Sanctions and Misallocation. How Sanctioned Firms Won and Russians Lost.

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Abstract

This study examines the impact of 'smart' sanctions, a leading international policy, on Russian firms in 2014-2020. With a natural experiment involving staggered sanctions and data from over 600,000 firms, it investigates effects on both targeted entities and the aggregate economy. Contrary to expectations, sanctioned firms experienced a 32% increase in capital and 20% in revenue, attributed to substantial government compensation via subsidies, contracts, and loans. However, these measures negatively affected the broader economy due to worsened misallocation leading to an up to 1.11% drop in TFP. Sanctions hurt the economy but helped the very firms they intended to target.

Keywords: Sanctions, Russia, national security, political economy and trade, trade wars, misallocation, macro development, political connections.

JEL: F51, F52, F6, F38, H56, O4, O11

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1 Introduction

Sanctions serve as a major economic instrument in shaping global peace, aiming to guide nations toward responsible conduct, and deter conflict. The importance of understanding if and how sanctions can prevent or defuse wars cannot be underestimated. In the last 20 years a particular type of sanctions, "smart sanctions" became the go-to policy (Felbermayr et al. 2020) - the policy intends to pressure the elites while leaving ordinary people unaffected. While the US has over 70 countries under this type of sanction and is doubling down, there is little evidence on whether economic warfare by "smart" sanctions can prevent actual warfare.

This paper significantly advances the understanding of how "smart" sanctions work. It is the first to show not only how smart sanctions affect the targets, but also the aggregate effects of sanctions on the overall economy and its interactions with the state. The paper thus contributes to a largely theoretical literature on predicting the economic effects of sanctions (Itskhoki & Mukhin 2022, Lorenzoni & Werning 2022, Bianchi & Sosa-Padilla 2023, de Souza et al. 2023) or empirical literature on embargoes (Hsieh & Moretti 2006, Haidar 2017) by empirically estimating how the more common policy - *targeted* embargoes affect the targets and targeted country's incentives and decisions, and what collateral damage they create. The paper also makes a methodological contribution by using causal estimates to guide a calibration of a general equilibrium framework of misallocation (Bau & Matray 2020, Sraer & Thesmar 2023, Baqaee & Farhi 2020).

Following Russia's annexation of Crimea in 2014, the US and the EU initiated a series of targeted sanctions against specific Russian firms and individuals. The sanctions unfolded in several phases before Russia's full-scale invasion of Ukraine in 2022. I study the 2012-2020 period before the full-scale invasion, which constitutes a textbook case of "smart" sanctions – aimed to pressure the government by dividing the elites from their rulers, creating an "elite split" without hurting the rest of the population.[1] This treatment partially or fully banned the trade in inputs and outputs

[1]" Sanctions should be targeted in a way that has maximum impact on those whose behaviour we want to influence. Targeting should reduce to the maximum extent possible any adverse humanitarian effects or unintended consequences for persons not targeted or neighbouring countries. Measures, such as arms embargoes, visa bans and the freezing of funds are a way of achieving this. COUNCIL OF THE EUROPEAN UNION, Brussels, 7 June 2004"https://data.consilium.europa.eu/doc/document/ST-10198-2004-REV-1/en/pdf

of specific Russian firms in Western markets by either restricting imports and loans to these firms or banning their economic activity in these markets entirely. The targets were selected state-owned firms, private firms determined to be close to the state, and firms owned by sanctioned individuals.

A priori, while targeting a select group of firms, these sanctions may have important aggregate implications. And the effect of sanctions on their targets and in the aggregate depends on how these targets are chosen. For example, sanctions may target firms that are more productive and central to the economy, in which case these sanctions would harm both the firms and the aggregate economy. Conversely, sanctions may hurt the firms that are politically connected and over-resourced, and in hurting these firms sanctions could improve allocative efficiency and productivity in the aggregate through cleansing the economy of unproductive firms. Additionally, a third actor, the government may intervene and reverse the direct effects of sanctions in part, in full, or even more than in full, especially if the targets are close to the state.

I start by constructing a novel panel of 600,000 medium and large Russian firms in the services, manufacturing and agricultural sectors from 2012-2020 and collecting information on firm-by-year sanctions imposed by the US on Russia from the SPARK database[2]. The bulk of the data on firms comes from the tax authority and is required to be reported by these firms.

I use the panel data and within-firm variation over time to empirically test whether the sanctions on inputs and outputs indeed changed the inputs and outputs of targeted firms, and whether this change led to a change in their marginal revenue products of capital (MRPK). The staggered nature of sanctions allows me to net out important changes that took place in the economy at the same time, such as oil price shocks, or devaluation of the Russian ruble. Further, the DID setup does not require the sanctioned and non-sanctioned firms to have the same fixed characteristics, as any fixed differences are absorbed by the firm fixed effects. To estimate the average effect of sanctions, this method does require that the sanctioned firms would have trended

[2]SPARK-Interfax provides information about legal entities and private entrepreneurs. It collects data from dozens of sources including official Russian agencies such as the Federal Tax Service, the Central Bank, the Federal State Statistics Service. SPARK analytical tools allow one to cover the distribution of economic activity across the country as accurately as possible. In 2022, SPARK offered information on about 7 million acting Russian companies and entrepreneurs. SPARK was developed by Interfax, a private and independent major news agency in Russia.

the same way as non-sanctioned firms in a world without sanctions (i.e. the parallel trend assumption), for which I provide convincing evidence based on pre-trends. The effects I find are robust to controlling for time shocks in each disaggregated industry (year-by-industry fixed effects) and time shocks in different firm size quartiles (year-by-firm-size fixed effects, with size measured in the pre-period). I also confirm all results when I estimate the effects comparing sanctioned to not-yet-sanctioned firms, for which the parallel trend assumption is more likely to hold.

Surprisingly, I find that the sanctioned firms gained, not lost, in inputs, such as assets, capital, materials, and labor, and in outputs, such as revenue, value-added, and profits, after having their inputs and outputs sanctioned. The gains in inputs are more pronounced for SOEs, strategic, and military firms. On average, the MRPK of sanctioned firms declined as a result.

I collect additional data and outcomes - firm-level contracts, subsidies, loans, and credit cash flows - and find that government contracts, subsidies and total credit increased for all sanctioned firms. These three outcomes strongly point to the government protecting the treated firms. Moreover, this channel of protection by the government is similar to other cases when autocracies use non-market actions to smooth economic shocks (Campante et al. 2023).

One challenge to identification is that sanctions were imposed on the systemic companies in the military or other strategic sectors, with the government needed to subsidize in the new regime of confrontation that started in 2014, and the government would have subsidized these companies with or without sanctions. I test this hypothesis by separating the military and several definitions of strategic companies from the rest of sanctioned companies and the positive and large effect of sanctions on revenues and book value of capital remains for sanctioned firms even if they are not military or strategic firms.

To understand the aggregate GDP effects of the relative gains of sanctioned firms in a setting where access to resources is disrupted or reallocated between firms of different sizes and productivity, I use an aggregated heterogenous firm framework. The framework helps to both understand how efficient allocation of resources had been between the targeted and non-targeted firms before sanctions and how the change in resource allocation from sanctions affected the economy-wide change in TFP or GDP.

To diagnose pre-existing distortions, I use a natural framework for this setting – a wedge accounting framework (Chari et al. 2007, Restuccia & Rogerson 2008, Hsieh & Klenow 2009). I diagnose pre-existing distortions in allocation, in particular, between to-be-sanctioned and un-sanctioned firms, which are proxied by the differences in MRPK between these groups within narrowly defined industries.[3] I measure the ex-ante marginal revenue products of capital (MRPK) for these firms and correct for measurement error and transient adjustment costs using only the time-invariant component of the pre-existing MRPK for each firm. If the to-be-sanctioned firms appear to already use more resources than it is efficient, their MRPK will be lower than that in the rest of the firms in the same industry. A further gains in resources and a further decline in MRPK for these firms will lead to a decline in aggregate TFP.

To calculate the effect on aggregate TFP, I then use the causally estimated change in the wedge as proxied by the causally estimated change in MRPK (whereby I avoid attributing all cross-sectional dispersion of the wedges to misallocation contrary to the prior literature, but instead measure how the policy changed the wedges for specific firms) and combine it with an aggregate framework of the Russian economy. I use the frontier (Baqaee & Farhi 2020) heterogeneous firm framework to aggregate up the causally-estimated changes in the wedges to the overall changes in the economy's TFP. For comparison with other policies, I also use established (Hsieh & Klenow 2009, Petrin & Levinsohn 2012) frameworks as robustness.

The Baqaee & Farhi (2020) framework is applied for the first time for aggregating a causally identified change in wedges. This latter framework incorporates how the input-output linkages between firms (or sectors) reinforce or alleviate changes in misallocation for more or less central firms in the production network. The aggregate effects depend on whether the targeted firms were large, whether they were ex-ante low MRPK firms, and whether the effect of government shielding has increased the effective wedge these firms face. Thus, an increase in resources going to firms with ex-ante low MRPK leads to more misallocation and lower TFP. Across three frameworks, I find between 0.16-1.11% TFP reduction, implying that there was collateral damage of targeted "smart" sanctions.

[3]To account for pre-existing misallocation, I use the "indirect approach" that does not make specific modelling assumptions about a particular source of misallocation, but benefits from a sufficient statistic, the MRPK of a firm, as compared to the industry's harmonic averages.

Given these findings, this paper makes two distinct contributions. First, this paper is the first to analyse both the micro and macroeconomic effects of "smart" sanctions and through these effects, evaluate "smart" sanctions as an instrument to promote peace. Second, this paper reveals important dynamics for the domestic economy under other sanctions-like disruptions - trade wars, disruptions in supply chains and financial flows.

This paper is the first to show that "smart" sanctions also fail to achieve their more narrowly stated goals. If their goal was to avoid hurting ordinary citizens, I show the sanctions did the contrary because the government reallocated resources away from ordinary citizens and firms, and in doing so worsened the economy's productivity. If the goal of "smart" sanctions was to split the elites, they have failed as well, as the elites have become more dependent on the Russian state over time.

Past evidence on the effects of sanctions did not look at their effects on the actual targets or the aggregate economy, but instead only on some outcomes such as export deflection (Haidar 2017, Hilgenstock et al. 2023), elections (Gold et al. 2019, Peeva 2018), stock exchange responses (Draca et al. 2019), and the banking sector (Mamonov et al. 2023).[4] I distinguish myself from these papers in that I am the first to show the real output responses to sanctions from the real economy, its interactions with the state, and I do so both at the firm level and in the aggregate (using causal estimates to guide my calibration of a general equilibrium model).[5]

[4]Additionally, there is a set of papers studying embargoes Hsieh & Moretti (2006), Hinz & Monastyrenko (2022), Etkes & Zimring (2015), effects on countries that impose sanctioning countries (Bachmann et al. 2022, Crozet & Hinz 2020, 2023), and theoretical papers are emerging due to the full-scale invasion of Ukraine by Russia in 2022 (Itskhoki & Mukhin 2022, Balyuk & Fedyk 2022).

[5]Ahn & Ludema (2020) also look at the effect of the first wave of sanctions on Russian firms. My results and method differ from this paper: I find positive effects of sanctions on most firm-level variables, as opposed to a negative effect. Ahn & Ludema (2020) find a negative effect of sanctions because they conflate the effect of sanctions on Russian firms with the effect on foreign firms owned by Russian individuals abroad. The assets of these latter individuals were frozen and, therefore, the foreign firm outcomes mechanically worsened. My results are different because I focus specifically on Russia-based sanctioned firms. I also look at the effects over several years and several waves of sanctions and estimate the aggregate implications.

Additionally, a later contribution by Keerati (2023) replicates the results of this paper on a subset of large Russian firms and emphasizes a complementary channel, namely more bank lending to sanctioned firms due to market incentives to lend to more productive borrowers. However, I show that these firms are not high-return, productive borrowers as they generate less revenue for a marginal unit of capital compared to non-sanctioned firms both before and, even more so, after sanctions. In addition, the Russian banking sector is dominated by the state (out of the top six Russian banks, five are state-owned) and such banks therefore have likely supported sanctioned firms primarily due to political economy rather than market incentives. This is also consistent with additional evidence I provide on subsidies and government contracts.

Additionally, as sanctions are a type of trade barrier, in examining their effectiveness, I contribute to the ongoing debate on the role of trade in promoting peace. Existing research suggests that the desire to trade can reduce the likelihood of conflict (Polachek & McDonald 1992, Mansfield 2021, Barbieri 2002). However, Martin et al. (2008) indicate that trade only deters conflict when nations have no alternative trading partners. The "smart" sanctions embody loss of trade but for very specific firms, and my results show it is not a strong enough lever to prevent conflict because sanctioned elites switch to an alternative trading partner, such as their government.

My second contribution from studying sanctions is to demonstrate the economy-wide dynamics under other sanctions-like disruptions, such as trade wars or supply-chain or financial flow disruptions. I show that as the economy closes to trade and international finance, the economy tilts towards politically connected activities, empowers the elites and not the society, and pushes factors of production into parts of the economy where the marginal value products of factors are low, using the terms of Atkin & Donaldson (2022).

While the disruption I study is targeted and not sector-level, I show that despite the trade disrupting for the low marginal product firms, these are exactly the firms that amass resources and grow, possibly at the expense of high marginal product firms. Therefore, in an economy-wide trade disruption where every firm is affected equally, the reallocation dynamics may be even further exacerbated, as the high marginal product firms are further hurt by trade.

I believe this to be especially relevant against the current backdrop of globalization reversal. By showing the detrimental effect on allocative efficiency of financial and trade disintegration, this paper validates previous findings on liberalizations (Kehrig (2015), Varela (2018), Larrain & Stumpner (2017) and Gopinath et al. (2017)) or financial crises (Huang et al. 2020).

In particular, by looking at the *tightening* of financial and trade barriers due to sanctions - rather than the removal of financial barriers due to liberalization - I complement Bau & Matray (2020) who show allocative efficiency improving after India's capital market liberalization. I find the worsening of allocative efficiency during financial and trade tightening. Along with Bau & Matray (2020), my paper is one of the first to use causal inference to guide changes in misallocation. In contrast to Bau

& Matray (2020), in my setting the sanctions treatment affects specific firms within a sector and not sectors as a whole, which helps to directly attribute changes in within-sector allocation to treatment.

Finally, this paper also contributes to the literature on political connections and economic outcomes (Fisman 2001, Schoenherr 2019, Bertrand et al. 2020, 2023, Faccio et al. 2006, Acemoglu et al. 2018, Khwaja & Mian 2005), on political economy and trade (Campante et al. 2023, Arthi et al. 2020, Kleinman et al. 2020), and on the political economy of Russia (Cheremukhin et al. 2017, Enikolopov et al. 2011, 2020, Zhuravskaya et al. 2021). This paper adds to this work by not just documenting the consequences of political connections per se, but also showing how these connections manifest themselves under negative shocks/

This paper has external validity beyond the case study of Russia. Recently, anecdotal evidence points that support of the Chinese state to sanctioned Huawei and connected companies has reached "unprecedented levels", whilst the company has been constrained by export controls from the US for the five past years.[6] Additionally, Iran announced "budget to resist US sanctions", signalling that the government will make attempts to offset the sanctions.[7]. Therefore, while I estimate a joint effect of sanctions and shielding, to the extent that the government's reallocation is expected to accompany sanctions in other settings (albeit conditional on the ability of the government to compensate), the joint effect is exactly the effect I want to estimate (rather than the isolated effect of sanctions which bears no external validity). In the cases when targeted countries are unable to compensate, are either when sanctions are strong and untargeted making it a different policy in question, or when the government has no power over country's rents, which is a rare case, with autocratic and centralized states sanctioned significantly more frequently than democratic states.

The paper is organised as follows. Section 2 provides three distinct heterogeneous firm frameworks for accounting for the effects of wedges. Section 3 describes the firm-level and sanctions data as well as the context of the sanctions episode and the allocation of resources in Russia before sanctions. Section 4 discusses the reduced-form empirical strategy. Section 5 reports the reduced-form effects of sanctions on

[6]https://www.bloomberg.com/graphics/2023-china-huawei-semiconductor/

[7]https://www.theguardian.com/world/2019/dec/08/iran-unveils-budget-of-resistance-to-sanctions-with-help-from-5bn-russian-loan

firms, as well as the aggregate effects of the sanction episode. Section 6 concludes.

2 Framework

Understanding how firm-level shocks in a granular economy change the aggregate GDP requires a framework. A framework that focuses on reallocation of resources is most suitable in this case because sanctions intended a change in specifically resource allocation (targeting the imports, loans and outputs of firms) and the government response, as I show in my empirical section, also responded with policies that target inputs and outputs of firms.[8] I therefore use a framework that allows to measure for every firm, whether it appears to have changed it allocation of resources towards optimum or away from after a shock that targeted the allocation. Furthermore, I use a series of established and frontier frameworks to connect the changes of misallocation at the firm-level, to the aggregate changes in economy.

To get a sufficient statistic of misallocation, I use a standard framework from the literature where firms have heterogeneous productivities and use capital K_i, labor L_i and materials M_i inputs, and face wedges on these inputs, modelled as taxes or subsidies τ_i^K, τ_i^L and τ_i^M.

These wedges create an arbitrary allocation of resources by increasing the effective price on inputs that a firm faces. Looking differently, the distortions in the operation of firms are represented as wedges that would rationalize the observed use of inputs by profit-maximizing firms.

The firm i maximizes its profits while facing taxes or subsidies τ_i^K, τ_i^L and τ_i^M on its inputs.[9]

$$\pi_i = p_i F_i(K, L, M) - (1 + \tau_i^L) w L_i - (1 + \tau_i^K) r K_i - (1 + \tau_i^M) p^M M_i \tag{1}$$

[8] While these changes might have also affected the ability of firms to churn inputs into outputs, I study a relatively short period of 5 years or less since the shock, which is unlikely to have granted enough time to have change their production technologies and physical productivity. Additionally, I find no suggestive evidence that physical productivity changes.

[9] This model also allows for the case that there is misallocation in output, rather than inputs, for example, markups. This can be added as a wedge on output $(1 - \tau_i^Y) P_i Q_i$, but the effect of τ_i^Y cannot be separately identified from the joint effect of τ_i^L, τ_i^K and τ_i^M . Therefore, I keep wedges on inputs, bearing in mind that these two wedges jointly can mean a distortion on output.

Here the $F_i(K, L, M)$ is firm i's production function with diminishing marginal returns in each input. When firm takes first-order conditions, it equalizes its marginal revenue to each input with the marginal cost of this input.

$$\{K_i\} : p_i \frac{\partial F_i(K_i, L_i, M_i)}{\partial K_i} = (1 + \tau_i^K) r \equiv MRPK_i \tag{2}$$

$$\{L_i\} : p_i \frac{\partial F_i(K_i, L_i, M_i)}{\partial L_i} = (1 + \tau_i^L) w \equiv MRPL_i \tag{3}$$

$$\{M_i\} : p_i \frac{\partial F_i(K_i, L_i, M_i)}{\partial M_i} = (1 + \tau_i^M) p^M \equiv MRPM_i \tag{4}$$

The bigger is the wedge on any input, call it τ_i^X, the higher is the marginal revenue product on that input. Positive τ^X represents implicit tax on inputs, and negative τ^X represents an implicit subsidy. I define $MRPK_i$, $MRPL_i$ and $MRPM_i$ as measures of the direction of misallocation. The higher these measures are, the higher are the implicit taxes on capital, labour and material inputs of firm i. With an assumption on the form of the production function $F_i(K, L, M)$, I recover $MRPKi$, $MRPLi$ and $MRPMi$.[10]

How does the presence of τ^K, τ^L τ^M affect aggregate output? I use three frameworks of the aggregate economy to make this calculation. I do so to make my results comparable to other studies of misallocation and provide bounds to the aggregate effect from different calculations using different assumptions. First, I use a framework of Hsieh and Klenow (2009) as a baseline (also called a "horizontal economy"

[10]For the empirical part of the paper, I assume the Cobb-Douglas production function assumption, which allows me to recover the marginal revenue products directly from the average products, for which I need data on the revenue ($P_i F_i$), the 4-digit-industry output elasticity α and the value of inputs rK_i, wL_i and $p^M M_i$:

$$\underset{L_i, K_i}{max} \pi_i = p_i A_i K_i^{\alpha} L_i^{\beta} M_i^{\gamma} - (1 + \tau_i^L) w L_i - (1 + \tau_i^K) r K_i - (1 + \tau_i^M) r M_i$$

$$\{K_i\} : \alpha \frac{P_i F_i}{K_i} = (1 + \tau_i^K) r \equiv MRPK_i \tag{5}$$

$$\{L_i\} : \beta \frac{P_i F_i}{L_i} = (1 + \tau_i^L) w \equiv MRPL_i \tag{6}$$

$$\{M_i\} : \gamma \frac{P_i F_i}{M_i} = (1 + \tau_i^M) p^M \equiv MRPM_i \tag{7}$$

In the modelling part of the paper, I relax the Cobb-Douglas assumption in the calculation of the aggregate effects.

model). It is simple and intuitive. Every firm only takes in capital and labor as inputs and faces wedges on those inputs. Therefore, it is possible to use value added production functions to model firms, and linkages between industries do not play a role in exacerbating misallocation. This approach uses a Cobb-Douglas assumption of each firm's production function, and aggregates firms in each industry with CES aggregator (and then each industry with a Cobb-Douglas aggregator).

The second approach relies on Petrin and Levinsohn (2012) as reformulated by Baquaee and Farhi (2019). I call it the PL framework. It relaxes parametric assumptions of the production function and is a general approach, requiring only the knowledge of wedges and general equilibrium changes in inputs for each firm. This approach can be applied for each industry separately, or for the economy as a whole.

Finally, I use the full Baquaee and Farhi (2020) framework with I-O linkages and arbitrary nested CES production functions to aggregate changes in wedges to changes to the aggregate productivity. Applying this framework to my data has an advantage of weighing more the distortions in sectors that play a more central role Russia's Input-Output network.

One more caveat is in order. Each of these frameworks will give me a value for aggregate Russian GDP, provided that the aggregate capital and labor in Russia stays constant, which in effect, is a number for aggregate TFP. How sanctions affected the total resources in Russia is impossible to identify without further assumptions separating sanctions from other Russia-level shocks. However, it is likely that sanctions has decreased rather than increased total capital coming into Russia. In view of this, the decline of aggregate Russian TFP will be a lower bound for the decline of Russian GDP due to sanctions.

3 Data and context

3.1 Firm-level data

My firm-level data comes from the SPARK-Interfax database that aggregates official and open balance-sheet, tax, employment and ownership information at the firm-by-year level. SPARK provides a firm-level panel dataset of Russian private and state-

owned firms covering manufacturing, agriculture and services sectors. The panel dimension of this dataset is useful for quantifying how firms change over time and will be also crucial to my adjustment procedure to measurement error in wedges. An additional beneficial feature of this dataset for this study is that it is firm-level and not plant-level. My goal is to study misallocation across decision-makers, which makes it crucial to identify the boundary of a firm. I define a firm as a unique tax-ID number, so that the branches or plants of the same firm in different regions are aggregated to one observation, but if one firms owns a stake at another firm, the two are coded as two observations. I expect a lesser role of measurement error and in a firm-level dataset, as opposed to a plant-level dataset.

I extract information on firm revenues, capital stock (as measured by book value) wage bill and payments to materials. The total number of firms that reported at least one of these variables in 2019 or 2020, as shown in Table 2, was 946,956[11]. For my analysis I only use for-profit firms, including for-profit SOEs, and I exclude banks. The value added of these firms covered 61% of Russian value added in 2018 and 30% of official employment (note that the total revenue of these firms *exceeds* Russian GDP by more than twice due to intermediate inputs being double-counted in the buyers' and sellers' revenues). The Table 2 below summarizes the sample by firm groups: private for-profit firms, state-owned for-profit firms. State-owned firms are defined by SPARK, as listed in the official Russian statistics bureau list of SOEs, and include not only firms that are directly owned by the state (e.g "PAO Rosneft"), but also private firms that are owned by the state-owned firms (e.g. "OOO RN-Vankor").

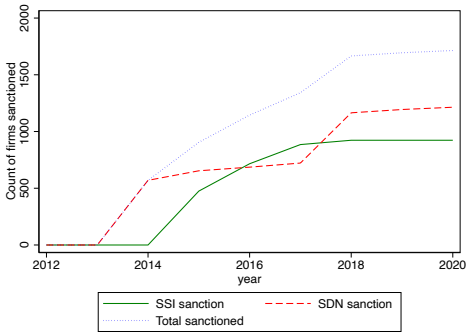
3.2 Sanctions on Russia 2014-2020

Sanctions were rolled out by the US and EU against Russian entities and individuals as a response to the annexation of Crimea through years 2014-2020[12].

The sanctions were of two types: SDN (Specially Designated National) and SSI (Sectoral Sanctions Identifications). The SDN-type sanctions forbid any transaction (e.g. export, import, lending, issuing stock, leasing) with a sanctioned firm or indi-

[11]The yearly coverage of firms that reported both book value of capital and revenues are on average 600,000.

[12]Japan, Canada, Australia, New Zealand and Ukraine have followed the US and EU and largely repeated list of sanctions entities of the US.



Notes: The graph shows how treatment was rolled out. The red dashed line indicates the roll-out of blocking sanctions. The green solid line indicates the roll-out of input sanctions. The blue dotted line indicates the roll-out of any sanctions.

Figure 1: Sanctions roll-out

vidual, as well as any firm owned by an SDN individual or an SDN firm by more than 50 per cent (this rule was named "OFAC rule of 50"). Further, the sanctions froze any assets in the United States of an SDN firm or individual. SSI sanctions instead, affect inputs: they restrict long-term (longer than 14 days) debt issuance, equity financing and transactions with any such debt of equity of the sanctioned firm[13]. The SSI sanctions were issued mostly against Russian banks and companies, military or double-use technology firms and companies in the oil and gas sector. However, after applying the OFAC 50% rule, the coverage extends to almost all industries.

I create a dataset of sanctions at the firm level that includes not only the firms directly listed by the US Department of Treasury but also the historical majority-owned level-one subsidiaries of these firms and of the firms belonging (by more than 50% ownership) to sanctioned individuals. I first use the list of firms and names and announcement dates from the US Department of Treasury announcements on the official website. Then, I add all one-level-down historical subsidiaries of these companies and business individuals with SPARK Database that keeps track of historical ownership[14]. I use confirmed ownership links at the time of imposition of sanctions, so that the effect I later estimate is the intention-to-treat effects. The ownership information in SPARK comes from three sources: Rosstat, the firm's annual report and the official firm registry EGRUL. I use the union of these three sources after I retrieve

[13]Most companies under the SSI sanctions were also treated with the US stopping certain technology exports to these companies. I consider this as still a negative capital input shock

[14]For individuals, the match is made using the first, middle, and last name. Since the list contains political figures as well, who cannot legally own business, I drop them by manually checking using open sources whether the individual matched with any firm is a business person or a political figure.

this information from SPARK Database.

Sanction type	Private	State-owned	Total
SDN	555	113	668
SSI	402	51	453
SSI and SDN	315	51	366
Total	1,272	215	1,487

Notes: This table is a cross-tabulation of the sanctioned firms (reporting balance sheet data) by ownership. SDN is the group of firms that are sanctioned by blocking sanctions, SSI indicated the group of firms sanctioned by input sanctions. The sample includes firms that are sanctioned by association with the directly sanctioned firm via majority ownership.

Table 1: Sanctions by ownership

I create a dataset of 1,714 sanctioned firms, for 1,487 of which I have firm-level data at least for one year. Appendix E describes the creation of the sanctioned dataset in detail. I record the distinction between the two types of sanctions in the US: SSI and SDN.[15] I look at both types of sanctions and each treatment separately.[16]. I assign the year of treatment as the year of the imposition of the sanctions if the announcement happened before May that year. Otherwise, I assign the following year as the year of treatment, since the application of sanctions takes place 60 days after the announcement.[17]

Figure 1 shows how the sanctions were rolled out. There is a gradual increase in total sanctions between 2014 and 2018 which then plateaus. Appendix Table 2.A2 tabulates sanctioned firms by year and the types of industries sanctioned in each wave. Each wave of sanctioned firms is comparable to each other, and a wide set of industries is represented by the first three waves.

3.3 Coverage of the economy

Table 2 reports the sample I use for the regressions and model calibrations. The value added of all firms in the analysis covers 61% of Russian GDP, the value taken from Rosstat, the Russian Statistics Bureau. Sanctioned firms cover 1% in total Russian employment and their value added is 13% of total Russian GDP (21% and 4% of value added and employment in the dataset). The majority of firms are private firms and they cover 54% of Russian GDP.

[15] the EU follows the US in the type of treatment with almost identical lists.
[16] The EU mimicked the US treatment for almost all firms
[17] "Russian Sanctions Update", Morgan Lewis, April 7th, 2020.

Sample	Count	Share of Value Added	Share of Revenue	Share of employment	Share of Value Added in Russian GDP	Share of Revenue in Russian GDP	Share of Russian employment
All firms	946,956	100	100	100	61	218	30
Firms with all variables	154,825	92	75	68	56	164	21
Private firms	942,542	89	93	94	54	202	29
State-owned firms	4,414	11	7	6	7	16	2
Sanctioned firms	1,046	21	13	4	13	28	1

Notes: This table reports the sample coverage for the firms in the SPARK dataset in 2020 or 2019 if data for year 2020 is missing. An observation is at the firm level. The Russian GDP and Russian employment in columns "Share of Value Added in Russian GDP" and "Share of Revenue in Russian GDP" and Russian employment in column "Share of Russian employment" are aggregate economy-wide values of GDP and employment and are taken from Rosstat.

Table 2: Sample used for analysis

Additionally, Appendix Table 2.A1 reports a summary table of all variables used in the main empirical exercises. Each observation is firm-year. The sample has 6,238,848 observations, which is 1,612,659 firms that ever reported and 927 industries. The typical firm is a domestic firm. There are 1,132 firms under any sanctions. The variables include revenue, value added, capital, wage bill, materials bill, employment, age, MRPK and MRPL and a type of firm. All the balance sheet variables are in rubles.

3.4 Static misallocation pre-sanctions

Do resources in Russia appear misallocated through the lens of the framework from Section 2? If capital and labour markets were not distorted, more capital and labour would flow to the relatively more productive firms. This means that input use and firm TFP should be positively related, while the marginal revenue products of labour and capital should be unrelated to firm TFP because inputs flow to more productive firms up until these marginal products are equalised. In Appendix D, I display the allocative efficiency characteristics of the whole economy and show how firms' marginal revenue products are strongly related to their TFP, i.e. more productive firms face higher (positive) wedges.

What role in overall misallocation play specific wedges, such as wedges between a group of to-be sanctioned firms and all other firms? Could such distortions explain the barriers faced by more productive firms? Figure 2 below compares the

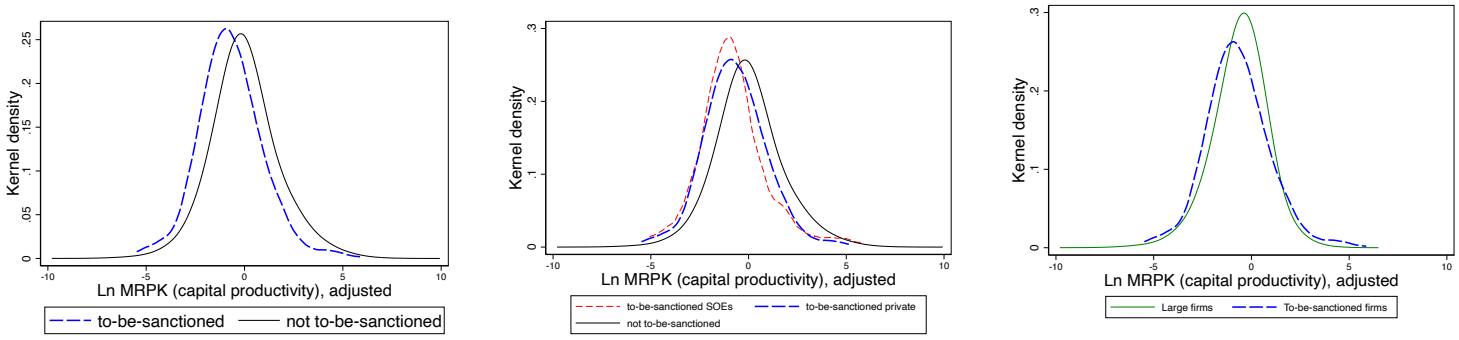
density distributions of the firm-level log capital productivity (log MRPK) between to-be sanctioned firms and not sanctioned firms. I compute MRPK as firm revenue divided by book value of capital for each firm. This measure of MRPK is adjusted for measurement error using firm, industry and year fixed effects as explained in Appendix B.[18]

Plot (a) in Figure 2 demonstrates that to-be-sanctioned firms have on average lower MRPK than the non-sanctioned group. This points at to-be sanctioned firms being already over-resourced in terms of capital prior to sanctions and productivity gains existing if capital is reallocated from the to-be treated group to the rest of the economy. It is important to note that the MRPK for to-be-sanctioned firms is lower when looking *within* each 4-digit industry.

What could explain a lower MRPK for the whole distribution of to-be-sancitoned firms? The sanctioned group contained disproportionately more SOEs, but also some private firms. Do to-be-sanctioned firms have low MRPK because they have too many SOEs? Plot (b) shows that the SOEs alone are not the reason. The private to-be sanctioned firms also have a lower average MRPK relative to the economy-wide distribution, as well as the the to-be-sanctioned SOEs. Additionally, could to-be-sanctioned firms have lower MRPK just because, being large firms, they have more assets (and therefore higher book value of capital)? Plot (c) compares the MRPK of to-be-sanctioned firms and the firms in the top quartile of firm capital. Again, the to-be-sanctioned firms show lower MRPK before sanctions.

In sum, I confirm that the to-be sanctioned firms were already "too large" before sanctions, at least in terms of capital. Since the sanctioned firms were chosen by the US intelligence services as connected to the current government, this finding points out that there is potential misallocation between connected firms and all other firms.

[18] Netting out the industry fixed effects allows me to net out the industry-level Cobb-Douglas power α and therefore gives me the correct measure of firm $MRPK_i \equiv (1 + \tau_i^K)r$ after adjustment.



(a) MRPK of sanctioned versus other firms pre-2015

(b) MRPK of to-be sanctioned firms, split by SOE and private pre-2015

(c) MRPK of to-be sanctioned firms versus large firms (top quartile by firm capital) pre-2015

Notes: The plots show the kernel density of natural logs of MRPK in the period before sanctions. First row: the blue dotted bell curves are the kernel densities for the sanctioned sample. The black bell curves in the top two graphs are the kernel densities for the sample of not sanctioned firms. The red dotted bell curve indicates the kernel density for the to-be-sanctioned SOEs. Second row: the green dashed bell curve plots the MRPK of large firms defined by being in the top quartile of book value of capital in 2014. Capital productivity (or $MRPK_i$) refers to revenue per unit of book value of capital, which is proportional to the marginal products of each factor in my framework. The MRPK measure is time-invariant because it is adjusted for measurement error with firm and year fixed effects and de-meaned by 4-digit industry using the firm panel for 2012-2014.

Figure 2: Allocations of capital before 2015

4 The effect of sanctions on firms

Table 3 shows the summary of the key variables by sanction type and compares the averages of these key variables. The sanctioned firms, either SSI, SDN or both are larger in terms of average value added, total revenue, the book value of capital and wage bill. The average raw MRPK is lower in the sanctioned firms relative to non-sanctioned firms.

Knowing that politically connected to-be-sanctioned firms have "too much capital", the first hypothesis is that sanctions, raising the cost of inputs would reduce misallocation. However, there is anecdotal evidence that the politically connected firms, both private and state-owned, managed to secure more funding from the Russian government as a response to sanctions.

Sberbank, Russia's largest state bank had the central bank purchase a significant amount of the bank's new debt since sanctioning, moreover, it is known to have extended preferential loans to all owners who own a share of a sanctioned company.[19] Viktor Vekselberg, Renova Group's owner has had the credit line extended

[19]This is the announcement of special "credit line": Central bank of Russia secured loans and this article also mentions that the companies started to get support staring after the sanctions were announced: RBC: Special law for sanctioned firms.

	Not Sanctioned	SDN	SSI	SSI and SDN	Total
Labor count, latest year	26.51	1005.2	772.1	997.9	27.68
	(193.0)	(1710.4)	(1702.9)	(1827.7)	(205.4)
Firm age, yrs	8.059	12.76	12.40	13.40	8.064
	(6.442)	(7.445)	(7.441)	(8.462)	(6.446)
Private firm (dummy)	0.995	0.839	0.869	0.848	0.995
	(0.0693)	(0.368)	(0.338)	(0.360)	(0.0705)
SOE (dummy)	0.00482	0.161	0.131	0.152	0.00499
	(0.0693)	(0.368)	(0.338)	(0.360)	(0.0705)
Direct sanction (dummy)	0	0.124	0.468	0.216	0.000295
	(0)	(0.330)	(0.500)	(0.412)	(0.0172)
Ln value added	15.83	19.37	20.00	19.82	15.85
	(2.572)	(2.414)	(2.791)	(2.914)	(2.586)
Ln revenue	16.27	20.07	20.35	20.17	16.28
	(2.235)	(2.782)	(3.438)	(3.383)	(2.240)
Ln book value of capital	13.43	18.54	19.10	19.12	13.44
	(2.819)	(3.314)	(4.089)	(3.640)	(2.825)
Ln payment to labor	14.77	18.00	18.32	18.10	14.79
	(2.459)	(2.871)	(2.928)	(2.869)	(2.473)
Ln materials	16.40	19.06	19.01	18.97	16.42
	(3.066)	(3.300)	(3.745)	(3.622)	(3.074)
Foreign-owned firm (dummy)	0.000490	0	0	0	0.000489
	(0.0221)	(0)	(0)	(0)	(0.0221)
Suppliers to state and SOEs (dummy)	0.117	0.591	0.598	0.668	0.118
	(0.321)	(0.492)	(0.491)	(0.472)	(0.322)
Ln firm MRPK	2.718	1.601	1.348	1.154	2.716
	(2.837)	(2.780)	(3.440)	(3.043)	(2.838)
Ln firm MRPL	2.500	1.975	1.841	1.757	2.496
	(1.964)	(1.787)	(1.865)	(2.074)	(1.964)
Observations	915,478				

Notes: This table reports summary statistics for the firms in the SPARK dataset in the pre-preiod: 2012 to 2014 by type of sanction. An observation is at the firm level, and the latest reported year out of 2012, 2013 or 2014 is kept. SDN is the group of firms that are sanctioned by blocking sanctions, SSI indicated the group of firms sanctioned by input sanctions. The sample includes firms that are sanctioned by association with the directly sanctioned firm via majority ownership.

Table 3: Summary by sanction type

by Promsvyazbank in 2018[20]. Leonid Mikhelson has been reported to request the government to help fund the creation of deepwater drilling equipment to replace the U.S. imports[21]. The minister of economy, Denis Manturov, openly shared that the government will use the targeted purchases and short-term loans to "stabilize" the sanctioned companies in the metals, machinery and energy sectors.[22]

Promsvyazbank was nationalized and then re-purposed to compensate the losses from sanctions of Russia's defence sectors[23]. By 2015 the Russian state started a bank recapitalization program worth about 1.4 trillion rub, or 1.2% of GDP to support all banks directly or indirectly affected by the sanctions.[24]

Further, there are two examples when the government strategically granted contracts to sanctioned firms: it provided sanctioned Bank Rossiya the sole contract to service the $36 billion domestic wholesale electricity market and it granted the contract to build a bridge linking the Russian mainland with Crimea to a sanctioned construction company (Stroygazmontazh), and selected a sanctioned bank (VTB) to be the sole manager of the government's international bond sales.[25]. How did allocation of resources change due to this governmental response after sanctions were imposed?

The sanctions were imposed on groups of Russian firms in waves every year starting from 2014. The staggered experiment of sanctions allows me to test the joint effect of the negative input shock and the government response. I run the following regression:

$$Y_{it} = \gamma_{jt} + \phi_i + \theta_{st} + \beta_1 * Sanctions_{it} + X_{it}\delta + u_{ijt} \tag{8}$$

[20]https://www.reuters.com/article/us-russia-renova-idUSKCN1IF2AG

[21]https://www.bloomberg.com/opinion/articles/2018-05-08/russia-sanctions-have-had-some-unexpected-consequences+cd=1hl=enct=clnkgl=ruclient=safari

[22]https://www.reuters.com/article/orubs-russia-sanctions-idRUKBN1HP2BV-ORUBS

[23]Max Seddon, "Moscow Creates Bank To Help It Avoid US Sanctions," Financial Times, January 19, 2018, https://www.ft.com/content/90c73fe4-fd15-11e7-9b32-d7d59aace167

[24]IMF, Russian Federation: Staff Report for the 2015 Article IV Consultation, August 2015, pp. 7. https://www.imf.org/external/pubs/ft/scr/2015/cr15211.pdf

[25]Moscow Times, "Sanctioned Bank Rossiya Becomes First Major Russian Bank to Expand in Crimea," April 15, 2017; Jack Stubbs and Yeganeh Torbati, "U.S. Imposes Sanctions on 'Putin's Bridge' to Crimea," Reuters, September 1, 2016; Thomas Hale and Max Seddon, "Russia to Tap Global Debt Markets for a Further $1.25 Billion," Financial Times, September 22, 2016. See the Congressional Research Service (2020), pp 53 for a more extensive list of measures by the Russian Government. https://fas.org/sgp/crs/row/R45415.pdf

I use the annual measures of $ln(MRPK_{it})$, $ln(ValueAdded_{it})$, $ln(Revenue_{it})$ or $ln(K_{it})$ for Y_{it} and regress these variables on firm-level time-variant sanctions dummy, $Sanctions_{it}$. To control for firm-level heterogeneity I include firm FE ϕ_i. Further, I add a 4-digit industry-year FE γ_{jt} to remove common industry changes over time, including the oil price shocks that were large in the period 2014-2016 and could have differentially affected some industries, which also have more sanctioned firms. Moreover, I include a size-by-year fixed effects θ_{st} to difference out how larger firms react to economic downturn (taking place after 2014) as opposed to smaller firms. The size s is defined by the pre-treatment quartile of average firm capital[26]. I cluster the errors by firm and 4-digit industry-by-year to account for possible serial correlation at firm level or across firms within an industry at a given point in time.

If β_1 is negative and significant and Y_{it} is ln(MRPK) in specification 8, this is the evidence that sanctioned firms, which already had "too much capital" received relatively more capital as a result of sanctions. This result can appear not just because the capital inputs grew, but also because the input-sanctioned firms increased their inputs more *relative* to their revenue. But what if revenue dropped for these firms, due to suspension of trade by foreign customers? If I further find that ln(MRPK) increased because both the revenue and inputs increased, but the inputs grew more (for instance, by β_1 being non-negative when Y_{it} is $ln(Revenue_{it})$ and by β_1 being positive and significant when Y_{it} is $ln(K_{it})$), this will be the evidence of shielding of sanctioned firms that overshot the direct (negative) effect of sanctions on inputs.

This experiment also helps me see whether the SOEs have responded differently to this negative input shock as opposed to private firms. To consider the differential effect for state-owned enterprises, I run the following regression:

$$Y_{it} = \gamma_{jt} + \phi_i + \theta_{st} + \beta_1 * Sanctions_{it} + \beta_2 * Sanctions_{it} * SOE_i + X_{it}\delta + u_{ijt} \quad (9)$$

In Specification 9, I repeat the specification 8 but add an interaction term $Sanctions_{it} * SOE_i$ to check if there is a differential effect with respect to the state owned firms.

Identification. Below, I discuss the extent to which my estimation is prone to two possible sources of bias: (1) non-random assignment of sanctions across firms, (2)

[26]I provide robustness of the results to alternative fixed effects in the Appendix Table 2.A9 and Figure 2.A4

measurement error in sanctions and $MRPK_i$, and (3) reverse causation of sanctions and government support.

One worry is that sanctioned firms have different characteristics relative to non-sanctioned firms. As shown in Table 3, the sanctioned firms have higher revenues, capital, employ more people and are on average four years older than the non-sanctioned firms and there may also be unobserved differences between these firms. However, so long as these observed or unobserved differences are time-invariant, these differences are fully accounted for by firm fixed effects. The firm fixed effects also account for any differences between SOEs and private firms. Therefore, this empirical strategy does not require that the sanctions were randomly assigned.

Of course, sanctions were not the only aggregate shock that happened in 2014 which included, for example, a devaluation of the ruble and and increase in the interest rate by the central bank. Sanctioned vs non/not-yet-sanctioned firms may have differential sensitivity to these country-wide shocks, where non-sanctioned firms respond more negatively. If sensitivity varies by firm size, controlling for firm's pre-treatment size-by-year fixed effects in all baseline specifications should keep the sensitivity fixed. If sensitivity to aggregate shocks varies by other characteristics, such as strategic status of firms, testing whether main findings hold when alternative control groups are used, would address this concern. Finally, if the totality of the effect was driven by the differential responsiveness to 2014 shock for sanctioned and non- or not-ye-sanctioned firms, one should not see an effect to sanctions for firms sanctioned in 2016 and later. Testing effects of sanctions by wave should bring support either toward sanctions being the largest driver of the effects, or the differential sensitivity being the driver of the effect.[27]

Another concern is that the sanctions were over-represented in some industries, such as the Oil and Gas, and this industry also differentially experienced a negative oil price shock in the same period. So long as these price shocks affected all firms within a narrow 4-digit industry similarly, my industry-by-year fixed effect fully controls for these time-variant industry shocks.

[27] A related concern is that allocation of resources is driven by Russia preparing for the war and not sanctions per se. In later sections I explore this alternative hypothesis by separating the effects for military and related sectors from other sectors. I the effect of sanctions is strong and positive for non-military sector alone, this is consistent with the fact that sanctions had indeed influenced the allocation of resources.

Therefore, this set-up does not require that the industries that had more sanctioned firms evolve in parallel over time, and it does not require that the sanctioned and non-sanctioned firms share the same time-invariant characteristics. The estimation of β_1 in Specifications 8 and 9 does rely on the classic assumption that the sanctioned firms evolve in parallel to the non-sanctioned firms at the time of sanctioning. I provide visual evidence that the pre-trends evolved in parallel in the next section.

To the extent that there is measurement error in sanctions treatment, it would bias against me finding an effect, so the effects I would find are a lower bound. A non-systematic measurement error would create an attenuation bias towards zero. A systematic under-counting sanctioned firms would place some truly sanctioned firms into the control group, so would again make the gaps between treatment and control groups smaller. Measurement error in $MRPK_i$, the outcome variable, is also not a great concern in the estimations I present. First, the non-systematic measurement error on the outcome variable $MRPK_i$ does not bias the coefficients that I find. If the measurement error is systematic, but fixed at firm-level, or is time-variant, but common for all firms in a 4-digit industry, it will be absorbed by the industry-by-year fixed effects and firm fixed effects.

The DID set-up and the yearly data I have make it somewhat tricky to conceptually distinguish the order of causation. Do sanctions lead to the government support, or, the government support induces the firms to be sanctioned? First, this concern is only warranted when the two actions coincide exactly - the pre-trends are suggestive of no gains to a sanctioned firm a year or more before it is sanctioned. Second, the timing of sanctions themselves is driven by geopolitical events, such as: the annexation of Crimea, Russia's military and political actions, such as referendums in the Donbass region, MH17 airplane crash, or the poisoning of Sergei Skripal. The timing of these events is not directly related to the Russian economy. Additionally, there is a lag in publication and procession of firm information so it is unlikely that the US officials were able to use real-time information on firm loans and deals in order to sanctions these firms. Therefore, together with all the anecdotal evidence that the government policy was reactive to sanctions (rather than pro-active), it is extremely unlikely that the support increases to certain firms in a particular year would bring on the sanctions

to them that year.

4.1 Event studies

As mentioned above, to identify β_1 in Specifications 8 and 9 I rest on the assumption that the sanctioned firms would have been on the same trends as the non-sanctioned firms at the time of sanctioning. To partially alleviate this concern, I include event studies that 1) test for sanction effect within sanctioned firms (Specification 10) and identifying the treatment effect off timing 2) test for the differential trends between sanctioned and non-sanctioned firms before 2014, the first year of sanctions taking an effect (11)[28].

$$Y_{it} = \gamma_{jt} + \phi_i + \theta_{st} + \alpha_s * \sum_{s=-4, s\neq -1}^{s=3} Sanctions_i * 1_{t=s} + X_{it}\delta + u_{ijt} \tag{10}$$

Specification 10 is identical to the regression 8, except that the average treatment on the treated effect is split into seven year-to-sanction effects. Each α_s identifies each year-to-sanction effects relative to the average outcome in the first year of sanctions. Only the variation *within* the sanctioned firms is used to identify α_s, however, the non-sanctioned firms can still be used to identify the γ_{jt} and θ_{st}.

$$Y_{it} = \gamma_{jt} + \phi_i + \theta_{st} + \alpha_s * \sum_{s=2012, s\neq 2014}^{s=2020} Sanctions_i * 1_{t=s} + X_{it}\delta + u_{ijt} \tag{11}$$

Specification 11 is aimed to test whether the sanctioned and non-sanctioned firms were trending in the same way prior to sanctions. Here, unlike in the previous specification, the full sample is used to identify the coefficients α_s, which show the difference in outcomes of the sanctioned firms in each year versus in 2014, compared to such difference in outcomes of the non-sanctioned firms.

[28] Even though officially sanctions began in 2014, because of the two month cool-down period, only a small number of firms are effectively treated in 2014

5 Results

5.1 Regression results

Table 4 shows my baseline results of any sanctions on firm's inputs. Both SSI (input) sanctions and SDN (input and output) sanctions affected input sourcing of sanctioned firms in the West. The sanctions however have led to an expansion of input volume across the key balance sheet variables that represent labor, material and capital inputs. The sanctioned firms did not shrink their input volume as a response to sanctions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln Assets	Ln Assets	Ln Book Value of Capital	Ln Book Value of Capital	Ln Materials	Ln Materials	Ln payment to labor	Ln payment to labor
Any Sanction	0.302	0.290	0.316	0.274	0.225	0.235	0.115	0.113
	(0.049)	(0.054)	(0.057)	(0.063)	(0.053)	(0.060)	(0.047)	(0.052)
Any Sanction × SOE		0.075		0.250		-0.063		0.015
		(0.109)		(0.121)		(0.104)		(0.091)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firms	641082	641082	678994	678994	180433	180433	175682	175682
Sanctioned firms	932	932	928	928	922	922	921	921
Industries	860	860	862	862	764	764	765	765
Observations	3313542	3313542	3852607	3852607	865618	865618	846356	846356
R-squared	.949	.949	.89	.89	.868	.868	.9	.9
Clustering: Firm and industry-by-year								

Notes: All dependent variables are in logs. Firms are classified as SOEs according to Rosstat (reported in SPARK). Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 4: Average effects of sanctions: Inputs

In columns (1) and (2) we see an average increase in 30% in assets and 32% in capital of sanctioned firms after sanctions are imposed. In columns (5)-(8) we see a similar pattern in payments to materials and to labor[29]. Then, column (4), shows the heterogeneity of this effect for sanctioned SOEs, which see their capital rising even more relative to sanctioned private firms. If the increase in the value of inputs is driven by the increase in the volume of inputs through substitution from domestic and non-western sources, this points at all sanctioned firms have been protected from sanctions and have seen full shielding of their assets and capital and all had more shielding than the effects of sanctions itself.

[29]The sample of firms reporting materials and payments to labor variable is smaller, because firms under 199 mln rub (2 mln USD in revenue) and 100 workers are exempt from mandatory reporting of these variables. The value added variable, which is calculated as revenue minus materials for each firm, is also calculated on a smaller sample of 150,236 firms.

One could expect that sanctions, some of which banned sales abroad would lead to a reduction in total sales. Table 5.1 gives us a different answer: the sales and value added increased, along with profits. Unlike the differential increase in capital inputs, there is no differential increase in revenue for the SOEs. Interestingly, the gross profits, as recorded in the balance sheets have grown as well for sanctioned firms after being sanctioned, despite the cost of inputs increasing for all sanctioned firms and trade cost for outputs worsening for some firms.

	(1) Ln Value Added	(2) Ln Value Added	(3) Ln Revenue	(4) Ln Revenue	(5) Ln Profit	(6) Ln Profit
Any Sanction	0.089	0.095	0.203	0.204	0.115	0.095
	(0.040)	(0.045)	(0.054)	(0.058)	(0.051)	(0.055)
Any Sanction × SOE		-0.038		-0.005		0.119
		(0.098)		(0.127)		(0.122)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓
Firms	150236	150236	635156	635156	570951	570951
Sanctioned firms	828	828	916	916	865	865
Industries	745	745	842	842	835	835
Observations	675118	675118	3577822	3577822	2789693	2789693
R-squared	.872	.872	.858	.858	.837	.837
Clustering: Firm and industry-by-year						

Notes: All dependent variables are in logs. Firms are classified as SOEs according to Rosstat (reported in SPARK). Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

What is the reported increase in sales and inputs mean for changes in (mis-) allocation for sanctioned firms? Table 7 shows my baseline results for specifications 8 and 9 on misallocation. The first thing to note is in columns (1) and (2) we see that the MRPK went down on average for sanctioned firms and this effect is mainly driven by MRPK going down differentially for the sanctioned SOEs relative to sanctioned private firms. This tells us two things 1) The negative input shock did not correct the implicit subsidies pre-sanction of politically connected private firms (reported in Figure 2); 2) The negative input shock has been reversed and SOEs appear as if they had experienced a positive input shock and even stronger implicit subsidies.

Does this negative MRPK result come from the capital input increase (denominator) or the output reduction (numerator)? From Tables 5.1 and 4 we know that

both the capital inputs and revenues have increased, just the capital inputs increased more than the revenues. The complete shielding of capital inputs would have kept misallocation at the same level as pre-sanctions, but the excessive shielding has, in fact, worsened it.

	(1) Ln MRPK	(2) Ln MRPK	(3) Ln MRPL	(4) Ln MRPL	(5) Ln MRPM	(6) Ln MRPM
Any Sanction	-0.081	-0.043	0.061	0.065	-0.026	-0.036
	(0.055)	(0.063)	(0.045)	(0.049)	(0.041)	(0.043)
Any Sanction × SOE		-0.229		-0.023		0.054
		(0.106)		(0.119)		(0.114)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓
Firms	628068	628068	170779	170779	173779	173779
Sanctioned firms	910	910	903	903	902	902
Industries	842	842	761	761	759	759
Observations	3502744	3502744	815750	815750	826218	826218
R-squared	.842	.842	.809	.809	.619	.619
Clustering: Firm and industry-by-year						

Notes: All dependent variables are in logs. Firms are classified as SOEs according to Rosstat (reported in SPARK). MRPK is calculated as Revenue/K, MRPL as Revenue/wage bill, MRPM as Revenue/Materials. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanction firms gives the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 5: Average effects of sanctions: Misallocation

Using the anecdotal evidence that the funds were taken from the Russian budget, one can conclude that the connected SOEs and private firms were saved at the expense of all other firms and Russian taxpayers. This also has implications for the goals that sanctioning countries hoped to achieve: the sanctions were meant to be targeted and narrow. However, the shielding that took place in response has made the effects being borne by everyone *but* the original targets.

The results in Tables 4 and 5.1 differ from early firm-level sanctions results of Ahn & Ludema (2020), who find a negative result on revenue and assets. This is because they estimate a combined effect of sanctions on all assets, including companies owned by Russian sanctioned individuals abroad. Some of the companies registered abroad had to indeed seize operation and eventually close, which is likely driving the negative result.

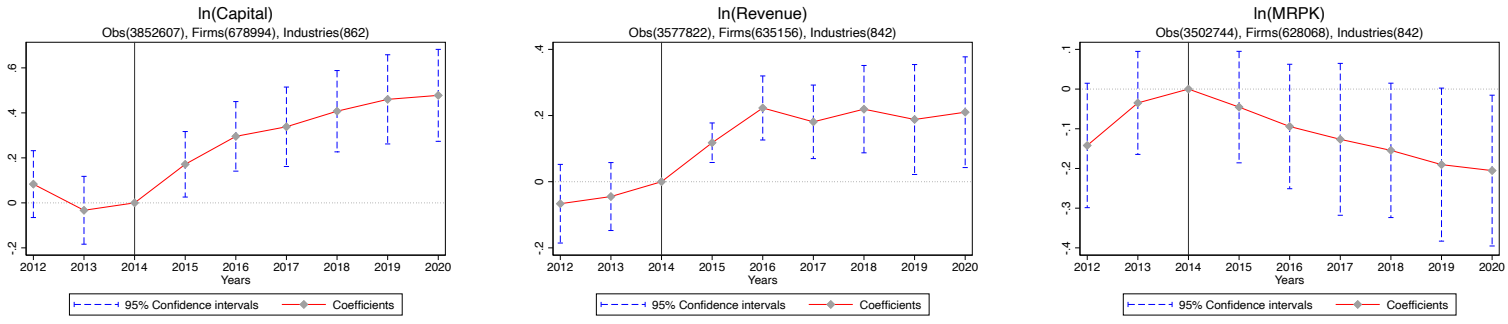
Finally, the results above could be consistent with the idea that sanctioned firms have fully passed through the sanction shock to the consumer: input value is price multiplied by quantity, and the increase in inputs could mean that either quantity or the price of inputs increased. While disentangling these is not possible without

data on quantities and customs data, if it was simply the higher price of inputs that is passed through to the consumer at a higher price, it is not clear why the profits of sanctioned firms also rose. The fact that profits increased along with revenues demonstrate that at least some shielding was taking place.

5.2 Event studies results

The identification in Tables 4-7 is subject to one obvious problem. What if the sanctioned firms would have seen their MRPK reduced and inputs and outputs increased regardless of sanctions and shielding. Sanction variable could be just picking up such trends.

In Figure 3, I show three event studies with ln(Capital), ln(Revenue) and ln(MRPK) as outcome variables in which the control group is the never-sanctioned firms. I cannot reject that the group of sanctioned firms was on the same trends as the group of non-sanctioned firms before 2014, the first year of sanctioning. If anything, the treated firms' MRPK were on the upward trend before the sanctions, so the regression results in Table 7 are a lower bound. In this case, the control group is the average time trend in the 4-digit industry and the treatment is the average outcome of the sanctioned firm in each year-to-sanction. The sample used to identify the coefficients in the event study is the full sample of firms, sanctioned or not.

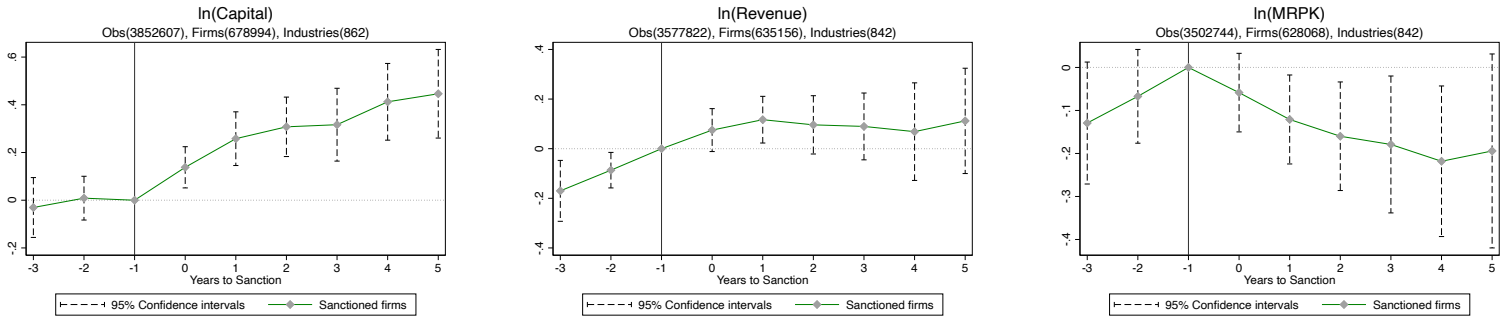


Notes: This figure reports event study graphs for the average effects of the sanctions on sanctioned firms relative to non-sanctioned firms. Each dot is the coefficient on the interaction between being observed in the year 2012, 2013, 2014, 2015, 2016, 2017, 2018, 2019 and 2020 and being sanctioned with SSI or SDN sanctions. The same control variables are used as in baseline regression: firm fixed effects, 4-digit industry-year fixed effects and firm size fixed effects. Effectively, each dot is the deviation of the sanctioned firm log firm-level variable from the 4-digit-industry-by-year fixed effects and firm-size-by-year fixed effects. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 3: Pre-post 2014 event study with never-sanctioned firms in the control group.

In Figure 4, I show an event study within the group of sanctioned firms and confirm that the positive effects persist even when the required assumption is weaker:

the firms that are sanctioned sooner are not on a different trend compared to the firms that are sanctioned later. In this case, the control group is the average outcome of the sanctioned firm in the year 0, the year it was sanctioned, and the treatment is each year-to-sanction. The coefficients in Figure 4 come from a specification, where I control for the industry-year fixed effects, pre-treatment size-by-year fixed effects and firm fixed effects. The full sample is used in this event study in order to identify the fixed effects only. Figure 4 shows that there is a trend break at the time of sanctioning for capital, material and MRPK. There seems to be some pre-existing increases in trends for the revenue result, which could be an anticipation effect or the true differential pre-trends. In any case, even if we imagine that the "true" sanctions+shielding effect on revenues is 0, this will not fundamentally alter the increase in capital and misallocation.



Notes: This figure reports event study graphs for the average effects of the sanctions on sanctioned firms. The effect is identified within sanctioned firms: sanctioned firms are compared to not-yet sanctioned firms. The first year of firm sanction is normalized to take place in year 0. Each dot is the coefficient on the indicator of being observed t years after the sanctions announcement. The same control variables are used as in baseline regression: firm fixed effects, 4-digit industry-year fixed effects and the firm-size-by-year fixed effects. The full sample, including the non sanctioned firms are used to identify the 4-digit industry-year fixed effects and the size-year fixed effects. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 4: Event study with not-yet sanctioned firms in the control group.

Event studies within treatment may be biased if there is treatment effect heterogeneity and dynamic effects, since this specification uses the already treated observations as controls for the not-yet -treated observations. However, estimating the effects using the De Chaisemartin & d'Haultfoeuille (2020) estimator in Figure 2.A2 shows that the effects are qualitatively the same and quantitatively close to the effects estimated in the Figures 3 and 4.

Attrition from the sample may be another concern, but these results generally hold on a constant sample of firms as shown in Appendix 2.A3.

5.3 Channels: Contracts, Subsidies, and Loans

What are the main drivers of the dramatic increases in revenue and book value of capital? Consistent with the anecdotal evidence on government support in the Data section there could be three important drivers: government subsidies, government contracts, and loans. The data on subsidies is hand-collected from "Spending.gov" website and is supplemented with the additional subsidies from SPARK database for the firms that report subsidies in "Spending.gov" at least once in the period.[30] The data on subsidies may not be complete as not all subsidies are public, so I treat all results to do with subsidies as indicative. The data on all procurement contracts of any firm to the government or a state-owned enterprise has been collected through SPARK database.

	(1) Got a Subsidy	(2) Ln Subsidy Volume	(3) Become a Supplier	(4) Ln Contract Volume
Any Sanction	0.010	1.130	0.047	0.311
	(0.003)	(0.657)	(0.013)	(0.122)
Firm FE	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓
Firms	686641	169	686641	140152
Sanctioned firms	935	12	878	591
Industries	862	26	862	704
Observations	3944233	620	3944233	605914
R-squared	.446	.701	.504	.763
Clustering: Firm and industry-by-year				

Notes: Columns (1) and (3) use the sample of firms that have non-zero values of the variable reported and identify the intensive margin. Columns (2) and (4) use the full sample of firms and identify the extensive margin: switching from no subsidies (or contracts) to some subsidies (or contracts). Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 6: Average effects of sanctions: subsidies and contracts

Table 6, columns (1) and (2) show that subsidy volume increased even for the limited subsidy data available for the firms that were receivers of subsidies (the effect is borderline significant), and using the full sample, sanctioned firms were more likely to get a subsidy. Similarly, the volume of contracts has increased for those firms who were getting contracts, and sanctioned firms were more likely to become a supplier to the government (or a state-owned firm).

[30] The website https://spending.gov.ru/subsidies/ has been closed in the past year, but can be accessed through the "Wayback Machine"

In addition, the firm credit, including credit via the specially-dedicated banks, was one of the policies that the Russian government has extended to the sanctioned firms to alleviate the burden of being sanctioned. In Table 7, I look at a series of balance sheet variables that can proxy for the changes in loaned money flowing into the company.

Columns (1) and (2) show the effect of sanctions on the stock of long-term and short-term loans respectively. I see a 9% increase in short-term loans (the effect is borderline significant). Column (3) looks at the "investment" variable, which is reported for a limited number of firms, and investment also increases, albeit insignificantly. Finally, columns (4) and (5) are taken form the cash-flow statement and they demonstrate the significant increases in the flows of cash for both getting more credit and paying for more credit.

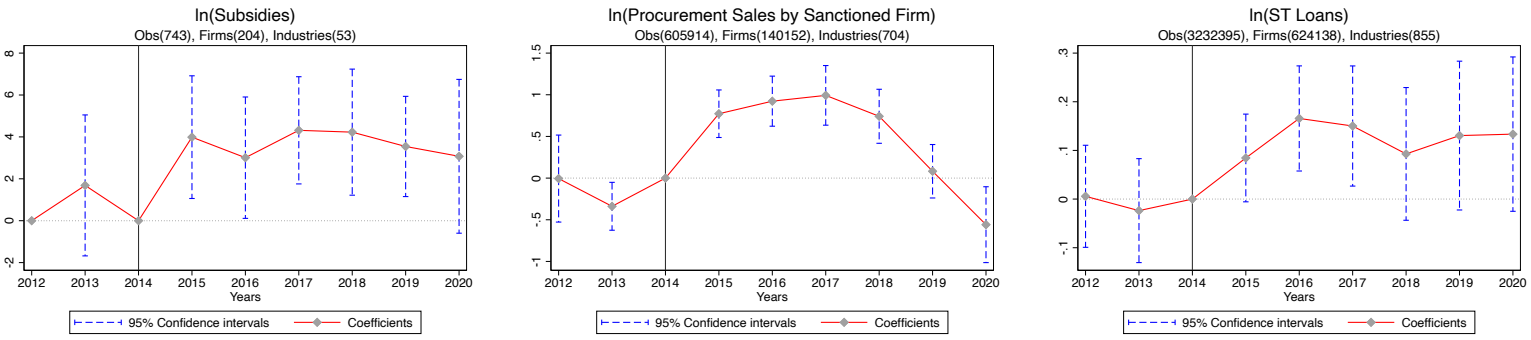
	(1) ln LT Loans	(2) ln ST Loans	(3) ln Investment	(4) ln Credit cash-in	(5) ln Credit cash-out
Any Sanction	0.146	0.090	0.103	0.282	0.367
	(0.086)	(0.046)	(0.068)	(0.092)	(0.092)
Firm FE	✓	✓	✓	✓	✓
4-digit industry-yr FE	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓
Firms	243404	624138	51826	86786	80712
Sanctioned firms	858	932	741	610	604
Industries	793	855	652	685	666
Observations	1129383	3232395	250688	358052	338991
R-squared	.846	.89	.764	.805	.806
Clustering: Firm and industry-by-year					

Notes: All dependent variables are in logs. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 7: Average effects of sanctions: proxies for loans

The increase in cash movements for repayment of credit is noteworthy, as it is consistent with the idea that sanctioned firms are deleveraging from their previous creditors, such as the Western banks, and replacing their credit with domestic credit.[31] While the terms of the domestic loans that replaced the Western loans might not have been as attractive, the deleveraging has likely brought further resilience to sanctioned targets. Figure 5 repeats the analysis in the form of event study for subsidies, procurement contracts and short-term loans for sanctioned versus non-sanctioned firms

[31]This is also consistent with Russian aggregate external debt declining from 41% of GDP in 2016 to an around 27% in 2021 according to the Central Bank of Russia: https://group.atradius.com/publications/economic-research/russia-fortress-strategy-is-not-for-free.html



Notes: This figure reports event study graphs for the average effects of the sanctions on sanctioned firms relative to non-sanctioned firms. Each dot is the coefficient on the interaction between being observed in the year 2012, 2013, 2014, 2015, 2016, 2017, 2018, 2019 and 2020 and being sanctioned with SSI or SDN sanctions. For all event studies but the first one on subsidies, the same control variables are used as in baseline regression: firm fixed effects, 4-digit industry-year fixed effects and firm-size-by-year fixed effects. For the event study on subsidies, the same controls are used, except a 2-digit industry-by-year FE replaces a 4-digit-industry-by-year FE due to a small sample size. Effectively, each dot is the deviation of the sanctioned firm log firm-level variable from the 4-digit-industry-by-year (or 2-digit-industry-by-year) fixed effects and firm size-quartile-by-year fixed effects. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 5: Pre-post 2014 event study of procurement sales, subsidies, and loans with never-sanctioned firms in the control group.

relative to 2014, the first year of sanctions. There is a significant increase all three variables for sanctioned firms after 2014 relative to never-sanctioned firms up until 2020 and no visible pre-trend beofre 2014. In 2020 a decline in reported government contracts for the treated group could be to do with Covid and a large increase of government procurement of medical supplies from the "control" firms.

Figure 2.A1 in the appendix repeats the event studies for a set of additional outcomes: the long-term loans, investment and repayment of credit as well as materials all increase differentially for the sanctioned firms after 2014.

5.4 Heterogeneity and Spillovers

My data allows me to explore important dimensions of heterogeneity of sanctioned firms in their reaction to being sanctioned. Table 2.A3 looks at heterogeneity of the firms' response along several important dimensions: being directly sanctioned versus via majority-ownership, being sanctioned by SSI versus SDN sanctions, effects on being minority-owned by a sanctioned firm, being part of the energy sector, and being exporter. I find no differential (positive) response of being listed directly in the US sanctions documents or via the "OFAC rule of 50%". All firms appear to be shielded in the same way. Next, in columns (3) and (4) I look at the heterogeneity of sanctioned firms by the type of sanction, and again, there is no difference in the

positive response whether a firm is sanctioned by the input sanction (SSI) or by a fully blocking sanction (SDN).

Columns (5) and (6) of Table 2.A3 demonstrate that the minority-owned firms see a significantly dampened but still positive effect, relative to those directly sanctioned or those that are majority-owned by directly sanctioned firms. Columns (7) and (8) show that the sanctioned firms in the energy sector (defined by producing in fossil fuel, oil refinement, electricity and related services) did not see a differential effect (after controlling for the 4-digit industry-by-year FE), so despite the strong oil price drop in the same period, the sanctioned energy firms have not experienced worse outcomes. The last two columns look at the heterogeneity by sanctioned exporters. Revenues increase more for sanctioned exporters relative to sanctioned non-exporters, even though the sanctioned treatment for some of the firms was to block all their economic transactions with other countries. This points out that sanctioned firms found other ways to export, potentially re-orienting to other countries.

It is important to separate the impact of sanctions from from the requirement of the government to provide financial support to these companies in the new era of confrontation, irrespective of sanctions. My data allows me to explore important dimensions of heterogeneity of sanctioned firms in their reaction to being sanctioned and test if the effect is driven by strategic firms and military firms alone. Has the government specifically targeted firms related to the military sector or firms with a special "strategic" status? Table 2.A4 looks at heterogeneity along the firm status related to strategic goals and shows the evidence that this was the case. Columns (1)-(4) look at the differential effects of companies involved in the defense sector, which I define in two ways. The variable "military" indicates that a company was listed in the defense industry magazine "Defence-Media" as a leading defence sector company.[32]. Variable "military supplier" indicates that a company was a supplier in a contract with "Gosoboronzakaz" (defense sector orders) in its title or text in the sample period. The latter group could include companies that do not operate solely in the defence sector (for example, provide printer cartridges or food). While there is a positive differential effect on capital for the sanctioned leading military players, and a very strong positive differential effects on revenues and capital for the firms

[32]The URL for the company list is https://dfnc.ru/predpriyatiya-vpk/ visited on 28/2/2023

involved in the defence sector supplies, the main positive response of revenues for non-military and non-strategic firms holds. The effect is not solely driven by Russia's preparation for the war and investing into military firms regardless of sanctions.

Columns (5) - (8) of Table 2.A4 show how revenue and capital of strategic enterprises changed. There are two group I look at: firms in a "System-forming enterprises" group, and those in any of the "Strategic firms" lists[33]. Again, there is a large positive differential for these firms. The "strategic" status overlaps with state-owned status. The last two columns - columns (9) and (10) - aim to look in parallel at the interaction with SOEs and the cross-interaction of SOE and Strategic status. Without having enough power to identify all the effects, it appears that the Strategic status is more important in the revenue response to sanctions relative to the ownership status.

Finally, to identify spillovers, I use data on the universe of government procurement contracts, to understand if there are any spillovers on sellers if buyers are sanctioned. Since a 16% share of sanctioned firms is state-owned, they will be normally participating in such contracts to source goods. If a firm is sanctioned (and the government responded endogenously), it may respectively change the demand of this firm towards its suppliers. Appendix Table 2.A5 shows the results. The unit of observation is firm pair-by-year. It seems that sanctioned firms do not increase the contract volume with their suppliers after sanctions. While these firms are less likely to become buyers and hold fewer number of contracts after sanctions, these reductions do not change the volume of contracts. I conclude that spillovers through procurement are limited.

5.5 Robustness

In this section I explore whether my results are robust to alternative sample and differential attrition. First, the results on revenue and capital could be driven by sanctioned firms that have low revenue and capital more likely to exit or stop reporting after sanctioning than the untreated firms. If this is the case, the increase in capital could be picked up because low-capital sanctioned firms are dropping out, whereas the high-capital sanctioned firms remain. To test this, I create a balanced sample of

[33]The lists are: "System-forming enterprises", "List of strategic organizations by Government Decree 1226-r", "List of JSCs by Government Decree 1870-r ("golden share")", "List of JSCs by Government Decree No. 91-R", List of Federal State Unitary Enterprises, of significant importance.

firms with a dummy variable indicating the existence of the firm. I proxy exit in two ways: by the "active" status in the firm registry, and by the fact that a firm reports its balance sheet variables. In Table 2.A6, in columns (3) and (4), I find that sanctioned firms are just as likely to remain active and are *more* likely to report balance sheet variables after being sanctioned. In columns (1) and (2), I find that sanctioned firms are less likely to exit and stop reporting after sanctions, as measured by the share of exits in each sanction-status-industry group.

Furthermore, I look at the event studies using only the firms that were active in all the 9 years and again confirm the baseline increases in capital and revenue and fall in MRPK in Figure 2.A3.

The second concern is that the results are driven only by the first wave of sanctions, when the overall uncertainty around the annexation of Crimea has hurt all Russian firms, but the sanctioned firms withstood the crisis better due to their proximity to the government. In Table 2.A7 I look at two waves of sanctions separately: the first wave in 2014, and the second large wave in 2016 (and exclude the prior-sanctioned firms from that regression). I find similar effects of later-sanctioned firms as those sanctioned in the first wave.

I also show that my results are robust to different control groups (other than a set of "non-sanctioned" firms, or "not-yet-sanctioned" firms). One natural control group for the treated firms is their minority-owned subsidiaries. They do not have a "sanctioned status" and, unlike the majority-owned subsidiaries, they are allowed to continue doing business in the West. However, they may still be politically connected and gain support from the government just the same. To estimate the effects, where I first subtract the sector-year fixed effects estimated using the whole sample and run the regression only on the sample of three groups: directly sanctioned firms, indirectly sanctioned firms by the OFAC rule of 50%, and those minority-owned firms acting as a control group. I repeat this with the following alternative control groups: SOEs, strategic firms and exporters. Table 2.A8 shows that even with alternative control groups, the results remain positive and similar in magnitude to those in the baseline specification.

Finally, an important robustness check is to see how results change if I reduce the fixed effects, since my econometric model is (conservatively) saturated with many

different fixed effects. Table 2.A9 shows that the results are actually very similar even if only the firm fixed effects are used, but none of the time-related fixed effects. I also show the same result as an events study relative to 2014 in Figures 2.A4 and 2.A5. In the first figure, I plot the effects of sanctions relative to 2014 as the first year of sanctions, only controlling for firm fixed effect and year fixed effect, and no longer using the industry time fixed effects or size-by-year controls. In the second figure, I separately plot the "raw changes" for the sanctioned group and the non-sanctioned group, without controlling for any year effects. I only de-mean the graphs by firm fixed effects (so that they are visually comparable to the control group). The sharp gains in capital and in materials in the treated group after 2014 are stark even in these graphs with minimal controls.

5.6 Aggregate effects

I use three frameworks to quantify aggregate effects of sanctions on the Russian economy. My baseline framework relies on Hsieh & Klenow (2009) model and Equation 12 in Section 2 to calculate the effects on aggregate sector TFP from the change in $MRPK_i$. I use both the full framework and the "simplified expression" $\Delta logTFP_s = -\frac{1}{2\eta} * VAR(logTFPR_i + \alpha \Delta logMRPK_i)$ to calculate the aggregate TFP change.[34]

The simplified expression requires that the firm TFP_i and wedges are jointly log-normally distributed, while the full model does not require this assumption. I first include the calculation of the change in TFP from the simplified expression because it gives the most intuitive explanation why TFP has declined. The industry TFP will decline if the variance of $TFPR_i$ grows. The resources were reallocated towards firms that already had too many, and the TFPR declined for firms that already had a low TFPR, so the variance of TFPR grew.

To make the actual calculation, I take value of $\Delta logMRPK_i$ from Figure 4 as the drop between 2014 and 2020 (it is approximately 20% for the sanctioned firms) and assign the change to only those firms i that are sanctioned in 2014-2020[35]. The value

[34] $TFPR_i$ is a function of MRPK and is defined as $TFPR_i \equiv \frac{p_i F_s(K_i, L_i)}{K_i^{\alpha_s} L_i^{1-\alpha_s}} \propto MRPK_i^{\alpha_s} * MRPL_i^{1-\alpha_s}$

[35] The differential exit of more under-resources firms is not taken into account, which estimates a lower bound of the misallocation effect, because the more under-resourced firms exited as shown in table 2.A6

of $logTFPR_i$, the log revenue productivity of each firm, and also a summary measure of distortions to these firms, is obtained as a pre-2015 level using the methodology in Appendix B. Whereby the $logTFPR_i$ is the residual from regressing $logTFPR_{it}$ on year and firm fixed effects (and then removing the common 4-digit industry component) for the pre-sanction period years 2012, 2013 and 2014. I conservatively assume that the labour productivity $MRPL_i$ stays the same as the pre-sanction level (this is also consistent with my findings in the Results section).

A crucial assumption in measuring wedges directly in the data is that this method relies on an assumption that production function for each firm did not change in the 2014-2020 period - for example, the increase in capital input is not a signed of the tageted firm becoming more capital intensive. Given that a period of 5 or fewer years is short enough to deplete the stocks of western technology and machinery, it is unlikely that many sanctioned companies were able to re-structure their production processes and have become differentially more capital intensive relative to the non-sanctioned firms in the same 4-digit industry.

Then, I use the full Hsieh and Klenow or "horizontal economy" model to calculate the change in aggregate TFP, i.e I plug in the changes of MRPK to Equation 12. In this counterfactual, I again use the 20% change in MRPK from the event study on MRPK in Figure 4. In the counterfactual, I first create two groups for each industry, the sanctioned group and the non-sanctioned group by distributing the existing inputs so that all wedges between firms within each group are equalized[36]. After this change, the only misallocation is between sanctioned group and non-sanctioned. I then allocate capital to the sanctioned group so that its MRPK declines by 20%. The remaining capital input of the industry is given to the non-sanctioned group. I then calculate the new aggregate TFP (for each industry and then overall) and compare it to the TFP before the counterfactual. The decline in TFP is small but negative and is approximately -0.65%.

Hsieh and Klenow framework. I assume in each industry I, a monopolistically competitive firm produces a different variety i demanded with a CES demand. Each firm produces with Cobb-Douglas value added production function: $F_i = A_i K_i^{\alpha} L_i^{1-\alpha}$.

How do the wedges affect the aggregate industry TFP? The industry TFP (TFP_s)

[36]See the derivation of the formula for the group sanctioned and non-sanctioned group MRPK in Appendix C, section 11.2

can be expressed as the following equation in the equilibrium:

$$TFP_I = \left(\sum_i \left(A_i \left(\frac{\overline{MRPL}}{MRPL_i} \right)^{1-\alpha} \left(\frac{\overline{MRPK}}{MRPK_i} \right)^{\alpha} \right)^{\frac{1-\eta}{\eta}} \right)^{\frac{\eta}{1-\eta}} \tag{12}$$

The term $(1-\eta)$ is the constant markup that comes from the monopolistic competition assumption. The $MRPK_i$ and $MRPL_i$ are taken from the data as revenues over input costs.[37] $\overline{MRPK}$ and $\overline{MRPL}$ are industry-wide harmonic averages of $MRPK_i$ and $MRPL_i$.

The highest achievable TFP_I is a CES aggregate of firm-level productivities A_i. It is obtained under no wedges or a uniform wedge across firms. Whenever $MRPK_i$ and $MRPL_i$ deviate from their industry harmonic averages, the industry TFP becomes lower than the efficient level. Therefore, any changes, for example due to sanctions, in $MRPK_i$ and $MRPL_i$ will change the aggregate TFP_I according to equation 12.

Furthermore, the model allows me to define a model-based firm TFP (TFP_i). With the assumption of CES demand and monopolistic competition, the size or market share of a firm is related to its real productivity (A_i or $TFPQ_i$):

$$A_i = \kappa \frac{(P_i Q_i)^{\frac{1}{1-\eta}}}{K_i^{\alpha} L_i^{1-\alpha}} \equiv TFP_i \equiv TFPQ_i \tag{13}$$

$$\kappa = (PQ^{\eta})^{-\frac{1}{1-\eta}} \tag{14}$$

To get the country aggregate TFP, I follow Hsieh and Klenow and take a Cobb-Douglas average of each of the industry TFP_I, using the industry value added shares as exponents (θ_I): $Y = \prod_{I=1}^{S} (TFP_I K_I^{\alpha_I} L_I^{1-\alpha_I})^{\theta_I}$.

Four things are important to note here. First, only the relative tax in an industry will matter for misallocation. A tax that is equal across firms will lead to efficient allocation across firms within an industry, but not across industries. Second, and related, for this baseline calculation, I follow Hsieh and Klenow and take into account only misallocation within industries.[38] Third, I will plug the changes in wedges to

[37] In the calculation of the overall TFP and country TFP only the *relative* τ^K and τ^L will matter, rather than the absolute levels because each industry will be aggregated to the economy-wide output with a Cobb-Douglas production function.

[38] Misallocation across sectors, arising from higher or lower average wedges across firms is assumed

the full horizontal economy model to calculate the change in aggregate TFP, so I will not use the popular simplified expression from Hsieh and Klenow (2009) that maps the variance of wedges to the aggregate TFP. Therefore, I will not need to rely on the assumption that wedges and $TFPQ_i$ are jointly log-normally distributed. Fourth, even though I assume monopolistic competition and therefore constant markups, if other forms of competition are present in the data, the different mark-ups will be reflected in wedges, which is desirable in accounting for the overall distance to the efficient frontier.

Levinsohn and Petrin framework. The framework applies to any set-up involving a group of producers that each have their own productivity and face an arbitrary set of wedges. The output of the group depends on the wedges, productivities and external resources that enter the group. The allocation of resources across producers and changes in outputs and inputs for each producer is a general equilibrium outcome. The set of producers in my case is an industry of firms. This framework is more general than that of Hsieh and Klenow (2009), as it does not require the assumptions on returns to scale, the structure of the market or functional form of the aggregate GDP. To map the producers to total output the detailed knowledge of the Input-Output network of the economy inside the industry (or inside the economy) is also not required. However, the knowledge of the general equilibrium changes in inputs is required.

The change in the Solow residual, equivalent to the proportional change in aggregate TFP is defined as:

$$\Delta log(TFP_I) \equiv \Delta Solow_I = \Delta NetOutput_I - \Delta NetInput_I = d\log Y - \sum_{f \in F} \Lambda_f d\log L_f \tag{15}$$

Net output is the output net of the products re-used by the same industry. Net Input is the input net of the products, made by the same industry and used as input

away in Hsieh and Klenow, due to the assumptions of Cobb-Douglas demand across sectors, and that the average wedges are *not* rebated lump sum (i.e. resources are wasted due to average wedges). Therefore, the average wedge increases in an industry act as average industry productivity declines. This does not lead to any reallocation in a Cobb-Douglas economy: price increase and decline in demand exactly offset the decline in industry output due to higher industry wedge; allocation of inputs across industries is constant.

by it. In other words, the change in the Solow Residual is the change in what exits the set of producers to the "outside", relative to what enters the set from the "outside". $\log Y$ is the net output, L_f is an external input from the set of external inputs F and Λ_f is the sales share of the factor or a Domar weight of the factor.[39]

The explicit equation for the change in the Solow residual is:

$$\Delta log(TFP_I) = \sum_{i \in I} \lambda_i d \log A_i + \sum_{i \in I} \lambda_i (1 - \mu_i^{-1})(d \log y_i - d \log A_i) \tag{16}$$

In this expression, λ_i is the sales share in total industry (or in the total economy) output of firm i, μ_i is the firm output wedge (or an implicit tax on outputs), y_i is the firm's physical output and A_i is the firm's physical productivity.

As Baquaee and Farhi (2019) show, this first-order approximation only correctly separates allocative efficiency from productivity in inefficient economies if there are no changes to physical productivity $\sum_{i \in I} \lambda_i d \log A_i$. Indeed, in the appendix table 2.A14, I show that there are no changes to treated firms' physical productivity A_i from the sanctions policy. Consistent with this evidence, I drop the term $\sum_{i \in I} \lambda_i d \log A_i$ to 0 and interpret the remaining term as the change in allocative efficiency.

Akin to Bau & Matray (2020), I adapt the version of this expression to aggregate the changes of capital inputs between distorted producers. I do so by creating a fictitious producer that only uses capital and sells it with a markup to other firms in the set I:

$$\Delta log(TFP_I) \approx \sum_{i \in I,\, x \in K,L,M} \lambda_i \alpha_i^K \frac{\tau_i^K}{1 + \tau_i^K} \Delta \log K_i \tag{17}$$

where $x \in K, L, M$ is the set of inputs and I is the set of industries. In this framework, the aggregate TFP changes not due to wedges, but due to inputs used by each firm with high or low wedges.

Like in Hsieh and Klenow framework, I take a Cobb-Douglas average of changes in $log(TFP_I)$ to calculate the economy-wide TFP change.

Baquaee and Farhi framework. Finally, I use the framework to aggregate changes in wedges to the aggregate TFP by Baquaee and Farhi (2020). To my knowledge it is

[39] As the change in the total net inputs used by Russian economy due to sanctions is not separable from the the time trend in the empirical exercise, I assume that $\sum_{f \in F} \Lambda_f d \log L_f$ is 0. Therefore the "distorted Solow residual" of Baquaee and Farhi (2020) coincides with the classical Solow residual in this exercise, and they both coincide with aggregate GDP.

the first application of this framework to aggregate the causally estimated changes in wedges to the overall TFP. The framework assumes a very general nested CES structure of the economy, arbitrary I-O linkages and has the benefit of taking into account the propagation of misallocation through input-output linkages across sectors of the economy. In this framework, the distortions are adjusted for the position of the firms in their production network. The rationale to use this framework in my setting is to see whether there is amplification of the drop of industry-level output from the gaps between sanctioned and non-sanctioned firms, while explicitly accounting for I-O linkages.

The change in aggregate TFP to a small change in k's firm markups μ_k (and GDP, if factors stay constant) is captured by:

$$\frac{d\log TFP}{d\log \mu_k} = -\tilde{\lambda}_k - \sum_f \tilde{\Lambda}_f \frac{d\log \Lambda_f}{d\log \mu_k} \tag{18}$$

Where $\tilde{\lambda}_k$ is the "cost-based" Domar weight of firm k, that is a share of firm k in the economy's costs. It is defined by the equation $\tilde{\lambda}_k \equiv b'\tilde{\Psi} \equiv b'(I-\tilde{\Omega})^{-1}$, where $\tilde{\Omega}$ is a cost-based input-output matrix, whose cell in row i and column j, that is $\tilde{\Omega}_{ij}$, is the share of firm's j's sales to i in firm i's costs. Vector b is the vector of shares of expenditure on firm i's products in overall final expenditures.

$\tilde{\Lambda}_f$ is the "cost-based" Domar weight of the factor f, that is a share of factor f (capital or labor) in the economy's costs. Λ_f is the Domar weight of factor f, or the sales share of the factor in GDP.

To find $\frac{d\log \Lambda_f}{d\log \mu_k}$ in terms of economic primitives, I use two-factor ($g = \{K, L\}$) version of Baquaee and Farhi (2020) framework:

$$\frac{d\log \Lambda_f}{d\log \mu_k} = -\sum_j \frac{\lambda_j}{\mu_j}(\theta_j - 1) Cov_{\tilde{\Omega}^{(j)}}(\tilde{\Psi}_{(k)} + \sum_g \tilde{\Psi}_{(g)} \frac{d\log \Lambda_g}{d\log \mu_k}, \frac{\Psi_{(f)}}{\Lambda_f}) - \lambda_k \frac{\Psi_{kf}}{\Lambda_k} \tag{19}$$

Where λ_j is the sales share of each firm, θ_j is the elasticity of substitution between inputs that producer j is using. $\log \mu_k$ is the change in firm's output wedge. The $\tilde{\Omega}^{(j)}$ is a j'th row of an cost-based input-output matrix. While $\tilde{\Psi}_{(g)}$ and $\Psi_{(f)}$ are the g'th and f'th columns of $\tilde{\Psi}$ and Ψ respectively. These matrices are defined as: $\tilde{\Psi} \equiv (I-\tilde{\Omega})^{-1}$ and $\Psi \equiv (I-\Omega)^{-1}$, where Ω is a revenue-based input-output matrix whose cell in row i and column j, that is Ω_{ij}, is firm j's sales to i in firm i's revenue.

Framework	ΔTFP
Hsieh and Klenow (2009), jointly log-normal assumption	-0.65%
Hsieh and Klenow (2009), full model	-0.16%
Levinsohn and Petrin (2012)	-1.11%
Baquaee and Farhi (2020)	-0.22%

Table 8: Change in aggregate TFP due to the joint effect of sanctions and government support

In my setting, $g = \{K, L\}$ and $f = \{K, L\}$.

The $\text{Cov}_{\tilde{\Omega}^{(j)}}$ is defined as a covariance that uses the $\tilde{\Omega}^{(j)}$ as a distribution. Or, formally:

$$Cov_{\tilde{\Omega}^{(j)}}(\tilde{\Psi}_{(k)}, \Psi_{(f)}) = \sum_i \tilde{\Omega}_{ji}\tilde{\Psi}_{ik}\Psi_{if} - \sum_i \left(\tilde{\Omega}_{ji}\tilde{\Psi}_{ik}\right) \sum_i \left(\tilde{\Omega}_{ji}\Psi_{if}\right) \tag{20}$$

Akin to Baquaee and Farhi (2020), I will use the Russian input-output matrix and assume that each firm within an industry has the same input-output linkages as the sector as a whole. As before, I will populate $\log \mu_k$ for firms as a change in their *input* wedges. For every sanctioned firm, I will create a fictitious producer who will buy capital input and sell it to the actual firm with a markup μ_k.

Next, I use the Levinsohn and Petrin (2012) decomposition, and calculate the change in TFP due to sanctions according to Equation 17. I calculate τ's directly from the Hsieh and Klenow formula, $\tau_i^K = \alpha_K \frac{p_i y_i}{r K_i} - 1$, but like with $MRPK_i$, remove measurement error by regressing the τ_i^K on firm fixed effect and subtracting the fixed 4-digit industry component. For the Levinsohn and Petrin (2012) decomposition, I need an estimate for the change in capital input for the sanctioned firms, and not the change in the wedge. I take the estimate from the change in capital input from Table 4, column (3). The average increase in capital for sanctioned firms is 32%. In addition, I need a Domar weight for each firm and an α, the elasticity of output for capital. The elasticity of output is, as before, one minus the labor share in value added of each industry. The Domar weight of each firm is calculated as the sales share of each sanctioned firm in the industry output not re-used by it. I take the "re-used shares" from the Input-Output table of 2016 from Rosstat. I can do this calculation for each industry, or for the economy as a whole. For the economy as a whole, I get a drop in TFP of 1.11%.

Finally, I use the Baquaee and Farhi (2020) framework to learn about the role of

I-O linkages in amplifying the reduction in TFP in industries whose sanctioned firms are inefficiently propped-up. For this exercise, akin to Baquaee and Farhi (2020), I use the aggregate input-output table and assume that each firm in an industry has all the same I-O linkages as all the others in that industry. In Russia, such a table is produced every five years and I pick the closest year to the first year of sanctions, 2016. This table has 97 rows and columns. I remove government and imports. I then combine the input-output table with the information on firms from SPARK and assume that each firm in an industry has the same production function up to a Hicks-neutral productivity shifter. I assume that each industry only consists of firms available in SPARK, and I aggregate the output of each firm from SPARK so that the weight of each firm in an industry corresponds to its sales in total sales of other firms in SPARK in that industry.

I also assume that the only source of distortions are the capital-input distortions and measure it as before, using the MRPK of each firm. This is still an internally-consistent way to measure distortions if we assume the same production function and no other distortions. Each firm trades with other industries and is exposed to its own capital-input distortion, and, indirectly, to other firms' capital-input distortions through the I-O linkages.

I further follow Baquaee and Farhi (2020) and assume that each firm produces via a nested CES structure, taking in a CES bundle of intermediate inputs and value added with an elasticity of substitution $\epsilon = 0.5$. Intermediate inputs bundle combines inputs from each of the other 96 industries with the elasticity of substitution $\theta = 0.2$, and the value added bundle combines capital and labor with the elasticity of substitution $\eta = 1$ (a Cobb-Douglas assumption for the value-added part of the production function, like in the Hsieh and Klenow framework). Finally, the elasticity of substitution between the firms in an industry - a "variety-level elasticity" is $\xi = 8$.

Since each firm has the same production function in an industry, to make the algorithm of calculation faster, I create two firms in each industry, a representative sanctioned firm and a representative non-sanctioned firm. The representative sanctioned firm has a weight equal to a sum of weights of all actual sanctioned firms in that industry and an input distortion equal to a harmonic average of the original firms' distortions. Analogously for the non-sanctioned firms. I then run the Baquaee

and Farhi calculation 19 to get $\frac{d \log \Lambda_f}{d \log \mu_j}$. I then plug it into the Equation 18 to get the change in output from the change in markups for every firm i (in my case, each i is a representative sanctioned or non-sanctioned firm in every industry). I then calculate, what would happen if the input wedge increased by 20% between non-sanctioned firms and sanctioned firms (I do so by reducing the markup on a fictitious capital seller to the sanctioned firms by 0.2). The result is a moderate decrease in output of 0.22%.

How large are the aggregate effects relative to the baseline misallocation? To put the worsening of misallocation from sanctions into perspective, I compare it to the efficiency gains of reallocating resources across the to-be-sanctioned and not-to-be-sanctioned groups (see Appendix D). For example, a 1% reduction of TFP explains about 3-4% of the "sanctioned group"-driven distance to the frontier.

6 Conclusion

Using structural and reduced-form evidence, I show that targeted or "smart" sanctions combined with government shielding help rather than hurt the firms they target. At the same time, these sanctions harm the wider economy. I use a unique natural experiment, the US and EU sanctions on Russia in 2014-2020, to causally estimate the combined effect of sanctions and shielding that affected sanctioned firms relative to non-sanctioned or not-yet-sanctioned firms. I use state-of-the-art tools to combine the estimates from this natural experiment with three distinct heterogeneous firm models to quantify the effects of sanctions on aggregate TFP and thus measure the collateral damage from sanctions.

I find that sanctions, combined with shielding, have led the targeted firms to gain 32% more capital and 20% more revenue. I find that the government used at least three mechanisms to counteract and over-compensate for sanctions: government contracts, subsidies and loans. Nonetheless, this reallocation was harmful for the economy, because the treated firms were already over-resourced prior to sanctions. By reallocating resources further towards the targets, the government did not internalize the implications of such actions for aggregate productivity. I quantify that this joint sanctions and shielding effect reduced aggregate TFP by up to 1.11% (and, therefore,

reduced GDP by up to 1.11% assuming total capital and labor resources stayed at the pre-sanctions level). This estimate is likely an understatement for the fall of GDP resulting from sanctions, as total capital resources have likely also shrunk over this period due to deterioration of terms of trade and capital flight.

This paper has important policy implications. First, sanctions, specifically sanctions against Russia, have come to the forefront of international policy in 2022-2023. This raises questions about whether "smart" sanctions - designed to disrupt the elite without impacting the average citizen - are able to meet their *stated* objectives. The evidence in this paper shows that "smart" sanctions have failed on both fronts. Rather than causing divisions among the economic elites, the sanctions have actually relatively strengthened their economic position, and tied these elites closer to the state through state support. One could argue that sanctions indeed increase the costs of support of the elites or the defence sector, and thus are successful, regardless of how much these enterprises benefited after state support. However, since the government wrought broader economic loss by increasing the share of these firms in a shrinking pie, sanctions did fail to be targeted or narrow. Since sanctions lead to collateral damage, even if self-inflicted, this contradicts at least on e mandate of such sanctions - sparing the average population. Thus, designers of sanctions should revise their goals and admit that any sanctions policy, even the smart sanctions policy will have collateral damage and having accepted this, focus on ways of increasing the costs of financing the elites and the military in a targeted country.

An unexpected outcome of sanctions could have been a long-term political cost due to the government's reallocation bringing lower productivity, lower GDP and less support by the ordinary citizens. However, this cost (at the current strength of sanctions) was not enough to deter the Russian state from future breaking of international law and the full-scale invasion of Ukraine. Such result may be specific to an autocratic state, which can bear the political risk for longer and therefore requires a lager political cost to change its actions.

The dynamics of reallocation revealed in this paper foreshadow current trends in the Russian economy, where economic resources, such as private assets, or assets of foreign owners are being requisitioned, nationalised and reallocated to the members of the elite, so that they further owe their new position and wealth to the current

state, and thus remain loyal.[40]

With over 70 countries under "smart" sanctions today, the key implication from this study is that these tools and/or their goals need to be revised. Due to the likelihood that resources will be reallocated by a targeted government, all sanctions risk inflicting collateral damage on ordinary people: regardless of which firms are targeted, there are important aggregate implications through reallocation of resources across firms. Once this is recognised, a successful sanctions regime would depend on its objectives.

If the objective of sanctions is to reduce military prowess or split elites once international law is broken, I show that targeting specific elites alone has not worked in the case of Russia. In this case, the elites had outside options (within the Russian market) and sanctions did not significantly constrain the government's ability to generate and redistribute rents.

Comprehensive trade and financial embargoes may have different effects and it remains an open question how large these effects are. However, even in the case of comprehensive sanctions, policymakers should factor in that targeted states will both likely mitigate the direct damage of sanctions, and become more resilient with time, especially when sanctions are imposed gradually. Future research will explore in more detail how stricter sanctions regimes work through the economy, and importantly, whether and which sanctions help to bring peace.

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7 Online Appendix

7.1 Appendix A. Additional tables and figures

	count	mean	sd	min	max
Labor count, latest year	5,865,074	28	204	0	16,757
Firm age, yrs	6,177,338	9.1	7	0	108
Revenue, rub	5,621,811	298,867,506	24,361,516,040	-202,930,000	39,149,843,354,000
Value added, rub	1,488,387	190,612,354	8,262,448,419	-601,160,548,352	5,261,399,949,312
Book value of capital, rub	5,651,784	108,793,040	11,789,788,445	-155,114,000	8,002,629,050,000
Payment to labor, rub	1,526,079	64,135,436	1,327,653,226	-8,446,435,000	531,865,337,000
Materials, rub	1,653,280	601,631,449	13,543,482,197	-122,617,309,000	4,820,693,835,000
Assets, rub	4,768,430	265,420,929	22,254,057,758	-155,068,000	20,986,162,315,264
Private for-profit firm (dummy)	6,238,848	.99	.073	0	1
SOE	6,238,848	.0053	.073	0	1
Foreign-owned firm (dummy)	6,238,848	.00035	.019	0	1
Suppliers to state and SOEs (dummy)	6,177,579	.14	.35	0	1
Any Sanction	6,238,848	.0015	.038	0	1
Firm under input sanction	6,238,848	.00082	.029	0	1
Firm under blocking sanction	6,238,848	.00031	.017	0	1
Firm MRPK	5,138,357	882	239,055	-374,481	447,111,333
Firm MRPL	1,437,266	195	7,513	-175,174	4,877,622
Observations	6,238,848				

Notes: This table reports summary statistics for the firms in the SPARK dataset from 2012 to 2020. An observation is at the firm-year level.

Table 2.A1: Summary statistics of key variables

Industry	Year sanctioned								
	2014	2015	2016	2017	2018	2019	2020	2022	Total
Activities auxiliary to financial services and insurance	2	4	7	2	6	0	0	7	28
Activities for the maintenance of buildings and territories	0	0	1	0	1	0	0	0	2
Activities for the provision of other personal services	0	1	0	0	0	0	0	0	1
Activities for the provision of places for temporary residence	3	1	2	3	3	1	0	1	14
Activities in the field of architecture and engineering	13	15	12	8	18	2	0	7	75
Activities in the field of law and accounting	3	1	2	1	2	0	0	4	13
Activities in the field of sports, recreation and entertainment	1	0	2	0	2	0	1	3	9
Activities in the field of telecommunications	0	3	0	0	5	0	0	4	12
Activities in the field of television and radio broadcasting	0	0	0	0	5	0	0	3	8
Activities of libraries, archives, museums and other cultural objects	0	0	0	0	1	0	0	0	1
Activities of state administration bodies to ensure military security	1	0	1	0	1	0	0	0	3
Administrative activities	0	1	4	2	1	0	0	1	9
Advertising activities and market research	29	8	3	1	1	0	0	17	59
Air and space transport activities	0	1	3	0	0	0	0	11	15
Beverage production	5	0	1	0	1	0	1	0	8
Building	3	1	2	5	10	3	1	5	30
Coal mining	0	0	0	0	1	0	0	0	1
Construction of engineering structures	10	2	0	7	6	0	0	0	25
Creative activities, arts and entertainment activities	0	0	0	0	1	0	0	0	1
Crop and livestock production, hunting and related services in these areas	8	3	2	2	2	0	0	5	22
Development of computer software, consulting services in this area	10	7	0	1	3	0	0	25	46
Electricity, gas and steam supply; air conditioning	6	11	3	10	21	1	0	1	53
Employment and recruiting activities	0	0	0	1	0	0	0	0	1
Extraction of oil and natural gas	18	17	4	11	6	0	0	0	56
Extraction of other minerals	2	0	0	0	0	0	0	4	6
Film, video and television program production, sound recording	0	0	0	1	1	0	0	0	2
Financial services activities other than insurance and pension services	17	48	44	18	22	0	0	49	198
Food and beverage provision activities	0	0	0	1	2	0	0	1	4
Food production	5	1	0	0	4	0	1	0	11
Forestry and logging	0	0	0	1	16	0	0	0	17
Head office activities; management consulting	27	12	20	13	12	0	0	23	107
Healthcare activities	2	1	2	2	3	3	0	2	15
Information technology activities	5	6	1	3	2	0	0	7	24
Insurance, reinsurance, activities of non-state pension funds	0	1	0	0	0	0	0	0	1
Land and pipeline transport activities	1	12	1	12	14	0	2	2	44
Manufacture of chemicals and chemical products	6	3	1	5	2	0	0	6	23
Manufacture of computers, electronic and optical products	48	9	6	0	1	2	0	39	105
Manufacture of electrical equipment	7	0	3	0	2	0	0	8	20
Manufacture of finished metal products, except for machinery and equipment	12	3	3	4	0	0	0	6	28
Manufacture of leather and leather products	2	0	0	0	0	0	0	0	2
Manufacture of machinery and equipment not included in other categories	8	2	1	4	2	2	0	9	28
Manufacture of motor vehicles, trailers and semi-trailers	3	1	0	0	3	0	0	2	9
Manufacture of other finished goods	1	0	0	0	0	0	0	0	1
Manufacture of other non-metallic mineral products	3	3	0	0	1	1	0	2	10
Manufacture of other vehicles and equipment	36	9	7	0	1	2	0	10	65
Manufacture of paper and paper products	0	1	0	0	1	0	0	0	2
Manufacture of rubber and plastic products	0	1	0	0	1	0	0	2	4
Manufacture of textiles	0	0	2	0	0	0	0	1	3
Manufacture of wearing apparel	1	0	0	0	0	0	0	1	2
Metallurgical production	3	0	0	1	7	0	0	5	16
Mining of metal ores	0	0	0	0	3	0	0	2	5
Other professional scientific and technical activities	0	0	0	0	2	0	0	2	4
Printing activities and copying of information carriers	2	1	0	0	0	0	0	1	4
Production of coke and petroleum products	4	9	7	2	4	0	0	0	26
Production of medicines and materials used for medical purposes	3	0	0	0	0	0	0	0	3
Provision of services in the field of mining	2	7	1	2	5	0	0	0	17
Publishing activities	0	0	1	1	1	0	0	1	4
Real estate operations	39	20	21	23	24	0	0	46	173
Rent and lease	0	1	1	0	1	0	0	2	5
Repair and installation of machinery and equipment	14	2	0	4	2	2	0	24	48
Repair of computers, personal items and household items	0	0	0	0	0	0	0	1	1
Research and development	102	12	19	2	6	3	1	90	235
Retail trade, excluding trade in motor vehicles and motorcycles	1	3	3	0	7	0	0	2	16
Security and investigation activities	1	0	0	0	1	0	0	0	2
Specialized construction works	1	1	0	4	3	0	0	2	11
Warehousing and auxiliary transport activities	7	6	5	18	8	0	5	8	57
Water intake, purification and distribution	1	0	1	0	0	0	0	1	3
Water transport activities	0	1	0	2	2	0	0	6	11
Wholesale and retail trade in motor vehicles and motorcycles and their repair	1	0	0	0	0	0	2	7	10
Wholesale trade, except for the wholesale trade of motor vehicles and motorcycles	20	19	7	9	16	1	0	34	106
Wood processing and manufacture	0	0	1	0	5	0	0	3	9
Total	499	271	207	186	284	23	14	505	1,989

Notes: This tabulates the sanctioned firms by year of treatment and the 2-digit industry in which the sanctioned firms operate.

Table 2.A2: Industry by year

	(1) Ln Revenue	(2) Ln Book Value of Capital	(3) Ln Revenue	(4) Ln Book Value of Capital	(5) Ln Revenue	(6) Ln Book Value of Capital	(7) Ln Revenue	(8) Ln Book Value of Capital	(9) Ln Revenue	(10) Ln Book Value of Capital
Any Sanction	0.209	0.307	0.177	0.273	0.250	0.348	0.217	0.341	0.147	0.306
	(0.062)	(0.067)	(0.082)	(0.073)	(0.065)	(0.066)	(0.061)	(0.063)	(0.065)	(0.070)
Any Sanction × Direct sanction dummy	-0.019	0.029								
	(0.113)	(0.120)								
Any Sanction × SDN Sanction			0.044	0.073						
			(0.096)	(0.089)						
Any Sanction × minority					-0.148	-0.180				
					(0.089)	(0.087)				
Any Sanction × energy							-0.096	-0.163		
							(0.109)	(0.148)		
Any Sanction × exporter									0.228	0.039
									(0.093)	(0.102)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firms	635156	678994	635156	678994	635156	678994	635156	678994	635156	678994
Sanctioned firms	916	928	916	928	916	928	916	928	916	928
Industries	842	862	842	862	842	862	842	862	842	862
Observations	3577822	3852607	3577822	3852607	3577822	3852607	3577822	3852607	3577822	3852607
R-squared	.858	.89	.858	.89	.858	.89	.858	.89	.858	.89
Clustering: Firm and industry-by-year										

Notes: All dependent variables are in logs. "Direct" is an indicator for being directly sanctioned rather than through being a majority-owned subsidiary of a directly sanctioned firm. "SDN" is an indicator for a blocking sanction. "Minority" is an indicator for being a minority-owned subsidiary of a directly-sanctioned firm. "Energy" is an indicator for being part of the energy sector, according to 4-digit industry. "Exporter" is an indicator for being an exporter. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A3: Average effects of sanctions: heterogeneity by status of sanctioned firm

	(1) Ln Revenue	(2) Ln Book Value of Capital	(3) Ln Revenue	(4) Ln Book Value of Capital	(5) Ln Revenue	(6) Ln Book Value of Capital	(7) Ln Revenue	(8) Ln Book Value of Capital	(9) Ln Revenue	(10) Ln Book Value of Capital
Any Sanction	0.198	0.303	0.156	0.291	0.159	0.290	0.150	0.286	0.163	0.271
	(0.060)	(0.063)	(0.063)	(0.066)	(0.056)	(0.059)	(0.065)	(0.068)	(0.068)	(0.073)
Any Sanction × military	0.044	0.114								
	(0.096)	(0.113)								
Any Sanction × military2			0.263	0.139						
			(0.096)	(0.101)						
Any Sanction × system forming					0.615	0.380				
					(0.161)	(0.185)				
Any Sanction × strategic							0.240	0.140	0.225	0.022
							(0.103)	(0.112)	(0.111)	(0.111)
Any Sanction × SOE									-0.100	0.124
									(0.187)	(0.162)
Any Sanction × SOE × strategic									0.107	0.297
									(0.226)	(0.256)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firms	635156	678994	635156	678994	635156	678994	635156	678994	635156	678994
Sanctioned firms	916	928	916	928	916	928	916	928	916	928
Industries	842	862	842	862	842	862	842	862	842	862
Observations	3577822	3852607	3577822	3852607	3577822	3852607	3577822	3852607	3577822	3852607
R-squared	.858	.89	.858	.89	.858	.89	.858	.89	.858	.89
Clustering: Firm and industry-by-year										

Notes: All dependent variables are in logs. "Military" is an indicator for being a prominent firm operating in the military sector, according to the leading military online magazine. "Military supplier" is an indicator for being part of a procurement supplier to the military. "System-forming" is an indicator of being part of the official government designation of enterprises central to production and employment. "Strategic" is an indicator for being part of the official government list of strategic firms. SOE is an indicator for state-owned enterprise. All indicators are taken from SPARK. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A4: Average effects of sanctions: heterogeneity by strategic status

	(1) ln(Contract Volume)	(2) Sell dummy	(3) Contract Count
Any Sanction	-0.033	-0.001	-0.004
	(0.046)	(0.000)	(0.002)
Buyer FE	✓	✓	✓
Seller FE	✓	✓	✓
Buyer Industry-year FE	✓	✓	✓
Buyers	8655	8667	8667
Sellers	71890	72249	72249
Sanctioned buyers	576	576	576
Industries of buyers	406	406	406
Observations	526162	530079	530079
R-squared	.595	.233	.216
Clustering: Buyer and Industry of buyer-by-year			

Notes: The regressions use the transactions between procuring firms and suppliers as observations. The data contains only the procurement contracts between the state or SOEs and their suppliers. "Any Sanction" is the indicator for when the buyer is sanctioned. Column (1) uses the sample of firms that have non-zero values of procurement volume reported and identify the intensive margin. Column (2) uses all observations with transaction data matching the balance sheet data, and "switching into buying" as the Y-variable. Column (3) uses the same sample as Column (2) and the total count of contracts as a Y-variable. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A5: Average effects of sanctions: spillovers on suppliers to sanctioned firms

	(1) Stop existing shr	(2) Stop reporting shr	(3) Remain	(4) Report
Any Sanction	-0.018	-0.047	0.003	0.054
	(0.002)	(0.004)	(0.001)	(0.007)
Firm FE			✓	✓
Industry-year FE	✓	✓	✓	✓
Size-year FE			✓	✓
Firms			721884	721884
Sanctioned firms			936	936
Industries	247	247	874	874
Observations	3508	3508	4196031	4196031
R-squared	.568	.566	.514	.693
Clustering: Columns (1) and (2) 4-digit industry; columns (3)-(6) firm and industry-by-year				

Notes: All dependent variables are in logs. Columns (1) and (2) use the shares of exits and firms that stopped reporting within sanction status-by-4-digit-industry groups. Columns (3) and (4) use firm-year observations with "Remain" and "Report" dummies as Y-variables, indicating that the firm continues to exist and continues to report the balance sheet variables. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A6: Exit as a result of sanctions

	(1) Ln Revenue	(2) Ln Book Value of Capital	(3) Ln Revenue	(4) Ln Book Value of Capital
Sanctions 1st wave	0.215	0.346		
	(0.073)	(0.080)		
Sanctions 2nd wave			0.195	0.293
			(0.075)	(0.077)
Firm FE	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓
Firms	634700	678534	634696	678526
Sanctioned firms	460	468	456	460
Industries	842	862	842	862
Observations	3574368	3849145	3574523	3849220
R-squared	.857	.89	.857	.89

Clustering: Firm and industry-by-year

Notes: All dependent variables are in logs. The 1st wave of sanctions is the indicator for the group of firms firs sanctioned by SSI or SDN in 2014-15. The 2nd wave of sanctions is the indicator for the group of firms first sanctioned by SSI or SDN in 2016 or after. Industry×Year FE are 4-digit industry by year fixed effects. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanction 1st wave is the sanction dummy of being sanctioned before 2016, Sanction-2nd wave is the sanction dummy of being sanctioned for the first time on or after 2016. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A7: Wave 1 or Wave 2 effect

	SOEs (controls)		Strategic Firms (controls)		Exporters (controls)		Minority Subsidiaries (controls)	
	(1) Ln Book Value of Capital	(2) Ln Revenue	(3) Ln Book Value of Capital	(4) Ln Revenue	(5) Ln Book Value of Capital	(6) Ln Revenue	(7) Ln Book Value of Capital	(8) Ln Revenue
Any Sanction	0.331	0.177	0.296	0.212	0.246	0.179	0.248	0.117
	(0.062)	(0.059)	(0.056)	(0.051)	(0.057)	(0.051)	(0.064)	(0.056)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firms	4799	4713	2241	2226	27569	27569	1273	1247
Sanctioned firms	863	856	864	856	869	862	889	883
Industries	399	393	365	362	668	663	248	241
Observations	30920	30243	15024	14931	195255	195412	7546	7285
R-squared	.00301	.000881	.00509	.00323	.00024	.000165	.00533	.00117

Clustering: Firm and industry-by-year

Notes: All dependent variables are in logs. Observations are de-meaned by 4-digit industry by year fixed effects and size-year trends using all observations in the sample before running the actual regression. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A8: Alternative Control Groups

	(1) Ln Book Value of Capital	(2) Ln Revenue	(3) Ln Book Value of Capital	(4) Ln Revenue	(5) Ln Book Value of Capital	(6) Ln Revenue	(7) Ln Book Value of Capital	(8) Ln Revenue	(9) Ln Book Value of Capital	(10) Ln Revenue
Any Sanction	0.266	0.173	0.296	0.153	0.241	0.112	0.208	0.121	0.316	0.203
	(0.056)	(0.051)	(0.057)	(0.051)	(0.055)	(0.050)	(0.056)	(0.051)	(0.057)	(0.054)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE			✓	✓						
2-digit industry-yr FE					✓	✓				
4-digit industry-yr FE							✓	✓	✓	✓
Size-year FE										
Firms	1126133	1140292	1126133	1140292	1126131	1140290	1126072	1140214	678994	635156
Sanctioned firms	1244	1277	1244	1277	1244	1277	1244	1277	928	916
Industries	917	909	917	909	915	907	869	859	862	842
Observations	5369438	5270826	5369438	5270826	5369426	5270814	5369070	5270425	3852607	3577822
R-squared	.883	.848	.883	.848	.884	.849	.885	.85	.89	.858
Clustering: Firm and industry-by-year										

Notes: All dependent variables are in logs. Industry×Year FE are 4-digit industry by year fixed effects. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A9: Robustness of the main results to the different fixed effects.

	(1) Ln Revenue	(2) Ln Book Value of Capital	(3) Ln Revenue	(4) Ln Book Value of Capital	(5) Ln Revenue	(6) Ln Book Value of Capital
Any Sanction	0.203	0.316				
	(0.054)	(0.057)				
sanc_all_1wave			0.215	0.346		
			(0.073)	(0.080)		
sanc_all_2wave					0.195	0.293
					(0.075)	(0.077)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓
Size-year FE	✓		✓	✓	✓	✓
Firms	635156	678994	634700	678534	634696	678526
Sanctioned firms	916	928	460	468	456	460
Industries	842	862	842	862	842	862
Observations	3577822	3852607	3574368	3849145	3574523	3849220
R-squared	.858	.89	.857	.89	.857	.89
Clustering: Firm and industry-by-year						

Notes: All dependent variables are in logs. Industry×Year FE are 4-digit industry by year fixed effects. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A10: Robustness of the main results to the different fixed effects.

	(1) ln Assets	(2) ln Assets	(3) Ln Book Value of Capital	(4) Ln Book Value of Capital	(5) Ln Materials	(6) Ln Materials	(7) Ln payment to labor	(8) Ln payment to labor
Any Sanction	0.181	0.171	0.209	0.169	0.225	0.235	0.104	0.103
	(0.049)	(0.054)	(0.054)	(0.060)	(0.053)	(0.060)	(0.046)	(0.051)
Any Sanction × SOE		0.064		0.236		-0.063		0.004
		(0.110)		(0.117)		(0.104)		(0.090)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firms	168666	168666	165735	165735	180433	180433	172938	172938
Sanctioned firms	921	921	908	908	922	922	920	920
Industries	755	755	751	751	764	764	762	762
Observations	810324	810324	785752	785752	865618	865618	831298	831298
R-squared	.961	.961	.917	.917	.868	.868	.906	.906
Clustering: Firm and industry-by-year								

Notes: All dependent variables are in logs. Industry×Year FE are 4-digit industry by year fixed effects. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A11: Robustness of the main results to the sample where materials are reported.

	(1) Ln Value Added	(2) Ln Value Added	(3) Ln Revenue	(4) Ln Revenue	(5) Ln Profit	(6) Ln Profit
Any Sanction	0.081	0.088	0.173	0.176	0.125	0.106
	(0.039)	(0.044)	(0.053)	(0.057)	(0.051)	(0.055)
Any Sanction × SOE		-0.037		-0.012		0.113
		(0.097)		(0.127)		(0.118)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓
Firms	149840	149840	173779	173779	153753	153753
Sanctioned firms	828	828	902	902	847	847
Industries	745	745	759	759	746	746
Observations	672815	672815	826218	826218	703966	703966
R-squared	.874	.874	.89	.89	.875	.875
Clustering: Firm and industry-by-year						

Notes: All dependent variables are in logs. Industry×Year FE are 4-digit industry by year fixed effects. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A12: Robustness of the main results to the sample where materials are reported.

	(1) Ln MRPK	(2) Ln MRPK	(3) Ln MRPL	(4) Ln MRPL	(5) Ln MRPM	(6) Ln MRPM
Any Sanction	-0.043	-0.013	0.054	0.056	-0.026	-0.036
	(0.052)	(0.059)	(0.045)	(0.049)	(0.041)	(0.043)
Any Sanction × SOE		-0.176		-0.012		0.054
		(0.103)		(0.118)		(0.114)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓	✓	✓
Size-year FE	✓	✓	✓	✓	✓	✓
Firms	160934	160934	168524	168524	173779	173779
Sanctioned firms	891	891	901	901	902	902
Industries	748	748	759	759	759	759
Observations	760401	760401	804037	804037	826218	826218
R-squared	.886	.886	.815	.815	.619	.619
Clustering: Firm and industry-by-year						

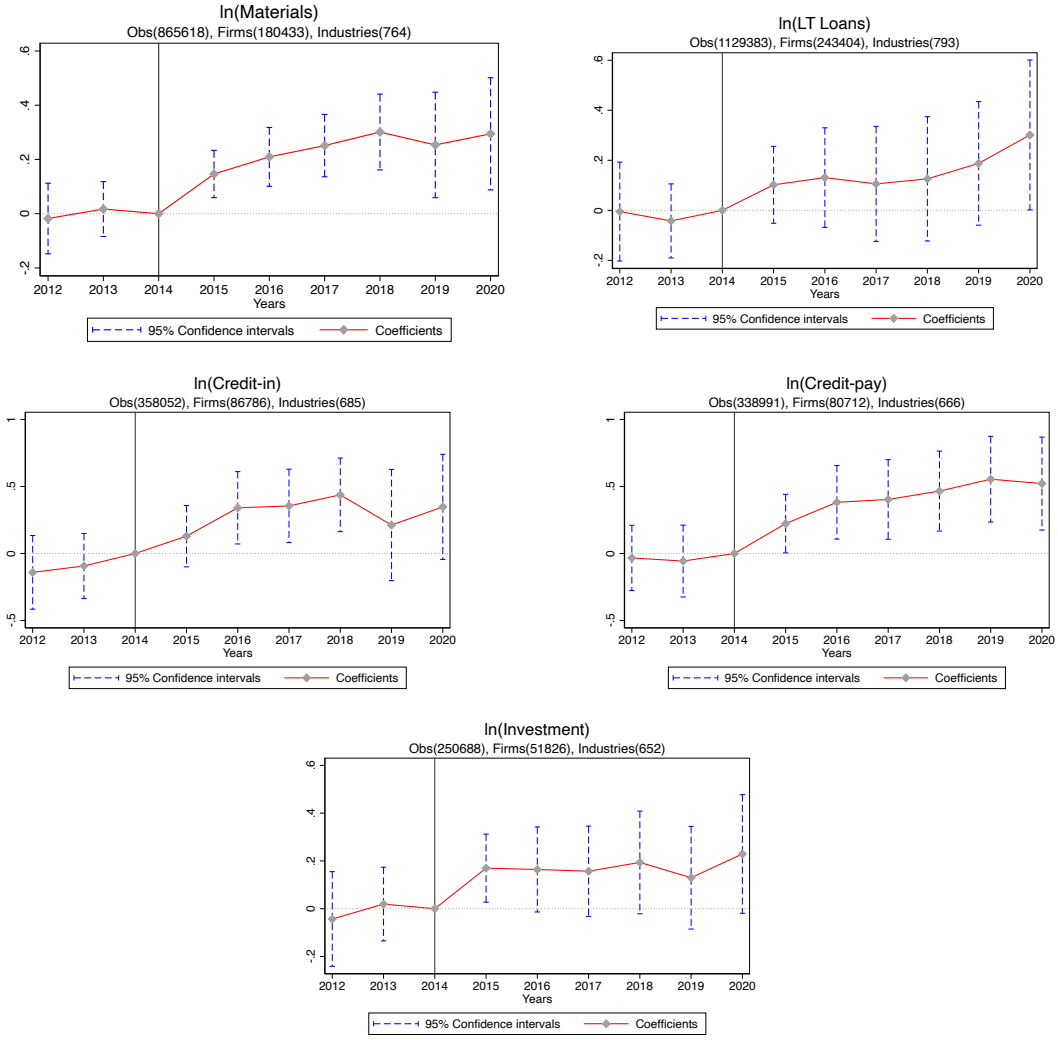
Notes: All dependent variables are in logs. Industry×Year FE are 4-digit industry by year fixed effects. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A13: Robustness of the main results to the sample where materials are reported.

	(1) ln(Ai)	(2) ln(Ai)
Any Sanction	-0.041	-0.009
	(0.040)	(0.042)
Any Sanction × SOE		-0.179
		(0.101)
Firm FE	✓	✓
Industry-year FE	✓	✓
SOE-year FE	✓	✓
Firms	134323	134323
Sanctioned firms	811	811
Industries	715	715
Observations	603840	603840
R-squared	.739	.739

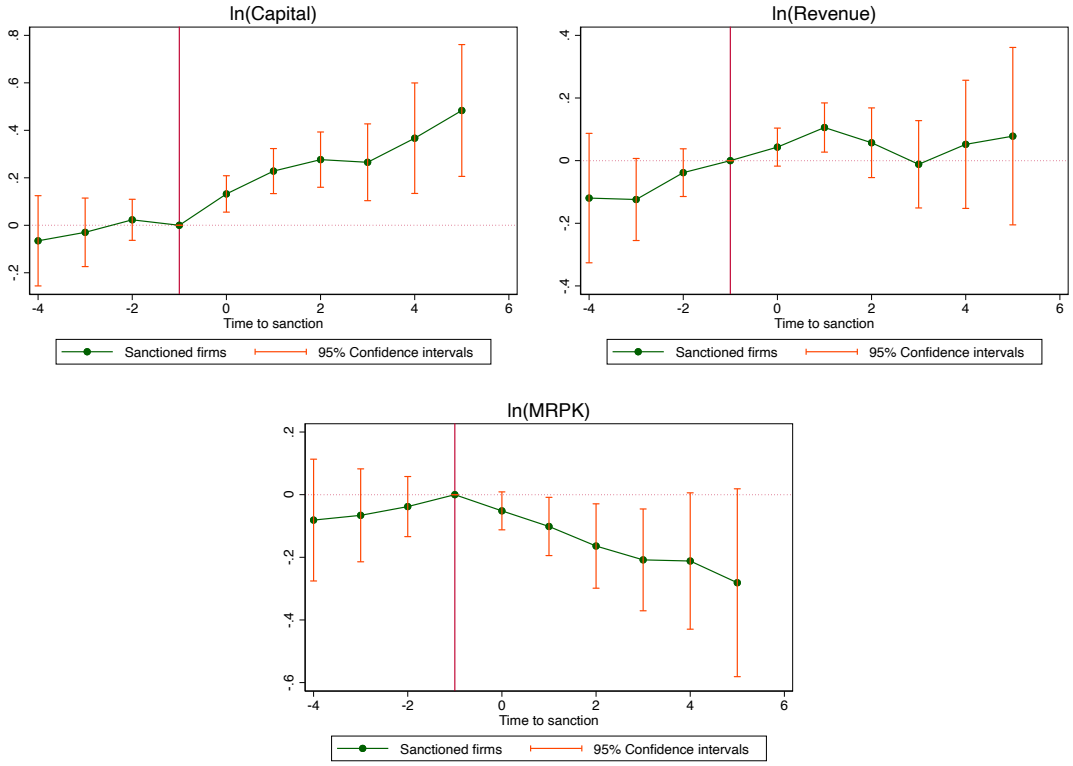
Notes: All dependent variables are in logs. Variable $ln(A_i)$ is backed out from the Hsieh and Klenow model using CES demand assumption with elasticity of substitution 7. Industry×Year FE are 4-digit industry by year fixed effects. Industry×Year FE are 4-digit industry by year fixed effects. Size×Year FE are firms' average pre-treatment capital quartiles interacted with year fixed effects. Sanctioned firms give the count of any sanction firm - SSI or SDN. Standard errors are two-way clustered at the firm and 4-digit industry by year level.

Table 2.A14: Effects of sanctions on firm TFPQ.



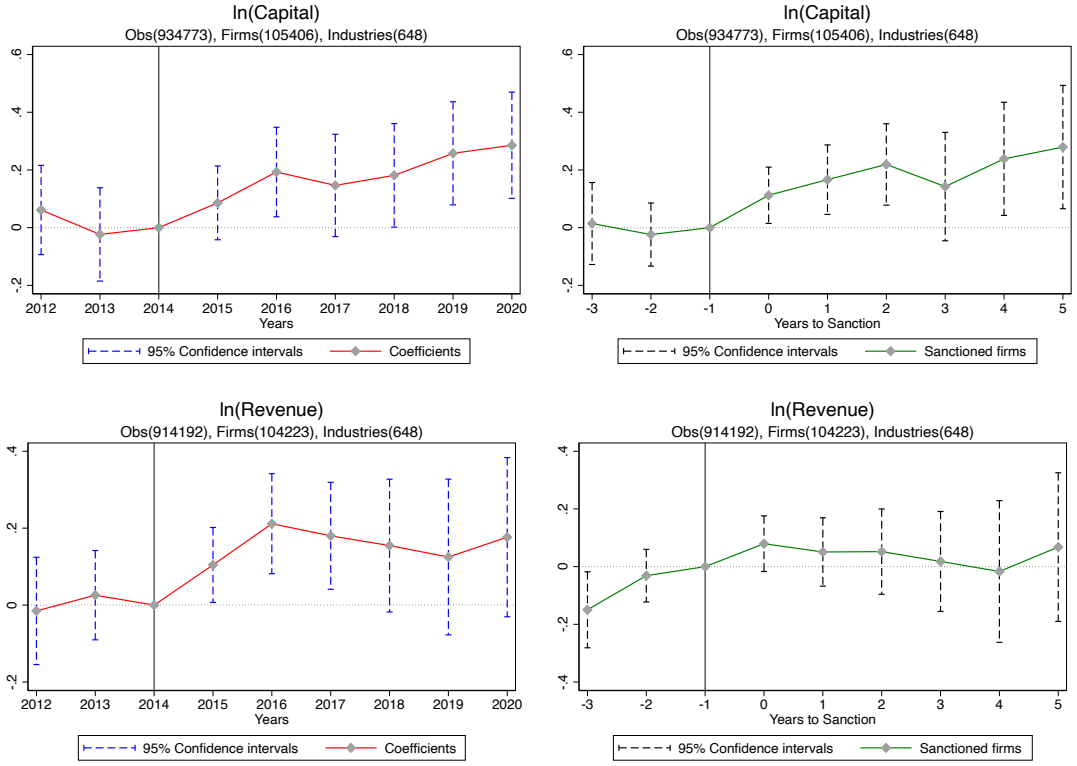
Notes: This figure reports event study graphs for the average effects of the sanctions on sanctioned firms relative to non-sanctioned firms. Each dot is the coefficient on the interaction between being observed in the year 2012, 2013, 2014, 2015, 2016, 2017, 2018, 2019 and 2020 and being sanctioned with SSI or SDN sanctions. For all event studies but the first one on subsidies, the same control variables are used as in baseline regression: firm fixed effects, 4-digit industry-year fixed effects and firm-size-by-year fixed effects. Effectively, each dot is the deviation of the sanctioned firm log firm-level variable from the 4-digit-industry-by-year fixed effects and firm size-quartile-by-year fixed effects. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 2.A1: Pre-post 2014 event study of procurement sales, subsidies, and loans with never-sanctioned firms in the control group.



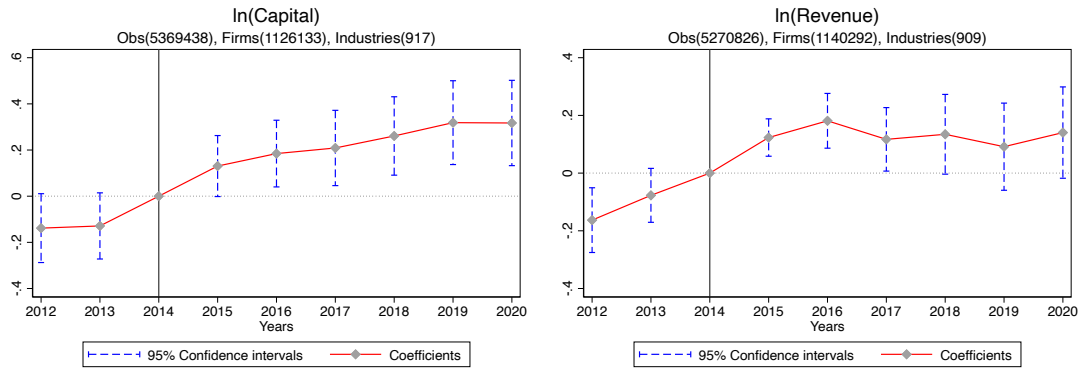
Notes: This figure reports event study graphs using the estimator de Chaisemartin, C and D'Haultfoeuille (2020b) for the average effects of the sanctions on sanctioned firms. The effect is identified within sanctioned firms: sanctioned firms are compared to not-yet sanctioned firms. The first year of firm sanction is normalized to take place in year 0. Each dot is the coefficient on the indicator of being observed t years after the sanctions announcement. Control variables used are firm fixed effects, 4-digit industry-year fixed effects and size-quartile-by-year fixed effects. Non sanctioned firms are used to identify the 4-digit industry-year fixed effects and the size-year fixed effects. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 2.A2: Event study with not-yet sanctioned firms in the control group (de Chaisemartin, C and D'Haultfoeuille (2020b) estimator).



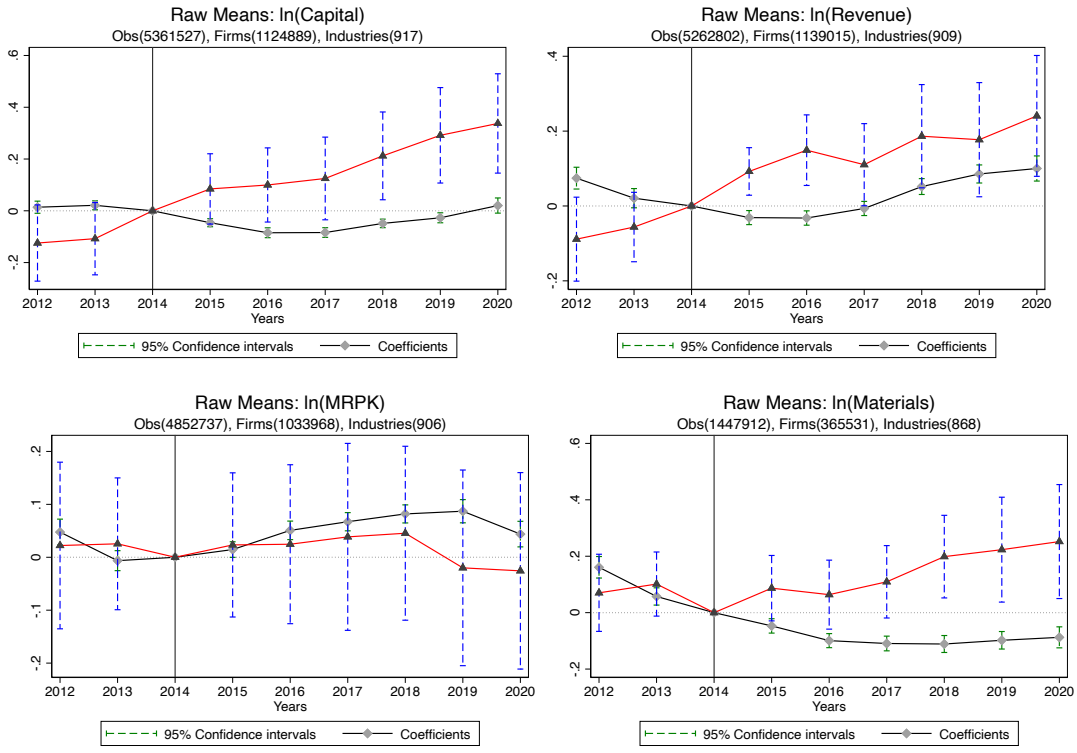
Notes: This figure reports event study graphs with the average effects of the sanctions on sanctioned firms for a constant sample of firms (firms with observations in every period). The effect is identified relative to non-sanctioned firms (red and blue) or relative to not-yet-sanctioned firms (green and grey). For the latter specification, the first year of firm sanction is normalized to take place in year 0 . Control variables used are firm fixed effects and 4-digit industry-year fixed effects and pre-treatment size quartile-by-year FE. Non sanctioned firms are used to identify the 4-digit industry-year fixed effects and the size-year fixed effects. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 2.A3: Event studies, constant sample of firms.



Notes: This figure reports event study graphs with the average effects of the sanctions on sanctioned firms. The effect is identified relative to non-sanctioned firms. Control variables used are firm fixed effects and year fixed effects. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 2.A4: Event studies, robustness to removing size-year- and industry-year-fixed effects.



Notes: This figure reports event study graphs for the average effects of the sanctions on sanctioned firms where none for the time fixed effects are controlled for (no industry-by-year FE, no size-by-year FE). The grey squares connected by the black line it the change for the non-sanctioned firms, and the red line connecting the black triangles is the change for the sanctioned firms. The only control variable is firm fixed effect. Each square is the coefficient on the indicator of being observed t years after 2014. Each triagle is the coefficient from an equivalent regression run for sanctioned firms. The dependent variables are in logs. The confidence intervals are at the 95% level.

Figure 2.A5: Raw means: no time controls, just firm FE

7.2 Appendix B. Measuring firm productivity and distortions

Using the framework in the model Section 2, I compute $MRPK_i$, $MRPL_i$, $TFPQ_i$ and $TFPR_i$. I use book value of capital for K_i, total wage bill for L_i and firm revenue in that year.[41]. I compute $TFPR_i$ as defined by Hsieh and Klenow, using the value added production function, where I divide the value added, not revenue, by firm inputs ($TFPR_i \equiv \frac{p_i F_s(K_i, L_i)}{K_i^{\alpha_s} L_i^{1-\alpha_s}}$). I compute $TFPQ_i$ by backing it our from Hsieh and Klenow model, and it is a function of firm size as defined in Equation 13. In order to compute either $TFPR_i$ or $TFPQ_i$, I need the production function parameter α. I take α as one minus the labor share in total value added in a 4-digit sector.[42] Finally, to calculate a model-based $TFPQ_i$ I also need the elasticity of demand η. I follow Hsieh & Song (2015) and use $\eta = 0.143$, which corresponds to the elasticity of substitution of 7. Using the values of α and η, I use Equation 13 to calculate physical productivity $TFPQ_i$ and Equations 6, 7 to get marginal revenue and capital productivities $MRPK_i$ and $MRPL_i$ for each firm in each year.

The measures calculated this way are prone to measurement error in inputs and outputs (Bils et al. (2020), Rotemberg & White (2017), Gollin & Udry (2021)). Even non-systematic measurement error will result in higher measured misallocation and higher gaps between real and efficient TFPs. I apply a state-of-the-art method to adjust for measurement error. I combine the baseline approach and winsorise top and bottom 1% of firm observations for each of the variables and follow Adamopoulos et al. (2017) to get the time invariant component of the measures. In particular, I regress the $TFPQ_i$, $MRPK_i$ and $MRPL_i$ on firm and year fixed effects. This removes the transient shocks short-term measurement error in inputs and outputs and gives me the time-invariant firm productivity and wedges.[43] I then separate the firm ef-

[41] The use of book value of capital is standard in the literature. Book value by Russian accounting includes, among other items, buildings and structures, machinery and equipment, computers, vehicles, household equipment, productive and pedigree livestock, perennial plantations. These items are subject to yearly amortization, which is usually linear.

[42] I drop a small number of sectors that are under 10 firms and those that have α over 1 or under 0 in the data. To calculate $MRPK_i$ and $MRPL_i$, I do not need to know the sector-level input elasticities, as they will drop out in the procedure described below.

[43] The regressions I run to correct for measurement error are shown below:

$$ln(TFPQ_i) = \beta_0^{TFPQ} + \gamma_t^{TFPQ} + \phi_i^{TFPQ} + \epsilon_{it}^{TFPQ} \tag{21}$$

$$ln(MRPX_i) = \beta_0^{MRPX} + \gamma_t^{MRPX} + \phi_i^{MRPX} + \epsilon_{it}^{MRPX} \tag{22}$$

fect from the sector component by taking the firm fixed effect and regressing these fixed effects on 4-digit-sector dummies to extract the residuals that are the pure permanent firm $ln(TFPQ_i)$, $ln(MRPK_i)$, $ln(MRPL_i)$, and $ln(TFPR_i)$ components. The $TFPQ_i$, $ln(MRPK_i)$, $ln(MRPL_i)$ and $TFPR_i$ are the exponentials of the residual, after regressing the firm fixed effects on industry dummies.

For the counterfactual exercises, I follow this procedure using the pre-period panel 2012-2014, the period before sanctions. I get the measures of firm $TFPQ_i$, $ln(MRPK_i)$, $ln(MRPL_i)$ and $ln(TFPR_i)$ that do not change over time. I will use these measures for the descriptives in the next section, and in the three frameworks that will produce the aggregate TFP changes.

7.3 Appendix C. Heterogeneous firm model a-la Hsieh and Klenow

7.4 One-industry model.

This is the established framework that uses the "indirect approach" to measure misallocation and a horizontal CES economy to aggregate the effect of wedges. It shows that a **dispersion** of wedges (taxes or subsidies) lead to the dispersion of MRPK and MRPL (marginal revenue products of labour and capital) and thus allocative inefficiency, and lower aggregate TFP. Aggregate output (GDP rather than TFP) in this model depends on the aggregate inputs as well.

Firm i produce quantity Q_i using capital K_i and labor L_i

$$Q_i = A_i K_i^{\alpha} L_i^{1-\alpha} \tag{23}$$

For simplicity of exposition I assume that the elasticity of output to capital input α is the same across firms. In empirical analysis, I will relax this assumption and make α industry-specific. Each firm's output is aggregated to a CES aggregate, where η is one over the constant elasticity of substitution between goods i:

Where β_0^{TFPQ} and β_0^{MRPX} are common intercepts, γ_t^{TFPQ} and γ_t^{TFPR} are the year fixed effects that capture time-varying shocks, such as a common component in trends in mark-ups or oil prices, and ϕ_i^{TFPQ} and ϕ_i^{TFPR} are the firm fixed effects and incorporates all firm-sector components. Finally, ϵ_{it}^{TFPQ} and ϵ_{it}^{MRPX} are the errors, including the transient measurement error and adjustment costs and noise.

$$Q = \left(\sum_{i=1}^{N} Q_i^{1-\eta} \right)^{\frac{1}{1-\eta}} \tag{24}$$

The aggregate firm demands outputs of individual firms and maximizes profits:

$$\max_{Q_i} P \left(\sum_{i=1}^{N} Q_i^{1-\eta} \right)^{\frac{1}{1-\eta}} - \sum_{i=1}^{N} P_i Q_i$$

$$FOC: \frac{1}{1-\eta} P \left(\sum_{i=1}^{N} Q_i^{1-\eta} \right)^{\frac{1}{1-\eta}-1} (1-\eta) Q_i^{-\eta} - P_i = 0$$

$$P \left(\sum_{i=1}^{N} Q_i^{1-\eta} \right)^{\frac{\eta}{1-\eta}} = P_i Q_i^{\eta}$$

$$P Q^{\eta} Q_i^{*1-\eta} = P_i Q_i^{*} \tag{25}$$

The above equation (implicitly) shows how much Q_i is demanded for each firm given P_i, and it is expressed as revenue each firm gets in equilibrium.

Each firm i maximizes profits $\pi_i = P_i Q_i - (1+\tau_i^L) w L_i - (1+\tau_i^K) r K_i$. Or, substituting the implicit expression of quantities demanded for the revenue:

$$\max_{L_i, K_i} \pi_i = P Q^{\eta} Q_i^{*1-\eta} - (1+\tau_i^L) w L_i - (1+\tau_i^K) r K_i$$

s.t.

$$Q_i = A_i K_i^{\alpha} L_i^{1-\alpha}$$

I assume w and r are the **common** and **exogenous** costs of labor and capital. Whereas τ_i^L and τ_i^K are firm-specific distortions to the cost of labor and capital.

$$\{L_i\} : (1-\alpha)(1-\eta) \frac{P Q^{\eta} (A_i K_i^{\alpha} L_i^{1-\alpha})^{1-\eta}}{L_i} = (1+\tau_i^L) w \tag{26}$$

The optimal labor allocation will satisfy this equation:

$$\boldsymbol{\{L_i\} : (1-\alpha)(1-\eta) \frac{P_i Q_i}{L_i} = (1+\tau_i^L) w \equiv MRPL_i} \tag{27}$$

$$\{L_i\}: \; L_i = (1-\alpha)(1-\eta)\frac{P_iQ_i}{MRPL_i} \tag{28}$$

Similarly, this equation will be satisfied by the optimal capital allocation:

$$\boldsymbol{\{K_i\}: \; \alpha(1-\eta)\frac{P_iQ_i}{K_i} = (1+\tau_i^K)r \equiv MRPK_i} \tag{29}$$

$$\{K_i\}: \; K_i = \alpha(1-\eta)\frac{P_iQ_i}{MRPK_i} \tag{30}$$

It is useful to add the definition of TFPR_i, which is often used in the literature and is a summary measure of distortions.

$$\boldsymbol{TFPR_i \equiv \frac{P_iQ_i}{K_i^{\alpha}L_i^{1-\alpha}} = \left(\frac{MRPK_i}{\alpha}\right)^{\alpha}\left(\frac{MRPL_i}{1-\alpha}\right)^{1-\alpha}\frac{1}{(1-\eta)}} \tag{31}$$

Re-arranging optimal output in terms of parameters that constitute the costs of firm i, we get:

$$P_iQ_i = PQ^{\eta}(A_iK_i^{\alpha}L_i^{1-\alpha})^{1-\eta} = PQ^{\eta}\left(A_i\left[\frac{(1-\alpha)(1-\eta)}{(1+\tau_i^L)w}P_iQ_i\right]^{1-\alpha}\left[\frac{\alpha(1-\eta)}{(1+\tau_i^K)r}P_iQ_i\right]^{\alpha}\right)^{1-\eta} \tag{32}$$

$$P_iQ_i = PQ^{\eta}(P_iQ_i)^{1-\eta}(1-\eta)^{1-\eta}\left(A_i\left[\frac{(1-\alpha)}{(1+\tau_i^L)w}\right]^{1-\alpha}\left[\frac{\alpha}{(1+\tau_i^K)r}\right]^{\alpha}\right)^{1-\eta} \tag{33}$$

$$P_iQ_i = P^{\frac{1}{\eta}}Q\left((1-\eta)A_i\left[\frac{(1-\alpha)}{(1+\tau_i^L)w}\right]^{1-\alpha}\left[\frac{\alpha}{(1+\tau_i^K)r}\right]^{\alpha}\right)^{\frac{1-\eta}{\eta}} \tag{34}$$

$$P_iQ_i \propto \left(\frac{A_i}{(1+\tau_i^L)^{1-\alpha}(1+\tau_i^K)^{\alpha}}\right)^{\frac{1-\eta}{\eta}} \tag{35}$$

Combine 27 , 29 and 35 to get that more labor and capital in the absence of τ_i^K and τ_i^L will go to the more productive firm - firm with higher A_i

$$L_i \propto \frac{1}{1+\tau_i^L}\left(\frac{A_i}{(1+\tau_i^L)^{1-\alpha}(1+\tau_i^K)^{\alpha}}\right)^{\frac{1-\eta}{\eta}} \tag{36}$$

$$K_i \propto \frac{1}{1+\tau_i^K}\left(\frac{A_i}{(1+\tau_i^L)^{1-\alpha}(1+\tau_i^K)^{\alpha}}\right)^{\frac{1-\eta}{\eta}} \tag{37}$$

Equivalently,

$$1+\tau_i^L \propto \frac{P_i Q_i}{wL_i} \tag{38}$$

$$1+\tau_i^K \propto \frac{P_i Q_i}{K_i} \tag{39}$$

Expressing 35 in terms of how we can measure each of the distortions:

$$P_i Q_i \propto \left(\frac{A_i}{(\frac{P_i Q_i}{L_i})^{1-\alpha}(\frac{P_i Q_i}{K_i})^{\alpha}}\right)^{\frac{1-\eta}{\eta}} \quad (4) \tag{40}$$

Revenues of firms will be negatively correlated to the geometric average of the distortions (themselves proportional to labour and capital productivities, implying higher labour and capital productivity - labour and capital input is too small) and positively correlated with their productivityA_i. Again, remember that this assumes: α, w, r, η are identical across firms. Any deviation in these will manifest itself in deviations in τ_K, and/or τ_L.

It is also useful to derive a model-based firm productivity:

$$PQ^{\eta}(A_i K_i^{\alpha} L_i^{1-\alpha})^{1-\eta} = P_i Q_i \tag{41}$$

$$A_i = (PQ^{\eta})^{\frac{-1}{1-\eta}} \frac{(P_i Q_i)^{\frac{1}{1-\eta}}}{K_i^{\alpha} L_i^{1-\alpha}} \tag{42}$$

Simplifying,

$$\boldsymbol{TFPQ_i \equiv A_i = \kappa \frac{(P_i Q_i)^{\frac{1}{1-\eta}}}{K_i^{\alpha} L_i^{1-\alpha}}} \tag{43}$$

where

$$\kappa = (PQ^{\eta})^{-\frac{1}{1-\eta}} \tag{44}$$

7.5 Aggregation

$$P_i Q_i = P^{\frac{1}{\eta}} Q \left((1-\eta) A_i \left[\frac{(1-\alpha)}{(1+\tau_i^L)w} \right]^{1-\alpha} \left[\frac{\alpha}{(1+\tau_i^K)r} \right]^{\alpha} \right)^{\frac{1-\eta}{\eta}} \tag{45}$$

Use the exact expressions for optimal L_i and K_i from equations 27 and 29:

$$L_i = \frac{(1-\alpha)(1-\eta) P^{\frac{1}{\eta}} Q \left((1-\eta) A_i \left[\frac{(1-\alpha)}{(1+\tau_i^L)w} \right]^{1-\alpha} \left[\frac{\alpha}{(1+\tau_i^K)r} \right]^{\alpha} \right)^{\frac{1-\eta}{\eta}}}{(1+\tau_i^L)w} \tag{46}$$

$$K_i = \frac{\alpha(1-\eta) P^{\frac{1}{\eta}} Q \left((1-\eta) A_i \left[\frac{(1-\alpha)}{(1+\tau_i^L)w} \right]^{1-\alpha} \left[\frac{\alpha}{(1+\tau_i^K)r} \right]^{\alpha} \right)^{\frac{1-\eta}{\eta}}}{(1+\tau_i^K)r} \tag{47}$$

$$L = \sum L_i = (1-\alpha)(1-\eta) \sum \frac{1}{(1+\tau_i^L)w} P_i Q_i = \tag{48}$$

$$L = (1-\alpha)(1-\eta) PQ \sum \frac{1}{(1+\tau_i^L)w} \frac{P_i Q_i}{PQ} \tag{49}$$

$$L = (1-\alpha)(1-\eta) PQ \frac{1}{\overline{MRPL}} \tag{50}$$

Equivalently, the expression from the market clearing condition for aggregate capital is:

$$K = \alpha(1-\eta) PQ \frac{1}{\overline{MRPK}} \tag{51}$$

Let's define the aggregate TFP the following way:

$$TFP \equiv \frac{Q}{K^{\alpha} L^{1-\alpha}} \tag{52}$$

$$TFP = \frac{Q}{\left(\alpha(1-\eta)PQ\frac{1}{\overline{MRPK}}\right)^{\alpha}\left((1-\alpha)(1-\eta)PQ\frac{1}{\overline{MRPL}}\right)^{1-\alpha}} \tag{53}$$

$$TFP = \frac{\overline{TFPR}}{P} = \frac{1}{P(1-\eta)}\left(\frac{\overline{MRPK}}{\alpha}\right)^{\alpha}\left(\frac{\overline{MRPL}}{1-\alpha}\right)^{1-\alpha} \tag{54}$$

To get P, aggregate the expression 54

$$PQ = \sum_i P^{\frac{1}{\eta}}Q\left((1-\eta)A_i\left[\frac{(1-\alpha)}{(1+\tau_i^L)w}\right]^{1-\alpha}\left[\frac{\alpha}{(1+\tau_i^K)r}\right]^{\alpha}\right)^{\frac{1-\eta}{\eta}} = \tag{55}$$

$$PQ = P^{\frac{1}{\eta}}Q\left((1-\alpha)^{1-\alpha}\alpha^{\alpha}\right)^{\frac{1-\eta}{\eta}}\sum_i\left(\frac{(1-\eta)A_i}{\left((1+\tau_i^L)w\right)^{1-\alpha}\left((1+\tau_i^K)r\right)^{\alpha}}\right)^{\frac{1-\eta}{\eta}} \tag{56}$$

$$P^{\frac{\eta-1}{\eta}} = \left((1-\alpha)^{1-\alpha}\alpha^{\alpha}\right)^{\frac{1-\eta}{\eta}}\sum_i\left(\frac{A_i(1-\eta)}{(MRPL_i)^{1-\alpha}(1+\tau_i^K)^{\alpha}}\right)^{\frac{1-\eta}{\eta}} \tag{57}$$

$$P = \frac{1}{(1-\eta)}\left((1-\alpha)^{1-\alpha}\alpha^{\alpha}\right)^{-1}\left(\sum_i\left(\frac{A_i}{(MRPL_i)^{1-\alpha}(MRPK_i)^{\alpha}}\right)^{\frac{1-\eta}{\eta}}\right)^{\frac{\eta}{\eta-1}} \tag{58}$$

Plug 58 into 54.

$$TFP = \frac{1/(1-\eta)\left(\frac{\overline{MRPK}}{\alpha}\right)^{\alpha}\left(\frac{\overline{MRPL}}{1-\alpha}\right)^{1-\alpha}}{1/(1-\eta)\left((1-\alpha)^{1-\alpha}\alpha^{\alpha}\right)^{-1}\left(\sum_i\left(\frac{A_i}{(MRPL_i)^{1-\alpha}(MRPK_i)^{\alpha}}\right)^{\frac{1-\eta}{\eta}}\right)^{\frac{\eta}{\eta-1}}} \tag{59}$$

Aggregate TFP if you have decentralized allocation with wedges.

$$TFP = \left(\sum_i\left(A_i\left(\frac{\overline{MRPL}}{MRPL_i}\right)^{1-\alpha}\left(\frac{\overline{MRPK}}{MRPK_i}\right)^{\alpha}\right)^{\frac{1-\eta}{\eta}}\right)^{\frac{\eta}{1-\eta}} \tag{60}$$

Aggregate TFP if you have efficient allocation without wedges.

$$TFP^e = \left(\sum_i (A_i)^{\frac{1-\eta}{\eta}}\right)^{\frac{\eta}{1-\eta}} \tag{61}$$

Distance of aggregate TFP to the efficient (frontier)

$$\frac{TFP^e}{TFP} - 1 \tag{62}$$

7.6 Equalizing TFPR within groups

I also consider a separate counterfactual in which I look at two groups in each sector: for example, to-be-sanctioned and non-sanctioned, and redistribute existing labour and existing capital of each group across firms within each group to equalize their MRPL's and MRPK's (i.e. all firms within each group have the same average wedge).

Thus, I get two expressions of group MRPL and MRPK:

1)

$$\frac{(L_{sanc})^{\eta} \left(\frac{L_{sanc}}{K_{sanc}}\right)^{\alpha(1-\eta)}}{(1-\alpha)(1-\eta)PQ^{\eta}\left(\sum (A_i)^{\frac{1-\eta}{\eta}}\right)^{\eta}} = \frac{1}{MRPL_{sanc}} \tag{63}$$

2)

$$\frac{(K_{sanc})^{\eta} \left[\frac{K_{sanc}}{L_{sanc}}\right]^{(1-\alpha)(1-\eta)}}{\alpha(1-\eta)PQ^{\eta}\left(\sum (A_i)^{\frac{1-\eta}{\eta}}\right)^{\eta}} = \frac{1}{MRPK_{sanc}} \tag{64}$$

I then combine (1) and (2) to get an expression for group TFPR for to-be-sanctioned group (the expression for not-to-be-sanctioned TFPR is similar):

$$1/TFPR_{sanc} = \left[\frac{(K_{sanc})^{\eta} \left[\frac{K_{sanc}}{L_{sanc}}\right]^{(1-\alpha)(1-\eta)}}{\alpha(1-\eta)PQ^{\eta}\left(\sum (A_i)^{\frac{1-\eta}{\eta}}\right)^{\eta}}\right]^{\alpha} \left[\frac{(L_{sanc})^{\eta} \left(\frac{L_{sanc}}{K_{sanc}}\right)^{\alpha(1-\eta)}}{(1-\alpha)(1-\eta)PQ^{\eta}\left(\sum (A_i)^{\frac{1-\eta}{\eta}}\right)^{\eta}}\right]^{1-\alpha} = \tag{65}$$

$$= \frac{(K_{sanc})^{\alpha\eta} (L_{sanc})^{(1-\alpha)\eta}}{(1-\alpha)^{1-\alpha}\alpha^{\alpha}(1-\eta)PQ^{\eta}\left(\sum (A_i)^{\frac{1-\eta}{\eta}}\right)^{\eta}} \tag{66}$$

$$TFPR_{sanc} = \frac{\left(\sum \left(\frac{A_i}{\kappa}\right)^{\frac{1-\eta}{\eta}}\right)^{\eta}}{\left(K_{sanc}\right)^{\alpha\eta} \left(L_{sanc}\right)^{(1-\alpha)\eta}} \tag{67}$$

$$\kappa = \left(PQ^{\eta}\right)^{-\frac{1}{1-\eta}} \tag{68}$$

where kappa cancels out in the aggregate TFP expression.

Note that this means that the Industry-level output, and thus industry-level TFPR (and industry-level MRPL's and MRPK's) will increase because adjustments towards a more optimal allocation are made.

Aggregate TFP after efficiently allocating capital and labour across firms within groups.

$$TFP = \left(\sum_{o \in \{nsanc, sanc\}} \left(\frac{\overline{MRPL}}{MRPL_o}\right)^{1-\alpha} \left(\frac{\overline{MRPK}}{MRPK_o}\right)^{\alpha} \sum_{i \in o} \left(A_i\right)^{\frac{1-\eta}{\eta}}\right)^{\frac{\eta}{1-\eta}} \tag{69}$$

or, equivalently:

$$TFP = \left(\sum_{o \in \{priv, so\}} \left(\frac{\overline{TFPR}}{TFPR_o}\right) \sum_{i \in o} \left(A_i\right)^{\frac{1-\eta}{\eta}}\right)^{\frac{\eta}{1-\eta}} \tag{70}$$

7.7 Appendix D. Misallocation in Russia

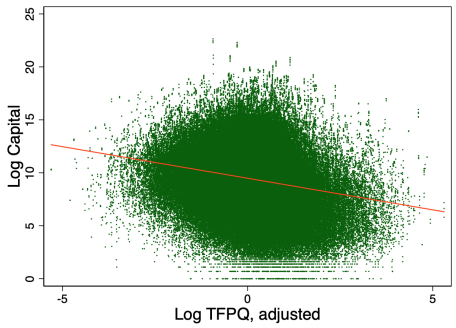
In Russia, I observe patterns that are consisten with strong misallocation of resources. Figure 2.A8 shows that more productive firms face higher wedges in both labour and capital and again confirms my findings. Firms that experience high productivity do not have a scope to grow because both capital and labour flows to less productive firms. These less productive firms could be the firms under state protection. Equally, higher distortions in more productive firms could also come from the market power of those productive firms, and export tariffs that prevent these firms' expansion into foreign markets. Overall, these patterns point at large institutional and economic frictions that prevent the flow of labour and capital resources to the most productive

firms. I break down the correlation of TFPR and TFPQ on that of MRPK and MRPL with physical productivity. I then show a correlation between capital or labor inputs and physical productivity. It is negative for capital. I also compare the misallocation in Russia to findings of Hsieh and Klenow in India and China and find that Russia is slightly further away from the efficient frontier than India and China.

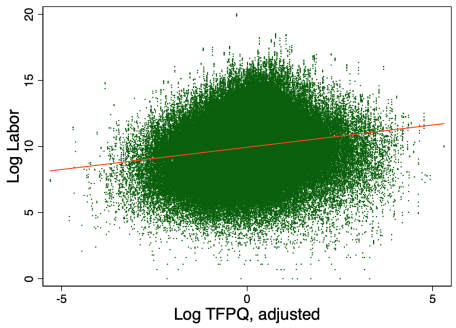
In part, this paper studies how much of this relationship is explained by the state taking away capital and labour from more productive private firms and giving it to less productive politically connected firms.

For descriptive purposes, I provide the summary statistics of misallocation in Russia using the measures of TFPR, MRPK, MRPL, and TFPQ as defined in the previous subsection. Figure 2.A8 the top two graphs demonstrate the overall distribution of capital and labour relative to the productivity of firms, the middle two graphs (c) and (d) show the capital and labour productivity on firm TFPQ, and the bottom graph (e) shows revenue productivity, TFPR on TFPQ. The measures of TFPQ, TFPR, capital and labour productivity are adjusted for measurement error. In an efficient economy, the slopes of the relationships between productivity and inputs are positive. In Russia, on the contrary, we see that at least capital to be lower on average in more productive firms. On the second row, where the efficient relationship between MRPL and MRPK and firm TFPQ should be flat if the marginal revenue of each input should be equalized across firms. Again, it is evident that more productive firms face larger positive wedges, this time in both capital and labour. Both relationships - between $TFPQ_i$ and $MRPK_i$, and between $TFPQ_i$ and $MRPL_i$ are positive, while in an efficient economy there should be no correlation between TFPQ and labour or capital productivity. Both capital and labour distortions to a firm can be summarised with a $TFPR_i$, the revenue productivity measure, defined in equation 31. The revenue productivity is correlated with physical productivity, as is MRPK and MRPL, whereas in an efficient economy it should be uncorrelated.

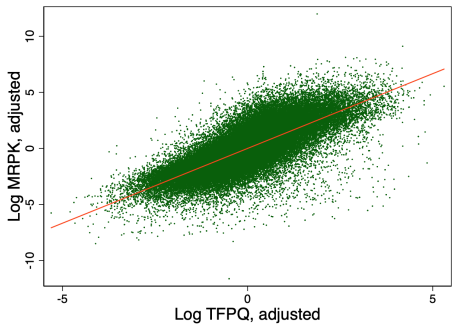
In addition, in Table 2.A15 I show comparable statistics to those reported in HK so that the key measures from the model can be cross-checked. Before adjusting for measurement error, I find that in Russia the dispersions of both TFPR and TFPQ are substantially larger than what HK find in China and India. HK report the p75-p25 variation in ln(TFPQ) of 1.28 and p90-p10 of 2.44 for China in 2005, while for India



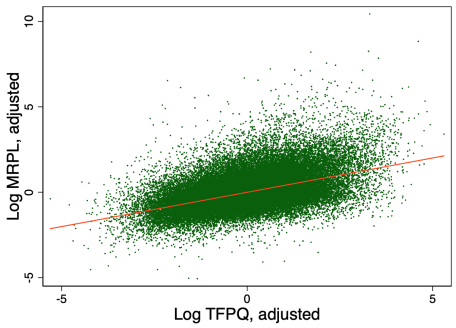
(a) Capital on TFPQ



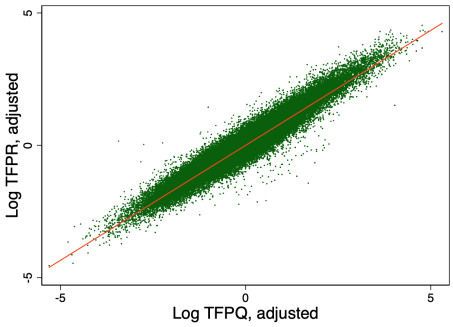
(b) Labor on TFPQ



(c) Capital productivity on TFPQ



(d) Labor productivity on TFPQ



(e) TFPR on TFPQ

Notes: Each observation (green dot) is a firm. Labour productivity (or $MRPL_i$) refers to value added per unit of wage bill and capital productivity (or $MRPK_i$) refers to value added per unit of capital, both of which are proportional to the marginal products of each factor in my framework. Raw TFPQ is calculated using the expression $TFPQ_i = \kappa \frac{(P_i Q_i)^{\frac{1}{1-\eta}}}{K_i^{\alpha} L_i^{1-\alpha}}$. $TFPR_i$, or revenue productivity, is a summary measure of distortions faced by each firm, with higher $TFPR_i$ implying higher distortions. The MRPK, MRPL, TFPR and TFPQ measures are adjusted for measurement error with firm and year fixed effects and de-meaned by 4-digit industry using the firm panel 2012-2018. The solid orange line is the line of best fit.

Figure 2.A6: Factor allocations by firm productivity

Variable	Statistic	Industry and Firm Fixed Effects	2018 Raw measures	Cross-section Average
		Panel A : Full dataset		
ln(TFPR)	SD	0.86	1.13	1.13
	p75-p25	0.91	1.17	1.16
	p90-p10	2.03	2.68	2.66
ln(MRPL)	SD	0.77	1.02	1.02
	p75-p25	0.65	0.89	0.89
	p90-p10	1.60	2.18	2.16
ln(MRPK)	SD	1.66	2.03	2.03
	p75-p25	1.93	2.41	2.39
	p90-p10	4.09	5.07	5.04
ln(TFPQ)	SD	0.94	1.50	1.49
	p75-p25	1.03	2.09	2.07
	p90-p10	2.24	3.86	3.84
		Panel B : Only the manufacturing sector		
Variable	Statistic	Industry and Firm Fixed Effects	2018 Raw measures	Cross-section Average
ln(TFPR)	SD	0.76	0.98	0.98
	p75-p25	0.82	1.00	0.99
	p90-p10	1.78	2.25	2.25
ln(MRPL)	SD	0.61	0.83	0.83
	p75-p25	0.53	0.71	0.71
	p90-p10	1.18	1.68	1.66
ln(MRPK)	SD	1.49	1.77	1.76
	p75-p25	1.72	1.98	1.97
	p90-p10	3.61	4.26	4.24
ln(TFPQ)	SD	0.83	1.34	1.32
	p75-p25	0.90	1.83	1.77
	p90-p10	1.95	3.40	3.35

Notes: For firm i, $TFPQ_i = \kappa \frac{(P_i Q_i)^{\frac{1}{1-\eta}}}{K_i^{\alpha} L_i^{1-\alpha}}$. Statistics are for deviations of log(TFPQ) from industry means. SD = standard deviation, p75 p25 is the difference between the 75th and 25th percentiles, and p90 p10 the 90th vs. 10th percentiles. Values in the column "Industry and Firm Fixed Effects" are adjusted for measurement error using firm and year fixed effects and de-meaned by 4-digit industry averages. Values in the column "2018 Raw measures" are the logs of raw measures of $TFPQ_i$, $MRPK_i$, $MRPL_i$, $TFPR_i$ for each firm, divided by the harmonic average of the same measure in the 4-digit industry. Values in the column "Cross-section Average" are the average of the statistics calculated as in the previous column, but the statistics are calculated for each cross-section of the panel 2012-2018 and then averaged across years. Panel A is calculated for the full sample of for-profit firms, and Panel B is calculated for the Manufacturing sector only.

Table 2.A15: Dispersion of ln(TFPR), ln(TFPQ), ln(MRPL), ln(MRPK)

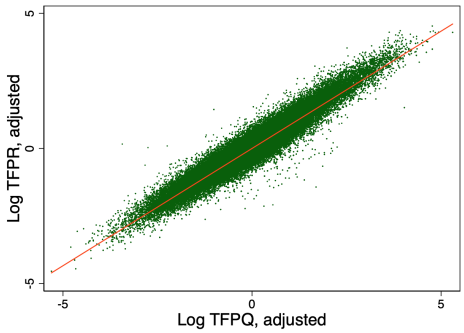


Figure 2.A7: TFPR on TFPQ

Notes: Each observation (green dot) is a firm. Raw TFPQ is calculated using the expression $TFPQ_i = \kappa \frac{(P_i Q_i)^{\frac{1}{1-\eta}}}{K_i^{\alpha} L_i^{1-\alpha}}$. $TFPR_i$, or revenue productivity, is a summary measure of distortions faced by each firm, with higher $TFPR_i$ implying higher distortions. The TFPR and TFPQ measures are adjusted for measurement error with firm and year fixed effects and de-meaned by 4-digit industry using the firm panel 2012-2014. The solid orange line is the line of best fit.

Figure 2.A8: Factor allocations by firm productivity

the corresponding values are 1.60 and 3.11. In Russia, without measurement error adjustment, the 2018 ln(TFPQ) variation is: p75-p25 is 2.14 and p90-p10 is 3.49.

Equally, ln(TFPR) variation in Russia is also larger: I find 1.18 and 2.71 in Russia in 2018 compared to 0.82 and 1.59 (China), 0.81 and 1.60 (India). However, Hsieh and Klenow only use the manufacturing sector, whereas my data include services and agriculture, and the diverse services sector can show much more variation in wedges and productivity.[44] Looking at Panel B, with only the manufacturing sector, the percentile variation in ln(TFPR) (1.00 and 2.25) and ln(TFPQ) (1.83 and 3.40) reduces but is still larger than in HK. Additionally, adjusting these measures for firm and year fixed effects further reduces the variation and gives the percentile variation of ln(TFPR) (0.82 and 1.78) and ln(TFPQ) (0.90 and 1.95) making the values on par or even smaller than numbers found in Hsieh and Klenow for India and China.

[44]The higher variation may also arise because of the way 4-digit industries are defined. As the country is transforming to the services economy, the level of detail may be much lower in the services sector, relative to manufacturing, so each 4-digit industry in the services sector may contain somewhat more diverse firms than a 4-digit industry in manufacturing

7.7.1 How does the pre-existing misallocation between sanctioned and non-sanctioned firms affect the aggregate TFP?

Using the Hsieh and Klenow framework I conduct two counterfactual exercises, which together give me how much of the distance to the efficient frontier is explained by the variation in wedges due to the treatment status.

To measure the contribution to overall misallocation from different groups (to-be-sanctioned versus not sanctioned), I first get the overall distance to the frontier, by equalizing all wedges (or TFPR) across firms within each four-digit industry, keeping total capital and labour fixed within industries, and comparing the aggregate TFP as measured in the data to this new efficient TFP (I call it TFP*). The distance of the aggregate TFP to the efficient (frontier) is a share:

$$\frac{TFP}{TFP^*} - 1 \tag{71}$$

This comparison will give a full distance to the efficient frontier from the current status quo in Russia. Second, I equalize wedges only within sanctioned-status-by-industry groups and compare the resulting TFP, call it TFPc, to the TFPe from the first exercise. The remaining distance to the frontier is attributed to the wedges *between* to-be-sanctioned and not sanctioned firms. I also repeat these exercises for the SOEs and private firms.

1) TFPe: Equalize all wedges within industries
2) TFPc: Equalize wedges within sanction status-industry groups

Measures	Count	TFP/TFP*	TFPc/TFP*	Gap explained by between-group wedge
To-be-sanctioned versus not	101,845	27%	73.9%	36%

Table 2.A16: Counterfactual exercises: to-be-sanctioned versus non-sanctioned

Table 2.A16 shows the results of the counterfactual exercises. The overall distance to the frontier of Russian TFP will slightly more than double if all wedges were equalized. The sanctioned vs non-sanctions wedge explains (100-73.9)/(100-27)=36% of the distance to the frontier. The wedge between to-be-sanctioned firms and not-

sanctioned firms explains a large share of the distance to the frontier and justifies further analysis.

7.8 Appendix E. Data appendix

I construct a dataset of sanctioned firms.

1) firm SDN sanctions+first-level subsidiaries (variable ”sdn”)
2) firm SSI sanctions +first-level subsidiaries (variable ”ssi”)
3) person SDN sanctions + directly owned firms (variable ”ind”)
4) EU sanctions, which mimic the US sanctions, be it SDN or SSI.

In most regressions, I take the union of variables (1)-(4) to create ”Any Sanction” variable, but I also create separate variables, SDN and SSI. For the SDN variable, I take the unions of the variables (1), (3) and part of (4) if the firms are ”blocked” by the EU. For the SSI variable, I take variable (2). There are only 9 firms that are sanctioned by the EU but not the US (some of them are subsidiaries). I have coded them as SDN as the EU treatment was to stop all transactions, and SSI if these were input sanctions.

I create separate treatment year variables for the SSI and SDN categories. However, even within categories, some firms have several treatment years, because they are sanctioned both by association with other sanctioned firms and directly. Priority of the first treatment year assignment for companies that fall into several sanction categories is the following:

1. the year of mother company’s treatment (if the company is majority-owned)
2. if (1) does not exist, the year of the company is explicitly listed on the Department of Treasury
3. if (1) and (2) years do not exist (happens if the company is minority-owned by multiple sanctioned firms, where the total shares from different companies add up to more than 50% with different sanctioned years), the assigned year is earliest among potential SDN years, ”individual SDN” sanction years for the SDN variable, and the earliest among the SSI owner company years, for the SSI variable.

I used the sanction announcement date to assign the year according to the April 30th split: if you get sanctioned after April 30th, your treatment year is the year after. These sanctions do not include sanctions that took place before 2014 and sanctions that are not to do with the Ukraine conflict. I also exclude firms that are in Crimea (around 40 firms), since they are embargoed based on their location in Crimea only, and not necessarily based on the connections to the current government.