioning exporters. This was done to prevent misclassification of sanctioning exporters as "controls."

Furthermore, since the statistical power of the baseline models is affected by the relatively large clusters used for computing the

[13] We thank an anonymous referee for alerting us to this concern.

Table 3
Parameter heterogeneity (BACI data).

Model			95% Confidence Interval			
Baseline (2)	Sample	ΔTrade	Classical		Clustered	
	All	-1.52	-2.95	-0.08	-3.23	0.2
	Top 5	-1.28	-2.24	-0.32	-2.84	0.28
	Large	-1.36	-2.25	-0.47	-2.92	0.2
	Large EU	-1.12	-1.89	-0.35	-2.62	0.38
	Small EU	-0.19	-1.29	0.9	-0.84	0.46
	Eastern EU	-0.25	-1.08	0.57	-0.76	0.25
Simplified (3)						
	All	-2.04	-2.94	-1.13	-4.03	-0.04
	Top 5	-1.94	-2.55	-1.34	-3.84	-0.05
	Large	-1.85	-2.41	-1.3	-3.71	0.01
	Large EU	-1.5	-1.98	-1.03	-3.22	0.21
	Small EU	-0.3	-0.99	0.4	-0.69	0.1
	Eastern EU	-0.18	-0.7	0.34	-0.48	0.12

Notes: Differences-in-differences models specified by Eqs. (2) and (3) estimated on different subsamples of the sanctioning exporters. These samples are: All = replication of results in Table 2; Top 5 = 5 countries that accounted for the greatest proportion of imports of sanctioned goods into Russia in 2010–2013 (i.e. Germany, USA, Italy, France, and Sweden); Large = largest exporters in terms of GDP (USA, Germany, United Kingdom, France); Large EU = largest European exporters in terms of GDP (Germany, United Kingdom, France); Small = smaller exporters (Austria, Belgium, Bulgaria, Cyprus, Czech Rep. Denmark, Estonia, Finland, Greece, Croatia, Hungary, Ireland, Italy, Lithuania, Latvia, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Sweden); Eastern EU = Eastern European and Baltic countries (Bulgaria, Czech Rep., Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia). Column ΔTrade reports the total change in trade in 2014–2017 in billions USD. Classical confidence interval (CI) assumes IID disturbances, while clustered CI assumes clustering by the exporting country. All specifications include time fixed effects as well as fixed effects for the exporter-good dyads.

variance-covariance matrix, we artificially increase the power here by using classical standard errors assuming independent and identically distributed (IID) error terms. The purpose of this exercise is to check to what extent statistical inference changes when a less conservative approach is taken. Non-clustering is likely to produce overly narrow confidence intervals, since it ignores correlations induced by the need to transport the goods by similar routes and making other logistical arrangements that depend on the country of origin.

As Table 3 shows, these additional results not change the conclusions from the baseline models in a substantial manner. Re-estimating equations (2) and (3) using inappropriately small standard errors leads to a modest narrowing of the confidence intervals. While some parameter heterogeneity can be inferred from Table 3, it is difficult to draw any firm conclusions. If anything, it appears that the top 5 exporters accounted for a slightly larger fraction of the lost trade than the smaller exporters. Taking the baseline specification in Table 3, the top 5 exporters lost 1.52 billion USD in trade, which is about 84% of the total trade lost among all sanctioning exporters. In contrast, these 5 exporters accounted for 77% of exports in the three years prior to the imposition of sanctions. It would therefore appear that the countries with stronger trade ties bore *higher* costs of sanctions, in comparison with the other sanctioning countries.

A related concern might be the pooling of two sources of variation in models (2) and (3). Since the control group in these models consists of goods originating in both sanctioning and non-sanctioning exporters, it may be argued that comparing the sanctioned trade flows to this heterogeneous control group can yield misleading results. While the main specifications (2) and (3) partly addressed this concern by including Exporter × Good fixed effects, which eliminate the unobserved differences between goods of the same type originating in different countries, we nevertheless include a modified version of the differences-in-differences model, which uses the variation within exporters only:

$$Y_{ijt} = \alpha_{it} + \gamma \text{Sanctions dummy}_{ij} + \sum_{s=2011}^{2017} \beta_s \text{Sanctions dummy}_{ij} \times 1(t = s) + \varepsilon_{ijt}, \tag{7}$$

where Y_{ijt} are imports originating in country i of commodity j in year t. As before, we also estimate a simplified specification as:

$$Y_{ijt} = \alpha^*_{it} + \gamma^* \text{Sanctions dummy}_{ij} + \beta^* \text{Sanctions dummy}_{ij} \times 1(t \geq 2014) + \varepsilon^*_{ijt}. \tag{8}$$

Models (7) and (8) only compare sanctioned goods to similar items (within the same 4-digit HS classification), but eliminate cross-country comparisons by including exporter-specific time trend captured by the α_{it} fixed effects. Table 4 reports the results from these modified regressions.

As may be observed in Table 4, these modified regressions lead to very similar results as our baseline models (cf. Table 2). The Western sanctions yield insignificant effects on the post-sanctions trade flows, while Russian counter-sanctions produce negative and significant effects. Due to the loss of cross-country variation, the results are somewhat less precise but the conclusions remain unchanged.

As another robustness check, we run a differences-in-differences model without the exclusion of countries that joined the sanctions regime other than the US and the EU. In addition, missing values in the dataset have been replaced by zeros. A comparison of

Table 4
Differences-in-differences model of trade inflows into Russia (millions of USD) with exporter × time fixed effects.

Model:	Western sanctions		Russian sanctions	
	(Extraction equip.)		(Foodstuffs)	
	(1)	(2)	(3)	(4)
Sanctions× 1(t = 2014)	-1.776	—	-2.831***	—
	(1.268)		(0.732)	
Sanctions× 1(t = 2015)	-1.900	—	-2.746**	—
	(1.223)		(0.904)	
Sanctions× 1(t = 2016)	-1.643	—	-2.301*	—
	(1.178)		(1.097)	
Sanctions× 1(t = 2017)	-0.181	—	-1.880	—
	(2.418)		(1.205)	
Sanctions × 1(t ≥ 2014)	—	-1.729	—	-2.490***
		(1.364)		(0.697)
Exporter × Time FE	Yes	Yes	Yes	Yes
Observations	14,927	14,927	22,636	22,636
Clusters	106	106	135	135
Tests for joint significance of Sanctions × year interactions (p-values)				
Test 2011–2013 (H_0^{PT})	0.48		0.63	
Test 2015–2017 (H_0^{S1})	0.5		< 0.01	
Test 2014–2017 (H_0^{S2})	0.36		< 0.01	

Notes: These models utilise variation between sanctioned and control exports within exporters. The results lead to the same conclusions as the estimates from the baseline models (cf. Table 2). Standard errors clustered at the exporter level are reported in parentheses. Significance codes: * 10%, ** 5%, *** 1%

results in Table 5 with the main specification reported in Table 2 shows that the differences in estimates are almost negligible. Even though the interaction coefficients in the extraction equipment model do become significant in this expanded dataset, their significance is marginal. Therefore, even though the models are consistent with a decline in the imports of extraction equipment, the decline is modest in comparison with the other fluctuations in the data.

In the expanded dataset, the coefficients are closer to zero than in the baseline models in Table 2 because here we are including many exporter-good dyads with zero trade inflows into Russia for the entire period 2010 – 2017. Hence the effect of sanctions appears "diluted" but the conclusions remain largely unchanged: just as before, the effect of Western sanctions led to a loss in traded value of

Table 5
Differences-in-differences model of trade inflows into Russia (millions of USD) with added zeros and full sample of exporters using BACI data.

Model:	Western sanctions		Russian sanctions	
	(Extraction equip.)		(Foodstuffs)	
	(1)	(2)	(3)	(4)
Sanctions× 1(t = 2014)	-0.967	—	-0.390**	—
	(0.621)		(0.119)	
Sanctions× 1(t = 2015)	-1.284	—	-0.891***	—
	(0.739)		(0.192)	
Sanctions× 1(t = 2016)	-1.395	—	-0.892***	—
	(0.726)		(0.199)	
Sanctions× 1(t = 2017)	-0.783	—	-0.924***	—
	(1.003)		(0.200)	
Sanctions × 1(t ≥ 2014)	—	-1.381	—	-0.899***
		(0.773)		(0.189)
Good × Exporter FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	96,096	96,096	284,544	284,544
Clusters	156	156	156	156
Tests for joint significance of Sanctions × year interactions (p-values)				
Test 2011–2013 (H_0^{PT})	0.4		0.08	
Test 2015–2017 (H_0^{S1})	0.27		< 0.01	
Test 2014–2017 (H_0^{S2})	0.21		< 0.01	

Notes: The results presented here test the robustness to the replacement of missing values by zero and expanding the sample to include sanctioning exporters beyond the EU and the US. These results lead largely to the same conclusions as the estimates from the baseline models (cf. Table 2). Standard errors clustered at the exporter level are reported in parentheses. Significance codes: * 10%, ** 5%, *** 1%

about 2.4 billion USD (SE = 1.6) while Russian sanctions cost about 23 billion USD (SE = 4.9). One slight change compared to the baseline results is that the test for pre-trend in Column (3) narrowly rejects parallel trends, albeit only at the 10% level. Given the fact that the significance is marginal and that our main results survive even if the parallel trend assumption is dropped altogether (see results in Table), the significance of pre-intervention terms seems to be of little consequence. As the main results presented in Section 5 survive even under a very different modelling methodology and using a very different sample, there is a solid basis for claiming their robustness.

Finally, we note that even though the data in this case do not reject the common trend assumption, which is crucial for the validity of the differences-in-differences methodology (see test of pre-trend in Table 2), we nevertheless conduct a robustness check that relaxes this assumption. This is achieved by estimating a time-series model for each exporter-good dyad in the period before sanctions and using the predictions from this model as counterfactuals in the period after sanctions. Encouragingly, even this modelling methodology agrees with the main results above: in this model, trade lost due to the Western sanctions amounts to about 0.8 billion USD, while the Russian counter-sanctions resulted in a loss of 5.1 billion USD (details are available in Appendix B.1).

7. Conclusion

This paper contributes to the long stream of economic literature on sanctions by providing empirical support for long-standing theoretical prediction that public choice considerations are central to the provess of crafting economic sanctions. Specifically, we examine the Russian sanctions imposed on European and American food imports and the impact of EU and US sanctions on exports of extraction equipment to Russia.

Using a differences-in-differences approach on data covering the imports of sanctioned goods into Russia, our results indicate that the Russian sanctions decreased imports by about 12.6 billion USD, while the EU and US sanctions led to about 1.5 billion USD of lost imports of the sanctioned goods. We find no evidence that this result is driven by substitution between different trade channels. Under this explanation, we would have found statistically significant effects in the differences-in-differences models of the Western sanctions. Furthermore, in the aggregate terms, non-sanctioning exporters would have had to supply more of the sanctioned goods after 2014. Instead, the opposite is the case: exports of the sanctioned goods into Russia declined even from non-sanctioning countries, which can be seen in Fig. 1 (a). While it is tempting to conclude that the discrepancy between the trade losses is due to the difference in the pre-sanctions trade volumes (food imports accounted for about ten times the trade value of extraction equipment), this conclusion ignores the evidence from the control group countries. Although trade flows of extraction equipment were very similar between sanctioning and non-sanctioning exporters, there was a stark difference in import dynamics between food imports from sanctioning as opposed to control exporters. We therefore conclude that the extraction equipment effectively follows the “counterfactual trajectory,” since our differences-in-differences model failed to find a statistically significant wedge between the treated and control groups. On the other hand, we find a pronounced difference between the treated and control groups in the foodstuffs sanctions.

The explanation for the differential impact of the sanctioning measures that best fits the available data is the one that has been put forward by political economy literature for more than half a century (Galtung, 1959; Lindsay, 1986; Kaempfer and Lowenberg, 1988); namely, sanctions create costs on sender countries, which will dampen the appetite of the sender countries to impose trade restrictions. The dampening effect is exacerbated in proportion to the strength of the opposition that will be triggered among the domestic population. Due to the EU’s reliance on energy imports from the Russian Federation, EU’s reluctance to restrict exports of oil and gas extraction equipment is fits squarely with this line of argumentation. By comparison, the costs incurred by the Russian Federation blocking European food imports are likely to be substantially smaller, especially due to Russia’s potential substitution to domestic producers (Deppermann et al., 2018).

The behaviour of the Western coalition closely matches the theoretical predictions of Kaempfer and Lowenberg (1988), who show that governments in the sender countries would design sanctioning policies in such a way as to minimize costs to domestic interest groups. Indeed, the EU and the US allowed exporters to honour the contracts made prior to the imposition of export restrictions, and, as a result, sanctioned goods continued to be imported into the Russian Federation. These results are also supported by contract theory. Levin (2003), using a general agency model, shows that trade relationships will be more flexible in changing contract conditions when trading is more frequent or when parties rely more on each other. This phenomenon is also supported by the classical results of Williamson (1985) and Joskow (1987), who show that in vertical supply contracting, this adaptability and flexibility is a key feature of successful longterm relationships. Similar arguments could be found in business and managerial literature showing that previous cooperation strengthens contractual adaptability, which in turn fosters current cooperation between the same partners (see, e.g. Luo, 2002). Similarly, Miromanova (2019), using a theoretical model and microdata from Russia, concludes that the trade decline after the sanctions were imposed is caused primarily by small importers being pushed out of the market, while the number of large firms experiences a smaller decline.

Hence, if the Russian importers have made sufficiently long-term contracts for the supply of extraction equipment and the Western exporters obtained permissions form the relevant authorities in their home countries, or both parties flexibly adjust the contracts to be a long-term, the Western sanctions have very limited effect. To our knowledge, no similar concessions have been made by the Russian government. Even if the Western food exporters were able to claim examptions for pre-existing contracts, it is improbable that many contracts for the food imports last more than a year. Therefore, the data on Russian sanctions are unlikely to contain much of these “grandfathered” imports.

Additional work at the firm-level might reveal interesting patterns. Even though Western exporters of mining equipment might be able to avoid sanctions due to pre-existing contracts, they are likely to incur additional costs of obtaining exemptions from the authorities. The extent of these costs (and the costs of Russian importers) would provide a more nuanced account of the impact of the sanctions.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.jce.2020.07.001

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