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ABSTRACT

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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-level variation. We use trade losses caused by the sanctions as measure for regional sanction exposure. For identification, we rely on a structural gravity model that allows us to compare observed trade flows to counterfactual flows in the absence of sanctions. 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, sanction exposure increases the vote share gained by president Putin and his party by 13 percent. Event studies and placebo estimations confirm the validity of our results.

Keywords: Economic sanctions, voting behavior, gravity estimation, rally-around-the-flag

JEL classification: F12; F14; F15

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1 Introduction

Do sanctions influence regime support in targeted countries? Unfortunately, we do not know. While the economic consequences of sanctions are comparatively well understood, both for sanctioned countries (Haider, 2017; Dreger et al., 2016; Ahn and Ludema, 2020) and for sanctioning countries (Besedeš et al., 2017; Crozet and Hinz, 2020), we still lack quantitative evidence on economic sanctions' political impacts.[1] 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 impacts, it was not straightforward to define concrete policy objectives the sanctions should achieve, apart from supporting Ukraine in a broad sense.

Our paper contributes to closing this — arguably large — research gap. Our focus is on Russia, and the sanctions imposed on the Russian economy after its 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 ($\approx$ district). We regress these changes on a measure of regional sanction exposure. To assess sanction exposure, we rely on regional trade flows with foreign countries, observed on the *federal subject*-level ($\approx$ state).[2] We then define sanction 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.[3] Trade flows in the absence of sanctions are computed using 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 incorporate simultaneous developments in international trade unrelated to sanctions, but account for substitution of trading partners and changes in regional supply and demand conditions as they were caused by the sanctions.

Russian regions' counterfactual trade flows serve two purposes in our empirical analysis. First, they allow us to assess regional variation in (not directly observable) sanction exposure. Second, the counterfactual measure allows for causal inference in a differences-in-differences setup. That

[1] 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.

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

[3] 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 sanction's trade effects would not be captured, though, e.g. effects from travel bans on selected individuals. However, given the specific nature of the 2014 sanctions, we are confident that to capture sanctions' main impact on the average voter.

is, instead of simply using time variation between pre- and post sanction im- 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.

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 benefiting in economic terms, e.g. workers in sectors where domestic production increases due to decreasing imports. Second, it is unclear how losses from economic sanctions translate into voting behavior. If voters blame the government for the economic hardships they experience, regime support should decline. Conversely, if voters blame the sanctioning countries, there could be a so-called "rally around the flag" effect, that leads voters to unite behind the government. Both effects could occur simultaneously, leading to political polarization. Of course, voters could also just be indifferent.

To assess regime support, we rely on election results.[4] 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 the 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 the sanctions led some nationalist supporters of the Communist party to supporting the ruling party. Placebo regressions and event studies rule out pre-trends and support our identification strategy. The effect is extraordinarily 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 et al., 2017; Etkes and Zimring, 2015; Dreger et al., 2016; Felbermayr et al., 2019). Ours is the first paper to identify economic sanctions' impact on political support of the sanctioned country's government.[5] With that, our paper also speaks to the literature on the political consequences of economic shocks (e.g. Dippel et al., 2015; Autor et al., 2016; Becker et al., 2017).

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

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

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).[6] Since the Russian government responded with an embargo on certain food and agricultural products, we will focus our analysis on sanction-induced export losses.[7]

Importantly, this paper should not be understood as an evaluation of whether sanctions achieved their goal(s). Contrarily, given the lack of research on economic sanctions' political impacts, it seems difficult to even define the 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. We discuss how we think our results fit into the broader research on sanctions' effectiveness in a dedicated section towards 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 relates our contribution to a broader research agenda on the political economy of sanctions. Section 6 concludes.

2 Context

Against the backdrop of an ever-escalating conflict between Russia, Ukraine, and Russian-backed separatists on Ukrainian territory, the international community step-wise imposed economic sanctions on the Russian economy in 2014. Following the refusal of Ukrainian President Viktor Yanukovich to sign the EU-Ukrainian Association agreements in 2013, Ukraine witnessed a series of massive demonstrations. The protests started on November 21, 2013 and, by November 30, had reached hundreds of thousand protesters. On the February 21, 2014, President Yanukovich 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 other countries reacted with "targeted sanctions" that hit selected Russian individuals with travel bans and asset freezes. On March 18, Russia annexed Crimea. In response, a total of 37 countries implemented further sanctions, still targeting Russian individuals and companies selectively.

Clashes between Russian-backed troops and the Ukrainian army intensified in the Eastern border regions of Ukraine ("Donbass"). The situation escalated in the downing of a civilian Malaysian

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

[7] Since imports and exports are correlated, it is not possible to unambiguously distinguish between import- and export effects. However, any measure of sanction effects on Russian imports will endogenously 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.

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).[8] In turn, the Russian government embargoed imports of agricultural goods, mainly of fresh food, from the sanctioning countries.[9]

3 Data and empirical method

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 actual trade and the trade that a region would have experienced in the absence of sanctions. Second, we adopt a difference-in-difference method that compares voting behavior across regions before and after the introduction of sanctions, conditional on the degree to which regions where sanction-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 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. The remaining sanctions either targeted international trade in selected goods directly, or indirectly affected selected companies' trade costs by freezing their or their owners' foreign assets.[10] 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 observed total trade of Russian region r in the period after the imposition

[8] 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).

[9] 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.

[10] 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.

of sanctions (2014, 2015). Furthermore, let $\widehat{T}_{r(post)}$ denote the (unobserved) trade this region would have had in the absence of sanctions. We define sanction exposure as:[11]

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

The challenge is to determine the trade that a region would have had in the absence of sanctions, i.e. $\widehat{T}_{r(post)}$. To illustrate this challenge, consider what would happen if we used pre-sanction trade $T_{r(pre)}$, e.g., measured using 2012 and 2013 data, to infer on $\widehat{T}_{r(post)}$. Using $T_{r(pre)}$ as counterfactual for $T_{r(post)}$ in a standard diff-in-diff setting would inevitably lead to biased estimates. For sure, $T_{r(pre)}$ would depend on $r's$ time-invariant propensity to trade, which would cancel out by first-differencing. However, using observed $T_{r(pre)}$ as unobserved counterfactual $\widehat{T}_{r(post)}$ for post-sanction $T_{r(post)}$, this counterfactual would not incorporate all the changes (other than sanctions) that took place between the pre-period 2012-13 and the post-period 2014-15. For example, it would not take into account changes in commodity prices or in global demand, or shifts in comparative advantage unrelated to sanctions. Since all these factors are, however, incorporated in $T_{r(post)}$, the difference $T_{r(post)} - T_{r(pre)}$ would confound any sanction-effect with simultaneous but unrelated developments. Hence, we would be using a poor proxy for regional sanction exposure.

Our strategy is instead to generate a measure of $\widehat{T}_{r(post)}$ that is derived from a structural model, using the universe of region-to-country and country-to-country trade flows before and after sanctions. Specifically, we rely on the well-established gravity model of international trade (Head and Mayer, 2014), where trade flows between an origin (that exports) and a destination (that imports) are a function of supply (at the origin) and demand (at the destination), general easiness to trade for origin and destination, and ad-hoc ability to trade between two specific partners. The key equation that describes the model is

$$X_{odt} = \frac{Y_{ot}}{\Omega_{ot}} \cdot \frac{X_{dt}}{\Phi_{dt}} \cdot \phi_{odt} \tag{2}$$

$$\begin{aligned} \text{where} \qquad & Y_{ot} = \sum_{\ell} X_{o\ell t},\ X_{dt} = \sum_{\ell} X_{\ell dt}, \\ \text{and} \qquad & \Omega_{ot} = \sum_{\ell} \frac{X_{\ell t}}{\Phi_{\ell t}} \cdot \phi_{o\ell t},\ \Phi_{dt} = \sum_{\ell} \frac{Y_{\ell t}}{\Omega_{\ell t}} \cdot \phi_{\ell dt}. \end{aligned}$$

X_{odt} are exports from an origin o to a destination d at time t. Y_{ot} are exports sales at the origin, X_{dt} is the import demand at the destination. Two crucial terms are Ω_{ot} and Φ_{dt}, the so-called outward and inward multilateral resistance terms, that capture, respectively, the origin's general propensity to export and the destination's propensity to import (i.e. their relationship to the

[11] We compute the measure both for total exports, as well as for total imports. Our main specification, as explained below, will focus on the former type of shock.

world market). ϕ_{odt} is an origin-destination-pair specific term that summarizes bilateral trade frictions between o and d at time t.

Equation (2) guides our empirical strategy. If o imposes sanctions on d at time t, this decreases ϕ_{odt}, in other words, their bilateral trade frictions increase. In turn, Ω_{ot} and Φ_{dt} adjust accordingly to o's and d's ability to divert trade to other partners. In other words — and ceteris paribus — other countries become relatively more attractive trading partners after trade frictions with sanctioning countries increase. Eventually, Y_{ot} and X_{dt} also adjust to the new trade equilibrium. Through the lens of this gravity model, predicting how trade would have looked like in the absence of sanctions boils down to how the bilateral trade costs would have looked like in the absence of sanctions. In particular, this holds in the short run. Sanction act as an unexpected shock to ϕ_{odt}, that leads to adjustments based on pre-determined characteristics of all trading partners o and d. In the longer run, a new equilibrium may emerge endogenously, but for the initial years after sanctions where imposed, we regard pre-sanction trade costs $\phi_{od(pre)}$ to be a reliable proxy for trade costs $\widehat{\phi}_{od(post)}$ in the absence of sanctions.[12] Hence, we employ the structural gravity model to assess counterfactual international post-sanction trade flows by holding pre-sanction bilateral trade costs constant. While we account for adjustments in all parameters to the changing ϕ_{odt}, this allows us to extract variation in trade flows caused by the sanctions, but unrelated to simultaneous changes in the international trading environment.

To derive counterfactual trade flows, we rely on regional-level trade data from the "Federal Customs Service of Russia".[13] 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 illegally 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.[14] We augment the regional data with international trade data covering imports and exports of the universe of countries other than Russia.[15] The final dataset covers the years 2012 to 2015, i.e. two years pre- and post sanctions' implementation. It contains information on all the bilateral trade flows between 124 countries and 75 Russian federal subjects.

The structural model (2) can be estimated as

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

using a Poisson Pseudo-Maximum Likelihood estimator, where Ψ_{ot}, Θ_{dt} and ϕ_{od} are origin-time,

[12] As a matter of fact, bilateral trade-frictions ϕ_{odt} constantly change 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.

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

[14] 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 1.

[15] 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.

destination-time and origin-destination fixed effects.[16] Estimated $\widehat{\Psi}_{ot}$ and $\widehat{\Theta}_{dt}$ assess multilateral resistance terms Ω_{ot} and Φ_{dt}, while $\widehat{\phi}_{od}$ measures bilateral trade frictions ϕ_{od}. Building on Crozet and Hinz (2020), we assess counterfactual trade $\widehat{T}_{r(post)}$ in a five-step procedure:

1. We estimate pre-sanction bilateral trade costs ($\phi_{od(pre)}$) by estimating equation (3) using pre-sanction data (2012, 2013).

2. We estimate post-sanction Ψ_{ot} and Θ_{dt} (affected by sanctions, and simultaneous developments) by estimating (3) using post-sanction data (2014, 2015).

3. We predict Partial Equilibrium (PE) trade flows ($\widehat{X}^{PE}_{odt}$) by plugging $\widehat{\Psi}_{ot}$ and $\widehat{\Theta}_{dt}$ from step 2) and $\widehat{\phi}_{od(pre)}$ from step 1) in equation (3).

4. We recompute the multilateral resistance terms Ω_{ot} and Φ_{dt} following equation (2). These are Conditional General Equilibrium (CGE) quantities because, relative to the ones implicitly estimated at point 2), they take into account the change in bilateral trade costs (from $\phi_{od(post)}$ to $\widehat{\phi}_{od(pre)}$).

5. We compute the export sales ($\widehat{Y}_{ot}$) and import demand ($\widehat{X}_{dt}$). These are General Equilibrium (GE) quantities because, relative to those estimated at point 2) and 3), they take into account the change in bilateral trade costs *and* the change in multilateral resistance terms (i.e., change in openness to trade).

The computation involves iterating between steps 4 and 5 until convergence.[17]

Now, we can determine counterfactual trade flows $\widehat{T}_{r(post)} = \sum_{t\in(post)} \sum_{\ell\in d} \widehat{X}^{GE}_{r\ell t}$ and, in turn, our sanction exposures (as in equation (1)).[18] All simultaneous changes in international trade unrelated to sanctions affect both $T_{r(post)}$ and $\widehat{T}_{r(post)}$ alike, hence, $T_{r(post)} - \widehat{T}_{r(post)}$ cancels them out.

Figure 1 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) and for import losses (lower panels).

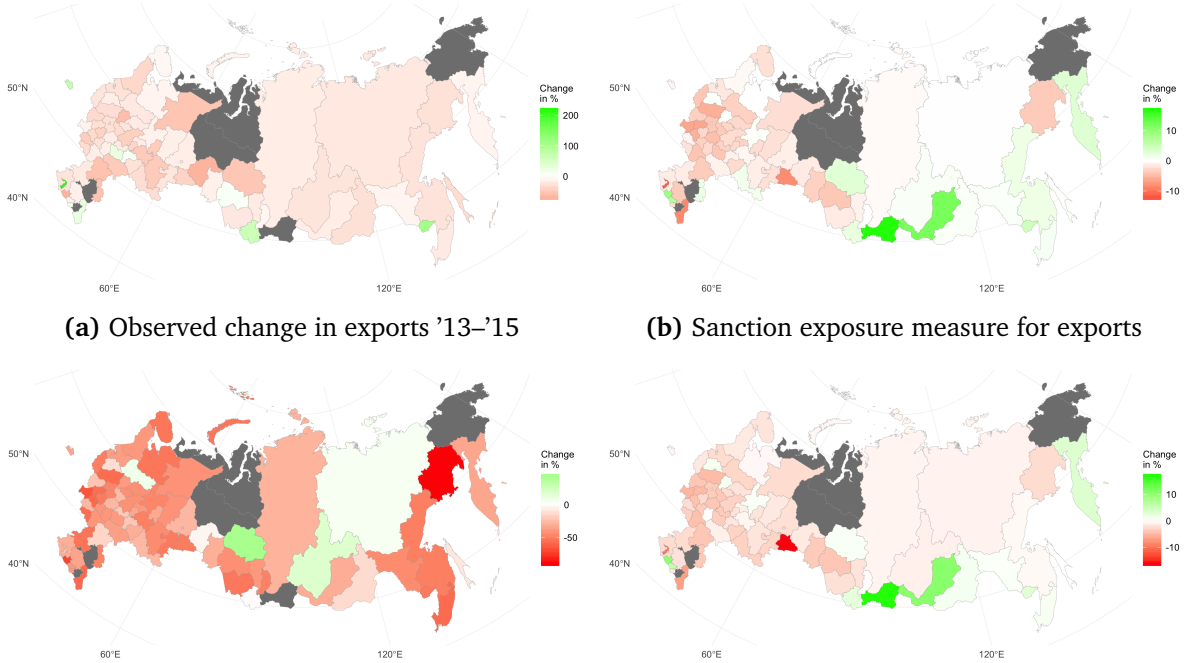
Basically, the right-hand panels extract the variation from the left-hand panels that is caused by economic sanctions, but no simultaneous developments. Interestingly, some regions can increase their international trade in response to the sanctions, e.g. by substituting trading partners. We will look 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

[16] 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.

[17] We provide more technical detail in in Appendix B.

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

Figure 1: Spatial distribution of regional sanction exposure



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

(b) Sanction exposure measure for exports

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

(d) Sanction exposure measure for imports

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 1, e.g. the relatively higher exposure in regions closer to the sanctioning countries.

Apparently, there is a distinct regional pattern in $sanctions\_exposure_r$. Based on the structural model, there are three reasons for this. First, a region's industrial structure determines whether it is hit by sanctions, or not (so much). Second, a regions' specialization on 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 can be easily by accounted for by panel econometrics, 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.[19] 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

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

map into a time-consistent spatial framework of about 2300 "rayons" (administrative districts) nested in 75 "federal subjects" (regions).[20]

We observe votes cast for every party (for the 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 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.[21] Moreover, we calculate election turnout.

Independent election observers like the OSCE have persistently criticized Russian elections over various irregularities.[22] In this respect, relying on electoral data at a very granular level (around 100,000 wards) has two advantages. First, it avoids aggregation fraud.[23] Second, it allows us to investigate statistical irregularities in the election data like an unusual clustering of even numbers around meaningful dates like 50 percent. In our subsequent analysis, panel econometrics will absorb regional variation in such irregularities. Remaining variation over time is unrelated to sanction exposure, as we will show. Thus, we are confident to use election data as indication for changing regime support in reaction to 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 the sanctions exposure computed above ($sanctions\_exposure_r$), 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

[20] 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.

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

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

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

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 and region r between t_{+1} and t_0.

Control variables $\text{X}_{ir,t}$[24] include regional demographics (population, migration, employment rate), labor force characteristics (age structure, qualification) and industry structure (sectoral employment shares).[25] In addition, we include a binary control for presidential elections. Note that, in this framework, this control captures potential differences in trends (rather than levels) between presidential and parliamentarian elections. Standard errors are clustered at the level of federal subjects r.

Regional variation in *sanctions_exposure*$_r$ used for identification in Equation (4) ultimately stems from three sources of variation. 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, such that *sanctions_exposure*$_r$ only depends on time-variant deviation of observed trade-flows from the counterfactual flows in the absent 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 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 a 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 general-equilibrium model, this build down to two identifying assumptions: One, there

[24] Source: Statistical Office of the Russian Federation.
[25] See Appendix A.2 for descriptive statistics on all the variables used.

may be no simultaneous change in bilateral trade frictions ϕ_{odt} for any country pair of relevant magnitude 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.

Thus, only major changes in bilateral trade costs could substantially bias our results, or changes that structurally affect Russian regions' trade in line with their sanction exposure.

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, that could potentially bias our results.

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

Simultaneously, five countries formally joined the WTO, and two of the new members (Tajikistan and Kazakhstan) share historical ties with Russia. Accordingly, 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 im- and exports relates to these countries. However, their accession to the WTO did not affect bilateral trade costs with Russian regions, since they were members of the Eurasian Economic Union before.

At first glance, Croatia's accession to the EU might seem problematic.[26] However, the Croatian economy was already integrated into the Single Market before it formally joined the Union 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 global trade costs in the 2012–2015 period. Specifically, no simultaneous development had a similar impact on Russian regions' bilateral trade costs. Accordingly, omitting the countries (Moldova, Georgia, Tajikistan, Kazakhstan, and Croatia) mentioned above from the calculation of the counterfactual trade flows has only minor impacts on the regression results.[27] We are thus confident that these minor violations of our identification assumption cannot meaningfully bias our estimates.

[26] With its new member, the sanctioning coalition increased its ability to affect Russia (Chowdhry et al., 2022).
[27] Indeed, point estimates even increase, while standard errors change only little.

Table 1: The effect of sanction exposure on Russian elections

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

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

4 Results

4.1 Main Results

We now turn to estimating our difference-in-differences model described in equation (4). Our focus is on the effect of *sanctions_exposure*$_r$ measured via regional export losses.[28] Table 1 reports results for different party outcomes and for overall turnout, with $\Delta\text{Voting}_{irt}$ calculated as changes between the first post-sanction election and the last pre-sanction election. Every cell reports another treatment coefficient for *sanctions_exposure*$_r$. Each line reports on a different outcome $\Delta\text{Voting}_{irt}$. Columns (1)–(4) successively include additional regional-level control variables. To facilitate comparison, Column (5) repeats results from our preferred specification in Column (4) with standardized coefficients. All estimations include election-type fixed effects.

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

[28] Corresponding results for import losses *sanctions_exposure*$_r$ can be found in Appendix C, Table 7.

subsequent elections in Russia. Regime support, i.e. the vote share of president Putin and his party "United Russia", increase significantly with regional sanction exposure. A one standard deviation increase in *sanctions_exposure*$_r$ (i.e. a decrease of 0.029 in regional exports relative to counterfactual exports in the absence of sanctions) increases electoral support of the governing regime by $(0.029 \times 0.486 \times 100 =)$ 1.4 percentage points. This is economically meaningful. Starting from high 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 *sanctions_exposure*$_r$ explains roughly 22 percent of the general increase in regime support.

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

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

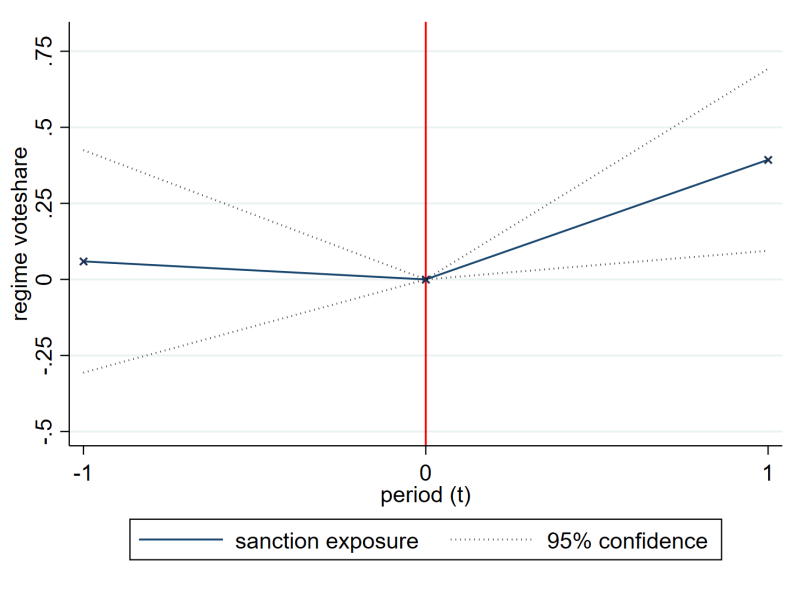
The turnout results, although statistically insignificant, speak against opponents of the regime just not participating in elections. Indeed, turnout tends to be higher the more a Russian region is affected by the sanctions.[29]

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

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

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

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



4.2 Effect Validity and Heterogeneity

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

Table 2: Placebo Effects on Pre-Sanction Outcomes

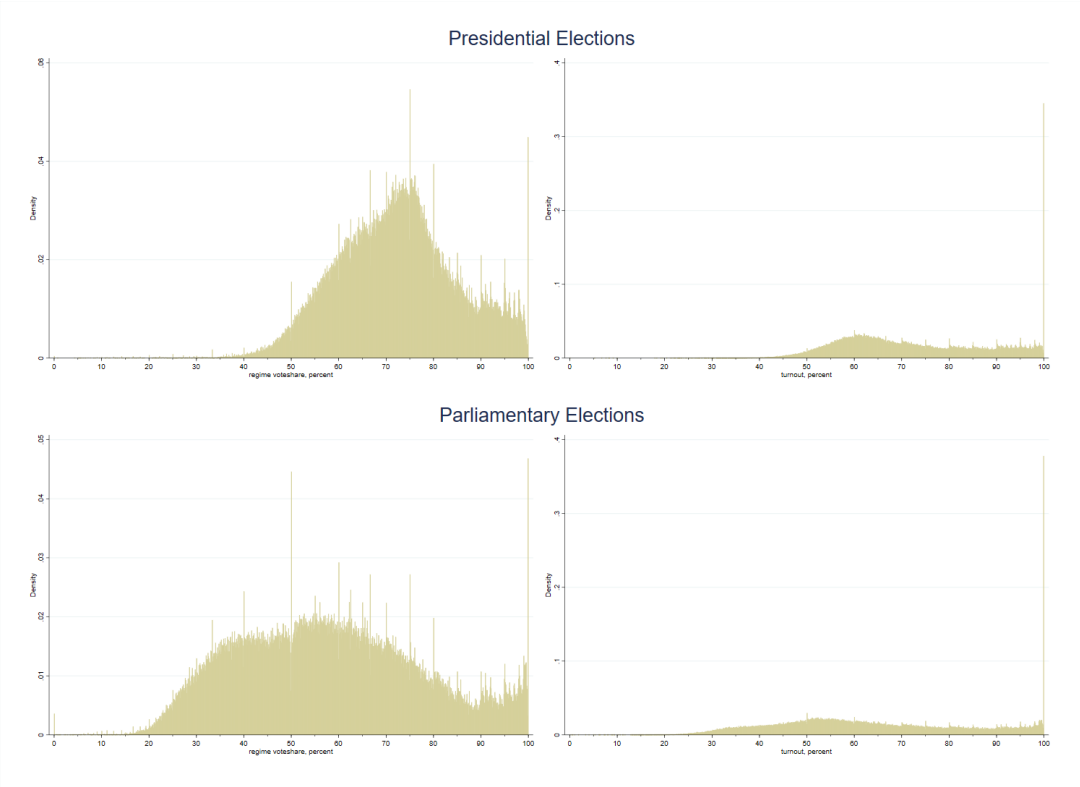
	(1) Δ regime	(2) Δ loyal	(3) Δ nationalist	(4) Δ communist	(5) Δ liberal	(6) Δ other	(7) Δ turnout
	Placebo-Effects (Exports) on Pre-Sanction Outcomes (Column)						
$sanctions\_exposure_r$	0.019	-0.069	0.040	-0.029	0.030	0.006	0.184
	(0.148)	(0.079)	(0.051)	(0.106)	(0.033)	(0.007)	(0.155)
Controls	+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

In Table 2, we regress changes in pre-treatment election outcomes on our sanction shock from the treatment period. The only way the sanction shock could have an impact on pre-treatment outcomes was through unobserved, time-invariant regional level characteristics. All the point estimates are small and statistically insignificant. This clearly supports our identification strategy.[30]

[30] Corresponding placebo tests for the import shock can be found in Appendix C, Table 8.

Figure 3: Even Numbers in Russian Election Results



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

These irregularities cannot bias our estimates as long as they are time-consistent (thus being absorbed by first-differencing or by regional fixed-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 3.

To assess whether statistical irregularities in the election data increase with $sanctions\_exposure_r$, we exploit the granular structure of our election data. Indeed, we observe election outcomes at the level of electoral precincts, with precincts being nested in rayons r. For each rayon, we calculate the share of precincts reporting even percentages (Columns 1, 3 and 5), or even percentages at meaningful dates like 50 or 75 percent (Columns 2, 4 and 6) in all precincts. We do so for all party outcomes (Columns 1–2), the vote shares of Putin and his party (Columns

Table 3: Placebo effect on election irregularities

	(1)	(2)	(3)	(4)	(5)	(6)
	All party shares		Regime shares		Turnout	
	Δ even	Δ meaningful	Δ even	Δ meaningful	Δ even	Δ meaningful
	Placebo-Effects (Exports) on Column-Outcomes					
$sanction\_exposure_r^{exp}$	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	+ political	+ political	+ political	+ political	+ political	+ political
Election-FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396	4,396	4,396

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

3–4), and turnout (Columns 5–6). Table 3 clearly speaks against sanctions leading to increased interference with election results. Consequently, we regard our main results to be unbiased by election fraud. Event study graphs on statistical irregularities, split by election type, can be found in Appendix C, Figure 5.

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

Table 4: Effect Heterogeneities

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

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

Table 4 shows that the effect of *sanctions_exposure*$_r$ on regime support does not vary across election type or between urban and rural regions. It is only slightly larger in oil- and gas exporting regions, and even in regions benefiting from the sanctions, i.e. with negative values

of $sanctions\_exposure_r$ indicating export gains, the point estimates remain positive, though statistically insignificant. This is true also when using the import measure. Additional results (not reported) show that the pattern is remarkably similar for all other party outcomes. Specifically, there is no indication that liberal parties might benefit from the sanctions in selective regions. As such, Table 4 reveals the positive effect of sanction exposure on regime support to be remarkably stable across Russian regions.

5 Discussion

Increasing regime support was certainly not the aim of the sanctions imposed on the Russian economy in 2014. Does this imply that sanctions failed to achieve their political goals? Not necessarily. Indeed, the concrete goals of economic sanctions are usually ill-defined, and thus difficult to evaluate. In economic terms, this implies that the counterfactual is difficult to assess. In the given case, one can presume that the political goals of the 2014 sanctions to Russia were to i) push the Russian government to withdraw its support to the Eastern Ukrainian rebels and its recognition of Crimea as Russian territory, ii) deter the Russian government from taking similar actions towards other territories (be they in Ukraine or somewhere else), and iii) deter other governments from taking similar actions.

Apparently, the sanctions did not achieve goal i). But did they prevent a counterfactual situation in which Russia invaded further parts of Ukraine already in 2014? Empirically, this question is not possible to answer. Indeed, it is not even possible to formulate a testable hypothesis, unless we have more reliable information on the political impacts of economic sanctions.

While sanctions might have been ineffective in reaching goal i), the fact that Russia did sign the 2015 Minsk agreements could be an indication that the sanctions might have been more effective in delaying similar actions towards other territories. Against this backdrop, more quantitative evidence on the different forms of sanctions' impact would be extraordinarily valuable. The 2014 sanctions might have achieved the first two goals from above by hitting its selectorate (Bueno De Mesquita et al. (2003)), i.e., the people who de facto make or influence policy making in Russia. Indeed, effects of targeted sanctions on the so-called *inner circle* could potentially compensate for the increasing support of the regime we measure. More research on the political consequences of economic sanctions is needed to thoroughly evaluate such tradeoffs.

Indeed, the results of our paper suggest that sanctioning countries face a trade-off: attempts to hit the inner circle have a political cost that comes in terms of greater popular support for the incumbent. In Russia, where the inner circle might matter substantially more than the population at large, the trade off is solved in favor of sanctions. In other countries, where the inner circle is weaker or the population at large more influential, policy makers might instead want to refrain from sanctions.

6 Conclusion

Our paper investigates political consequences of economic sanctions. We measure Russian regions' exposure to the sanctions imposed on the Russian economy in 2014 on the basis of trade losses caused by the sanctions. Our results reveal adverse political effects of economic sanctions. The sanctions imposed on the Russian economy in 2014 increased internal support for the sanctioned government. Specifically, regional exposure to the sanctions increased the vote share gained by president Putin and his party. We cannot infer on the long-run effects, but in the short-run, sanctions strengthen the sanctioned government.

This does not imply that sanctions were a political failure. Our analysis just reveals (some of) the political costs attached to economic sanctions. Similar to economic costs for the sanctioning countries, it might be worth paying these costs. However, to thoroughly evaluate such trade-offs, more research on the political consequences of economic sanctions is needed.[31]

A concrete policy conclusion from our results is that sanctioning countries should think about ways to minimize the "rally around the flag" effect resulting from economic sanctions. In the Russian case, economic sanctions nicely fit into the Kremlin's narrative of a hostile "Western World" interfering with the Russian way of living. Obviously, it is difficult to counter such propaganda in a country where the government controls the media. Still, it seems worthwhile to explore ways to accompany sanctions with measures to inform the general public about the very reasons for imposing the sanctions.

[31] In this spirit, Crozet and Hinz (2023) investigate the effect of the 2014 sanctions on political outcomes in France.

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A Data

A.1 Descriptive Statistics Main Variables

Table 5: Main Variables Observed

		t_{-1}	t_0	t_1
regime	mean voteshare	0.704	0.600	0.663
	SD	0.107	0.159	0.164
loyal	mean voteshare	0.050	0.084	0.035
	SD	0.059	0.070	0.038
nationalist	mean voteshare	0.089	0.096	0.118
	SD	0.042	0.054	0.077
communist	mean voteshare	0.140	0.180	0.152
	SD	0.068	0.065	0.062
liberal	mean voteshare	0.011	0.034	0.010
	SD	0.009	0.028	0.012
other	mean voteshare	0.007	0.007	0.022
	SD	0.009	0.008	0.021
turnout	mean value	0.718	0.656	0.624
	SD	0.128	0.130	0.172
sanction_exposure$_r^{exp}$	mean export loss	n.a.	n.a.	0.017
	SD			0.029
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 for presidential and for parliamentary elections.

A.2 Descriptive Statistics Covariates

Table 6: Control Variables Observed

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

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

B Computing General Equilibrium Counterfactual Trade Flows

In the following we describe the computation of general equilibrium counterfactual trade flows using the structural gravity equation of international trade, in the spirit of Dekle et al. (2007, 2008) and Anderson et al. (2018). The computation consists of five steps, including an iteration over the last two steps until convergence.

Recall the gravity model as in Head and Meyer:

$$X_{odt} = \frac{Y_{ot}}{\Omega_{ot}} \cdot \frac{X_{dt}}{\Phi_{dt}} \cdot \phi_{od} \tag{5}$$

and the fact it is estimated using Poisson Pseudo-Maximum Likelihood estimator

$$X_{odt} = \exp\left(\Psi_{ot} + \Theta_{dt} + \phi_{od}\right) \tag{6}$$

1. Estimate equation 5 using pre-sanction (2012, 2013) data and the PPML estimator described by equation 6. Keep $\widehat{\phi_{od}}$ (henceforth called $\widehat{\phi_{od(pre)}}$) and discard the rest.

2. Estimate equation 5 using post-sanction (2014, 2015) data and the PPML estimator described by equation 6. Keep $\widehat{\Psi_{ot}}$ and $\widehat{\Theta_{dt}}$ and discard the rest.

3. **Partial equilibrium**: use $\widehat{\Psi_{ot}}$ and $\widehat{\Theta_{dt}}$ from step 2) and $\widehat{\phi_{od(pre)}}$ from step 1) to compute to obtain Partial Equilibrium (PE) post-sanction counterfactual trade flows:

$$\begin{aligned}
\widehat{X}_{odt}^{PE} &= \exp\left(\widehat{\Psi_{ot}} + \widehat{\Theta_{dt}} + \widehat{\phi_{od(pre)}}\right) \\
\widehat{Y}_{ot}^{PE} &= \sum_{l \in d} \exp\left(\widehat{\Psi_{ot}} + \widehat{\Theta_{lt}} + \widehat{\phi_{ol(pre)}}\right) \\
\widehat{X}_{dt}^{PE} &= \sum_{l \in o} \exp\left(\widehat{\Psi_{lt}} + \widehat{\Theta_{dt}} + \widehat{\phi_{ld(pre)}}\right)
\end{aligned}$$

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

$$\widehat{\Omega_{ot}}^{CGE} = \sum_{\ell} \frac{\widehat{X_{\ell t}}^{PE}}{\widehat{\Phi_{\ell t}}^{CGE}} \widehat{\phi_{o\ell(pre)}} \quad \text{and} \quad \widehat{\Phi_{dt}}^{CGE} = \sum_{\ell} \frac{\widehat{Y_{\ell t}}^{PE}}{\widehat{\Omega_{\ell t}}^{CGE}} \widehat{\phi_{\ell d(pre)}}$$

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

obtained as

$$\widehat{Y_{ot}}^{\text{GE}} = \widehat{Y_{ot}}^{PE} \cdot \left(\frac{\widehat{\Psi_{ot}}^{\text{CGE}}}{\widehat{\Psi_{ot}}^{PE}} \right)^{\frac{1}{1-\sigma}} \quad \text{and} \quad \widehat{X_{dt}}^{\text{GE}} = \widehat{X_{dt}}^{PE} \cdot \left(\frac{\widehat{\Theta_{dt}}^{\text{CGE}}}{\widehat{\Theta_{dt}}^{PE}} \right)^{\frac{1}{1-\sigma}}$$

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

$$\widehat{X_{odt}}^{\text{GE}} = \frac{\widehat{Y_{ot}}^{GE}}{\widehat{\Omega_{ot}}^{\text{CGE}}} \cdot \frac{\widehat{X_{dt}}^{GE}}{\widehat{\Phi_{dt}}^{\text{CGE}}} \cdot \widehat{\phi_{od(pre)}}$$

The computation involves iterating between steps 4 and 5 until convergence.

Our final regional quantity is $\widehat{T}_{r(post)}$:[32]

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

[32]Had we calculated the measure using imports instead of exports, we would have had:

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

C Additional Results

C.1 Effects of Import Losses

Table 7: 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)
Δ loyal	-0.010	-0.012	0.020	0.064	1.291
	(0.118)	(0.100)	(0.095)	(0.054)	(1.096)
Δ nationalist	-0.109	-0.085	-0.062	-0.071	-1.739
	(0.074)	(0.073)	(0.065)	(0.062)	(1.501)
Δ communist	-0.393***	-0.400***	-0.381***	-0.304***	-5.376***
	(0.136)	(0.134)	(0.129)	(0.077)	(1.362)
Δ liberal	-0.021	-0.021	-0.040	-0.005	-0.158
	(0.049)	(0.041)	(0.035)	(0.012)	(0.392)
Δ other	-0.033	-0.033	-0.037	-0.041	-2.830
	(0.030)	(0.023)	(0.026)	(0.025)	(1.742)
Δ turnout	0.154	0.128	-0.040	-0.048	-0.446
	(0.203)	(0.207)	(0.185)	(0.189)	(1.749)
Controls	Baseline	+ labor force	+ industry	+ political	∼(4) STD.
Election-FE	Yes	Yes	Yes	Yes	Yes
Observations	4,396	4,396	4,396	4,396	4,396

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

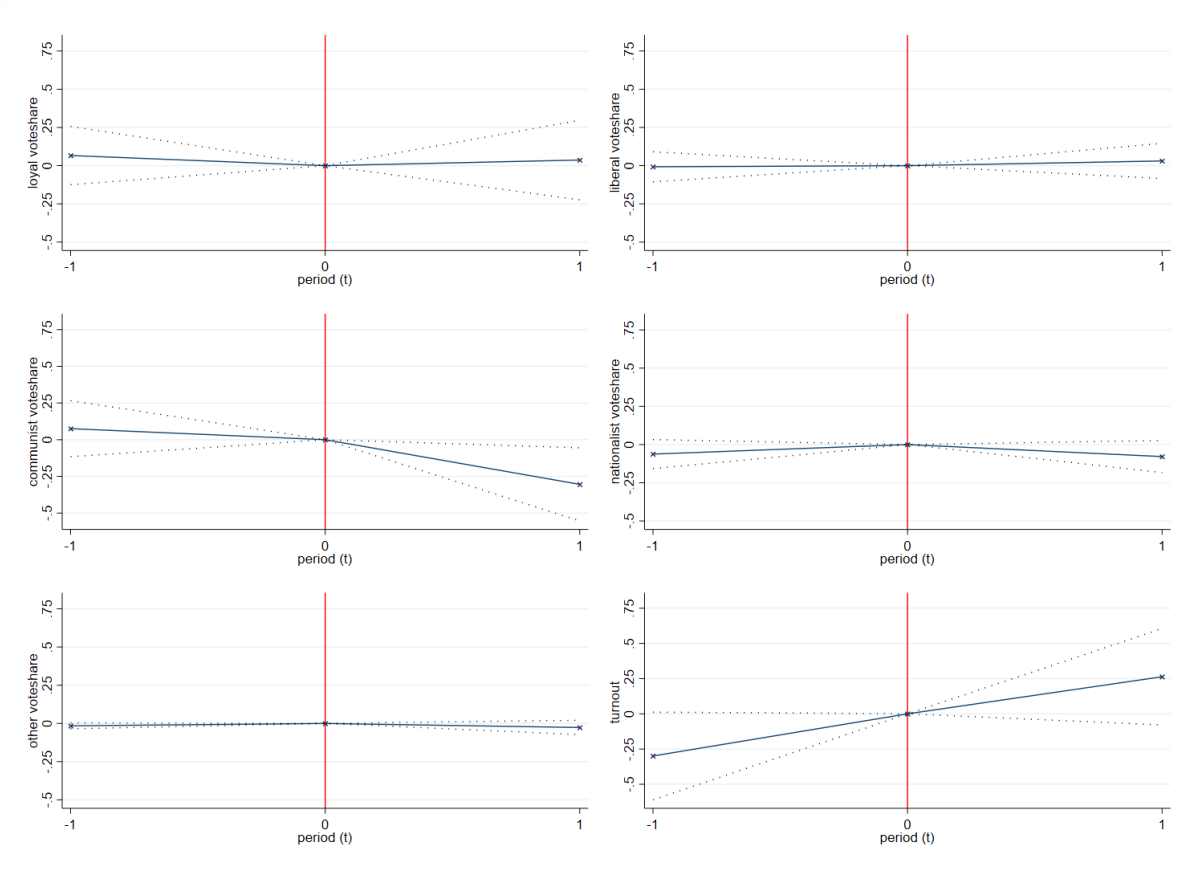
Table 8: Placebo Effects on Pre-Sanction Outcomes: Import losses

	(1) Δ regime	(2) Δ loyal	(3) Δ nationalist	(4) Δ communist	(5) Δ liberal	(6) Δ other	(7) Δ turnout
	Placebo-Effects (Imports) on Pre-Sanction Outcomes (Column)						
$sanction\_exposure_r^{imp}$	0.121	-0.063	0.063	-0.090	0.006	0.009	0.152
	(0.157)	(0.087)	(0.057)	(0.112)	(0.032)	(0.007)	(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.

C.2 Event Studies on Election Outcomes

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



C.3 Event Studies on Election-Irregularities

Figure 5: Placebo Effect on Statistical Irregularities