Sanctions' Impact on Elections: The Russian Case

Robert Gold,[*] Julian Hinz,[†] and Michele Valsecchi[‡]

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Abstract

Do economic sanctions affect internal support of sanctioned countries' governments? To answer this question, we focus on the sanctions imposed on Russia in 2014 and identify their effect on voting behavior in both presidential and parliamentary elections. On the economic side, the sanctions significantly hurt Russia's foreign trade, but with regional-level variation. We use trade losses caused by the sanctions as measure for regional sanction exposure. For identification, we rely on a structural gravity model that allows us to compare observed trade flows to counterfactual flows in the absence of sanctions. DiD-estimations reveal that regime support significantly *increases* in response to the sanctions, at the expense of voting support of Communist parties. For the average Russian district, sanction exposure increases the vote share gained by president Putin and his party by 13 percent. Event studies and placebo estimations confirm the validity of our results.

Keywords: Economic sanctions, voting behavior, gravity estimation, rally-around-the-flag
JEL Classification: F12, F14, F15

[*] Kiel Institute for the World Economy and CESifo
[†] Bielefeld University and Kiel Institute for the World Economy
[‡] New Economic School (NES)

1 Introduction

Do sanctions influence regime support in targeted countries? Unfortunately, we do not know. While the economic consequences of sanctions are comparatively well understood, both for sanctioned countries (Haider, 2017; Dreger et al., 2016; Ahn and Ludema, 2020) and for sanctioning countries (Besedeš et al., 2017; Crozet and Hinz, 2017), we still lack quantitative evidence on economic sanctions' political impacts. This is unsatisfying, since the economic consequences of sanctions are just a means to achieving political goals.[1] This lack of research puts policymakers in a difficult position, as could be observed in spring 2022, when the international community had to decide on sanctions against Russia in reaction to its invasion of Ukraine. It was possible to predict the sanctions' impact on the Russian economy, as well as the economic costs for the sanctioning countries. However, given the lack of reliable evidence on sanctions' political impacts, it was not straightforward to define concrete policy objectives the sanctions should achieve, apart from supporting Ukraine in a broad sense.

Our paper contributes to closing this — arguably large — research gap. Our focus is on Russia, and the sanctions imposed on the Russian economy after its annexation of the Crimean peninsula in 2014. The question is whether the sanctions had any effect on the Russian population's support of the ruling regime – or its opposition.

To assess regime support, we rely on election results.[2] We observe the universe of political parties and candidates participating in Russian elections between 2007 and 2018, and group them into six mutually exclusive categories. It turns out that the effects of sanctions are centered on three political groups. President Putin and his party "United Russia" significantly gain in vote shares, both in parliamentary and in presidential elections. Communist parties and — to a lesser degree — nationalist parties lose support, while vote shares of other opposition parties, specifically the liberal opposition, remain largely unaffected. There are no significant effects on turnout. Since the Communist party of Russia mainly campaigns on a nationalist platform, stressing the forgone strength of the Soviet Union more than Marxist ideology, we interpret these results as providing a consistent picture: Voters attracted by a nationalist agenda to re-establish Russia's former glory turn to supporting the ruling regime in reaction to the economic sanctions imposed by foreign countries.

Our empirical strategy rests on comparing post-sanction election results to pre-sanction election results, both observed at the *rayon*-level ($\sim$ district). We regress changes on a measure of regional sanction exposure. To assess sanction exposure, we rely on regional trade flows with foreign countries, observed on the *subject*-level (roughly $\sim$ state).[3] However, comparing observed trade flows pre- and post-sanctions would only provide a coarse measure of sanction effects,

[1] To the best of our knowledge, Marinov (2005) is the only attempt to estimating the effect of sanctions on regime change in a cross-country study that compares sanctioned to non-sanctioned countries.

[2] We detect indications of election fraud in our election data. However, this could only bias our results if election fraud structurally increased with sanction exposure. We provide evidence for this not being the case.

[3] Russian "Rayons" are nested within federal "Subjects".

since international trade flows change for a plethora of reasons, many of which are unrelated to sanctions. Thus, we derive counterfactual trade flows in the absence of sanctions from a structural model. More precisely, we feed a general-equilibrium gravity model with information on pre-sanction trade flows. Holding bilateral trade-costs from the pre-sanction period constant but allowing for adjustments in the overall patterns of trade and production, the structural model allows us to determine trade flows in the absence of sanctions, i.e. net of simultaneous developments in international trade, substitution of trading partners, and changes in domestic production. Comparing observed post-sanction trade-flows to this counterfactual approximates regional exposure to sanctions.[4]

Ultimately, whether and how sanctions impact on electoral support is an open empirical question. First, voters are differently affected by sanctions, with some of them potentially benefiting in economic terms, e.g. workers in sectors where domestic production increases due to decreasing imports. Second, it is unclear how losses from economic sanctions translate into voting behavior. If voters blame the government for the economic hardships they experience, regime support should decline. Conversely, if voters blame the sanctioning countries, there could be a "rally around the flag" effect, that leads voters to unite behind the government. Both effects could occur simultaneously, leading to political polarization. Of course, voters could also just be indifferent.

Our results provide a clear answer: The sanctions imposed on the Russian economy significantly increased the internal support of president Putin and his regime in the subsequent elections. Increasing regime support mainly comes at the expense of the Communist party, while support of the liberal opposition is not affected. The effect is astonishingly stable across various subsamples, including larger cities or oil-exporting regions, speaking against any meaningful effect heterogeneity. Placebo regressions and event studies rule out pre-trends and support our identification strategy.

Our paper adds to the resurgent literature on sanction effects (Haider, 2017; Crozet and Hinz, 2017; Besedeš et al., 2017; Etkes and Zimring, 2015; Dreger et al., 2016; Felbermayr et al., 2019). Ours is the first paper to causally infer on economic sanctions' impact on political support of the sanctioned country's government.With that, our paper also speaks to the literature on the political consequences of economic shocks (e.g. Dippel et al., 2015; Autor et al., 2016; Becker et al., 2017).

Importantly, this paper should not be misunderstood as an evaluation of sanctions' success. On the contrary, we would like to stress that as a profession, we still have some way to go to thoroughly evaluate economic sanctions' effectiveness, if not their efficiency as policy tool. Most crucially, we need more information on sanctions' political impacts. This paper provides limited, though relevant insights in this direction. Our results hint at some unintended consequences

[4]This is an exposure measure, since it captures all sanction effects correlated with regional trade losses and gains caused by the sanctions. Sanction effects orthogonal to sanction's trade effects would not be captured, though, e.g. effects from travel bans on selected individuals. However, given the specific nature of the 2014 sanctions, we are confident to measure its main impact on the average voter by focusing on the sanctions' impacts on regional trade flows with foreign countries.

of economic sanctions that strengthen regime support. It may well be worth paying this price if sanctions are effective in achieving other goals. However, to evaluate such trade-offs, more quantitative research is needed. At this juncture, our paper suggest that policymakers should think more carefully about ways to mitigate economic sanctions' "rally-around-the-flag"-effect, that helps to stabilize the targeted regime – at least in the short run.

The remainder of this paper is organized as follows: In Section 2 we provide some context for our empirical analysis. Section 3 presents the data and identification strategy. Results are presented and discussed in Section 4. Section 5 concludes.

2 Context and Measurement

2.1 Historical Background

We aim at identifying (some) political consequences of the economic sanctions imposed on the Russian economy in 2014. Against the backdrop of an ever-escalating conflict between Ukraine and Russia, 37 countries step-wise imposed different types of sanctions in an attempt to hold the Russian aggression back.[5]

Following the refusal of Ukrainian President Viktor Yanukovich to sign the EU-Ukrainian Association agreements in 2013, Ukraine witnessed a series of massive demonstrations. The protests started on the 21^{st} of November 2013 and, by the 30^{th} of November, they reached 400-800 thousand protesters. On the 21^{st} of February 2014, President Yanukovich fled to Russia and his government was replaced by a Western-oriented one.

On February 27, Russian troops occupied the Ukrainian peninsula Crimea. The U.S., EU, and other countries reacted with "targeted sanctions" that hit selected Russian individuals with travel bans and asset freezes. On March 18, Russia annexed Crimea. In response, a total of 37 countries escalated the sanctions. Clashes between Russian-backed separatists and the Ukrainian army intensified in the Eastern border regions of Ukraine ("Donbass"), peaking in the downing of a civilian Malaysian airplane on July 17, killing 298. In response, the 37 countries imposed a package of additional sanctions on Russia, broadly consisting of three elements: (1) additional asset freezes and travel bans targeting selected individuals; (2) an export ban on military goods, dual-use goods, and selected equipment for the oil industry; (3) a transaction ban on major Russian banks, accompanied by measures restricting Russian companies' access to the international financial markets (e.g. a ban on issuing bonds with longer maturity).

Our analysis cannot distinguish between the different types of sanctions. However, with our focus on trade losses caused by sanctions, we will mainly capture effects of the financial

[5] This exemplifies the core challenge for evaluating sanction effects, i.e. the choice of an appropriate counterfactual. Most obviously, the sanctions were not successful in forcing Russia to retrieve from Ukrainian territory. Conversely, whether Russian troops would have further advanced in 2014 already without the sanctions is difficult to determine.

sanctions (3), which also had the most significant impact on the Russian economy (Hinz and Monastyrenko, 2022).[6] Since the Russian government responded with an embargo on certain food and agricultural products, we will focus our analysis on exports.[7]

2.2 Measuring Sanction Effects

Most obviously, sanction effects on the Russian economy cannot be observed in their entirety, thus we have to rely on a proxy. A natural candidate is trade losses caused by sanctions. First, most sanctions deliberately aim at restricting a sanctioned country's ability to trade internationally. Second, significant shocks to a country's ability to export are inevitably correlated with the economic consequences of sanctions more generally. In the given case, it is important to note that the 2014 sanctions did not only affect international trade of a selective set of embargoed goods. On the contrary, the financial sanctions imposed on Russia affected Russian companies across the board, increasing their capital costs in general and their trade costs in particular, which led to an overall decrease in both exports from and imports to Russia.

The analytical challenge is to determine trade losses caused by the sanctions, but no simultaneous developments. In order to do so, we rely on a structural model, the well-established gravity model of international trade (Head and Mayer, 2014) as given by Equation (1).

$$X_{odt} = \frac{Y_{ot}}{\Omega_{ot}} \cdot \frac{S_{dt}}{\Phi_{dt}} \cdot \phi_{odt} \tag{1}$$

Essentially, this model decomposes observed trade flows X_{odt} between any origin o and any destination d at time t into different components, namely exporter-specific, importer-specific, and bilateral characteristics. Y_{ot} and X_{dt} capture overall exports sales and import demand in the origin and destination, respectively. Ω_{ot} and Φ_{dt} are so-called outward and inward multilateral resistance terms that capture both locations' overall propensity to im- or export, i.e. their relationship to the world market. Conversely, ϕ_{odt} is country-pair specific and summarizes bilateral trade frictions between o and d and time t. If o imposes sanctions on d, this increases ϕ_{odt}, first and foremost. In consequence, Ω_{ot} and Φ_{dt} may adjust in line with $o's$ and $d's$ ability to divert trade to different partners. Eventually, Y_{ot} and X_{dt} may adjust to the new trade equilibrium as well.

Equation (1) guides our empirical strategy to follow. Basically, we will use information on Russian trade with the rest of the world from the pre-sanction years to hold bilateral trade costs ϕ_{odt} constant. Moreover, we will account for adjustments in Ω_{ot}, Φ_{dt}, Y_{ot} and X_{dt} to the

[6] Sanctions on individuals or companies will be captured to the degree that their impacts coincide with regional trade losses (or gains). Embargoes on selected goods affect only a tiny share of international trade, but will be captured to the degree that they are observable in administrative trade data.

[7] Since imports and exports are correlated, it is not possible to unambiguously distinguish between import- and export effects. However, any measure of sanction effects on Russian imports will endogenously affected by Russian retaliation and efforts to prop up domestic supply, thus we primarily rely on the more exogenous sanction-effects on Russian exports.

sanctions imposed. Within this conceptual framework, all other changes in X_{odt} are unrelated to sanctions but result from simultaneous developments. Thus, based on Equation (1), we can compare observed changes in X_{odt} to counterfactual changes in absence of the sanctions. Using regional-level trade data, we will do so for 75 federal subjects in Russia, which allows us to empirically exploit regional variation in sanction exposure for identification.

2.3 Measuring Regime Support

To assess regime support, we rely on administrative data on election outcomes for the presidential elections and the elections to the national parliament "Duma", provided by the Russian Election Commission (izbirkom.ru). We consider elections held before and after the 2014 sanctions for both presidential (2008, 2012, 2018) and parliamentary (2007, 2011, 2016) elections. Election outcomes are observed at a very granular level of around 100,000 electoral wards, which we map into a time-consistent spatial framework of about 2300 "rayons", i.e. administrative districts.[8]

We observe votes cast for every party (in parliamentary elections) or candidate (in presidential elections) participating in an election, and group those outcomes into six mutually exclusive categories: regime, nationalist, communist, loyal opposition, liberal opposition, and others. We count votes for Vladimir Putin, his substitute in the 2008 election, Dmitry Medvedev, and their party "United Russia" as *regime* votes. Over our period of analysis, these individuals and their party were constantly in power. *Nationalist* votes mainly refer to Vladimir Zhirinovsky and his "Liberal Democratic Party of Russia." *Communist* votes mainly refer to Gennady Zyuganov and his "Communist Party of the Russian Federation." A peculiarity of Russian politics under Putin is what we call *loyal opposition*: in parliamentary elections, these are opposition parties that explicitly endorse the regime (e.g., A Just Russia) and, in return, get supported by the Kremlin; in presidential elections, there are close allies of Putin (e.g., Boris Titow) who run for election to split opposition votes. Conversely, we account as *liberal opposition* votes for parties and candidates striving to actually replace the ruling regime, and to implement liberal and democratic reforms, such as Grigori Jawlinski and his party "Jabloko." Eventually, a residual category *others* captures votes for candidates with an ill-defined political agenda, or single-issue parties like the pensioners' party or the greens.[9] Moreover, we calculate election turnout.

Independent election observers like the OSCE have persistently criticized Russian elections over various irregularities.[10] In this respect, using electoral data at a very granular level (around 100,000 wards) has two advantages. First, it avoids aggregation fraud.[11] Second, it allows us to investigate statistical irregularities in the election data like an unusual clustering of even numbers

[8] After accounting for territorial reforms, our rayon-level data largely corresponds to the 2018 territorial structure of Russia. If rayons split in the later years, we merge them to consistently observe the initial aggregate. Many cities consist of several rayons, we merge them into one observation. We disregard observations from occupied Crimea and Sevastopol.

[9] Empirical results are robust to classification changes.

[10] Reported fraudulent practices include direct manipulation of ballots and vote counts, as well as intimidation of voters and candidates. See e.g. Mebane Jr and Kalinin (2009), Enikolopov et al. (2011), and Kobak et al. (2016).

[11] See Callen and Long (2015) for an analysis of this type of electoral fraud in Afghanistan.

around meaningful dates like 50 percent. In our subsequent analysis, panel econometrics will absorb regional variation in such irregularities. Remaining variation over time is unrelated to sanction exposure, as we will show. Thus, we are confident to use election data as indication for changing regime support in reaction to sanction exposure.

3 Data and Empirical Strategy

3.1 Baseline Difference-in-Differences Model

Our empirical strategy to identify the effects of sanction exposure on voting behavior follows two steps. First, we employ a structural gravity model of international trade to determine counterfactual trade flows in the absence of sanctions. Second, we adopt a difference-in-difference method that compares voting behavior across regions, with regions being differently affected by sanction-induced trade losses (or gains)[12] caused by sanctions.

Our resulting baseline model is described in Equation (2).

$$\Delta \text{Voting}_{ir,t} = \alpha + \beta \text{ sanction\_exposure}_r + \Gamma \ \Delta \text{X}_{ir,t} + \epsilon_{ir,t} \tag{2}$$

Our data is organized as a stacked panel of first differences, where $\Delta\text{Voting}_{ir,t}$ relates to changes in election outcomes for different groups of parties and candidates observed at rayon-level i nested in region (i.e. federal subject) r.

Since the imposition of sanctions fell amidst the election cycle of both the presidential and the parliamentary elections in Russia, we can compare election results that were affected by the sanctions in treatment years t_{+1} to those that were not affected in pre-treatment years t_0. Moreover, observations from earlier elections in placebo years t_{-1} allow for testing the common trends assumption, and rule out unobserved regional-level confounders.

Accordingly, all time-invariant differences between regions get absorbed by the first differences. Control variables $\text{X}_{ir,t}$ are obtained from the Statistical Office of the Russian Federation. Specifically, we include information on regional demographics (population, migration, employment rate), labor force characteristics (age structure, qualification) and industry structure (sectoral employment shares).[13] Note that, in this framework, these controls capture potential differences in trends (rather than levels) between treatment and control units. In addition, we include a binary control for presidential elections. Standard errors are clustered at the level of federal subjects.

[12] Indeed, some regions manage to substitute trading partners which may even lead to increasing trade volumes. For the sake of brevity, we will subsequently refer to the dominant trade losses only.

[13] See Appendix A.2 for descriptive statistics on all the variables used.

To identify sanction effects on regime support, and on voting behavior more broadly, we exploit regional variation in trade losses caused by sanctions, i.e. $sanction\_exposure_r$, as well as time-variation in the support for different parties and candidates in elections pre- and post-sanctions.

3.2 Assessing regional sanction exposure

Our main explanatory variable, $sanction\_exposure_r$, captures regional variation in trade losses caused by the sanctions – and all related economic impacts. To measure $sanction\_exposure_r$, we rely on regional-level trade data from the "Federal Customs Service of Russia".[14] As it is common in trade data, im- and exports are reported on the product level for all international trading partners. A unique feature of the data is that it reports trade flows on the level of "Federal Subjects", i.e. the first sub-national level of federal division in Russia. Disregarding occupied Crimea and Sevastopol, there are 83 Federal Subjects, which makes them roughly comparable to U.S. states.[15] For 75 of these Federal Subjects, we have precise information on their imports from and exports to the rest of the world.[16]

Let observed total exports or imports of a Russian region r be described by T_r. We observe $T_{r(pre)}$ for the years before 2014 and $T_{r(post)}$ after the sanctions were imposed. Most obviously, it would be insufficient to just exploit time variation in Russian regions' r trade with foreign countries to identify sanction effects. Over time, trade flows may vary for different reasons, like changing commodity prices, changing global demand, or shifts in comparative advantage unrelated to sanctions. Put differently, $T_{r(pre)}$ would not be a valid counterfactual for $T_{r(post)}$, thus observed time-variation $T_{r(post)} - T_{r(pre)}$ would be a poor proxy for regional sanction exposure. Instead, we will use information on both $T_{r(pre)}$ and $T_{r(post)}$ combined with our structural model (1) to infer on counterfactual trade flows $\widehat{T}'_{r(post)}$, i.e. regional trade flows that would – everything else equal – have occurred in the absence of sanctions. Comparing observed $T_{r(post)}$ to counterfactual $\widehat{T}'_{r(post)}$ allows to infer on regional trade losses caused by the sanctions, i.e. our exogenous measure of regional sanction exposure as defined by

$$\text{sanction\_exposure}_r = \frac{T_{r(post)} - \widehat{T}'_{r(post)}}{\widehat{T}'_{r(post)}} \tag{3}$$

Fortunately, the regional-level trade data is consistent with the official trade data reported to the UN Comtrade database.[17] It is thus straightforward to augment the regional data by international trade data covering im- and exports of countries other than Russia. In total we combine trade data on 75 Russian subjects and the universe of countries –including 37 sanctioning countries– for the years 2012 to 2015, i.e. two years pre- and post sanctions' implementation. Thus,

[14] See http://stat.customs.ru/. At the time of writing data access has been restricted to Russian IP addresses.

[15] In our empirical analysis, panel econometrics will account for time-consistent differences between the regions.

[16] We drop observations from the war-torn Chechen Republic. The remaining Subjects we drop are only sparsely populated and report trade figures only infrequently.

[17] See http://comtrade.un.org, for the years 2012 to 2015.

structural model (1) can easily be estimated as

$$X_{odt} = \exp\left(\Psi_{ot} + \Theta_{dt} + \phi_{odt}\right) + \epsilon_{odt} \tag{4}$$

using a Poisson Pseudo-Maximum Likelihood estimator, where Ψ_{ot}, Θ_{dt} and ϕ_{odt} are simple fixed effects.[18] Estimated $\widehat{\Psi}_{ot}$ and $\widehat{\Theta}_{dt}$ assess multilateral resistance terms Ω_{ot} and Φ_{dt}, while $\widehat{\phi}_{odt}$ measures bilateral trade frictions ϕ_{odt}.

Assessing counterfactual trade follows $\widehat{T}'_{r(post)}$ follows a three-step procedure, as in Crozet and Hinz (2017): First, we use $T_{r(pre)}$ as outcome in a regression of Equation (4). We retain $\widehat{\phi}_{odt}$ and keep this parameter – indicating bilateral trade costs – constant in all subsequent estimations. Conditional on the pre-sanction $\widehat{\phi}_{odt}$, we repeat the estimation of Equation (4), but with $T_{r(post)}$ as outcome. This allows assessing counterfactual trade flows if only bilateral frictions had changed. In a second step, we update estimates of $\widehat{\Psi}_{ot}$ and $\widehat{\Theta}_{dt}$ from the post-sanction period to account for adjustments in the multilateral resistance terms due to changing ϕ_{odt}. As a consequence, counterfactual trade flows account for substitution of trading partners in reaction to the sanctions. Eventually, we update $\widehat{Y}_{ot}$ and $\widehat{X}_{dt}$ in an iterative process, to account for adjustments in export- and import volumes triggered by the sanctions. A detailed description of this procedure is provided in Appendix B.

To determine Russian regions' sanction exposure, We aggregate predicted counterfactual trade flows for a world without sanctions for the sanctions years, i.e. 2014 and 2015, according to $\widehat{T}'_r = \sum_d \widehat{X}_{odt}\ \forall r \in \{\text{Russian regions}\},\ \forall t \in \{\text{Sanctions period}\}$. Now, we can assess $sanction\_exposure_r$ as in Equation (3).

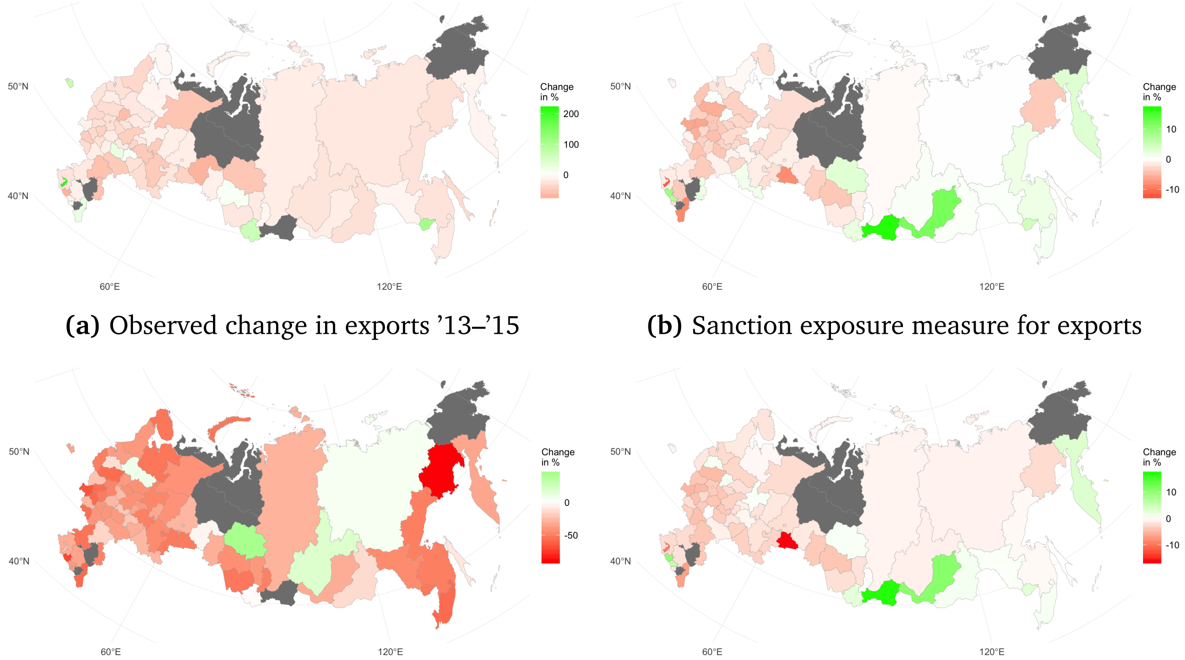
Figure 1 compares the spatial distribution of the resulting measure of $sanction\_exposure_r$ (right panel), both for exports (upper panel) and imports (lower panel), to the underlying changes in observed trade flows (left panel). Most obviously, all measures are related, but vary in the degree to which regions are affected by trade losses. Most obviously, regions in close proximity to sanctioning countries, i.e. those in the Western part of the country, are on average more adversely affected by $sanction\_exposure_r$ than other subjects. Notably, when accounting for general equilibrium effects and the substitution of trading partners, some regions even manage to increase their exports due to $sanction\_exposure_r$.

3.3 Identifying Variation

Regional variation in $sanction\_exposure_r$ used for identification according to Equation (2) ultimately stems from three interdependent sources. First, regional industry structure determines

[18] Santos Silva and Tenreyro (2006) show that a GLM estimation with an assumed Poisson distributed error term is preferable to an OLS estimation of the gravity equation. Fally (2015) shows that, as an additional benefit, the exporter and importer (-time) fixed effects in a PPML estimation of the gravity equation have a functional form that is isomorphic to production and expenditure figures, divided by their respective multilateral resistance terms of structural gravity equations.

Figure 1: Spatial distribution of regional sanction exposure



(a) Observed change in exports '13–'15

(b) Sanction exposure measure for exports

(c) Observed change in imports '13–'15

(d) Sanction exposure measure for imports

the relevance of international trade for the local economy in general. Second, regional specialization in trade with specific partners make some regions more susceptible to sanctions than others. Third, regional characteristics determine how easy the local economies can compensate trade losses by substituting trading partners. In other words, regional variation in sanctions' impacts on trade flows is largely pre-determined by regional characteristics, at least in the short run. Time-persistent differences in voting patterns and in trade patterns cancel out by first-differencing, such that changes in voting patterns can be related to changes in trade flows. $sanction\_exposure_r$ gets a causal interpretation by restricting the variation in trade flows to changes that were caused by the sanctions.

Our identification strategy rests on two assumptions. First, like always, we assume that the structural model guiding the empirical analysis is correct. More interestingly, within the model framework, the crucial assumption is that over the period 2012–2013 and 2014–2015, bilateral trade frictions ϕ_{odt} between all countries change due to the 2014 sanctions only.

Technically, this exclusion restriction is most likely violated, since bilateral frictions between some countries will certainly have changed, e.g. due to improvements in transportation infrastructure that decreases trade costs. However, from an a applied perspective, minor violations of the exclusion restriction can be tolerated as long as they have no significant impact on the results. Specifically, for identifying sanction effects it is crucial that no significant changes in ϕ_{odt} occurred for Russian regions, or their major trading partners, simultaneously.

Indeed, we are convinced that in quantitative terms, nothing of remotely similar magnitude to sanction effects has affected trade flows to and from Russia over the time period of interest.

Trade flows between the 37 sanctioning countries and Russia accounted for 2.9 % of world trade in the pre-sanctions years of 2012 and 2013, according to UN Comtrade data. No simulateous change in trade costs changes came close to this figure, especially not in terms of its effect Russian regions' trade with foreign countries.

Arguably, during the period in question between 2012 and 2015, a number of Free Trade Agreements (FTAs) were signed, five countries became new WTO members, and Croatia officially joined the EU.

Trade flows between countries forming new FTAs accounted for roughly 1.6 % of global trade. Some of the most affected countries were Australia (59 % of trade affected by new FTAs), Cameroon (55 %), Moldova (32 %), and Georgia (23 %). Moldova's and Georgia's changing trade costs could have had an impact on Russias trade through trade diversion, as both were part of the Soviet Union and thus share deep historical ties with their big neighbor. In practice, though, before Moldova and Georgia signed a "Deep and Comprehensive Free Trade Area" with the EU, the two countries only accounted for 0.2 % of Russia's exports and 0.3 % of its imports — not nearly enough to affect gross figures through diversion effects.

Two of the five new WTO member states over the course of the period, Tajikistan and Kazakhstan, also share deep historical ties with Russia. Their accession to the global trade agreement could have had an impact on their trade with Russia. While only 0.8 % of world trade were affected by the new entrants, both Tajikistan and Kazakhstan are moderately important trading partners for the Russian Federation: roughly 3.7 % of its export and imports take place with these countries. However, in practice no trade cost change happened. The three countries had already been members of the Eurasian Economic Union and of an FTA between former members of the Soviet Union, the so-called CIS countries.

The last potentially relevant change in global trade costs was Croatia's accession to the European Union. With their new member, the sanctioning coalition increased its ability to affect Russia[19] However, two important points decrease the relevance of this change in trade costs. First, the process of becoming a EU member state is gradual, why integration into the EU market long preceded Croatia's official membership. Second, trade ties between Croatia and Russia are negligible: Only 0.3 % of Russian exports go there, 0.1 % of its imports originate in the Adreatic country.

Overall, the 2014 sanctions against the Russian Federation are by far the largest shock to global trade costs in the 2012–2015 period. Furthermore, simultaneous changes could have only very marginally affected Russia's trade with the rest of the world — and with sanctioning countries in particular. We are thus confident that these minor violations of the exclusion restriction cannot bias our estimates.

[19] See also Chowdhry et al. (2022) who investigate the effect of sanctions coalitions and individual member's contributions to the sanctions' effect.

Table 1: The effect of sanction exposure on Russian elections

	(1)	(2)	(3)	(4)	(5)
	Effect of Sanction Exposure (Exports)				
Δ regime	0.576**	0.565**	0.575***	0.486***	5.070***
	(0.229)	(0.214)	(0.170)	(0.103)	(1.074)
Δ loyal	-0.032	-0.047	-0.031	-0.005	-0.108
	(0.098)	(0.081)	(0.071)	(0.040)	(0.798)
Δ nationalist	-0.110*	-0.081	-0.076	-0.078	-1.906
	(0.065)	(0.063)	(0.062)	(0.054)	(1.316)
Δ communist	-0.396***	-0.399***	-0.406***	-0.330***	-5.833***
	(0.139)	(0.136)	(0.129)	(0.072)	(1.279)
Δ liberal	-0.010	-0.012	-0.032	0.006	0.186
	(0.047)	(0.040)	(0.029)	(0.011)	(0.372)
Δ other	-0.028	-0.026	-0.030	-0.032	-2.181
	(0.025)	(0.019)	(0.022)	(0.022)	(1.518)
Δ turnout	0.184	0.145	0.030	0.035	0.320
	(0.201)	(0.200)	(0.184)	(0.189)	(1.746)
Controls	Baseline	+ labor force	+ industry	+ political	∼(4) STD.
Election-FE	Yes	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396	4,396

Notes: (*a*) Each cell reports results from a separate regression. (*b*) Rows refer to different outcome variables observed at the *rayon*-level. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) Columns incrementally add controls: Column 1 controls only for regional demographics. Column 2 adds further controls for regional labor force characteristics listed in the text. Column 3 adds further controls for regional industry structure listed in the text. Column 4 adds start-of-period outcomes and, in the case of party-outcomes, first differences in turnout. Column 5 replicates column 4 but reports standardized treatment coefficients to facilitate comparison. All specifications include election-type fixed effects. (*d*) Standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

4 Results

4.1 Main Results

We now turn to estimating our difference-in-differences model described in equation (2). Our focus is on the effect of $sanction\_exposure_r$ measured via regional export losses.[20] Table 1 reports results for different party outcomes and for overall turnout, with $\Delta\text{Voting}_{irt}$ calculated as changes between the first post-sanction election and the last pre-sanction election. Every cell reports another treatment coefficient for $sanction\_exposure_r$. Each line reports on a different outcome $\Delta\text{Voting}_{irt}$. Columns (1)–(4) successively include additional regional-level control variables. To facilitate comparison, Column (5) repeats results from our preferred specification in Column (4) with standardized coefficients. All estimations include election-type fixed effects.

The results consistently show that the sanctions imposed in 2014 have a significant impact on

[20]Corresponding results for import losses can be found in Appendix C, Table 7.

subsequent elections in Russia. Regime support, i.e. the vote share of president Putin and his party "United Russia", increase significantly with regional sanction exposure. A one standard deviation increase in $sanction\_exposure_r$ (i.e. a decrease of 0.029 in regional exports relative to counterfactual exports in the absence of sanctions) increases electoral support of the governing regime by $(0.029 \times 0.486 \times 100 =)$ 1.4 percentage points. This is economically meaningful. Starting from high levels of pre-sanction support, the governing regime was able to increase its overall support by around 6.3 percentage points over our period of analysis. Hence, a 1 standard deviation increase in $sanction\_exposure_r$ explains roughly 22 percent of the general increase in regime support.

Naturally, the gains of one political camp must come at the expense of other parties. It turns out the regime gains support at the expense of communist parties, first and foremost. The Communist camp is dominated by the successor of the Communist party, led by Gennady Zyuganov, that ruled the Soviet union. Our understanding of Russian politics is that in their campaigning, the Communists more frequently refer to the greatness of the Russian nation in the Soviet era, than to Marxist ideology. The Communist camp strives to restore Russian power and defend the nation against malicious Western influence. It seems plausible that adherents of the Communist camp decided to support Putin once Russia became "under attack" from "Western" sanctions.

No other opposition party is affected by the sanctions. Specifically, the liberal opposition does not benefit from voters' discontent with the sanctions — nor does it lose support. One might have expected that opposition to the ruling regime increased in reaction to the sanctions. Our results clearly speak against such a polarizing effect.

The turnout results, although statistically insignificant, speak against opponents of the regime just not participating in elections. Indeed, turnout tends to be higher the more a Russian region is affected by the sanctions.[21]

Figure 2 summarizes our main finding in an event-study graph. It plots treatment-coefficients from a fixed-effects model were we regress regime support Voting_{irt} observed at three points in time — two election cycles before the sanctions were imposed and one after the sanctions were in place — on our measure of $sanction\_exposure_r$, interacted with the period indicators. Covariates correspond to our preferred specification from Column (4) in Table 1. Treatment coefficients are evaluated against the omitted effect in the pre-sanction period t_0. Corresponding event-study graphs for all other election outcomes can be found in Appendix C, Figure 4.

Figure 2 shows that the effect of $sanction\_exposure_r$ is measurable only when it should be, i.e. after the sanctions where actually imposed. The regional variation in $sanction\_exposure_r$ has no explanatory power for earlier elections, confirming the assumption of common trends underlying our difference-in-differences estimations. We explore potential confounders more thoroughly in the following subsection.

[21] Unfortunately, we are not aware of reliable individual-level panel data for Russia that would allow us to measure changes in political support on the individual level. We account for potential changes in the composition of the electorate by conditioning on turnout in our preferred specification (4).

Figure 2: Event Study: the effect of sanction exposure on Regime Support

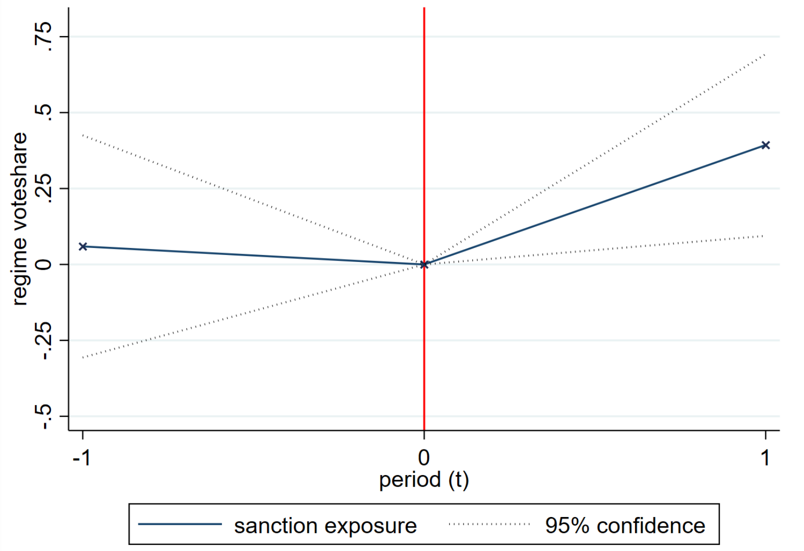


Table 2: Placebo Effects on Pre-Sanction Outcomes

	(1) Δ regime	(2) Δ loyal	(3) Δ nationalist	(4) Δ communist	(5) Δ liberal	(6) Δ other	(7) Δ turnout
	Placebo-Effects (Exports) on Pre-Sanction Outcomes (Column)						
$sanction\_exposure_r$	0.019 (0.148)	-0.069 (0.079)	0.040 (0.051)	-0.029 (0.106)	0.030 (0.033)	0.006 (0.007)	0.184 (0.155)
Controls	+ political	+ political	+ political	+ political	+ political	+ political	+ political
Election-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396	4,396	4,396	4,396

Notes: (*a*) Each column reports results from a separate regression. (*b*) Columns refer to different outcome variables observed at the *rayon*-level. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) All specifications control for regional demographics, regional labor force characteristics, regional industry structure, start-of-period outcomes and, in the case of party-outcomes, first differences in turnout. All specifications include election-type fixed effects. (*d*) Standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

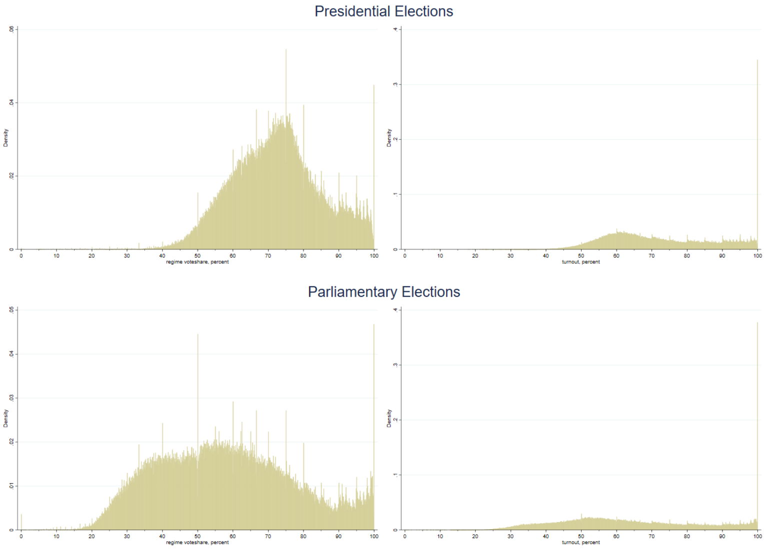
4.2 Effect Validity and Heterogeneity

To test for pre-trends, we repeat our difference-in-difference regressions, but calculate first-differences in election outcomes for the election cycle before the sanctions set in. We focus on our preferred specification as in Column (4) of Table 1. Results are reported in Table 2.

In Table 2, we regress changes in pre-treatment election outcomes on our sanction shock from the treatment period. The only way the sanction shock could have an impact on pre-treatment outcomes was through unobserved, time-invariant regional level characteristics. All the point estimates are small and statistically insignificant. This clearly supports our identification strategy.[22]

[22]Corresponding Placebos for the Import-Shock can be found in Appendix C, Table 8.

Figure 3: Even Numbers in Russian Election Results



Another concern in the context of our paper is about election fraud biasing our results. Indeed, we can detect some statistical irregularities in our election data, like an unusual clustering of election results with "even numbers" in vote shares or turnout, specifically around "meaningful" dates like 50 % or 75 %. Figure 3 shows the density of vote-shares received by Putin and his party (left) and of turnout (right), observed at the level of electoral precincts, for presidential (upper panel) and for parliamentary (lower panel) elections.

These irregularities cannot bias our estimates as long as they are time-consistent (thus being absorbed by first-differencing or by regional fixed-effect), or uncorrelated with $sanction\_exposure_r$. While there is no specific reason to assume that election fraud increases or decreases with $sanction\_exposure_r$, we empirically test for such a relationship in additional placebo regressions. We resort to our initial difference-in-differences model described in Equation (2) and to our preferred specification from column (4) of Table 1. Based on the frequency with which statistical irregularities occur on the rayon-level, we construct several placebo-outcomes and regress them on $sanction\_exposure_r$. Results are presented in Table 3.

To assess whether statistical irregularities in the election data increase with $sanction\_exposure_r$, we exploit the granular structure of our election data. Indeed, we observe election outcomes at the level of electoral precincts, with precincts being nested in rayons r. For each rayon, we calculate the share of precincts reporting even percentages (Columns 1, 3 and 5), or even percentages at meaningful dates like 50 or 75 percent (Columns 2, 4 and 6) in all precincts. We do so for all party outcomes (Columns 1–2), the vote shares of Putin and his party (Columns 3–4), and turnout (Columns 5–6). Table 3 clearly speaks against sanctions leading to increased interference with election results. Consequently, we regard our main results to be unbiased by election fraud. Event study graphs on statistical irregularities, split by election type, can be found in Appendix C, Figure 5.

Table 3: Placebo effect on election irregularities

	(1)	(2)	(3)	(4)	(5)	(6)
	All party shares		Regime shares		Turnout	
	Δ even	Δ meaningful	Δ even	Δ meaningful	Δ even	Δ meaningful
	Placebo-Effects (Exports) on Column-Outcomes					
$sanction\_exposure_r$	0.113	0.109	0.044	0.041	0.021	0.008
	(0.166)	(0.166)	(0.043)	(0.042)	(0.047)	(0.046)
Controls	+ political	+ political	+ political	+ political	+ political	+ political
Election-FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396	4,396	4,396

Notes: (*a*) Each cell reports results from a separate regression, following the empirical specification reported in column (4) of Table 1. (*b*) Columns refer to different outcome variables observed at the *rayon*-level. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) Columns 1, 3 and 5 show the effect on the share of even numbers. Columns 2, 4 and 6 show the effect on the share of meaningful numbers in all precinct-level election results for: Column 1-2 all parties and candidates. Column 3-4 regime party and candidates. Column 5-6 Turnout. All specifications include election-type fixed effects. (*d*) Standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effect Heterogeneities

	(1)	(2)	(3)	(4)	(5)
	Presidential Election	City	Oil/Gas Region	Focused on Sanctioning	Benefits from sanctions
Panel A: Subsample where Columns is “No”					
$sanction\_exposure_r$	0.316**	0.464***	0.473***	0.468***	0.530**
	(0.146)	(0.104)	(0.134)	(0.103)	(0.251)
Observations	2,198	4,104	3,242	2,116	3,474
Panel B: Subsample where Columns is “Yes”					
$sanction\_exposure_r$	0.382***	0.576***	0.771***	0.591***	0.214
	(0.110)	(0.157)	(0.245)	(0.209)	(0.194)
Observations	2,198	292	1,154	2,280	922

(*a*) Notes: Each cell reports results from a separate regression. (*b*) Rows refer to different outcome variables observed at the *rayon*-level. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) All specifications control for election-type fixed effects, regional demographics, regional labor force characteristics, regional industry structure, start-of-period outcomes and, in the case of party-outcomes, first differences in turnout. (*d*) Standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

We now turn to exploring effect heterogeneities. Given the spatial heterogeneities depicted in Figure 1, different regions could react differently to sanction exposure. We thus split the sample along various categories and repeat our initial estimations on the subsamples.

Table 4 shows that the effect of $sanction\_exposure_r$ on regime support does not vary across election type or between urban and rural regions. It is only slightly larger in oil- and gas exporting regions, and even in regions benefiting from the sanctions, i.e. with negative values of $sanction\_exposure_r$ indicating export gains, the point estimates remain positive, though statistically insignificant. This is true also when using the import measure. Additional results (not reported) show that the pattern is remarkably similar for all other party outcomes. Specifically, there is no indication that liberal parties might benefit from the sanctions in selective regions. As such, Table 4 reveals the positive effect of sanction exposure on regime support to be remarkably stable across Russian regions.

5 Conclusion

The aim of this paper is to quantitatively gauge the impact of broad economic sanctions on the political support of the ruling regime. We find that sanctions increase government support in the sanctioned country. Specifically, the sanctions imposed on the Russian economy after Russia's annexation of the Crimean peninsula in 2014 increased support of president Putin and his party in the subsequent elections. We cannot infer on potential long-run effects, but in the short-run, sanctions strengthen the sanctioned government.

This does not imply that sanctions were a political failure. Our analysis just reveals (some of) the political costs attached to economic sanctions. Similar to economic costs for the sanctioning countries, it might be worth paying these costs. As a profession, we just do not know enough about all the political consequences of economic sanctions to thoroughly evaluate their success.

A concrete policy conclusion from our results is that sanctioning countries should think about ways to minimize the "rally around the flag" effect resulting from economic sanctions. In the Russian case, economic sanctions nicely fit into the Kremlin's narrative of a hostile "Western World" interfering with the Russian way of living. Obviously, it is difficult to counter such propaganda in a country where the government controls the media. Still, it seems worthwhile exploring ways to accompany sanctions with informational measures to inform the general public about the very reasons for imposing the sanctions. Moreover, one should think about ways to support the "liberal opposition", that has obviously not been able to tap into the voting potential generated by the economic distress resulting from the sanctions. Eventually, it could just be that the economic consequences of the sanctions were too mild and not targeted enough to spur significant discontent with the ruling regime.

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A Data

A.1 Descriptive Statistics Main Variables

Table 5: Main Variables Observed

		t_{-1}	t_0	t_1
regime	mean voteshare	0.704	0.600	0.663
	SD	0.107	0.159	0.164
loyal	mean voteshare	0.050	0.084	0.035
	SD	0.059	0.070	0.038
nationalist	mean voteshare	0.089	0.096	0.118
	SD	0.042	0.054	0.077
communist	mean voteshare	0.140	0.180	0.152
	SD	0.068	0.065	0.062
liberal	mean voteshare	0.011	0.034	0.010
‘	SD	0.009	0.028	0.012
other	mean voteshare	0.007	0.007	0.022
	SD	0.009	0.008	0.021
turnout	mean value	0.718	0.656	0.624
	SD	0.128	0.130	0.172
$\text{sanction\_exposure}_r$	mean export loss	n.a.	n.a.	0.017
	SD			0.029
$\text{sanction\_exposure}_r$	mean import loss	n.a.	n.a.	0.020
	SD			0.027
Obs.	Number	4,396	4,396	4,396
of which presidential	Number	2,198	2,198	2,198

Notes: Main Variables and their Standard Deviations observed at time $t\_{-}1$,t_0, and t_1. All variables observed at rayon-level for presidential and for parliamentary elections.

Table 6: Control Variables Observed

		t_{-1}	t_0	t_1
population	*1000	2213.350	2208.401	2208.401
	SD	1431.370	1530.080	1530.080
migration	growth rate	2.125	-1.986	-1.986
	SD	34.356	42.333	42.333
eleigibl voters	*1000	47.532	47.215	47.215
	SD	191.252	199.384	199.384
density	polling spots / eligible voters	0.002	0.002	0.002
	SD	0.001	0.001	0.001
employment	share in population	0.468	0.466	0.466
	SD	0.040	0.041	0.041
unemployment	rate	6.989	6.118	6.118
	SD	3.290	1.860	1.860
young	proportion of employed younger 30	25.099	22.068	22.068
	SD	2.151	1.781	1.781
old	proportion of employed older 49	22.702	27.349	27.349
	SD	2.269	2.146	2.146
high edu	share of employees with upper secondary education or higher	47.890	49.586	49.586
	SD	6.440	6.493	6.493
vocational edu	share of employees with vocational education	44.964	46.099	46.099
	SD	6.432	6.332	6.332
manufacturing	employment share (in all employment)	0.170	0.152	0.152
	SD	0.058	0.050	0.050
mining and quarrying	employment share (in all employment)	0.016	0.017	0.017
	SD	0.024	0.027	0.027
Agriculture, hunting, forestry and fishing	employment share (in all employment)	0.129	0.105	0.105
	SD	0.053	0.052	0.052
Gas, water, electricity	employment share (in all employment)	0.032	0.032	0.032
	SD	0.009	0.010	0.010
Construction	employment share (in all employment)	0.068	0.077	0.077
	SD	0.016	0.017	0.017
Transportation and Communication	employment share (in all employment)	0.081	0.081	0.081
	SD	0.018	0.016	0.016
Wholesale at retail trade	employment share (in all employment)	0.159	0.178	0.178
	SD	0.027	0.028	0.028
Hotels and restaurants	employment share (in all employment)	0.017	0.020	0.020
	SD	0.004	0.005	0.005
Real estate and renting	employment share (in all employment)	0.058	0.073	0.073
	SD	0.018	0.019	0.019
Healthcare and Social Services	employment share (in all employment)	0.071	0.070	0.070
	SD	0.008	0.008	0.008
Education	employment share (in all employment)	0.095	0.085	0.085
	SD	0.016	0.014	0.014
Communal and social services	employment share (in all employment)	0.036	0.037	0.037
	SD	0.005	0.006	0.006
Obs.	Number	4,396	4,396	4,396
of which presidential	Number	2,198	2,198	2,198

Notes: Controls and their Standard Deviations observed at different points in time. All variables observed at rayon-level for presidential and for parliamentary elections.

A.2 Descriptive Statistics Covariates

B Computing General Equilibrium Counterfactual Trade Flows

In the following we describe the computation of general equilibrium counterfactual trade flows using the structural gravity equation of international trade, in the spirit of Dekle et al. (2007,

2008) and Anderson et al. (2018). The computation consists of three steps, with an iteration over the last two steps until convergence.

1. **Partial equilibrium**: Replace the estimated $\widehat{\phi}_{od}$ with $\widehat{\phi}'_{od}$ to obtain counterfactual trade flows that exhibit the partial equilibrium effects of the sanctions, i.e. the direct bilateral changes in trade frictions:

$$\begin{aligned}\widehat{X}_{odt}^{PE} &= \exp\left(\widehat{\Psi}_{ot} + \widehat{\Theta}_{dt} + \widehat{\phi}'_{od}\right) \\ &= \frac{\widehat{Y}_{ot}}{\widehat{\Omega}_{ot}} \cdot \frac{\widehat{X}_{dt}}{\widehat{\Phi}_{dt}} \cdot \widehat{\phi}'_{od}\end{aligned}$$

2. **Conditional general equilibrium**: Recompute the multilateral resistance terms to obtain trade flows that take into account that the relative ease of exporting/importing of *all* towards *all* countries is changing due to the changes of *some* bilateral frictions. The multilateral resistances can be recomputed by iterating over the two following systems of equations:

$$\widehat{\Omega}_{ot}^{CGE} = \sum_{\ell} \frac{\widehat{X}_{\ell t}}{\widehat{\Phi}_{\ell t}^{CGE}} \widehat{\phi}'_{o\ell} \quad \text{and} \quad \widehat{\Phi}_{dt}^{CGE} = \sum_{\ell} \frac{\widehat{Y}_{\ell t}}{\widehat{\Omega}_{\ell t}^{CGE}} \widehat{\phi}'_{\ell d}$$

   Incorporating these updated multilateral resistance terms yields the *conditional* general equilibrium trade flows given by

$$\widehat{X}_{odt}^{\text{CGE}} = \frac{\widehat{Y}_{ot}}{\widehat{\Omega}_{ot}^{\text{CGE}}} \cdot \frac{\widehat{X}_{dt}}{\widehat{\Phi}_{dt}^{\text{CGE}}} \cdot \widehat{\phi}'_{od}$$

3. **Full general equilibrium**: The *full* general equilibrium trade flows incorporate implied changes to the last two remaining components, the export sales and expenditure figures. Following Anderson et al. (2018) and setting $\sigma = 5$ this *factory-gate* price adjustment is obtained as

$$\widehat{Y}_{ot}^{\text{GE}} = \widehat{Y}_{ot} \cdot \left(\frac{\widehat{\Psi}_{ot}^{\text{CGE}}}{\widehat{\Psi}_{ot}}\right)^{\frac{1}{1-\sigma}} \quad \text{and} \quad \widehat{X}_{dt}^{\text{GE}} = \widehat{X}_{dt} \cdot \left(\frac{\widehat{\Psi}_{dt}^{\text{CGE}}}{\widehat{\Psi}_{dt}}\right)^{\frac{1}{1-\sigma}}$$

C Additional Results

C.1 Effects of Import Losses

C.2 Event Studies on Election Outcomes

C.3 Event Studies on Election-Irregularities

Table 7: Effect of sanctions on Russian Elections: Import losses

	(1)	(2)	(3)	(4)	(5)
	Effect of Sanction Exposure (Imports)				
Δ regime	0.566**	0.551**	0.501***	0.403***	4.204***
	(0.232)	(0.217)	(0.186)	(0.121)	(1.262)
Δ loyal	-0.010	-0.012	0.020	0.064	1.291
	(0.118)	(0.100)	(0.095)	(0.054)	(1.096)
Δ nationalist	-0.109	-0.085	-0.062	-0.071	-1.739
	(0.074)	(0.073)	(0.065)	(0.062)	(1.501)
Δ communist	-0.393***	-0.400***	-0.381***	-0.304***	-5.376***
	(0.136)	(0.134)	(0.129)	(0.077)	(1.362)
Δ liberal	-0.021	-0.021	-0.040	-0.005	-0.158
	(0.049)	(0.041)	(0.035)	(0.012)	(0.392)
Δ other	-0.033	-0.033	-0.037	-0.041	-2.830
	(0.030)	(0.023)	(0.026)	(0.025)	(1.742)
Δ turnout	0.154	0.128	-0.040	-0.048	-0.446
	(0.203)	(0.207)	(0.185)	(0.189)	(1.749)
Controls	Baseline	+ labor force	+ industry	+ political	∼(4) STD.
Election-FE	Yes	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396	4,396

Notes: (*a*) Each cell reports results from a separate regression. (*b*) Rows refer to different outcome variables observed at the *rayon*-level. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) Columns incrementally add controls: Column 1 controls only for regional demographics. Column 2 adds further controls for regional labor force characteristics listed in the text. Column 3 adds further controls for regional industry structure listed in the text. Column 4 adds start-of-period outcomes and, in the case of party-outcomes, first differences in turnout. Column 5 replicates column 4 but reports standardized treatment coefficients to facilitate comparison. All specifications include election-type fixed effects. (*d*) Standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Placebo Effects on Pre-Sanction Outcomes: Import losses

	(1) Δ regime	(2) Δ loyal	(3) Δ nationalist	(4) Δ communist	(5) Δ liberal	(6) Δ other	(7) Δ turnout
	Placebo-Effects (Imports) on Pre-Sanction Outcomes (Column)						
$sanction\_exposure_r$	0.121 (0.157)	-0.063 (0.087)	0.063 (0.057)	-0.090 (0.112)	0.006 (0.032)	0.009 (0.007)	0.152 (0.174)
Controls	+ political	+ political	+ political	+ political	+ political	+ political	+ political
Election-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396	4,396	4,396	4,396

Notes: (*a*) Each cell reports results from a separate regression. (*b*) Rows refer to different outcome variables observed at the *rayon*-level. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) Columns incrementally add controls: Column 1 controls only for regional demographics. Column 2 adds further controls for regional labor force characteristics listed in the text. Column 3 adds further controls for regional industry structure listed in the text. Column 4 adds start-of-period outcomes and, in the case of party-outcomes, first differences in turnout. Column 5 replicates column 4 but reports standardized treatment coefficients to facilitate comparison. All specifications include election-type fixed effects. (*d*) Standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 4: Event Study: effect of Sanctions on Election Outcomes

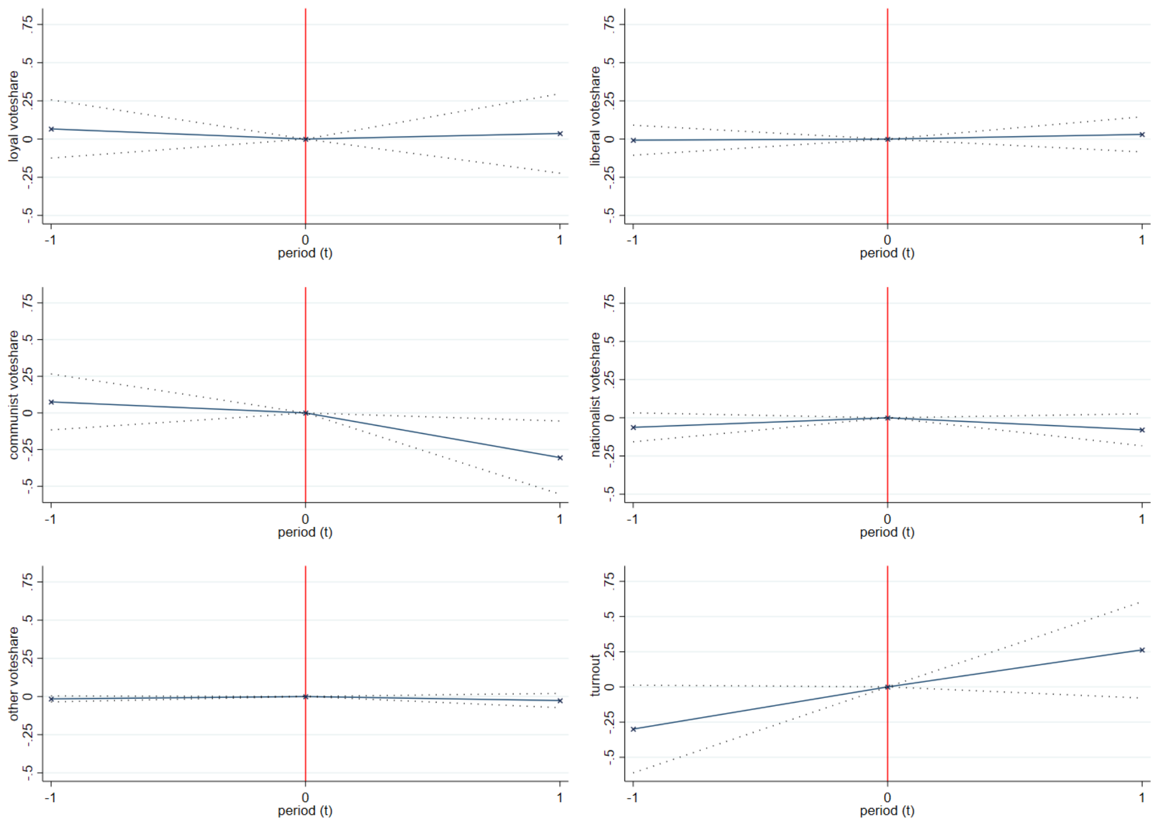

Figure 5: Placebo Effect on Statistical Irregularities