To Russia with Love? The Impact of Sanctions on Regime Support*

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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 — with regional variance. We use trade losses caused by the sanctions as measure for regional sanctions 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. Difference-in-differences 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, sanctions 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

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1 Introduction

Do sanctions influence regime support in targeted countries? Judging by the evolution of the approval ratings of Russian President Vladimir Putin and his government, the answer seems to be yes — but not in the way one might expect. In 2014, the international community imposed economic sanctions on Russia in response to its incursion in Eastern Ukraine and the annexation of the Crimean Peninsula. In the following months, Putin's approval ratings increased significantly, from 65% in February 2014 to 80% in April 2014. Similarly, in 2022, Putin's approval ratings increased from 64% in February to 72% in April, after Russia invaded Ukraine.[1]

However, sound causal evidence is scarce. While the economic consequences of sanctions are comparatively well understood, both for sanctioned countries (Dreger et al., 2016; Haider, 2017; Ahn and Ludema, 2020; Nigmatulina, 2022; Draca et al., 2022) and for sanctioning countries (Besedeš, Goldbach, and Nitsch, 2017; Crozet and Hinz, 2020; Crozet, Hinz, et al., 2021), we still lack quantitative evidence on economic sanctions' political impacts.[2] This is unsatisfying, since the economic consequences of sanctions are just a means to achieving political goals. 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 impact, it was not straightforward to define concrete policy objectives the sanctions should achieve, apart from supporting Ukraine by weakening Russia in a broad sense.

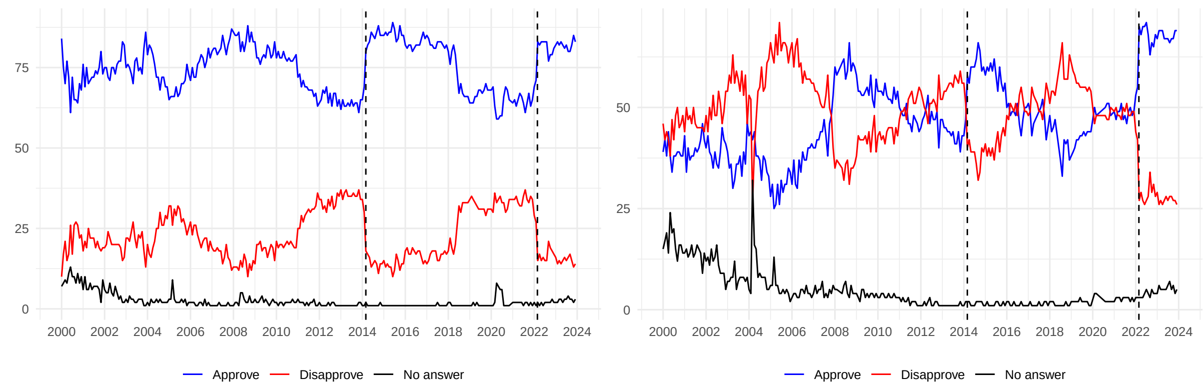
Our paper contributes to closing this — arguably large — research gap. Our focus is on Russia, and the sanctions imposed on the Russian economy after its incursion in Eastern Ukraine and the annexation of the Crimean Peninsula in 2014. The question is whether the sanctions had any effect on the Russian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

Our empirical strategy rests on comparing post-sanction to pre-sanction election results, observed at the *rayon*-level ($\approx$ county). We regress these changes on a measure of regional sanctions exposure. To assess sanctions exposure, we rely on regional trade flows with foreign countries,

[1] See Figures 1a and 1b, which depict approval ratings according to the Levada Center, a Russian polling organization (https://www.levada.ru/en/ratings/) that is widely regarded as independent. The blue lines denote the share of respondents who approve of Putin's and the government's performance, respectively. The red lines denote the share of respondents who disapprove. The dashed vertical lines mark the times in which first sanctions were imposed, i.e. in March 2014 and March 2022, respectively.

[2] One of the few counterexamples is Marinov (2005), who estimates the effect of sanctions on regime change in a cross-country study that compares sanctioned to non-sanctioned countries. Draca et al. (2022) show that sanctions economically hurt the political elite in Iran.

Figure 1: Approval Ratings of President Putin (a) and the Russian Government (b)



(a) **(b)**

Notes: The figures plot approval, disapproval and "no answer" ratings of President Putin (a) and the Russian government (b) from the Levada Center, a Russian polling organization (https://www.levada.ru/en/ratings/). The blue lines denote the share of respondents who approve of Putin's and the government's performance, respectively. The red lines denote the share of respondents who disapprove. The dashed vertical lines mark the times in which first sanctions were imposed, i.e. in March 2014 and March 2022, respectively.

observed on the *federal subject*-level (≈ state).[3] We then define sanctions exposure as the relative difference between observed post-sanctions trade flows and the trade flows that a region would have experienced in absence of sanctions.[4] Counterfactual trade flows are derived from a structural model. Specifically, we feed a general-equilibrium gravity model with information on pre- and post-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. As a consequence, counterfactual trade flows remove all sanctions effects from the observed changes in Russian im- and exports, while keeping simultaneous developments that affected international trade, but were unrelated to sanctions.

Russian regions' counterfactual trade flows serve two purposes in our empirical analysis. First, they allow us to assess regional variation in sanctions exposure, a measure that is not directly observable. Second, the counterfactual measure allows for causal inference in a difference-in-differences setup. That is, instead of simply using time variation between pre- and post-sanction imports and exports — a measure inevitably confounded by simultaneous developments unrelated to sanctions — we rely on the difference between observed and counterfactual flows, i.e. a difference that was caused by the sanctions only.

[3] *Rayons* are nested within *federal subjects*.

[4] This is an exposure measure that captures all sanctions effects correlated with regional trade losses and gains caused by the sanctions. Sanction effects orthogonal to sanctions' 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 capture sanctions' main impact on the average voter.

Ultimately, whether and how sanctions affect electoral support is an open empirical question. First, voters are differently affected by sanctions, with some of them potentially even benefiting in economic terms. 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 so-called "rally around the flag" effect that leads voters to unite behind the government (Kaempfer and Lowenberg, 1988). Both effects could occur simultaneously, leading to political polarization. Of course, voters could also just be indifferent.

To assess regime support, we rely on election results.[5] 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. This allows us to contrast sanction effects on government support, i.e. the regional vote-shares received by President Putin and his party "United Russia," with effects on the support of different groups of opposition parties.

Our results indicate that sanction effects are centered on three political groups. Putin and his party significantly *gain*, 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. Based on these results, and the fact that the Russian Communist party largely campaigns on a nationalist platform stressing the foregone strength of the Soviet Union, the most straightforward explanation is that voters with a nationalist orientation turned to supporting the ruling regime after foreign countries imposed sanctions on Russia. Placebo regressions and event studies rule out pre-trends and support our identification strategy. The effect is remarkably stable across various sub-samples, including larger cities or oil-exporting regions.

Our paper adds to the resurgent literature on sanction effects (Haider, 2017; Crozet and Hinz, 2020; Besedeš, Goldbach, and Nitsch, 2017; Etkes and Zimring, 2015; Dreger et al., 2016; Felbermayr et al., 2020). Ours is the first paper to identify economic sanctions' impact on political support of the sanctioned country's government.[6] With that, our paper also speaks to the literature on the political consequences of economic shocks (e.g. Dippel, Gold, and Heblich,

[5]Like others (e.g. Myagkov, Ordeshook, and Shakin, 2009; Kobak, Shpilkin, and Pshenichnikov, 2016), we find statistical irregularities in the administrative data that hint at election fraud in Russia. However, in our econometric model, this could only bias our results if election fraud structurally increased with sanctions exposure. We will provide evidence for this not being the case. Moreover, we show that election results correspond to other data measuring regime support.

[6]The financial sanctions imposed on Russia hit the economy at large, not only select firms or companies. Our methodological approach takes this into account and, by relying on a structurally defined counterfactual, differentiates our paper from other ongoing attempts to study the electoral effects of sanctions, like Peeva (2022) and Hinz (2023).

2015; Autor et al., 2016; Becker, Fetzer, and Novy, 2017).

Our analysis cannot distinguish between the different types of sanctions imposed on Russia in 2014. However, with our focus on trade losses caused by sanctions, we will mainly capture effects of direct trade restrictions and indirect financial impediments affecting trade, which also had the most significant impact on the Russian economy (Hinz and Monastyrenko, 2022).[7] Since the Russian government responded with an import-embargo on certain food and agricultural products, we will focus our analysis on sanction-induced export losses.[8]

Importantly, this paper should not be understood as an evaluation of whether sanctions are successful in achieving their goal(s). Contrarily, given the lack of research on economic sanctions' political impact, it seems difficult to even define concrete political goals that sanctions could reasonably achieve. Our paper contributes to better understanding the political consequences of economic sanctions, but with a distinct focus on regime support in the sanctioned country. We discuss how our results fit into the broader research on sanctions' effectiveness in the conclusions at the end of the paper.

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

2 Context

Against the backdrop of an ever-escalating conflict between Russia and Ukraine on Ukrainian territory, the international community step-wise imposed economic sanctions on the Russian economy in 2014. Following the refusal of Ukrainian President Viktor Yanukovych to sign the EU-Ukrainian Association agreements in 2013, Ukraine witnessed a series of massive demonstrations. The protests started on November 21, 2013, and, by November 30, had reached hundreds of thousands of protesters. On the February 21, 2014, President Yanukovych fled to Russia and his government was replaced by a Western-oriented administration.

On February 27, Russian troops occupied the Ukrainian peninsula Crimea. The US, EU, and

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

[8] 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 be affected by Russian retaliation and efforts to prop up domestic supply. Thus, we primarily rely on the more exogenous sanction-effects on Russian exports.

other countries reacted with "targeted sanctions" that hit selected Russian individuals with travel bans and asset freezes. On March 18, Russia, in breach of international law, annexed Crimea. In response, a total of 37 countries implemented further sanctions, still targeting Russian individuals and companies selectively.

Subsequently, clashes between Russian(-backed) troops and the Ukrainian army intensified in the Eastern border regions of Ukraine ("Donbas"). The situation escalated 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 and companies; (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 international financial markets (e.g. a ban on issuing bonds with longer maturity).[9] In turn, the Russian government embargoed imports of some agricultural goods, mainly of fresh food, from the sanctioning countries.[10]

3 Data and Empirical Strategy

To estimate the causal effect of sanctions exposure on voting behavior, we take two steps. First, we generate an exogenous measure of exposure to sanctions for each Russian region. This measure relies on a comparison between actually observed trade flows and the trade that a region would have experienced in the absence of sanctions. Second, we adopt a difference-in-differences method that compares voting results across regions before and after the introduction of sanctions, conditional on the degree to which regions where sanctions-exposed.

3.1 Measuring Sanction Effects

Ideally, we would like to measure the overall economic effects of sanctions on Russian voters. However, sanction effects cannot be observed in their entirety, so we rely on a proxy: trade losses caused by sanctions.

Sanction-induced trade losses are a natural candidate to approximate sanction effects for two reasons. First, most sanctions deliberately aim at restricting a sanctioned country's ability to

[9]Among these different measures, the financial restrictions have had the most distinct economic impact, because they increased Russian firms' financing costs, specifically for trade financing, at large (Crozet and Hinz, 2020).

[10]The 48 products in the embargo-list include meat, milk, dairy products, fruits, vegetables, and nuts. Hinz and Monastyrenko (2022) estimate that the embargo increased the price of embargoed products in Russia by 7.7-14.9% in the short term (6 months), and 2.6-8.1% in the medium term (2 years), with a modest spillover effect (0.27%) on non-embargoed goods in the short run.

trade internationally. In our case, the financial sanctions of 2014 affected all Russian companies, increasing their capital costs in general and their trade costs in particular.[11] The remaining sanctions either targeted international trade in specific goods directly, or indirectly affected selected companies' trade costs by freezing these companies' or their owners' foreign assets.[12] This led to an overall decrease in both exports from and imports to Russia. Second, significant shocks to a country's ability to trade internationally are inevitably correlated with the broader economic consequences of sanctions.

Let $T_{r(post)}$ indicate Russian region r's observed trade in the period after the imposition of sanctions, i.e. 2014 and 2015 in our case.[13] Furthermore, let $\tilde{T}_{r(post)}$ denote the (unobserved) trade this region would have had in the absence of sanctions. We define sanctions exposure in region r as:

$$\textit{sanctions\_exposure}_r = -\frac{T_{r(post)} - \tilde{T}_{r(post)}}{\tilde{T}_{r(post)}} \tag{1}$$

The challenge is to determine the counterfactual $\tilde{T}_{r(post)}$. To illustrate this challenge, consider a standard difference-in-differences setting that used observed pre-sanction trade $T_{r(pre)}$ as a counterfactual for $T_{r(post)}$. Of course $T_{r(pre)}$, in our case observed for the years 2012 and 2013, would not be affected by sanctions. Moreover, it would depend on r's time-invariant propensity to trade, which would cancel out by first-differencing. Thus, $T_{r(pre)}$ could serve as proxy for unobserved $\tilde{T}_{r(post)}$ — but only as a poor one. Inevitably, the difference $T_{r(post)} - T_{r(pre)}$ would confound any sanction-effect with simultaneous but unrelated developments. For instance, observed $T_{r(pre)}$ does not incorporate changes in commodity prices or in global demand, or shifts in comparative advantage unrelated to sanctions, that took place between the pre-period 2012–13 and the post-period 2014–15. In contrast, a reliable counterfactual $\tilde{T}_{r(post)}$ should differ from $T_{r(post)}$ only due to sanction effects, but should account for all other developments in international trade that took place simultaneously.

We thus resort to a structural model to derive a measure of $\tilde{T}_{r(post)}$ that differs from $T_{r(post)}$ only because of sanction effects on Russian regions' imports and exports. Specifically, we rely on the well-established gravity model of international trade (Head and Mayer, 2014). Employing the universe of Russian region-to-country and global country-to-country trade flows before and after sanctions, the structural model allows us to decompose international trade flows between

[11] Compare e.g. Crozet and Hinz (2020).

[12] In how far targeted sanctions on specific individuals, e.g. travel bans, affect international trade, does of course depend on those individuals' involvement in international business.

[13] We compute the measure for both exports and imports. Our main specification, as already explained, will focus on the export shock.

all trading partners $\mathbf{L}$ into importer-specific, exporter-specific, and trading-partner-specific determinants. More precisely, trade flows between an origin o (that exports) and a destination d (that imports) are expressed as a function of supply (at o) and demand (at d), overall easiness to trade for o and for d, respectively, and the idiosyncratic ability to trade between two specific partners. The key equation that describes the model is

$$X_{odt} = \frac{Y_{ot}}{\Omega_{ot}} \cdot \frac{X_{dt}}{\Phi_{dt}} \cdot \phi_{odt} \qquad \text{with} \qquad o \in \mathbf{L};\;\; d \in \mathbf{L};\;\; \mathbf{L} = \{l_1, l_2, \cdots, l_n\}, \tag{2}$$

$$\text{where} \qquad Y_{ot} = \sum_{\ell \in \mathbf{L}} X_{o\ell t}, \quad X_{dt} = \sum_{\ell \in \mathbf{L}} X_{\ell dt},$$

$$\text{and} \qquad \Omega_{ot} = \sum_{\ell \in \mathbf{L}} \frac{X_{\ell t}}{\Phi_{\ell t}} \cdot \phi_{o\ell t}, \quad \Phi_{dt} = \sum_{\ell \in \mathbf{L}} \frac{Y_{\ell t}}{\Omega_{\ell t}} \cdot \phi_{\ell dt}.$$

All partners l trade with each other on the world market. X_{odt} are exports from an origin o to a destination d at time t.[14] Y_{ot} are all exports sales at an origin, X_{dt} is the overall import demand at the destination. Two crucial terms are Ω_{ot} and Φ_{dt}, the so-called outward and inward multilateral resistance terms. They capture, respectively, the origin's general propensity to export and the destination's general propensity to import (i.e. their relationship to the world market). ϕ_{odt} is an origin-destination-pair specific term that summarizes bilateral trade frictions between o and d at time t. Higher frictions translate into a lower ϕ_{odt}.

Equation (2) guides our empirical strategy. If some trading partner l_A imposes sanctions on some l_B at time t, this decreases ϕ_{odt} for this specific set of trading partners.[15] In other words, their bilateral trade frictions increase. Within the model, sanctions do not directly affect bilateral trade costs for other l's. However, the international trade network adjusts to any change in ϕ_{odt} via the other components of the model, specifically Ω_{ot} and Φ_{dt}, as well as Y_{ot} and X_{dt}. Hence, a single bilateral shock to trade frictions anywhere in the world has an effect on trade flows everywhere.

In our case, when focusing on Russian export losses, sanctions decrease ϕ_{odt} between Russian regions o and sanctioning countries d (and vice versa), first and foremost. In turn, Ω_{ot} and Φ_{dt} adjust for all participants in international trade, according to Russian o's and sanctioning d's ability to divert trade to other partners. In other words — and ceteris paribus — other countries become relatively more attractive trading partners after Russian regions' trade frictions with sanctioning countries increase. In any case, all Russian regions o and and all sanctioning

[14] A given partner l is at the same time origin of exports to all other partners in $\mathbf{L}$ and destination for all other l's exports. Trade flows between o and d may be zero or unobserved.

[15] That is, sanctions from l_A targeting l_B increase $\phi_{o=l_A,d=l_B,t}$ and $\phi_{o=l_B,d=l_A,t}$.

countries d will be somewhat worse off, as their average accessibility decreases. Accordingly, overall sales Y_{ot} and expenditures X_{dt} must adjust to the new trade equilibrium.

Through the lens of this gravity model, predicting how Russian regions' trade would have looked like in the absence of sanctions boils down to determining how the bilateral trade costs ϕ_{odt} would have looked like in the post-period. In particular, this holds in the short run. Sanctions act as an unexpected shock to ϕ_{odt}, that leads to adjustments based on pre-determined characteristics of all trading partners l. In the longer run, a new equilibrium may emerge endogenously, but for the initial years after sanctions were imposed, pre-sanction trade costs $\phi_{od(pre)}$ can be regarded as a reliable proxy for counterfactual trade costs $\tilde{\phi}_{od(post)}$ in the absence of sanctions.[16]

Hence, we employ the structural gravity model to assess counterfactual international post-sanction trade flows by holding pre-sanction bilateral trade costs constant.[17] Moreover, we account for adjustments in all other parameters to the changing ϕ_{odt}. The resulting $\tilde{T}_{r(post)}$ allows us to extract from observed trade flows the variation caused by the sanctions, but unrelated to simultaneous changes in the international trading environment, c.f. Equation (1).

To derive counterfactual $\tilde{T}_{r(post)}$, we rely on regional-level trade data from the "Federal Customs Service of Russia".[18] 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 (very roughly comparable to a US State). Disregarding occupied Crimea and Sevastopol, there are 83 Federal Subjects. For 75 of these Federal Subjects, we have precise and reliable information on their imports from and exports to the rest of the world.[19] We augment the regional data with international trade data covering imports and exports for the universe of countries other than Russia.[20] The final dataset covers the years 2012 to 2015, i.e. two years pre- and post sanctions implementation. The dataset thus contains information on all the bilateral trade flows between 124 countries and 75 Russian federal subjects.

[16]As a matter of fact, bilateral trade-frictions ϕ_{odt} constantly change. e.g. due to the establishment or closure of highways, ports, etc. However, to significantly divert trade-flows internationally, major changes in ϕ_{odt} are required, e.g. by the signing of free-trade-agreements, imposition of tariffs — or sanctions.

[17]We rely on international flows exclusively. We therefore split up Russia's total trade flows into exports and imports by its origin or destination regions, hence effectively treating any Russian Federal Subject like a country.

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

[19]We disregard observations from the war-torn Chechen Republic. Moreover, we drop information from a few sparsely populated subjects that report trade figures less than 6 times in the 24 months of the pre-sanction period, c.f. Figure 2.

[20]For this, we use the UN Comtrade database. See http://comtrade.un.org, for the years 2012 to 2015. We drop small and infrequent reporters from the sample, i.e. countries trading with less than 10 percent of all possible destinations in any year.

The structural model, equation (2), can be estimated as

$$X_{odt} = \exp\left(\Psi_{ot} + \Theta_{dt} + \phi_{odt}\right) + \epsilon_{odt} \tag{3}$$

using a Poisson Pseudo-Maximum Likelihood estimator, where Ψ_{ot}, Θ_{dt} and ϕ_{odt} are origin $\times$ time, destination $\times$ time and origin $\times$ destination $\times$ pre/post fixed effects.[21] Estimated $\widehat{\Psi}_{ot}$ and $\widehat{\Theta}_{dt}$ assess export sales Y_{ot} and import demand X_d, as well as multilateral resistance terms Ω_{ot}, Φ_{dt}, respectively, while $\widehat{\phi}_{odt}$ measures bilateral trade frictions in the pre- or post-sanction period.

With Equation (3), all parameters are assessed for the *pre*-sanction and the *post*-sanction period separately. Inserting the pre-sanction bilateral frictions back into Equation (2) and combining it with post-sanction measures of all other parameters, we can "clean" the post-sanction estimates from sanction effects in an iterative process. Eventually, this allows to determine counterfactual trade flows unaffected by sanctions, but affected by all simultaneous developments impacting sales and demand. Building on Crozet and Hinz (2020), we compute counterfactual trade flows $\tilde{T}_{r(post)}$ in a five-step procedure.[22]

1. Estimate $\widehat{\phi}_{od(pre)}$. Use pre-sanction data (2012, 2013) to estimate equation (3). These bilateral trade costs from the pre-sanction period will be held constant to assess post-sanction counterfactuals.

2. Estimate $\widehat{\Psi}_{ot}$ and $\widehat{\Theta}_{dt}$ for the post-sanction period using data from 2014 and 2015 for equation (3). The estimated multilateral resistance terms are affected by sanctions, and simultaneous developments.

3. Compute Y_{ot} and X_{dt} using post-sanction data (2014, 2015). These export sales and import demand figures reflect actual events, including sanctions and simultaneous developments.

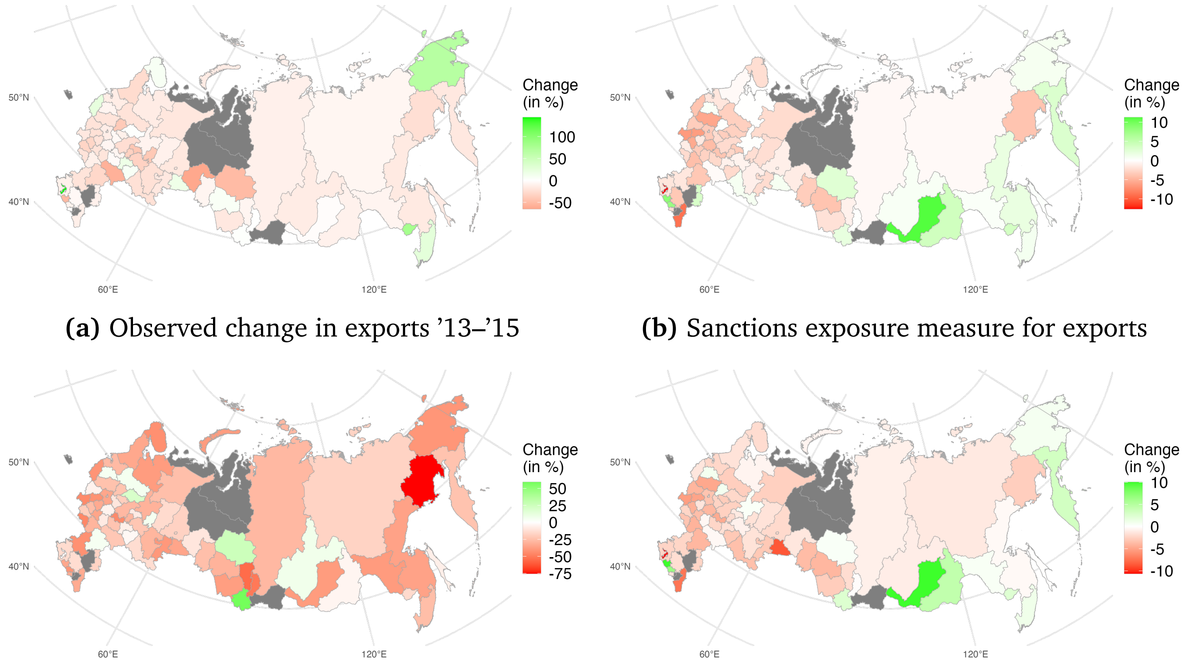
4. Assess counterfactual, Conditional General Equilibrium (CGE) trade flows $\tilde{X}_{ot}^{\text{CGE}}$. Specifically, derive CGE-multilateral resistance terms $\tilde{\Omega}_{ot}$ and $\tilde{\Phi}_{dt}$. Use estimates from steps 1 and 3 in equation (2) to iteratively compute counterfactual $\tilde{\Omega}_{ot}$ and $\tilde{\Phi}_{dt}$ for the post-sanction period, thus "rewinding" the changes from $\phi_{od(pre)}$ to $\phi_{od(post)}$.

5. Derive counterfactual, General Equilibrium (GE) trade flows $\tilde{X}_{odt}^{\text{GE}}$. Use estimates for

[21] 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.

[22] See Appendix B for more details.

Figure 2: Spatial Distribution of Regional Sanctions Exposure



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

(b) Sanctions exposure measure for exports

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

(d) Sanctions exposure measure for imports

$\widehat{\Psi}_{ot}$ and $\widehat{\Theta}_{dt}$ from step 2 as well as $\tilde{\Omega}_{ot}$ and $\tilde{\Phi}_{dt}$ from step 4 in equation (2) to compute counterfactual export sales $\tilde{Y}_{ot}$ and import demand $\tilde{X}_{dt}$.

Iterate between step 4 and step 5, thus updating counterfactual multilateral resistance terms and corresponding export sales and import demand, until convergence. The resulting trade flows are General Equilibrium (GE) quantities because — in contrast to those assessed in the previous steps — they account for all repercussions of bilateral sanctions on the global market. Consequently, they are *neither* affected by the change in bilateral trade costs caused by the sanctions, *nor* the resulting change in multilateral resistance terms (i.e., change in openness to trade), *nor* related changes in the international trade network.

Now, we can determine counterfactual trade flows $\tilde{T}_{r(post)} = \sum_{t\in(post)} \sum_{\ell\in d} \tilde{X}^{\text{GE}}_{r\ell t}$ and, in turn, $sanctions\_exposure_r$ as in equation (1).[23] All simultaneous changes in international trade unrelated to sanctions affect both observed $T_{r(post)}$ and structurally derived $\tilde{T}_{r(post)}$ alike, hence, $T_{r(post)} - \tilde{T}_{r(post)}$ cancels them out.

Figure 2 shows the resulting spatial distribution of *sanctions_exposure$_r$* (right panels) and the underlying change in observed trade flows (left panels), both for export losses (upper panels)

[23] The equivalent measure for the import side is computed as $\tilde{T}_{r(post)} = \sum_{t\in(post)} \sum_{\ell\in o} \tilde{X}^{\text{GE}}_{\ell r t}$.

and for import losses (lower panels). Interestingly, some regions can increase their international trade in response to the sanctions, e.g. by substituting sanctioning trading partners with non-sanctioning ones. We will look into these regions more closely in our empirical analysis but for the sake of brevity, we will subsequently refer to sanctions' dominant impact on Russian regions' international trade as trade losses. Obviously, regional import losses are correlated with regional export losses. As already explained, we will focus our analyses on export losses caused by the sanctions, since they are not confounded by Russian retaliation. We will carefully examine the spatial patterns depicted in Figure 2, e.g. the relatively higher exposure in regions closer to the sanctioning countries.

As is obvious in Figure 2, there is a distinct regional pattern in $sanctions\_exposure_r$. Based on the structural model, regional variation stems from three sources that precede the treatment. First, a region's industrial structure determines whether it is hit by sanctions, or not (so much). Second, a regions' specialization in trade with sanctioning countries matters. Third, a region's ability to divert trade to new partners matters for its exposure. In the short run, all these regional characteristics are accounted for in the difference-in-differences specification, such that the resulting variation in $sanctions\_exposure_r$ can indeed be asserted to the sanctions effect itself.

3.2 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.[24] 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 for around 100,000 electoral wards, which we map into a time-consistent spatial framework of about 2300 "rayons" (administrative districts), nested in 75 "federal subjects" (regions).[25]

We observe votes cast for every party (running for parliament) or candidate (for the presidency) 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

[24]The data was previously publicly available at izbirkom.ru. At the time of writing, the website was not accessible anymore outside the Russian Federation.

[25]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. When cities consist of several rayons, we merge them into one observation.

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 ambiguous political agenda, or single-issue parties like the Pensioners' Party or the Greens.[26] Moreover, we calculate election turnout.

Independent election observers like the OSCE have persistently criticized Russian elections over various irregularities.[27] In this respect, relying on electoral data at a very granular level (around 100,000 wards) has two advantages. First, it is less likely to suffer from aggregation fraud.[28] Second, it allows us to investigate statistical irregularities in the election data like an unusual clustering of even numbers around meaningful values, like 50 or 75 percent.[29] In our subsequent analysis, panel econometrics will absorb regional variation in such irregularities. Remaining variation over time is unrelated to sanctions exposure, as we will show. Although there is good reason to assume that the government interferes with democratic elections in Russia, there is no indication of election fraud structurally increasing or decreasing with our measure of $sanctions\_exposure_r$. Thus, we are confident in our use of election data as an indicator for changing regime support in reaction to a Russian region's exposure to sanctions.

3.3 Main Difference-in-Differences Model

To identify sanction effects on regime support, and on voting behavior more broadly, we exploit cross-sectional regional variation in Russian regions' $sanction\_exposure_r$ computed above, as well as time-variation in the support for different parties and candidates in elections pre- and post-sanctions.

Since the imposition of sanctions fell amidst the election cycle of both the presidential and

[26] Empirical results are robust re-classifying arbitrary parties or candidates.

[27] 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, Petrova, and Zhuravskaya (2011), and Kobak, Shpilkin, and Pshenichnikov (2016).

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

[29] See Section 4.5.

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 us to test the common trends assumption.

Our data is organized as a stacked panel of first differences. Our main specification is

$$\Delta \text{Voting}^{g}_{ir,t} = \alpha + \beta \; \textit{sanctions\_exposure}_r + \Gamma \; \Delta \text{X}_{ir,t} + \epsilon_{ir,t} \tag{4}$$

where $\Delta\text{Voting}^{g}_{ir,t}$ is the change in election outcomes for the group of parties (or candidates) g in rayon i nested in region r between t_0 and t_{+1} for treatment assessment, or between t_{-1} and t_0 for the placebo regressions.

Control variables $\text{X}_{ir,t}$ include regional demographics (population, migration, employment rate), labor force characteristics (age structure, qualification) and industry structure (sectoral employment shares).[30] In addition, we include a binary control for presidential elections.[31]

Throughout the paper, we report least-square standard errors clustered at the level of federal subjects r. To account for our treatment variable $\textit{sanctions\_exposure}_r$ being derived from estimating Equation (3), we bootstrap standard errors by the same clusters as reference.

Regional variation in $\textit{sanctions\_exposure}_r$, used for identification in Equation (4) ultimately stems from three sources of variation that were determined pre-treatment, i.e. before the sanctions were imposed. The first one is variation in regional industry structure, which determines the relevance of international trade for the local economy in general. The second one is regional specialization in trade with specific partners, which makes some regions more exposed to sanctions than others. The third one is a region's ability to divert trade to non-sanctioning countries. All these time-consistent confounders cancel out, so that $\textit{sanctions\_exposure}_r$ only depends on the time-varying deviation of observed trade-flows from counterfactual flows in the absence of sanctions.

3.4 Exclusion Restriction

Our identification strategy rests on two assumptions. First, like always, we assume that the structural model guiding our analysis is correct. Second, within the model framework, the crucial

[30]The data for the variables is taken from the Statistical Office of the Russian Federation. See Appendix A.2 for descriptive statistics on all the variables used.

[31]Note that, in this framework, this control captures potential differences in trends (rather than levels) between presidential and parliamentarian elections.

assumption is that between the periods 2012–2013 and 2014–2015, bilateral trade frictions ϕ_{odt} for all o and d change only due to the 2014 sanctions.

In its narrowest sense, this assumption is 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 applied perspective, minor violations of this assumption can be tolerated as long as they have no significant impact on the results. Since we rely on a general equilibrium model, this boils down to two restrictions: One, there may be no simultaneous change in bilateral trade frictions ϕ_{odt} of relevant magnitude for any country pair and two, there may be no simultaneous change in ϕ_{odt} that affects Russian regions in a way similar to the sanctions. Both assumptions must hold for the period 2012/13 to 2014/15.

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. Indeed, a few Free Trade Agreements (FTAs) were signed between 2012 and 2015. If these FTAs had a significant impact on ϕ_{odt}, or if they had a particular impact on Russian regions, this could potentially bias our results.

Over our period of analysis, trade flows between countries that formed 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 Russia's trade through trade diversion, as both were part of the Soviet Union and thus share 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.

Simultaneously, five countries formally joined the WTO, and two of the new members, Tajikistan and Kazakhstan, share historical ties with Russia. Indeed, while only 0.8% of world trade was affected by the new entrants, both Tajikistan and Kazakhstan are moderately important trading partners for the Russian Federation: roughly 3.7% of Russian imports and exports relates to these countries. However, their accession to the WTO did not affect bilateral trade costs with Russian regions, since they had been members of the Eurasian Economic Union before.

At first glance, Croatia's accession to the EU might seem problematic.[32] However, the Croatian economy was already integrated into the Single Market before it formally joined the Union

[32]With its new member, the sanctioning coalition increased its ability to affect Russia (Chowdhry et al., 2024).

in 2013. 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 Adriatic country.

Overall, the 2014 sanctions against the Russian Federation are by far the largest shock to bilateral trade costs in the 2012–2015 period. Specifically, no simultaneous development had a similar impact on Russian regions' bilateral trade costs. Accordingly, our regression results are insensitive to excluding the countries mentioned above (Moldova, Georgia, Tajikistan, Kazakhstan, and Croatia) from the calculation of counterfactual trade flows, as can be seen in Appendix C.1.[33] We are thus convinced that these minor violations of our identifying assumption cannot meaningfully bias our estimates.

4 Results

4.1 Main Results

We now turn to estimating our difference-in-differences model described in Equation (4). Our focus is on the effect of $sanction\_exposure_r$ measured via regional export losses. Corresponding results based on import losses, i.e. $sanction\_exposure_r^{imp}$, can be found in Appendix D, Table A4.

Table 1 reports results for different party outcomes and for overall turnout, with $\Delta\text{Voting}_{ir,t}^{g}$ 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 $\Delta\text{Voting}_{ir,t}^{g}$. Columns (1)–(4) successively include additional regional-level control variables. In (1), we condition on baseline demographics like log of population and eligible voters. In (2), we add labor force characteristics like age and qualification. Column (3) additionally controls for regional industry structure, i.e. employment shares in 12 different industries. In Column (4), we add controls for start-of-period outcome levels, and, in the case of party outcomes, for changes in turnout.[34] To facilitate comparison, Column (5) repeats results from our preferred specification in Column (4) with standardized coefficients (for outcomes with a mean of zero and a standard deviation of one). All estimations include election-type fixed effects.

We report least-square standard errors, clustered on the regional level of 75 Federal Subjects, in parentheses. To account for the errors-in-variables that result from using a structural model to assess counterfactual trade flows, we report in brackets standard errors that were bootstrapped

[33]Point estimates increase very slightly and standard errors change only marginally when using this alternative measure of sanctions exposure, omitting countries that signed FTA's over our period of analysis.

[34]More details on the control variables can be found in Appendix A.2.

Table 1: Sanction Effects on Russian Elections

	(1)	(2)	(3)	(4)	(5)
	Effects of sanction_exposure$_r$				
Δ regime	0.576**	0.565**	0.575***	0.486***	5.070***
	(0.229)	(0.214)	(0.170)	(0.103)	(1.074)
	[0.236]	[0.229]	[0.225]	[0.146]	[1.519]
Δ loyal	-0.032	-0.047	-0.031	-0.005	-0.108
	(0.098)	(0.081)	(0.071)	(0.040)	(0.798)
	[0.106]	[0.093]	[0.097]	[0.055]	[1.116]
Δ nationalist	-0.110*	-0.081	-0.076	-0.078	-1.906
	(0.065)	(0.063)	(0.062)	(0.054)	(1.316)
	[0.070]	[0.070]	[0.082]	[0.072]	[1.750]
Δ communist	-0.396***	-0.399***	-0.406***	-0.330***	-5.833***
	(0.139)	(0.136)	(0.129)	(0.072)	(1.279)
	[0.148]	[0.149]	[0.166]	[0.108]	[1.910]
Δ liberal	-0.010	-0.012	-0.032	0.006	0.186
	(0.047)	(0.040)	(0.029)	(0.011)	(0.372)
	[0.049]	[0.042]	[0.042]	[0.015]	[0.513]
Δ other	-0.028	-0.026	-0.030	-0.032	-2.181
	(0.025)	(0.019)	(0.022)	(0.022)	(1.518)
	[0.026]	[0.021]	[0.028]	[0.028]	[1.887]
Δ turnout	0.184	0.145	0.030	0.035	0.320
	(0.201)	(0.200)	(0.184)	(0.189)	(1.746)
	[0.222]	[0.227]	[0.247]	[0.254]	[2.350]
Controls	Baseline	+ labor force	+ industry	+ political	= (4)
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*) Least-square standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. Bootstrapped standard errors based on 1000 replications in brackets. *** p<0.01, ** p<0.05, * p<0.1, on the basis of the least-square SE.

on the same clusters.[35] Bootstrapped SE are of course larger, but not substantially so. Throughout the paper, we report *p*-values and confidence intervals on the basis of the least-square-SE.

The results consistently show that the sanctions imposed in 2014 have a significant impact on subsequent elections in Russia. Regime support, i.e. the vote share of President Putin and his party "United Russia", increase significantly with regional sanctions 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

[35]Each estimate is based on 1000 replications.

regime by $(0.029 \times 0.486 \times 100\ =)$ 1.4 percentage points. This is economically meaningful. Starting from high pre-sanction levels, the governing regime was able to increase its overall support by around 6.3 percentage points over our period of analysis. Hence, a one standard deviation increase in $sanction\_exposure_r$ explains roughly 22 percent of the general increase in regime support. Thus for an average Russian rayon, regime support increases by 13 percent due to the sanctions.

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 a supposed 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 significantly 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 insignificant turnout results speak against opponents of the regime just not participating in elections. Indeed, turnout tends to be somewhat higher in sanction-exposed regions.[36]

4.2 Common Trends

The validity of all our regression results depends on the common trends assumption to hold. 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

[36]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).

Table 2: Placebo Effects on Pre-Sanction Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δ regime	Δ loyal	Δ nationalist	Δ communist	Δ liberal	Δ other	Δ turnout
sanctions_exposure$_r$	0.019	-0.069	0.040	-0.029	0.030	0.006	0.184
	(0.148)	(0.079)	(0.051)	(0.106)	(0.033)	(0.007)	(0.155)
Controls	all	all	all	all	all	all	all
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 two elections preceding the sanctions. (*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.

the point estimates are small and statistically insignificant. This clearly supports our identification strategy.[37]

Additionally, we perform event studies, re-estimating our previous regressions in a fixed-effects model. We stack election outcomes observed in levels in t_{-1} and t_0, both before the sanctions, and in t_{+1} that were affected by the sanctions. We include rayon-level fixed effects and covariates in levels, corresponding to specification (4) of Table 1. Time-invariant *sanction_exposure*$_r$ is interacted with period-dummies an evaluated against the last pre-sanction elections in t_0. Therefore, we estimate Equation (5) as follows

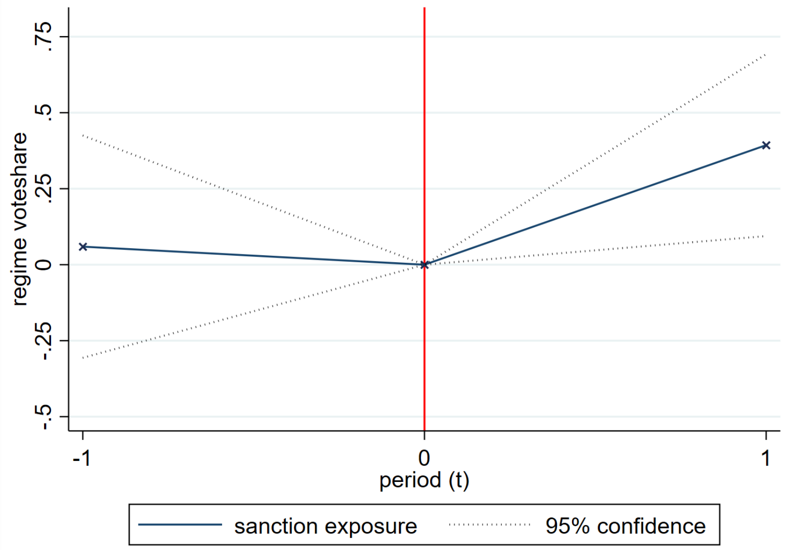
$$\begin{aligned} \text{Voting}^g_{ir,t} = \alpha + \lambda_t + \beta_1\ \textit{sanction\_exposure}_r * \lambda_{t=-1} \\ + \beta_2\ \textit{sanction\_exposure}_r * \lambda_{t=+1} + \Gamma\ \mathrm{X}_{ir,t} + \epsilon_{ir,t} \end{aligned} \tag{5}$$

and report on treatment coefficient β_2 and placebo coefficient β_1. Figure 3 summarizes our main finding in an event-study graph.

Figure 3 shows that the effect of *sanction_exposure*$_r$ on regime support 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. Corresponding event-study graphs for all other election outcomes can be found in Appendix C, Figure A1. They all confirm our previous results.

[37]Corresponding placebo tests for the import shock can be found in Appendix D, Table A5.

Figure 3: Event Study: The Effect of Sanctions Exposure on Regime Support



Notes: The figure plots point estimates for interaction effects of *sanction_exposure*$_r$ with a categorical period-indicator. The outcome is the vote share obtained by President Putin and his party. The omitted category is sanction exposure in t_0, i.e. the last elections before the sanctions were imposed. Previous election results in t_{-1} and elections under sanctions in t_{+1} are evaluated against this reference point, conditional on the same set of controls as before. Corresponding confidence intervals are based on least-square SE, clustered at the level of 75 *Federal Subjects*.

4.3 Structural Model vs. Observed Trade Flows

Deriving counterfactual trade flows from a structural model does not only allow for causal inference, it also helps to center the analysis on the political effects of *sanctions*, net of contemporaneous developments. To exemplify the advantages of a full general-equilibrium gravity model over using variation in observed trade flows only, we contrast our results to estimates obtained for alternative measures of *sanction_exposure*$_r$.

First, following a naïve difference-in-differences approach, one might use pre-sanction trade flows as a counterfactual for post-sanction trade flows. Indeed, taking first differences $\Delta Trade$ for either imports or exports should be a measure of sanction effects — but a rough one. Instead, one might concentrate on observed changes in imports from or exports to sanctioning countries, i.e. $\Delta Trade(sanctioning)$. This should be less affected by world-market effects, but might still capture supply or demand shocks common to all sanctioning countries, while unrelated to sanctions. Thus, a structural model is needed to separate sanction effects from simultaneous developments of the international trading environment.

Table 3 compares our structurally derived measure of *sanction_exposure*$_r$ to alternative measures. To facilitate comparison, all measures are standardized to have a mean of zero and a standard deviation of one. Every cell reports another treatment coefficient, corresponding to our main

Table 3: Comparison with Alternative Sanction Measures

	(1)	(2)	(3)	(4)
	Δ regime		Δ turnout	
	Exports	Imports	Exports	Imports
$sanction\_exposure_r^{trade}$	0.014***	0.011***	0.001	-0.001
	(0.003)	(0.003)	(0.006)	(0.005)
Δ Trade	0.005*	-0.001	-0.004	0.011**
	(0.003)	(0.004)	(0.006)	(0.006)
Δ Trade (sanctioning)	0.009***	-0.000	-0.002	0.009**
	(0.003)	(0.004)	(0.006)	(0.004)
Controls	+ political	+ political	+ political	+ political
Election-FE	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396

Notes: (*a*) Each cell reports results from a separate regression. (*b*) Columns refer to different outcome variables observed at the *rayon*-level, and to sanction-measures based on either im- our exports. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) Rows refer to alternative measures of a Russian region's exposure to sanctions. (*d*) 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. (*e*) Standard errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

specification (4) of Table 1. Columns (1) and (2) assess the impact of the different measures of sanctions effects on regime support; Columns (3) and (4) report sanction effects on turnout. Columns with odd numbers contrast export measures to import-based measures, reported in columns with even numbers.

Our benchmark for comparison is *sanction_exposure*$_r$, which repeats, for the case of exports, Table 1, Column (4) as standardized coefficient and, for the case of imports, Table A4 in Appendix D. Focusing on column (1), we see that the alternative measures point in a similar direction as our main effect. Using the observed change in Russian regions' exports $\Delta Trade$ instead shows a smaller effect that is less precisely estimated. This is no surprise, since $\Delta Trade$ contains *sanction_exposure*$_r$, but many simultaneous developments as well. Restricting the variation to $\Delta Trade(sanctioning)$ comes closer to the original effect.[38] However, it is important to note that this is just by chance. Apparently, Russian regions' observed exports to sanctioning countries were mainly affected by the sanctions. In different settings, this could well be different.

Accordingly, the picture changes when looking at import-based measures in Column (2). The effects of *sanction_exposure*$_r$ are still consistent, while effects of observed trade flows differ significantly. Apparently, after the sanctions were imposed in 2014, Russian regions' imports

[38]Smaller point estimates suggest that regional sanction exposure is overstated by $\Delta Trade(sanctioning)$. Since *sanction_exposure*$_r$ accounts for trade diversion mitigating observed losses in exports to sanctioning countries, it is plausible that comparatively lower levels of exposure lead to higher point estimates on the same outcome.

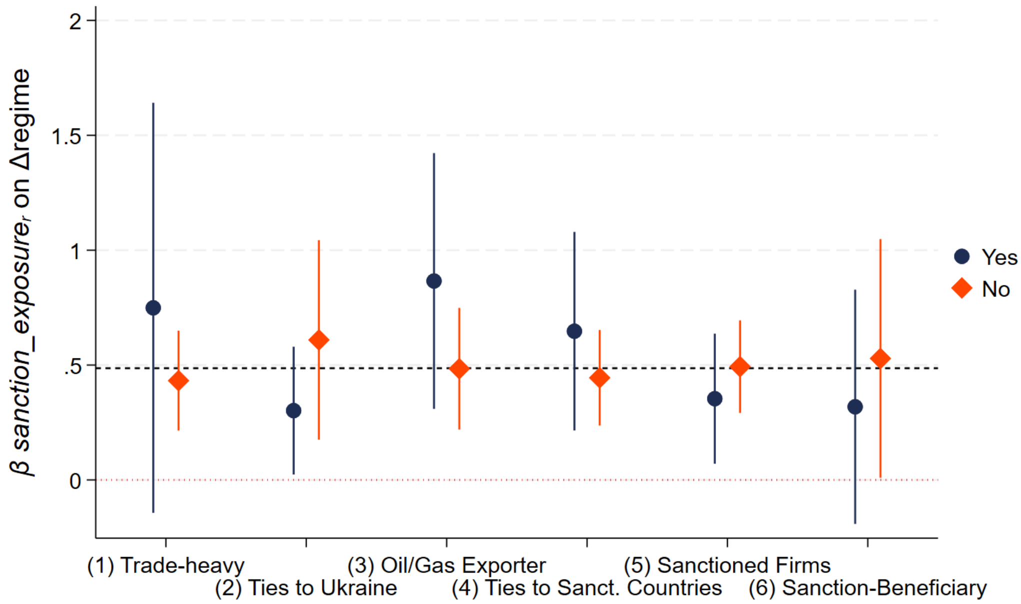
from other countries — and from sanctioning countries in particular — were affected by factors unrelated to sanctions, that had differential impacts on voting behavior. The same holds when comparing Columns (4) and (3).

Overall, Table 3 shows that using observed changes in Russian regions' im- or exports – relying on observed pre-sanction trade as counterfactual – does not lead to robust estimates of the effects of regional sanctions exposure. Thus, to reliably infer on sanction effects, it is not sufficient to rely on observational data only. One needs a structural model that disentangles sanction-induced trade losses from simultaneous developments in Russian regions' foreign trade relationships.

4.4 Effect Heterogeneity

We now turn to exploring effect heterogeneities to further qualify the sanction effect, detect potential mechanisms, and rule out further sources of bias. We center the discussion on heterogeneities with respect to sanction effects on regime support. Results on other outcomes are reported in Appendix C.3.

Figure 4: Regional Effect Heterogeneity: Trading Patterns



Notes: The figure plots treatment coefficients for *sanction_exposure$_r$*. Corresponding confidence intervals (95%) are based on least-square SE, clustered at the level of 75 *Federal Subjects*. The outcome is observed change in vote shares of President Putin and his party. In specification (1) the sample is split at the median for pre-sanction (Imports+Exports)/GDP. In specification (2) the sample is split at the median for (Imports+Exports) from or to Ukraine / total (Imports+Exports). In specification (3) the sample is split at the 75-percentile for the share of oil- and gas-exports in all exports. In specification (4) the sample is split at the median for (Imports+Exports) from or to the 37 sanction countries / total (Imports+Exports). In specification (5) we split according to whether any firm directly sanctioned is located in a rayon. In specification (6) we split according to whether a region experiences export gains caused by the sanction. The dashed line marks the original point estimate.

First, we split the sample according to regional trade patterns, and repeat the estimations from

Column (4) of Table 1 on split samples. We report treatment coefficients for $sanction\_exposure_r$ in Figure 4. In specification (1), we split regions according to the relevance of international trade for the regional economy, i.e. (imports+exports)/regional GDP. Although our measure of $sanction\_exposure_r$ is based on export losses, we regard it to be a proxy for overall sanction effects. The results from (1) support this interpretation. Even in regions that do not heavily depend on international trade, the sanction effect is still measurable. In trade-heavy regions, the effect is somewhat larger, but with higher variance, which may relate to the heterogeneity of Russian regions' trading partners.

Our identification rests on one crucial assumption, i.e. that in the short run, Russian regions' foreign trade was mainly affected by the sanctions. Technically, this implies that bilateral trade frictions ϕ_{odt} are not affected by simultaneous shocks, as discussed in Section 3.4. However, the conflict with Ukraine itself could have distorted trade patterns between Russia an Ukraine, which might bias our results. Thus, in specification (2), we split the sample according to whether Russian regions had strong trade-ties to Ukraine before the sanctions were imposed. If anything, sanctions have a stronger effect in regions with weak ties to Ukraine.

Deriving $sanction\_exposure_r$ from a structural model ensures that sanction effects are not confounded by simultaneous developments on the world market. Indeed, over our period of analysis, there have been substantial changes to the oil price, as well as to the exchange rate of the Russian Ruble. Thus, it is reassuring to see in specification (3) that the effect of $sanction\_exposure_r$ is pretty much the same in regions that do not primarily export oil or gas.

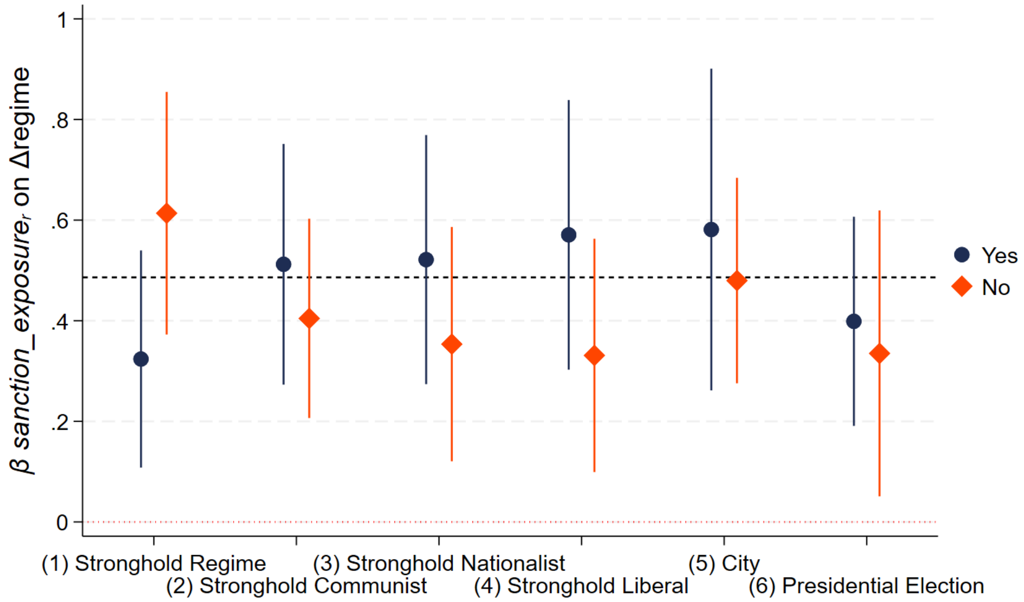
Somewhat related, specification (4) splits the Russian regions according to whether their export share to sanctioning countries is high or low. Intuitively, the sanctions effect is larger for regions strongly tied to sanctioning countries (often regions exporting a lot of gas and oil, as in specification (3)), but it is not dominated by these regions.

In specification (5), we split the sample by whether a region hosts a firm that is directly affected by the sanctions, i.e. on the EU or US Sanctions lists. This turns out not to make any difference, again confirming that we measure an overall sanction effect.

In specification (6), we split the sample between the regions that experience export losses caused by sanctions (the majority), and regions seeing an increase in exports due to the sanctions. Even the regions benefiting from the sanctions show a treatment effect, although with higher variance.

Next, in Figure 5, we turn to exploring heterogeneities in the political response to sanctions.

Figure 5: Regional Effect Heterogeneity: Politics



Notes: The figure plots treatment coefficients for *sanction_exposure$_r$*. Corresponding confidence intervals (95%) are based on least-square SE, clustered at the level of 75 *Federal Subjects*. The outcome is observed change in vote shares of President Putin and his party. In specification (1) the sample is split at the median for pre-sanction vote shares received by regime parties and candidates. In specification (2) the sample is split at the median for pre-sanction vote shares received by Communist parties and candidates. In specification (3) the sample is split at the median for pre-sanction vote shares received by Nationalist parties and candidates. In specification (4) the sample is split at the median for pre-sanction vote shares received by liberal opposition parties and candidates. In specification (5) we split into cities with at least 100k inhabitants, and the rest. In specification (6) we split by election type. The dashed line marks the original point estimate.

Again, we perform sample splits as in Figure 4 above. In specification (1)–(4), we split the sample according to pre-sanction voting results. Apparently, the sanctions had a stronger effect in regions that were previously supportive of opposition parties. This confirms our interpretation that the regime wins at the expense of the Communists and Nationalists, primarily.

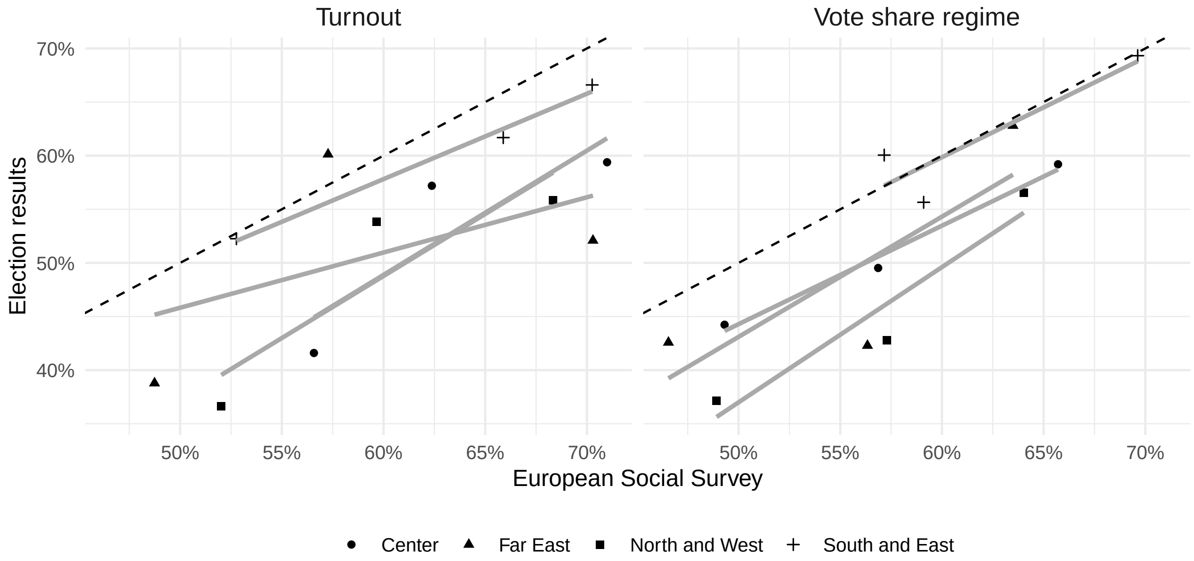
We carefully checked for potential polarization effects, i.e. the liberal opposition winning in certain regions. Results in specification (5) split between city districts and rural districts, and are reported here by way of example. For neither party outcome reported in Table 1 we find substantial effect heterogeneities (see also Appendix C.3). In cities or elsewhere, it is always the regime that benefits from *sanction_exposure$_r$*.

Eventually, specification (6) shows that the effect is very similar for presidential and parliamentary elections. Altogether, the absence of any meaningful source of regional heterogeneity clearly speaks against unobserved confounders that might bias our results.

4.5 Reliability of Russian Election Data and Potential Fraud

Eventually, when looking at Russian election outcomes, one might be concerned about the reliability of administrative election data in general, and about election fraud biasing our results in particular.

Figure 6: Correlation between Administrative Data and European Social Survey Results.



Notes: The figure shows turnout and vote shares for the regime as observed in the election data for the 2007, 2011 and 2016 parliamentary elections on the y-axis, and the corresponding European Social Survey questions on voting behavior in rounds 4, 6, and 8 on the x-axis.

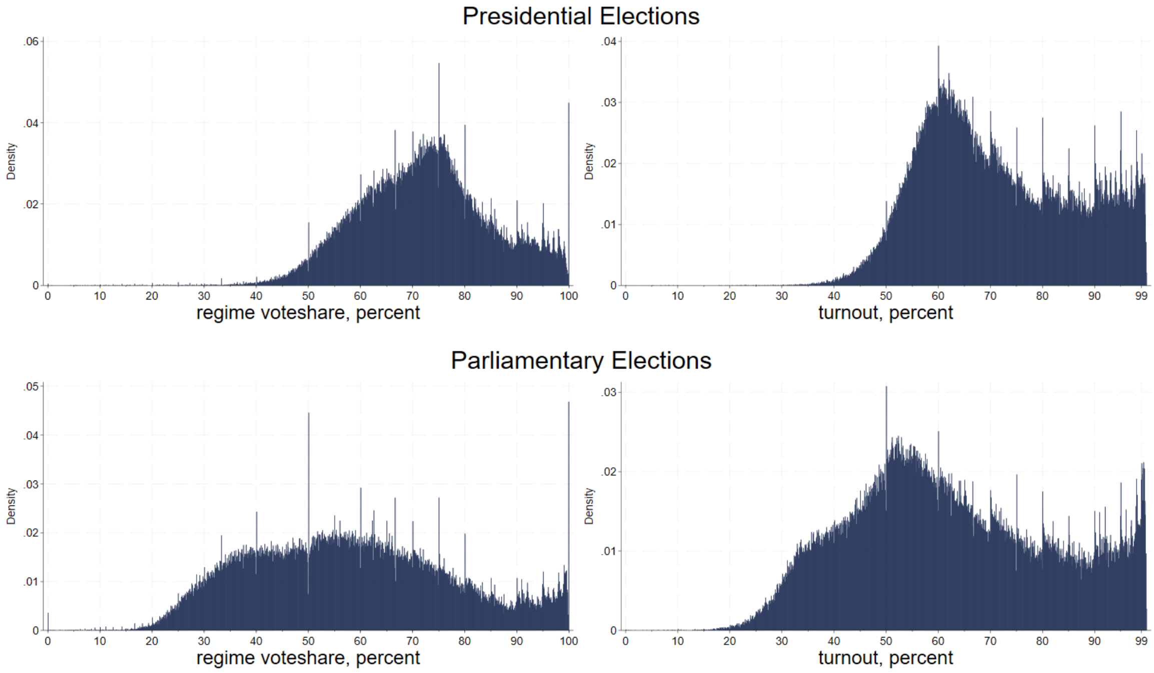
To address concerns regarding the overall quality of the administrative data, we contrast official numbers on vote shares obtained by the regime and on turnout to survey data from the European Social Survey (ESS) (Jowell et al., 2007). In the three relevant survey rounds in years after an election — round 4 in 2008 (*European Social Survey Round 4 Data* 2008), round 6 in 2012 (*European Social Survey Round 6 Data* 2012), and round 8 in 2016 (*European Social Survey Round 8 Data* 2016) — two questions are directly comparable to our variables of interest: "*Did you vote in the last national election*" and "*Party voted for in last national election*".

The ESS is conducted by a consortium of European universities, led by City, University of London, in 30 European countries.[39] In Russia, the survey is carried out by CESSI,[40] an independent research company that is active in many post-Soviet states, including Ukraine. In reach round, a representative sample is drawn with the help of the national statistical agencies, and roughly

[39]The consortium consists of Centerdata, Netherlands, GESIS — Leibniz Institute for the Social Sciences, Germany, Sikt — Norwegian Agency for Shared Services in Education and Research, Norway, The Netherlands Institute for Social Research (SCP), Netherlands, the University of Essex, UK, the University of Ljubljana, Slovenia, and Universitat Pompeu Fabra, Spain.

[40]https://www.cessi.ru/o-nas?lang=en.

Figure 7: Even Numbers in Russian Election Results



Notes: The figure shows vote shares and turnout observed at the level of electoral precincts. The left panel shows a histogram of vote shares received by President Putin and his party "United Russia". The right panel shows the histogram of turnout. The upper panel shows results for presidential elections, the lower panel for parliamentary elections. Notable clustering at even numbers as evidenced by unusual peaks are observed for both vote shares and turnout. Another peak of turnout values of 100 percent has been omitted for expositional reasons.

2500 respondents are interviewed.[41] Within Russia, respondents are residents from different regions of the country, allowing us to compare ESS outcomes with administrative data both across time and across space.[42] In figure 6 we map reported outcomes in the European Social Survey on the x-axis to the administrative election data on the y-axis, aggregated to the same geographical entities, for all three parliamentary elections. The results are clear: There is a strong positive correlation between reported turnout and vote share for the regime in the ESS and the raw election data. Accordingly, independently collected survey data corroborate the administrative election data.

A second concern is that electoral fraud at the local level might drive our previous 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 values like 50% or 75%. Figure 7 shows the histogram of vote-shares received by Putin and his

[41]For a detailed description of the sampling strategy see Jowell et al. (2007) and https://ess.sikt.no/en/study/f8e11f55-0c14-4ab3-abde-96d3f14d3c76.

[42]Note that the ESS changed the subnational classification within Russia from the 4th to the 6th and 8th round: In the former, the regions correspond to the Economic regions of Russia. In the latter two, the regions correspond to the Federal districts of Russia. A consistent aggregated mapping results in the four regions Center, Far East, North and West, and South and East.

party (left) and of turnout (right), observed at the level of electoral precincts, for presidential (upper panel) and for parliamentary (lower panel) elections.[43]

These irregularities cannot bias our estimates as long as they are time-consistent, thus being absorbed by first-differencing or by regional fixed-effects, or uncorrelated with *sanctions_exposure*$_r$. While there is no specific reason to assume that election fraud increases or decreases with *sanctions_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 (4) 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 *sanctions_exposure*$_r$. Results are presented in Table 4.

Table 4: 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
$sanction\_exposure_r^{exp}$	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	all	all	all	all	all	all
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 of sanction exposure 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*) Least-square Standard Errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

To assess whether statistical irregularities in the election data vary with *sanction_exposure*$_r$, we exploit the granular structure of our election data. Indeed, we observe election outcomes at the level of electoral precincts, which are nested in rayons r. For each rayon, we calculate the share of precincts reporting even percentages (Columns 1, 3, 5), or even percentages at meaningful dates like 50 or 75 percent (Columns 2, 4, 6). We do so for all party outcomes together (Columns 1–2), vote shares of Putin and his party (Columns 3–4), and turnout (Columns 5–6). Table 4 clearly speaks against sanctions leading to increased interference with election results. Consequently, we regard our main results to be unbiased by election fraud. Corresponding event-study graphs, split by election type, can be found in Appendix C.4, Figure A4.

[43]For expositional reasons, the large spike at 100% turnout in both presidential and parliamentary elections was omitted.

5 Conclusion

Our paper investigates political consequences of economic sanctions. We assess Russian regions' exposure to the sanctions imposed on the Russian economy in 2014 on the basis of trade losses caused by the sanctions. It turns out the sanctions increased internal support of the ruling regime. The vote share gained by President Putin and his party increases with regional exposure to the sanctions in both presidential and in parliamentary election. While Communist parties lost support in response to the sanctions, the vote share of liberal opposition parties remains unaffected – even in regions strongly hit by the sanctions, in former liberal strongholds, and in cities. We cannot infer on the long-run effects, but in the short- and medium-run, sanctions strengthen the sanctioned government.

Increasing regime support was certainly not the aim of the sanctions imposed on the Russian economy in 2014. Does this imply that sanctions failed politically? Not necessarily. Given the lack of research on that matter, it is not even clear what political goals the sanctions could realistically achieve. Our analysis reveals some of the specific political costs attached to economic sanctions. Similar to economic costs for the sanctioning countries, it might be worth paying these costs. However, more research on the political consequences of economic sanctions is needed to thoroughly evaluate such trade-offs, and to infer on sanctions' overall success.

In the given case, the sanctions most obviously were not successful in convincing the Russian government to hand back Crimea, if this had ever been the goal. Neither did the Russian regime withdraw support of the militant separatists in Eastern Ukraine. However, they might have delayed or prevented the Russian occupation of further parts of Ukraine or other countries, and might have deterred other governments considering similar actions. Empirically, the answer to all these questions depends on the counterfactual. But without more reliable evidence on sanctions' political consequences, the relevant counterfactuals are impossible to determine.

To better guide policy advice, it would be particularly important to better understand the mechanisms that translate exposure to sanctions into regime support. Unfortunately, the relevant information is usually not observable for countries under sanctions. Likewise, it would be helpful to better understand the impacts of different types of sanctions. For instance, the concrete political impacts of the sanctions targeting Putin's inner circle, i.e. the *selectorate* (Bueno De Mesquita et al. (2003)), might differ from the political consequences of the financial sanctions. With the available data, we can only assess the overall effect of the sanctions imposed on Russia on regime support.

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 portrayal of a hostile “Western World interfering with the Russian way of living.” Obviously, it is difficult to counter such a narrative in a country where the government controls the media. Still, it seems worthwhile to explore ways to accompany sanctions with measures to inform the general public about the very reasons for imposing the sanctions, the scope of the sanctioning coalition, and about political alternatives to the situation that is causing economic distress.

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Appendix A Data

A.1 Descriptive Statistics Main Variables

Table A1: Main Variables Observed

		t_{-1}	t_0	t_1
regime_{ir}	mean voteshare	0.704	0.600	0.663
	SD	0.107	0.159	0.164
loyal_{ir}	mean voteshare	0.050	0.084	0.035
	SD	0.059	0.070	0.038
nationalist_{ir}	mean voteshare	0.089	0.096	0.118
	SD	0.042	0.054	0.077
communist_{ir}	mean voteshare	0.140	0.180	0.152
	SD	0.068	0.065	0.062
liberal_{ir}	mean voteshare	0.011	0.034	0.010
	SD	0.009	0.028	0.012
other_{ir}	mean voteshare	0.007	0.007	0.022
	SD	0.009	0.008	0.021
turnout_{ir}	mean value	0.718	0.656	0.624
	SD	0.128	0.130	0.172
$\text{sanction\_exposure}_r^{exp}$	mean export loss	n.a.	n.a.	0.017
	SD			0.029
$\text{sanction\_exposure}_r^{imp}$	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 i or subject-level r for presidential and for parliamentary elections.

A.2 Descriptive Statistics Covariates

Table A2: Control Variables Observed

		t_{-1}	t_0	t_1
population_r	*1000	2213.350	2203.330	2208.401
	SD	1431.370	1469.593	1530.080
migration_r	growth rate	2.125	-1.842	-1.986
	SD	34.356	46.624	42.333
$\text{eligible voters}_{ir}$	*1000	47.532	48.077	47.215
	SD	191.252	196.260	199.384
density_{ir}	polling spots / eligible voters	0.002	0.001	0.002
	SD	0.001	0.001	0.001
employment_r	share in population	0.468	0.470	0.466
	SD	0.040	0.043	0.041
unemployment_r	rate	6.989	6.118	6.118
	SD	3.290	1.860	1.860
young_r	proportion of employed younger 30	25.099	25.074	22.068
	SD	2.151	1.978	1.781
old_r	proportion of employed older 49	22.702	24.732	27.349
	SD	2.269	2.270	2.146
high edu_r	share of employees with upper	47.890	47.206	49.586
	sec. education or higher SD	6.440	6.634	6.493
vocational edu_r	share of employees with	44.964	47.738	46.099
	vocational education SD	6.432	6.455	6.332
manufacturing_r	employment share	0.170	0.157	0.152
	(in all employment) SD	0.058	0.052	0.050
$\text{mining and quarrying}_r$	employment share	0.016	0.016	0.017
	(in all employment) SD	0.024	0.026	0.027
$\text{Agriculture, hunting, forestry and fishing}_r$	employment share	0.129	0.122	0.105
	(in all employment) SD	0.053	0.054	0.052
$\text{Gas, water, electricity}_r$	employment share	0.032	0.032	0.032
	(in all employment) SD	0.009	0.011	0.010
Construction_r	employment share	0.068	0.071	0.077
	(in all employment) SD	0.016	0.016	0.017
$\text{Transportation and Communication}_r$	employment share	0.081	0.079	0.081
	(in all employment) SD	0.018	0.016	0.016
$\text{Wholesale and retail trade}_r$	employment share	0.159	0.168	0.178
	(in all employment) SD	0.027	0.029	0.028
$\text{Hotels and restaurants}_r$	employment share	0.017	0.016	0.020
	(in all employment) SD	0.004	0.005	0.005
$\text{Real estate and renting}_r$	employment share	0.058	0.064	0.073
	(in all employment) SD	0.018	0.020	0.019
$\text{Healthcare and Social Services}_r$	employment share	0.071	0.072	0.070
	(in all employment) SD	0.008	0.008	0.008
Education_r	employment share	0.095	0.091	0.085
	(in all employment) SD	0.016	0.015	0.014
$\text{Communal and social services}_r$	employment share	0.036	0.036	0.037
	(in all employment) 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 i or subject-level r for presidential and for parliamentary elections.

Appendix B Computing General Equilibrium Counterfactual Trade Flows

In this section, we describe in more detail the computation of general equilibrium counterfactual trade flows using the structural gravity equation of international trade, in the spirit of Crozet and Hinz (2020) and Anderson, Larch, and Yotov (2018). The computation consists of five steps, including an iteration over the last two steps until convergence.

Recall the gravity model as in Head and Meyer

$$
\begin{aligned}
& X_{odt} = \frac{Y_{ot}}{\Omega_{ot}} \cdot \frac{X_{dt}}{\Phi_{dt}} \cdot \phi_{odt} \qquad \text{with} \qquad o \in \mathbf{L}; \;\; d \in \mathbf{L}; \;\; \mathbf{L} = \{l_1, l_2, \cdots, l_n\}, \\
\text{where} \quad & Y_{ot} = \sum_{\ell \in \mathbf{L}} X_{o\ell t}, \;\; X_{dt} = \sum_{\ell \in \mathbf{L}} X_{\ell dt}, \\
\text{and} \quad & \Omega_{ot} = \sum_{\ell \in \mathbf{L}} \frac{X_{\ell t}}{\Phi_{\ell t}} \cdot \phi_{o\ell t}, \;\; \Phi_{dt} = \sum_{\ell \in \mathbf{L}} \frac{Y_{\ell t}}{\Omega_{\ell t}} \cdot \phi_{\ell dt}
\end{aligned} \tag{6}
$$

which is estimated using Poisson Pseudo-Maximum Likelihood estimator

$$
X_{odt} = \exp\left(\Psi_{ot} + \Theta_{dt} + \phi_{odt}\right) \tag{7}
$$

with Ψ_{ot} and Θ_{dt} being origin $\times$ time and destination $\times$ time fixed effects, and ϕ_{odt} an origin $\times$ destination fixed effect for either *pre-* or *post*-sanction period. The procedure is as follows:

1. Estimate Equation 7 using *pre*-sanction (2012, 2013) data and a PPML estimator. Keep $\widehat{\phi}_{od(pre)}$ and discard the rest. This is the crucial parameter to hold the conditions of bilateral trade relationships constant, just as if no sanctions had been imposed. The purpose of all coming steps is to purge the remaining components of the gravity equation — and thus the counterfactual trade flows— from the repercussions caused by the change in bilateral frictions, i.e. caused by the sanctions.[44]

2. Estimate Equation 7 using *post*-sanction (2014, 2015) data and a PPML estimator. Keep $\widehat{\Psi}_{o(post)}$ and $\widehat{\Theta}_{d(post)}$ and discard the rest. The two parameters capture overall import and export "openness" as well as price levels in the post-sanction period. They are used in step 5 to update export sales $Y_{o(post)}$ and import expenditures $X_{o(post)}$. Specifically, they are needed to compute the so-called *factory-gate* price adjustment that summarizes differences

[44] That is to say that conceptionally, even with a constant ϕ_{odt}, all other parameters adjust to an ever-changing trade environment. The empirical challenge is to disentangle adjustments of the international trade network that would have happened regardless of the sanctions from adjustments to the sanctions. To these ends, the subsequent steps clean post-sanction gravity parameters from changes that are caused by the change from $\phi_{od(pre)}$ to $\phi_{od(post)}$.

in the economic conditions between the observed and counterfactual world.

3. Compute post-sanction period export sales and import demand, based on the data observed, by summing over all exports, $Y_{o,t\in(post)} = \sum_{\ell\in\mathbf{L}} X_{o\ell,t\in(post)}$, and imports $X_{d,t\in(post)} = \sum_{\ell\in\mathbf{L}} X_{\ell d,t\in(post)}$ for all countries and time-periods in the post-sanction period. These observed data points are updated in step 5 to counterfactual export sales and import demand. The later update removes the impact of the sanctions.

4. For the so-called *conditional general equilibrium*, recompute the multilateral resistance terms for the post-sanction period, $\tilde{\Omega}^{\text{CGE}}_{o,t\in(post)}$ and $\tilde{\Phi}^{\text{CGE}}_{d,t\in(post)}$, with bilateral frictions from the pre-sanction period. The multilateral resistances can be recomputed by iterating over the two following systems of equations:

$$\tilde{\Omega}^{\text{CGE}}_{o,t\in(post)} = \sum_{\ell\in\mathbf{L}} \frac{\tilde{X}_{\ell,t\in(post)}}{\tilde{\Phi}^{\text{CGE}}_{\ell,t\in(post)}} \widehat{\phi}_{o\ell,t\in(pre)} \quad \text{and} \quad \tilde{\Phi}^{\text{CGE}}_{d,t\in(post)} = \sum_{\ell\in\mathbf{L}} \frac{\tilde{Y}_{\ell,t\in(post)}}{\tilde{\Omega}^{\text{CGE}}_{\ell,t\in(post)}} \widehat{\phi}_{\ell d,t\in(pre)}$$

Note that initially, $\tilde{X}_{d,t\in(post)}$ and $\tilde{Y}_{o,t\in(post)}$ are simply the values calculated in step 3, i.e. $X_{d,t\in(post)}$ and $Y_{o,t\in(post)}$. Plugging the recomputed multilateral resistance, $\tilde{\Omega}^{\text{CGE}}_{o,t\in(post)}$ and $\tilde{\Phi}^{\text{CGE}}_{d,t\in(post)}$, along with the counterfactual bilateral frictions $\widehat{\phi}_{od(pre)}$ into Equation (6) yields the *conditional* general equilibrium trade flows given by

$$\tilde{X}^{\text{CGE}}_{od,t\in(post)} = \frac{\tilde{Y}_{o,t\in(post)}}{\tilde{\Omega}^{\text{CGE}}_{o,t\in(post)}} \cdot \frac{\tilde{X}_{d,t\in(post)}}{\tilde{\Phi}^{\text{CGE}}_{d,t\in(post)}} \cdot \widehat{\phi}_{od(pre)}$$

These trade flows take into account that the relative ease of exporting/importing between *all* country/region pairs is changing due to the changes of *some* bilateral frictions. Note that how much is imported and exported in total, i.e. $\tilde{Y}_{ot}$ and $\tilde{X}_{dt}$, is not yet adjusted for a counterfactual world without sanctions.

5. The *full general equilibrium* step incorporates endogenous changes to the last two remaining components, the export sales and expenditure figures. Following Anderson, Larch, and Yotov (2018) and setting $\sigma = 5$ this *factory-gate* price adjustment is obtained as

$$\tilde{Y}^{\text{GE}}_{o,t\in(post)} = \tilde{Y}_{o,t\in(post)} \cdot \left(\frac{\tilde{\Omega}^{\text{CGE}}_{o,t\in(post)}}{\widehat{\Psi}_{o,t\in(post)}} \right)^{\frac{1}{1-\sigma}} \quad \text{and} \quad \tilde{X}^{\text{GE}}_{d,t\in(post)} = \tilde{X}_{d,t\in(post)} \cdot \left(\frac{\tilde{\Phi}^{\text{CGE}}_{d,t\in(post)}}{\widehat{\Theta}_{d,t\in(post)}} \right)^{\frac{1}{1-\sigma}}$$

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

$$\tilde{X}^{\text{GE'}}_{od,t\in(post)} = \frac{\tilde{Y}^{\text{GE}}_{o,t\in(post)}}{\tilde{\Omega}^{\text{CGE}}_{o,t\in(post)}} \cdot \frac{\tilde{X}^{\text{GE}}_{d,t\in(post)}}{\tilde{\Phi}^{\text{CGE}}_{d,t\in(post)}} \cdot \widehat{\phi}_{od,t\in(pre)}$$

where $\tilde{X}^{\text{GE'}}_{od,t\in(post)}$ reflects the updated $\tilde{Y}^{\text{GE}}_{o,t\in(post)}$ and $\tilde{X}^{\text{GE}}_{d,t\in(post)}$. Note that now $\tilde{X}^{\text{CGE}}_{od,t\in(post)}$ and $\tilde{\Phi}^{\text{CGE}}_{d,t\in(post)}$ are "outdated" and need to be updated. Hence, steps 4 and 5 are iterated until convergence.

At convergence, counterfactual trade flows are given by

$$\tilde{X}^{\text{GE}}_{od,t\in(post)} = \frac{\tilde{Y}^{\text{GE}}_{o,t\in(post)}}{\tilde{\Omega}^{\text{GE}}_{o,t\in(post)}} \cdot \frac{\tilde{X}^{\text{GE}}_{d,t\in(post)}}{\tilde{\Phi}^{\text{GE}}_{d,t\in(post)}} \cdot \widehat{\phi}_{od,t\in(pre)}$$

where all components of Equation (6) reflect the counterfactual world in which bilateral friction from the pre-sanction period remain, mimicking a world without sanctions.

The final regional quantity of interest is $\tilde{T}_{r(post)}$:

$$\tilde{T}_{r(post)} = \sum_{t\in(post)} \sum_{l\in d} \tilde{X}^{\text{GE}}_{rlt}$$

The alternative measure used on the basis of imports is:

$$\tilde{T}_{r(post)} = \sum_{t\in(post)} \sum_{l\in o} \tilde{X}^{\text{GE}}_{lrt}.$$

Appendix C Additional Results for sanction_exposure$_r$

C.1 Sensitivity to Excluding Countries with FTAs

As discussed in Section 3.4, our identification strategy rests on the assumption that in the short run, bilateral trade frictions ϕ_{odt} were primarily affected by the sanctions, and simultaneous developments had only a neglectable impact on international trade flows. As always, there is no direct way to test this exclusion restriction. However, we can test how sensitive our results are to excluding countries that signed Free Trade Agreements (FTAs) from the calculation of counterfactual trade flows. That is we estimate Equation 2 and calculate $\widehat{T}_{r(post)}$ as before, but disregard information from Moldova, Georgia, Tajikistan, Kazakhstan, and Croatia, i.e. countries that signed FTAs and are potentially relevant for Russia's trade. If the resulting measure of $sanction\_exposure_r$ would lead to significantly different results, this would indicated that our initial results were particularly sensitive to simultaneous changes in ϕ_{odt}. Accordingly, we repeat our main regressions from Column (4) of Table 1 with this adjusted measure of $sanction\_exposure_r$. Results are reported in Table A3.

Table A3: Sanction Effects with FTA-countries Omitted

	(1) Δ regime	(2) Δ loyal	(3) Δ nationalist	(4) Δ communist	(5) Δ liberal	(6) Δ other	(7) Δ turnout
	Sanction Effects on Column-Outcomes						
$sanction\_exposure_r^{exp}$	0.502***	-0.021	-0.086	-0.332***	0.008	-0.026	0.027
	(0.100)	(0.039)	(0.052)	-0.069	(0.011)	(0.020)	-0.173
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

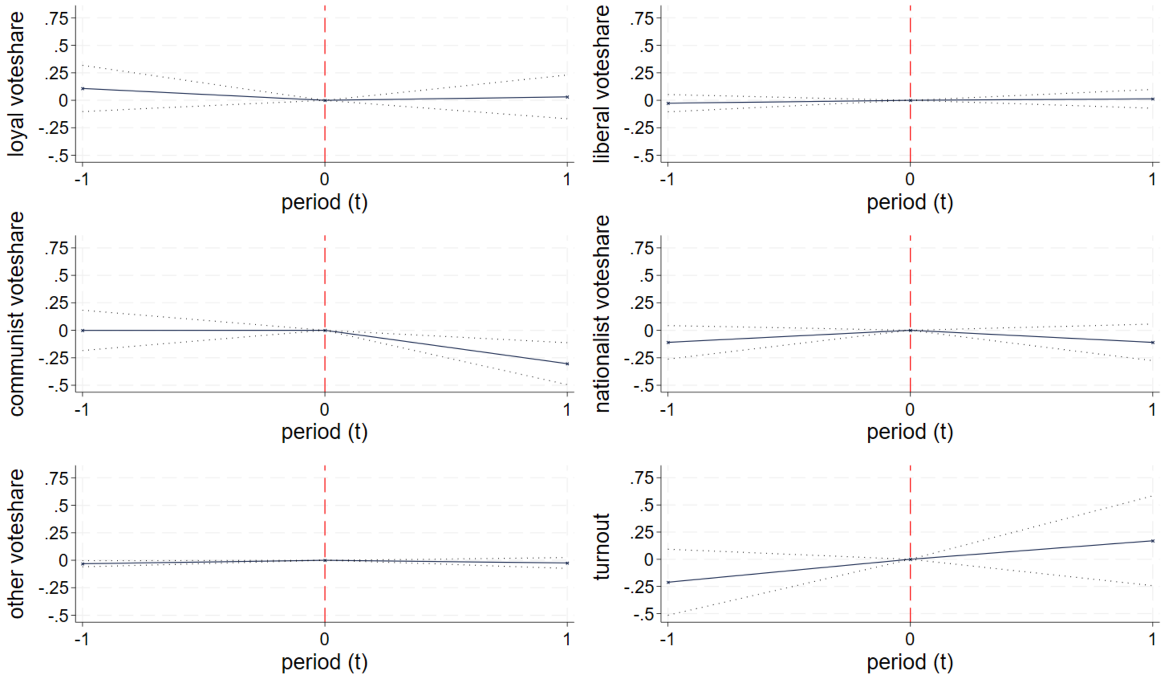
Notes: (*a*) Each column reports results from a separate regression. (*b*) Columns refer to different outcome variables observed at the *rayon*-level. First differences are calculated between the first post-sanction and the last pre-sanction election. (*c*) All specifications control for regional demographics, 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*) Least-square Standard Errors, clustered at the level of 75 *Federal Subjects*, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The results reported in Table A3 are very similar to the original results reported in Table 1. If anything, point estimates are slightly larger. Apparently, our initial results were not driven by countries that signed free trade agreements, i.e. countries experiencing simultaneous changes in bilateral trade frictions independent of the sanctions. Since these FTAs were the most significant developments in international trade apart from the sanctions over our period of analysis, we conclude that our identifying assumption of no simultaneous (and significant) changes in ϕ_{odt} hold.

C.2 Event Studies on Further Election Outcomes

Figure A1 corresponds to Figure 3, and shows event study graphs for all other voting outcomes reported in Table 1.

Figure A1: Event Study: Effect of Sanctions on Election Outcomes

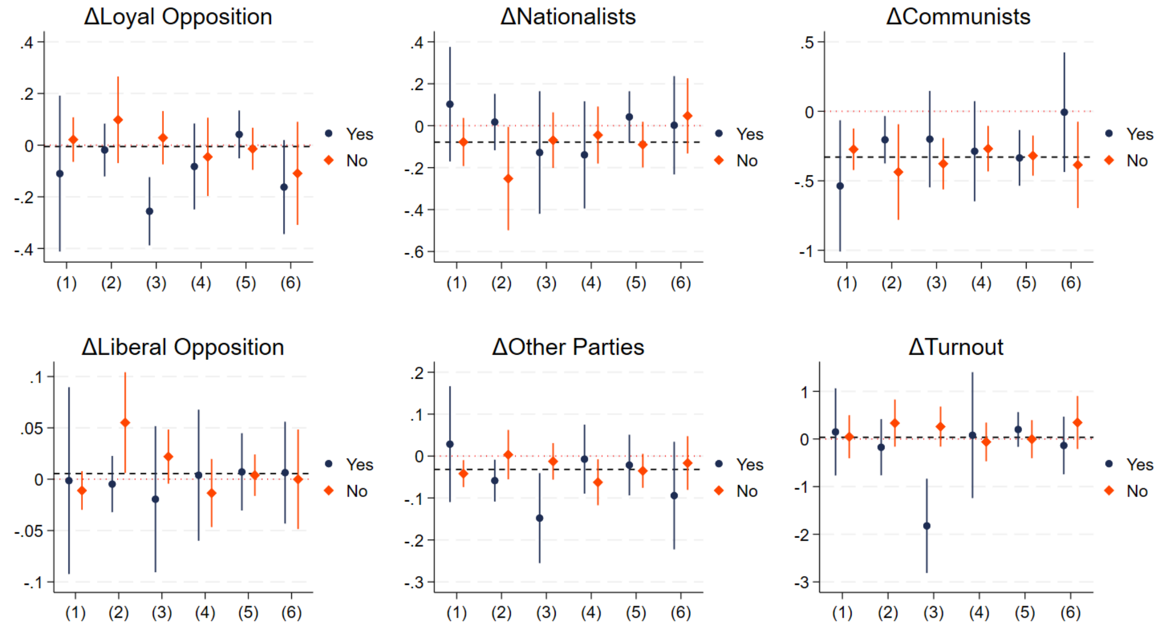


Notes: The figure plots point estimates for interaction effects of *sanction_exposure*$_r$ with a categorical period-indicator. The respective outcome is indicated at the y-axis. The omitted category is sanction exposure in t_0, i.e. the last elections before the sanctions were imposed. Previous election results in t_{-1} and elections under sanctions in t_{+1} are evaluated against this reference point, conditional on the same set of controls as before. Corresponding confidence intervals are based on least-square SE, clustered at the level of 75 *Federal Subjects*.

C.3 Effect Heterogeneities for Further Election Outcomes

Figure A2 corresponds to Figure 4, and Figure A3 corresponds to Figure 5. Both figures explore effect heterogeneities for all other voting outcomes reported in Table 1.

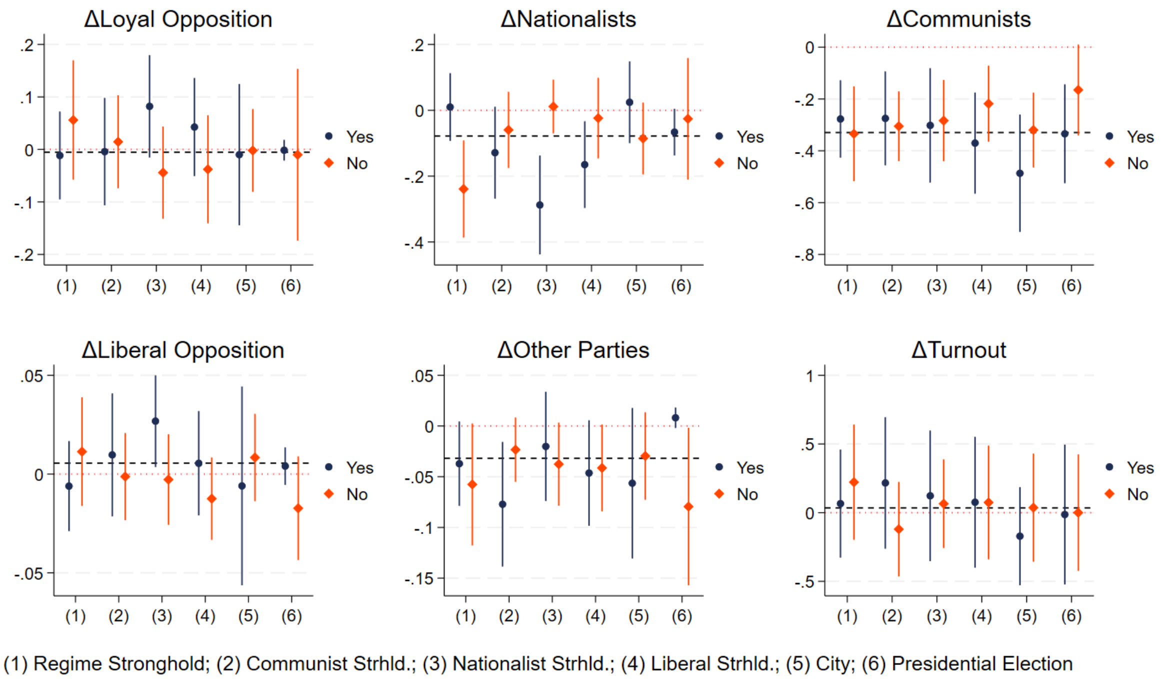
Figure A2: Regional Effect Heterogeneity: Trading Patterns



(1) Trade-Heavy; (2) Ties to UKR; (3) Oil/Gas; (4) Ties to Sanctioning; (5) Sanctioned Firms; (6) Benefits from Sanctions

Notes: The figure plots treatment coefficients for $sanction\_exposure_r$. Corresponding confidence intervals (95%) are based on least-square SE, clustered at the level of 75 *Federal Subjects*. Every panel is titled with the respective outcome. In specification (1) the sample is split at the median for pre-sanction (Imports+Exports)/GDP. In specification (2) the sample is split at the median for (Imports+Exports) from or to Ukraine / total (Imports+Exports). In specification (3) the sample is split at the 75-percentile for the share of oil- and gas-exports in all exports. In specification (4) the sample is split at the median for (Imports+Exports) from or to the 37 sanction countries / total (Imports+Exports). In specification (5) we split according to whether any firm directly sanctioned is located in a rayon. In specification (6) we split according to whether a region experiences export gains caused by the sanction. The dashed line marks the original point estimate.

Figure A3: Regional Effect Heterogeneity: Politics

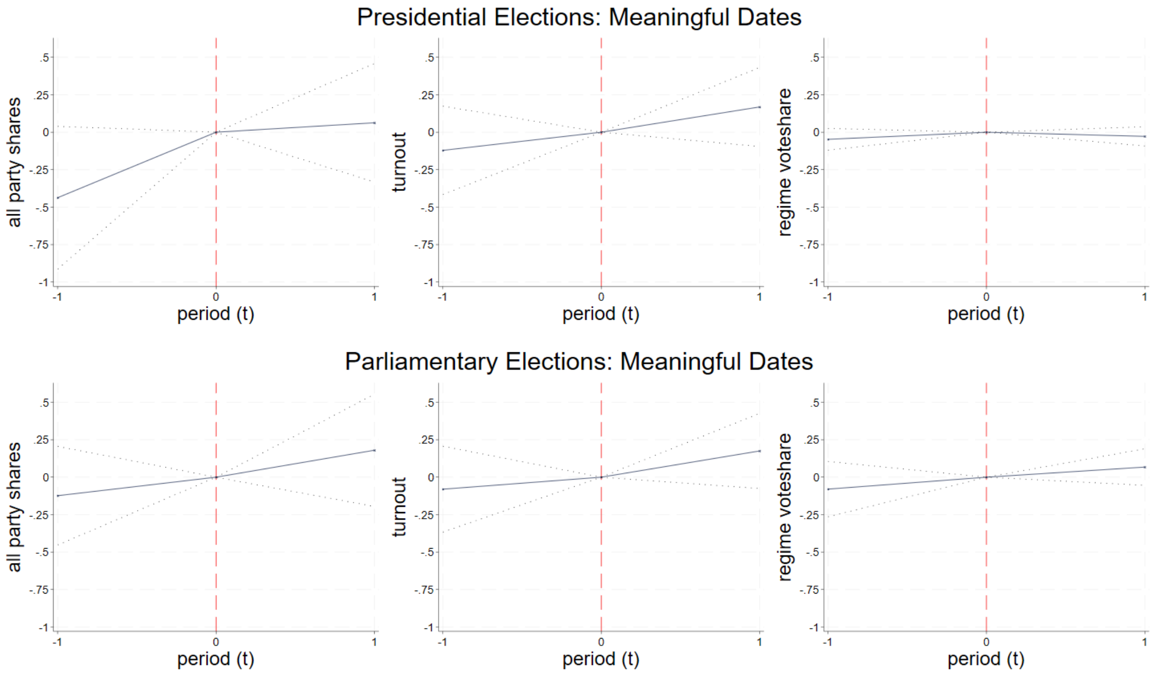


Notes: The figure plots treatment coefficients for $sanction\_exposure_r$. Corresponding confidence intervals (95%) are based on least-square SE, clustered at the level of 75 *Federal Subjects*. Every panel is titled with the respective outcome. In specification (1) the sample is split at the median for pre-sanction vote shares received by regime parties and candidates. In specification (2) the sample is split at the median for pre-sanction vote shares received by Communist parties and candidates. In specification (3) the sample is split at the median for pre-sanction vote shares received by Nationalist parties and candidates. In specification (4) the sample is split at the median for pre-sanction vote shares received by liberal opposition parties and candidates. In specification (5) we split into cities with at least 100k inhabitants, and the rest. In specification (6) we split by election type. The dashed line marks the original point estimate.

C.4 Event Studies on Election-Irregularities

Figure A4 repeats the regressions reported in Table 4 as event studies.

Figure A4: Placebo Effect on Statistical Irregularities



Notes:Each panel reports results from a separate regression. Outcomes are the *rayon*-level shares of election results with even and meaningful numbers, based on (left) all parties, (middle) turnout, (right) President Putin and his party. Upper panels report on presidential election, lower panels report on parliamentary elections. The figures plots point estimates for interaction effects of $sanction\_exposure_r$ with a categorical period-indicator. The omitted category is sanction exposure in t_0, i.e. the last elections before the sanctions were imposed. Previous election results in t_{-1} and elections under sanctions in t_{+1} are evaluated against this reference point, conditional on the full set of control variables. Corresponding confidence intervals are based on least-square SE, clustered at the level of 75 *Federal Subjects*.

Appendix D Sanction Effects Based on Imports

All effects of $sanction\_exposure_r$ reported in the paper are based on export losses caused by the sanctions. Alternatively, one might look at import losses, i.e. assess the effects of $sanction\_exposure_r^{imp}$. Both measures approximate the same underlying sanction-effect. It is just that $sanction\_exposure_r^{imp}$ may be affected by Russian retaliation, while $sanction\_exposure_r$ based on exports is the more exogenous measure. For comparison, we repeat all the previous regressions using import-based $sanction\_exposure_r^{imp}$ in this section.

D.1 Main Effects of Sanctions Exposure Based on Imports

Table A4 repeats the results from Table 1 with the import-based measure $sanction\_exposure_r^{imp}$.

Table A4: Effect of Sanctions on Russian Elections: Import losses

	(1)	(2)	(3)	(4)	(5)
	Effect of sanction_exposure$_r^{imp}$				
Δ regime	0.566**	0.551**	0.501***	0.403***	4.204***
	(0.232)	(0.217)	(0.186)	(0.121)	(1.262)
	[0.249]	[0.240]	[0.256]	[0.171]	[1.778]
Δ loyal	-0.010	-0.012	0.020	0.064	1.291
	(0.118)	(0.100)	(0.095)	(0.054)	(1.096)
	[0.127]	[0.113]	[0.123]	[0.071]	[1.433]
Δ nationalist	-0.109	-0.085	-0.062	-0.071	-1.739
	(0.074)	(0.073)	(0.065)	(0.062)	(1.501)
	[0.078]	[0.078]	[0.086]	[0.079]	[1.924]
Δ communist	-0.393***	-0.400***	-0.381***	-0.304***	-5.376***
	(0.136)	(0.134)	(0.129)	(0.077)	(1.362)
	[0.149]	[0.152]	[0.174]	[0.117]	[2.077]
Δ liberal	-0.021	-0.021	-0.040	-0.005	-0.158
	(0.049)	(0.041)	(0.035)	(0.012)	(0.392)
	[0.052]	[0.044]	[0.047]	[0.016]	[0.548]
Δ other	-0.033	-0.033	-0.037	-0.041	-2.830
	(0.030)	(0.023)	(0.026)	(0.025)	(1.742)
	[0.031]	[0.024]	[0.032]	[0.031]	[2.127]
Δ turnout	0.154	0.128	-0.040	-0.048	-0.446
	(0.203)	(0.207)	(0.185)	(0.189)	(1.749)
	[0.227]	[0.237]	[0.251]	[0.258]	[2.386]
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.

D.2 Placebo Effects of Sanctions Exposure Based on Imports

Table A5 repeats the results from Table 2 with the import-based measure $sanction\_exposure_r^{imp}$.

Table A5: 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)						
$sanction\_exposure_r^{imp}$	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.

D.3 Sensitivity to Excluding Countries with FTAs from $\text{sanction\_exposure}_r^{imp}$

Table A6 repeats the results from Table A3 with the import-based measure $sanction\_exposure_r^{imp}$.

Table A6: Sanction Effects (Imports) with FTA-countries Omitted

	(1) Δ regime	(2) Δ loyal	(3) Δ nationalist	(4) Δ communist	(5) Δ liberal	(6) Δ other	(7) Δ turnout
	Sanction Effects on Column-Outcomes						
$sanction\_exposure_r^{imp}$	0.458*** (0.113)	0.048 (0.046)	-0.088 -0.059	-0.327*** -0.075	-0.001 -0.011	-0.035 -0.024	-0.014 -0.187
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.