Worth the pain? Firms' exporting behaviour to countries under sanctions*

Matthieu Crozet,† Julian Hinz,‡ Amrei Stammann,§ and Joschka Wanner¶

January 2021

Abstract

How do exporting firms react to sanctions? Specifically, which firms are willing — or capable — to serve the market of a sanctioned country? We investigate this question for four sanctions episodes using monthly data on the universe of French exporting firms. We draw on recent econometric advances in the estimation of dynamic fixed effects binary choice models. We find that the introduction of new sanctions in Iran and Russia significantly lowered firm-level probabilities of serving these sanctioned markets, while the (temporary) lifting of the U.S. sanctions on Cuba and the removal of sanctions against Myanmar had no or only small trade-inducing effects, respectively. Additionally, the impact of sanctions is very heterogeneous along firm dimensions and by case particularities. Firms that depend more on trade finance instruments are more strongly affected, while prior experience in the sanctioned country considerably softens the blow of sanctions, and firms can be partly immune to the sanctions effect if they are specialized in serving "crisis countries". Finally, we find suggestive evidence for sanctions avoidance by exporting indirectly via neighboring countries.

Keywords: Sanctions, trade, foreign policy, extensive margin, firm behaviour
JEL Classification: F51, F14, F13, F52

*The project leading to this paper has received funding from the European Union's Horizon 2020 research and innovation programme under grant agreement No 770680.

†Université Paris-Saclay. E-mail: matthieu.crozet@universite-paris-saclay.fr.

‡Bielefeld University, Kiel Institute for the World Economy and Kiel Centre for Globalization. E-mail: mail@julianhinz.com.

§Ruhr-Universität Bochum. E-mail: Amrei.Stammann@rub.de.

¶University of Potsdam, Kiel Institute for the World Economy. E-mail: joschka.wanner@uni-potsdam.de.

1 Introduction

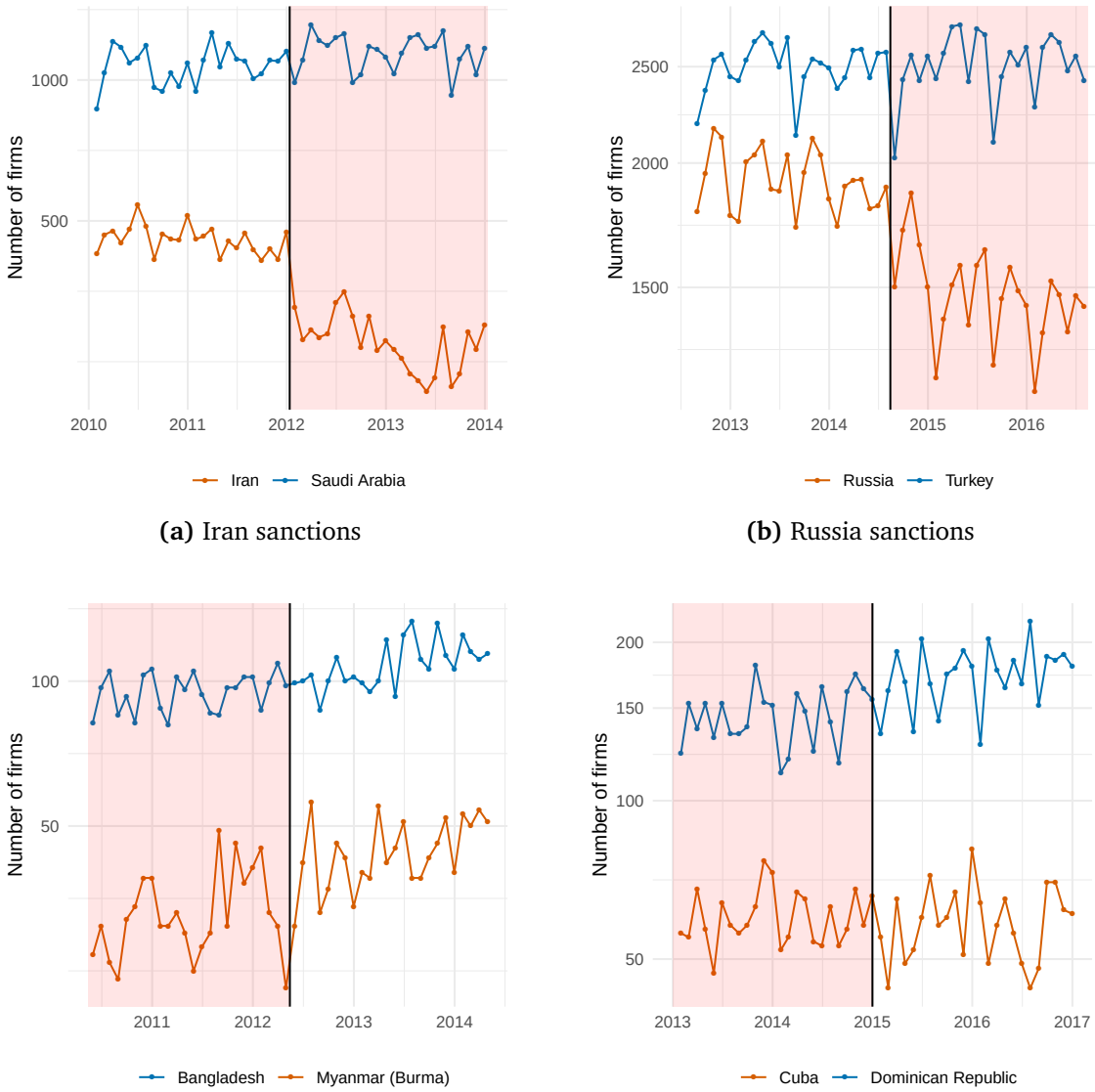
Sanctions restrict access to a market. Restrictions can be explicit — an arms embargo or the expulsion from the SWIFT system — or rather implicit — such as legal uncertainty or political instability. Yet many measures may ask companies to question their business dealings in the sanctioned country as a whole.

In this paper, we explore the impact of sanctions on this so-called *extensive* margin of trade, i.e. the firms' decision to either start or continue to serve a sanctioned market, or not to enter or withdraw. Figure 1 visually tells the story of firms under sanctions. Each panel depicts the simple comparison of the evolution of the number of French firms exporting to an affected country (red line) to a country that is comparable in geographic and economic terms (blue line). The y-axis denotes the raw number of active firms in a given month. The vertical black line signals the time of imposition or abolition of the respective sanctions regime, while the red shaded area corresponds to the sanctions period.

Figure 1a shows the case of Iran, where the drop in the number of active firms is particularly dramatic: Whereas in the two years prior to the sanctions imposed by the EU and other countries the number of firms in any given month hovers around an average of 457, afterwards this number decreases to 276, a minus of 40 percent. In the case of the Russia sanctions in figure 1b, the drop is a little less dramatic, but still severe: Before the sanctions a monthly average of 1922 firms were present on the Russian market, afterwards this number falls to 1480, a decrease by 23 percent.

Figure 1c shows the inverse effect for the case of Myanmar: Lifting sanctions after a prolonged period of being largely off-limits for French firms, their average monthly number increases somewhat gradually from 33 to 44 in the two years afterwards, an increase of 33 percent. Figure 1d shows the case of Cuba: While there are no sanctions by France or the EU against Cuba, there is an embargo by the United States in place that could indirectly influence French-Cuban business relations. In early 2015, under the Obama administration, political relations between the U.S. and Cuba thawed and the former lifted a number of restrictions — until the Trump administration reversed course immediately in early 2017. French firms appear to have been largely unfazed by these political developments, with on average 59 firms present on the Cuban market in a given month before 2015, and around 60 afterwards.

In our empirical analysis using French customs data, we investigate the firm-level trade response more thoroughly and estimate how much less likely firms are to export to a market if the destination country is sanctioned. While the specific reasons for firms' individual decisions may be manifold and the sanctions episodes differ in specific policies applied, we can draw on firm characteristics to describe patterns of determinants. Are firms that have experience in the sanctioned country more likely to stay? Is the extent of a firm's trade finance dependence indicative of further activity under sanctions? Are large firms differently affected? Does a focus on exporting consumer or intermediate goods make a difference? Are certain firms more robust or even specialized in serving "crisis countries"? And could firms try to circumvent sanctions by sidestepping to neighboring countries? We explore these questions by relying on highly-detailed



(a) Iran sanctions

(b) Russia sanctions

(c) Myanmar sanctions

(d) Cuba embargo

Figure 1: Evolution of number of firms in face of sanctions

monthly customs data on the universe of French firms and making use of recent advances in estimation techniques for fixed effects binary choice models.

The paper thus primarily contributes to the recently flourishing literature on sanctions, and as such is also related to the broader literature on firm behaviour and trade policy. There is now a vast corpus of studies assessing the consequences of international economic sanctions on international trade. Bergeijk (2009) and Hufbauer, Schott, and Elliott (2009) provide a useful overview of earlier work in this field. The case of the 1807 U.S. embargo has been particularly well studied because it is one of the most enlightening cases of trade sanctions in the modern era and is exemplary in its simplicity and scope. Frankel (1982), Irwin (2005) and O'Rourke (2007) find that the self-inflicted blockade led to a severe reduction in international trade and a drop in U.S. GDP of about 4 to 8%. Caruso (2003) provides an estimate of the effects of a large number of sanctions from the second half of the 20th century on global trade flows, based on a simple gravity analysis. More recently, Hinz and Monastyrenko (2019) and Cheptea and Gaigné

(2019) assess the consequence of the embargo on agricultural products imposed by Russia in 2014. Hinz (2019) uses a structural gravity model to estimate international trade losses related to sanctions against Iran, Russia, and Myanmar. He estimates that these losses amounted to US$ 50 billion in 2014, or about 0.4% of world trade.

Several studies have also examined periods of diplomatic tension which, although not leading to official sanctions, have affected trade relations. For example, Fuchs and Klann (2013) show that meetings between the Dalai Lama and Western political leaders are followed by significant decreases of bilateral trade between China and the host countries. Michaels and Zhi (2010) and Pandya and Venkatesan (2016) show that the diplomatic confrontation between France and the United States over the 2003 Iraq war significantly reduced trade between the two countries in a short period of time. In a similar vein, Heilmann (2016) studies the impact of various boycott campaigns, including the boycott of Danish products in some Muslim-majority countries in 2006. However, few studies have examined the impact of sanctions at the company level. Going down to the microeconomic level is necessary to accurately estimate, as Ahn and Ludema (2020) do, the impact of smart sanctions that explicitly target a small number of firms. It is also useful to better understand why sanctions reduce trade when this is not always the goal. It is for this purpose that Crozet and Hinz (2020) and Gullstrand (2020) use firm-level data (French and Swedish respectively) to study the case of the Russia sanctions. More generally, microeconomic trade data allow to study firms' behaviour when facing constraints resulting from diplomatic conflict. Haidar (2017), for example, shows how (some) Iranian exporters have been able to circumvent Western sanctions by diverting a part of their trade flows to non-sanctioning countries.

In this paper, we exploit French firm-level data to focus exclusively on the consequences of sanctions on the extensive margin of trade, i.e. on the propensity of firms to export at a given time to a given market. In doing so, our paper is linked with the literature studying the dynamics of firms' export decisions. A first strand of this literature focuses on the importance of sunk export costs and, as a result, on the importance of prior exporting experience on the decisions to enter foreign markets. In their seminal analysis of Colombian exporters, Roberts and Tybout (1997) find evidence of high sunk costs since the prior experience is shown to increase greatly the probability of exporting (see also Bernard and Jensen, 2004).

More recent work has examined the dynamics of new exporters. Indeed, a detailed analysis of the data suggests that companies seeking to enter a foreign market often go through an exploratory period of trial and error.[1] This literature also highlights the fact that the response of firms to shocks is likely to be heterogeneous. It depends on the nature of the commercial contracts, and therefore on the characteristics of the goods traded (Timoshenko, 2015; Mejean, Martin, and Parenti, 2019). It also depends on the characteristics of the firms themselves, such as firms' size and experience on foreign markets (Dickstein and Morales, 2018; Berman, Rebeyrol, and Vicard, 2019). Finally, our study is also linked to Békés, Fontagné, Muraközy, and Vicard (2017) who use monthly customs data — as we do — to study the frequency of international shipments.

[1] See e.g. Iacovone and Javorcik (2010), Albornoz, Calvo Pardo, Corcos, and Ornelas (2012), Cadot, Iacovone, Pierola, and Rauch (2013), Blum, Claro, and Horstmann (2013), Eaton, Eslava, Jinkins, Krizan, and Tybout (2015), Timoshenko (2015), Ruhl and Willis (2017), Dickstein and Morales (2018), Sager and Timoshenko (2019), or Piveteau (2019).

They show that firms send less frequent, larger shipments to more uncertain markets.

In our analysis, we find that French firms are significantly less likely to export to Iran and Russia after the introduction of sanctions on these countries. Additionally, we find that the easing of U.S. sanctions against Cuba (and the corresponding extraterritorial provisions) has had no significant impact on French exports to that country, and that the end of sanctions against Myanmar has had a positive but relatively small impact on the likelihood of exporting. Specifically, we estimate the average probability of firms that export to these markets at *any* point in our sample to serve the Iranian, Russian, or Burmese in a given month to be reduced by 39%, 23%, and 11%, respectively while sanctions are in place.

Our findings suggest that sanctions partly act via increased market entry costs, implying dynamic phase-in effects, and that the specific sanctions design drives which firms are more or less affected. This is illustrated, for example, by the much stronger sanctions effect for exporters relying on trade finance instruments extensively, due to the measures imposed on the financial sector in both Iran and Russia. Firms' presence in other countries — coincidental or strategic — may allow them to better cope with sanctions when these countries are neighboring the sanctioned one or are facing a high level of political instability.

The remainder of this paper is as follows. In section 2 we provide a brief description of the four sanctions episodes we study quantitatively. In section 3 we sketch a model of firms' decision to enter/stay in/exit a given market subject to sanctions. The model produces an equation that characterizes this decision, which can be estimated with a *dynamic* probit estimator and appropriate fixed effects. We briefly sketch a suitable estimation procedure in section 4 and discuss the data set we use in our estimations. In sections 5 and 6 we then estimate the model and test several channels that may influence the firms' export decision. Section 7 concludes.

2 Background

Sanctions are a broad basket of different types of policies, ranging from travel bans, asset freezes to all-out embargoes. In this paper, we mainly focus on the sanctions applied against Iran and Russia. In section 6 we also investigate the cases of Myanmar and Cuba, where long-standing sanctions were partially lifted during our period of observation. This allows us to observe the impact of the lifting of sanctions and to try to see to what extent trade can return to normal after a prolonged sanctions period. All four cases are instructive in their own ways, and vary in the policies applied (or lifted). We hence first review similarities and differences of the four cases to inform the empirical analyses in sections 5 and 6.

Sanctions against Iran

Of the four cases, the measures taken against Iran were the most severe.[2] Sanctions against Iran were first imposed by the United States in response to the hostage crisis after the Iranian revolution in 1979 with Executive Order 12170, which froze Iranian assets in the United States

[2]At the time of writing, the U.S. has reimposed most of the sanctions detailed below, whereas multilateral and EU sanctions have been largely lifted as of January 2016.

and imposed a trade embargo.[3] These measures were lifted in 1981 as part of the negotiations to secure the hostages' release. In 1984 the U.S. then imposed new sanctions during the height of the Iran-Iraq war, prohibiting the sale of weapons and ending U.S. assistance in Iran. In 1995 the U.S. then moved to essentially ban all trade with and investment in Iran, in response to its support for terrorist organizations and its nuclear program.[4] Multilateral sanctions by the United Nations were introduced in 2006, after Iran had failed to comply with UN Security Council Resolution 1696, which demanded Iran to halt its uranium enrichment program.[5] Through UN Security Council Resolution 1737 trade with potentially nuclear-related technology and material to Iran was prohibited, and certain individuals' and companies' assets abroad frozen. These measures were subsequently expanded.[6]

Starting in 2010 the European Union began to impose own sanctions against Iran, successively banning Iranian airlines, air cargo and shipping companies from operating in the European Union. New measures also put restrictions on Iran's financial services and energy sectors, banning insurance and reinsurance by EU insurers of Iranian entities.[7] In January 2012, after raising concerns regarding the Iranian nuclear program, the European Union introduced, among other measures, an import embargo on Iranian oil, other petrochemicals and precious metals. Existing oil contracts were allowed to be honored until July 2012.[8] The next and hitherto ultimate escalation of the sanctions measures against Iran was put forward in March 2012, when banks in violation of EU sanctions were disconnected from SWIFT, thereby effectively cutting of Iran from the global financial system.[9] These drastic measures appear to have brought Iran to the negotiating table, resulting in a rapprochement between Iran and Western countries. On 16 January 2016 sanctions were lifted as part of the "Joint Comprehensive Plan of Action", which had been agreed upon in July 2015.[10] In the empirical analysis we study the behaviour of firms after the imposition of the severe sanctions measures introduced in January 2012.

Sanctions against Russia

In late 2013, Ukraine was rocked by a massive protest movement known as the "Euromaidan". The protests were a new episode in the deep polarization of the country, which has long been torn between the hope of a closer relationship with Western Europe and the desire to forge closer ties with Russia. This division is both geographical and cultural. A majority in the Western regions and in Kiev expresses a relative mistrust of Russia, in contrast to the Russian-speaking regions

[3] U.S. Executive Order 12170 - Blocking Iranian Government property, November 14, 1979.

[4] U.S. Executive Order 12959 - Prohibiting Certain Transactions With Respect to Iran, May 6, 1995.

[5] See e.g. `http://news.bbc.co.uk/2/hi/middle_east/6205295.stm` (accessed on June 29th, 2020).

[6] Resolutions 1747 imposed an arms embargo and expanded the freeze on Iranian assets; Resolution 1803 mandated UN member states to monitor activities of Iranian financial institutions, inspect Iranian aircraft and maritime vessels, and monitor the movement of individuals in their jurisdiction; Resolution 1929 banned trade in dual-use and other military goods, introduced further travel bans and called on states to actively inspect Iranian vessels and aircraft. See `https://www.consilium.europa.eu/en/policies/sanctions/iran/` (accessed on June 29th, 2020) for reference.

[7] European Council Decision 2007/140/CFSP, July 26, 2010.

[8] Other measures further included travel bans and asset freezes. See Council conclusions on Iran 3142th, FOREIGN AFFAIRS Council meeting Brussels, 23 January 2012.

[9] See `https://www.swift.com/insights/press-releases/swift-instructed-to-disconnect-sanctioned-iranian-banks-following-eu-council-decision` (accessed on June 29th, 2020).

[10] See e.g. `https://www.swift.com/insights/press-releases/update_iran-sanctions-agreement` (accessed on June 29th, 2020).

in the East. On February 2014, the Euromaidan movement ousted the Ukrainian government, headed by the pro-Russian President Yanukovic.

In reaction, the situation deteriorated rapidly in parts of south-eastern Ukraine and Crimea, where pro-Russian uprisings degenerated into separatist movements and armed conflict with Russian involvement. On February 27, 2014, armed men seized key public infrastructures of the peninsula of Crimea. On March 16, 2014, a much-criticized referendum paved the way for the absorption of the Crimea by the Russian Federation (Dreyer, Luengo-Cabrera, Bazoobandi, Biersteker, Connolly, Giumelli, Portela, Secrieru, Seeberg, and van Bergeijk, 2015).

To protest against Russia's involvement in this breach of Ukraine's territorial integrity, European and allied Western countries issued a first series of sanctions against the Russian Federation in mid-March 2014. This first wave of sanctions from Western countries targeted senior political and military personnel. These sanctions consisted mainly of travel bans, asset freezes, and the prohibition of financial transactions involving a range of political and military personnel and financial institutions in Russia and Ukraine.[11] The list of targeted individuals and entities was amended several times until the end of 2015.[12]

The situation further deteriorated after July 17, 2014, when a civilian aircraft (Malaysian Airlines flight MH17) was shot down over the separatist region of Donbass, with the probable involvement of pro-Russian insurgents. The EU, like most Western allies, then broadened sanctions by imposing trade restrictions and severely tightening financial restrictions.[13]

European entities were restricted from exporting certain goods to the Russian Federation and buying certain Russian financial assets. The trade restrictions were primarily directed at military use and dual-use products and technology.[14] They also covered capital goods specific to the oil and mining industry. Perhaps more importantly, the financial sanctions have targeted major Russian financial institutions, as well as a number of defense and energy companies (including Gazprom, for instance). The sanctions have essentially cut off the access to financing on European markets for these institutions. The United States, but also Japan and other European and Oceanian countries have taken similar measures. Russia retaliated on August 7, 2014, by imposing a strict embargo on imports of agricultural and food products from countries that had introduced sanctions. In the analysis below, we study the exporting behaviour of firms after August 2014.

Sanctions against Myanmar

Myanmar's political history has been extremely troubled since the country's independence in 1947. After a number of years of relative stability, the 1962 coup d'état led to the establishment of a military dictatorship led by Ne Win and the Burma Socialist Programme Party. Despite an

[11] Council regulation No 269/2014, Official journal of the European Union, 17.3.2014.

[12] See, e.g., Council Implementing Regulation No 284/2014, Official Journal of the European Union 21.3.2014; Council implementing decision of 28 April 2014, Official journal of the European Union, 29.4.2014; etc.

[13] Council regulation No. 833/2014, Official journal of the European Union, 31.7.2014.

[14] Dual-use goods are those products which, although not arms, are essential inputs in the manufacture of weapons. Cf. Council Regulation No 428/2009.

authoritarian and isolationist policy, this government was not subject to explicit international sanctions. This government was removed in 1988, in the wake of massive nationwide protests, which peaked on August 8, 1988. The crackdown of this "8888 uprising" was violent (350 dead according to official sources, but more likely several thousand casualties, cf. Hufbauer, Schott, Elliott, and Oegg, 2008). The protest ended the Ne Win regime, but not the dictatorship. On September 18, 1988, the military regained control of the situation, with a new coup that ended the protests brutally. This violent repression led to protests from India and other major democracies, but the promise to organize free elections quickly made it possible to minimize foreign concern. Elections were indeed held in 1990 which gave a landslide victory to the National League for Democracy, led by Aung San Suu Kyi. It is the military junta's refusal to accept the results of the 1990 elections that finally triggered the first significant international sanctions. The U.S. and most Western European countries downgraded diplomatic relations, imposed an embargo on arms, munitions and military equipment, and suspended defense cooperation and all non-humanitarian foreign aid. They also imposed a visa ban on members of the Burmese political leadership, senior military, and their family members. Furthermore, they suspended high-level governmental visits to Myanmar. In 1991, the U.S. refused to renew the bilateral U.S./Myanmar textile agreement. In 1996, the EU Common Position on Myanmar confirmed the EU sanctions imposed in 1990.[15]

From then, the pressure on Myanmar increased progressively. NGOs, buoyed up by the Nobel Prize awarded to Aung San Suu Kyi in 1991, redoubled their calls for tougher sanctions and boycotts. This led a large number of Western companies to stop their investments and withdraw from the country. The United States banned new investment in Myanmar by U.S. citizens and firms in 1997. In the same year, the EU imposed trade sanctions in the form of an exclusion of Myanmar from the General System of Preferences and the "Everything But Arms" trade and development initiative. In 1998, a resolution extended the visa bans.[16] The EU embargo on arms and munitions was reinforced significantly in 2000.[17] The 2000 resolution imposed an asset freeze and a ban on the financing of state-owned enterprises and those owned by senior members of the Myanma government and security forces. These measures were extended again in 2006.[18]

An important step was taken in 2008. After severely repressing a new protest movement in the summer of 2007, the government proposed a new constitution and called for elections in 2010. Western diplomats, doubting the sincerity of the Burmese authorities, decided to step up their pressure. In 2008, the EU banned the imports of a large number of products corresponding to Myanmar's comparative advantages (metals and stones, coal, wood, etc) and exports of key equipment for these industries.[19] In 2011, after a few uncertain years, the Myanmar government started a genuine democratization process. This included measures in favor of freedom of speech, the legalization of political parties and the release of political prisoners. The 2012 elections offered a clear victory to the main opposition party and the 2015 elections were the first truly

[15] Official Journal of the European Union, 8.11.96.
[16] Official Journal of the European Union, 30.10.98.
[17] Official Journal of the European Union, 24.5.2000.
[18] Official Journal of the European Union, 2.6.2006.
[19] Official Journal of the European Union, 10.3.2008.

free general elections.

In response to these positive developments, the EU, like the U.S., progressively dismantled its sanctions. The EU council regulation, published in late April 2012, suspended all sanctions against Myanmar with the exception of the arms embargo.[20] In May 2013, this suspension was confirmed and sanctions were definitively lifted.[21] The embargo on arms and munitions remained in force, but financial and trade restrictions were removed and Myanmar was reintegrated into the Generalized System of Preferences and obtained the most favorable "Everything But Arms" regime. The analysis below studies the behaviour of firms after sanctions were lifted at the end of April 2012.

U.S. embargo against Cuba

The case of Cuba is particular in our study. As in the case of Myanmar, what we can observe is only a lifting of sanctions. Above all, this is a case where French firms could only be affected by an extraterritorial effect. France – and then the EU – have not imposed severe sanctions against Cuba. However, the US embargo does contain provisions aimed at limiting the activities of non-US companies in Cuba.
The U.S. embargo against Cuba is one of the oldest international sanctions packages still in force. The first measures came in the form of an embargo on exports of arms and ammunition introduced by the Eisenhower administration in 1958, during the civil conflict between Castro's rebel forces and Batista's regular army. In 1960, trade restrictions increased in response to Fidel Castro's seizure of power and the nationalizations of U.S. companies operating in Cuba. In 1961, after the Bay of Pigs invasion, Congress passed the Foreign Assistance Act[22] allowing the U.S. to cut off aid to Cuba. This law was amended the following year to prohibit any assistance to foreign countries that would send aid to Cuba, and to establish a general trade embargo, with the exception of medical products and food. Following the 1962 missile crisis, travel to Cuba was banned and Cuban assets in the United States were frozen. The sanctions regime evolved marginally over the following decades, but most of it remained unchanged until the end of the Cold War. After the collapse of the USSR, Cuba lost its most important trading partner. This loss magnified mechanically the economic consequences of the U.S. sanctions and triggered a severe economic crisis in the early 1990s. By losing the support of the USSR, Cuba also lost a valuable political ally, which led the United States to further tighten its sanctions against the island. Laws passed during the 1990s strengthened the embargo and codified previous regulations.

The Cuban Democracy Act (also known as the Torricelli Act),[23] adopted by the U.S. Congress in 1992, banned subsidiaries of U.S. companies based abroad from trading with the island. It also prevented merchant ships that had called in Cuba in the last 180 days from docking in the United States. In 1996, the Cuban Liberty and Democratic Solidarity (LIBERTAD) Act (also known as the Helms-Burton Act)[24] paved the way for legal action against non-U.S. companies operating in Cuba that use property owned by American citizens (including former Cuban citizens) and

[20] European Council regulation 2012/0101, April 27, 2012.
[21] Official Journal of the European Union, 15.5.2012.
[22] Public Law 87-194 - Sept 1, 1961.
[23] H.R.5323 - 102nd Congress (1991-1992).
[24] .H.R.927 — 104th Congress (1995-1996).

confiscated after the Cuban revolution. With these successive laws, U.S. sanctions have clearly taken on a new dimension since they are explicitly extra-territorial in nature, and are able to penalize companies or individuals based abroad if they also have an activity linked to the United States. It is noteworthy that the European Union reacted to the Helms-Burton Act with an explicit Council Regulation.[25] The aim of this regulation was to denounce the extra-territorial aspects of the provisions of the Helms-Burton Act, to require the European companies affected to report the situation to the European Commission and to propose various support measures. Obviously, this has not prevented U.S. restrictions from entering into force and affecting third countries' exports to Cuba. For example, the Torricelli Act forced the Swedish company Pharmacia to stop exporting to Cuba after its merger with the U.S. company Upjohn.[26] Similarly, the Helms-Burton Act led the Mexican cement manufacturer Cemex to stop its activities in Cuba under threat of a lawsuit by its American competitor Lone Star Industries (see Gordon, 2016, for details and additional examples).

More recently, the extraterritoriality of U.S. sanctions has been reinforced by more aggressive enforcement of financial transaction regulations. The scope of regulations prohibiting dollar transactions involving Cuban nationals is indeed very broad. Between 2009 and 2012, the Treasury Department imposed fines ranging from $375 million to over $600 million on several European banks (Credit Suisse, ING, HSBC) for violating U.S. sanctions. In these cases, the main litigations involved transactions with Iran, but violations of the embargo with Cuba were also alleged.

A process of de-escalation began with the Obama administration. In April 2009, the U.S. presidency used its statutory power to modulate certain provisions of the Torricelli and Helms-Burton acts in order to ease the sanctions. More travels were allowed, some constraints on remittances were lifted and the government issued licences authorising U.S. telecommunications companies to operate in Cuba (see LeoGrande, 2016). In December 2014, the U.S. administration announced its commitment to normalizing relations with Cuba and ending the embargo, seen as a policy "that was long past its expiration date". In January 2015, an important step was taken. The United States significantly eased the constraints on travels to Cuba. Above all, U.S. companies were allowed to export goods and services to the Cuban private sector. This was a significant breach of the embargo, although these export authorizations were limited to contracts with the private sector, which remains relatively marginal in Cuba. This last important constraint was lifted in January 2016. A new set of regulations allowed U.S. companies to sell their goods and services to Cuban state-owned enterprises if these sales "meet the needs of the Cuban people" — a criterion sufficiently vague to cover a wide range of situations. The ban on offering trade credits to Cuba has also been partially lifted.

Starting immediately in 2017 the Trump administration reversed the loosening of sanctions against Cuba. In April 2019, the United States reactivated Title III of the Helms-Burton Act, allowing the prosecution of foreign companies that benefited from properties nationalized after the 1957. In October 2019, a revision of the Cuban Assets Control Regulations (CACR) reversed certain travel authorizations, imposed the closure of airlines to Cuba, limited remittances, etc.

[25] Council Regulation No 2271/96.

[26] Compare https://www.medicc.org/resources/documents/embargo/Chapter%20Three.pdf.

In the empirical analysis below, we will focus on the (brief) time of loosened sanctions, which started in January 2015. The period characterized by the return of sanctions is not covered in our econometric analysis.

3 Model

In this section, we present a partial equilibrium heterogeneous firms model of international trade for which we derive an expression for the firm-level decision of serving specific markets. We lean on Hinz, Stammann, and Wanner (2020), who focus on the aggregate extensive margin of trade between two countries. Our firm-level consideration will allow us to rely on a somewhat more general set of assumptions.

3.1 Theory

Consumers in N countries (denoted by a j subscript) obtain utility from consumption of different varieties ω in period t via the following CES function:

$$u_{jt} = \left(\int_{\omega \in \Omega_{jt}} (\xi_{\omega jt})^{\frac{1}{\sigma}} q_{\omega jt}^{\frac{\sigma-1}{\sigma}} \, d\omega \right)^{\frac{\sigma}{\sigma-1}},$$

where Ω_{jt} denotes the set of available varieties, $\xi_{\omega jt}$ is a log-standard-normally-distributed demand shock, $q_{\omega jt}$ represents the consumed quantity, and $\sigma > 1$ denotes the elasticity of substitution across varieties. One period corresponds to one month in our empirical setup. Each variety is produced by a distinct firm. Hence, the ω subscript can equivalently index a variety or a firm.

In each origin country i, there is a set of firms Ω_i.[27] In our empirical application, we will consider firms from one specific origin, namely French firms only. Firms differ in their unit production costs $c_{\omega t}$, e.g. due to different draws in a Melitz (2003)-type productivity lottery or due to firm-specific wages, as for instance in Helpman and Itskhoki (2010). Due to iceberg trade costs, firms have to produce $\tau_{\omega jt}$ units in order to deliver one unit to market j. The consumers' love of variety allows firms to charge a constant markup $\sigma/(\sigma-1)$ over marginal costs $\tau_{\omega jt} c_{\omega t}$. The demand for ω in market j depends on the variety price relative to the overall price level in the respective market, the idiosyncratic demand shock, and the market size:

$$q_{\omega jt} = \frac{p_{\omega jt}^{-\sigma}}{P_{jt}^{1-\sigma}} \xi_{\omega jt} E_{jt},$$

where $p_{\omega jt} = \frac{\sigma}{\sigma-1} \tau_{\omega jt} c_{\omega t}$ is the variety price discussed above, $P_{jt} = \left(\int_{\omega \in \Omega_{jt}} \xi_{\omega jt} p_{\omega jt}^{1-\sigma} \, d\omega \right)^{\frac{1}{1-\sigma}}$ denotes the ideal price index, and E_{jt} is total expenditure in market j in period t.

Due to its monopolistic competition markup, a firm can earn an operating profit $\pi_{\omega jt} = (1/\sigma) p_{\omega jt} q_{\omega jt}$ by serving the respective market. However, it has to pay a fixed exporting cost $f_{\omega jt}^{exp}$. If the firm has not been active in the previous year, it additionally incurs a market entry

[27] We abstract from firm creation and firm death and hence consider a constant set of firms here.

cost which scales up the fixed cost of exporting, i.e. it has to pay $f^{entry} f^{exp}_{\omega jt}$ rather than only $f^{exp}_{\omega jt}$. A firm hence only decides to export to a market at all, if the profits to be earned are high enough to overcome the associated fixed (and entry) costs. For simplicity, we let firms base their export decision only on the current and past period and abstract from "investments" into specific markets, where a firm is willing to incur a loss due to entry costs in order to earn future profits in the market.[28] Then, we can derive the following expression for the indicator variable $y_{\omega jt}$ for the firm-level extensive margin:

$$y_{\omega jt} = \begin{cases} 1 & \text{if} \quad \frac{\left(\frac{1}{\sigma}\left(\frac{\sigma}{\sigma-1}\tau_{\omega jt}c_{\omega t}\right)^{1-\sigma} P_{jt}^{\sigma-1}\xi_{\omega jt}E_{jt}\right)}{f^{exp}_{\omega jt}(f^{entry})^{(1-\max_{s\in\{1,\dots,12\}}\{y_{\omega j(t-s)}\})}} \geq 1, \\ 0 & \text{else.} \end{cases} \tag{1}$$

Note that entry cost expression includes twelve lags (captured by superscript s), because one period in our data set will correspond to one *month*, while entry costs only have to be paid if the firm has not been active in the market in the previous *year*.

As Equation (1) shows, a firm is likely to export if it is very competitive (due to low production costs), the destination market is attractive (due to low competitive pressure and/or large size), if it was already active in the market in the previous year, and if it faces low barriers of exporting (due to low variable, fixed, and/or entry costs). The latter part is where trade sanctions come into play: they make trading with the sanctioned country more costly. This can happen via increased transaction costs that make every unit sold to the sanctioned country more expensive. Or it could mean additional administrative costs for the firm to make sure that it acts in line with the imposed sanctions, implying higher fixed costs of selling to the sanctioned country at all.

3.2 Empirical Specifications

3.2.1 Baseline

As a baseline, we choose the following flexible specifications for fixed and variable trade costs:

$$\tau_{\omega jt} = \exp\left(\lambda^{\tau}_{\omega t} + \psi^{\tau}_{jt} + \mu^{\tau}_{\omega j} + \beta_{\tau} SANCT_{jt}\right), \tag{2}$$

$$f^{exp}_{\omega jt} = \exp\left(\lambda^{f}_{\omega t} + \psi^{f}_{jt} + \mu^{f}_{\omega j} + \beta_{f} SANCT_{jt}\right), \tag{3}$$

where the λ's, ψ's, and μ's are (potentially unobservable) firm-time, destination-time, and firm-destination trade cost components, and $SANCT_{jt}$ is the treatment variable, i.e. an indicator variable that is equal to one if country j is sanctioned in period t. We will consider the four different sanctions episodes in separate regressions and drop the three other sanctioned countries from the sample in each case.

With trade cost specifications (2) and (3), the theoretical expression (1) translates into the following empirical specification:

$$y_{\omega jt} = \mathbf{1}\Big[\kappa + \lambda_{\omega t} + \psi_{jt} + \mu_{\omega j} + \gamma \max_{s\in\{1,\dots,12\}}\{y_{\omega j(t-s)}\} \geq \zeta_{\omega jt}\Big], \tag{4}$$

[28] See Hinz, Stammann, and Wanner (2020) for a very similar formulation of the extensive margin that allows for this investment motive.

where

$$
\begin{aligned}
\kappa &\equiv -\sigma \log(\sigma) - (1-\sigma)\log(\sigma-1) - \gamma, \\
\lambda_{\omega t} &\equiv (1-\sigma)(\log(c_{\omega t}) + (1-\sigma)\lambda^{\tau}_{\omega t} - \lambda^{f}_{\omega t}, \\
\psi_{jt} &\equiv (\sigma-1)\log(P_{jt}) + \log(E_{jt}) + (1-\sigma)\psi^{\tau}_{jt} - \psi^{f}_{jt} \\
&\qquad + SANCT_{jt}\left((1-\sigma)\alpha_{1\tau} - \alpha_{1f}\right), \\
\mu_{\omega j} &\equiv (1-\sigma)\mu^{\tau}_{\omega j} - \mu^{f}_{\omega j}, \\
\gamma &\equiv \log(f^{entry}) \quad \text{, and} \\
\zeta_{\omega jt} &\equiv -\log(\xi_{\omega jt}) \sim \mathcal{N}(0,1).
\end{aligned}
$$

$\lambda_{\omega t}$, ψ_{jt}, and $\mu_{\omega j}$ are three high-dimensional sets of firm-time, destination-time, and firm-destination fixed effects. These fixed effects flexibly control for the unobserved components of the theoretical expression (1) (such as firm-level production costs or the competitive environment in the destination market), as well as for many, partly unobserved, trade cost determinants. However, besides posing some computational and econometric challenges discussed in the next section, they have an additional drawback: the jt fixed effects not only successfully control for a number of important unobservables, they also capture the overall sanctions effect on fixed and variable trade costs. As we will discuss below, if the sanctions affect firms' trade costs differently depending on characteristics of the firms or their products, this heterogeneous impact can be identified — yet only jointly for fixed and variable trade costs.

However, we also want to estimate an overall sanctions effect. To do so, we follow a variation of the strategy by Crozet, Head, and Mayer (2012) and include *estimated values* for the destination-time fixed effects, obtained from a standard gravity estimation of the *intensive* margin of aggregate trade flows and corrected for the intensive margin sanctions effect, as a regressor (see Section 4.3 for details). The additional regressor controls for the importing countries' overall market potential, while leaving identifying variation at the destination-time level for the sanctions effect. This approach leads to the second type of specification that we will consider (here and in the following we shorten notation by suppressing the constant and defining $y^{max}_{\omega j(t-12)} \equiv \max_{s\in\{1,\dots,12\}}\{y_{\omega j(t-s)}\}$):

$$
y_{\omega jt} = \mathbf{1}\left[\lambda_{\omega t} + \alpha\widehat{\psi}_{jt} + \mu_{\omega j} + \gamma y^{max}_{\omega j(t-12)} + \beta SANCT_{jt} \geq \zeta_{\omega jt}\right] \tag{5}
$$

Importantly, this specification features $\beta \equiv (1-\sigma)\alpha_{1\tau} - \alpha_{1f}$ and therefore allows identification of the sanctions effect.

3.2.2 Entry Cost Sanctions Effect

Besides fixed and variable trade costs, sanctions can also affect market entry costs. Coping with the sanctions may be easier (and cheaper) if the firm has already a business network in the partner country, implying that the relative costs of *starting* to export to the sanctioned country compared to continuing to serve it may also increase. We incorporate this possibility by allowing

the entry cost factor to be variable:

$$f_{jt}^{entry} = \exp\Big(\gamma_0 + \gamma_1 SC_j + \gamma_2 SP_t + \gamma_3 SANCT_{jt}\Big), \tag{6}$$

where SC_j and SP_t are dummy variables for the sanctioned country (Iran, Russia, Myanmar, and Cuba in the different estimations) and the corresponding sanctions periods, respectively. They are included in addition to the $SANCT_{jt}$ variable to ensure that the sanctions effect does not pick up general differences in trade costs for the countries that *at some point* get sanctioned or general time trends.

Incorporating the more general entry cost specification given in (6), the three-way fixed effects specification (4) becomes:

$$y_{\omega jt} = \mathbf{1}\Big[\lambda_{\omega t} + \psi_{jt} + \mu_{\omega j} + y_{\omega j(t-12)}^{max}\left(\gamma_0 + \gamma_1 SC_j + \gamma_2 SP_t + \gamma_3 SANCT_{jt}\right) \geq \zeta_{\omega jt}\Big]. \tag{7}$$

While the overall sanctions effect is not identified in the thee-way fixed effects specification, a different picture emerges for the differential impact on firms that are previously active in the sanctioned market. Entry costs are only relevant for firms that are not yet active in the destination market. Therefore, the (firm-destination-time varying) lagged dependent variable features in the estimation of all entry cost components and hence the effect of all variables of interest on the entry costs can be identified.

For comparability and to be able to identify overall sanctions treatment effects, we again complement the three-way specification with a two-way fixed effects model that additionally incorporates estimated destination-time fixed effects as a regressor:

$$y_{\omega jt} = \mathbf{1}\Big[\lambda_{\omega t} + \alpha\widehat{\psi}_{jt} + \mu_{\omega j} + y_{\omega j(t-12)}^{max}\left(\gamma_0 + \gamma_1 SC_j + \gamma_2 SP_t\right) + SANCT_{jt}\left(\beta + \gamma_3 y_{\omega j(t-12)}^{max}\right) \geq \zeta_{\omega jt}\Big] \tag{8}$$

A comparison of the estimates for β and γ_3 in this specification can inform us which part of the sanctions effect can be avoided by firms that are already active in the sanctioned market.

3.2.3 Heterogeneous Sanctions Effects Along Firm Characteristics

Previous export activity in the sanctioned country is not the only potential source of heterogeneous treatment effects. Firms could for example be differently affected if they rely more or less strongly on trade finance, if they are specialized in consumer or intermediate goods, or if they have gathered experience by exporting to other "crisis countries" (see Section 5 for a discussion of all sources of heterogeneity that we consider). We test whether sanctions effects differ along these firm characteristics by introducing the following more general trade cost specifications:

$$\tau_{\omega jt} = \exp\Big(\lambda_{\omega t}^{\tau} + \psi_{jt}^{\tau} + \mu_{\omega j}^{\tau} + SANCT_{jt}\left(\alpha_{1\tau} + \alpha_{2\tau} x_{\omega}\right)\Big), \tag{9}$$

$$f_{\omega jt}^{exp} = \exp\Big(\lambda_{\omega t}^{f} + \psi_{jt}^{f} + \mu_{\omega j}^{f} + SANCT_{jt}\left(\alpha_{1f} + \alpha_{2f} x_{\omega}\right)\Big), \tag{10}$$

where x_ω refers to the observable firm characteristic under investigation. With these variable and fixed trade costs, our three- and two-way fixed effects specifications become

$$y_{\omega jt} = \mathbf{1}\Big[\lambda_{\omega t} + \psi_{jt} + \mu_{\omega j} + \gamma y^{max}_{\omega j(t-12)} + SANCT_{jt}\left(\beta_2 x_\omega\right) \geq \zeta_{\omega jt}\Big] \tag{11}$$

and

$$y_{\omega jt} = \mathbf{1}\Big[\lambda_{\omega t} + \alpha\widehat{\psi}_{jt} + \mu_{\omega j} + \gamma y^{max}_{\omega j(t-12)} + SANCT_{jt}\left(\beta_1 + \beta_2 x_\omega\right) \geq \zeta_{\omega jt}\Big]. \tag{12}$$

Similar to the lagged dependent variable in (7), x_ω introduces firm-level variation that allows identification of the heterogeneity coefficient in the three-way specification. The results can be compared to the ones from the two-way fixed effects framework, in which we can again additionally identify the overall sanctions effect.

4 Data and Estimation Methodology

Our empirical specifications (4), (5), (7), (8), (11), and (12) are equivalent to probit models with two or three sets of fixed effects and a lagged dependent variable. This poses econometric as well as computational challenges. In this section we first briefly describe the firm-level data that is used in all estimations, and then discuss how we overcome these challenges.

4.1 Data

To estimate the effect of sanctions on the extensive margin of trade at the firm-level, i.e. a firm's decision to export or not to a given market, we rely on French customs data. The data set encompasses the universe of French exporting firms from 2009 until 2016 with monthly frequency. It comprises the date, i.e. year-month, an identifier of the firm (the so-called SIREN), the importing or exporting partner, the products, with an 8 digit HS-code, as well as the value in Euros and quantity traded. We restrict our analysis to information on the firm and exporting destination and hence trace firms' participation in exporting markets over time.

For each of the four cases discussed above — sanctions against Iran, Russia, Myanmar and a U.S.imposed embargo on Cuba — we restrict the sample to two years before and after the imposition or lifting of sanctions, respectively. The specific points in time we consider as the starting or end points of the sanctions are January 2012, August 2014, May 2012, and January 2015 for Iran, Russia, Myanmar, and Cuba, respectively. As the data set records positive trade flows, we need to construct the absence thereof. We do so by assigning zeros to those combinations of date $\times$ firm $\times$ destination, where the firm has been active on a given market at least once and only if the firm exports at all on a given date.[29] For each regression we omit all observations of the three other sanctioned countries. All in all, the case subsets of the data each comprise around 35 million observations and cover more than 150,000 firms potentially exporting to up to 223 destinations in 48 months.[30]

[29] With the employed three sets of fixed effects this covers exactly all possible combinations that are not trivially perfectly classified. E.g. if a firm does not export at all on a given date, the firm $\times$ date fixed effect will consume these observations; if a firm never exports to a given market, the firm $\times$ destination effect will consume these.

[30] See Table 14 in Appendix A for case-specific summary statistics. Note that in the estimations we report the sample size as the number of those observations actually used in the estimation, i.e. net of those observations perfectly classified with one, or a combination of multiple, fixed effects.

Furthermore we construct a number of firm-level variables for use in the estimation of equations (11) and (12). When constructing shares and means, these are computed using trade weights from the 12 months prior to the first year in the sample.[31]

4.2 Estimation Methodology

The standard approach to estimate fixed effects probit models is a maximum likelihood estimator (MLE) that estimates the parameters of interest and the fixed effects simultaneously. Due to the network structure of our data and the two- or three-way fixed effects specifications of our model, a large number of parameters has to be estimated. Thanks to recent advances in the field of computational econometrics, however, such high-dimensional specifications are no longer an obstacle.[32]

Another issue that needs to be addressed is the so-called incidental parameter problem (IPP), which leads to asymptotically biased estimates of the structural parameters and average partial effects (see Neyman and Scott, 1948). More precisely, the inclusion of each set of fixed effects, $\lambda_{\omega t}$, ψ_{jt} and $\mu_{\omega j}$, creates a separate bias, each of a different order.[33] Fernández-Val and Weidner (2018) formulate a simple heuristic helping to assess the order of the incidental parameter bias: $bias \sim p/n$, where p denotes the number of parameters and n the number of observations. Hence, the composite bias in our two-way fixed effects specification is of order $(\Omega T + \Omega J)/(\Omega JT)$ and in our three-way fixed effects specification it is of order $(\Omega T + JT + \Omega J)/(\Omega JT)$, where Ω, J, and T are the number of firms, destination markets, and periods, respectively. Thus, the bias term induced by $\lambda_{\omega t}$ decreases with increasing J, the one induced by ψ_{jt} shrinks with growing Ω, and the one induced by $\mu_{\omega j}$ gets smaller with larger T. Since in our applications Ω and J will be large (around 150,000 exporters and more than 220 destinations), the biases induced by $\lambda_{\omega t}$ and ψ_{jt} are expected to be relatively small. The main driver of the bias in our application will come from the inclusion of the $\mu_{\omega j}$ fixed effect, because T is only moderately large (48 months). Further, the combination of pair fixed effects and lagged dependent variables as regressors leads to a violation of the strict exogeneity assumption, which further amplifies the bias (see Fernández-Val and Weidner, 2018; Hinz, Stammann, and Wanner, 2020).[34] Recently, Hinz, Stammann, and Wanner (2020) developed appropriate bias corrections for coefficients and average partial effects of dynamic binary choice models with the same fixed effects structure we are considering. In our

[31] As an example, the sample for Iran starts in January 2010 and hence the 4 digit-product trade weights are taken as the sum of export values by the specific firm to all destinations observed in the months January – December 2009. Information on the trade finance intensity is sourced from Crozet et al. (2020), on external financial dependence from Kroszner et al. (2007), the classification for consumer and intermediate goods by broad economic categories (BEC) is provided by the United Nations Statistics Division.

[32] Stammann (2018) proposed a pseudo-demeaning approach which is a generalization of the within-transformation known from the linear fixed effects estimator for nonlinear models.

[33] The intuition is as follows. Due to the nonlinearity of the probit model, the MLE cannot estimate the structural parameters separately from the fixed effects. Specifically, the IPP arises because only a subset of observations contributes to the identification of the fixed effects, which leads to noisy estimates of the fixed effects. For example, there are only T observations contributing to the identification of a firm-destination fixed effect $\mu_{\omega j}$. Due to the dependence of the estimates of the structural parameters on the estimates of the fixed effects, they will be affected by this estimation noise.

[34] This also includes various functional forms of the lagged dependent variable. For example, interactions between an exogenous regressor with a lagged dependent variable or lags aggregated on a different time dimension than the dependent variable (e.g. if the dependent variable is on a monthly basis, but the lag is based on years).

econometric analysis we follow their recommendation and use an analytical bias correction.

We compute the bias-corrected coefficients according to

$$\tilde{\boldsymbol{\beta}}^a = \hat{\boldsymbol{\beta}}_{\Omega,J,T} - \frac{\widehat{\mathbf{B}}_2^{\beta}}{J} - \frac{\widehat{\mathbf{B}}_3^{\beta}}{T}$$

in the case of two-way fixed effects specifications and according to

$$\tilde{\boldsymbol{\beta}}^a = \hat{\boldsymbol{\beta}}_{\Omega,J,T} - \frac{\widehat{\mathbf{B}}_1^{\beta}}{\Omega} - \frac{\widehat{\mathbf{B}}_2^{\beta}}{J} - \frac{\widehat{\mathbf{B}}_3^{\beta}}{T}$$

in the case of three-way fixed effects specifications, where $\hat{\boldsymbol{\beta}}_{\Omega,J,T}$ denotes the uncorrected coefficients and the expressions for the bias terms $\widehat{\mathbf{B}}_1^{\beta}$, $\widehat{\mathbf{B}}_2^{\beta}$, and $\widehat{\mathbf{B}}_3^{\beta}$ are further specified in Hinz, Stammann, and Wanner (2020).

As coefficients are not directly interpretable beyond their signs and relative sizes in a binary choice context, we will also obtain average partial effects. Specifically, we will report average treatment effects on the treated (ATT) which we obtain as follows:

$$\widehat{ATT} = \frac{\sum_{\omega}\sum_{j}\sum_{t} SC_j \times SP_t \times \widehat{\Delta}_{\omega jt}}{\sum_{\omega}\sum_{j}\sum_{t} SC_j \times SP_t}, \tag{13}$$

where $\widehat{\Delta}_{\omega jt}$ takes slightly different forms for the three types of specifications discussed in Sections 3.2.1 to 3.2.3 . Specifically, it is given by

$$\widehat{\Delta}_{\omega jt} = \Phi\big(\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + \hat{\gamma} y^{max}_{\omega j(t-12)} + \hat{\beta}\big) - \Phi\big(\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + \hat{\gamma} y^{max}_{\omega j(t-12)}\big) \tag{14}$$

in the baseline specification (5), by

$$\begin{aligned}\widehat{\Delta}_{\omega jt} = \Phi\big(&\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + (\hat{\gamma}_0 + \hat{\gamma}_1 SC_j + \hat{\gamma}_2 SP_t + \hat{\gamma}_3)\, y^{max}_{\omega j(t-12)} + \hat{\beta}\big) \\ &- \Phi\big(\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + (\hat{\gamma}_0 + \hat{\gamma}_1 SC_j + \hat{\gamma}_2 SP_t)\, y^{max}_{\omega j(t-12)}\big)\end{aligned} \tag{15}$$

in the sanctions entry cost effects specification (8), and by

$$\widehat{\Delta}_{\omega jt} = \Phi\big(\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + \hat{\gamma} y^{max}_{\omega j(t-12)} + \hat{\beta}_1 + \hat{\beta}_2 x'_{\omega}\big) - \Phi\big(\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + \hat{\gamma} y^{max}_{\omega j(t-12)}\big) \tag{16}$$

in the sanctions effects specification varying with firm characteristics (12).

Note that we focus on the specification with firm-destination and firm-time fixed effects and *estimated* destination-time fixed effects for the ATTs, as it allows the separate identification of all coefficients featuring in the partial effect expression. Similar as for the coefficients, we apply a bias correction for the ATTs following Fernández-Val (2009) and Hinz, Stammann, and Wanner (2020). More precisely, we bias-correct the ATT according to

$$\widetilde{\widehat{ATT}}^a = \widehat{ATT} - \frac{\widehat{\mathbf{B}}_2^{\delta}}{J} - \frac{\widehat{\mathbf{B}}_3^{\delta}}{T}$$

where the expressions $\widehat{\mathbf{B}}_2^\delta$ and $\widehat{\mathbf{B}}_3^\delta$ are further specified in Hinz, Stammann, and Wanner (2020).[35]

We allow the sanctions effect to be heterogeneous along different dimensions. To infer the effect of drivers of heterogeneity, it is not sufficient to split the treatment group along this dimension and obtain separate ATTs for the "high x_ω" and "low x_ω" groups. This is because the groups potentially have different fitted probabilities and therefore have different average partial effects irrespective of whether the source of heterogeneity actually has an additional impact on the sanctions effect or not. We therefore calculate group ATTs *with and without* taking the respective source of heterogeneity into account in the specifications and then look at how the incorporation of the heterogeneity *changes* the different group ATTs. For the heterogeneous sanctions effects, we hence calculate five bias-corrected ATTs altogether: $\widetilde{ATT}_{high}^{baseline}$, $\widetilde{ATT}_{low}^{baseline}$, $\widetilde{ATT}_{high}^{heterogeneous}$, $\widetilde{ATT}_{low}^{heterogeneous}$, and an $\widetilde{ATT}_{high\ vs.\ low}^{change}$ which captures the change discussed above and is defined as

$$\widetilde{ATT}_{high\ vs.\ low}^{change} = \left(\widetilde{ATT}_{high}^{heterogeneous} - \widetilde{ATT}_{low}^{heterogeneous}\right) - \left(\widetilde{ATT}_{high}^{baseline} - \widetilde{ATT}_{low}^{baseline}\right). \tag{17}$$

In the entry cost sanctions effect specification (8), "high" corresponds to firms active in the market in the previous year and "low" to firms that did not sell to the respective market in the year before. As (8) not only includes one additional interaction term of the sanctions variable, but also the interaction terms of SC_j and SP_t with the lagged dependent variable, we calculate a new baseline in this case to ensure comparability, omitting only $y_{\omega j(t-12)}^{max}\gamma_3 SANCT_{jt}$ from (8), leading to the following partial effects:

$$\begin{aligned}\widehat{\Delta}_{\omega jt} = \Phi\big(&\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + (\hat{\gamma}_0 + \hat{\gamma}_1 SC_j + \hat{\gamma}_2 SP_t)\, y_{\omega j(t-12)}^{max} + \hat{\beta}\big) \\ &- \Phi\big(\hat{\lambda}_{\omega t} + \hat{\alpha}\hat{\psi}_{jt} + \hat{\mu}_{\omega j} + (\hat{\gamma}_0 + \hat{\gamma}_1 SC_j + \hat{\gamma}_2 SP_t)\, y_{\omega j(t-12)}^{max}\big).\end{aligned} \tag{18}$$

4.3 Construction of the estimated fixed effects regressor

For the construction of our sanctions-corrected estimated destination-time fixed effects we use an intensive-margin structural gravity formulation for bilateral trade flows. Chaney (2008) derives such a structural gravity expression for a Melitz (2003) framework that is closely related to our model, however adding some more specific assumptions. If an exogenously given number N_{it} of firms draw their input requirements from country-specific Pareto distributions with support $\varphi \in [0, \bar{\varphi}_{it}]$ and dispersion parameter θ, use only labor with wage w_i as an input, and pay the fixed exporting costs in terms of domestic labor, there are no firm-specific demand shocks, and trade costs vary only on the bilateral level rather than by firm, trade flows from country i to j in period t adhere to the following expression:

$$X_{ijt} = \frac{N_{it}\bar{\varphi_{it}}^{-\theta} w_{it}^{-\theta-\frac{\theta}{\sigma-1}+1} \tau_{ijt}^{-\theta} \left(f_{ijt}^{exp}\right)^{-\left[\frac{\theta}{\sigma-1}-1\right]} E_{jt}}{\sum_k N_{kt}\bar{\varphi_{kt}}^{-\theta} w_{kt}^{-\theta-\frac{\theta}{\sigma-1}+1} \tau_{kjt}^{-\theta} \left(f_{kjt}^{exp}\right)^{-\left[\frac{\theta}{\sigma-1}-1\right]}}. \tag{19}$$

[35] Note that Hinz, Stammann, and Wanner (2020) obtain bias corrected average partial effects and hence the expressions $\widehat{\mathbf{B}}_2^\delta$ and $\widehat{\mathbf{B}}_3^\delta$ depend on all observations, while we obtain ATTs and hence the expressions only depend on observations with $SP_t = SC_j = 1$.

As shown by Head and Mayer (2014), this can be reformulated as:

$$X_{ijt} = \frac{Y_{it}}{\Omega_{it}} \frac{E_{jt}}{\Theta jt} \phi_{ijt}, \tag{20}$$

$$\Omega_{it} = \sum_{k} \frac{\phi_{ikt} E_{kt}}{\Theta_{kt}}, \tag{21}$$

$$\Theta_{jt} = \sum_{k} \frac{\phi_{kjt} Y_{kt}}{\Omega_{kt}}, \tag{22}$$

where Y_{it} is the value of total (international) sales of country i, E_{jt} is total (import) expenditure of country j across all origin countries, Ω_{it} and Θ_{jt} are out- and inward multilateral resistance terms (MRTs), respectively, and $\phi_{ijt} = \tau_{ijt}^{-\theta} (f_{ijt}^{exp})^{-\left[\frac{\theta}{\sigma-1}+1\right]}$ denotes the bilateral accessibility between the two countries in period t. Note that the inward multilateral resistance term is closely linked though not identical to the importing country's price index (see Equation (8) in Chaney, 2008).

Using aggregate monthly data on trade flows from UN Comtrade for the world's 20 largest exporting countries to all destinations,[36] we estimate (20) with the following specification:

$$X_{ijt} = \exp(\lambda_{it}^{int} + \psi_{jt}^{int} + \mu_{ijp}^{int}) + \varepsilon_{ijt}, \tag{23}$$

where the int superscript stands for the intensive margin considered here and $p \in \{\text{pre}, \text{post}\}$ denotes whether t is in the pre- or post-sanctions period, such that effectively two bilateral fixed effects for each country pair are estimated, one during the sanctions period and one before (or after, for the cases of Myanmar and Cuba). Different from the firm-level margin, we can now use bilateral variation in sanction imposition — if there is any — to capture the sanctions effect on fixed and variable trade costs, jointly modelled as bilateral accessibility $\phi_{ijt} = \exp(\mu_{ijp}^{int})$.

The estimated intensive margin destination-time fixed effects $\hat{\psi}_{jt}^{int}$ capture total expenditure (also present in the firm-level extensive margin) and the inward multilateral resistance term (closely related to the price index featuring in the extensive margin), and approximate the overall attractiveness of the export market. In principal, they could be used as a control variable in our two-way fixed effects extensive margin specification. However, the overall attractiveness is directly affected by the existence or absence of sanctions, through the inward multilateral resistance term as well as overall spending on imports, which is precisely an effect we want to identify separately. We therefore construct counterfactual estimated fixed effects in which we attempt to remove this sanctions effect.

Specifically, we assume counterfactual bilateral accessibilities $\phi_{ijt}^{'} = \exp(\mu_{ij,\text{pre}}^{int})$, i.e. to be equal to those from the pre-sanctions period. Different from our extensive margin estimations, where all firms are from France, there is bilateral variation in sanction imposition here. We can use this to purge the trade costs of the sanctions effect. Note, however, that if there is no bilateral variation in imposed sanction when all countries put in force the same sanctions vis-à-vis the target country, the fixed effect will also absorb this destination-specific component of bilateral

[36] We restrict the data set to these 20 countries in order to ensure consistent availability of trade data over the entire time period, as otherwise changes in the estimated fixed effects could be due to changes in the sample composition.

accessibility. Using ϕ'_{ijt} will not take out this "undesired" part of ψ_{jt}. We therefore additionally make a second adjustment to the fixed effects by assuming that import expenditure by the sanctioned country would have followed the pre-sanctions trend in absence of the imposition of sanctions, giving us counterfactual expenditure values E'_{jt}.[37] We then solve the multilateral resistance system (21) and (22) for these values to obtain the counterfactual inward multilateral resistance terms Θ'_{jt} and can then construct new, counterfactual destination-time fixed effects that control for the overall attractiveness of market j in period t, net of the sanctions effect as

$$\hat{\psi}_{jt} = \hat{\psi}_{jt}^{int\prime} = \ln\left(\frac{E'_{jt}}{\Theta'_{jt}}\right). \tag{24}$$

We are now ready to move on to the estimation of the model. All tables shown in the two following sections report bias-corrected estimates.[38] Tables 16 and 17 in Appendix B show the uncorrected estimates of the baseline specifications (4) and (5), as well as for the lag-interaction specifications (7) and (8).[39] The differences between the two sets of estimates do not alter the qualitative nature of the conclusions, but varying magnitudes and significances nevertheless highlight the importance of the bias correction.

Section 5 below presents the core results, which assess the impact of Iranian and Russian sanctions on French exporters. Section 6 examines the cases of Cuba and Myanmar.

5 Estimation results for Iran and Russia sanctions

We first estimate our model to assess the average impact of the Iranian and Russian sanctions on French exports. In a second step, we will see how this impact varies according to the characteristics of the exporting firms or the the exported products.

5.1 Baseline

We first estimate the baseline specifications (4) and (5) for the cases of Iran and Russia, to get an overall picture of the respective impact of the two sanctions episodes where sanctions were imposed. We report the results in Table 1. Columns (1) and (3) display the three-way fixed effects results. As discussed in Section 3, the sanctions effect is not identified in this case and there is hence only the coefficient of the (extended) lagged dependent variable to report. As expected, it is highly statistically significant, indicating strong true state dependence, or — in terms of the model — the existence of considerable entry costs. Interpreting the magnitudes strictly in terms of the model parameters, the results imply entry cost factors $f^{entry} = \exp(0.209) = 1.232$ and $f^{entry} = \exp(0.172) = 1.188$, i.e. fixed costs of exporting are 23% or 19% higher for firms that have not yet entered the market.

Columns (2) and (4) show the results of the two-way fixed effects specification. Again, previous

[37] Note that this also requires adjusting world exports by the same amount in order to satisfy $\sum_{it} Y_{it} = \sum_{jt} E_{jt}$, which we obtain by proportionally adjusting all production values to Y'_{it}.

[38] The bias correction uses a bandwidth parameter $L = 2$ (see Hinz, Stammann, and Wanner, 2020, for details).

[39] Uncorrected estimates for all other specifications are available in the Online Appendix (https://julianhi.nz/worth_the_pain_online_appendix.pdf).

Table 1: Baseline specification for Iran and Russia

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.209***	0.303***	0.172***	0.249***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.434***	-	-0.296***
	-	(0.012)	-	(0.007)
Estimated Date-Partner FE$_{jt}$	-	0.288***	-	0.219***
	-	(0.003)	-	(0.003)
Overall ATTs of sanctions	-	-0.075***	-	-0.057***
	-	(0.001)	-	(0.001)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	27,396,537	27,168,057	27,785,280	26,846,577

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

export activity significantly increases the probability to serve a market. The corresponding coefficients actually are somewhat larger than in the three-way specification. The *estimated* jt fixed effects also turn out to be a highly significant determinant of the firm-level extensive margin as they capture the overall market potential of the destination country. Importantly, the two-way specification now also allows the identification of the sanctions effect. We find that the introduction of sanctions on Iran and Russia significantly lowered the probability of French firms to export to these destinations by increasing the fixed and/or variable costs of serving them.



(a) Iran sanctions **(b)** Russia sanctions

Figure 2: Evolution of observed, predicted and counterfactual number of firms

The average treatment effects on the treated are reported below the coefficient estimates in Table 1. French firms were 7.5 percentage points less likely to export to Iran and 5.7 percentage points less likely to export to Russia due to the sanctions imposed. Given the arguably stronger

Table 2: Previous experience

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.338***	0.429***	0.312***	0.383***
	(0.002)	(0.002)	(0.002)	(0.002)
— × Sanctions Period$_t$	-0.254***	-0.253***	-0.282***	-0.266***
	(0.002)	(0.002)	(0.002)	(0.002)
— × Sanctioned Country$_j$	-0.081***	-0.100***	0.065***	0.074***
	(0.023)	(0.023)	(0.014)	(0.014)
— × Sanctions$_{jt}$	0.125***	0.121***	0.035*	0.017
	(0.032)	(0.031)	(0.018)	(0.018)
Sanctions$_{jt}$	-	-0.541***	-	-0.316***
	-	(0.027)	-	(0.016)
Estimated Date-Partner FE$_{jt}$	-	0.278***	-	0.223***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	27,396,537	27,168,057	27,785,280	26,846,577

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

sanctions enacted vis-à-vis Iran, the ordering of the magnitudes is as one would have expected.

To get a better impression for the size of these ATTs, consider the average probability of the firms entering the ATT calculation of serving the Iranian and Russian market in any given month during the sanctions. It is 11.4% and 18.6%, respectively, but would be $11.4 + 7.5 = 18.9\%$ and $18.6 + 5.7 = 24.3\%$, if it wasn't for the sanctions. We can further illustrate this by looking at the implied predicted number of exporting firms. Figures 2a and 2b show the evolution of the observed (in red) and predicted (in green, obtained as the sum of the fitted probabilities over all firms in the given month) number of firms exporting to Iran and Russia. Additionally, they plot the counterfactual predictions in the absence of sanctions (in blue, obtained by summing the counterfactual fitted probabilities for $SANCT_{jt} = 0$). We would very clearly expect many more exporters to both these destinations without sanctions. Specifically, we estimate sanctions to lower the number of exporters to Iran by 39.2% and to Russia by 23.4%.

We now explore different sources of heterogeneity along which we suspect the exporting behaviour of firms to be impacted.

5.2 Previous experience

As a first source of heterogeneity in the sanctions effect, the model lays out a mechanism through which firms may be affected along their fixed cost of entry. We therefore estimate specifications (7) and (8). The results are displayed in Table 2.

Columns (1) and (3) again report the results from the three-way fixed effects specification. All

Table 3: Previous experience

	Iran	Russia
Not active previous year (*baseline*)	-0.048*** (0.001)	-0.033*** (0.000)
Active previous year (*baseline*)	-0.108*** (0.001)	-0.075*** (0.001)
Not active previous year (*with heterogeneity*)	-0.056*** (0.001)	-0.034*** (0.001)
Active previous year (*with heterogeneity*)	-0.101*** (0.002)	-0.074*** (0.001)
Implied ATT difference between active and non-active in previous year	1.6 p.p.	0.2 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (18), heterogeneity refers to equation (15). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

drivers of heterogeneity in the state dependence (i.e. in entry costs) can be identified even with this strongest set of fixed effects. The estimated coefficients for sanctions period and sanctioned country make clear that there is considerable heterogeneity in entry costs across countries and time. The interaction of the sanctions variable with previous export activity clearly indicates in the case of Iran that already serving the Iranian market softens the blow of the sanctions introduction. In terms of our empirical framework, this implies that sanctions partly act via increased entry costs.[40] Specifically, sanctions increase the entry cost factor f^{entry} by $\exp(0.125) - 1 = 13.3\%$. The corresponding coefficient in the Russian case is much lower, and only marginally significant.

Columns (2) and (4) show the results for the entry cost interaction in a two-way fixed effects setting. Reassuringly, most of the interaction coefficients that can be identified in both the two- and three-way specification are very similar. However, in the Russia case the sanctions interactions term with previous export experience is not significant. For Iran, the relative magnitude of the coefficients indicates that about $0.121/0.541 = 22.4\%$ of the sanctions effect is offset for a firm that already served the market, giving us an impression of the relative importance of the entry cost effect.[41]

Corresponding ATTs and the ATT differences to the baseline are reported in Table 3. Rows three and four show the ATTs corresponding to the coefficients in Table 2, separately for firms that were active in the destination market in the previous year and such that were not. Both for the Iranian and the Russian case, previously active firms are found to be much stronger affected by the sanctions, a 10.1 percentage point drop in the probability of serving the market

[40] An alternative explanation also in line with the empirical evidence, but not captured in our theoretical consideration, is that previously active firms face systematically different demand shocks after the introduction of sanctions than previously non-active firms.

[41] Note that this specification also highlights the importance of applying the analytical bias correction. Comparing the coefficients in Table 2 and Table 17 in appendix B, we find that the latter particularly overstate the entry-cost channel that is captured by the lagged dependent variable and its interactions. In fact, in the case of Russia, without bias correction, one would falsely conclude a significant impact of the entry-cost channel of this particular sanctions regime.

compared to 5.6 percentage points in Iran and 7.4 vs. 3.4 percentage points in Russia. At first sight, this appears to be at odds with the entry cost effect identified by the positive coefficient on the $y^{max}_{\omega j(t-12)} \times SANCT_{jt}$ interaction term. However, the first two rows consider the same group ATTs by previous activity in a specification that does not allow for heterogeneity in the sanctions effect along the lagged dependent variable and finds a very similar pattern: ATTs for previously active firms are much more clearly negative than for the non-active ones. This indicates that the difference does not stem from a sanctions effect on entry costs, but from the different positioning of these firms along the probability distribution. Firms that did not export in the previous year are unlikely to do so now. Their fitted probability is therefore close to zero and they hence lie in a very flat part of the cumulative distribution function (cdf). Previously active firms lie further to the right in the cdf and hence in a steeper part. Any given *identical* shock to the linear index will therefore on average translate into a larger change in the probability of exporting of the previously active firms. The idea of the calculation of an implied ATT difference according to Equation (17) is exactly to net out these differences in the positioning on the cdf. The result of applying this procedure is shown in the fifth row and is in line with the evidence of increased entry costs due to sanctions: We find that previous activity in the Iranian market softens the average sanctions impact by 1.6 percentage points, in line with the positive sign of the corresponding coefficient, because they do not have to pay the now increased entry costs. Comparing this to the overall ATT of 7.5 percentage points reported in Table 1 also suggests that about one fifth of the overall sanctions effect in Iran is accounted for by the entry cost effect. In line with the coefficient estimates, the implied ATT difference in the Russian case is very close to zero.

5.3 Firm characteristics

Exporting behaviour could also be heterogeneous along other characteristics. Large firms and multi-product firms may be more (or less) affected than the average firm, depending on the sanctions regime. As most sanctions schemes incorporate financial measures, firms predominantly exporting products relying on trade finance instruments may be more severely affected than others.[42] In terms of the exported products, other characteristics may also play an important role. As Western sanctions against the Russian Federation were met with an import embargo on food and agricultural products and, as reported in the media, an at least moderate boycott of Western products by Russian consumers, firms exporting consumer products were likely more adversely affected than others. Finally, some firms may — by coincidence or strategy — export to *other* markets that make them more or less resilient in the face of sanctions. Exporting to a country in the neighborhood of the sanctioned country, e.g., may allow them a relatively easy path to circumvent sanctions.[43] Still other firms may actually specialize in "tough" markets, allowing them to stay in the sanctioned market when otherwise comparable firms decide not to.

[42] Crozet and Hinz (2020), e.g., find the trade finance intensity of products to be an important driver of the decline in volume of exports of French goods to Russia, i.e. the intensive margin. Products that were in no other obvious way affected by policies put in place by either the European or Russian side, but use trade finance instruments intensively, were much more impacted than other comparable products.

[43] Note that the sanctions regimes against Russia were not UN mandated, and out of its neighbours only the EU members Finland, Estonia, Latvia, Lithuania, Poland, and Ukraine implemented sanctions. In the case of Iran, the measures *were* UN mandated, however trade with generally friendly neighboring countries such as Azerbaijan and Turkey continued to thrive (compare e.g. https://www.theatlantic.com/international/archive/2013/05/how-iran-benefits-from-an-illicit-gold-trade-with-turkey/275948/, accessed on June 29th, 2020).

Table 4: Total exports

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.284***	0.309***	0.251***	0.255***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	0.666***	-	-0.323***
	-	(0.093)	-	(0.05)
Sanctions$_{jt}$ ×	-0.068***	-0.070***	0.003	0.002
log total exports$_{\omega}$	(0.006)	(0.006)	(0.003)	(0.003)
Estimated Date-Partner FE$_{jt}$	-	0.290***	-	0.218***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	26,826,432	26,603,909	26,304,830	26,079,708

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5: Number of products

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.284***	0.309***	0.251***	0.255***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.186***	-	-0.315***
	-	(0.032)	-	(0.017)
Sanctions$_{jt}$ ×	-0.092***	-0.096***	0.008	0.007
log number products$_{\omega}$	(0.012)	(0.012)	(0.006)	(0.006)
Estimated Date-Partner FE$_{jt}$	-	0.290***	-	0.218***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	26,826,432	26,603,909	26,305,187	26,080,065

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

We investigate these potential sources of heterogeneity by estimating equations (11) and (12), interacting the sanctions indicator with the variable in question. The variables are generated using data from the year prior to the first year of the sample used in the estimation.[44]

5.3.1 Firm size and multi-product firms

We first explore how firm size and the number of products a firm exports affect the impact of sanctions. It is difficult to predict whether large exporters are likely to be hit harder than smaller ones. On the one hand, large firms may find it easier to pay the fixed costs of exporting and have the resources to adapt to the new legal environment. On the other hand, larger firms are more visible, need to preserve their reputation and are more likely to come under political pressure. We estimate specifications (11) and (12) interacting the sanctions indicator with the logarithm of the respective firms' annual total exports, as a proxy for firm size, and with the number of unique products at the HS6-level the firm has exported in the reference year.

The regression results displayed in Tables 4 and 5 paint similar pictures.[45] In the case of the Iran sanctions, larger firms were much more affected than smaller firms. The coefficient of interest, on the interaction of Sanctions$_{jt}$ $\times$ log total exports$_{\omega}$, is very similar in the three-way fixed effects model (column 1) and two-way fixed effects model (column 2), controlling with an estimated fixed effects from an intensive margin regression. The implied difference in the average treatment effect between those firms that export more than the mean and those that do so less is a marked 5.7 percentage points. Hence, larger firms were much more likely to leave and/or not to enter the Iranian market due to the imposition of the sanctions than smaller firms. Interestingly, in the case of the Russia sanctions there is no significant difference in the firms' behaviour. The coefficients in both regressions are close to zero and insignificant. Large and small firms exhibited a very similar behaviour with respect to leaving or staying, or entering the Russian market after the sanctions were imposed in mid 2014.

The message is reinforced by results from the specification interacting Sanctions$_{jt}$ $\times$ log number of products$_{\omega}$. Firms with a larger number of unique products in their export portfolio were less likely to export to Iran after January 2012. The estimated effect is again remarkably similar in the two-way and three-way fixed effects regressions, implying an average treatment effect 3 percentage points stronger for firms exporting more than the average number of products. As in the previous regression, the results in Table 5 show that in the case of Russia larger firms did not behave any differently.

Interestingly, these results from the extensive margin mirror those found for the intensive margin, as reported in Crozet and Hinz (2020). In the case of French firms exporting to Russia, large firms are reported to perform no differently than small firms in terms of the *value* exported, given they do export. At the same time the results stand somewhat in contrast to Haidar (2017)'s findings for Iranian firms, who, essentially looking at the issue from the other side, reports that smaller exporting firms were harder hit than larger firms. Importantly, however, this dissonance may be due to firms either being from the target or the sender country. Big Iranian firms may have the resources to circumvent the sanctions and nothing to lose in terms of reputation or political support. This is likely not true for Western firms.

[44] E.g., for the case of Iran, the sample spans January 2010 to January 2014, hence we take firm-level trade data from 2009 to construct the mean and shares used in the analysis below. We report summary statistics and display histograms of the interacted variables in Appendix A.

[45] ATTs and the ATT differences across specifications for all heterogeneous sanctions effects are reported in the appendix in Tables 18ff.

Table 6: Trade finance intensity

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.284***	0.309***	0.251***	0.255***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.462***	-	-0.314***
	-	(0.021)	-	(0.011)
Sanctions$_{jt}$ ×	-0.370**	-0.350**	-0.177*	-0.172*
mean trade finance intensity$_{\omega}$	(0.177)	(0.177)	(0.102)	(0.102)
Estimated Date-Partner FE$_{jt}$	-	0.290***	-	0.218***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	26,826,432	26,603,909	26,305,187	26,080,065

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.3.2 Trade finance intensity

One sanctions instrument that is styled as surgical yet severe are financial sanctions (compare e.g. Drezner, 2015; Ahn and Ludema, 2020). In the past decade, their use has proliferated, with the sanctions regimes against Russia and Iran being the most prominent cases. In the case of Iran, as described above, measures were particularly severe, largely cutting the country off from existing financial links, and effectively branding the country a no-go area. In the case of Russia, financial sanctions were designed *not* to directly impede trade flows, yet they may have altered the supply of trade finance services. In any case, firms seeking to export to sanctioned countries may have experienced difficulties in securing their shipments and payments because Western banks have refrained from interacting with counterparts targeted by sanctions and/or have been reluctant to insure international transactions in an economically and legally unstable environment. We hence now investigate whether those firms exporting products relying heavily on trade finance instruments are any different in their exporting behaviour under sanctions than those that do not.

As in Crozet and Hinz (2020), we rely on an indicator that describes the *product*-level intensity of the use of trade finance instruments, letters-of-credit in particular.[46] We therefore estimate equations (11) and (12) interacting the sanctions indicator with the respective firms' mean trade insurance intensity.

The results in Table 6 paint a clear picture: In both sanctions regimes, Russia and Iran, where significant financial sanctions are imposed, firms that rely on trade finance instruments more heavily than otherwise comparable firms are much more affected. As before, the coefficient on the interaction term is very similar in magnitude (and standard errors) in both specifications. For Iran, the coefficient is about twice as high as the one for Russia. The implied difference between the two groups relying either more or less on trade insurance than the average is 0.6 percentage

[46] We use the indicator constructed by Crozet, Demir, and Javorcik (2020).

Table 7: External financial dependence

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.284***	0.309***	0.251***	0.255***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.433***	-	-0.298***
	-	(0.012)	-	(0.007)
Sanctions$_{jt}$ $\times$	0.074*	0.074*	0.066***	0.064***
mean external finance dependence$_{\omega}$	(0.039)	(0.039)	(0.025)	(0.025)
Estimated Date-Partner FE$_{jt}$	-	0.290***	-	0.218***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	26,826,432	26,603,909	26,305,187	26,080,065

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ $\times$ Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

points in the case of Iran, and 0.3 percentage points in the case of Russia. As one would expect, the impact is hence stronger for the former case, where sanctions effectively cut off the country from the international monetary system, and therefore likely strongly disincentivized banks to involve themselves in transactions with the targeted country.

It could be, however, that these results merely reflect product characteristics associated with generally higher financial dependence (see e.g. Manova, 2008; Chan and Manova, 2015), rather than unmet needs for insurance through trade finance instruments. We therefore repeat the exercise, now interacting the sanctions indicator with the measure of external financial dependence from Kroszner et al. (2007). The results are displayed in Table 7. It turns out the worry is not warranted, as in fact the coefficient of interest in both sanctions cases in question are both *positive* and significant. This confirms that financial sanctions on Iran and Russia have in fact no impact on how French companies finance their production of goods, at least as long as these are not specifically designed for use tangential to sanctions, as e.g. certain oil drilling equipment for Arctic conditions that currently requires a special export license for transactions to Russia.[47] Thus, the positive coefficients may simply reflect that certain industries that continued to be in demand from Russia and to a lesser extent Iran, as e.g. machinery and heavy industry products, are products with particularly high need for external financing. Hence, results from Table 7 do not run counter to those presented in Table 6.

Overall it appears that financial sanctions are an important driver of the heterogeneity in the impact of sanctions on the extensive margin. Mirroring results for the intensive margin of trade,

[47] See the EU's Council Regulation No 833/2014 from 31 July 2014 that specifies certain 6 and 8 digit HS codes for which an export licence is "required to export, sell, supply or transfer goods suited to certain oil exploration and production projects to Russia or to other destinations where they are ultimately for use in Russia. A licence will not be granted if the items are intended for use in deep water oil exploration and production, arctic oil exploration and production, or shale oil projects in Russia." (https://www.gov.uk/guidance/trade-sanctions-on-russia, accessed on June 29th, 2020).

Table 8: Consumer goods

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.284***	0.309***	0.251***	0.255***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.479***	-	-0.209***
	-	(0.015)	-	(0.009)
Sanctions$_{jt}$ ×	0.216***	0.211***	-0.249***	-0.245***
share consumer goods$_{\omega}$	(0.032)	(0.032)	(0.016)	(0.016)
Estimated Date-Partner FE$_{jt}$	-	0.290***	-	0.218***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	26,826,432	26,603,909	26,305,187	26,080,065

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 9: Intermediate goods

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.284***	0.309***	0.251***	0.255***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.397***	-	-0.363***
	-	(0.017)	-	(0.009)
Sanctions$_{jt}$ ×	-0.085***	-0.083***	0.201***	0.195***
share intermediate goods$_{\omega}$	(0.029)	(0.029)	(0.017)	(0.017)
Estimated Date-Partner FE$_{jt}$	-	0.290***	-	0.218***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	26,826,432	26,603,909	26,305,187	26,080,065

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

firms relying on trade finance instruments more adjust their exporting behaviour more than those that do not.

5.3.3 Product mix

As we describe above, each sanctions case is to some degree unique. In the case of the Russia sanctions, e.g., the country reacted against Western sanctions by imposing an embargo on food and agricultural products. At the same time, the media reported that at least some Russian consumers were staging a general boycott against Western consumer products.

Such idiosyncracies may therefore also matter in the ways certain types of *products* are more affected than others. As we study the exporting behaviour of firms, we now construct two indicators that reflect the product mix of firms, denoting the share of consumer products and intermediate products in the respective firms' export portfolio. We then re-estimate equations (11) and (12) interacting the sanctions indicator with these variables.

Table 8 shows the results for the interaction of the sanctions indicator with the variable denoting the share of consumer goods in a firm's export portfolio. The coefficient on the interaction with the variable of interest, Sanctions$_{jt}$ $\times$ share consumer goods$_{\omega}$, is significantly negative for Russia in both specifications. The implied difference between the average treatment effect for groups with above and below mean share of consumer products is -3.7 percentage points (-4.2 for low share of consumer product vs. -7.9 for high share of consumer products), and hence a sizeable driver of the overall effect.[48] Interestingly, the coefficient is positive and significant for the case of Iran. While this may look counter-intuitive at first sight, two possible explanations come to mind: First, consumer products really may have been much less affected due to ongoing strong demand for these goods from Iranian consumers. There is a plethora of anecdotal evidence about Western consumer products finding their way to Iranian consumers, even in the form of knock-off stores and so-called front men selling legitimate products.[49] Second, as the interacted variable denotes a share in the firms' product portfolio, other types of exported goods may just be even more severely affected by the sanctions. One type of product that is likely to have suffered more severely are intermediate products. French companies are historically comparatively active in Iran, with prominent car makers Renault and Peugeot, food producer Danone, and energy company Total operating own plants and joint ventures in the country, likely requiring inputs from abroad.[50]

Table 9 shows the results for estimating equations (11) and (12) with an interaction of the sanctions indicator with the firms' share of intermediate goods in their product portfolio. As hypothesized, firms exporting intermediate goods to Iran adjusted their behaviour more than otherwise comparable firms. The estimated coefficients are again very similar in the two specifications. The difference in the average treatment effect reflects the negative coefficient: firms with a higher than average share of intermediate products in their product mix have a 1.1 percentage point stronger reaction to the the sanctions than those below. The results for Russia indicate a similar mechanism described above, only in reverse. As firms predominantly exporting consumer products were more severely hit by the sanctions than others, those with a higher share of intermediate products are less severely hit by sanctions.

[48] Compare Table 22 in Appendix C. As the baseline ATTs are statistically indistinguishable for both groups, we can in this case directly compare the absolute ATTs, as the last term from equation 17 drops out.

[49] Compare e.g. https://www.economist.com/middle-east-and-africa/2015/03/05/fading-hope, https://www.nytimes.com/2016/04/05/fashion/iran-fashion-retail.html, https://www.lensculture.com/articles/thomas-cristofoletti-iran-s-booming-consumer-culture and https://ec.europa.eu/chafea/agri/sites/chafea/files/market-entry-handbook-iran_en.pdf (all accessed on June 29th, 2020).

[50] Compare e.g. https://www.bbc.com/news/business-40850181, https://www.wsj.com/articles/western-companies-see-potential-in-reaching-irans-consumers-after-nuclear-deal-1428313107?cb=logged0.5079263212005186 and https://www.gao.gov/new.items/d10515r.pdf (all accessed on June 29th, 2020).

Table 10: Exposure crisis countries

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.278***	0.303***	0.245***	0.249***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.403***	-	-0.320***
	-	(0.026)	-	(0.01)
Sanctions$_{jt}$ ×	-0.029	-0.039	0.044***	0.046***
exposure crisis country$_{\omega}$	(0.03)	(0.03)	(0.014)	(0.014)
Estimated Date-Partner FE$_{jt}$	-	0.288***	-	0.219***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	27,396,537	27,168,057	27,078,704	26,846,577

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.3.4 Specialized firms

Finally, as hypothesized above, certain firms may be — by strategy or coincidence — in a better situation to weather sanctions than others. Some firms may be specialized in "tough" markets, in terms of political stability and legal uncertainty. As an indicator for such countries we turn to the official advice by the French foreign ministry as to which countries have been given a "travel warning".[51] Firms with experience in these countries may be more used to operating in difficult environments and hence be able to handle the sanctions with lower additional costs than other firms.

Table 10 displays the results for estimating equations (11) and (12) with an interaction with an indicator variable for whether the firm exported to any of the "crisis countries". For Russia, we find indeed significant evidence that firms with experience in such tough markets are less affected by the sanctions. The corresponding difference in the average treatment effect between those firms that are present in another crisis country and those that are not is 0.8 percentage points. The coefficients are insignificant for the case of Iran.

Furthermore, some firms may be able to circumvent the sanctions by exporting via a third country rather than directly to the sanctioned country itself. This sanctions avoidance channel should be easier to use for firms that are also active in neighboring countries of the sanctioned country through which they can enter the final destination market.

In Table 11, we test this channel by including an interaction term between our sanctions variable and an indicator variable that is equal to one if firms have also been active in at least one neigh-

[51] See `https://www.diplomatie.gouv.fr/fr/conseils-aux-voyageurs/` (accessed on October 7th, 2019). The countries where in whole travels are adviced against — "formellement déconseillé" or "déconseillé sauf raison impérative" — are Haiti, Guatemala, Venezuela, Honduras, Mali, Libya, Chad, Niger, Central African Republic, Sudan, Democratic Republic of the Congo, Somalia, Yemen, Syria, Iraq, Afghanistan, Pakistan, Bangladesh, and North Korea.

Table 11: Exposure neighboring country

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.278***	0.303***	0.245***	0.249***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.347***	-	-0.236***
	-	(0.041)	-	(0.015)
Sanctions$_{jt}$ ×	-0.076*	-0.095**	-0.075***	-0.078***
exposure neighbor$_{\omega}$	(0.043)	(0.043)	(0.017)	(0.017)
Estimated Date-Partner FE$_{jt}$	-	0.288***	-	0.219***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	27,396,537	27,168,057	27,078,704	26,846,577

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

boring country, or those that are historically or geographically closely-related to the sanctioned countries.[52] Indeed, for both Iran and Russia we find significantly stronger sanctions effects for firms that also serve neighbouring countries. The difference in average treatment effects between the two groups of firms are -1.3 and -1.2 percentage points, respectively. As there is no reason, why these firms should actually be hit more severely, we interpret this as suggestive evidence that they are less likely to export to the sanctioned country not because they actually are less likely to serve the market, but because they do so indirectly.

6 Estimation results for Myanmar and Cuba

We now turn to the estimation results for the two cases where sanctions are *lifted*, i.e. Myanmar and Cuba. The results should be interpreted with caution, especially in the case of Cuba, where no formal bilateral sanctions were actually in place (or lifted) between France and Cuba. Thus, what we are seeking to assess in the case of Cuba is whether the United States' extraterritorial sanctions have affected the behavior of French exporters. We again estimate the baseline specifications (4) and (5), to get an overall picture of the respective impact of the two different sanctions episodes.

[52] In the case of Iran we take Armenia, Azerbaijan, Turkmenistan, Afghanistan, Pakistan, Iraq and Turkey. For Russia, we take former countries of the Soviet Union, i.e. Armenia, Azerbaijan, Belarus, Estonia, Georgia, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Moldova, Tajikistan, Turkmenistan, Ukraine and Uzbekistan. For Myanmar, we take India, Bangladesh, Thailand and Laos. And for Cuba, we take other Caribbean countries, namely Anguilla, Antigua and Barbuda, Aruba, Bahamas, Barbados, British Virgin Islands, Cayman Islands, Curacao, Dominica, Dominican Republic, Grenada, Haiti, Jamaica, Martinique, Montserrat, Puerto Rico, Saint Barthelemy, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadines, Sint Maarten, Trinidad and Tobago, Turks and Caicos Islands, and the United States Virgin Islands. In the cases of Iran and Russia, some neighboring countries are presumably not an ideal conduit country, because of bilateral hostilities. However, in both cases some other neighbouring countries are either neutral or allies.

Table 12: Baseline specification for Myanmar and Cuba

	Dependent variable: $y_{\omega jt}$			
	Myanmar		Cuba	
	(1)	(2)	(3)	(4)
Active previous year	0.206***	0.297***	0.169***	0.246***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.089***	-	0.033
	-	(0.033)	-	(0.031)
Estimated Date-Partner FE$_{jt}$	-	0.253***	-	0.217***
	-	(0.003)	-	(0.003)
Overall ATTs of sanctions	-	-0.011***	-	0.005*
	-	(0.002)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	27,640,157	27,128,620	27,499,973	26,582,707

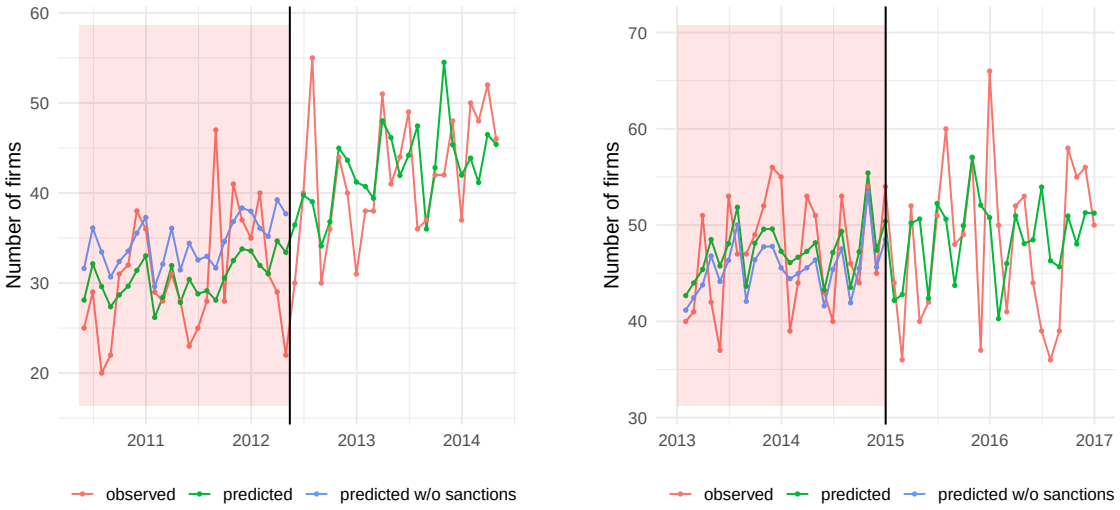
Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ $\times$ Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

We report the baseline results in Table 12. As before columns (1) and (3) display the three-way fixed effects results. As discussed in Section 3, the sanctions effect is not identified in this case and there is hence only the coefficient of the (extended) lagged dependent variable to report. In both cases, it is highly statistically significant, indicating strong true state dependence, or — in terms of the model — the existence of large entry costs.

Columns (2) and (4) show the results of the two-way fixed effects specification. Again, previous export activity significantly increases the probability to serve a market. The corresponding coefficients actually are somewhat larger than in the three-way specification. As before, the two-way specification now also allows the identification of the sanctions effect. We see in column (4) that the (temporary) relaxing of the U.S. sanctions against Cuba had no impact on the likelihood of exporting. There are three possible interpretations of this absence of result. Either the extraterritorial measures imposed by the United States did not constitute a sufficiently strong constraint (or threat) to affect the decisions of French companies. Or the dismantling of sanctions has been too slow and too uncertain for Western exporters to invest in the search for business opportunities in Cuba. Alternatively, business relations between France and Cuba have fallen to such a low level of intensity (after decades of U.S. sanctions and centralized management of the Cuban economy) that restoring strong commercial relations is a long and complicated process. By contrast, in the case of Myanmar, the significantly negative coefficient (-0.089) indicates that the lifting of the sanctions did lead to an upsurge in the number of active French exporters. The coefficient is however much smaller than the ones estimated in the case of Iran and Russia, suggesting either that the sanctions against Myanmar were much less stringent or that the lifting of sanctions does not produce trade recovery effects symmetrical to the trade destruction resulting from their implementation.

Figures 3a and 3b underline these results visually by plotting the actual number of active firms, and the sum of the predicted probabilities with and without sanctions.

As for the cases of Iran and Russia, we report in Table 13 the estimates for specifications (7)



(a) Myanmar sanctions

(b) Cuba sanctions

Figure 3: Evolution of observed, predicted and counterfactual number of firms

Table 13: Previous experience

	Dependent variable: $y_{\omega jt}$			
	Myanmar		Cuba	
	(1)	(2)	(3)	(4)
Active previous year	0.080***	0.171***	0.029***	0.115***
	(0.002)	(0.002)	(0.002)	(0.002)
— × Sanctions Period$_t$	0.256***	0.251***	0.281***	0.265***
	(0.002)	(0.002)	(0.002)	(0.002)
— × Sanctioned Country$_j$	-0.300***	-0.305***	-0.150***	-0.169***
	(0.053)	(0.053)	(0.056)	(0.055)
— × Sanctions$_{jt}$	0.296***	0.272***	0.009	0.018
	(0.075)	(0.073)	(0.07)	(0.07)
Sanctions$_{jt}$	-	-0.217***	-	0.052
	-	(0.055)	-	(0.057)
Estimated Date-Partner FE$_{jt}$	-	0.243***	-	0.226***
	-	(0.003)	-	(0.003)
Fixed effects	$\omega t, jt, \omega j$	$\omega t, \omega j$	$\omega t, jt, \omega j$	$\omega t, \omega j$
Sample size	27,640,157	27,128,620	27,499,973	26,582,707

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ × Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

and (8). These results confirm the absence of a significant impact for Cuba, and the significant increase in export probability to Myanmar after the lifting of sanctions. Mirroring our results for Iran and Russia, we observe that the end of the sanctions have benefited less to incumbent exporters than to new entrants. In a strict sense, the fact that the magnitude of the positive coefficient of the interaction of the lagged dependent variable and the sanctions variable is as large as the one of the negative sanctions coefficient (or in fact somewhat larger) implies the interpretation that the sanctions effect found for Myanmar is entirely driven by a higher entry

cost factor rather than by higher fixed or variable trade costs.

There are few sanctions episodes that can be used to analyze the impact of lifting sanctions. While all instances are unique and have their particularities, the Myanmar and Cuba cases offer a glimpse at possible outcomes of — at least temporarily — lifting sanctions. However, contrary to the cases of Russia and Iran where we investigate the effects of the imposition of sanctions, neither of the (temporary) lifting in the cases of Myanmar and Cuba appear to produce a quantitatively large impact. While this does not constitute hard evidence on the lack of an effect of lifting sanctions, it provides an indication these effects may be harder to attain than those of imposing sanctions.

7 Conclusion

Studying the cases of the Iran and Russia sanctions, we find that the introduction of sanctions significantly lowers the probability of French firms to export to these countries. The two episodes of lifting sanctions that we have studied do not lead to symmetrical conclusions. The end of sanctions against Myanmar resulted in a significant increase in the probability of exporting to this country, although much smaller in magnitude than the effect of imposing sanctions, as estimated for the cases of Russia and Iran. For Cuba, the temporary easing of U.S. sanctions does not appear to have led to an increase in the presence of French exporters on this market. This is not really surprising since we are studying the case of an indirect effect: France did not impose any sanctions against Cuba and only the extraterritoriality provisions of the U.S. sanctions were potentially able to curb French trade with Cuba.

Our second take-away is that financial sanctions, as imposed in both the Iran and Russia cases, impact trade through the firm-level extensive margin. Firms that rely on trade finance instruments reduce their activity on these markets significantly stronger than otherwise comparable ones. This result complements related findings for the intensive margin sanctions impact of those products using trade finance instruments intensively.

Third, there is some indicative evidence for trade deflection or sanctions avoidance. Firms that were previously active in neighbouring countries of the sanctioned country are less likely to serve the sanctioned country when the sanctions hit, suggesting that they take an indirect route instead.

As expected when analyzing very different policy settings, we find various sources of heterogeneity in the impact of sanctions on firms' exporting behaviour, along the lines of idiosyncracies and particularities of the respective sanctions case. This is interesting in its own regard, since while certain sanctions measures are styled as surgical and precise, it suggests that it is hard to have reliable regularities allowing to predict who will be hit the most. The design of the sanctions, pre-existing trade relationships, the counter party's policy response and the public's reaction in fact matter a lot.

Sanctions are policy tools that should be used cautiously: They have strong effects on trade, on its intensive and extensive margin, and it is generally hard to know who will be hit, how and for

how long.

References

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

Albornoz, F., H. F. Calvo Pardo, G. Corcos, and E. Ornelas (2012). Sequential exporting. *Journal of International Economics 88*(1), 17–31.

Bergeijk, P. A. V. (2009). Economic diplomacy and the geography of international trade.

Berman, N., V. Rebeyrol, and V. Vicard (2019). Demand learning and firm dynamics: Evidence from exporters. *Review of Economics and Statistics 101*(1), 91–106.

Bernard, A. B. and J. B. Jensen (2004, May). Why Some Firms Export. *The Review of Economics and Statistics 86*(2), 561–569.

Blum, B. S., S. Claro, and I. J. Horstmann (2013). Occasional and perennial exporters. *Journal of International Economics 90*(1), 65–74.

Békés, G., L. Fontagné, B. Muraközy, and V. Vicard (2017). Shipment frequency of exporters and demand uncertainty. *Review of World Economics 153*(4), 779—807.

Cadot, O., L. Iacovone, M. D. Pierola, and F. Rauch (2013). Success and failure of african exporters. *Journal of Development Economics 101*, 284–296.

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

Chan, J. M. and K. Manova (2015). Financial development and the choice of trade partners. *Journal of Development Economics 116*, 122–145.

Chaney, T. (2008). Distorted Gravity: The Intensive and Extensive Margins of International Trade. *American Economic Review 98*(4), 1707–1721.

Cheptea, A. and C. Gaigné (2019, 09). Russian food embargo and the lost trade. *European Review of Agricultural Economics 47*(2), 684–718.

Crozet, M., B. Demir, and B. Javorcik (2020). Does trade insurance matter?

Crozet, M., K. Head, and T. Mayer (2012). Quality Sorting and Trade: Firm-level Evidence for French Wine. *The Review of Economic Studies 79*(2), 609–644.

Crozet, M. and J. Hinz (2020). Friendly Fire: The Trade Impact of the Russia Sanctions and Counter-Sanctions. *Economic Policy 35(101)*, 97–146.

Dickstein, M. J. and E. Morales (2018). What do exporters know? *The Quarterly Journal of Economics 133*(4), 1753—-1801.

Dreyer, I., J. Luengo-Cabrera, S. Bazoobandi, T. Biersteker, R. Connolly, F. Giumelli, C. Portela, S. Secrieru, P. Seeberg, and P. A. van Bergeijk (2015). *On Target?: EU Sanctions as Security Policy Tools*. European Union Institute for Security Studies.

Drezner, D. W. (2015). Targeted sanctions in a world of global finance. *International Interactions 41*(4), 755–764.

Eaton, J., M. Eslava, D. Jinkins, C. Krizan, and J. Tybout (2015). A Search and Learning Model of Export Dynamics. 2015 Meeting Papers 1535, Society for Economic Dynamics.

Fernández-Val, I. (2009). Fixed Effects Estimation of Structural Parameters and Marginal Effects in Panel Probit Models. *Journal of Econometrics 150*(1), 71–85.

Fernández-Val, I. and M. Weidner (2018). Fixed Effects Estimation of Large-T Panel Data Models. *Annual Review of Economics 10*(1), 109–138.

Frankel, J. A. (1982). The 1807–1809 Embargo Against Great Britain. *The Journal of Economic History 42*(02), 291–308.

Fuchs, A. and N.-H. Klann (2013). Paying a visit: The Dalai Lama effect on international trade. *Journal of International Economics 91*(1), 164–177.

Gordon, J. (2016). Economic sanctions as 'Negative Development': the case of Cuba. *Journal of International Development 28*(4), 473–484.

Gullstrand, J. (2020). What goes around comes around: The effects of sanctions on swedish firms in the wake of the ukraine crisis. Technical report, Lund University.

Haidar, J. I. (2017). Sanctions and export deflection: evidence from iran. *Economic Policy 32*(90), 319–35.

Head, K. and T. Mayer (2014). Gravity Equations: Workhorse, Toolkit, and Cookbook. In G. Gopinath, E. Helpman, and K. Rogoff (Eds.), *Handbook of International Economics* (4 ed.), Volume 4, Chapter 3, pp. 131–195. North Holland.

Heilmann, K. (2016). Does political conflict hurt trade? Evidence from consumer boycotts. *Journal of International Economics 99*(C), 179–191.

Helpman, E. and O. Itskhoki (2010). Labour Market Rigidities, Trade and Unemployment. *Review of Economic Studies 77*(3), 1100–1137.

Hinz, J. (2019). Cost of sanctions: Estimating lost trade with gravity. In *Disrupted Economic Relationships: Disasters, Sanctions, Dissolutions*. CESifo Seminar Series at MIT Press, Volker Nitsch and Tibor Besedeš eds. Ch. 3.

Hinz, J. and E. Monastyrenko (2019). Bearing the cost of politics: Consumer prices and welfare in russia. Technical report, Kiel Working Papers.

Hinz, J., A. Stammann, and J. Wanner (2020). State Dependence and Unobserved Heterogeneity in the Extensive Margin of Trade. Technical report, ArXiV.

Hufbauer, G. C., J. J. Schott, and K. A. Elliott (2009). *Economic Sanctions Reconsidered, 3rd Edition (paper)*. Number 4129 in Peterson Institute Press: All Books. Peterson Institute for International Economics.

Hufbauer, G. C., J. J. Schott, K. A. Elliott, and B. Oegg (2008). Speeches & Papers, Peterson Institute for International Economics - Case Studies in Economic Sanctions and Terrorism - Burma (Myanmar) Case: 88-1.

Iacovone, L. and B. S. Javorcik (2010). Multi-product exporters: Product churning, uncertainty and export discoveries. *Economic Journal 120*(544), 481—-499.

Irwin, D. A. (2005). The Welfare Cost of Autarky: Evidence from the Jeffersonian Trade Embargo, 1807-09. *Review of International Economics 13*(4), 631–645.

Kroszner, R. S., L. Laeven, and D. Klingebiel (2007). Banking crises, financial dependence, and growth. *Journal of financial Economics 84*(1), 187–228.

LeoGrande, W. M. (2016). Chipping away at the embargo: President obama and the u.s. economic sanctions against cuba. In *Cuba-US relations: Normalization and its challenges*. Institute of Latin American Studies, Margaret E. Crahan and Soraya M. Castro Mariño eds.

Manova, K. (2008). Credit constraints, equity market liberalizations and international trade. *Journal of International Economics 76*(1), 33–47.

Mejean, I., J. Martin, and M. Parenti (2019). Relationship specificity: Measurement and applications to trade. Unpublished Working Paper.

Melitz, M. J. (2003). The Impact of Trade on Intra-Industry Reallocations and Aggregate Industry Productivity. *Econometrica 71*(6), 1695–1725.

Michaels, G. and X. Zhi (2010). Freedom fries. *American Journal of Applied Economics 2*(3), 256–281.

Neyman, J. and E. L. Scott (1948). Consistent Estimates Based on Partially Consistent Observations. *Econometrica 16*(1), 1–32.

O'Rourke, K. H. (2007). War and welfare: Britain, france, and the united states 1807-14. *Oxford Economic Papers 59*, i8–i30.

Pandya, S. S. and R. Venkatesan (2016). French roast: Consumer response to international conflict - evidence from supermarket scanner data. *The Review of Economics and Statistics 98*(1), 42–56.

Piveteau, P. (2019). An empirical dynamic model of trade with consumer accumulation. Unpublished Working Paper.

Roberts, M. J. and J. R. Tybout (1997). The Decision to Export in Colombia: An Empirical Model of Entry with Sunk Costs. *American Economic Review 87*(4), 545–564.

Ruhl, K. J. and J. L. Willis (2017). New Exporter Dynamics. *International Economic Review 58*(3), 703–726.

Sager, E. and O. A. Timoshenko (2019). Demand uncertainty, selection, and trade. Unpublished Working Paper.

Stammann, A. (2018). Fast and feasible estimation of generalized linear models with high-dimensional k-way fixed effects. Unpublished Working Paper.

Timoshenko, O. A. (2015). Learning versus sunk costs explanations of export persistence. *European Economic Review 79*, 113–128.

A Summary statistics

Table 14: Summary statistics for case-specific sample of the customs data set

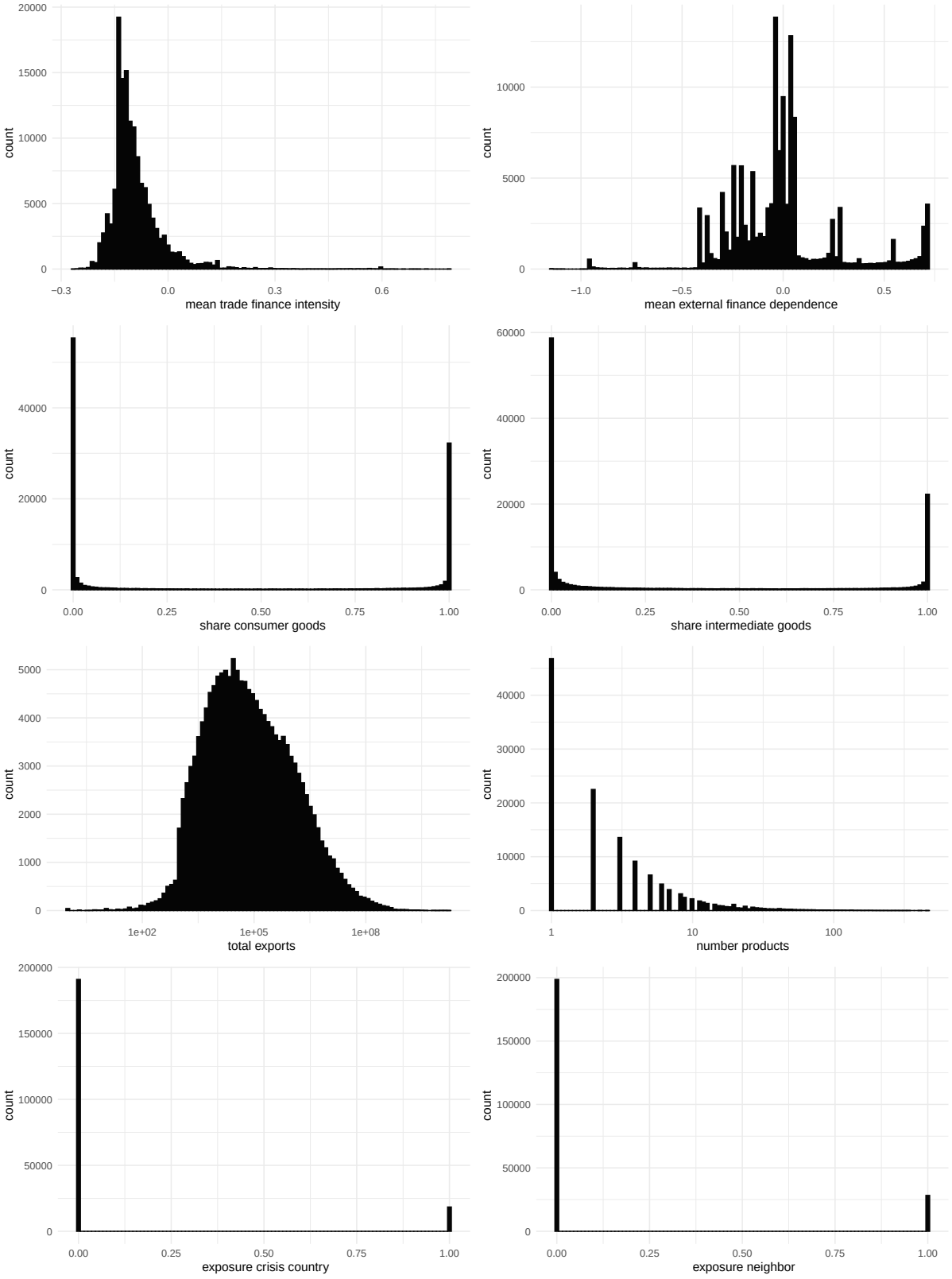
	Cuba	Iran	Myanmar	Russia
Number of observations	34,664,323	35,070,266	35,041,843	34,941,967
Number of destinations	223	222	222	222
Mean number of destinations per firm	39	38	39	40
Number of firms in sample	156,992	153,035	151,881	157,400
Number of firms active in case country	528	2,877	438	9,912
— during sanctions	344	1,628	219	6,235

Table 15: Summary statistics for variables of interest

Variable	Mean	Std. dev.	Median	Max	Min
log total exports	11.38	2.71	11.18	23.56	0.00
log number products	1.02	1.07	0.69	6.14	0.00
mean trade finance intensity	-0.09	0.08	-0.11	0.79	-0.26
mean external finance dependence	-0.01	0.27	-0.02	0.72	-1.14
share consumer goods	0.38	0.46	0.00	1.00	0.00
share intermediate goods	0.31	0.42	0.00	1.00	0.00
exposure crisis country	0.09	0.28	0.00	1.00	0.00
exposure neighbor	0.08	0.27	0.00	1.00	0.00

Note: Figures for all firms present in dataset in 2013.

Figure 4: Histograms of variables of interest



B Uncorrected coefficients

Table 16: Uncorrected baseline specification

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.239***	0.267***	0.196***	0.208***
	(0.001)	(0.001)	(0.001)	(0.001)
Sanctions$_{jt}$	-	-0.475***	-	-0.329***
	-	(0.012)	-	(0.007)
Estimated Date-Partner FE$_{jt}$	-	0.320***	-	0.242***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	27,396,537	27,168,057	27,785,280	26,846,577

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ $\times$ Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 17: Uncorrected lag specification

	Dependent variable: $y_{\omega jt}$			
	Iran		Russia	
	(1)	(2)	(3)	(4)
Active previous year	0.387***	0.414***	0.360***	0.368***
	(0.002)	(0.002)	(0.002)	(0.002)
— $\times$ Sanctions Period$_t$	-0.291***	-0.295***	-0.329***	-0.321***
	(0.002)	(0.002)	(0.002)	(0.002)
— $\times$ Sanctioned Country$_j$	-0.110***	-0.130***	0.062***	0.069***
	(0.023)	(0.023)	(0.014)	(0.014)
— $\times$ Sanctions$_{jt}$	0.161***	0.181***	0.060***	0.058***
	(0.032)	(0.031)	(0.018)	(0.018)
Sanctions$_{jt}$	-	-0.631***	-	-0.382***
	-	(0.027)	-	(0.016)
Estimated Date-Partner FE$_{jt}$	-	0.309***	-	0.248***
	-	(0.003)	-	(0.003)
Fixed effects	ωt, $jt, \omega j$	ωt, ωj	ωt, $jt, \omega j$	ωt, ωj
Sample size	27,396,537	27,168,057	27,785,280	26,846,577

Notes: Sanctions$_{jt}$ = Sanctions Period$_t$ $\times$ Sanctioned Country$_j$. Sample includes two years pre and post treatment periods, respectively. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

C ATTs for Imposition of Sanctions

Table 18: Total exports

	Iran	Russia
Low log total exports	-0.077***	-0.049***
(*baseline*)	(0.003)	(0.001)
High log total exports	-0.074***	-0.057***
(*baseline*)	(0.001)	(0.001)
Low log total exports	-0.022***	-0.049***
(*with heterogeneity*)	(0.001)	(0.001)
High log total exports	-0.076***	-0.058***
(*with heterogeneity*)	(0.001)	(0.001)
Implied ATT difference between high and low log total exports groups	-5.7 p.p.	-0.2 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 19: Number of products

	Iran	Russia
Low log number products	-0.072***	-0.052***
(*baseline*)	(0.001)	(0.000)
High log number products	-0.075***	-0.057***
(*baseline*)	(0.001)	(0.001)
Low log number products	-0.045***	-0.055***
(*with heterogeneity*)	(0.001)	(0.001)
High log number products	-0.079***	-0.057***
(*with heterogeneity*)	(0.001)	(0.001)
Implied ATT difference between high and low log number products groups	-3 p.p.	0.3 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 20: Trade finance intensity

	Iran	Russia
Low mean trade finance intensity	-0.079***	-0.056***
(*baseline*)	(0.001)	(0.001)
High mean trade finance intensity	-0.067***	-0.057***
(*baseline*)	(0.000)	(0.000)
Low mean trade finance intensity	-0.076***	-0.055***
(*with heterogeneity*)	(0.001)	(0.001)
High mean trade finance intensity	-0.070***	-0.059***
(*with heterogeneity*)	(0.001)	(0.001)
Implied ATT difference between high and low mean trade finance intensity groups	-0.6 p.p.	-0.3 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 21: Financial dependence

	Iran	Russia
Low mean external finance dependence	-0.074***	-0.057***
(*baseline*)	(0.001)	(0.001)
High mean external finance dependence	-0.075***	-0.056***
(*baseline*)	(0.001)	(0.000)
Low mean external finance dependence	-0.075***	-0.059***
(*with heterogeneity*)	(0.001)	(0.001)
High mean external finance dependence	-0.071***	-0.053***
(*with heterogeneity*)	(0.001)	(0.001)
Implied ATT difference between high and low mean external finance dependence groups	0.6 p.p.	0.5 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 22: Consumer goods

	Iran	Russia
Low share consumer goods	-0.075***	-0.056***
(*baseline*)	(0.001)	(0.001)
High share consumer goods	-0.072***	-0.056***
(*baseline*)	(0.001)	(0.000)
Low share consumer goods	-0.081***	-0.042***
(*with heterogeneity*)	(0.001)	(0.001)
High share consumer goods	-0.051***	-0.079***
(*with heterogeneity*)	(0.001)	(0.001)
Implied ATT difference between high and low share consumer goods groups	2.7 p.p.	-3.7 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 23: Intermediate goods

	Iran	Russia
Low share intermediate goods	-0.073***	-0.056***
(*baseline*)	(0.001)	(0.001)
High share intermediate goods	-0.077***	-0.057***
(*baseline*)	(0.001)	(0.000)
Low share intermediate goods	-0.068***	-0.066***
(*with heterogeneity*)	(0.001)	(0.001)
High share intermediate goods	-0.082***	-0.039***
(*with heterogeneity*)	(0.001)	(0.001)
Implied ATT difference between high and low share intermediate goods groups	-1.1 p.p.	2.8 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 24: Exposure crisis countries

	Iran	Russia
Low exposure crisis country	-0.067***	-0.053***
(*baseline*)	(0.001)	(0.000)
High exposure crisis country	-0.078***	-0.060***
(*baseline*)	(0.001)	(0.001)
Low exposure crisis country	-0.063***	-0.057***
(*with heterogeneity*)	(0.001)	(0.001)
High exposure crisis country	-0.079***	-0.056***
(*with heterogeneity*)	(0.002)	(0.001)
Implied ATT difference between high and low exposure crisis country groups	-0.6 p.p.	0.8 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 25: Exposure neighboring countries

	Iran	Russia
Low exposure neighbor	-0.062***	-0.048***
(*baseline*)	(0.001)	(0.000)
High exposure neighbor	-0.077***	-0.060***
(*baseline*)	(0.001)	(0.001)
Low exposure neighbor	-0.051***	-0.039***
(*with heterogeneity*)	(0.001)	(0.001)
High exposure neighbor	-0.078***	-0.063***
(*with heterogeneity*)	(0.003)	(0.001)
Implied ATT difference between high and low exposure neighbor groups	-1.3 p.p.	-1.2 p.p.

Notes: ATT difference based on equation (17), baseline refers to equation (14), heterogeneity refers to equation (16). Estimates are bias-corrected using the analytical correction proposed by Hinz, Stammann, and Wanner (2020). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.