CERGE
Center for Economics Research and Graduate Education
Charles University Prague

Essays on Political Distortions in Banking and the Real Economy

Mikhail Mamonov

Dissertation

Prague, October 2023

Mikhail Mamonov

Essays on Political Distortions in Banking and the Real Economy

Dissertation

Prague, October 2023

Dissertation Committee

CTIRAD SLAVIK (CERGE-EI; chair)

MAREK KAPICKA (CERGE-EI)

VERONIKA SELEZNEVA (CERGE-EI)

SERGEY SLOBODYAN (CERGE-EI)

Referees

THORSTEN BECK (European University Institute (EUI), Italy)

VOLKER NITSCH (Technical University Darmstadt, Germany)

To Anna and Margarita.

Table of Contents

Abstract

Politics brings numerous distortions into the work of the banking system and the real economy. This Thesis explores how political distortions affect bank lending decisions, impact the real decisions made by firms borrowing from "politically distorted" banks, and change the overall shape of the macroeconomy.

The first chapter studies the impacts of global financial sanctions on banks and their corporate borrowers in Russia at the micro level. Following Crimea's annexation by the Kremlin, the West consecutively imposed financial sanctions between 2014 and 2019, which allowed targeted (but not yet sanctioned) banks to adapt their international and domestic exposure in advance. We suggest a modification of a staggered difference-in-differences approach in which we explicitly estimate bank's in-advance adaptation to anticipated treatment and the value added of the realized treatment. We establish that the overall effect of sanctions consists of a combination of large negative anticipation effects (*intended*) and large positive added-value effects (*unintended*), which completely offset each other in our setting. Using the syndicated loan data, we find that the negative real effects of sanctions materialized only when sanctioned firms borrowed from sanctioned banks. When borrowing from unsanctioned banks, sanctioned firms actually increased employment and investment, though they still lost in terms of market sales.

The second chapter takes a macroeconomic perspective to examine the effects of sanctions on key aggregated indicators including GDP, consumption, and investment, and then explores cross-sectional variations of those effects in samples of firms and households. We introduce a novel approach to measuring sanctions shock—a *high-frequency identification* (HFI) based on US sanction announcements and daily data on Russia's US Dollar-denominated sovereign bonds. We then exploit my HFI-based sanctions news shock as an instrument in a VAR model of the Russian economy. We show that the sanction announcements in 2014–2015 were actually very potent: the underlying sanctions may have led to a GDP decline of 3.2%, not 0 to 1.5% as found in previous literature.

The third and fourth chapters analyze a "parallel" political distortion in Russia in the 2010s—a large-scale bank closure policy initiated by the Central Bank of Russia six months before the sanctions began, which resulted in the detection and closure of roughly 700 private banks by 2022. Using unique credit register data on the universe of loans in Russia, we investigate how firms were sorting between "good" and (not-yet-detected) "bad" banks following the closure of their prior banks, and how this sorting affected these firms' performance in terms of employment, investment, and market sales.

Abstrakt

Politika přináší řadu deformací do fungování bankovního systému a reálné ekonomiky. Tato dizertace zkoumá, jak politické deformace ovlivňují rozhodování bank o půjčkách, jaký mají dopad na skutečná rozhodnutí firem, které si půjčují od „politicky deformovaných“ bank, a jak mění celkový tvar makroekonomiky.

První kapitola studuje na mikroúrovni dopady globálních finančních sankcí na banky a jejich korporátní dlužníky v Rusku. Po anexi Krymu Kremlem Západ v letech 2014 až 2019 postupně uvalil finanční sankce, které umožnily cíleným (ale zatím nepostihnutým) bankám předem přizpůsobit své mezinárodní a domácí postavení. Navrhuji modifikaci přístupu „staggered difference-in-difference“, ve které explicitně odhaduji předběžnou adaptaci na očekávaný treatment a přidanou hodnotu realizovaného treatmentu. Zjišťuji, že celkový účinek sankcí se rovná kombinaci velkých negativních anticipačních efektů (záměrných) a velkých pozitivních efektů přidané hodnoty (nezáměrných), které se v mém prostředí zcela kompenzují. Pomocí údajů o syndikovaných půjčkách odhaluji, že negativní reálné účinky sankcí se projevily pouze tehdy, když si sankcionované firmy půjčovaly od sankcionovaných bank. Při půjčování si od nesankcionovaných bank sankcionované firmy dokonce získaly na zaměstnanosti a investicích, ale stále ztratily na tržních prodejích.

Druhá kapitola se makroekonomicky zaměřuje na dopady sankcí na klíčové agregované ukazatele, jako je HDP, spotřeba a investice, a poté zkoumá průřezové variace těchto vlivů ve vzorcích firem a domácností. Představuji nový přístup ke měření sankčního šoku - vysokofrekvenční identifikaci (HFI) - založenou na oznámeních amerických sankcí a denních datech o ruských státních dluhopisech denominovaných v amerických dolarech. Poté využívám svůj šok ze zpráv o sankcích založený na HFI jako instrument v modelu VAR ruské ekonomiky. Ukazuji, že oznámení o sankcích v letech 2014–2015 byla ve skutečnosti velmi účinná: základní sankce by mohly vést k poklesu HDP o 3,2 %, nikoli o 0 až 1,5 % jako v předchozí literatuře.

Třetí a čtvrtá kapitola analyzuje „paralelní“ politickou deformaci v Rusku v prvním desetiletí 20. století – politiku rozsáhlého zavírání bank, kterou zahájila Centrální banka Ruska půl roku před sankcemi, a vedla k odhalení a uzavření zhruba 700 soukromých bank do roku 2022. Pomocí unikátních údajů z úvěrového registru o databázi půjček v Rusku zkoumám, jak firmy rozlišovaly mezi „dobrými“ a (zatím nezjištěnými) „špatnými“ bankami po uzavření jejich současných bank, a jak toto rozlišování ovlivnilo výkonnost firem z hlediska zaměstnanosti, investic a tržního prodeje.

Acknowledgments

I am extremely thankful to my supervisor, Ctirad Slavik, and to Marek Kapicka for their joint support of my work over the last five years at CERGE-EI, for their constructive critiques, for pushing me to be more general, and for helping me to shape my research. Special thanks to Vasily Korovkin, Christian Ochsner, and Paolo Zacchia for fruitful discussions about my research and for sharing their ideas. I am also grateful to the CERGE-EI community for being supportive, helpful, and encouraging.

I am indebted to Steven Ongena who not only kindly hosted my research at the University of Zurich in 2021–2022 during times that were difficult for me, and who also showed me the beauty of empirical banking research that goes beyond the problems of the banking system per se. I am also indebted to Hans Degryse and Roman Goncharenko, who hosted me at KU Leuven in 2022–2023, thoroughly discussed my research, and helped me substantially during my job market stint. I also appreciate the discussion of my research with Atif Mian during my research stay at Princeton University in 2023.

In addition, I would like to thank the Academic Skills Center and personally Andrea Downing, Grayson Krueger, and Deborah Novakova for their invaluable help in the language editing of the Thesis.

Most importantly, I want to thank my partner and best friend Anna Pestova, my daughter Margarita, and my mother Elena Mamonova for their unconditional love and invaluable support.

Financial support from the NPO "Systemic Risk Institute" grant number LX22NPO5101, funded by European Union – Next Generation EU (Ministry of Education, Youth and Sports, NPO: EXCELES), and the ERC ADG 2016 – GA 740272 (lending) is gratefully acknowledged.

All remaining errors are my own.

Czech Republic, Prague　　　　Mikhail
October 2023

Introduction

"It is perfectly evident that, if we continue to live as we are now living, guided in our private lives, as well as in the life of separate States, by the sole desire of welfare for ourselves and for our State, and will, as we do now, think to ensure this welfare by violence, then, inevitably increasing the means of violence of one against the other and of State against State, we shall, first, keep subjecting ourselves to bankruptcy more and more, transferring the major portion of our productivity to armaments; and, secondly, by killing in mutual wars the best physically developed men, we must become more and more degenerate and morally depraved."

— Leo Tolstoy on the Russo–Japanese war in "Bethink Yourselves!" (1904), *Times* magazine

Politics brings numerous distortions into the work of the banking system and the real economy. By imposing financial sanctions, politicians may limit free cross-border flows of capital. By forcibly closing private banks, they can further deteriorate a free competitive environment in the domestic banking system, provoke an otherwise undesirable expansion of state-controlled banks, and induce capital misallocation. In each case, every political distortion, i.e., the "punishment," is a response to some "crime," e.g., one state launching a war against another(s) or state-controlled banks in autocratic political regimes squeezing private banks out of the market. This Thesis takes a closer look at these effects by appealing to the case of Russia, where many of them materialized over the past decade.

In the first chapter, co-authored with Anna Pestova and Steven Ongena, we study the impacts of global financial sanctions on banks and their corporate borrowers in Russia. Financial sanctions were imposed consecutively between 2014 and 2019, allowing targeted (but not-yet-sanctioned) banks to adapt their international and domestic exposure in advance. Using a staggered difference-in-differences approach with in-advance adaptation to anticipated treatment, we establish that targeted banks immediately reduced their foreign assets and actually *increased* their international borrowings after the first sanction announcement compared to other similar banks. We find that the added value of the next rounds of sanction announcements was rather limited. Despite considerable outflow of domestic private deposits, government support prevented disorderly bank failures and resulted in *credit reshuffling*: the banks contracted corporate lending by 4% of GDP and increased household lending by almost the same magnitude, which mostly offset the overall economic loss. Further, we introduce a two-stage *treatment diffusion* approach that flexibly addresses potential spillovers of the sanctions to private banks with political connections. Employing unique hand-collected board membership and bank location data, our approach shows that throughout this period, politically-connected banks were not all equally recognized as potential sanction targets. Finally, using syndicated loan data, we establish that the real negative effects of sanctions materialized only when sanctioned firms were borrowing from sanctioned banks. When borrowing from unsanctioned banks, sanctioned firms actually increased employment and investment, though they still lost in terms of market sales, pointing to a misallocation of government support.

In the second chapter, co-authored with Anna Pestova, we further ask how much sanctions harm the sanctioned economy. We examine the case of Russia, which has faced three major waves of international sanctions over the last decade (in 2014, 2017, and 2022). In a VAR model of the Russian economy, we first apply sign restrictions to isolate shocks to international credit supply, to proxy for the financial sanctions shocks. We provide a microeconomic foundation for the sign restriction approach by exploiting syndicated loan deals in Russia. We then explore the effects of the overall sanctions shocks (financial, trade, technological, etc.) by employing a high-frequency identification (HFI) approach. Our HFI is based on each OFAC/EU sanction announcement and the associated daily changes in the yield-to-maturity of Russia's US dollar-denominated sovereign bonds. Our macroeconomic estimates indicate that Russia's GDP may have lost no more than 0.8% due to the financial sanctions shock, and up to 3.2% due to the overall sanctions shock, cumulatively over the 2014–2015 period. In 2017, the respective effects are 0 and 0.5%, while in 2022, they are 8 and 12%. Our cross-sectional estimates show that the real income of richer households declined by 1.5–2.0% during the first year after the sanctions shock, whereas the real income of poorer households rose by 1.2% over the same period. Finally, we find that the real total revenue of large firms with high (low) TFPs declined by 2.2 (4.0)% during the first year after the sanctions shock, whereas the effects on small firms are close to zero. Overall, our results indicate heterogeneous effects of sanctions with richer households residing in large cities and larger firms with high TFPs being the most affected.

In the third chapter, co-authored with Roman Goncharenko, Steven Ongena, Svetlana Popova, and Natalia Turdyeva, we analyze how firms search for new lenders after a financial regulator forcibly closes their prior banks, and what happens to the firms' performance during this transition period. In 2013, the Central Bank of Russia launched a large-scale bank closure policy and began to detect fraudulent ("sin") banks and to revoke their licenses. By 2020, two-thirds of all operating banks had been shuttered. We analyze this unique period in history using credit register data. First, we establish that before sin bank closures, there was no informational leakage and the borrowing firms remained unaffected. After the closures, there is a clear sorting pattern: poorly-performing firms rushed to other (not-yet-detected) sin banks, while profitable firms transfered to financially solid banks. We find that the coupling of poorly-performing firms and not-yet-detected sin banks occurs more frequently when the two sin banks (the prior and the next lender) are commonly owned, or when the local banking market is unconcentrated. Finally, we show that during the transition period (i.e., after the sin bank closures and before matching to new banks), poorly-performing firms shrink in size and experience a sharp decline in borrowings and market sales, whereas profitable firms strengthen in terms of employment, investment, and market sales. A potential mechanism involves *credit risk underpricing* by sin banks: we find that poorly-performing firms (especially those that are commonly owned) received loans at lower interest rates than profitable firms prior to sin bank closures.

In the fourth chapter, I suggest a novel approach to measuring fraud in banking and to evaluating its cross-sectional and aggregate implications. I explore unique evidence of declining regulatory forbearance in the Russian banking system in the 2010s, when the central bank forcibly closed roughly two-thirds of all operating banks for fraudulent activities. I first introduce an empirical model of the regulatory decision rule that determines whether a regulator is likely to conduct an unscheduled on-site inspection of a suspicious bank in the near future. I estimate the model using unique data on asset losses hidden by commercial banks and discovered by the Central Bank of Russia during unscheduled on-site inspections across the two decades. I find that the average size of hidden asset losses detected by the rule equals 38% of the total assets of not-yet-closed fraudulent banks, and that the likelihood of fraud detection soared by a factor of 5 after 2013. With quarter-by-quarter predictions from the estimated rule, I form a "treatment" group of likely-to-be-inspected banks and then run a "fuzzy" difference-in-differences (FDID) regression to estimate the effects of the tightened regulation. FDID estimates show that likely-to-be-inspected banks substantially reduced credit to households and firms after the policy began in 2013, compared to similar untreated banks. Interpreting the FDID estimates of credit contraction as a credit supply shock and evaluating the macroeconomic implications of this shock using a VAR model of the Russian economy, I find that Russia's GDP could have been cumulatively 7.3% larger by the end of 2016 in the absence of the policy. This is the price the economy pays for reducing fraud in the banking system.

Chapter 1

Crime and punishment? How banks anticipate and propagate global financial sanctions

Co-authored with Anna Pestova (CERGE-EI) and Steven Ongena (University of Zurich, Swiss Finance Institute, KU Leuven, CEPR)

Published as CEPR Discussion paper No 16075 and CERGE-EI Working Paper Series No 753 and Swiss Finance Institute Research Paper No. 23-59

1.1 Introduction

Politics affects the banking sector in many ways (Calomiris and Haber 2014; Delis, Hasan, and Ongena 2020). For example, governments in various countries direct commercial bank lending to specific sectors (Cole 2009) or firms (Khwaja and Mian 2005; Claessens, Feijen, and Laeven 2008), stimulate lending to small and medium-sized enterprises and local politicians (Koetter and Popov 2020) and manipulate regulators' decisions on bank closures (Brown and Dinc 2005; Kang, Lowery, and Wardlaw 2015). During the recent COVID-19 pandemic, many governments created emergency loan guarantee schemes that were covering and spurring their banks' lending (Aizenman, Jinjarak, and Spiegel 2022; Jimenez et al. 2022). In this chapter, we turn to another recent and striking episode of political impact, i.e., the global financial sanctions on Russian banks and firms with close ties to their domestic government that commenced in 2014 and were sequentially imposed over a five-year period.[1]

Indeed, following the annexation of the Crimean peninsula by the Russian Federation in early 2014, the US and many other Western countries began imposing sanctions on major banks and non-financial firms linked to the Kremlin to curtail their international operations (Besedes, Goldbach, and Nitsch 2017; Ahn and Ludema 2020; Crozet et al. 2021). A very important but thus far neglected feature of this internationally coordinated restrictive policy was that the sanctions were not imposed all at once, i.e., on a full list of politically connected entities in Russia, but in contrast were phased-in over at least half a decade from 2014 to 2019, with different types of restrictive measures being *sequentially* imposed on various entities from the list.

This staggered implementation of the sanctions constitutes a very interesting and policy-relevant laboratory to analyze not only the immediate effects on the *already*-sanctioned banks but also on those banks that are *not yet* sanctioned but that seem targeted (because they are also politically connected) and may thus anticipate being sanctioned in the near future. The point is that such targeted banks have time to adapt their international operations before the actual sanctions materialize, and they may indeed want to do so in advance in a fear of asset freezes and to escape fire sales (Shleifer and Vishny 2011) once the sanctions arrive.[2] Therefore, the use of even the staggered difference-in-differences approach (Baker, Larcker, and Wang 2022) to gauge the effects of sanctions may end up in too conservative, downward-biased estimates due to ignored in-advance adaptations by not-yet-sanctioned banks. What makes the story even more interesting is that the domestic creditors of the targeted banks may also anticipate sanctions being imposed and, having observed the effect of the sanctions on already-sanctioned banks, these creditors may run on not-yet-sanctioned banks. The potentially targeted banks thus have to take such a run into account. Effectively, these possible runs can enlarge the total impact of sanctions without any intention from the side of sanctioning

[1]We are not able to formally explore the unprecedented sanctions against the Russian banks that were imposed in 2022 in response to Russia's war against Ukraine because the Central Bank of Russia had closed otherwise publicly available access to banks' balance sheet information. Interested readers are referred to Berner, Cecchetti, and Schoenholtz (2022) and Cipriani, Goldberg, and La Spada (2023) who deliver a comprehensive summary of these (and other) sanctions. Some preliminary estimates for the early war period are performed by Drott, Goldbach, and Nitsch (2022) who study the effects of banning Russian banks from SWIFT using daily transaction data from Bundesbank covering inter-bank deals between Russian and German banks.

[2]In the data section, we present many case studies documenting that different state-connected banks immediately started to adjust their international borrowings, including the issuance of Eurobonds, and foreign asset holdings after observing Crimea's annexation and the very first sanction announcement but before they themselves faced the international restrictions from the West.

countries. In these circumstances, we may naturally expect that the domestic government will step in to provide financial support to both already- and not-yet-sanctioned banks. These attempts, by contrast, can reduce the overall effect of sanctions.[3]

Henceforth, we refer to the immediate effects of sanctions on already-sanctioned banks as *direct* effects, and we call the in-advance adaptations of the potentially targeted but not-yet-sanctioned banks to anticipated sanctions *informational* effects. We thus ask how potent the informational effects are, compared to the direct effects, and what is the added value of the next sanction announcements conditional on the very first one. We then explore the potential of *treatment diffusion*: we ask whether the news on sanctions can force in-advance adaptation of not only targeted banks from the list but also those banks that are formally private (and thus non-targeted) but have various connections to the government. Finally, we ask how the sanctions propagate from the targeted banks to the balance sheets of borrowing firms which themselves can be sanctioned or not. Put differently, we are interested in what are the real effects of already-imposed and anticipated financial sanctions and how these depend on the government connections of the borrowing firms.

To identify sanctioned banks, we appeal to the official press releases of the US Office of Foreign Asset Control (OFAC) of the Department of the Treasury on the reasons and types of sanctions being imposed on particular entities. We also use the website of Risk Advisory (a leading global risk management consultancy) which provides an aggregated list of sanctioned banks, by sanction types and jurisdictions. From these two sources, one can infer that there are two major types of sanctions, i.e., those affecting only debt and those restricting both debt and assets. The former represents restrictions mainly on the placement of new debt in international markets, whereas the latter imposes restrictions on both new debt and foreign assets holdings of treated banks. Henceforth, for convenience, we label these two types of sanctions as "*debt*" and "*assets*" sanctions, respectively.[4]

By the end of the 2010s, the debt sanctions were imposed on 20 financial entities, including all state-owned banks (historically the largest banks in Russia, see Bircan and De Haas 2019) and their affiliates. The assets sanctions, in turn, were introduced against

[3]Nigmatulina (2022) shows that the *government-support channel* was active in the case of non-financial firms in Russia that were targeted by the sanctions. However, the support pouring through this channel led to capital misallocation. In the case of targeted banks, it is not clear *a-priori* whether the banks direct their support from the government to increase lending to sanctioned or non-sanctioned firms or to households, which face a much lower risk of being sanctioned, if any.

[4]According to the US Department of the Treasury, debt sanctions are called "*sectoral*" (SSI) while assets sanctions are titled "*entity*" (or blocking, SDN), see details in Section 1.2.1.

24 other politically influenced financial corporations (either owned by major oligarchs or operating in annexed Crimea). The difference in the size of the two sanctioned groups of banks is remarkable: on the eve of sanctions, the debt-sanctioned banks held a 50% share in the banking system's total assets while asset-sanctioned banks possessed less than 2%. In our analysis, we distinguish the direct and informational effects of debt and asset sanctions to measure the "price of being" either a state- or oligarch-owned financial firm in Russia.

To further reveal the political connections of banks, we create a novel database that contains manually collected *personal-level* data on each member of the board of directors and owners for every state-controlled bank and private bank in Russia. We extract this information from several sources, starting from the nationwide media source banki.ru which provides detailed information on the ownership and management structure of each bank operating in Russia in early 2020 (end of the sample period).[5] We then write a textual code that retrieves the CVs of a given bank's owners and directors from the Web to trace their political connections.[6] Our bank manager-owner database is annual and covers the period from 2013 to 2020.

Using detailed monthly balance sheets of Russian banks and applying an event-study approach, we begin by establishing that in response to the first sanction announcement in March 2014, targeted (but not yet *debt*-sanctioned) Russian banks increased, rather than decreased, their international borrowings over their total liabilities by 2.1 pp in a two-year horizon.[7] As a control group, we use similar never-sanctioned banks with no recognized political connections.[8] Against this background, the banks reduced their international assets over their total assets by 2.3 pp in response to the same first sanction announcement, on average and all else being equal. In contrast, during the same period, the targeted (but not yet *asset*-sanctioned) Russian banks decreased, as one would expect, their international borrowings by 2.4 pp of their total liabilities and likewise reduced their international assets by 2.4 pp of their total assets (all estimates are significant at 1%). These are the main estimates of *the in-advance adaptation effects* in an anticipation of

[5]We appeal to web-scrapping techniques to recover the content of banki.ru for previous years.

[6]We discuss all the necessary details in Section 1.4.

[7]This effect is equivalent to 1% of Russia's GDP and is thus large. Clearly, this is an unintended effect of the staggered implementation of sanctions. In Section 1.3.3, we provide a broad discussion on the supply- and demand-side factors behind this striking and unintended effect of sanctions.

[8]We employ the nearest-neighborhood matching estimator of Abadie and Imbens (2011) to construct the control group of banks in the period of 2012–2013, and we fix the composition of the control group throughout our analysis.

upcoming sanctions.[9]

To explore *the added value effects of the next sanction announcements*, we suggest separating the first and all the other such announcements to jointly estimate the informational and direct effects of sanctions in one staggered difference-in-differences equation. Using the same bank balance sheets at the monthly frequency as before, we find that (already) debt-sanctioned banks switched from increasing to significantly reducing their international liabilities, almost offsetting the previous rise, compared to the control group of banks. We also reveal that they did not, however, continue selling their foreign assets in response to the realized sanctions, which means that the banks could have fully adapted to the sanctions in advance. In turn, our estimates indicate that the (already) asset-sanctioned banks continued to significantly shrink their international borrowings, by up to 5 pp of their total liabilities in a three-year horizon, which largely exceeds the corresponding in-advance adaptation effect. However, we find that these banks actually slowed down the reduction of their foreign assets after the sanctions were realized. The estimated rebound in their foreign asset holdings could have reached up to +2.3 pp, compared to the control group. The latter finding implies that the banks could have been too pessimistic regarding the upcoming sanctions and could oversell their foreign asset holdings before being actually asset-sanctioned. We argue that if one would ignore the informational effects and estimate only the direct effects of sanctions, all the estimated effects would be misleadingly lower and mostly insignificant.

Having quantified the sanction-induced changes in the targeted banks' international operations, we then estimate the effects of sanctions on banks' *domestic* liabilities and assets. We show that not yet debt-sanctioned banks encountered a –2 pp outflow of private deposits during the two years after the first sanction announcement (large information effect). All targeted banks faced additional depositors' withdrawals once the sanctions had been imposed—up to –3 and –8 pp of the debt- and asset-sanctioned banks' total liabilities, during the three years after sanction announcements. The government then stepped in and, as our estimates reveal, fully supported the targeted banks, thus preventing their disorderly failure. This *government-support channel* made it possible for the targeted banks to avoid shrinking the overall size of their assets. Accordingly, we

[9] We also investigate how these average effects vary across Russia and reveal that they tend to diminish with a distance between a targeted bank's headquarter and Moscow, i.e., the center of political decision-making (the banks located near the Kremlin could have informational advantages over the banks in more distant cities in Russia). However, we also find that this diminishing effect does not work for those banks located in (even remote) oil-extracting regions.

establish a *credit reshuffling effect*: the targeted banks reduced the credit to firms by an equivalent of 4.0% of GDP but they raised the credit to households by 4.1% of GDP.[10] The reduction of corporate credit possibly reflects the anticipation of sanctions against Russian firms, which are more likely to appear in the Western governments' sanctions list than Russian households. The unintended effect of the sanction-driven expansion of private loans (mostly mortgages) is positive for households and may be responsible for improving their perception of the Kremlin's policies during the 2010s (Simonov and Rao 2022).

We further address the issue of treatment diffusion from targeted (i.e., state) banks to non-targeted (i.e., private) banks after the first sanction announcement. The treatment diffusion is likely to arise in our setting because as our analysis of banks' political connections shows, there are roughly 40 private banks with ties to the Russian government that remained unrecognized by Western countries. We introduce a two-stage treatment diffusion approach to understand how close was the behavior of non-targeted banks to those targeted by the sanctions. The first stage runs a loop of logit regressions at the monthly frequency and delivers the subjectively perceived probabilities of being sanctioned by each bank in the sample in the next few months depending on the share of state-connected persons in the board of managers or owners. The second stage enlarges the treatment group from the baseline analysis with politically-connected private banks whose predicted probabilities of being sanctioned exceeds a convenient threshold.[11] Our baseline estimates of the informational effects of the first sanction announcement become predictably lower when estimated with the two-stage treatment diffusion approach (by 46% on average), but survive statistically.

Finally, using DealScan's syndicated loan data and the borrowing firms' balance sheets at the annual frequency, we quantify *the real effects* of financial sanctions on the Russian economy. Our analysis here should be treated as only suggestive, because the number of loans in this database for Russia is relatively small—roughly 126 loans in a window of

[10]We aggregate the difference-in-differences (DID) estimates of the informational and direct effects of sanctions using a structural vector autoregressive (SVAR) model of the Russian economy. We use the narrative sign restriction approach of Antolin-Diaz and Rubio-Ramirez (2018) and apply the credit supply shock identification scheme of Gambetti and Musso (2017). The idea here is that sanctions could be treated as negative shocks to bank loan supply since banks may face binding borrowing constraints in international markets. Overall, our micro-level analysis delivers DID estimates of credit reduction caused by sanctions and our SVAR analysis then tracks its aggregated effect in terms of the reduction in GDP.

[11]As such, we set the unconditional probability of being sanctioned in the full sample, i.e., 2%.

2011–2017 centered around the first sanction announcement.[12] Given that the average number of Russian banks in those syndicates equals three, the total number of observations for our regression analysis here rises to roughly 335. At the loan level, we show that not-yet-sanctioned banks reduced the supply of credit to non-sanctioned firms by 20% and to not-yet-sanctioned firms by 92% (an almost complete stop) within three years of the first sanction announcement in March 2014. Given the loan supply reductions, our further difference-in-differences estimates at the firm-year level indicate that the not-yet-sanctioned firms that had relationships with not-yet-sanctioned banks experienced an average 44% decline in their real characteristics (employment, investment, sales, and others), as cumulatively over 2014–2017. The not-yet-sanctioned firms that *did not* have relationships with not-yet-sanctioned banks enjoyed, by contrast, rising employment and investment by an average of 41%, but their market sales declined by 16%. Though we do not test them directly, we attribute these findings to the government-support channel, through which it is possible to support employment and investment but not to force consumers and other firms to buy the output of sanctioned firms.

Our analysis of banks' adaptation to sanctions delivers several contributions to the literature. First, a substantial number of empirical studies in various fields of economics and finance evaluate the effects of staggered reforms by comparing early and late-treated entities with each other and with never-treated entities (Baker, Larcker, and Wang 2022; Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfoeuille 2020). Our results, however, clearly show that a bulk of the effect may come from an in-advance adaptation of not-yet-treated entities that share similar features with those (at least one) already treated. In-advance adaptation occurs due to anticipation of the treatment in the near future after the first policy announcement.[13] When we account for the in-advance adaptation after the first sanction announcement, the immediate effects of the 'reform' (sanctions, in our case) become much less significant, both statistically and economically. This means that previous studies may have

[12]Though they are gigantic covering roughly 30% of the total amount of the banking system's loans to firms in Russia.

[13]There are two related strands of the literature in this case. The first explores the adaptive behavior of economic agents in the presence of regulatory uncertainty. Gissler, Oldfather, and Ruffino (2016) show that US banks were reducing mortgage lending between 2011 and 2013 when the regulator (CFPB) was discussing the necessity to raise the debt-to-income cut-off rule to adapt to the forthcoming regulation in advance. The second strand investigates the role of information in the economy and how agents adapt to news (Jaimovich and Rebelo 2009; Blanchard, L'Huillier, and Lorenzoni 2013). By showing that targeted banks adjusted their operations in advance, i.e., after the first sanction announcement but before being actually sanctioned, we provide empirical evidence of the forward-looking behavior of economic agents in anticipation of sanctions.

over-estimated the immediate effects of reforms in their settings. A close study in this respect is D'Acunto, Weber, and Xie (2019) which shows that there is a significant peer-punishment effect on unpunished entities. However, in their setting, there is no staggered reform, and unpunished entities are not supposed to be on the list of potentially punished entities unless they break a particular rule (wrongdoing in loan guarantees to related parties). In our setting, not-yet-treated entities should be sanctioned anyway—not because they are doing something wrong, but just because they are connected with the (wrongdoing) government.

Second, a growing theoretical literature in econometrics addresses the issue of fuzzy treatment in quasi-experimental designs and claims that many empirical published in top-ranked journals neglects this issue (de Chaisemartin and D'Haultfoeuille 2017). We contribute to this field by suggesting a two-stage treatment diffusion approach that identifies those entities (private banks, in our case) that formally should not be treated (i.e., sanctioned), but that behave as though they are expecting the treatment. By measuring political connections at the personal level and showing that this matters for the final outcome of treatment, we contribute to the literature on the value of political connections (Fisman 2001; Brown and Dinc 2005; Khwaja and Mian 2005; Faccio 2006; Koetter and Popov 2020). We believe our two-stage approach can be applied in different settings such as tax evasion (Slemrod 2007; Artavanis, Morse, and Tsoutsoura 2016) and peer effects in schooling (Duflo, Dupas, and Kremer 2011), where spillovers from treated to (a part of) control objects are possible (Leung 2020).

Third, a growing strand of the literature studies the economic effects of sanctions, not only in Russia (Ahn and Ludema 2020; Crozet and Hinz 2020; Nigmatulina 2022; Keerati 2022; Mamonov and Pestova 2022) but in other sanctioned countries (Laudati and Pesaran 2021; Felbermayr et al. 2020; Etkes and Zimring 2015; Levy 1999), and for the sanctioning countries themselves (Belin and Hanousek 2021; Crozet et al. 2021; Efing, Goldbach, and Nitsch 2023). While the majority of existing research deals with trade sanctions and delivers analyses at the firm level, we show how the financial sanctions work: how they affect banks and how they propagate from the banks to their borrowers in the real sector of the economy. We establish not only the in-advance adaptation effects of the sanctions on not-yet-treated banks, but also the credit reshuffling effect, the diffusion of treatment on formally private banks with political connections, and the real effects of the financial sanctions against banks on borrowing firms' performance. For example, Ahn and Ludema (2020) show that trade sanctions caused a 33% slump in employment and a 25%

reduction of operating revenue of sanctioned non-financial firms in Russia (compared to similar non-sanctioned peers after 2014). Our results, in turn, highlight one of the channels through which these effects could materialize—binding borrowing constraints for firms due to credit reshuffling. In addition to the work of Nigmatulina (2022), who establishes the government-support channel at the level of sanctioned firms in Russia, our results provide evidence of the efficacy of this channel at the bank level. In our case, the channel operates through both directed government deposits and the Central Bank of Russia's loans pouring into the liability side of the targeted banks' balance sheets to substitute for losses in international borrowings and domestic household deposits after runs on the banks. Differently from Keerati (2022) who reasonably claims that "*the short interval between the initial move by the Kremlin to annex Crimea and the first round of sanctions by the United States offered little room for anticipatory reaction by Russian firms*," our event-study analysis delivers clear evidence on strong anticipation effects *after* the first sanction announcement in March 2014 and during the next two years.

Fourth, we add to the literature on market discipline by providing novel evidence on the nature of information vs. panic-based deposit withdrawals (Martinez Peria and Schmukler 2001; Iyer and Puri 2012; Karas, Pyle, and Schoors 2013; He and Manela 2016). We show that private depositors may begin to punish banks not because of their weak performance or myopic herding behavior, caused by negative news on some other banks in the system, but for their connection to the (wrongdoing) government.

From the policy perspective, our estimates imply that if the imposition of sanctions were not phased-in, the negative effect on Russia could have been much larger (than what we observed), which would be economically inefficient for a country with long-lasting recessions and high dependence on foreign financing. For Western countries, our results indicate that even despite the sequential imposition, the sanctions still had significant effects. The staggered implementation of the global sanction policy and the lack of the threat of secondary sanctions on the international partners of Russian banks and firms allowed for in-advance adaptation by not-yet-sanctioned banks and an opportunity for sanction evasion, which led to an overall small impact of sanctions on Russia in the 2010s.

We, nonetheless, must mention several limitations to generalizing our empirical findings and policy recommendations. First, Russia is a clear outlier in terms of sanctions due its enormous geographical longitude and the fact that it borders both the West, which imposes sanctions, and the East, which helps Russia evade those sanctions. Second, Russia is typically ranked very low in terms of cross-country measures of the quality of governance and legal environment.[14] It is not necessarily the case that countries being sanctioned in the future will be as poor in terms of corporate governance as Russia in this respect. Therefore, their counter-sanctions may be more effective than those imposed by Russia over the past decade.

The remainder of this chapter is structured as follows. Section 1.2 introduces the main stylized facts on the financial sanctions and targeted banks in Russia. Section 1.3 presents the main estimation results on the actually sanctioned banks, including the in-advance adaptation effects. Section 1.4 then enlarges the main results with an analysis of the treatment diffusion to formally private banks with political connections. Section 1.5 reports estimates of the real effects of the financial sanctions on borrowing firms. Section 1.6 concludes.

1.2 Banks and sanctions: stylized facts

1.2.1 U.S. OFAC and the targets of the financial sanctions across Russia

Differently from the apartheid-related sanctions on South Africa back in the 1980s (Levy 1999) or more recent cases of Iran in 2006/2012 (Laudati and Pesaran 2021) or Gaza in the late 2000s (Etkes and Zimring 2015), in 2014 the West had decided to pursue the strategy of "targeted" sanctions instead of a full embargo on Russia in response to the annexation of Crimea (Ahn and Ludema 2020).

The US Office of Foreign Assets Control (OFAC) administers economic sanctions, including those against Russia, and specifies the two sanction lists: Specially Designated Nationals (SDN) and Sectoral Sanctions Identifications (SSI). SDN implies the complete prohibition of economic relationships with certain individuals and their businesses, whereas SSI targets specific activities to be forbidden. In mid-2014, OFAC issued four directives shaping the SSI-prohibited activities.[15] We focus here only on Directive 1 which

[14] According to the World Bank, for instance, Russia has a percentile rank of only about 20 at global scale (ranging from 0 (lowest) to 100 (highest) rank) for governance indicators such as rule of law and control of corruption.

[15] All the technical details, including an overview of the sanctions policy, all the directives, and executive orders, can be found on the website of the US Department of the Treasury, see https://home.treasury.gov/policy-issues/financial-sanctions/sanctions-programs-and-country-information/ukraine-russia-related-sanctions. See also a special alert by ReedSmith devoted to sanctions at https://www.reedsmith.com/en/perspectives/2014/10/overview-of-the-us-and-eu-sanctions-on-russia.

explicitly targets the financial sector. Specifically, Directive 1 eliminates any opportunities for "*...engaging in transactions in, providing financing for, or otherwise dealing in new debt with a maturity of longer than 30 days, or equity for persons identified on the SSI List.*"

Focusing on the financial sector, the targeted sanctions of the 2010s were prohibiting state-owned or -controlled banks and non-financial firms in Russia from either placing new longer-term debts in Western financial markets (*debt* sanctions under the SSI list) or from any operations with the West, including buying stocks and equities, granting loans to foreign banks, firms, or individuals (*asset* sanctions, SDN).[16] Though not explicitly stated, the borderline between applying a debt- (less restrictive) or asset- (more restrictive) sanction lies in whether a targeted bank or firm operates in annexed Crimea and/or is owned or governed by persons the West deems to be responsible for the war in the east of Ukraine, or other offensive activities.[17]

With this information, we are ready to form the treatment group for our analysis. We collect the dates of sanction announcements, types of sanctions, and all other relevant bank-level information from the official OFAC website.[18] The resultant list of debt- and asset-sanctioned banks consists of 44 financial institutions (see Appendix 1.A). Among the 20 banks in the debt sublist we have (i) 4 different state-owned or -controlled commercial banks, which constitute the "big-4" of the Russian banking system (i.e., Sberbank, VTB, Gazprombank, and the Russian Agricultural Bank), (ii) 1 state-owned development bank (VEB), and (iii) 15 major subsidiaries of the "big-4" or VEB. Within this sublist, we have to exclude VEB and 3 subsidiaries because they do not disclose their balance sheets through the Central Bank of Russia's database.

[16]In contrast to the unprecedented 2022 sanctions on Russia for its full-blown war in Ukraine, the sanctions in the 2010s did not prohibit the targeted entities from, e.g., operations in foreign currencies domestically. There was no intention to impose oil and gas embargo/tariffs and take Russian banks off the SWIFT system. The Central Bank of Russia was not under the threat of its international assets being frozen. Because at the moment of writing, there is no bank-level information available for the analysis, we leave this new episode of sanctions for future research.

[17]For example, *Rossiya Bank*, a large privately-held bank owned by one of the richest oligarch families in Russia, the Kovalchiyk family, had operations in Crimea and was also known as "Putin's wallet" for its close ties to the Kremlin (see OFAC's press release on https://home.treasury.gov/news/press-releases/jl23331). Conversely, Sberbank, state-owned and the largest bank in Russia in terms of assets, did not operate in the occupied territories and was not governed by those who could direct the bank's funds to finance the Kremlin's foreign policy (see OFAC's press release on https://home.treasury.gov/news/press-releases/jl2629). Consequently, *Rossiya Bank* encountered asset sanctions while Sberbank faced only debt sanctions.

[18]In addition, we cross check the resultant list of sanctioned banks by other sources: specifically, we retrieve the lists of debt- and asset-sanctioned banks from the website of the international consulting company "Risk Advisory." https://www.riskadvisory.com/sanctions/russia-sanctions-list/.

Further, among the 24 banks in the asset sublist, we have (i) 12 banks operating in the Crimean peninsula, (ii) at least 2 banks controlled by the Rotenberg family (a rich oligarch family), and (iii) 10 banks controlled by either local governments or other state-owned entities. Within these 24 banks, we exclude 4 banks because the sanctions were eventually repealed for 2 of them and the other 2 did not disclose their balance sheets. In total, we have 36 banks in the treatment group for our empirical analysis.

The 44 banks targeted by the sanctions are distributed throughout Russia, with their headquarters being located in 9 (of more than 80) regions across Russia and 2 annexed regions within the Crimean peninsula (Figure 1.1.(a)). Notably, some of these headquarters are located in those regions that are characterized by the largest oil extraction intensities (e.g., Tyumen region, located 2.1 thousand km to the East of Moscow) while the headquarters of the largest targeted banks are located either in Moscow or Saint-Petersburg (0.7 thousand km to the North from Moscow), i.e., the regions that both have zero oil extraction intensities (Figure 1.1.(b)). We will use this fact as a source of heterogeneity in our empirical design below when exploring the effects of targeted banks' in-advance adaptation to upcoming sanctions.

1.2.2 Timing of the financial sanctions

The 44 targeted banks did not face debt- or asset-type financial sanctions all at once, but sequentially between 2014 and 2019. Within this five-year period, there were 12 sanctions announcements covering these banks: the first 8 announcements during the so-called 'first wave' of sanctions related to Crimea, and the last 4 in the 'second wave' punishing for Russia's support of the dictatorship regime in Syria and electoral interference in the 2016 U.S. presidential elections.[19] As Figure 1.2 reveals, the major state-owned or connected banks were sanctioned in the first wave in 2014–2015, whereas the second wave mostly dealt with the subsidiaries of these banks, which are much smaller in size. However, Sberbank—the largest bank in the system—was sanctioned only half a year after the first sanction announcement and some other large banks were sanctioned only in 2015, creating soil for in-advance adaptation of not-yet-treated banks to upcoming sanctions.

[19] Note that we are mostly focused on the US OFAC sanctions rather than European sanctions. This is because there were several episodes of miscoordination between the EU and the US in this respect. A major example is that Sberbank avoided the US sanctions until September 12, 2014, whereas the EU sanctioned it on July 31st. During this transitional period, nothing prevented Sberbank from borrowing from the US banks, thus reducing the effect of the European sanctions.

(a) Size of total assets and location of the targeted banks

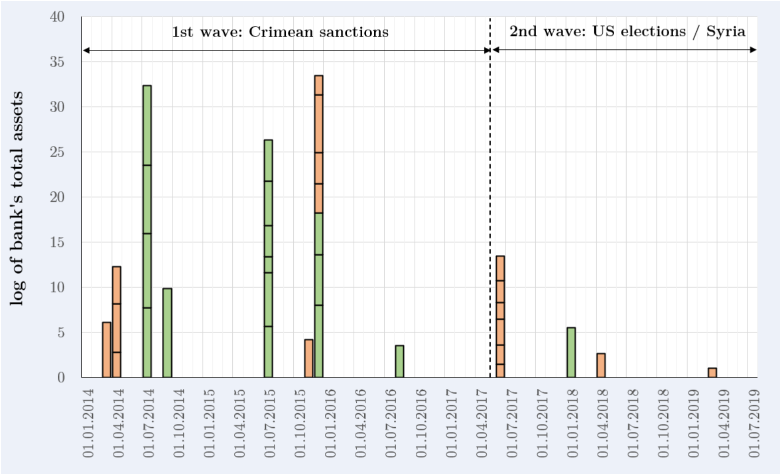
(b) Oil extraction intensities

Note: Subfigure (*a*) reports the locations of headquarters of the 44 banks targeted by either debt or asset sanctions and the size of these banks' total assets (in billion rubles, as of January 2014 on the eve of the first sanctions). Subfigure (*b*) reports regions' oil extraction intensities, as measured by thousand tons per year (in 2007).

Figure 1.1: Location of the headquarters of the targeted banks and regions' oil extraction intensities across Russia and annexed Crimea

1.2.3 Aggregated balance sheet of the sanctioned banks

We now illustrate how the aggregated balance sheets of the banks targeted by debt or asset sanctions changed over the (first) five years of the sanction policy. Figure 1.3.(a) shows that on the eve of the sanctions in January 2014, the 16 (not yet) debt-sanctioned banks relied intensively on international borrowings and held a sizeable portion of their



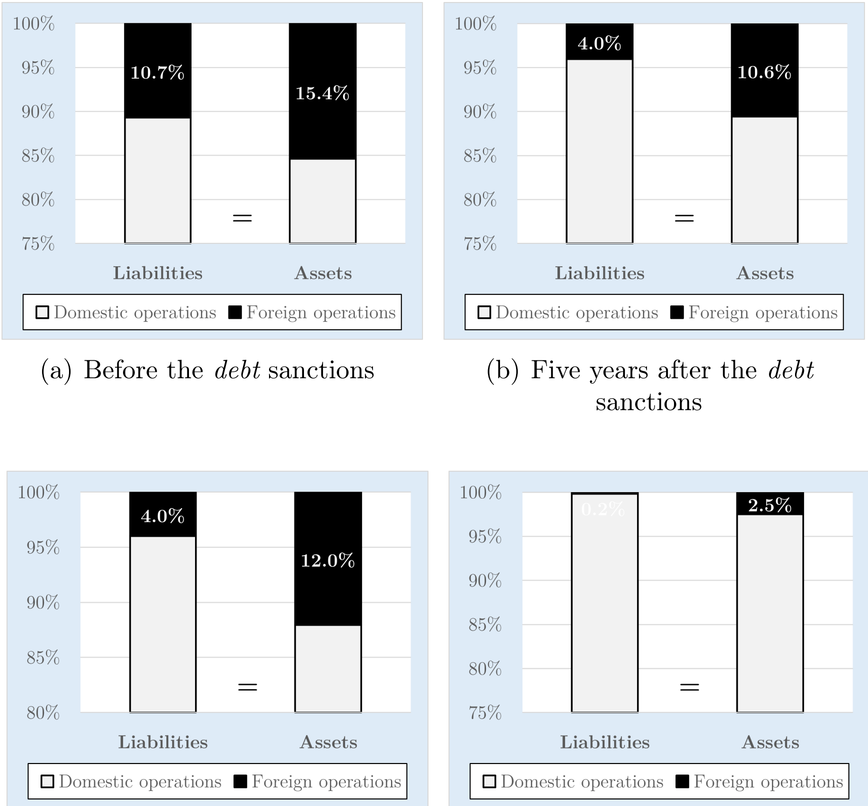
Note: The figure depicts the timeline of the sanction announcements and differences in the size of the targeted banks, as proxied with the log of banks' total assets. There were 12 announcements by the US OFAC/EU between 2014 and 2019. For each announcement, the figure plots a bar that stacks the sizes of each of the targeted banks covered by the announcement at the respective date: *pale red* depicts debt-type sanctions and *pale green* reflects asset-type sanctions. For example, the first sanction announcement in March 2014 affected only one bank, whereas the sanction announcement in December 2015 restricted 7 banks from the list. The sanctions are divided into two waves: the first responds to Russia's annexation of Crimea and the second to Russia's interference in the U.S. presidential elections, cyberattacks and support of the dictatorship in Syria.

Figure 1.2: Timing of financial sanctions and size of targeted banks

assets abroad: foreign liabilities constituted 11% of their total liabilities and foreign assets equaled 15% of their total assets. As Figure 1.3.(b) shows, five years later, i.e., in January 2019 when all major targeted banks had already been debt-sanctioned, these numbers dropped to 4% and 11%, respectively. The stock of foreign borrowings had therefore been reduced by much more than the stock of foreign assets, as the OFAC's design of debt sanctions assumes. However, during these five years, the Russian economy encountered a world oil price collapse (in 2014) and entered a local recession (in 2015). A formal analysis is thus needed to isolate the effects of sanctions.

As for the 20 asset-sanctioned banks, Figure 1.3.(c) shows that they were not very dependent on borrowings abroad even before the sanctions (4% share of their total liabilities) while they were investing intensively in foreign assets (12% share). However, as Figure 1.3.(d) illustrates, five years later, the asset-sanctioned banks had nearly 0% share of foreign liabilities and only a 2.5% share of foreign assets. Differently from the debt-sanctioned banks, the stock of foreign assets in this case had been reduced by much more than the stock of foreign liabilities, as the design of the OFAC's asset sanctions implies.

Note that even five years after the first sanctions had been imposed, the contributions



(a) Before the *debt* sanctions

(b) Five years after the *debt* sanctions

(c) Before the *asset* sanctions

(d) Five years after the *asset* sanctions

Note: The figure compares the states of the aggregated balance sheet of banks targeted by debt sanctions (*a, b*) or asset sanctions (*c,d*) in January 2014, i.e., on the eve of the first sanction announcement, and January 2019, five years after the bulk of sanctions had already been imposed. The debt-sanctioned group consists of 16 major state-owned and controlled banks and their subsidiaries across Russia. The asset-sanctioned group comprises 20 state-connected banks either owned by oligarch families with close ties to the Kremlin or operating in the annexed Crimea, or both.

Figure 1.3: Foreign asset holdings and international borrowings of the targeted banks: before and after the financial sanctions

of foreign operations to the sanctioned banks' aggregated balance sheets were still far from zero. This is because the banks were diversifying their international borrowings and foreign asset holdings geographically across the Western and Eastern parts of the world. Unfortunately, the balance sheet data does not contain this geographical information, and we thus have to keep in mind this limitation in our analysis. Any effect of the sanctions on foreign operations that we will find with this data will likely reflect a mix of the 'true' effect on the Western operations and 'confounding' effect on the Eastern operations that may or may not be reduced, depending on whether the Eastern partners of the Russian targeted banks fear the secondary sanctions from the West (Efing, Goldbach, and Nitsch 2023) and whether and how much their political preferences aligned with the Russian government (Kempf et al. 2022).[20]

Full schedules of the time evolution of the targeted banks' foreign operations are reported in Figure 1.1.(a) and Figure 1.1.(b) for the debt- and asset-sanctioned banks, respectively (See Appendix 1.B). At the group level, we can already clearly observe changing time trends around the imposition of the first sanction in March 2014 in almost all types of foreign operations. This evidence is in line with an in-advance adaptation of not-yet-sanctioned banks to the upcoming sanctions.

1.2.4 Case-studies: in-advance adaptation to sanctions?

Figure 1.4 plots the time evolution of international borrowings and foreign assets of selected banks that ended up either under debt or asset sanctions. For illustration purposes, we select the two largest and/or most interesting cases of banks from each debt- and asset-sanctioned group and investigate their behavior around the imposition of sanctions on the *Rossiya Bank* (vertical *red line*) and/or around the period they themselves faced sanctions (vertical *blue line*).[21] From the debt list we take Sberbank and GazPromBank (which, jointly with VTB (not shown), constitute the top-3 in terms of size); from the asset list—the *Rossiya Bank* itself and one more bank operating in the Crimean Peninsula.

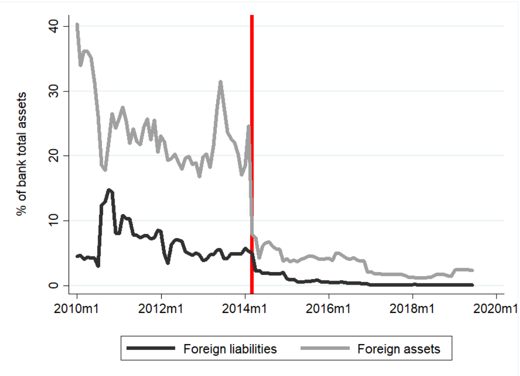
(Not yet) Asset-sanctioned banks. Assets sanctions imposed on the *Rossiya Bank* in March 2014 had an immediate negative effect: the bank dramatically decreased its foreign assets, by 17 pp of total assets (from 25 to 8%) within just one month, and reduced its foreign liabilities, by about 3 pp of total liabilities (from 5 to 2%, Figure 1.4.(a)). Until the end of the sample period in mid-2019, both positions remained at very low, if not zero, levels. This speaks to long run negative, and potent, effects of the first sanction announcement on its target.[22] Strikingly, the RNCB bank, one of the two other

[20] The strictly positive international operations of sanctioned banks in the presence of geographical diversification may indicate sanction evasion through third parties. This is consistent with the story of the German banks that evaded sanctions through their subsidiaries in 11 sanctioned countries over the last 20 years (Efing, Goldbach, and Nitsch 2023).

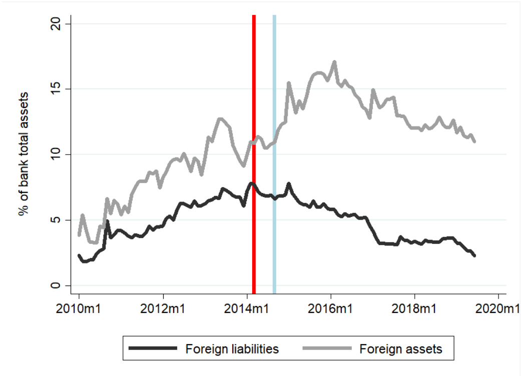
[21] Respective figures for the other sanctioned banks are available from the authors upon request.

[22] Before the sanctions, the *Rossiya Bank* had intensive international operations borrowing funds from financial markets and especially granting loans to foreign banks and foreign non-financial firms and investing in financial instruments. All these became minor after the sanctions in the long run. Another implication of sanctions is that Visa and Mastercard had blocked all credit/debit card operations of the bank's customers. The bank had lost its ability to carry out transactions in foreign currency. However, the Kremlin had fully, and even over-, compensated these restrictions to the bank by increasing direct injections of government deposits to the bank's balance sheet. The Kremlin had also replaced the "Alfa-bank" (the largest private bank in Russia, inside the top-10 banks in terms of assets, never facing sanctions before 2022) with the *Rossiya Bank* as an operator of the wholesale energy market in the country (with annual turnover equaled about 1.5% of GDP).

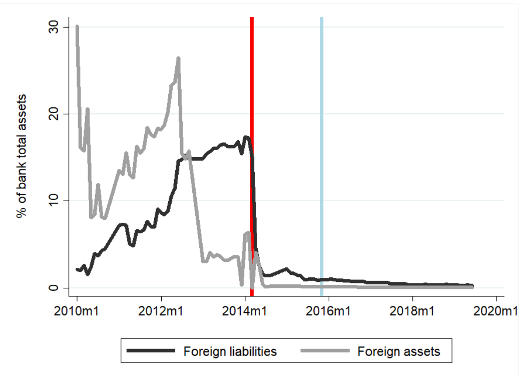
Assets sanctions	Debt sanctions



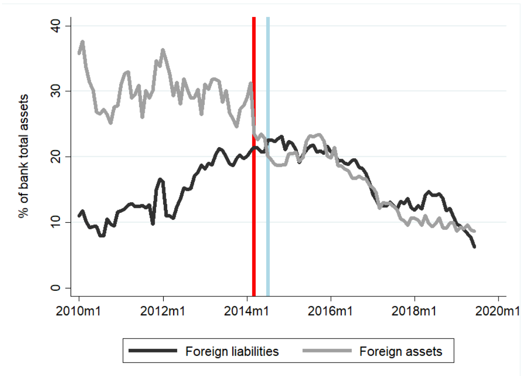
(a) The *Rossiya Bank* (*the first bank under sanctions*)



(b) Sberbank (top-1, state-owned)



(c) Russian National Commercial Bank (RNCB, operates in Crimea)



(d) Gazprombank (top-3, state-controlled)

Note: The subfigures report foreign liabilities (black line) and foreign assets (grey line), as % of total assets, of selected banks that faced sanctions. The red vertical red line marks March 2014—the month in which financial sanctions against Russian banks were imposed for the first time (the *Rossiya Bank*, SDN list). The Blue vertical line represents the period when individual sanctions were then introduced.

Figure 1.4: Selected largest Russian banks: Time evolution of foreign assets and liabilities before and after sanctions

selected asset-sanctioned banks, had also decreased its foreign assets dramatically right after the news on the *Rossiya Bank*—by about 15 pp of total assets, from 17% to 2% within two months of March 2014 (Figure 1.4.(c)). But the sanctions against RNCB were only imposed 20 months later, in December 2015. Similarly, RNCB turned to reduce its foreign liabilities well before December 2015—to near zero level.

(Not yet) Debt-sanctioned banks. As the raw data shows, before sanctions on the *Rossiya Bank* in March 2014, Sberbank was steadily increasing its international borrow-

ings (Figure 1.4.(b)). However, between March and September 2014, when it encountered sanctions, this trend stalled, and soon after September 2014, it began to decline. Notably, the peak level over the whole of 2010s was reached exactly in March 2014—about 7% in terms of total liabilities. By the end of the sample period, this figure fell to no more than 2.5%. Regarding Sberbank's foreign assets, we observe a largely positive trend that lasted from at least the beginning of the 2010s, i.e., long before the first sanctions in March 2014, until the beginning of 2016, i.e., more than a year after September 2014. Clearly, by design, debt sanctions do not target foreign assets. Finally, we observe that Gazprombank turned to decrease its foreign assets and foreign liabilities twice—first, after the news on the *Rossiya Bank* on March 2014, and second, after it faced sanctions in July 2014 (Figure 1.4.(d)).

Overall, these cases favor our view that not-yet-sanctioned banks turned to an in-advance adaptation of their international operations after the first sanction announcement. And we observe the same patterns for the other (not yet) sanctioned banks not described here for the sake of space.

Complementary evidence from the Eurobonds data. To partly overcome the drawback of the balance sheet data and zoom in on the targeted banks' foreign operations, we appeal to the https://cbonds.com/ data on bond issuance by Russian banks across the world. From this data, we learn that Russia's Big-4 state-owned banks (Sberbank, VTB, Gazprombank, and the Russian Agricultural Bank) successfully placed eight Eurobond issues between the end of February to July 2014, i.e., the period after Crimeaâ€™s annexation but before they were actually sanctioned. The banks borrowed 7.3 billion US Dollars at 4.4% (Table 1.1). Importantly, during the previous five months and during the analogous five months one year before they borrowed only 3.4 and 4.1 billion US Dollars, respectively, at 4.4% and 4.2%. Put differently, the banks borrowed two times more in 2014 but paid the same price as before 2014. It is thus clear that the banks were adapting their international liabilities in advance, i.e., until the opportunity window of cheaper borrowing is closed by the highly likely sanctions, while international investors were still ready to lend.[23] Note also that never-sanctioned banks that were active in international financial markets substantially reduced their demand for foreign borrowings through Eurobonds at the same time. As opposed to not-yet-sanctioned banks, nothing

[23]It has been already established in the literature that for the entities from emerging market economies, like Russia, it is cheaper to borrow abroad than borrow domestically due to rising interest rate differentials (Bruno and Shin 2017).

threatened these banks, and they were likely to simply follow the contracted aggregate demand caused by the world oil price collapse and a recession in the Russian economy.

Table 1.1: Eurobonds issuance by Russia's targeted and non-targeted banks around Crimea's annexation

	Not-yet-sanctioned banks		Never-sanctioned banks	
	Amount, bn USD	Interest rate, %	Amount, bn USD	Interest rate, %
After the Crimea's annexation: Feb.2014 to Jul.2014	7.3	4.4%	0.2	10.2%
Before the Crimea's annexation:				
— Oct.2013 to Feb.2014	3.4	4.4%	2.8	9.4%
— Feb.2013 to Jul.2013	3.4	4.4%	2.8	9.4%

Note: According to the cbonds.com data, the Big-4 state-owned banks—Sberbank, VTB, Gazprombank, and the Russian Agricultural Bank—issued 8 Eurobonds between the end of February to July 2014, i.e., the period after Crimea's annexation and before they were actually sanctioned (*not-yet-sanctioned banks*). As a comparison group, we consider all other banks—privately-held financial institutions—that issued Eurobonds within the same period (*Never sanctioned banks*).

1.3 The effects of sanctions on actually treated banks

1.3.1 Bank-level data

We use domestic sources of the bank-level data, namely the CBR's official database on monthly balance sheets and quarterly profit and loss accounts, which are publicly available from 2004 to the beginning of 2022. Specifically, we cull bank balance sheet information from the so-called Forms 101 and 102, respectively. We do not rely on international sources of bank-level data, i.e., the former Bureau Van Dijk's Bankscope and current Orbis database, because the domestic data we have access to covers almost all banks over the past two decades and is published in both monthly and quarterly formats.[24] Table 4.1 in Appendix 1.C reports descriptive statistics on the key bank operations that we explore in this chapter in a breakdown by sanction type–debt (SSI), asset (SDN), and unsanctioned—and by the origin of the operations—domestic or foreign.

[24]Our study is not the first to exploit domestic data on Russian banks. Among others, Karas, Pyle, and Schoors (2013) and Chernykh and Cole (2011) also analyze these data, in an application to market discipline during the crisis periods of 1998 and 2008, respectively.

1.3.2 Matching: constructing the control group of never-sanctioned banks

Methodology of matching

When the first sanction was imposed in March 2014 the Russian banking system comprised 956 banks. During the next five years, only 44 of them were sanctioned. Approximately 40 banks had state connections but had not been recognized by the West (see details in Section 1.4). This leaves us with nearly 850 banks as potential candidates to enter the control group for our empirical analysis. However, the sanctioned banks are predominantly very large entities whereas the potential candidates are mostly very small—in both the overall size of their total assets and in their cross-border operations. To overcome this issue we first note that if we simply divide all 850 potential candidates into large and small using a convenient threshold, e.g., a $200th$ position in the ranking of banks by their total assets, then the subsample of large banks will be much more comparable to the sanctioned banks than the subsample of small banks, at least in terms of cross-border operations (see respectively Figures 1.2.(a) and 1.2.(b) in Appendix 1.B and note how they compare to Figure 1.1). This implies that we can find appropriate *matches* for our sanctioned banks among the 200 largest never-sanctioned and not-state-connected banks (that is, truly private banks, either domestic or foreign).

Given the staggered implementation of sanctions, there are at least two ways to construct the matched sample of banks in our case: by finding matches on the pre-treatment period around the first date when sanctions materialized (March 2014) and by matching around each individual date of sanctions during 2014–2019. We argue that being owned or controlled by the government is exogenous to the date of sanctions only prior to the first sanction announcement—as, after March 2014, not-yet-treated banks could start anticipating sanctions against them because of their ties to the Kremlin, whereas domestic private and foreign-owned banks knew they were likely immune to sanctions. If a state-connected bank anticipates sanctions it may adapt in advance, and thus matching it with truly private banks around the individual date of sanctions rather than on the pre-March 2014 period is subject to a behavioral bias and is likely to end up violating the parallel trend assumption. We, therefore, want to ensure that we find matches only during the pre-March 2014 period when neither state nor private banks in the system could have known about the threat of sanctions.

Importantly, recent literature on staggered difference-in-differences design compares treated objects with not only never-treated ones but also, depending on the date of treatment, 'early-treated' and 'later-treated' counterparts (the so-called 'problematic' $\mathbf{2} \times \mathbf{2}$s, see Goodman-Bacon 2021 and Baker, Larcker, and Wang 2022, among others).[25] We argue that if in-advance adaptation works, then there is little sense, if any, in considering 'later-treated' banks as controls for 'early-treated' banks *before* the later treatment arrives—both are likely to behave similarly to each other after the earlier treatment hits (this eliminates one of the two 'problematic' $\mathbf{2} \times \mathbf{2}$s). However, we further argue that if there is any added value in each next sanction announcement, then this added value can be properly captured by exactly considering 'early-treated' banks as controls for the 'later-treated' banks *after* the later treatment is set (the second 'problematic' $\mathbf{2} \times \mathbf{2}$s). Yet, we also argue that this added value can be easily evaluated as a difference between the two effects: the effect of later sanction announcements with respect to never-sanctioned banks and the effect of earlier sanction announcements with respect to *the same* never-sanctioned banks. This implies that we can use the most simple control group—the (large) never-sanctioned, not state-connected banks—and escape unnecessary difficulties that arise from considering either 'later-sanctioned' or 'early-sanctioned' banks as controls to one another. We elaborate more on this issue in Section 1.3.5 where we formally introduce our version of the staggered difference-in-differences equation.

With these arguments at hand, let us now formalize our chosen approach to bank matching. Suppose that index b reflects a bank from the treatment group $\mathfrak{A}$ with the sanction date t_b, where $b = 1...S$ ($S = 44$) and $t_b \in [2014, 2019]$. Here, t_1 denotes March 2014, i.e., the first sanction date. For each b we need to find n matches among presumably large never-sanctioned, not state-connected banks $\mathfrak{B}$ at the common pre-treatment period $[t_1 - k, t_1)$, where $n = 1, 2...n^*$ and $k = 1, 2...k^*$. For choosing n^*, we follow the rule of thumb of Abadie and Imbens (2011) and set $n^* = 4$ in our baseline estimations.[26] In turn, for choosing k^*, we have no specific rule of thumb, except that it cannot be too large if we want to capture causal effects. For the baseline estimates, we set $k^* = 24$ months.[27] We also check more narrow and more wide windows to capture the peak effects and reveal

[25]The first 'problematic' $\mathbf{2} \times \mathbf{2}$ considers 'later-treated' objects as controls for 'early-treated' objects *before* the later treatment is imposed. The second 'problematic' $\mathbf{2} \times \mathbf{2}$, in turn, considers 'early-treated' objects as controls for 'later-treated' objects *after* the later treatment is imposed.

[26]Four matches were shown to be a good trade-off between preserving enough variance in the sample and decreasing the bias of the final estimates. Gropp et al. (2018) follow the same rule of thumb when constructing a matched sample of banks for their analysis.

[27]We repeated the matching exercise with 12 and 36 months as pre-treatment periods before March 2014. All our results remained qualitatively, and even quantitatively, very similar.

when the effects die out. We index matching banks with $j^{(n)}$, where $j = 1, 2...S \times n^*$.

We apply the bias-adjusted near-neighbor matching estimator of Abadie and Imbens (2011) to find matches to sanctioned banks during the pre-treatment period. Following Gropp et al. (2018), we employ the following bank-specific observables $\mathbf{X}_{b,t}$ in the matching procedure: bank size (measured by the log of total assets), equity capital, loans granted to the economy, deposits and accounts attracted from the economy, net income, and net interest income (all but size are as % of bank total assets). These measures reflect (i) bank asset structure, (ii) bank liability structure, (iii) size and capitalization, and (iv) profitability of interest-bearing and other assets. In addition, we include a non-performing loans ratio and cash and other reserves holdings (also as % of bank total assets) to control for (ex-post) credit and liquidity risk exposures. We need bias adjustment because the number of continuous covariates exceeds two. Finally, we control for time (month) fixed effects when running the matching estimator, to account for the differences in common shock exposures between different blocks of banks. By a block, we mean a sanctioned bank b with its n_b^* matches. Including time fixed effects is especially important in this light because we have time-varying periods of treatment imposition (Goodman-Bacon 2021).

Having run a 1:4 matching estimator, we obtain the matched control banks $\widetilde{\mathfrak{B}} \subset \mathfrak{B}$ and apply the Welch test on mean differences between the control and the not-yet-treated banks for each covariate on the pre-treatment period $[t_1 - k, t_1)$. Having ensured that the two groups are comparable before the first sanction announcement, we construct a binary indicator, which we use further in our DID framework to test the informational effect of sanctions:

$$SANCTION_b = \begin{cases} 1 \quad \text{in} \quad [t_1 - k, t_1 + k]\ , & \text{if } b \in \mathfrak{A} \\ 0 \quad \text{in} \quad [t_1 - k, t_1 + k]\ , & \text{if } b \in \widetilde{\mathfrak{B}} \\ ., & \text{if else or } t \notin [t_1 - k, t_1 + k] \end{cases} \tag{1.1}$$

where $[t_1 - k, t_1 + k]$ is a squeezed estimation window for our DID regressions (see below).

Matching estimation results

The Welch test results appear in Table 1.2. First, we note that the number of actually matched banks is less than the 1:4 matching procedure implied. This is because of repetitions: the same bank $b \in \widetilde{\mathfrak{B}}$ can be a match for more than one not-yet-treated bank $b \in \mathfrak{A}$. Second, the table indicates that, in terms of (i) equity capital to total assets ratio, (ii) attracting deposits from and granting loans to individuals and non-financial firms, (iii) net (interest) income, (iv) cash and reserves, and (v) non-performing loans our control and treatment groups are statistically identical at the pre-treatment period (two years before March 2014). Third, some differences remain in terms of the size of total assets when we compare not yet debt-sanctioned banks and their matches. This is because the former includes Sberbank, which is the largest bank in the system and is disproportionately larger than the other banks. It is therefore not possible to fully match the sizes of debt-sanctioned and non-sanctioned banks.

Table 1.2: Matching characteristics of banks at the pre-sanction level: Results of the two-sided Welch test

	Never-sanctioned banks		Not-yet-sanctioned banks		Difference
	N obs	Mean	N obs	Mean	
Panel 1: Not yet *debt* sanctioned banks vs. matched banks					
Log of total assets	37	4.2	16	5.6	–1.4**
Equity capital / total assets	37	13.7	16	12.1	1.6
Loans to individuals and firms / total assets	37	51.3	16	48.7	2.6
Deposits of individuals and firms / total assets	37	40.5	16	39.4	1.1
Net income (monthly) / total assets	37	0.10	16	0.04	0.06
Net interest income (monthly) / total assets	37	0.37	16	0.32	0.05
Cash & reserves / total assets	37	5.6	16	4.1	1.5
Non-performing loans / total assets	37	4.1	16	6.6	–2.5
Panel 2: Not yet *asset* sanctioned banks vs. matched banks					
Log of total assets	61	2.3	16	2.3	0.0
Equity capital / total assets	61	16.9	16	18.1	–1.2
Loans to individuals and firms / total assets	61	50.1	16	45.1	5.0
Deposits of individuals and firms / total assets	61	62.5	16	59.9	2.6
Net income (monthly) / total assets	61	0.10	16	0.12	–0.02
Net interest income (monthly) / total assets	61	0.37	16	0.34	0.03
Cash & reserves / total assets	61	8.2	16	9.1	–0.9
Non-performing loans / total assets	61	3.8	16	5.6	–1.8

Note: The table reports the results of the Welch test with unequal variances for comparisons of the mean values in treatment and control groups during the pre-treatment period (two years prior to March 2014). The control group is constructed using the bias-adjusted matching estimator of Abadie and Imbens (2011) with four matches.

***, **, * indicate that an estimated difference is significant at the 1%, 5%, and 10% levels, respectively.

1.3.3 In-advance adaptation to sanctions: how large, and when peaks?

The central question of our study is whether not-yet-treated banks adapted their international operations in advance. We begin exploring the in-advance adaptation phenomenon with an event-study approach at a monthly frequency. This allows us to (i) analyze whether there were confounding events during the pre-treatment period before March 2014 or not and (ii) reveal the peak magnitudes of the underlying in-advance adaptation effects and establish their timing.

Event-study approach and estimation results

Consider the following equation:

$$Y_{b,t} = \alpha_b + \sum_{k=-24,\, k\neq 0}^{k=24} \beta_k \cdot \Big(SANCTION_b \times \mathbf{1}_{\{t=k\}} \Big) + \sum_{k=-24,\, k\neq 0}^{k=24} \gamma_k \cdot \mathbf{1}_{\{t=k\}} + \psi' \mathbf{X}_{b,t} + \varepsilon_{b,t} \tag{1.2}$$

where $Y_{b,t}$ is the stock of either international borrowings or foreign assets, as % of total assets, of bank b at month $t \in [t_1 - 24, t_1 + 24]$. α_b is a bank's b fixed effect. $\mathbf{1}_{\{t=k\}}$ is a month t indicator variable. γ_k is a month k fixed effect that captures common macroeconomic shocks for all banks, e.g., declining oil prices and endogenous monetary tightening in the respective months of 2014. $\mathbf{X}_{b,t}$ are bank-specific variables aimed at eliminating any remaining differences between not-yet-treated and control banks in terms of market power, government support, and the role played at the domestic inter-bank market (net lender or net borrower).[28] However, the results survive if we drop $\mathbf{X}_{b,t}$. $\varepsilon_{b,t}$ is the regression error.

We run Equation (1.2) separately for debt-sanctioned banks (with their matches) and

[28]Clearly, (not yet) debt-sanctioned banks contain the largest banks in the system, which possess (and likely exploit) the most *market power.* We control for that by including (effective) interest rates, as proxied by the ratios of (i) interest expenses on private deposits in total private deposits and (ii) interest income on loans to households and non-financial firms over respective loans. *Government support* is also important, and we will investigate this issue in greater detail in Section 1.3.6, because, after the first sanction announcement (or even before) the Russian government could start injecting its supportive funds to the liability side of (not yet) sanctioned banks in the forms of direct deposits from the government or loans from the Central Bank of Russia. Such support likely affects the targeted banks' (i) willingness to search for substitution of Western foreign borrowings on Eastern financial markets and (ii) ability to invest in foreign assets in Eastern jurisdictions to substitute for Western assets. Also *domestic inter-bank market* matters for similar reasons. It is possible that the largest targeted banks obtain disproportionately more government funds and then re-distribute them to other banks using domestic facilities.

for asset-sanctioned banks (with their respective matches) to account for the differences in the design of sanctions. To address the concern of serial correlation (Bertrand, Duflo, and Mullainathan 2004), we cluster standard errors at the level of sanctioned banks, thus allowing for correlation across these banks. We also experiment with clustering within the two sanction types (debt vs. assets), thus allowing correlation within each of the types.

We can formalize the first portion of our hypotheses as follows:

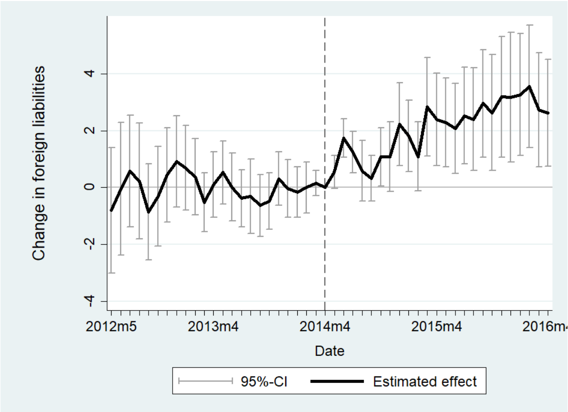
Hypothesis H1 "*No anticipation of punishment before the crime*": $\beta_k = 0$ for $k < 0$, i.e., there is no (statistical) difference between the to-be-treated banks and their control peers in terms of international operations before the Annexation of Crimea.

Hypothesis H2 "*Anticipating a punishment after the crime*": $\beta_k \neq 0$ for $k > 0$, i.e., having observed the very first sanction announcement not-yet-treated banks update their subjectively perceived probability of being sanctioned and begin to adapt their international operations, as compared to their matched control banks, in advance of the next sanction announcements.

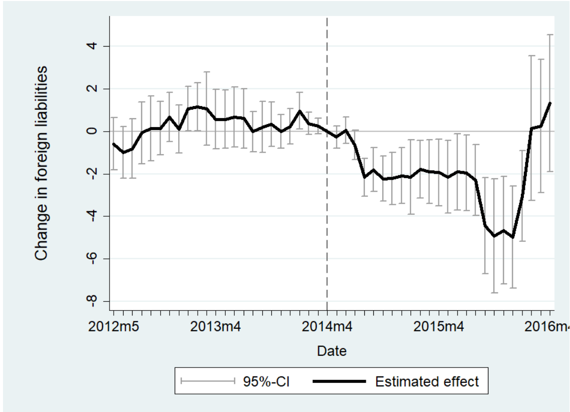
Hypothesis H3 "*Heterogeneous in-advance adaptation*": $\beta^{DS}_{k>0} \neq \beta^{AS}_{k>0}$, i.e., the banks that anticipate debt sanctions (DS) may adapt their international operations differently from the banks that likely face asset sanctions (AS). How differently depends on whether we consider international borrowings or foreign assets. For example, it is clear by definition that anticipation of AS may force banks to sell foreign assets in advance, whereas anticipation of DS may not affect the banks' foreign assets (two different sides of the balance sheet are affected).

Estimation results of Equation (1.2) are reported in the four subfigures of Figure 1.5. Subfigures (a) and (b) contain the estimates of β_k coefficients ($k = -24, 23, ..., 24$) along with their corresponding 95% confidence bands for the international borrowings of not yet debt- and not yet assets-sanctioned banks, respectively. Subfigures (c) and (d) do the same for the foreign assets of the same banks.

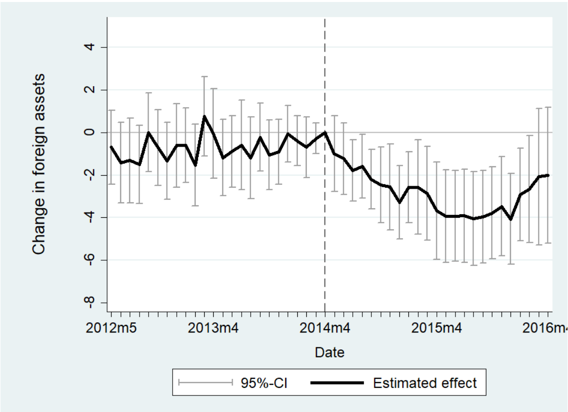
Adaptation of international borrowings (as % of total liabilities). Let us begin with the notion of pre-trends. For both not yet debt- and not yet asset-sanctioned banks, we obtain insignificant estimates of the β_k coefficients before the very first sanction announcement in March 2014, i.e., when k runs from -24 (March 2012) to 0 (February 2014). This eliminates the concerns that there could be significant differences between not-yet-treated and control banks in terms of their international borrowings before Crimea's annexation



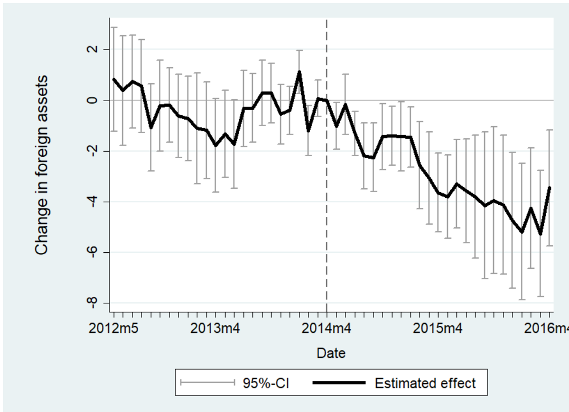
(a) Not yet debt-sanctioned banks: Foreign liabilities, pp of total liabilities



(b) Not yet asset-sanctioned banks: Foreign liabilities, pp of total liabilities



(c) Not yet debt-sanctioned banks: Foreign assets,
pp of total assets



(d) Not yet asset-sanctioned banks: Foreign assets,
pp of total assets

Note: The figures report the monthly estimates of the coefficient on $TREAT_b \times \mathbf{1}_{\{t=k\}}$ ($k = -24, -23, ..., 24$), as implied by the event-study equation (1.2).

Figure 1.5: The event-study estimates of the anticipation effects of sanctions, *by sanction type*

and favors the parallel trend assumption.[29]

Further, after March 2014, when k runs from 1 to 24 (March 2016), the β_k coefficients turn significant for both groups of banks. Strikingly, for the not yet *debt*-sanctioned banks, the estimates are all positive, and not negative (as one could expect), rising in time, and

[29]Ex-ante, one could fairly anticipate that the parallel trend assumption does not necessarily hold. This is because of the political crisis that occurred in Ukraine in late 2013 (the so-called "Euromaidan" crisis, see, e.g., https://www.bbc.com/news/world-europe-30131108). The Kremlin could have already started to plan Crimea's annexation then and, anticipating a response from the West in the form of sanctions, the Russian government could instruct the major state-owned banks to adapt their international operations—for example, to sell the foreign assets located in the West. However, this is not the case: our results indicate an absence of any pre-trends in the data.

reaching their peak of +3.8 pp closer to the end of the estimation window (Figure 1.5.(*a*)). This means that the not yet debt-sanctioned banks were actually raising, not reducing, their international borrowings after the *Rossiya Bank* was sanctioned, and before they faced bans on placing new debts on Western financial markets (SSI). Conversely, for the not yet *asset*-sanctioned banks, the β_k estimates are significantly negative after March 2014, declining in time, reaching their peak of -5.0 pp at larger ks, but then quickly attenuating towards zero at the end of the estimation window in March 2016 (Figure 1.5.(*b*)). This implies that these banks were reducing their international borrowings after the first sanction announcement and before they faced fully-blocking restrictions (SDN).

Interpreting the obtained results, we conclude that rising international borrowings by not yet debt-sanctioned banks is an unintended positive effect of the Western sanction policy, because Russia enjoyed a greater inflow of foreign funds after the sanction regime had been adopted. We argue that this unintended effect arises from the staggered implementation of the policy, its design distinguishing debt and asset restrictions, and a favorable combination of supply and demand factors.

From the *demand* side, we know that borrowing abroad is on average cheaper than borrowing domestically for the entities operating in EMEs (Bruno and Shin 2017).[30] Under the increased political uncertainty, for the borrowing banks it may be very important to maintain their reputation and repay their debts according to the schedule to be able to restore access to new borrowings once the political conflict is resolved. In addition, not yet debt-sanctioned banks could perceive additional international borrowings to be a cushion against potential panic runs that could have been launched by domestic depositors, especially households, after the imposition of sanctions (we explore such effects in Section 1.3.6).

From the *supply* side, foreign (Western) investors were willing to lend to not yet debt-sanctioned banks, and it is highly unlikely that they were unaware of upcoming sanctions against the largest of Russia's state-owned banks.[31] This, in turn, implies that foreign investors did not perceive sanctions to be an obstacle to the borrowing state-owned banks repaying their debts regularly and were convinced enough that the banks will continue

[30] Recall that the largest not yet debt-sanctioned banks substantially increased their borrowings through, e.g., placing Eurobonds during the five months after Crimea's annexation and before they were actually sanctioned in July 2014 (see Section 1.2.4).

[31] Recall again from our Eurobonds data example in Section 1.2.4 that foreign investors did not require a mark-up for potential sanctions to the coupon rate when they were buying the debt of not yet debt-sanctioned banks.

repaying even after they are sanctioned.[32]

Differently from not yet debt-sanctioned banks, we argue that if a bank anticipates asset-type sanctions for, e.g., operating in Crimea or being owned by an oligarch family tied to the Kremlin, then the bank may no longer need new international borrowings. The bank likely realizes that it could be difficult, if not impossible (depending on the threat of secondary sanctions), to invest these new funds in foreign assets.

Adaptation of foreign assets (as % of total assets). Let us again begin with the notion of pre-trends. As in the previous cases, we obtain insignificant estimates of the β_k coefficients for all ks before March 2014, indicating an absence of pre-trends in foreign asset holdings of not yet debt- and not yet asset-sanctioned banks. For not yet *debt*-sanctioned banks, we further obtain negative and significant β_k after March 2014, with a peak of -4.0 pp that has been reached in a year (Figure 1.5.(c)).[33] Apparently, the reason why not yet debt-sanctioned banks could have facilitated sales of their foreign assets is that the banks' foreign assets exceeded their foreign liabilities by up to 5 pp on the eve of the first sanction announcement. Losing an opportunity for new borrowings, the banks could have attempted to rebalance the currency structure of their assets and liabilities (a natural hedge against the risk of large fluctuation in the ruble's exchange rate).

For not yet *asset*-sanctioned banks, we also obtain negative estimates of β_k after March 2014, peaking at -5.5 pp by the end of the estimation period in March 2016 (Figure 1.5.(d)). This is expected and implies that the banks anticipated asset sanctions in advance and reduced their positions (for fear of potential asset freezes).

Summary. Not yet debt-sanctioned banks unexpectedly increased their international borrowings ("*borrow while you can*") but turned to reducing their foreign assets after the first sanction announcement in March 2014 and before they faced sanctions. Not yet asset-sanctioned banks, by contrast, began to shrink both international borrowings and foreign

[32] Recall again that back in the 2010s, as distinctly from the sanction policy in the 2020s, the sanctioned banks in Russia were not banned from the SWIFT system and were still allowed to conduct operations with major Western currencies.

[33] Media reports provide numerous supporting evidence to the finding on the preemptive reduction of foreign assets by Russian banks after the first sanction announcement. First, Gazprombank, the third largest state-owned bank in Russia (was eventually sanctioned in July 2014), reduced the stocks of its term-deposits and corresponding accounts in Western banks by 5 and 1.5 billion USD within 10 days of the "Bank Rossiya" being sanctioned on 20 March 2014, as reported by the national media-holding RBC, see https://www.rbc.ru/economics/25/04/2014/57041bc39a794761c0ce950b. Second, as reported by the Russian branch of Forbes magazine on 7 April 2014, major Russian banks turned to re-directing their deposits from Western to Asian banks, see https://www.forbes.ru/finansy/253865-obkhodnoi-manevr-rossiiskie-banki-begut-ot-sanktsii-v-kitai.

asset holdings ("*sell until your assets are frozen*"). Taken separately, these findings favor the **H2** hypothesis on the existence of in-advance adaptation, and if taken together—also the **H3** hypothesis on the heterogeneous effects of in-advance adaptation. The parallel trend assumption holds in all cases, thus supporting the **H1** hypothesis on the absence of confounders before the treatment. Thus, we find that the in-advance adaptation effects of sanctions on not-yet-sanctioned banks clearly exist, possess meaningful magnitudes, and may therefore matter for the overall assessment of the sanctions policy.[34]

1.3.4 In-advance adaptation to sanctions: the role of banks' geographical locations and regions' oil extraction intensities

Difference-in-differences approach

We now examine the cross-sectional heterogeneity of the in-advance adaptation effects of sanctions. For this purpose, we use a conventional difference-in-differences approach.

Consider the following equation:

$$\begin{aligned} Y_{b,t} = \alpha_b + \gamma_t + \beta_1 \cdot \Big(SANCTION_b \times POST.FIRST_t\Big) + \delta \cdot POST.FIRST_t \\ + \psi' \mathbf{X}_{b,t} + \varepsilon_{b,t}, \quad \text{if} \quad t \in [t_1 - k, t_1 + k] \end{aligned} \tag{1.3}$$

where all notations follow the previous section, γ_t is month FE, t_1 is March 2014, $k = 24$, and the indicator variable separating the timeline on 'before' and 'after' the first sanction announcement is

$$POST.FIRST_t = \begin{cases} 1, & \text{if } b \in \mathfrak{A} \cup \mathfrak{B} \text{ and } t \in [t_1, t_1 + k] \\ 0, & \text{if } b \in \mathfrak{A} \cup \mathfrak{B} \text{ and } t \in [t_1 - k, t_1) \\ ., & \text{if else} \end{cases} \tag{1.4}$$

Hypotheses **H2–H3** from the previous section apply. Our next hypothesis is that the targeted (but not-yet-sanctioned) banks located in Moscow could have an informational advantage over the banks located in remote regions due to their closer proximity to the center of political decision-making. Therefore, the degree of the in-advance adaptation

[34] As a robustness check, we re-run the event-study analysis without Sberbank, VTB, Gazprombank, or the Russian Agricultural Bank because they are disproportionately large and it is difficult to find exact matches for them. All the results remain valid.

effect must depend on the *geographical distance* between the headquarter of a potentially targeted bank and Moscow. To formalize this hypothesis, we extend Equation (1.3) by adding the triple interaction with the distance to Moscow variable, $Distance_b$ (in thousand km), and all necessary sub-products of the three (not shown to preserve space):

$$\begin{aligned} Y_{b,t} = \alpha_b + \gamma_t + \beta_1\Big(SANCTION_b \times POST.FIRST_t\Big) \\ + \beta_2\Big(SANCTION_b \times POST.FIRST_t \times Distance_b\Big) \\ + \psi'\mathbf{X}_{b,t} + \varepsilon_{b,t}, \quad \text{if} \quad t \in [t_1 - k, t_1 + k] \end{aligned} \tag{1.5}$$

To construct the $Distance_b$ variable, we collect exact addresses with their zip codes of the Kremlin and the headquarters of each bank in our sample and then apply a geocoder.[35] The hypothesis is:

Hypothesis H4 "*Distance from the political center*": $|\beta_1 + \beta_2 \cdot Distance_b| < |\beta_1|$, i.e., β_2 weakens the average β_1 effect, and the more so the larger the distance between a targeted bank's b headquarter and Moscow where the core political decisions are made.

However, some of the remote regions in Russia may still be very important to the Russian government. Recall from Section 1.2.1 that the Tyumen region located in Siberia is a leader in oil extraction across all regions in Russia and that some state-connected banks are located there. These banks may provide financial services to the oil companies located in the same region, and from this standpoint be perceived by the Russian government as important as those state banks that are located in Moscow. If so, then the weakening effect of the distance from Moscow should be offset. To test this hypothesis, we include a quadruple interaction with a region's r oil extraction intensity, as measured by million tons in 2007:[36]

$$Y_{b,t} = \alpha_b + \gamma_t + \beta_1\Big(SANCTION_b \times POST.FIRST_t\Big) \tag{1.6}$$

[35]See https://geopy.readthedocs.io/en/stable/. Here, we face an obstacle in that the Central Bank of Russia discloses names, registration numbers, and addresses of only those banks that are registered within the Russian Federation at the current date but not before it (see https://www.cbr.ru/banking_sector/credit/FullCoList/, in Russian). We, however, need these addresses back to 2011, i.e., at least three years before the first portion of sanctions was imposed. To overcome this issue, we exploit an Internet archive that allows one to browse the history of any website for a chosen date (in days, see archive.org). With this tool, we gather the necessary data from snapshots of the CBR's website at a monthly frequency from 2011 till 2020.

[36]The annual data on regions' oil extraction intensities shows that these intensities do not vary much across the years. For our purposes, it is enough to employ the regional data for a single year. We choose 2007 because it was the last year when such data was disclosed by the Federal State Statistics Service of the Russian Federation in its regional statistics database, see https://www.gks.ru/bgd/regl/b08_13/IssWWW.exe/Stg/d3/13-27.htm.

$$
\begin{aligned}
&+\beta_2\Big(SANCTION_b \times POST.FIRST_t \times Distance_b\Big)\\
&+\beta_3\Big(SANCTION_b \times POST.FIRST_t \times Distance_b \times \ln Oil_{r(b)}\Big)\\
&+\psi'\mathbf{X}_{b,t} + \varepsilon_{b,t}, \quad \text{if} \quad t \in [t_1 - k, t_1 + k]
\end{aligned}
$$

where $Oil_{r(b)}$ is oil extraction intensity in the region r where a bank b—not-yet-treated or its matched control peer—has its headquarters. For computational reasons, the value of the $\ln Oil_{r(b)}$ variable is set to zero for non-oil-extraction regions. The underlying hypothesis reads as follows:

Hypothesis H5 "*Oil extraction intensity*": $|\beta_2 + \beta_3 \cdot \ln Oil_{r(b)}| < |\beta_2|$, i.e., β_3 weakens the average β_2 effect, and the more so for regions r with larger oil extraction intensities.

Baseline estimation results

Table 1.3 contains the difference-in-differences estimation results on the in-advance adaptation of not-yet-treated banks to impending sanctions. The first three columns report the results for not yet *debt*-sanctioned banks and the last three columns for not yet *asset*-sanctioned banks. Panel 1 describes by rows how the banks adapted their international borrowings, and Panel 2 does the same for the foreign assets of these banks.

Not yet debt-sanctioned banks. Column 1 of Table 1.3 reports the estimation results of equation (1.3). We obtain a positive and highly statistically significant estimate of the coefficient on the $SANCTION_b \times POST.FIRST_t$ variable in Panel 1 and a negative and also highly statistically significant estimate in Panel 2. Quantitatively, the estimates imply that within two years of the first sanction announcement in March 2014, not yet debt-sanctioned banks raised their international borrowings by 2.1 pp and reduced their foreign assets by 2.3 pp of total assets, compared to similar never-sanctioned banks. Note that the magnitudes of these estimates are averages of respective event-study estimates from the previous section. This just confirms the H2 hypothesis.

Column 2 of Table 1.3 contains the estimation results of equation (1.5). We obtain negative but insignificant estimates of the coefficients on the $SANCTION_b \times POST.FIRST_t \times Distance_b$ variable in both Panels 1 and 2, meaning that on average, the in-advance adaptation effects revealed in the previous column do not depend on how far from the political center a not yet debt-sanctioned bank is located. This evidence does not support the H4 hypothesis. However, as we discussed above, it could be the case that not all distant-from-Moscow regions are equally important for the federal government.

Table 1.3: In-advance adaptation to sanctions, distance to Moscow, and oil extraction intensity:
Difference-in-differences estimates on the matched samples of banks

Sanction type:	Not yet debt-sanctioned			Not yet asset-sanctioned		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = Foreign liabilities, as % of bank total liabilities						
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	2.138***	2.297***	2.637***	–2.354***	–2.723***	–2.944***
	(0.649)	(0.659)	(0.722)	(0.634)	(0.709)	(0.815)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b$		–0.039	–1.280**		0.305	–0.776
		(0.423)	(0.517)		(0.302)	(0.537)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b \times \ln OIL_{r(b)}$			0.126**			0.292**
			(0.057)			(0.115)
N obs	2,241	2,241	2,241	3,148	3,148	3,148
N treated / control banks	14 / 35	14 / 35	14 / 35	16 / 59	16 / 59	16 / 59
R^2_{within}	0.620	0.622	0.626	0.457	0.458	0.465
Mean distance (km): treated / control		284/904			929/1,183	
Mean oil extrac. (mln tons): treated / control			20/10			0.7/10
Panel 2: Dependent variable = Foreign assets, as % of bank total assets						
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	–2.306***	–2.158***	–2.080***	–2.384***	–2.114**	–2.703**
	(0.516)	(0.624)	(0.719)	(0.786)	(0.923)	(1.030)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b$		–0.291	–0.541		–0.195	–0.829*
		(0.440)	(0.619)		(0.366)	(0.429)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b \times \ln OIL_{r(b)}$			0.029			0.056
			(0.072)			(0.089)
N obs	2,241	2,241	2,241	3,105	3,105	3,105
N treated / control banks	14 / 35	14 / 35	14 / 35	16 / 59	16 / 59	16 / 59
R^2_{within}	0.636	0.637	0.637	0.249	0.250	0.261
Mean distance (km): treated / control		284/904			929/1,183	
Mean oil extrac. (mln tons): treated / control			20/10			0.7/10

Note: The table reports the DID estimates of the effects of sanctions on foreign liabilities (*Panel 1*) and foreign assets (*Panel 2*) of Russia's targeted banks, as implied by equation (1.3). The estimation Window is $k = 24$ months around the imposition of sanctions on the *Rossiya Bank* (March 2014). $SANCTION_b = 1$ if a bank b will ever face sanctions within our sample period. $POST.FIRST_t = 1$ after March 2014 and is aimed at capturing the in-advance adaptation effect. Sanctioned (i.e., treated) and never-sanctioned (i.e., control) banks are 1:4 matched within two years before March 2014. Private banks with political connections are not allowed to enter the control group. Bank FE, Month FE, Bank controls, and all necessary cross-products of the SANCTION, POST.FIRST, DISTANCE, and OIL variables are included but not reported.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the sanctioned group level and the level of each non-sanctioned bank and appear in brackets under the estimated coefficients.

Indeed, when we distinguish the Russian regions by their oil extraction intensities in column 3, we achieve a more intriguing result when we run equation (1.6). For international borrowings (Panel 1), we again obtain a negative estimate on the triple interaction variable, but now it appears to be significant (at 5%) and much stronger than in column 2. Note that the estimate on the main double interaction variable almost does not change. Strikingly, we also obtain a positive and significant (at 5%) estimate of the coefficient on the $SANCTION_b \times POST.FIRST_t \times Distance_b \times \ln Oil_{r(b)}$ variable. Jointly, these two estimates indicate that not yet debt-sanctioned banks were raising their international borrowings by less if located in the regions farther from Moscow, as Hypothesis H4 implies, but only if these regions were not oil-extracting, as Hypothesis H5 states. For illustration purposes, let us compare the total in-advance adaptation effect for the not yet debt-sanctioned banks located in the Tyumen region (champion in oil extraction, located 2.12 thousand km on the East from Moscow) and Chelyabinsk region (roughly zero oil extraction, located 1.82 thousand km also on the East from Moscow). In the first case, the total effect equals +3.3 pp, whereas in the second case the total effect is just +0.3 pp.[37] For foreign assets (Panel 2), we still get insignificant estimates of the coefficients on both the triple and quadruple interaction variables, meaning the same not yet debt-sanctioned banks were unlikely to sell more foreign assets in advance if located in remote regions—even if these regions were specializing in oil extraction.

Not yet asset-sanctioned banks. Column 4 of the table reports the estimation results of equation (1.3). We obtain a negative and highly statistically significant estimate of the coefficient on the $SANCTION_b \times POST.FIRST_t$ variable in Panel 1, where the dependent variable is international borrowings (as % of total liabilities), and we also obtain a negative and highly statistically significant estimate in Panel 2, where the dependent variable is switched to foreign assets (as % of total assets). Quantitatively, the estimates imply that, within two years after the first sanction announcement in March 2014, not yet asset-sanctioned banks reduced their international borrowings by 2.4 pp and decreased their foreign assets by virtually the same 2.4 pp as compared to similar never-sanctioned banks. Note that the magnitudes of these estimates are also averages of respective event-study estimates from the previous section. The H2 hypothesis is thus confirmed.

Column 5 of the table contains the estimation results of equation (1.5). As it was true in column 2 for international borrowings, we again obtain insignificant estimates of the coefficients on the $SANCTION_b \times POST.FIRST_t \times Distance_b$ variable in both Panels 1 and 2. This indicates that, on average, the in-advance adaptation effects that we found in

[37]The computations are $2.637 - 1.280 \cdot 2.12 + 0.126 \cdot 2.12 \cdot \ln(323814) = 3.313$ and $2.637 - 1.280 \cdot 1.82 + 0.126 \cdot 1.82 \cdot 0 = 0.307$, respectively.

the previous column are not influenced by the distance between not yet asset-sanctioned banks and the political center. We thus cannot support the H4 hypothesis.

Finally, in Column 6 of the table, we report the estimation results of equation (1.6). In Panel 1, we still obtain insignificant estimates on the triple interaction with the distance variable, but we also obtain a positive and significant (at 5%) estimate of the coefficient on the $SANCTION_b \times POST.FIRST_t \times Distance_b \times \ln Oil_{r(b)}$ variable. Similarly to not yet debt-sanctioned banks, these results indicate that the not yet asset-sanctioned banks were reducing by less, or even increasing, their international borrowings if located in an oil extracting region and despite being remote from the political center. In our two-region example above, we obtain that a not yet asset-sanctioned bank from the Tyumen region (champion in oil extraction) would even turn to raise its international borrowings after the first sanction announcement—by 3.3 pp, whereas the same bank from the Chelyabinsk region (non-oil-extracting area) would be reducing its international borrowings by 4.4 pp.[38] Therefore, location in an oil extracting region where a bank can enjoy servicing oil exporting operations plays so much important role that can even change the sign of the average effect.The H4 hypothesis is rejected, whereas the H5 hypothesis is not.

In Panel 2 where we switch to the foreign assets as a dependent variable, we obtain a negative and marginally significant coefficient on the triple interaction term but an insignificant estimate for the quadruple interaction term. This means that not yet asset-sanctioned banks were tending to sell their foreign assets after the first sanction announcement more if located farther from the political center and irrespective of whether their regions extract oil or not. Fear of asset freezes can thus be perceived as a function of the distance between the headquarter of a not yet asset-sanctioned bank and the Kremlin. For our two-region example above, the banks located in the Tyumen and Chelyabinsk regions (approximately 2 thousand km from Moscow) are likely to reduce their foreign assets by roughly 4.4 pp of their total assets.[39] Differently from the case of international borrowings, we conclude that the H4 hypothesis is not rejected, whereas the H5 hypothesis is.

As an additional exercise, we run the same regression analysis by first shrinking and then enlarging the estimation window compared to our baseline choice (i.e., ±24 months around March 2014). Table 1.1 reports the estimation results of equation (1.6) with

[38]The computations are $-2.944 - 0.776 \cdot 2.12 + 0.292 \cdot 2.12 \cdot \ln(323814) = 3.265$ and $-2.94 - 0.7760 \cdot 1.82 + 0.292 \cdot 1.82 \cdot 0 = -4.356$, respectively.

[39]The computation is $-2.703 - 0.829 \cdot 2 = -4.361$.

the $[t_1 - k, t_1 + k]$ estimation window with $k = 12, 24$ *(baseline)*, 36 months and $t_1 =$ *March 2014* (see Appendix 1.D). We can observe that the effects discussed in this section reach their peaks under the $k = 24$ case. This also corresponds to the fact that most of the largest banks were sanctioned within 2 years after March 2014 (recall Figure 1.2 with the timing of the sanction impositions).

As another additional exercise, we disaggregate total international borrowings by maturity (below 1 year (*short-run*), between 1 and 3 years (*medium-run*), and above 3 years (*long-run*) and show that not yet debt-sanctioned banks were raising exactly the long-run borrowings after the first sanction announcement, but less so if located farther from Moscow, see Columns 1–3 of Table 1.2 in Appendix 1.D. Not yet asset-sanctioned banks were instead reducing their international borrowings across all maturities, and even more so if located farther from Moscow, see columns 4–6 of the table.

1.3.5 Further sanction announcements: any added value?

Staggered difference-in-differences with in-advance adaptation

Having explored the potential and heterogeneity of in-advance adaptation effects, we now analyze whether there is any added value of further sanction announcements in terms of targeted banks' international operations. Accordingly, we suggest an extension of the staggered difference-in-differences design (Baker, Larcker, and Wang 2022), in which we explicitly separate the first sanction announcement producing the in-advance adaptation effects and all the other announcements, which may (or may not) possess an added value. That is, we do not pool all the sanction announcements together—from the first to last, as a common design of the staggered DID would otherwise suggest.

Following the discussion on how to properly choose a control group in the presence of in-advance adaptation effects in Section 1.3.2 we now illustrate more formally the ideas behind our extended version of the staggered DID design. For simplicity, assume that we have only two sanction announcements—the first takes place at t_1 (early treatment) and the second at t_2 (late treatment), as depicted in Figure 1.6. Consider a stock variable $Y_{b,t}$ that is measured at the bank's b and month t level and is targeted by sanctions (for concreteness, suppose this is foreign assets, which have to fall after the asset sanctions). Suppose we have a plausible control group composed of never-treated banks. Suppose $\beta_{ET} < 0$ is the effect of the first sanction announcement on the early-treated banks (i.e., the sanctions reduce the stock of foreign asset holdings). Suppose also the later-treated banks start to adapt their $Y_{b,t}$ inbetween t_1 and t_2, as our results in the previous sections indicate, and the in-advance adaptation effect is β_{LT}.[40] We want to understand whether the later-treated banks will further reduce their $Y_{b,t}$ in response to the second sanction announcement after t_2. In this environment we have the following three outcomes with respect to the in-advance adaptation effect:

1. *Further deterioration* of $Y_{b,t}$. In this case, the later sanction announcement has a positive value (later-treated banks continue selling their foreign assets, forcibly). Graphically, this means that the slope of the line reflecting the time evolution of $Y_{b,t}$ of the later-treated banks turns steeper after t_2 as compared to in between t_1 and t_2. We mark this added value effect as $\widetilde{\delta}_1 < 0$.

2. *No changes* in $Y_{b,t}$. In this case, the later sanction announcement has no added value with respect to the in-advance adaptation, i.e., the slope of the line remains the same, and thus $\widetilde{\delta}_2 = 0$.

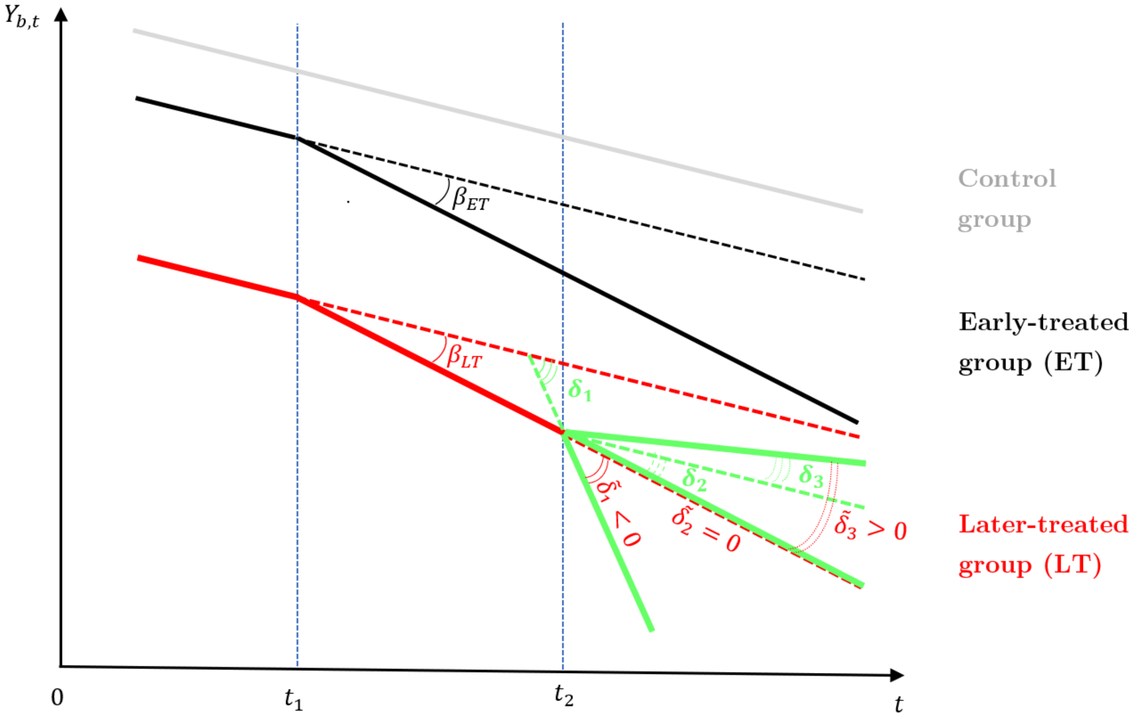
3. *Partial rebound* of $Y_{b,t}$. In this case, the later treatment has a negative added value (later-treated banks slow down the selling of their foreign assets). The slope of the line turns flatter, and thus $\widetilde{\delta}_3 > 0$.

In the first case, a positive added value of the later treatment appears because the later-treated banks did not fully adapt their $Y_{b,t}$'s in advance, i.e., between t_1 and t_2. The later treatment is harsher than expected. In the second case, the later-treated banks do not change their $Y_{b,t}$ because they have fully adapted in advance. This could be possible if there are no changes in the design of sanctions, and thus banks can fully predict the strength of the upcoming punishment. In the third case, the later treatment turns out to be milder than expected, and banks reduce the speed of their $Y_{b,t}$'s contraction.

Note that $\widetilde{\delta}_\jmath$ ($\jmath = 1, 2, 3$) is not feasible in terms of DID estimate if the control group is only composed of never-treated banks. What is feasible is $\delta_\jmath$, which measures the effect of the later treatment on the later-treated banks vis-a-vis the banks from the control group. However, by construction (and as long as the treatment is not diffused on the control banks from the treated banks), we can recover $\widetilde{\delta}_\jmath$ as $\widetilde{\delta}_\jmath = \delta_\jmath - \beta_{LT}$.

Empirical implementation. To test for the added value effects of further sanction announcements with respect to the first sanction announcement we begin by building an

[40]Presumably, $\beta_{LT} \leq \beta_{ET}$ because the later-treated banks were not-yet-sanctioned and thus could be less responsive. But this is not crucial for understanding the added value effects of further sanction announcements.



Note: The figure reports three potential outcomes of further sanction announcements: positive, no, and negative added value. $Y_{b,t}$ is a variable of interest at bank b and month t level (suppose foreign assets, for concreteness). Suppose that staggered implementation of the sanctions policy boils down to the two announcements: at t_1 (*early-treated*) and t_2 (*later-treated*). *Grey line* stands for the (matched) control group of similar never-sanctioned banks, *Black lines* depict potential outcomes for the early-treated banks, and *Red & green lines* mark potential outcomes for the later-treated banks, with *solid* red line reflecting an in-advance adaptation after t_1 and before t_2, red *dashed* lines reflecting either no in-advance adaptations before t_2 or no later-treatment effect after t_2, and green *solid* lines reflecting three potential outcomes for the added values of the later treatment: (1) further deterioration of $Y_{b,t}$, (2) no effect, and (3) partial rebound. β_{ET} is the effect of the first treatment on early-treated banks and β_{LT} is the effect of the same first treatment on later-treated banks (i.e., the in-advance adaptation effect). $\widetilde{\delta}_\jmath$ measures the added value of the later treatment with respect to the in-advance adaptation effect in outcome $\jmath$ ($\jmath = 1, 2, 3$). However, if the control group is composed of never-treated banks, $\widetilde{\delta}_\jmath$ is not feasible. Instead, $\delta_\jmath$ is feasible being an estimate of the later-treatment effect on the later-treated banks with respect to the control group, which is composed of never-treated banks. By construction, $\widetilde{\delta}_\jmath = \delta_\jmath - \beta_{LT}$.

Figure 1.6: Potential outcomes of sanction announcements: Generic cases

indicator variable, which equals 1 for each later-sanctioned bank $b = 2...S \in \mathfrak{A}$ and its matches from $\mathfrak{B}$ after the imposition of sanctions at t_b, equals 0 for the same banks at the respective pre-treatment period, and is empty for all other cases:

$$POST.NEXT_{b,t} = \begin{cases} 1, & \text{if } b \in \mathfrak{A} \cup \mathfrak{B} \text{ and } t \in [t_b, t_b + k] \\ 0, & \text{if } b \in \mathfrak{A} \cup \mathfrak{B} \text{ and } t \in [t_b - k, t_b) \\ ., & \text{if else} \end{cases} \tag{1.7}$$

The DID regression (1.3) from the previous section modifies to:

$$\begin{aligned} Y_{b,t} = \alpha_b + \gamma_t + \beta\Big(SANCTION_b \times POST.FIRST_t\Big) \\ + \delta\Big(SANCTION_b \times POST.NEXT_{b,t}\Big) + \psi'\mathbf{X}_{b,t} + \varepsilon_{b,t} \text{ if } t \in [t_b - k, t_b + k] \end{aligned} \tag{1.8}$$

Using Equation (1.8) we can test the following hypothesis:

Hypothesis H6 "*No added value of further sanction announcements*": $\widetilde{\delta} = \delta - \beta = 0$, i.e., the full effect of sanctions is absorbed by in-advance adaptation. Alternatives are either $\widetilde{\delta} < 0$ (further deterioration) or $\widetilde{\delta} > 0$ (partial rebound).

Staggered estimation results with in-advance adaptation

The estimations results of equation (1.8) appear in Table 1.4 below. The structure of the table remains the same as in the previous section, i.e., by rows in Panel 1 the dependent variable is foreign liabilities (as % of total liabilities), and in Panel 2 foreign assets (as % of total assets); columns 1–3 are for debt sanctions and columns 4–6 for asset sanctions. However, what changes is that now in columns 1 to 3 we consecutively expand estimation window $[t_b - k, t_b + k]$ by increasing parameter k: from 12 months in column 1 to 24 months in column 2 and to 36 months in column 3. The same applies to columns 4 to 6. This allows us to trace the time evolution of the estimated in-advance adaptation and added value effects.

Debt sanctions. As can be observed in Columns 1–3 in Panel 1, we obtain (i) positive and statistically significant (at 5%) coefficient on the interaction of the treatment variable and the indicator of the first sanction announcement, i.e., $SANCTION_b \times POST.FIRST_t$, and (ii) insignificant coefficient on the interaction of the same treatment variable and the indicator of the actual sanction introduction, i.e., $SANCTION_b \times POST.NEXT_{b,t}$. Strikingly, these estimates suggest that the banks that started to raise more international borrowings before being debt-sanctioned ($\widehat{\beta} > 0$) reduced them by

Table 1.4: Added value of further sanction announcements: Staggered difference-in-differences estimates on matched samples

	Sanction type:	Debt sanctions			Assets sanctions		
	Estimation Window $[t_b - k, t_b + k]$	$k = 12$	$k = 24$	$k = 36$	$k = 12$	$k = 24$	$k = 36$
		(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = Foreign liabilities, as % of bank total liabilities							
β	SANCTION$_b$ × POST.FIRST$_t$	2.205**	3.530**	3.959**	–0.397	–0.056	0.409
		(1.051)	(1.371)	(1.579)	(0.843)	(0.847)	(1.005)
δ	SANCTION$_b$ × POST.NEXT$_{b,t}$	0.548	0.345	0.155	–3.789***	–4.176***	–4.519***
		(0.860)	(1.069)	(1.222)	(0.676)	(1.026)	(1.283)
Added value of next sanction announcements:							
	$\widetilde{\delta} = \delta - \beta$	*–1.657**	*–3.185***	*–3.804***	*–3.392****	*–4.119****	*–4.927****
		(0.990)	*(1.412)*	*(1.549)*	*(0.640)*	*(1.230)*	*(1.642)*
	N obs	2,130	3,411	4,549	2,884	4,719	6,040
	R^2_{within}	0.335	0.361	0.330	0.240	0.263	0.249
Panel 2: Dependent variable = Foreign assets, as % of bank total assets							
β	SANCTION$_b$ × POST.FIRST$_t$	–0.612	–0.503	–0.846	–1.411	–1.978**	–2.411***
		(1.221)	(1.438)	(1.451)	(1.042)	(0.864)	(0.869)
δ	SANCTION$_b$ × POST.NEXT$_{b,t}$	0.284	0.830	0.958	–0.561	0.096	–0.085
		(0.854)	(1.184)	(1.224)	(0.949)	(0.838)	(0.833)
Added value of next sanction announcements:							
	$\widetilde{\delta} = \delta - \beta$	*0.896*	*1.332*	*1.804*	*0.849*	*2.074***	*2.325***
		(1.308)	*(1.782)*	*(1.911)*	*(0.952)*	*(0.985)*	*(1.001)*
	N obs	2,130	3,411	4,549	2,884	4,719	6,040
	R^2_{within}	0.189	0.193	0.214	0.152	0.132	0.146

Note: The table reports the staggered DID estimates of the effects of sanctions on foreign liabilities (*Panel 1*) and foreign assets (*Panel 2*) of Russia's targeted banks, as implied by equation (1.8). The estimation Window is $[t_b - k, t_b + k]$ months around the imposition of sanctions on each of the 44 sanctioned banks b, starting from the *Rossiya Bank* in March 2014. $SANCTION_b = 1$ if a bank b will ever face sanctions within our sample period. $POST.FIRST_t = 1$ after March 2014 and is aimed at capturing the in-advance adaptation effect. $POST.NEXT_{b,t} = 1$ after every next sanction announcement against each bank b after the *Rossiya Bank* (i.e., $b = 2, 3...44$) and is aimed at absorbing the added value of such announcements, if any. Sanctioned (i.e., treated) and never-sanctioned (i.e., control) banks are 1:4 matched within two years before March 2014. Private banks with political connections are not allowed to enter the control group. Bank FE, Month FE, Bank controls, and all necessary subcomponents of the two cross-product variables are included but not reported.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the sanctioned group level and at the level of each non-sanctioned bank and appear in brackets under the estimated coefficients.

almost *the same magnitude* once sanctioned ($\widehat{\delta} = -\widehat{\beta}$ because $\widehat{\delta} = 0$, statistically). In this case, the added value of further sanction announcement—by up to –3.8 pp in three years—is just the elimination of excessive borrowings observed after the first sanction announcement, but nothing more. The sanctions could not force the (already) debt-sanctioned banks to shrink their foreign liabilities faster. We also find that the added value effect grows in time—but this likely mirrors the increasing trend in the estimated in-advance adaptation effect. Turning to Panel 2 across the same Columns 1 to 3 and considering the international assets of debt-sanctioned banks, we conclude that neither the informational nor direct effects are significant.[41] This means that on average debt-sanctioned banks were barely nervous about possible asset freezes by Western countries, which is consistent with the design of the debt sanctions.

Asset sanctions. The estimates for the asset-sanctioned banks are rather different. As can be inferred from Columns 4–6 in Panel 1, we lose the significance of the in-advance adaptation effects pertaining to the international borrowings of not yet asset-sanctioned banks. But we gain negative and highly statistically significant estimates of the added value effects in this case, which are also growing in time. This implies that on a larger time horizon, these banks started to reduce their international borrowings more extensively only when they were actually sanctioned. The added value of further sanction announcements is thus very large—reaching up to –5 pp of total liabilities. This is because the in-advance adaptation effects were insignificant and, once asset-sanctioned, the banks were forced to turn to substantially reducing their borrowings from abroad. Finally, moving to Panel 2 across the same Columns 4–6 and considering foreign asset holdings as the dependent variable, we obtain negative and significant estimates of the in-advance adaptation effects β and insignificant estimates of the coefficient δ on the $SANCTION_b \times POST.NEXT_{b,t}$ variable. Taken together, this means that the added value of further sanction announcements is negative in the sense that after the sanctions were imposed, the (already) asset-sanctioned banks turned to slow down the selling of their foreign assets instead of accelerating it. The rebound could have reached up to $+2.3$ pp. This finding indicates that before being asset-sanctioned the banks could have been too pessimistic regarding the upcoming sanctions and were overselling their foreign asset holdings; once sanctioned, the banks could have stopped overselling.[42]

[41]Recall that in the previous section the estimate of the in-advance adaptation effect was negative and significant in this case, whereas now it lost its significance. This is likely because before we had a fixed estimation window $[t_1 - 24, t_1 + 24]$ months where t_1 is March 2014, whereas now we have an expanding estimation window which accommodates *all* t_b's. Therefore, the numbers of observations differ in these two cases, and thus the models are not directly comparable in the quantitative sense. Combining the earlier and current estimates, we can conclude that, while *not yet* debt-sanctioned, the banks first reduced their foreign asset holdings, but, after each updating of their knowledge regarding how the debt sanctions work, they apparently slowed down the sales of foreign assets. This pattern is very much consistent with respective event-study estimates (see Figure 1.5.c), and therefore, in a larger time span, it could be the case that the in-advance adaptation effect is blurred.

[42]This type of behavior is aligned with diagnostic beliefs (Bordalo, Gennaioli, and Shleifer 2018), which arise in the presence of uncertainty regarding who next and when will be sanctioned. An alternative

Overall, we conclude that the effects of sanctions were very much heterogeneous in terms of (i) the sanction type (debt vs. assets) and (ii) the timing (announcement vs. actual imposition).[43] If one would apply a staggered difference-in-differences design (Baker, Larcker, and Wang 2022) without separating the in-advance adaptation effects from the effects of later sanction announcements, then one would misleadingly *under*-estimate the effect of sanctions on the international borrowings of debt-sanctioned banks and on the foreign assets of asset-sanctioned banks. The in-advance adaptation effects matter, and having them with the added value effects within the same regression model provides a flexible approach to disaggregate the early- and later-treatment effects in the presence of staggered implementation of a policy.

1.3.6 From international to domestic operations of targeted banks: unintended effects of sanctions?

Domestic liabilities: depositor runs mitigated by the government support?

In this section, we consider (i) private deposits, corporate deposits, and deposits attracted from the inter-bank market as those funds originating from *non-government sources* and (ii) government deposits and loans obtained from the Central Bank of Russia (CBR) as those from *government sources.* One may expect that all types of private depositors could launch a panic, withdrawing their funds from not-yet-treated banks in response to sanctions, while the government could step in and substitute for these funds to prevent disordered failures.

We run a series of the staggered DID regressions with in-advance adaptation effects, as implied by equation (1.8), by performing estimates on *expanding* estimation windows $[t_b - k, t_b + k]$, where $k = 1, 2...36$ months after either the date of sanctions against the *Rossiya Bank* or a bank-specific sanction date. We expand the estimation window to make sure that we do not omit the effects of sanctions. This is important because domestic operations of state-connected banks were not targeted by the sanctions, and it is not clear whether—and for how long—domestic operations reacted to the compositional changes in the banks' balance sheets caused by the sanctions.

possibility is that the banks could have appealed to evading sanctions through foreign subsidiaries—a channel similar to that explored by Efing, Goldbach, and Nitsch (2023) for the German banks—does not apply here because the Russian asset-sanctioned banks did not operate abroad.

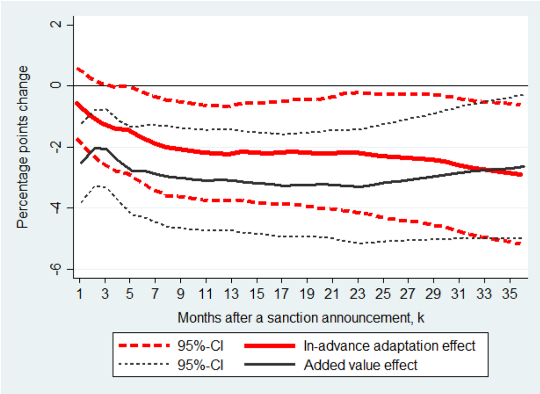
[43] As we show in Appendix 1.E, the pooling of debt- and asset-sanctioned banks in one treatment group confounds the baseline effects. Specifically, it attenuates both the in-advance adaptation effects and the added value effects of the next sanction announcements.

We report the estimated coefficients β_k and δ_k on $SANCTION_b \times POST.FIRST_t$ and $SANCTION_b \times POST.NEXT_{b,t}$, respectively, for each k in Figure 1.7. Recall that β_k reflects the in-advance adaptation effect and δ_k stands for the added-value effects. The figure reports the results for deposits that are attracted from households (upper panel) and non-financial firms (lower panel).

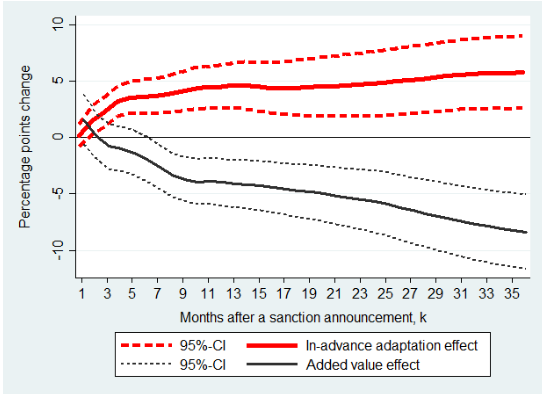
Household deposits. For not yet debt-sanctioned banks, we obtain negative and significant estimates of β_k and negative and significant estimates of δ_k, both reaching their troughs at –3 pp of the banks' total liabilities (see Figure 1.7.a). Assuming the supply-side forces (depositor panic), the estimation results thus suggest that households were already responsive to the informational effects of sanctions (even when the news pertained to state-connected banks that did not hold their savings). This might seem surprising because we could imagine a lack of attention and/or expertise from households in predicting who is going to be the next sanctioned bank. However, recall that these are the largest banks in the system, with a share of the deposits market exceeding 50%. In this sense, our finding is hardly surprising.

In the case of not yet asset-sanctioned banks, we obtain positive, not negative, and statistically significant estimates of β_k peaking at +5 pp of the group's total liabilities, and we get negative, as expected, and also significant estimates of δ_k peaking at –8 pp, see Figure 1.7.b). First, the unexpectedly positive in-advance adaptation effects obtained here may indicate that not yet asset-sanctioned banks were trying to accumulate additional funds from households in domestic markets (demand-driven factor) before they were sanctioned and had to reduce their international borrowings. These banks, differently from their debt-sanctioned peers, were much smaller, and thus households could have been less attentive regarding the prospects of these banks and were ready to lend them their savings.

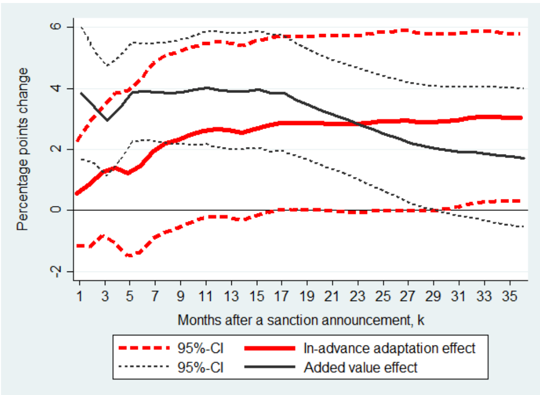
Deposits of non-financial firms. We further obtain positive and significant estimates of β_k for not yet debt-sanctioned banks, peaking at +3 pp (three years after March 2014), and also positive and significant estimates of β_k for not yet asset-sanctioned banks, peaking at +2 pp within a year of March 2014 (see Figures 1.7.c,d, respectively). We could expect that differently from households, firms are more likely to be better informed regarding upcoming sanctions and could thus have launched *information*-based withdrawals of their funds from banks (a precautionary motive). However, our results highlight a different mechanism that could potentially materialize: indirect government support through large state-owned firms, which could have been ordered to increase their deposits at



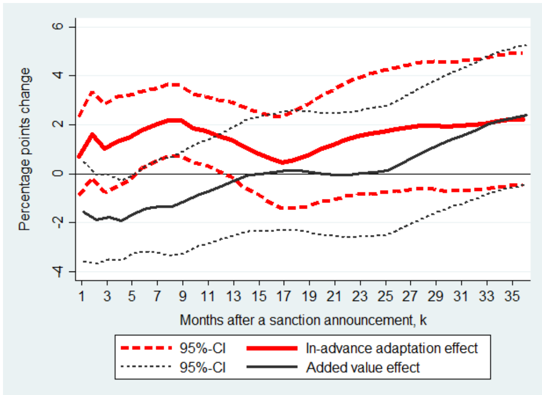
(a) Household deposits, *debt*-sanctioned banks



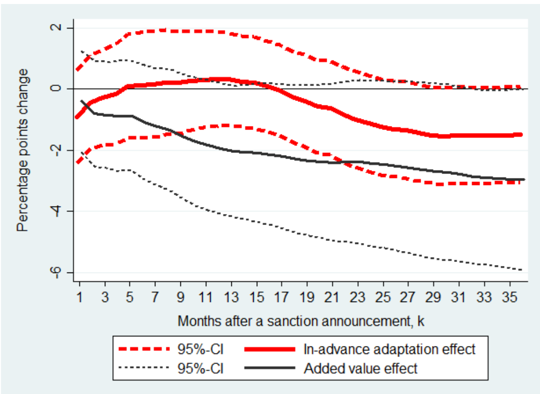
(b) Household deposits, *asset*-sanctioned banks



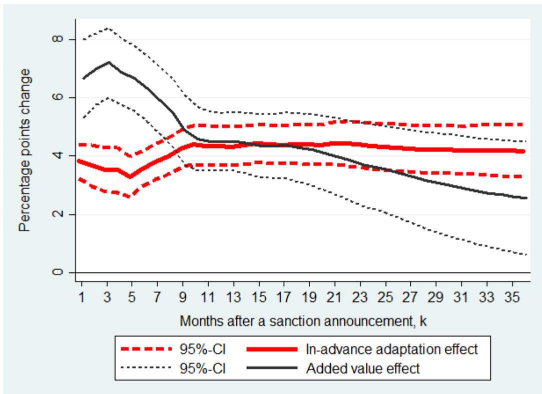
(c) Firm deposits, *debt*-sanctioned banks



(d) Firm deposits, *asset*-sanctioned banks



(e) Inter-bank deposits, *debt*-sanctioned banks



(f) Inter-bank deposits, *asset-sanctioned banks*

Note: The figures report the staggered difference-in-differences estimates of the coefficients on $SANCTION_b \times POST.FIRST_t$ and $SANCTION_b \times POST.NEXT_{b,t}$ in equation (1.8), with the dependent variable reflecting either household, non-financial firm, or inter-bank deposits (as % of bank total liabilities). The estimates are obtained by running the staggered DID with in-advance adaptation on the expanding window $[t_b - k, t_b + k]$, where $k = 1, 2...36$ months after either bank-specific sanction date (added value effects, black lines) or the date of sanctions against the *Rossiya Bank* (in-advance adaptation effects, red lines).

Figure 1.7: What happened with key domestic bank liabilities after sanctions? (*by sanction type*)

certain not-yet-sanctioned banks.

When it comes to the added value of the next sanction announcements, δ_k, we obtain a positive and significant estimate for already debt-sanctioned banks (+4 pp of total liabilities, within a year of the announcements) but a negative and significant estimate for already asset-sanctioned banks (–2 pp, within half a year of the announcements). For the asset-sanctioned banks, the sanction-driven reduction of non-financial firms' deposits could have been forced by the firms themselves.

Inter-bank deposits. Another portion of heterogeneous responses comes from inter-bank deposits. For debt-sanctioned banks, we obtain negative but insignificant estimates of β_k and negative and marginally significant estimates of δ_k, peaking at –3 pp by the end of the third year after the sanction announcements. For asset-sanctioned banks, the results are different: we obtain positive and significant estimates of both β_k and δ_k, with the former peaking at +4 pp of total liabilities in three years after March 2014 and the latter peaking at +7 pp in the quarter after the bank-specific sanction announcement dates. Taken together, these results indicate that debt-sanctioned banks could encounter inter-bank runs after being sanctioned, whereas asset-sanctioned banks enjoyed an increasing flow of wholesale funds. Recall, however, that the debt-sanctioned banks are the largest banks in the system and, despite also opting for borrowings from the inter-bank market, are recognized as the major re-distributors of the liquidity from the Central Bank of Russia to the rest of the banking system. Therefore, we may suggest an indirect government support interpretation in the case of asset-sanctioned banks.

(Direct) Government support. Regarding the (other) sources of government-provided funds that could have been appealed to by the sanctioned banks, we again reveal substantially different patterns across debt and asset-sanctioned banks. First, our DID estimates imply that (not yet) debt-sanctioned banks obtained government support in the form of municipal- or federal-state deposits (increase by up to +0.5 pp of total liabilities) and loans from the Central Bank of Russia (+2.0 pp, see Figure 1.1.a,c in Appendix 1.F). Conversely, (not yet) asset-sanctioned banks effectively obtained nothing from the government. Direct state deposits had been increased by a negligible amount (+0.15 pp). The loans from the Central Bank of Russia first had been reduced (before the sanctions) and then increased (after the sanctions) by virtually the same amounts ($\pm$2 pp), see Figure 1.1.b,d).

Quantitatively, by bringing together all the estimation results obtained in the previous sections and here, we can conclude that the targeted banks were over-supported by the government. Our computations show that, in response to all sanction packages, debt-sanctioned banks managed to raise the total size of their liabilities by 0.6 pp and asset-sanctioned banks—by another 3.7 pp.[44]

Overall, we find that runs on sanctioned banks were substantial. However, the government stepped in and—either directly (through federal or municipal deposits or the loans from the Central Bank of Russia) or indirectly (through the inter-bank market or funding through state firms)—supported, or even *over*-supported, the targeted banks, thus preventing their disorderly failure. Clearly, the sanctions had forced the Russian economy to mobilize financial resources and direct them to targeted banks. Unexpectedly, this mobilization had fully offset the intended negative effects of sanctions. In this situation, we can expect that the targeted banks can in principle expand loan supply to the economy. And this is what we investigate in the next section.

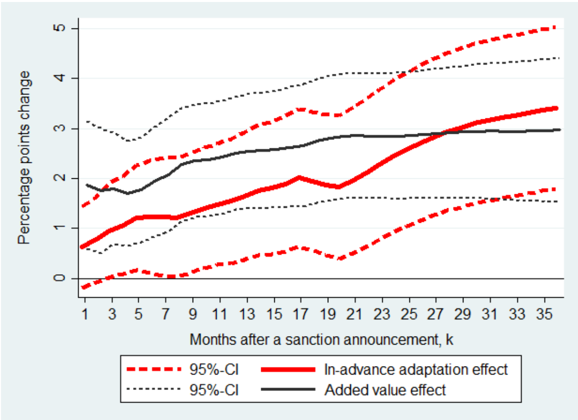
Domestic assets: the credit re-shuffling effect

How did sanctioned banks adjust their domestic lending in response to the sanction-driven changes in their liabilities? We report the estimation results for domestic lending in Figure 1.8 below.

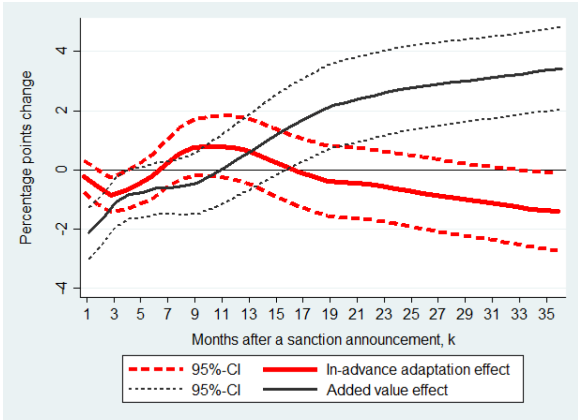
The estimates suggest that (not yet) debt-sanctioned banks could have decreased their loans to non-financial firms by 2 pp of their total assets, as a matter of in-advance adaptation, and by another 3.2 pp, as a result of the direct effects of sanctions (Figure 1.8.c). The estimates further indicate that the same banks increased loans to individuals by up to 3.5 pp of their total assets, as after March 2014 and before being actually sanctioned, and by another 2.9 pp once sanctioned (Figure 1.8.a). As one can infer, the loan portfolio was effectively re-balanced, not squeezed.

For the (not yet) asset-sanctioned banks, the estimates imply a reduction of corporate loans by 4.0 pp, as a matter of in-advance adaptation, though a further recovery by virtually the same 4.0 pp after being actually sanctioned (Figure 1.8.d). Loans to households were unlikely to be in-advance adapted but they increased substantially, by 4 pp of total assets, as a result of the direct effects of sanctions (Figure 1.8.b). Effectively,

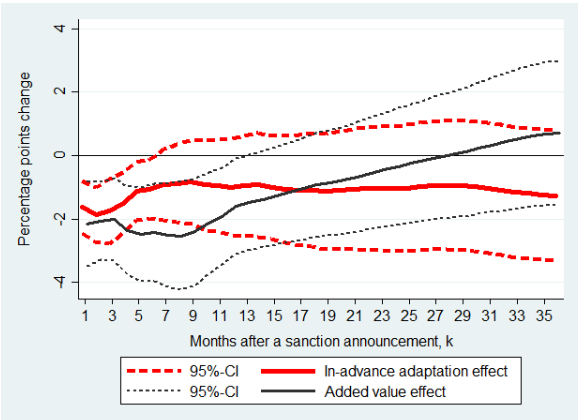
[44] The computations are (in pp): (i) for *debt*-sanctioned banks, +0.2 (i.e., +4.0 − 3.8, international borrowings) – 6.0 (i.e, –3.0 + (–3.0), households) + 7.0 (i.e., 3.0 + 4.0, non-financial firms) – 3 (inter-bank deposits) + 0.4 (direct government deposits) + 2.0 (loans from the CBR) = +0.6, and (ii) for *asset*-sanctioned banks, –4.5 (i.e., +0.4 – 4.9, international borrowings) – 3.0 (i.e, +5.0 + (–8.0), households) + 0.0 (i.e., –2.0 + 2.0, non-financial firms) + 11.0 (i.e., 4 + 7, inter-bank deposits) + 0.2 (direct government deposits) + 0.0 (i.e., –2.0 + 2.0, loans from the CBR) = +3.7.



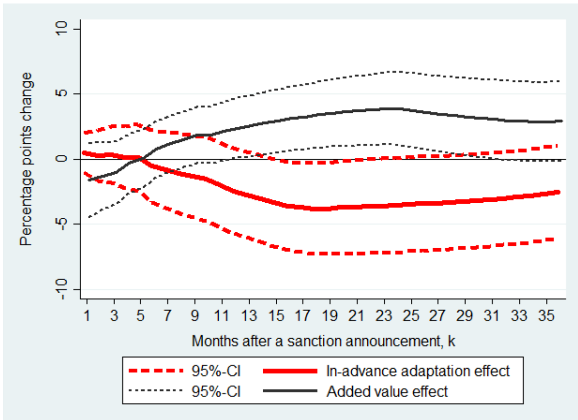
(a) Household loans, *debt*-sanctioned banks



(b) Household loans, *asset*-sanctioned banks



(c) Non-financial firm loans, *debt*-sanctioned banks



(d) Non-financial firm loans, *asset*-sanctioned banks

Note: The figures report the staggered difference-in-differences estimates of the coefficients on $SANCTION_b \times POST.FIRST_t$ and $SANCTION_b \times POST.NEXT_{b,t}$ in equation (1.8), with the dependent variable reflecting the stock of bank credit to either households or non-financial firms (as % of bank total assets). The estimates are obtained by running the staggered DID with in-advance adaptation on the expanding window $[t_b - k, t_b + k]$, where $k = 1, 2...36$ months after either bank-specific sanction date (added value effects, black lines) or the date of sanctions against the *Rossiya Bank* (in-advance adaptation effects, red lines).

Figure 1.8: How banks adjusted their assets after sanctions?
by sanction type

the asset-sanctioned banks were even able to increase their lending intensity after the sanctions.

Overall, the most striking result is that both (not yet) debt- and asset-sanctioned banks began to re-shuffle the structure of their loan portfolios by decreasing the volume of credit granted to non-financial firms and increasing the volume of credit allocated to households in response to either informational or direct effects of sanctions (or both). We interpret this result as the banks' forward-looking willingness to insure their loan portfolios from a rising risk of sanctions against Russian firms *per se*. Firms themselves could face sanctions and stop repaying their debts, whereas households (at least, those not from the SDN list) were free of such "sudden" constraints. As a result, the sanctioned banks became more specialized in retail lending than before. Our conclusion on reductions of loans to firms is consistent with the findings in previous studies (Ahn and Ludema 2020; Crozet et al. 2021) who revealed the sanctions did indeed have a negative effect on Russian firms.

Regarding other domestic assets, the DID results indicate that debt-sanctioned banks reduced their inter-bank exposures in response to the direct effects of sanctions (Figure 1.2.a), whereas asset-sanctioned banks effectively increased such exposures—by about 7.0 pp as a means of in-advance adaptation but then partially reduced the exposures—by roughly 4.0 pp shortly after the sanction announcements (see Figure 1.2.b). Finally, we find that (not yet) debt-sanctioned banks did not create additional cash & reserves buffers as a matter of in-advance adaptation to upcoming sanctions (Figure 1.2.c), whereas not yet asset-sanctioned banks did so in a similar situation—they increased the ratio of cash and reserves to total assets by up to 8.0 pp (Figure 1.2.d). However, when sanctions hit, the already debt- and already asset-sanctioned banks both had to reduce their exposures at the inter-bank market (by up to –3.2 and –4.0 pp, respectively) and spend cash and reserves (in amounts of 1.5 and 7.0 pp, respectively), presumably to manage the deposit runs. In Appendix 1.G, we describe the DID estimation results on what happened to the (effective) interest rates of the targeted banks.

1.3.7 Macroeconomic effects: back-of-the-envelope calculations

We now analyze the aggregate implications of the just discussed staggered DID estimates of the effects of sanctions on the largest Russian banks. We appeal to structural vector autoregressive models (SVAR) with sign restrictions, which allow us to identify credit supply shocks and their effects on the real economy, at the aggregated level. We follow Gambetti and Musso (2017) in the identification of credit supply shocks (SR) and add the narrative component (NSR) to the analysis, as suggested by Antolin-Diaz and Rubio-Ramirez (2018).

Estimation results of the SVAR model appear in Appendix 1.H. Given the estimated impulse response functions from our SVAR model, we can describe how we use them jointly with our microeconomic estimates of sanctions to evaluate the real effects on the economy.

Recall now the microeconomic estimates of the in-advance adaptation and added value effects of sanctions on loans. Let us start with loans to non-financial firms. As we reported in Section 1.3.6, the in-advance adaptation effects of sanctions on (not yet) debt- and asset-sanctioned banks peaked at –2 and –4 pp, respectively. The average distance, at which the in-advance adaptation effects are in work, equals 21 months, i.e., this is the actual distance between March 2014 (the first portion of sanctions) and the average date at which other portions of sanctions were introduced. During these 21 months, the average volume of total assets of the debt-sanctioned banks equals 2,604 billion rubles and of the asset-sanctioned banks equals just 85 billion rubles.[45] Recall that we have 16 banks in the debt- and (effectively) 17 banks in the assets sanctions list. Therefore, we can estimate the aggregate decline of loans to non-financial firms caused by the in-advance adaptation effect of sanctions as –833 and –58 billion rubles, respectively for the debt- and asset-sanctioned banks.[46] Now, apply the elasticity of output (GDP) with respect to loan volumes estimated from our SVAR-analysis (1.52) and obtain that the in-advance adaptation effect of sanctions has caused a decline of the Russian economy's GDP (i) by –1.5 pp because of credit reductions by (not yet) debt-sanctioned banks and (ii) –0.2 pp because of loan reductions by (not yet) asset-sanctioned banks (averages for 2014–2015), amounting to –1.7 pp in total. This result implies that the very first announcement of sanctions in March 2014 had a rather moderate though noticeable negative effect on the Russian GDP through reductions of loans to non-financial firms.

In contrast to the existing research on sanctions, we can disaggregate the effects of sanctions on those channeling through firms versus those through households. Further, the in-advance adaptation effect through households was positive, not negative. Com-

[45]For comparative reasons, if we take the 2014-2015 average ruble to US dollar exchange rate (49.69 rubles per 1 dollar), these are equivalent to 52.4 and 1.7 billion US dollars, respectively.

[46]These are computed as $-0.020 \times 16 \times 2,604$ and $-0.04 \times 17 \times 85$.

puted similarly, we find that Russian GDP in 2014–2015 rose by an additional 2.3 pp because of credit re-shuffling—that is, through increasing lending to households initiated by (not yet) debt-sanctioned banks. These positive effects outweigh the negative ones by 0.5 pp in terms of GDP after the in-advance adaptation effects of sanctions.

Further, the direct effects of sanctions also appear to be large compared to the just described in-advance adaptation one. Average volumes of assets during respective periods increased to 3,137 and 160 billion rubles for the debt- and asset-sanctioned banks, respectively. Applying the same logic as above, we can estimate that debt-sanctioned banks decreased corporate loans by 1,606 billion rubles whereas the asset-sanctioned banks compensated for this decline by only 109 billion rubles. Through the estimated elasticity of GDP to loan volumes during the periods of credit supply shocks, these figures imply the Russian GDP declined by 2.5 pp over 2015–2017 as a result of corporate credit reduction by the debt-sanctioned banks and recovered by only 0.2 pp due to expanded corporate credit by asset-sanctioned banks. Finally, Russia's GDP increased over the same period by 1.8 and 0.2 pp due to increasing credit to households initiated by debt- and asset-sanctioned banks in response to the direct effects of sanctions. On net, the credit re-shuffling led to a rather moderate decline of GDP—by only 0.4 pp after the direct effects of sanctions materialized.

Overall, this macroeconometric exercise indicates that the sanctions against the largest Russian banks could have a large negative (in-advance adaptation and direct) effect on the Russian economy through declined bank lending to non-financial firms (–4.0 pp of GDP) but, at the same time, almost equally positive (in-advance adaptation and direct) effect through expanded lending to households (+4.1 pp of GDP). These numbers can shed more light on why previous research reveals no disruptive effects of the Western sanctions against Russia (Dreger et al. 2016; Ahn and Ludema 2020).

1.4 Treatment diffusion to non-targeted banks

1.4.1 The idea

One may have a concern that the Western countries did not recognize all state-connected banks in Russia, especially those indirectly controlled by the Russian government.[47] If

[47] Recall that among the 20 debt-sanctioned banks, 15 were sanctioned because they were subsidiaries of either the "*Big-4*" state-owned banks or the development bank VEB.

some banks were left unrecognized, then our baseline estimates of the in-advance adaptation effects of sanctions can be biased upward due to omitted *treatment diffusion*, i.e., due to possible adaptation of international operations by the unrecognized banks.[48]

We suggest the following mechanism of treatment diffusion. The probability of being sanctioned subjectively perceived by not yet recognized banks crucially depends on the share of *government-connected persons* on the board of directors/owners of such banks: the greater the share, the higher the subjective probability, the larger the in-advance adaptation of international operations. This is because a greater share of government-connected persons makes it easier for Western countries to recognize a bank as state-controlled and sanction it.

To facilitate the search of unrecognized banks, we follow Karas and Vernikov (2019), who provide a comprehensive hand-collected database on the ownership structure of all 3,176 banks that were operating in the Russian banking system over the last three decades. Focusing on the 2014–2020 period using this database we find that a total of 55 state-owned and controlled banks in Russia, of which only 20 were actually (debt) sanctioned. The other 35 banks were untouched by the West. In Appendix 1.I, we analyze the balance sheets of those banks and compare them to those of the sanctioned banks. The key outcome of the comparison is that the unrecognized banks are in between the debt- and asset-sanctioned banks in terms of size, have similar portions of foreign borrowings and international assets before March 2014, and they reduced those after March 2014.

1.4.2 Construction of the government share variable

For each bank b from the subgroup of the 33 eventually-sanctioned banks and the 35 unrecognized state-controlled banks, we first access bank b's official website and download annual reports for each year $t \geq t^* = \mathit{2014}$, where possible, up to 2019. Further, we extract information on the composition of the board of directors in the respective year from the annual reports. We gather name, surname, date of birth, and career-path information, where possible, for each person p entering the board of directors of the bank b at year t. Of course, we face large variations in the degree of such data disclosure, ranging from no disclosure at all (8 banks out of the 35 unrecognized banks) to at least names and

[48]Though we cannot test it directly with the data at hand, it is clear that the unrecognized banks, if any, might be used to evade sanctions. These banks could, if necessary, borrow from abroad and transfer these funds to the already-sanctioned banks through the domestic inter-bank market. Likewise, the unrecognized banks could buy the foreign asset holdings of the already-sanctioned banks and thus effectively keep the government control over those.

surnames being disclosed (all actually-sanctioned banks and $35 - 8 = 27$ unrecognized banks) or even full CVs attached to the reports. If the annual reports contain all the necessary information on each person p, we stop searching; if not, we take the names, surnames, and dates of birth and use publicly available sources: the search through either the nationwide database on the Russian banks,[49] the database on managers employed in the Russian companies, more broadly,[50] or the search over the rest of the Web. Third, with this rich information aggregated from various sources, we construct the government share variable, $Gov.Share_{b,t}$. For this purpose, we attribute a person p from bank b's board of directors/owners to that who had relations with the Russian government in period t (or before) if the person p:

1. enters at t, or entered before t, the board of directors of at least one other state-owned or -controlled financial (e.g., the "Big-4" and VEB) or non-financial (e.g., Rosneft[51]) entity;

2. is at t, or was before t, either a local or federal minister or deputy/senator from the ruling party ("Edinaya Rossiya");

3. represents at t an oligarch family with close ties to the Kremlin (e.g., Kovalchuk, Rotenberg).

Below in Table 1.5 we report a description of the constructed $Gov.Share_{b,t}$ variable. We were successful to gather the necessary data on government-connected persons in half of the 17 asset-sanctioned banks; 15 of the 16 debt-sanctioned banks, and 27 of the 35 unrecognized banks. We obtain that *on average*, the 27 unrecognized banks are in between the asset- and debt-sanctioned banks in terms of the share of government-connected persons on the board of directors/owners. The mean value of the $Gov.Share_{b,t}$ variable for them equals roughly 54%, which is 26 pp larger than in the asset-sanctioned banks but 30 pp lower than in the debt-sanctioned banks. We also find substantial variation across the three subgroups, ranging from 8 to 100%, which is important for the upcoming logit analysis.

[49] https://www.banki.ru/.
[50] https://www.e-disclosure.ru/poisk-po-kompaniyam.
[51] The major oil extracting and exporting company in Russia

Table 1.5: Summary statistics on the $Gov.Share_{b,t}$ variable across the subgroups of treated and diffused banks

	Obs*	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
Treated & Not state	8 / 17	26.4	9.7	17	50
Treated & State	15 / 16	83.5	15.4	25	100
Not treated & State	27 / 35	53.9	25.7	8	100

Note: "Treated" stands for actually sanctioned banks. "Not treated" denotes potentially diffused banks. "State" implies a bank is in the Karas and Vernikov (2019) list of state-controlled banks.
* In the "Obs" column we report for how many banks from a given subgroup we were successful in constructing the $Gov.Share_{b,t}$ variable.

1.4.3 Treatment diffusion: a two-stage approach

The first stage

In the first stage, we predict a subjectively perceived probability of being sanctioned based on the variation in the $Gov.Share_{b,t}$ variable using a logit regression framework at the monthly frequency. Since we are working with subjective perceptions of sanctions, we further hypothesize that such perceptions depend crucially on the distance to Moscow, a variable that earlier proved its relevance in determining the heterogeneity of the in-advance adaptation effect of sanctions. The resulting logit specification reads as:

$$Pr\{Sanctioned_{b,t} = 1 \mid \mathbf{X}_{b,t}\} = \\ = \Lambda\Bigg(\beta_1 Gov.Share_{b,t} + \beta_2\Big(Gov.Share_{b,t} \times Distance_b\Big) + \phi'\mathbf{X}_{b,t}\Bigg) \tag{1.9}$$

where $Sanctioned_{b,t}$ is an indicator variable that equals 1 if a bank b was sanctioned at t or before, and 0 if not. $X_{b,t}$ are observables that encompass the government shares in the boards of directors of each bank b, its distance to Moscow, and other bank-specific controls. For the latter, we consider (i) the structure of international operations, proxied by the difference between foreign assets and foreign liabilities, relative to bank b's total assets (TA); (ii) the structure of domestic operations, measured by the difference between individuals' deposits and loans, % of TA; (iii) annual growth of the bank's b TA; (iv) the quality of bank b's loan portfolio, measured by NPLs ratio to TA; (v) the role played by bank b's in the domestic inter-bank market, measured by the difference between loans issued and deposits attracted there, % of TA; and (vi) profitability of bank's b TA, measured by a monthly ROA indicator. Finally, $\Lambda(\cdot)$ is the logistic distribution.

We estimate a *series* of cross-sectional, not panel, logit regressions for each month $t \geq t^*$. This implies a time variation in the estimated coefficients in Equation (1.9), which in turn allows us to flexibly account for the changing nature of bank adaptation to negative news on upcoming sanctions. Importantly, we consider two versions of the $Sanctioned_{b,t}$ variable: one for *debt*- and the other for *asset*-sanctioned banks; that is, we run two parallel loops of cross-sectional logit regressions. This is crucial in the second stage, when it is necessary to assume which of the two types of sanctions a not-yet-treated bank can encounter.

Table 1.6 reports a part of the estimation results pertaining to the peaks of sanction imposition during the first wave (2014, Panel 1) and the second wave (2017, Panel 2).[52] In columns (1)–(3) we report the results for the full subsample, composed of debt- and asset-sanctioned banks, while columns (4)–(6) show the results for the debt-sanctioned banks and columns (7)–(9) for the asset-sanctioned banks.

Several outcomes emerge from the estimation results of the first stage. *First*, and most importantly, our constructed $Gov.Share_{b,t}$ variable is informative in predicting the imposition of sanctions and captures a differential impact on debt- and asset-sanctioned banks. Specifically, the estimated coefficients on the $Gov.Share_{b,t}$ variable are always highly statistically significant for the debt-sanctioned banks and never—for the asset-sanctioned banks.[53] *Second*, mixing the two types of sanctions (in columns (1)–(3)) deteriorates the precision of estimated coefficients on $Gov.Share_{b,t}$, which is true for the first wave of sanctions (Panel 1) but not true for the second (Panel 2). *Third*, we find that the greater the distance to Moscow is, the larger is the effect of $Gov.Share_{b,t}$ on the subjectively perceived probability of being debt-sanctioned. This may speak to an informational asymmetry: those banks located farther from Moscow could assign a larger weight to the presence of government-connected persons on their board of directors when assessing the likelihood of sanctions than banks located near the Kremlin. During the second wave of sanctions, this heterogeneous effect disappears, which possibly indicates that the informational asymmetry regarding the upcoming sanctions had vanished, and banks, no matter where they were located, started to treat the signals of sanctions equally when negative news occurred.[54]

[52]The full estimation results are not reported to preserve space and are available upon request.

[53]We also note that the pseudo-R^2 in the models for debt-sanctioned banks is greater by factors 2 to 3 than those in the models for asset-sanctioned banks.

[54]We also considered a binary version of the $Gov.Share_{b,t}$ variable: 1 if the share of government-

Table 1.6: Treatment diffusion: a fragment of the estimation results from the first stage

Sanction type:	Debt + Asset			Debt			Asset		
	t^*-1	t^*	t^*+1	t^*-1	t^*	t^*+1	t^*-1	t^*	t^*+1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel 1: First wave of sanctions, t^* = September 2014									
Government share (GS)	0.183*	0.140	0.156	0.089***	0.092***	0.089***	–0.048	–0.062	–0.048
	(0.107)	(0.116)	(0.103)	(0.014)	(0.012)	(0.011)	(0.175)	(0.153)	(0.168)
Distance to Moscow (DM) / 1,000	–1.320	–1.612	–1.347	–2.481*	–2.410*	–2.445*	–4.420**	–4.528**	–4.266**
	(1.242)	(1.156)	(1.097)	(1.515)	(1.517)	(1.382)	(1.933)	(1.910)	(1.816)
GS × DM / 1,000	0.779	0.492	0.571	0.048**	0.046**	0.049***	–0.510	–0.584	–0.501
	(0.594)	(0.545)	(0.564)	(0.020)	(0.020)	(0.019)	(0.993)	(0.863)	(0.953)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	813	805	791	796	789	775	798	790	776
Pseudo-R^2	0.373	0.394	0.408	0.641	0.650	0.653	0.276	0.286	0.303
Panel 2: Second wave of sanctions, t^* = June 2017									
Government share (GS)	0.072***	0.064***	0.061***	0.117***	0.121***	0.126***	–0.071	–0.085	–0.046
	(0.012)	(0.009)	(0.008)	(0.028)	(0.031)	(0.034)	(0.159)	(0.162)	(0.153)
Distance to Moscow (DM) / 1,000	–5.010*	–4.097***	–4.267***	–2.438	–3.809	–3.943	–6.881	–6.304*	–6.613*
	(2.948)	(1.521)	(1.494)	(1.980)	(3.156)	(3.610)	(4.339)	(3.385)	(3.945)
GS × DM / 1,000	0.056	0.043**	0.045**	0.014	0.026	0.024	–0.621	–0.705	–0.489
	(0.038)	(0.021)	(0.021)	(0.025)	(0.039)	(0.044)	(0.862)	(0.879)	(0.829)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	543	541	534	527	525	518	528	526	519
Pseudo-R^2	0.424	0.400	0.320	0.705	0.713	0.712	0.201	0.212	0.194

Note: ***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the bank level and appear in brackets under the estimated coefficients.

To understand the economic significance of possessing government-connected persons on the board of directors, we compute the product of the marginal effect of the $Gov.Share_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$ and a one standard deviation of the $Gov.Share_{b,t}$ variable. We do this for each month t after we estimate the respective logit model in the loop. Because the regressors contain the interaction of $Gov.Share_{b,t}$ and the distance to Moscow, we set the $Distance_b$ variable at its sample mean for concreteness.[55] The resultant economic effects of $Gov.Share_{b,t}$ are plotted in Figure 1.9.(a) for

connected persons in the bank's b board of directors is strictly greater than zero at time t, and 0 if else. The logit estimations produce qualitatively the same results (available upon request).

[55]We demean our variables before running regressions by subtracting the respective unconditional means from each variable to address multicollinearity concerns arising in the models with cross-products of explanatory variables. Thus, the mean of the demeaned distance variable varies from some 5 to 35 km depending on the month, and the min of the demeaned distance equals roughly –180 km and stands for the banks located in Moscow. We can therefore interpret our results to be those relevant for the banks

debt sanctions and Figure 1.9.(b) for asset sanctions. Our results suggest that an increase in the share of government-connected persons on the board of directors by one standard deviation significantly raises the probability of being debt-sanctioned by 10 to 22%, depending on the month, whereas the effects are mostly insignificant for asset sanctions. Interestingly, these economic effects exceed those pertaining to the same banks' overall size, as measured with the log of total assets, and foreign asset position (see Appendix 1.J). Overall, the effects are meaningful, given that the unconditional probability of being debt-sanctioned in the combined subsample of debt-sanctioned and diffused banks is roughly 40%.[56]

To complete the first stage of our treatment diffusion approach, we report the time evolution of the predicted probabilities of being sanctioned for each month and bank, i.e., $\widehat{Pr}\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$ for the debt- and asset-sanctioned banks, see Figure 1.9.(c)–(d). In the case of debt sanctions, we find that the median predicted probability centers around 15-20% and that the variation is rather large, from nearly 0% to 100%. For the asset-sanctioned banks, the median estimated probability is very similar, but the variation is narrower across the months. Having the predicted probabilities of being sanctioned, we are now ready to describe the second stage of our approach.

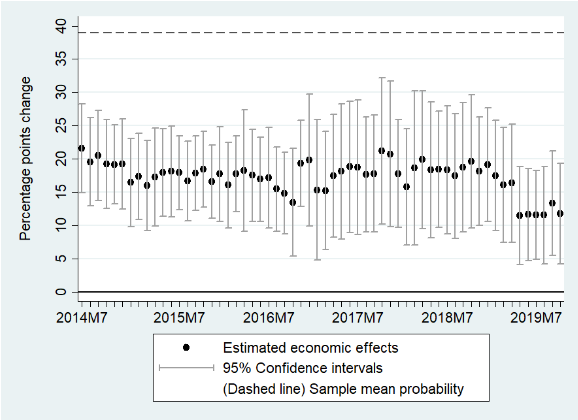
The second stage

In the second stage, we then run almost the same difference-in-differences regressions as before, except now we *extend* the treatment group (recall that the control group remains fixed, and that it does not contain politically-connected private banks). Specifically, we include a bank b in the extended treatment group if the bank b ever faced sanctions within the sample period *or* $\widehat{Pr}\{Sanctioned_{b,t} = 1 \mid X_{b,t}\} \geq \overline{Pr}$, where $\overline{Pr} = 0.02$ is set at the unconditional probability of being sanctioned in the sample. For convenience, we refer to bank b as either actually sanctioned (S), diffused (D), or never-sanctioned matched (NSM) bank. The underlying indicator variable $TREAT.DIFFUSION_b = 1$ if $b \in S$ or $b \in D$, and 0 if $b \in NSM$. The second-stage regression then reads as:

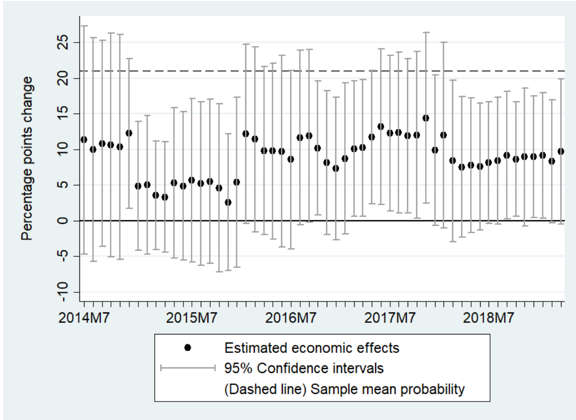
$$Y_{b,t} = \alpha_b + \gamma_t + \beta_1\Big(TREAT.DIFFUSION_b \times INFO.FIRST_t\Big) + \xi'\mathbf{X}_{b,t} + \varepsilon_{it}, \text{ if } t \in [t_1 - k, t_1 + k] \tag{1.10}$$

located *outside* the city of Moscow in the Western part of Russia.

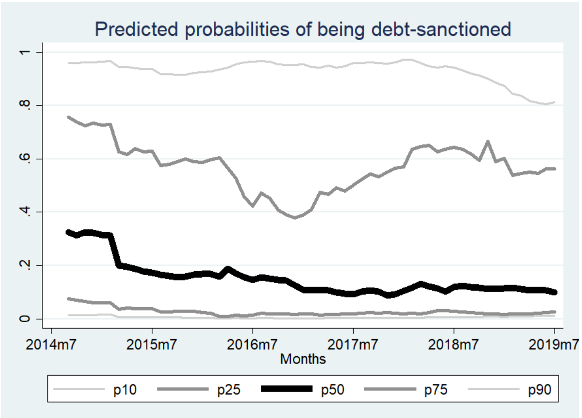
[56] In the full sample of Russian banks, the analog is 1.2% (with a standard deviation being 11 pp).



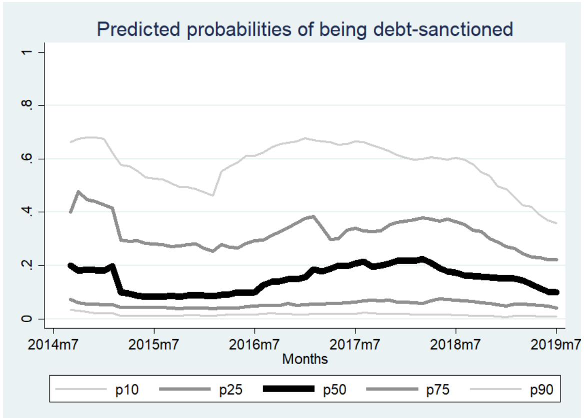
(a) Debt sanctions: Economic effects of $Gov.Share_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$



(b) Asset sanctions: Economic effects of $Gov.Share_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$



(c) Debt sanctions: predicted $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$



(d) Asset sanctions: predicted $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$

Note: The figures report the estimated economic effects of the government-connected members in banks' board of directors on the probability of being debt (a) or asset (b) sanctioned, and the predicted probabilities of being debt (c) or asset (d) sanctioned. The economic effect is computed as the product of the marginal effect of the $Gov.Share_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$ and a one standard deviation of the $Gov.Share_{b,t}$ variable. "p10" to "p90" are respectively 10 to 90%-tiles.

Figure 1.9: Post-estimation after the logit models of the probability of being sanctioned: economic effects and predictions

where t_1 is March 2014 (the first sanction announcement), and the rest of the variables are as before.

The estimation results from the second stage appear in Table 1.7. First, we obtain a positive and significant (at 5%) coefficient on the $SANCTION.DIFFUS_b \times POST.FIRST_t$ variable in Column 1 and Panel 1, as in the baseline exercise. Moreover, quantitatively, the magnitudes of the estimates for the diffused banks are very close to the actually treated banks—about a 2 pp increase in international borrowings after the first sanction announcement. As can be inferred from Column 3 and Panel 1, adding the diffused banks to the treatment group leads to a slightly lower but still significant estimate of the in-advance adaptation effects of sanctions on the not yet debt-sanctioned banks.

Second, as can be further inferred from Column 4 and Panel 2, we also obtain a negative and significant (at 5%) estimate of the coefficient on the $SANCTION.DIFFUS_b \times POST.FIRST_t$ variable, also as in the main exercise above. In this case, the effect on the diffused banks is substantially lower than on the actually asset-sanctioned banks. Further, if we add these diffused banks to our initial treatment group, we still obtain a negative and highly significant coefficient (as in the main exercise).

Table 1.7: Treatment diffusion in international operations: the estimation results from the second stage

Sanction type:	Debt sanctions			Assets sanctions		
Treatment:	Diffused	Actual	Actual + Diffused	Diffused	Actual	Actual + Diffused
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = Foreign liabilities, as % of bank total liabilities						
SANCTION.DIFFUS$_b$ × POST.FIRST$_t$	1.960**	2.138***	1.270**	0.315	–2.354***	–0.473
	(0.545)	(0.649)	(0.890)	(0.336)	(0.634)	(0.390)
N obs	2,707	2,241	4,400	2,569	3,148	4,863
N treated / control banks	13 / 54	14 / 35	27 / 100	13 / 53	16 / 59	29 / 99
R^2_{within}	0.547	0.620	0.457	0.305	0.457	0.261
Panel 2: Dependent variable = Foreign assets, as % of bank total assets						
SANCTION.DIFFUSE$_b$ × POST.FIRST$_t$	–0.721	–2.306***	–0.911*	–1.444**	–2.384***	–2.366***
	(0.682)	(0.516)	(0.554)	(0.646)	(0.786)	(0.580)
N obs	2,707	2,241	4,400	2,540	3,105	4,767
N treated / control banks	13 / 54	14 / 35	27 / 100	13 / 53	16 / 59	29 / 99
R^2_{within}	0.382	0.636	0.426	0.273	0.249	0.229

Note: The table reports the 2^{nd}-stage treatment diffusion DID estimates of the effects of sanctions on foreign liabilities (*Panel 1*) and foreign assets (*Panel 2*) of Russia's targeted banks, as implied by equation (1.10), against the background of the baseline DID estimates obtained when ignoring diffusion (columns 3 and 5). The estimation window is $k = 24$ months around the imposition of sanctions on the *Rossiya Bank* (March 2014). $SANCTION.DIFFUS_b = 1$ if a bank b either will ever face sanctions within our sample period (*actually treated*) or never faced sanctions but has a high probability of being sanctioned due to political connections (*diffused*). $POST.FIRST_t = 1$ after March 2014 and is aimed at capturing the in-advance adaptation effect. Actually sanctioned and/or diffused (i.e., treated) and never-sanctioned (i.e., control) banks are 1:4 matched within two years prior to March 2014. Diffused banks are not allowed to enter the control group. Bank FE, Month FE, Bank controls, and all necessary cross-products of the SANCTION.DIFFUS and POST.FIRST variables are included but not reported.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the sanction group level and at the level of non-sanction banks and appear in brackets under the estimated coefficients.

Third, if we were to ignore the first stage and feed all private banks with political connections to the extended treatment group, we would lose two of the four important outcomes compared to the truly two-stage approach: one on foreign assets and the other on foreign liabilities (see Appendix 1.L). This is because there are too many banks that are not only less responsive but also responsive in *opposite* directions compared to the actually treated or highly likely treated (diffused) banks.

1.5 Transmission of sanctions from banks to firms: some evidence from syndicated loan data

1.5.1 Supply of syndicated loans before and after sanctions

We now appeal to the syndicated loan deals data to answer the question of how the financial sanctions were transmitted from the targeted banks to their borrowers. Of course, a clear limitation of this analysis is that it covers only a small portion of all loans in Russia in terms of quantity. However, in terms of the volume of loans, our analysis may be rather instructive.

Indeed, using the https://www.cbonds.com data source, we reveal that there were only 126 syndicated loans issued to Russian non-financial firms by the syndicates that contained at least one Russian bank, sanctioned or not, during the three years before and the three years after March 2014, i.e., between 2011 and 2017. We intentionally limit the time window to reduce confounding effects of other events that could have occurred at the same time. These loans were issued jointly by 135 banks in total (Russian and foreign), of which 48 are Russian banks and the rest are foreign. Among the 48 Russian banks, 11 were eventually sanctioned ($SANCTION_b = 1$). The average number of Russian banks on those syndicates is three, and thus the total number of observations available for our regression analysis is roughly 335. Of course, we explicitly emphasize that the upcoming results may at best be treated as suggestive, and more research is required in the future. However, these loans are gigantic: the sum of their volumes, deflated by CPI (March 2014=100%), is equivalent to 30% of the total corporate loans in the Russian banking system.

From the borrowers' side, these 126 syndicated loans were demanded by 59 large firms in Russia, of which 16 faced sanctions from the West or Ukraine at some point ($SANCTION_f = 1$). The firms operated in 16 different industries of the economy.

With this data at hand, we specify the following difference-in-differences regression to quantify the supply-side effects of sanctions on the volume of loans:

$$
\begin{aligned}
\ln(Loan_{b(s),f,t}) = \; & \beta_1 \cdot \Big(SANCTION_b \times POST.FIRST_t\Big) \\
& + \beta_2 \cdot \Big(SANCTION_b \times POST.NEXT_{b,t}\Big) \\
& + \beta_3 \cdot \Big(SANCTION_f \times POST.FIRST_t\Big) \\
& + \beta_4 \cdot \Big(SANCTION_f \times POST.NEXT_{f,t}\Big) \\
& + \beta_5 \cdot \Big(SANCTION_b \times POST.FIRST_t \times SANCTION_f\Big) \\
& + \beta_6 \cdot \Big(SANCTION_b \times POST.NEXT_{b,t} \times SANCTION_f\Big) \\
& + \alpha_{b(g)} + \alpha_{i(f)\times t} + Controls_{b(s),f,t} + \varepsilon_{b(s),f,t}
\end{aligned}
\tag{1.11}
$$

where $Loan_{b(s),f,t}$ is the amount of loan that bank b in syndicate s provides to firm f when the contract is signed. In the absence of actual weights within the syndicates, equal weights are assumed. We split all firms on those being sanctioned or not ($SANCTION_f = 0$ or 1) to directly test the hypothesis that banks reduced the loans to not-yet-sanctioned firms. $\alpha_{b(g)}$ is the fixed effect of group g ($g = 1, 2...10$) which bank b belongs to. We cannot include a bank b's fixed effect due to a limited number of observations. We consider 12 bank groups: 10 decile groups (in terms of assets), with the top-3 banks being excluded from the 10^{th} group, and 2 more groups composed of VTB and Gazprombank (11^{th} group) and Sberbank (12^{th} group) to account for their disproportionately larger size than other banks in the 10^{th} group. These fixed effects intend to capture unobserved heterogeneity across banks in terms of their ability to establish relationships with borrowers and bargain loan contract conditions. In turn, $\alpha_{i(f)\times t}$ is the product of the fixed effect of industry i, to which firm f belongs, and the fixed effect of the month t when the loan deal was signed. These fixed effects capture the demand-driven factors determining the size of loans at the industry level. Similarly to the bank group fixed effects, we have to use firm group fixed effects instead of firm fixed effects because of data limitations. We thus effectively assume that industry- and firm-level demand for loans are identical. This is our limitation. $Controls_{b(s),f,t}$ include the log of the loan maturity, and bank-specific characteristics reflecting bank equity capital to total assets ratio (leverage), non-performing loans ratio (ability to extend new loans), net position in foreign operations (assets net of liabilities, to control for direction of bank loans), term deposits with a maturity of three and more years in total deposits (funding stability).

$\varepsilon_{b(s),f,t}$ is regression error.

Estimation results of equation (1.11) appear in Table 1.8. First, regarding the *sanctioned banks–non-sanctioned firms* relationship ($SANCTION_b = 1$ and $SANCTION_f = 0$), we obtain negative and significant coefficients in columns 1 and 2 for the US/EU sanctions and negative insignificant coefficients in columns 3 and 4 for the Ukrainian sanctions. Notably, the significant effects in columns 1 and 2 hold only when we compare before and after March 2014 ($POST.FIRST_t = 1$) but not before and after individual bank sanction dates ($POST.NEXT_{b,t} = 1$). Taken together, these estimates mean that not-yet-sanctioned banks reduced by 20% ($exp\{-0.227\} - 1$) the supply of loans to those firms that never faced even US/EU sanctions afterward (private firms). When the banks faced their sanctions, they were not reducing the supply of loans anymore: everything was adapted in advance.

Second, regarding the *never-sanctioned banks-sanctioned firms* relationship (when $SANCTION_b = 0$ and $SANCTION_f = 1$), we obtain negative and significant coefficients in columns 1 and 2 for the US/EU sanctions and insignificant coefficients in columns 3 and 4 for the Ukrainian sanctions. This again holds only in the case of before and after March 2014 ($POST.FIRST_t = 1$). After the firms face sanctions, there are nearly zero new deals in the market, and the model fails to estimate the coefficient if $POST.NEXT_{f,t} = 1$. These estimates imply that banks halted syndicated loans to those firms that have not yet faced US/EU sanctions (state-owned and -controlled entities). Anticipation matters. Reduction of the supply equals 87% ($exp\{-2.062\} - 1$).

Finally, as for the *sanctioned banks-sanctioned firms* relationship (when $SANCTION_b = 1$ and $SANCTION_f = 1$), the model fails to estimate any coefficient and we obtain no estimates of the effects of sanctions. This is because of a too small number of new deals in respective pairs, as we already encountered above.

1.5.2 Borrowing firms' performance before and after sanctions

Having established that sanctioned banks reduced the supply of (syndicated) loans to both sanctioned and non-sanctioned firms, we now explore whether this reduction negatively impacted the firms' performance. We consider the following four characteristics at the firm-year level: firm size, as proxied with the total assets, investment, employment (number of workers), and total revenue, with deflating by CPI and taking logs where appropriate. Again, we restrict the sample to the 2011–2017 period. Description of the

Table 1.8: The effects of sanctions on the supply of syndicated loans: Difference-in-differences estimates

	US + EU sanctions		Ukrainian sanctions	
	Info + Direct	Info + Direct + Controls	Info + Direct	Info + Direct + Controls
	(1)	(2)	(3)	(4)
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	–0.227**	–0.235**	–0.019	–0.017
	(0.091)	(0.116)	(0.053)	(0.068)
$\text{SANCTION}_b \times \text{POST.NEXT}_{b,t}$	–0.015	–0.011	–0.221	–0.242
	(0.074)	(0.091)	(0.165)	(0.164)
$\text{SANCTION}_f \times \text{POST.FIRST}_t$	–2.062***	–2.065**	–0.207	–0.205
	(0.391)	(0.384)	(0.639)	(0.631)
$\text{SANCTION}_f \times \text{POST.NEXT}_{b,t}$	*n/a*	*n/a*	0.230	0.286*
			(0.156)	(0.162)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{SANCTION}_f$	*n/a*	*n/a*	*n/a*	*n/a*
$\text{SANCTION}_b \times \text{POST.NEXT}_{b,t} \times \text{SANCTION}_f$	*n/a*	*n/a*	*n/a*	*n/a*
Bank control variables		Yes		Yes
log of loan $\text{maturity}_{b(s),f,t}$	2.028***	2.031***	2.028***	2.030***
	(0.622)	(0.626)	(0.630)	(0.634)
Industry × Month FE	Yes	Yes	Yes	Yes
Bank group FE	Yes	Yes	Yes	Yes
N obs	335	330	335	330
R^2	0.832	0.831	0.832	0.831

Note: The table reports the DiD estimates of the effects of sanctions on the log of syndicated loans issued by targeted, as compared to non-targeted, Russian banks. The estimation window is $[-36, 36]$ months around the imposition of sanction on the first targeted bank in Russia (March 2014). The data sample covers 126 syndicated loans issued to Russian firms by syndicates that contain at least one Russian bank, sanctioned or not; 135 banks in total (Russian and foreign), of which 48 are Russian banks and 11 are the banks that were eventually sanctioned ($SANCTION_b = 1$). The volumes of loans are deflated by CPI (March 2014=100%). The total volume of the 126 syndicated loans covers roughly 20% of the total loans in the Russian banking system. In the absence of actual weights within the syndicates, equal weights are assumed.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the bank level and appear in brackets under the estimated coefficients.

firm-level data and summary statistics are reported in Table 1.1 (see Appendix 1.K).

We specify the following difference-in-differences equation to understand how the reduced supply of loans affected firms depending on whether they had relationships with sanctioned banks or not:

$$\begin{aligned} \ln Y_{f,t} = \alpha_f + \gamma_t + \beta_1 \cdot \Big(RELATIONSHIP_{f,b} \times POST.FIRST_t \Big) \\ + \beta_2 \cdot \Big(POST.FIRST_t \times SANCTION_f \Big) \end{aligned} \tag{1.12}$$

$$+\beta_3 \cdot \Big(RELATIONSHIP_{f,b} \times POST.FIRST_t \times SANCTION_f\Big)$$
$$+ Controls_{f,t-1} + \varepsilon_{f,t}$$

where $RELATIONSHIP_{f,b}$ is a binary variable that equals 1 if firm f had a relationship with a syndicate that contained at least one ever-sanctioned bank. $Controls_{f,t-1}$ include the lagged dependent variables in the respective equation. Other notations remain the same.

Estimation results of equation (1.12) are reported in Table 1.9. First, we obtain no significant estimates of the β_1 coefficient across columns 1 to 4. This means that, despite having relationships with sanctioned banks before March 2014, never-sanctioned firms did not experience negative effects on their assets, investment, employment, or total revenue after March 2014 as compared to the never-sanctioned firms that did not have relationships with sanctioned banks.

Second, strikingly, we obtain positive estimates of the β_2 coefficient on the $POST.FIRST_t \times SANCTION_f$ variable in columns 1–3 and a negative one in column 4. This implies that sanctioned firms that did not have relationships with sanctioned banks before 2014 enjoy rising size of assets, expand employment, and increase investment after March 2014 as compared to the sanctioned firms that did have relationships with sanctioned banks. The average increase of the three characteristics reaches 41%. However, rising assets, employment, and investment did not result in a greater total revenue; instead, the firms were suffering from declining market sales—and the decline equals –16%.

And third, we obtain negative, as one would expect, and significant estimates of the β_3 coefficient on the $RELATIONSHIP_{f,b} \times POST.FIRST_t \times SANCTION_f$ variable consistently across columns 1 to 4. The underlying decline is huge being bounded between –52 to –35% over the course of 2014–2017. This indicates that, if sanctioned firms had relationships with sanctioned banks before March 2014, they experience a sharp contraction in their key characteristics.

Our results are consistent with the government-support channel of the targeted firms that led to capital misallocation that has been recently established by Nigmatulina (2022). Key firm characteristics such as investment and employment could have been supported but this never resulted in the growing market performance of the firms. In addition, our results also point to rather differential support by the government. Apparently, the government preferred to support sanctioned firms disproportionately more if they did not have relationships with sanctioned banks. Those firms that did have such relationships suffered much more.

Table 1.9: The effects of sanctions on firms that obtain syndicated loans: Difference-in-differences estimates

Dependent variable:		Firm size	Invest	Employ	Revenue
		(1)	(2)	(3)	(4)
β_1	RELATIONSHIP$_{f,b}$ × POST.FIRST$_t$	–0.059	0.217	–0.132	–0.092
		(0.143)	(0.153)	(0.264)	(0.159)
β_2	POST.FIRST$_t$ × SANCTION$_f$	0.142**	0.398*	0.463**	–0.184*
		(0.065)	(0.208)	(0.233)	(0.104)
β_3	RELATIONSHIP$_{f,b}$ × POST.FIRST$_t$ × SANCTION$_f$	–0.428**	–0.540**	–0.741**	–0.672***
		(0.204)	(0.264)	(0.352)	(0.193)
Firm controls (lag = 1 year)		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
N obs		433	389	328	408
R^2_{within}		0.480	0.056	0.519	0.460

Note: The table reports the DiD estimates of the effects of sanctions on firms that obtain loans from syndicates with at least one targeted bank as compared to firms whose syndicates had no targeted Russian banks. The estimation window is $[-3, 3]$ years around the imposition of sanctions on the first bank targeted in Russia (2014). The data sample covers 59 firmsâ€™ syndicated loans issued to a Russian firm by a syndicate that contains at least one Russian bank, sanctioned or not. *Firm size* is the log of a firm's total assets, *Invest* is the annual growth rate of a firm's fixed assets (as a proxy for investment), *Employ* is the log of the number of workers employed, and *Revenue* is the log of the total revenue of a firm.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level and appear in brackets under the estimated coefficients.

1.6 Conclusion

Financial sanctions against the largest Russian banks were imposed at different time points between 2014 and 2019, leaving room for not-yet-sanctioned banks to *anticipate punishment* and prepare in advance. Our estimates indicate that these effects of sanctions did exist and pushed banks to preemptively adjust their foreign and domestic assets and liabilities, i.e., before the sanctions hit. We then document how these primary effects of sanctions, i.e., effects on international operations, lead to secondary effects, i.e., effects on domestic assets and liabilities. We also document a treatment-diffusion effect on unrecognized state-controlled banks in Russia and propose an extension to the difference-in-differences approach that can capture this diffusion. We believe our treatment-diffusion approach could be applied in many empirical settings, either with fuzzy treatments or in which some untreated agents have non-trivial exposures to the treated agents and could thus behave similarly.

Our results shed light on how to improve the design of the sanction policy to make it more effective in the future. The key point is that any staggered policy design allows not-yet-treated banks to adapt in advance—reduce foreign assets (in fear of asset freezes) and increase (cheap) foreign borrowings. Apart from a naive recommendation to sanction the largest state-connected banks first (which is, nonetheless, absolutely correct), we suggest policymakers trace interdependencies between the potentially targeted banks (i.e., in Russia) and their partners abroad, both in Western and Eastern countries. By "partners" we mean (*i*) the subsidiaries of the targeted banks abroad (e.g., Sberbank Europe AG in the EU, Denizbank in Turkey, which were subsidiaries of Russia's largest state-controlled bank Sberbank in the 2010s, etc.) and (*ii*) domestic banks outside the sanctioned country, which have active inter-bank operations with the banks in the sanctioned country. Potential pyramids aimed at evading sanctions may involve two schemes:

1. a domestic partner bank (A) lends to its own subsidiary (B) operating in the sanctioned country, which, in turn, continues lending to a targeted bank (C);

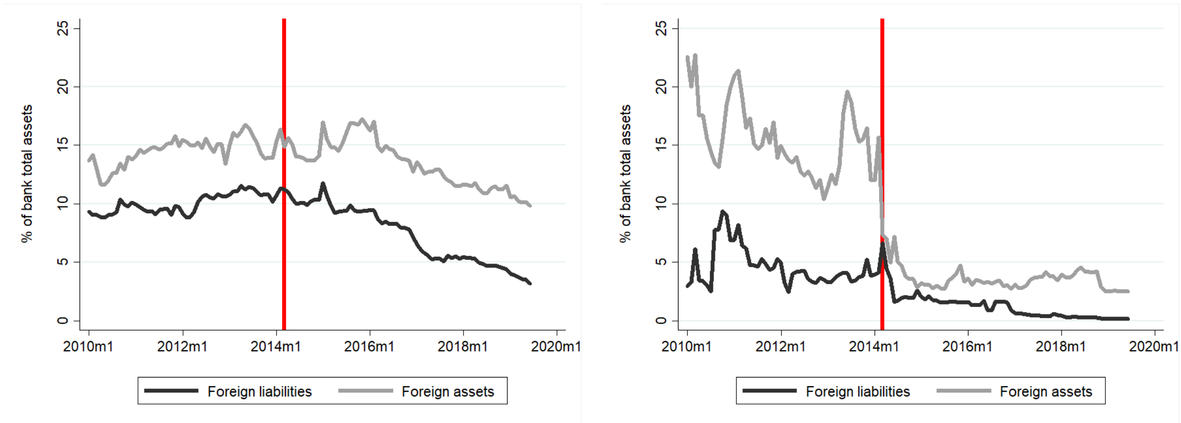
2. the domestic bank (A) first lends to a bank (D) operating in a neutral country and possessing inter-bank relationships with the sanctioned bank (C), which then transfers the funds to bank (C)

The major condition of sanction evasion in these cases is the absence of sanctions on the ability of sanctioned banks to use international payment systems such as SWIFT. Using machine-learning algorithms, it is not difficult to trace the pre-sanction intensity of cross-border operations between a (not-yet) sanctioned bank and its partner in the EU or the US. It is then easy to trace any unusual spikes in the lending intensities from the partner bank to either its subsidiary in the sanctioned country or any other neutral country: the spikes must be of the same order of magnitude as the pre-sanction volumes of loans sent to the (not-yet) sanctioned banks before. The underlying reasons why the domestic EU or US (to less extent) banks may still be willing to sustain their operations with the sanctioned banks involve: first, the high profitability of such operations (the famous case of Raiffeisenbank, which refuses to leave Russia even after the Kremlin launched the full-scale war against Ukraine in 2022) and, second, as the finance literature has generally agreed on, the importance of bank-firm and bank-bank relationships which may persist through the times (*A friend in need is a friend indeed*). Both concerns may be addressed by policymakers using various types of standard policy tools: offering domestic EU or US banks some temporary relief in terms of capital adequacy, liquidity, and FX-operations reserves. We believe that, taken together, these preemptive measures by policymakers—that is, performed before the first sanction announcement—will substantially increase the overall efficiency of sanctions against aggressive governments in the future.

1.A The list of debt- and asset-sanctioned banks

#	REGN	Name	Sanctions Type	Sanction Date	Comments	Sanctions Remain
1	2748	Bank of Moscow (OAO)	Sectoral	29Jul2014	The fifth largest bank in Russia. Controlled by VTB (see below)	1
2	1623	Bank VTB 24 (PAO)	Sectoral	22Dec2015	A subsidiary of VTB	1
3	2584	Credit Ural Bank (AO)	Sectoral	1Sep2016	A subsidiary of Gazprombank	1
4	0	Cryogenmash PAO	Sectoral		A subsidiary of Gazprombank	0
5	354	Gazprombank (OAO)	Sectoral	16Jul2014	Russia's third largest bank. Partially owned by the state	1
6	1942	Globexbank (AO)	Sectoral	30Jul2015	A subsidiary of VEB	1
7	1470	Sviaz-Bank	Sectoral	30Jul2015	A subsidiary of VEB	1
8	2546	Novikombank	Sectoral	22Dec2015	A subsidiary of Rostec state corporation	1
9	2433	Prominvestbank	Sectoral	30Jul2015	A subsidiary of VEB	1
10	2790	Roseximbank (ZAO)	Sectoral	30Jul2015	A subsidiary of VEB	1
11	3349	Russian Agricultural Bank (OAO)	Sectoral	29Jul2014	State-owned Russian bank	1
12	3340	SME Bank	Sectoral	30Jul2015	A subsidiary of VEB	1
13	3287	Russian Regional Development Bank (OAO)	Sectoral	30Jul2015	A subsidiary of Rosneft	1
14	1481	Sberbank	Sectoral	12Sep2014	Russia's largest bank. State-controlled.	1
15	2168	Setelem Bank (OOO)	Sectoral	22Dec2015	A subsidiary of Sberbank	1
16	0	Sovremennye Technologii (OOO)	Sectoral		A subsidiary of Sberbank	0
17	588	Surgutneftegazbank (AO)	Sectoral	26Jan2018	A subsidiary of Surgutneftegaz, another sanctioned entity	1
18	0	Vnesheconombank (VEB)	Sectoral		Russian state-owned development bank	0
19	1000	VTB Bank (OAO)	Sectoral	29Jul2014	The second largest Russian bank. State-controlled.	1
20	0	VTB Insurance (OOO)	Sectoral		A subsidiary of VTB Bank	0
21	328	Bank Rossiya	Entity	20Mar2014	Bank owned by several individuals from the sanctions list	1
22	3527	Black Sea Bank of Development and Reconstruction (AO)	Entity	20Jun2017	Russian commercial bank with activities in Crimea	1
23	2398	Commercial Bank North Credit (AO)	Entity	20Jun2017	Russian commercial bank with activities in Crimea	1
24	3098	Commercial Bank Rublev (AO)	Entity	20Jun2017	Russian commercial bank with activities in Crimea	1
25	2402	Evrofinance Mosnarbank (AO AKB)	Entity	11Mar2019	A Russian bank involved in transactions with Venezuela	1
26	2998	ExpoBank	Entity		Russia's 102nd largest bank	0
27	2490	Genbank (AO)	Entity	22Dec2015	A Russain bank, which operates in Crimea	1
28	2571	Inresbank (OOO)	Entity	22Dec2015	The bank is being merged into Mosoblbank	1
29	2377	Investcapitalbank (OAO)	Entity	28Apr2014	Bank controlled by the Rotenberg brothers	1
30	3175	IS Bank (AO)	Entity	20Jun2017	Russian commercial bank with activities in Crimea	1
31	3360	Krayinvestbank (OAO)	Entity	22Dec2015	A Russian bank, which operates in Crimea	1
32	1751	Mosoblbank PAO	Entity	22Dec2015	Russia's 22d largest bank	1
33	0	Mostotrest (PAO)	Entity		A major Russian construction company engaged in the development of the Kerch Bridg	0
34	2546	Novikombank	Entity	22Dec2015	A subsidiary of Rostec	0
35	1354	RNKB (OAO)	Entity	3Nov2015	Russian National Commercial Bank, operates in Crimea and allegedly controlled by th	1
36	2211	RosEnergoBank	Entity		Russia's 130th largest bank	0
37	3099	Russian Financial Corporation Bank (RFC Bank)	Entity	18Apr2018	Bank owned by Rosoboroneksport, another sanctioned entity	1
38	3368	SMP Bank (OAO)	Entity	28Apr2014	Bank controlled by the Rotenberg brothers	1
39	1317	Sobinbank (OAO)	Entity	28Apr2014	Russian bank wholly owned by Bank Rossiya	1
40	1249	TAATTA Bank (AO)	Entity	20Jun2017	Russian commercial bank with activities in Crimea	1
41	3531	TsMRBank (OOO)	Entity	20Jun2017	Russian commercial bank with activities in Crimea	1
42	1084	Verkhnevolzhsky (PAO)	Entity	22Dec2015	A Russian commercial bank, which operates in Crimea	1
43	3528	Sevastopolsky Morskoy Bank (OAO)	Entity	22Dec2015	Not listed in riskadvisory.com but appeared in treasury.gov	1
44	1093	VVB (PAO)	Entity	20Jun2017	Not listed in riskadvisory.com but appeared in treasury.gov	1

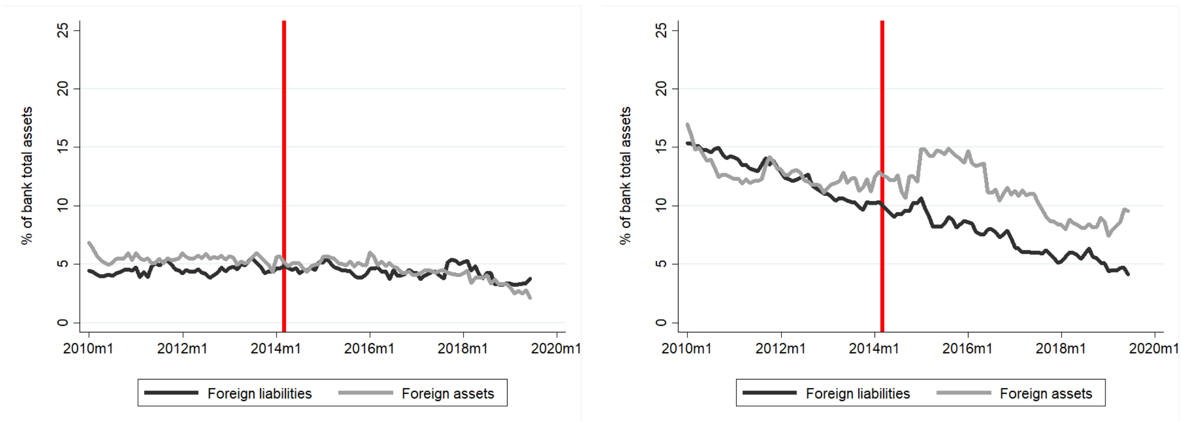
1.B Foreign operations of targeted and non-targeted banks



(a) debt-sanctioned banks (b) asset-sanctioned banks

Note: The figures report foreign liabilities (black line) and foreign assets (grey line), as % of respective total assets of banks that eventually faced debt sanctions (*a*) or assets sanctions (*b*). The red vertical red line marks March 2014—the month in which financial sanctions against Russian banks were imposed for the first time (the *Rossiya Bank*).

Figure 1.1: International operations of debt- and asset-sanctioned banks, % of total assets



(a) Never-sanctioned banks outside top-200 (b) Never-sanctioned banks inside top-200

Note: The figures report foreign liabilities (black line) and foreign assets (grey line), as % of respective total assets of banks that had never been sanctioned and either entered the top-200 bank rating in terms of asset size (*a*) or remained outside the top-200 (*b*). The red vertical red line marks March 2014—the month in which financial sanctions against Russian banks were imposed for the first time (on the *Rossiya Bank*).

Figure 1.2: International operations of large and small never-sanctioned banks, % of total assets

1.C Bank-level data: description

Table 1.1 reports descriptive statistics on the key bank operations. By columns, we present means, medians, and standard deviations for the 16 debt-sanctioned banks, 20 asset-sanctioned banks, and all the rest more than 900 unsanctioned banks over the period of 2009M1–2019M6. By rows, we have five panels of variables: panel 1 for foreign assets and liabilities; panels 2–4 for domestic liabilities, assets, and their prices, respectively; and panel 5 for bank size, equity capital, and non-performing loans.

Table 1.1: Descriptive statistics
(*at the bank-month level, from 2009M1 to 2019M6*)

Sanction type:	Debt (SSI)			Assets (SDN)			Unsanctioned		
	Mean	p50	SD	Mean	p50	SD	Mean	p50	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel 1: International operations, as % of bank total assets									
Foreign liabilities	7.39	2.36	10.71	4.09	0.67	10.25	4.48	0.02	11.17
Foreign assets	11.51	9.04	9.64	4.18	1.47	6.69	4.92	0.48	9.47
Panel 2: Domestic operations: liabilities, as % of bank total assets									
Private deposits	19.06	12.36	19.49	32.23	33.96	22.65	29.68	29.38	21.41
Corporate deposits	21.50	18.93	16.78	22.58	19.48	16.69	23.57	20.52	17.64
Inter-bank deposits	11.56	3.69	18.58	5.87	1.40	11.43	3.36	0.00	8.39
Government deposits	2.20	0.01	3.57	0.19	0.00	0.68	0.14	0.00	1.49
Central Bank deposits	3.42	0.34	5.59	1.42	0.00	3.83	1.22	0.00	4.47
Panel 3: Domestic operations: assets, as % of bank total assets									
Loans to individuals	13.45	6.57	19.27	11.89	8.13	13.21	15.69	10.88	16.00
Loans to firms	33.35	35.20	19.60	29.14	30.27	15.26	32.91	32.19	19.90
Inter-bank loans	11.41	4.94	14.81	8.80	5.51	10.80	8.79	4.23	12.19
Cash & reserves	5.63	3.86	7.03	13.85	7.55	15.94	14.74	9.21	15.22
Panel 4: Monthly expenses & returns, as % of bank total assets (*) or respective liability (**)									
Personnel expenses (*)	0.13	0.11	0.09	0.30	0.25	0.22	0.32	0.28	0.20
Average funding rate (*)	0.34	0.34	0.15	0.34	0.37	0.21	0.30	0.30	0.19
Expenses on private deposits (**)	0.38	0.43	0.21	0.54	0.59	0.30	0.52	0.57	0.29
Expenses on corporate deposits (**)	0.23	0.20	0.21	0.19	0.14	0.19	0.18	0.11	0.20
Average return rate (*)	0.65	0.64	0.19	0.65	0.66	0.26	0.75	0.75	0.27
Returns on loans to individuals	1.11	1.10	0.21	1.39	1.32	0.44	1.33	1.25	0.50
Returns on loans to firms	0.80	0.82	0.30	1.06	1.10	0.45	1.18	1.17	0.41
Panel 5: Other variables, as % of bank total assets									
Log of total assets	5.63	5.44	2.28	2.56	2.48	2.04	1.46	1.23	1.80
Equity capital	12.95	11.15	6.94	14.20	11.20	15.32	21.62	16.28	16.11
Non-performing loans	8.75	5.17	12.78	10.78	3.35	19.34	5.97	2.98	10.57

Analysis of the descriptive statistics across the three groups of banks in the 2010s shows that, on average, debt-sanctioned banks are those most dependent on foreign liabilities and most engaged in foreign asset purchases compared to the other two types of banks in Russia. As shares in total assets, both operations are approximately twice as large as those in the asset-sanctioned and unsanctioned group of banks (see Panel 1). In this respect, the debt sanctions were properly addressed. We also notice that even for the debt-sanctioned banks, both foreign assets and international borrowings are unlikely to be the major positions in their balance sheets covering about 10% of the total whereas private deposits and corporate deposits hold by about 20% of the balance sheets each. At the same time, foreign assets and liabilities of the debt-sanctioned banks are comparable with the role of inter-bank loans and deposits, respectively. Other types of attracted funds, namely, government deposits and loans from the Central Bank of Russia account for only 5.6% jointly. A similar picture applies to assets and non-sanctioned banks (see Panel 2).

As for the assets, all three groups of banks are rather similar in terms of the direction of credit, being much more specialized in corporate lending rather than granting loans to individuals. Loans to non-financial firms account for 30–35% of assets while loans to individuals take about 12 to 16%. For the rest, debt-sanctioned banks lend somewhat more in the inter-bank market and hold much fewer assets in cash and reserves than the assets- and non-sanctioned banks (see Panel 3).

What concerns expenses and returns, debt-sanctioned banks pay much lower wages to their personnel, pay less interest to private depositors but higher interest to corporate depositors, and earn less on lending to households and firms compared to the other banks (see Panel 4). Without going further into the details, these features are historically attributed to state-owned banks in Russia, with their private depositors associating the stability of these banks with the overall stability of the government (and thus supplying funds at lower rates) and with their borrowers being either among those of the highest quality in the economy (in case of Sberbank) or those politically motivated (for the rest), thus demanding loans at lower rates.

Finally, debt-sanctioned banks are the largest banks in the system being as much as two times larger than the asset-sanctioned banks, which, in turn, are 1.7 times larger than the average non-sanctioned banks. Correspondingly, the equity-to-assets ratio reverts, with the debt-sanctioned banks operating historically near the regulatory threshold and the average non-sanctioned bank being at least two times farther from the threshold. As for the non-performing loans (NPL) ratio, we observe that the NPLs of both groups of sanctioned banks are higher, not lower, than in non-sanctioned banks. Politically motivated loans are eventually less profitable, which speaks to a classical notion of government being less efficient in the economy than other economic agents La Porta, Lopez-de Silanes, and Shleifer (2002, Khwaja and Mian (2005).

1.D In-advance adaptation to sanctions: estimation results at different horizons

Table 1.1: In-advance adaptation effects of sanctions, distance to Moscow, and oil extraction:
Difference-in-differences estimates on matched samples

Sanction type:	Debt sanctions			Assets sanctions		
Estimation Window $[-k, k]$	$k = 12$	$k = 24$	$k = 36$	$k = 12$	$k = 24$	$k = 36$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = Foreign liabilities, as % of bank total liabilities						
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	1.046**	2.637***	1.865**	–1.557***	–2.944***	–2.560*
	(0.412)	(0.722)	(0.791)	(0.425)	(0.815)	(1.437)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b$	–0.515	–1.280**	–0.230	–0.310	–0.776	–0.502
	(0.364)	(0.517)	(0.653)	(0.278)	(0.537)	(0.831)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b \times \ln OIL_{r(b)}$	0.063	0.126**	0.067	0.040	0.292**	0.223**
	(0.047)	(0.057)	(0.086)	(0.056)	(0.115)	(0.110)
N obs	1,165	2,241	2,827	1,642	3,148	4,580
N treated / control banks	14 / 35	14 / 35	14 / 35	16 / 59	16 / 59	16 / 59
R^2_{within}	0.569	0.623	0.610	0.355	0.465	0.451
Mean distance (km): treated / control	284	/	904	929	/	1,183
Mean oil extrac. (mln tons): treated / control	20	/	10	0.7	/	10
Panel 2: Dependent variable = Foreign assets, as % of bank total assets						
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	–1.470**	–2.080***	–1.607	–1.243**	–2.703**	–2.727**
	(0.714)	(0.719)	(1.044)	(0.608)	(1.030)	(1.294)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b$	–0.555	–0.541	–2.445**	–0.695**	–0.829*	–1.074*
	(0.544)	(0.619)	(0.971)	(0.324)	(0.429)	(0.610)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b \times \ln OIL_{r(b)}$	–0.015	0.029	0.140	–0.018	0.056	0.147
	(0.095)	(0.072)	(0.089)	(0.086)	(0.089)	(0.094)
N obs	1,165	2,241	2,827	1,640	3,105	3,864
N treated / control banks	14 / 35	14 / 35	14 / 35	16 / 59	16 / 59	16 / 59
R^2_{within}	0.678	0.637	0.582	0.330	0.261	0.238
Mean distance (km): treated / control	284	/	904	929	/	1,183
Mean oil extrac. (mln tons): treated / control	20	/	10	0.7	/	10

Note: The table reports the DID estimates of the effects of sanctions on foreign liabilities (*Panel 1*) and foreign assets (*Panel 2*) of Russia's targeted banks, as implied by equation (1.3). The estimation Window is $k = 24$ months around the imposition of sanctions on the *Rossiya Bank* (March 2014). Sanctioned (i.e., treated) and never-sanctioned (i.e., control) banks are 1:4 matched within two years prior to March 2014. Private banks with political connections are not allowed to enter the control group. Bank FE, Month FE, Bank controls, and All necessary cross-products of the TREAT, POST.FIRST, DISTANCE, and OIL variables are included but not reported.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the sanctioned group level and at the level of each non-sanctioned bank and appear in brackets under the estimated coefficients.

Table 1.2: In-advance adaptation to sanctions: Maturity disaggregation of foreign liabilities

Sanction type:	Debt sanctions			Assets sanctions		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = Foreign liabilities with maturity ≥ 3 years, as % of bank total liabilities						
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	0.694*	1.368***	1.645***	–1.701***	–1.633***	–1.819***
	(0.350)	(0.489)	(0.501)	(0.620)	(0.598)	(0.619)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b$		–1.448***	–1.543***		–0.070	–0.780**
		(0.522)	(0.445)		(0.131)	(0.359)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b \times \ln OIL_{r(b)}$			–0.038			0.164
			(0.0504)			(0.103)
N obs	2,241	2,657	2,241	3,148	3,148	4,148
N treated / control banks	14 / 35	14 / 35	14 / 35	16 / 59	16 / 59	16 / 59
R^2_{within}	0.503	0.516	0.522	0.414	0.414	0.420
Panel 2: Dependent variable = Foreign liabilities with maturity $\in [1, 3)$ years, as % of bank total assets						
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	0.391	0.677	0.836	–1.769***	–1.998***	–2.134***
	(0.355)	(0.464)	(0.503)	(0.638)	(0.602)	(0.641)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b$		–0.400	–0.320		0.144	–0.644*
		(0.504)	(0.514)		(0.168)	(0.370)
$\text{SANCTION}_b \times \text{POST.FIRST}_t \times \text{DISTANCE}_b \times \ln OIL_{r(b)}$			–0.067			0.174*
			(0.056)			(0.101)
N obs	2,241	2,657	2,241	3,148	3,148	4,148
N treated / control banks	14 / 35	14 / 35	14 / 35	16 / 59	16 / 59	16 / 59
R^2_{within}	0.588	0.592	0.597	0.412	0.414	0.421

Note: The table reports the DID estimates of the effects of sanctions on foreign liabilities with a maturity of 3 years and more (*Panel 1*) and foreign liabilities with a maturity between 1 and 3 years (*Panel 2*) of targeted Russian banks, as implied by equation (1.3). The estimation window is $k = 24$ months around the imposition of sanctions on the *Rossiya Bank* (March 2014). Sanctioned (i.e., treated) and never-sanctioned (i.e., control) banks are 1:4 matched within two years prior to March 2014. Private banks with political connections are not allowed to enter the control group. Bank FE, Month FE, Bank controls, and all necessary cross-products of the TREAT, POST.FIRST, DISTANCE, and OIL variables are included but not reported.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the sanctioned group level and at the level of non-sanctioned banks and appear in brackets under the estimated coefficients.

1.E In-advance adaptation vs. added value effects of financial sanctions: pooling sanction types

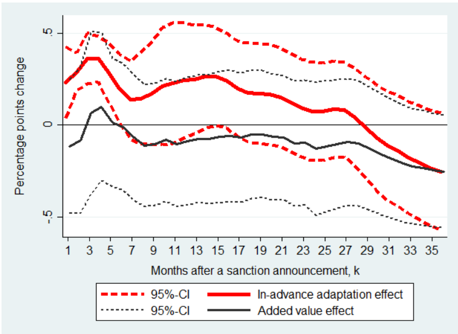
Table 1.1: In-advance adaptation vs. added value effects of sanctions: Difference-in-differences estimates on a pooled sample of debt and assets sanctions

Sanction type:	Debt + assets sanctions		
Estimation Window $[-k, k]$	$k = 12$	$k = 24$	$k = 36$
	(1)	(2)	(3)
Panel 1: Dependent variable = Foreign liabilities, as % of bank total assets			
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	0.018	0.650	1.360
	(0.603)	(0.870)	(0.991)
$\text{SANCTION}_b \times \text{POST.NEXT}_{b,t}$	–1.571*	–1.049	–1.881*
	(0.933)	(0.918)	(0.952)
Bank FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Bank control variables	Yes	Yes	Yes
N obs	4,523	7,622	10,142
N treated / control banks	33 / 97	33 / 97	33 / 97
R^2_{within}	0.260	0.262	0.271
Panel 2: Dependent variable = Foreign assets, as % of bank total assets			
$\text{SANCTION}_b \times \text{POST.FIRST}_t$	–0.432	–0.531	–0.778
	(0.740)	(0.676)	(0.659)
$\text{SANCTION}_b \times \text{POST.NEXT}_{b,t}$	–2.962**	–1.883**	–1.767**
	(1.236)	(0.899)	(0.777)
Bank FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Bank control variables	Yes	Yes	Yes
N obs	4,523	7,622	10,142
N treated / control banks	33 / 97	33 / 97	33 / 97
R^2_{within}	0.156	0.122	0.124

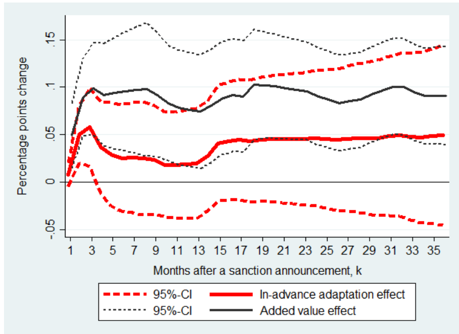
Note: The table reports the DID estimated effects of sanctions on international assets and foreign borrowings of Russian banks, as implied by Equation (1.3). The estimation Window is $[-k, k]$ month around the imposition of sanction on the *Rossiya Bank* (March 2014) joined with $[-k, k]$ month around the imposition of sanction on a bank j ($j \neq$ bank "Rossiya") from either the debt sanction list or assets sanction list. Sanctioned and non-sanctioned bank groups are matched within two years prior to March 2014.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the sanctioned group level and at the level of non-sanctioned banks and appear in brackets under the estimated coefficients.

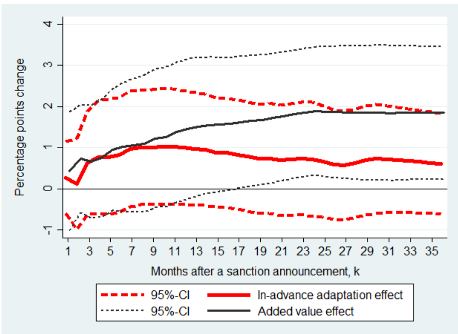
1.F The effects of sanctions on other domestic operations of Russian banks



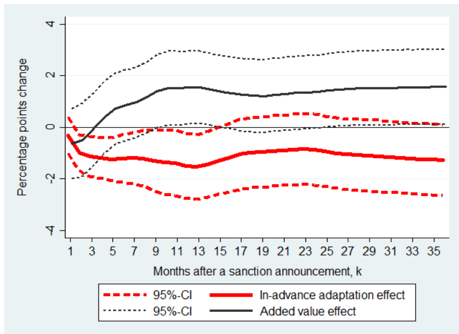
(a) Government deposits, *debt*-sanctioned banks



(b) Government deposits, *asset*-sanctioned banks



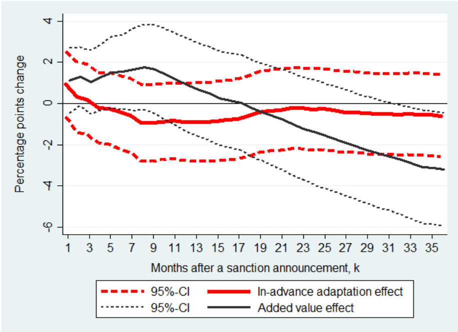
(c) Central Bank deposits, *debt*-sanctioned banks



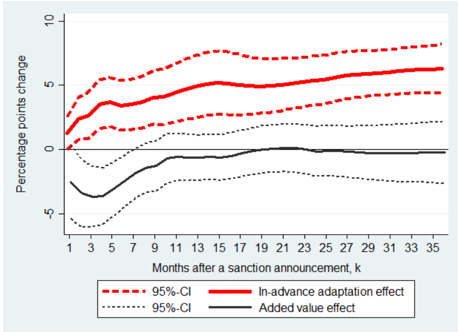
(d) Central Bank, *asset*-sanctioned banks

Note: The figures report the staggered difference-in-differences estimates of the coefficients on $SANCTION_b \times POST.FIRST_t$ and $SANCTION_b \times POST.NEXT_{b,t}$ in equation (1.8), with the dependent variable reflecting either government deposits or loans from the Central Bank of Russia (as % of bank total liabilities). The estimates are obtained by running the staggered DID with in-advance adaptation on the expanding window $[t_b - k, t_b + k]$, where $k = 1, 2...36$ months after either bank-specific sanction date (added value effects, black lines) or the date of sanctions against the *Rossiya Bank* (in-advance adaptation effects, red lines).

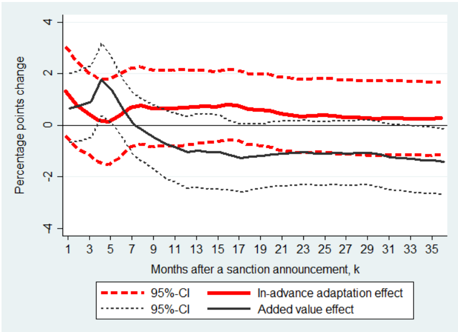
Figure 1.1: What happened with other domestic bank liabilities after sanctions? (*by sanction type*)



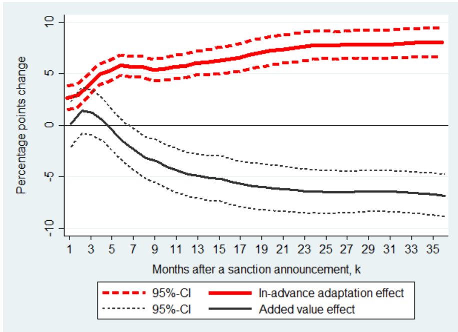
(a) Inter-bank loans, *debt*-sanctioned banks



(b) Inter-bank loans, *asset*-sanctioned banks



(c) Cash and reserves, *debt*-sanctioned banks



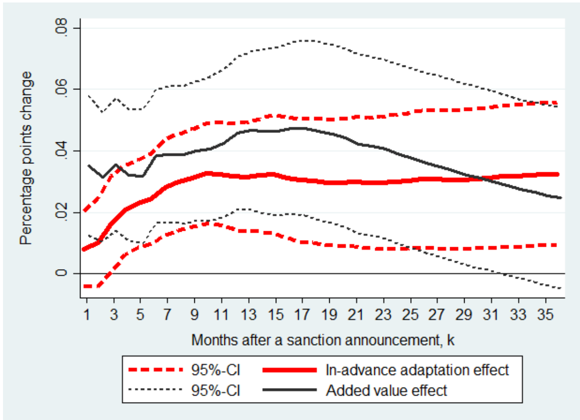
(d) Cash and reserves, *asset*-sanctioned banks

Note: The figures report the staggered difference-in-differences estimates of the coefficients on $SANCTION_b \times POST.FIRST_t$ and $SANCTION_b \times POST.NEXT_{b,t}$ in equation (1.8), with the dependent variable reflecting either the loans granted through the inter-bank market or banks' holdings of cash and reserves (as % of bank total assets). The estimates are obtained by running the staggered DID with in-advance adaptation on the expanding window $[t_b - k, t_b + k]$, where $k = 1, 2...36$ months after either bank-specific sanction date (added value effects, black lines) or the date of sanctions against the *Rossiya Bank* (in-advance adaptation effects, red lines).

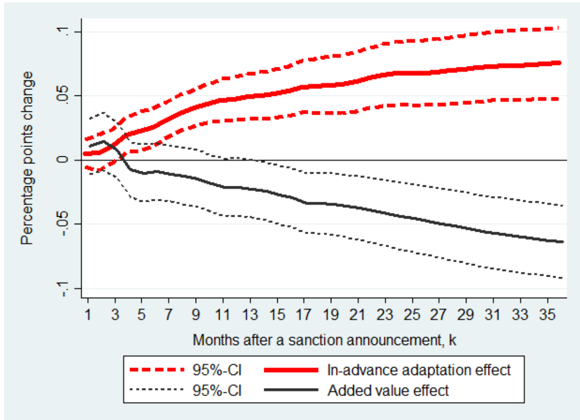
Figure 1.2: What happened with other domestic bank assets after sanctions? (*by sanction type*)

1.G (Effective) interest rates: pay more on deposits, return less on loans?

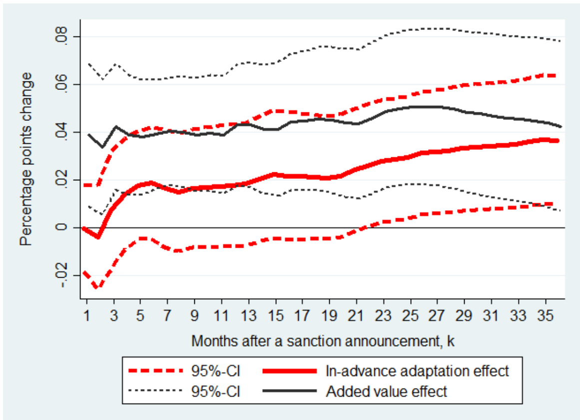
In this section, we consider changes in the prices of bank assets and liabilities, as measured by effective interest rates, after the banks had to adapt to upcoming sanctions and deal with the existing sanctions. The estimation results appear in Figure 1.1 below.



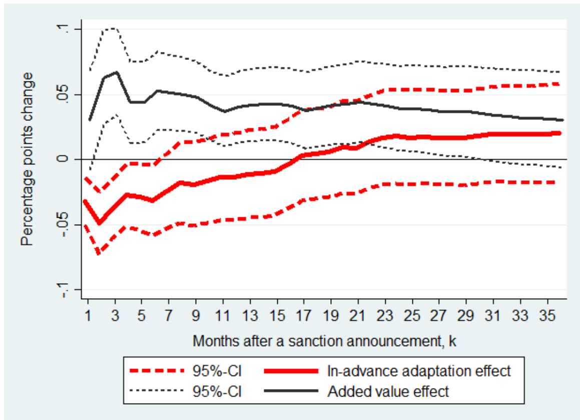
(a) Average funding rate, *debt*-sanctioned banks



(b) Average funding rate, *asset*-sanctioned banks



(c) Average return rate, *debt*-sanctioned banks



(d) Average return rate, *asset*-sanctioned banks

Note: The figures report the staggered difference-in-differences estimates of the coefficients on $SANCTION_b \times POST.FIRST_t$ and $SANCTION_b \times POST.NEXT_{b,t}$ in equation (1.8), with the dependent variable reflecting either the average funding rate (monthly, as % of total liabilities) or average return rate (monthly, as % of total assets). The estimates are obtained by running the staggered DID with in-advance adaptation on the expanding window $[t_b - k, t_b + k]$, where $k = 1, 2...36$ months after either bank-specific sanction date (added value effects, black lines) or the date of sanctions against the *Rossiya Bank* (in-advance adaptation effects, red lines).

Figure 1.1: The effects of sanctions on effective interest rates, *by sanction type*

First, for the average funding rate, the estimated in-advance adaptation effects imply that not yet debt-sanctioned banks were forced to significantly increase the interest rate offered on household deposits and other sources of funds (see Figure 1.1.a), presumably to cope with the depositors' run on the banks. After the sanctions hit, the already debt-sanctioned banks had to significantly raise their average funding rate again, likely because of another wave of depositors' withdrawals, as we found in the previous sections. Therefore, the supply-side effects are indeed more likely than the demand-side. Conversely, for asset-sanctioned banks we obtain evidence favoring demand-side factors: before the sanctions, the banks raised their interest rates on deposits simultaneously with raising the volume of deposits attracted from customers (see Figure 1.1.b). After the sanctions, the already asset-sanctioned banks reduced their interest rates and faced an outflow of deposits.

Second, for the average return rate on the banks' assets, we find that in most cases, the targeted banks were able to grow profits from lending to firms and households and owning other assets they were allowed to keep on their balance sheets (see Figures 1.1.c,d). On the one hand, increased lending to households, which we established in the previous sections, could have been rewarded by growing returns. On the other hand, declining lending to firms could have negatively contributed to the returns. But the overall effect is positive for both debt- and asset-sanctioned banks, as our results here indicate.

1.H SVAR-analysis

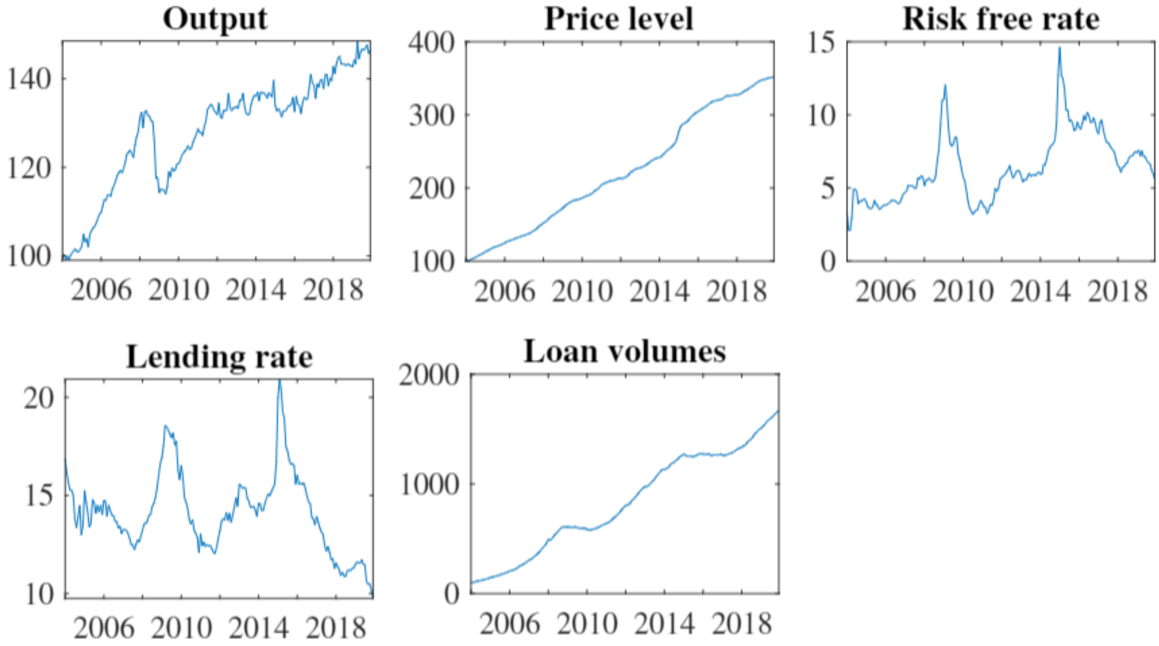
Methodology. We aggregate the microeconomic effects of sanctions obtained using the difference-in-differences approach to the macroeconomic level by means of the SVAR model with 5 endogenous variables, namely, output, CPI inflation, risk-free interest rate, composite bank lending rate, and bank loan volumes to the economy, along the lines of Gambetti and Musso (2017). We follow the authors' sign restriction scheme and identify loan supply shock by a set of on-impact restrictions, in which the lending rate reacts negatively and loan volumes react positively to an expansionary loan supply shock, and output, prices, and risk-free rate adjust upward to the same shock. In order to make sure we deal with loan supply shock, we simultaneously identify three additional shocks — monetary, aggregate demand (AD), and supply (AS).

We make one more step and follow the narrative sign restrictions approach of Antolin-Diaz and Rubio-Ramirez (2018) and specify December 2014 as a period of commonly accepted restrictive monetary policy shock in Russia. During the "black Monday" of December 15, the Central Bank of Russia raised the key interest rate from 10.5 to 17%, which could trigger loans decline in the economy. We account for this concern.

Macroeconomic data for SVAR analysis. In our SVAR model, we use monthly data on output, CPI inflation, risk-free rate, composite lending rate, and the volume of loans to households and non-financial firms (see Figure 1.1). The data come from the Rosstat and CBR official databases, as discussed in the Introduction.

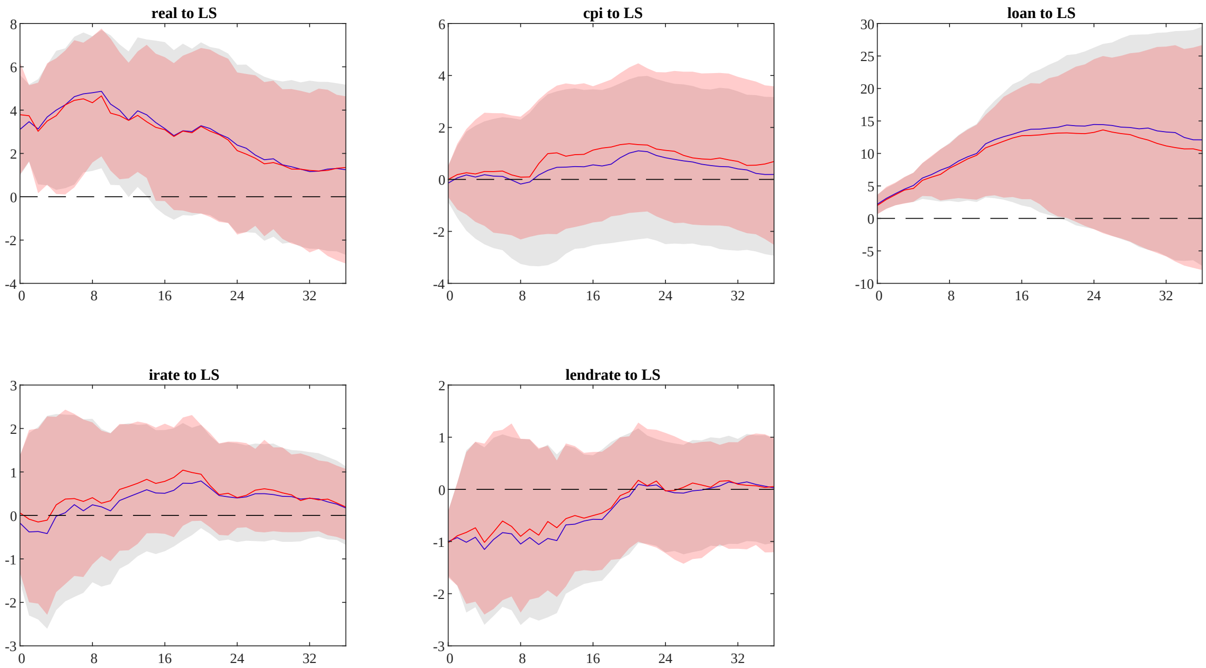
What can be inferred from the data is that output has grown 1.5 times over the last 15 years, exhibiting strong cyclical features (especially before the global financial crisis of 2007–2009) and clearly slowed down since the recession of 2014–2015. Prices during the same period more than tripled. Loan volumes substantially outpaced the growth of output and prices, having increased by approximately 17 times. This is a typical feature of emerging economies. Risk-free and lending rates vary considerably between 5 to 15% and 10 to 20% per annum, respectively, also exhibiting strong pro-cyclical features.

Impulse response functions (IRFs). Figure 1.2 below reports the estimated IRFs to the positive credit supply shock, in which we normalize the lending rate on-impact reaction to –1 pp (per annum). What we observe is that output reacts positively (as we defined through the sign restriction scheme) until at least the 15th month after the shock, with the on-impact response equaled +3.2 to +3.9 pp (under the "SR" and "SR+NSR" schemes, respectively). Loan volumes also react positively until at least the 20th month after the shock, so that the on-impact response is +2.1 pp (under both schemes). We infer from these two last estimates that the implied on-impact elasticity of output with respect to loan volumes is bounded between 1.52 and 1.86, which is comparable, though larger, with those obtained in Gambetti and Musso (2017) for developed countries.



Note: The figures show the data inputs to our SVAR analysis, in levels. Base indices are normalized to 100 as of January 2004. Interest rates are in per cents. *Output* reflects the index of basic economic activities. *Price level* corresponds to the consumer price index. *Loan volumes* stand for the amount of bank loans outstanding. *Risk-free rate* is short-term government bond yields, which proxies the policy rate. *Lending rate* is a weighted average of the lending rates on loans of different maturities.

Figure 1.1: Time evolution of selected real and financial characteristics of the Russian economy



Note: The figures present the estimated impulse response functions (IRFs) to the credit supply (CS) shocks identified in the 5-variables SVAR with either one or two sign restriction schemes imposed. The first (SR) follows the sign restriction scheme used to identify credit supply shocks in Gambetti and Musso (2017). The second one (NSR), the narrative sign restrictions, as introduced by Antolin-Diaz and Rubio-Ramirez (2018), implies considering December 2014 as a period of negative (restrictive) monetary policy shock in the Russian economy. The blue line indicates the case in which only SR is considered. The red line represents the case in which both SR and NSR are in place. The confidence bands are defined as the range bounded by the 16^{th} and 84^{th} percentiles of distribution constructed from the successful draws from the posterior. The X-axis shows the months after the CS shock. IRFs are normalized so that the lending rate reacts by -1 pp on impact. Finally, the IRFs for output, CPI, and loan volumes are cumulative, i.e., they represent the effects of shocks on the sum of one-month log-differences from period -1 to t, i.e. $log(y_t) - log(y_{-1})$.

Figure 1.2: Impulse response functions to the identified credit supply shock (CS)

1.I The state-controlled banks unrecognized by the West

What is the profile of these 35 state-connected banks unrecognized by the West and how do they compare to the actually treated 44 banks? Recall that, due to data limitations, we have bank-level data on 33 out of the 44 sanctioned banks. The 16 banks that are at the intersection of the 33 actually treated and the 55 state-controlled banks from Karas and Vernikov (2019) are primarily the debt-sanctioned banks. The $33 - 16 = 17$ banks that are actually treated but are *not* in the Karas and Vernikov (2019) list are predominantly asset-sanctioned banks. We thus have three subgroups of banks: (i) 17 asset-, (ii) 16 debt-sanctioned banks, and (iii) 35 uncovered banks to which we refer as *potentially diffused* banks. We report comparative summary statistics on the mean size of total assets and relative size of international operations as before-and-after $t^* =$ *March 2014* in Table 1.1 below.

Table 1.1: Summary of treated and potentially diffused banks

	Treated & Not state (N=17)			Treated & State (N=16)			Not treated & State (N=35)		
	$t \leq t^*$	$t > t^*$	Diff	$t \leq t^*$	$t > t^*$	Diff	$t \leq t^*$	$t > t^*$	Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total assets (TA)	36	154	118	1,313	2,924	1,611	77	256	179
Foreign liabilities, % TA	2.6	2.7	0.1	9.6	7.2	–2.4	4	3.5	–0.5
Foreign assets, % TA	6.5	3.5	–3	12.5	8.1	–4.4	5	4.2	–0.8

Note: $t^* =$ *March 2014*, the date of the first sanction announcement. TA is measured in billion Rubles. "Treated" stands for actually sanctioned banks. "Not treated" denotes potentially diffused banks. "State" implies a bank is in the Karas and Vernikov (2019) list of state-controlled banks.

The descriptive data presented in Table 1.1 clearly shows why it is important to account for treatment diffusion. First, we observe that the defined 35 potentially diffused banks (columns (7)–(9)) are *larger* in terms of total assets than the 17 asset-sanctioned banks (columns (1)–(3)). This eliminates the concern that these banks are too small to pay attention to. Of course, they are much smaller than the 16 debt-sanctioned banks (columns (4)–(6)). Second, the 35 potentially diffused banks have non-trivial portions of international operations on their balance sheets, which are comparable to those of the 17 asset-sanctioned banks. This in turn eliminates a concern that these banks could have not been targeted because they had nearly zero international operations. Of course, again the ratios of their foreign assets and liabilities in total assets are well below those observed in the debt-sanctioned banks.[57] Finally, we also observe that these 35

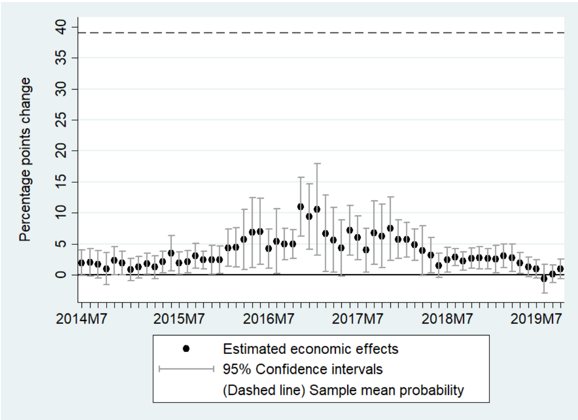
[57] But this is likely to be again a reflection of their lower size compared to the "Big-4" (i.e., lower size

potentially diffused banks *decreased* their international operations after March 2014, as the other two subgroups of banks. Of course, these reductions cannot be fully attributed to the in-advance adaptation in the anticipation of upcoming sanctions,[58] but we argue the evidence favors this view. At least, we can say that these banks were unlikely, *on average*, to expand the international operations of their hidden owners who actually faced sanctions.

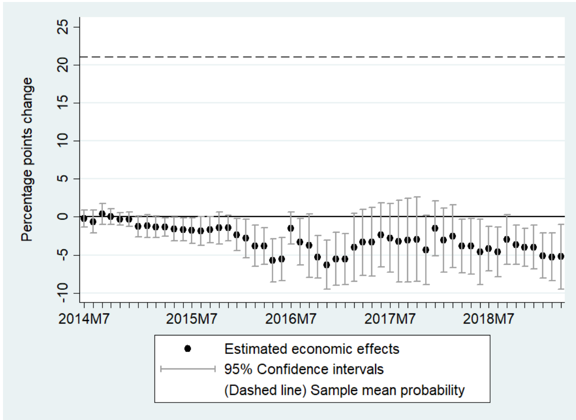
— less diversified activities).

[58]Recall the Russian economy entered a recession driven by the negative oil price shock at the same time.

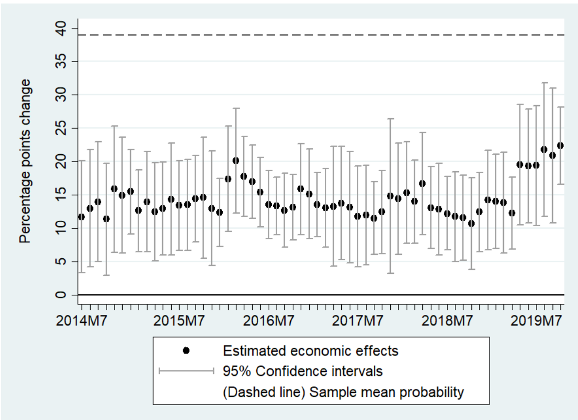
1.J Bank size and foreign assets exposures as predictors of upcoming sanctions



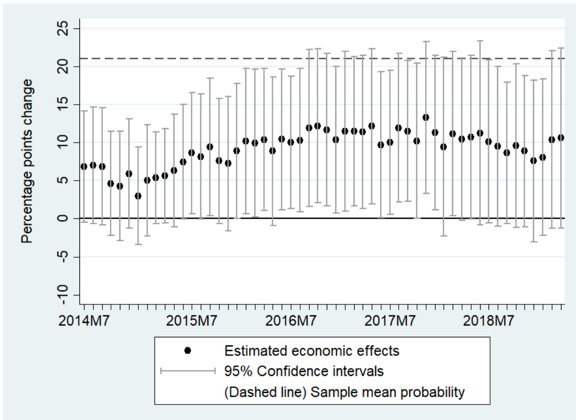
(a) Debt sanctions: Economic effects of $Foreign.Asset_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$



(b) Asset sanctions: Economic effects of $Foreign.Asset_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$



(c) Debt sanctions: Economic effects of $Bank.Size_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$



(d) Asset sanctions: Economic effects of $Bank.Size_{b,t}$ on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$

Note: The figures report the estimated economic effects of banks' holdings of foreign assets and bank size on the probability of being debt (a,c) or asset (b,d) sanctioned. The economic effect is computed as the product of the marginal effect of one of the two variables on $Pr\{Sanctioned_{b,t} = 1 \mid X_{b,t}\}$ and one standard deviation of respective variable. The marginal effects are computed after running the logit models of the probability of being sanctioned, as implied by equation (1.9). "p10" to "p90" are respectively 10 to 90%-tiles.

Figure 1.1: Economic effects of foreign asset holdings and bank size

1.K The sample of non-financial firms borrowing from the syndicated loan market in Russia

We retrieve firm-level data from the SPARK-Interfax database for the 2011 to 2017 period.[59] We require firms to simultaneously have non-missing non-negative values on total assets, total revenue, capital, number of employees and wages, and bank and non-bank borrowed funds. In addition, we only leave in the sample firms that operated for at least three consecutive years. The final sample comprises 7,460 large and small firms, resulting in roughly 40,000 firm–year observations.[60] The firms operate in as many as 16 different sectors of the Russian economy (two-digit classification) ranging from natural resources extraction to IT. The table below contains all necessary descriptive statistics at the firm level.

Table 1.1: Descriptive statistics at the firm level, 2011–2017

	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
log of real total assets	493	4.1	2.5	–9.1	10.2
Real fixed assets' growth rate	389	0.2	0.7	–0.9	5.0
log of the number of workers	415	–0.4	2.4	–6.9	5.6
log of real total revenue	466	3.0	2.7	–8.0	11.0
Whether operates after March 2014	531	0.67	0.47	0	1
Whether has relationship with sanctioned bank	531	0.71	0.45	0	1
Whether firm is sanctioned	531	0.07	0.25	0	1

[59] See https://spark-interfax.com/.

[60] The initial sample consists of roughly 300,000 firms. The substantial decline in the number of firms is caused by many missing values on the employees' and wages' data in the firms' balance sheets and the requirement to work for at least three years in a raw.

1.L Additional results on treatment diffusion

Table 1.1: Treatment diffusion in international operations: the estimation results using all politically-connected banks

Sanction type:	Debt sanctions			Assets sanctions		
Treatment:	Diffused	Actual	Actual + Diffused	Diffused	Actual	Actual + Diffused
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = Foreign liabilities, as % of bank total liabilities						
$\text{SANCTION.DIFFUS}_b \times \text{POST.FIRST}_t$	1.309***	2.138***	1.230***	0.953***	–2.354***	0.355
	(0.404)	(0.649)	(0.348)	(0.342)	(0.634)	(0.315)
N obs	6,254	2,241	7,690	6,431	3,148	8,837
N treated / control banks	36 / 115	14 / 35	50 / 133	36 / 115	16 / 59	52 / 161
R^2_{within}	0.281	0.620	0.255	0.113	0.457	0.119
Panel 2: Dependent variable = Foreign assets, as % of bank total assets						
$\text{SANCTION.DIFFUSE}_b \times \text{POST.FIRST}_t$	–0.732*	–2.306***	–0.694	–0.416	–2.384***	–0.937**
	(0.393)	(0.516)	(0.442)	(0.441)	(0.786)	(0.430)
N obs	6,254	2,241	7,690	6,337	3,105	8,676
N treated / control banks	36 / 115	14 / 35	50 / 133	36 / 115	16 / 59	52 / 160
R^2_{within}	0.292	0.636	0.312	0.144	0.249	0.146

Note: The table reports the treatment diffusion DID estimates of the effects of sanctions on foreign liabilities (*Panel 1*) and foreign assets (*Panel 2*) of Russia's targeted banks, as implied by equation (1.10), against the background of the baseline DID estimates obtained when ignoring diffusion (columns 3 and 5). The estimation Window is $k = 24$ months around the imposition of sanctions on the *Rossiya Bank* (March 2014). $SANCTION.DIFFUS_b = 1$ if a bank b either will ever face sanctions within our sample period (*actually treated*) or never faced sanctions but has recognizable political connections, small or large (*diffused*). The 1^{st} stage does not apply in this case because we use all politically-connected banks, i.e., not only those with high connections. $POST.FIRST_t = 1$ after March 2014 and is aimed at capturing the in-advance adaptation effect. Actually sanctioned and/or diffused (i.e., treated) and never-sanctioned (i.e., control) banks are 1:4 matched within two years prior to March 2014. Diffused banks are not allowed to enter the control group. Bank FE, Month FE, Bank controls, and All necessary cross-products of the SANCTION.DIFFUS and POST.FIRST variables are included but not reported.

***, **, * indicate that a coefficient is significant at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the sanction group level and at the level of non-sanction banks and appear in brackets under the estimated coefficients.

Chapter 2

The price of war: macroeconomic and cross-sectional effects of sanctions on Russia

Co-authored with Anna Pestova (CERGE-EI)

Published as CERGE-EI Working Paper Series No 756.
Earlier version was published as BOFIT Discussion Paper Series No 13/2019.

2.1 Introduction

Since World War II, more than 100 countries have faced economic sanctions imposed by the West with the aim of shifting the countries' unfavorable political regimes by damaging the local economies without launching full-scale wars (Levy 1999; Etkes and Zimring 2015; Felbermayr et al. 2020). Although there is a consensus in the literature that sanctions are seldom effective in shifting political regimes, there is strong evidence that the non-financial firms targeted by sanctions are forced to substantially reduce their international and local trade (Crozet et al. 2021) and decrease employment (Ahn and Ludema 2020). However, there are many ways to evade sanctions. As, e.g., Efing, Goldbach, and Nitsch (2023) show, German banks ceased direct lending to the sanctioned firms, but at the same time, the local demand for credit had been caught up by the German banks' subsidiaries in respective sanctioned countries. In addition, as Mamonov, Pestova, and Ongena (2021) and Nigmatulina (2022) find, the impact of sanctions on targeted banks and firms could be substantially softened if the sanctioned governments have accumulated sufficient buffers in terms of, e.g., the central bank's international reserves and are able to support the economy. Given potential spillovers between sanctioned and non-sanctioned economic agents, however, it is less clear what the overall economic implications of sanctions are for the sanctioned countries. In this chapter, we take a broader perspective and estimate the aggregate effects of sanctions on the macroeconomy and identify the cross-sectional effects of sanctions on different parts of the population (rural vs. city, rich vs. poor) and firms (more vs. less productive, large vs. small).

For this purpose, we examine the case of Russia which provides a valid laboratory to study the effects of sanctions across time because of the three major waves of sanctions sequentially imposed on the country over the last decade. First, following the annexation of the Crimean peninsula by Russia in early 2014, the US, EU, and other Western countries introduced financial and non-financial sanctions on Russian officials, government-owned companies, and banks to restrict their abilities to borrow from abroad, invest in foreign assets, develop international trade, and attract advanced technologies.[1] Second, Western countries extended existing and launched a new set of international financial restrictions in 2017 in response to Russia's interference in the US presidential election of 2016, including cyber-attacks, and military activities that were supporting the Assad regime in Syria (Welt et al. 2020). Third, after Russia's full-scale invasion of Ukraine in February 2022, Western countries introduced an unprecedented set of blocking sanctions, including freezes of half of the Russian Central Bank's international assets (USD 311 billion), private and corporate asset freezes, a ban on state-owned and politically connected privately-held banks from using the SWIFT international payment system, a full or partial ban on Russia's imports and exports, among others (Berner, Cecchetti,

[1] In general, the sanction regime includes financial restrictions (Mamonov, Pestova, and Ongena 2021), trade bans (Ahn and Ludema 2020), travel restrictions and asset freezes imposed on specific Russian officials and business people, an embargo on arms and related materials (including dual-use goods and technologies), and restrictions on technology specific to oil and gas exploration and production.

and Schoenholtz 2022). In these circumstances, we have clear timing of the sanctions imposition and a large variation in the strength of the underlying sanctions shock across time within one country.

We capture the *financial sanctions shock* by negative innovations to international credit supply (Mian, Sufi, and Verner 2017; Ben Zeev 2019) using standard macroeconometric tools such as structural VARs. We then examine the *overall sanctions shock* by applying a high frequency identification (HFI) approach. With the use of HFI, we introduce a sanctions news shock. We extract this type of shock using daily changes in the yield-to-maturity of Russia's US dollar-denominated sovereign bonds shortly before and after the OFAC/EU announcements of each and every new portion of international restrictions on Russia's officials, state-owned or connected businesses.[2] The difference between the sign restrictions estimates and HFI estimates thus captures the effects of non-financial—trade, technological, etc.—sanctions.

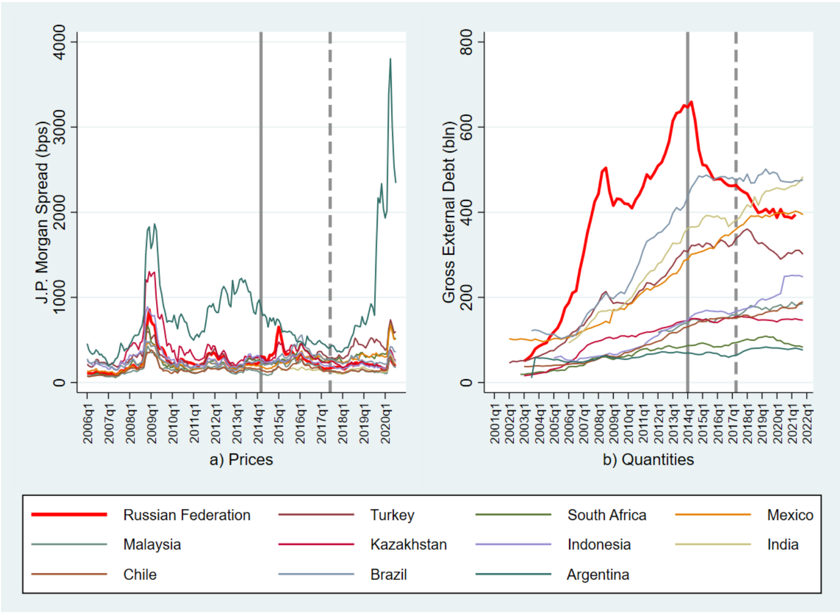
During each of the three waves of sanctions, raw data on the Russian economy reveals a rising country spread, as the price of international credit, and declining amounts of external debt, as the quantity of international credit. For example, during the first wave of sanctions in 2014 Russia's country spread, as measured by the J.P. Morgan Emerging Markets Bond Spread (EMBI+), spiked by roughly 500 bps (Fig. 2.1.*a*) while the amount of Russia's gross external debt slumped by about USD 103 billion (or by 20%, Fig. 2.1.*b*).[3] Similar but much less dramatic events occurred in 2017, i.e., during the second wave of sanctions. These events in Russia during the first two waves of sanctions are consistent with the supply-side story.[4] Importantly, the raw data also eliminates any concerns that

[2]OFAC—Office of Foreign Assets Control, a division of the US Department of the Treasury responsible for administering of sanction imposition.

[3]In 2013, prior to the first wave of sanctions, the ratio of external debt to GDP amounted to 32%, meaning that the Western financial markets were crucial for Russia. 90% of the total amount of external debt was owed by the corporate sector—banks and non-financial companies. In 2014–2015, the external debt of Russia's banks fell by almost 40% and that of non-financial companies declined by 20%. Notably, 2014 and subsequent years were the first in the Russian market economy's history in which the country's corporate external debt was not rising, except for the global economic crisis of 2007–2009 when it declined by 6%.

[4]Additional exercises with the raw data show that the first two waves of sanctions were unlikely to transmit to the Russian economy through the demand (on foreign borrowings) channel. First, the Central Bank of Russia's statistics on net foreign debt positions of different economic agents on the eve

rising spreads and declining amounts of external debt were common trends across different emerging market economies.

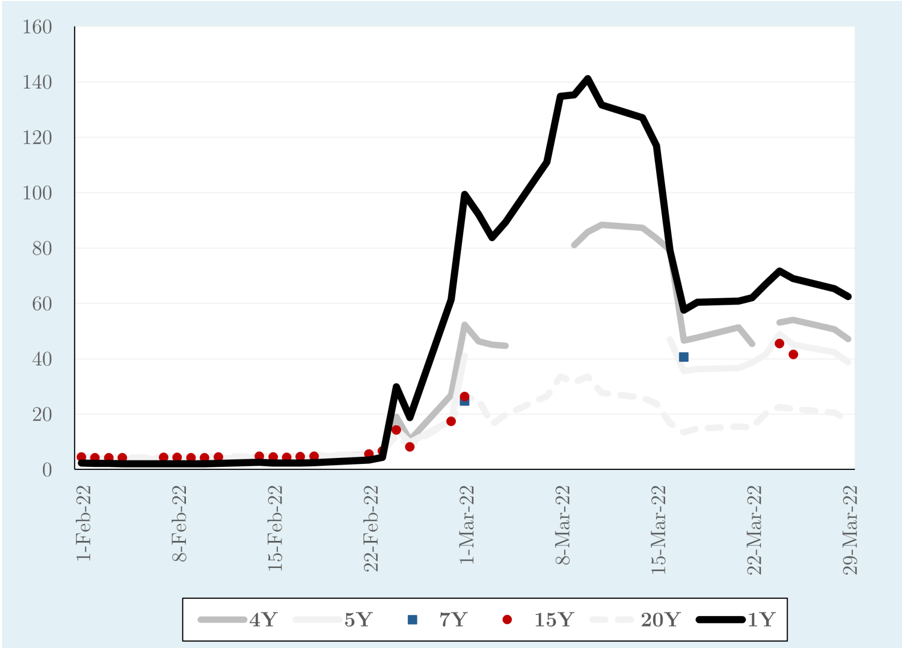


Notes: The figure plots the time evolution of the price of corporate external debt, as measured by the J.P. Morgan spread (*a*), and the amount of the debt outstanding (*b*) for Russia and other emerging market economies over the last 15-20 years. The solid vertical line marks the beginning of the first wave of sanctions against Russia (2014Q1) and the dashed vertical line reflects the start of the second wave (2017Q2).
Source: World Bank/IMF QEDS (Quarterly External Debt Statistics), J.P. Morgan.

Figure 2.1: *The first and second waves of sanctions*: Corporate external debt in Russia in the context of other emerging economies

As for the third wave of sanctions, data is limited because the Russian government closed access to it, but we can zoom in on the daily data on Russia's country spread during the first days of the war in Ukraine in February–March 2022. Clearly, Russia has experienced the most dramatic rise in the price of international borrowings in its history: the country's sovereign spread soared by 3,500–4,500 bps on average across the debts of different maturity (see Fig. 2.2).

of and two years after 2014 clearly show that private foreign assets had barely changed over the years (see Appendix 2.A for further details). Second, Russian banks' balance sheet data indicates that, as of February 2014, i.e., on the eve of the first wave of sanctions, the share of (not yet) asset-sanctioned banks' foreign asset holdings in total foreign assets of the Russian banking system was just 2%, thus limiting substantially the concerns regarding potential asset freezes by Western governments. On the contrary, the share of (not yet) debt-sanctioned banks' foreign debts in total foreign borrowings of the Russian banking system was substantial equaling 63%.



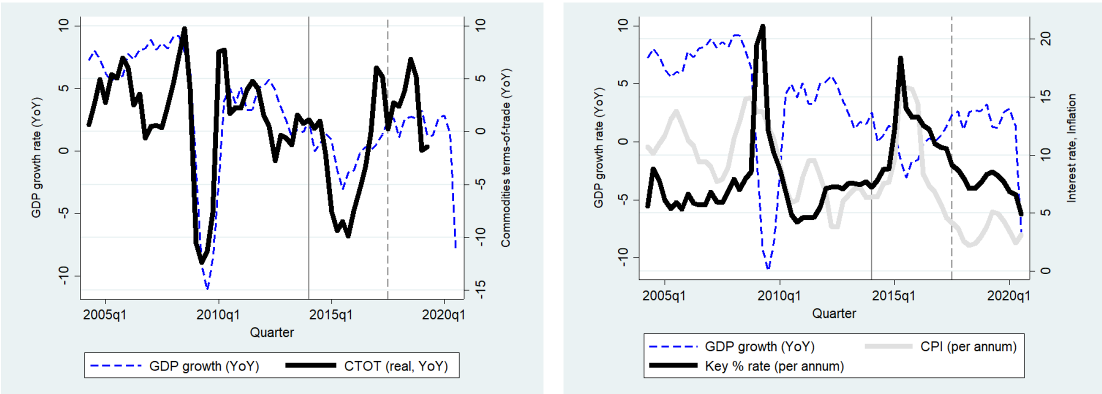
Note: The figure plots the daily data on the yields to maturity of Russia's US dollar-denominated government bonds of different maturity before and during the first weeks of the war in Ukraine in February–March 2022.
Source: Bloomberg.

Figure 2.2: *The third wave of sanctions*: Soaring Russian sovereign spreads during the first weeks of the invasion of Ukraine in 2022

Despite the clear timing of the sanctions, we, however, encounter certain confounders on the way to estimating the precise effects of the international restrictions on Russia. The first wave of sanctions in 2014 coincided with a *dramatic oil price drop*—from around USD 100 a barrel for Urals crude in the summer of 2014 to under USD 50 a barrel at the start of 2015. This had largely contributed to the observed total decline in the commodities terms-of-trade (CTOT) for Russia that amounted to –10% over that period (see Fig. 2.3.a).[5] As a result, Russia's ruble lost 90% of its value, the price of imported goods soared and consumer price inflation in the country spiked from 6 to 11% during 2014. In these circumstances, the Bank of Russia turned from soft to tight monetary policy and *raised the regulated interest rate* from 5.5 to 17% over the same period (see Fig. 2.3.(b)). In contrast, the subsequent expansion of financial sanctions in 2017–2018

[5]The Russian economy is highly dependent on revenue from oil and gas exports. Oil, oil products, and gas represented 50 to 70% of Russian goods exports in various years (see, e.g., Korhonen and Ledyaeva 2010 and Cespedes and Velasco 2012).

(the second wave) and the sanctions of 2022 (the third wave) coincided with an increase in oil prices and soft monetary policy, thus also confounding attempts to disentangle the effects of sanctions. We therefore aim to evaluate the effects of sanctions *net* of oil price fluctuations and endogenous monetary policy responses to rising prices.



(a) Commodities terms-of-trade (CTOT)　　(b) Regulated interest rate in Russia

Note: The figure reports the time evolution of commodities terms-of-trade (YoY, %) and the (nominal) Key interest rate of the Central Bank of Russia (%).

Figure 2.3: Confounders of sanctions: commodities terms-of-trade and monetary policy responses in Russia

We begin our empirical analysis by employing a structural VAR approach to model the Russian economy. The baseline VAR model encompasses the following sets of variables. First, following the literature on small open economies (Uribe and Yue 2006; Akinci 2013; Ben Zeev, Pappa, and Vicondoa 2017) we include domestic production, final consumption, investment, trade balance, the country's interest rate spread, corporate external debt, and real effective exchange rate (REER). Second, to control for the sanctions' confounders, we include a domestic regulated real interest rate (domestic monetary policy) and a set of exogenous variables—CTOT, the US corporate bond (Baa) spread, and the real US interest rate (global monetary policy).

Using monthly data from January 2000 to December 2018, we run the VAR model and estimate the residuals. We then apply the sign restrictions approach to isolate innovations to *international credit supply* (ICS; see, e.g., Cesa-Bianchi, Ferrero, and Rebucci 2018; Ben Zeev 2019; di Giovanni et al. 2021) from the estimated residuals. We require Russia's country spread to rise and the amount of corporate external debt to decline on impact in the baseline version (and within several months in robustness). To distinguish between supply and demand-side forces, we also isolate innovations to the demand on international credit by forcing the price and quantity variables to change in the same direction. We then plot the time evolution of our ICS shock, and we show that it contains substantial spikes in 2014, i.e., during the first wave of sanctions against Russia. These spikes are the largest after those that our ICS shock variable exhibits for the period of the 2007–9 global economic crisis. By contrast, no visible jumps are observed for the second wave of sanctions in 2017–2018.[6] These results clearly imply that we can use the variation in the estimated ICS shock to evaluate the effects of financial sanctions on the Russian economy in the 2010s.

However, before doing so, we provide microeconomic evidence favoring our sign restriction approach to back up the ICS shock. We employ data on syndicated loan deals in Russia from January 2011 to December 2017. The data contains information on the amount of loan, interest rate, currency, and maturity, as well as the structure of the underlying syndicate, thus allowing us to analyze the loan contracts between the borrowers—firms or banks, which are either sanctioned after 2014 or never-sanctioned—and their lenders, i.e., the banks that could also be either sanctioned or not. The data covers roughly 128 deals, which result in 335 observations and is thus not large in terms of quantity but is extremely large in terms of the volume of loans, being equivalent to nearly 30% of the Russian banking system's total loans to firms. By controlling for industry*month fixed effects, we run a difference-in-difference regression to isolate the supply effects before and after the Crimea-related sanctions.[7] We show that the syndicates with at least one sanctioned bank *reduced* the volume of loans by 72% and charged 1.4 pp *higher* interest rate on those loans after 2014 and as compared to the syndicates without sanctioned

[6]As Mamonov, Pestova, and Ongena (2021) find, there was a great deal of in-advance adaptation of international operations, including placing new debts, between 2014 and 2017 by not-yet-sanctioned banks in Russia. This could lower the potential strength of the second wave of sanctions, given that these sanctions were nothing new but an extension of the previous ones.

[7]The data is rather limited so that applying firm*month fixed effects is not feasible.

banks. The results thus clearly support the sign restriction approach we apply for our VAR analysis.

Having established the effects of financial sanctions that pertain to the ICS shock, we then consider a wider range of sanctions and employ an HFI approach. In the first stage, we run a regression of Russia's country spread on daily changes in the yield-to-maturity of Russia's US dollar-denominated sovereign bonds that occur *shortly before* sanction announcements. We show that there is an informational leakage: news on upcoming sanctions appears several (at least three) days before the official announcements. Exactly with this timing, we obtain the strongest positive coefficient at the first stage. In the second stage, we then run Jorda (2005)'s local projection (LP) approach to predict the effects of the sanctions news shock on the chosen domestic macroeconomic variables in a three-year horizon.

With the SVAR-based ICS shock estimates and the HFI-based estimates of the sanctions news shock, we then quantify the overall macroeconomic effects of each of the three waves of sanctions. Our computations at the monthly frequency show that the industrial production in Russia declines by 1.2% due to the financial sanctions shock and by 4.8% due to the overall sanctions shock cumulatively over 2014–2015. The effects of the second wave are 0 and minus 0.7% in 2017–2018, respectively. And the effects of the third wave are much more pronounced: minus 12% and 18%, correspondingly. Turning from monthly to quarterly frequency and assuming 0.67 elasticity of GDP to industrial production (linear regression estimate, significant at 1%), these numbers imply that Russia's real GDP could have lost up to –3.2% in response to the first wave of sanctions, –0.5% as a result of the second wave, and up to –12% during the third wave of sanctions (the largest decline in the Russian economy since the collapse of the USSR in 1991). We emphasize that our approach is intended at only capturing the potential of the sanction impact itself, not the net effect of sanctions by the West and the counter-sanction response by Russia. Overall, our results reveal that conditional on the scope of international financial restrictions, (a) the *financial* sanctions can have substantial *real* implications for the economy, and (b) the strength of the overall sanctions shock is much larger than that of the financial sanctions shock.

We then investigate the cross-sectional implications of sanctions for the representative samples of households and firms in Russia. The idea is that sanctions can hit disproportionately more: (*a*) richer households in larger cities as compared to poorer households in rural areas, and (*b*) more productive and larger firms as compared to less productive and smaller firms. We retrieve data on roughly 5,000 households across Russia from the survey database RLMS-HSE, which has been collected by the Higher School of Economics since 1994, and the data on 7,460 firms from the SPARK-Interfax database from 2012 to 2018.

Households. Using Jorda's LP approach, we show that, in a year after sanctions (as proxied with a negative ICS shock), the real income of richer households declines by 1.5% if residing in regions' capital cities, and by 2.0% if living everywhere else (larger towns, smaller towns, or rural areas). Strikingly, poorer households enjoy rising real income during the first year after the shock: +1.2% if in regions' capital cities, and +1.1% if everywhere else. These estimates control for CTOT and domestic monetary policy and are consistent with the observation that, during crisis times, the Russian government supports first those parts of the population that are more likely to re-elect it during the next electoral cycle. The government support channel is consistent with micro evidence from Mamonov, Pestova, and Ongena (2021) and Nigmatulina (2022). However, as our estimates suggest, this government help is not enough: in two to three years after the shock, the real income of the poorer households starts to decline, which offsets the growth during the first year after the shock hits.

Firms. First, we apply a popular methodology to estimate firm-level TFPs put forward by Wooldridge (2009) and Petrin and Levinsohn (2012) and employed in many studies that followed (e.g., Gopinath et al. 2017). Second, using Jorda's LP approach, we find that during the first year after sanctions (a negative ICS shock), the real total revenue of large firms with high TFPs declines by 2.2%. This is equivalent to 16% of these firms' overall decline in revenues, controlling for CTOT and monetary policy. For large firms with low TFPs, the effect of the sanctions peaks two years after the shock, reaching –4% (or 29% of the overall decline in revenues for these firms). This clearly shows that productivity matters in softening the effects of sanctions. Conversely, we estimate that the sanctions could have caused no larger than a 1% decline in the real total revenue of small firms with low TFPs and literally zero effect on small firms with high TFPs. This clearly suggests that smaller firms in Russia were much less affected by the sanctions than larger firms.

The contribution of this chapter is fourfold. First, we introduce the sanctions news shock based on the HFI approach. In contrast to Laudati and Pesaran (2021), who build a sanctions news intensity index and employ it in a VAR model to quantify the effects of sanctions on the Iranian economy, we suggest a two-stage procedure that exploits time variation in the yield-to-maturity of Russia's bonds around the sanctions announcements by OFAC/EU (*first stage*) and then uses this variation to capture the effects of sanctions (*second stage*). The idea of the sanctions news shock is inspired by the oil news shock, as embedded in OPEC's announcements, which has been recently introduced by Kanzig (2021a).

Second, our study contributes to the literature on the economic effects of sanctions. While the few existing *macroeconomic* studies focus on specific variables—Russia's ruble exchange rate in Dreger et al. (2016) or GDP growth rates in Barseghyan (2019)—our study is the first to provide a broader picture by covering a larger set of variables describing the real economy, domestic monetary policy, financial sector, and international trade. Dreger et al. (2016) exploit a cointegrated VAR and establish that the drop in oil prices in 2014 had a greater effect on the ruble dynamics than the sanctions. In turn, Barseghyan (2019) uses the synthetic control method and estimates the effects of sanctions to be 1.5% of annual GDP over the 2014–2017 period. In contrast to these studies, we use the concept of negative ICS shocks to estimate the effects of sanctions, which has a clear counterpart in the data, at both macro- and syndicated loan levels. We show that the channel of ruble depreciation is exactly the corporate debt de-leveraging due to sanctions, and we also show that GDP decreases in response to sanctions because consumption and investment fall together by more than the trade balance rises. In addition, we analyze time variation in the effects of sanctions across the three waves that occurred in 2014 after Crimea's annexation, in 2017 after the cyber-attacks in the US, and in 2022 after Russia invaded Ukraine, whereas the mentioned studies focus solely on Crimea's sanctions. Finally, Gutmann et al. (2021) apply an event-study approach in a cross-country setting and reveals that the sanctions lead to a 2.2% decline in consumption and a 24% decline in investment. Our estimates for consumption are larger, but are much more conservative for investment.

Third, by quantifying the cross-sectional implications of sanctions, we also contribute to the *microeconomic* studies on sanctions (Besedes, Goldbach, and Nitsch 2017; Efing, Goldbach, and Nitsch 2023; Belin and Hanousek 2021; Ahn and Ludema 2020; Felbermayr et al. 2020; Crozet et al. 2021; Mamonov, Pestova, and Ongena 2021). While most of these studies focus on the effect of sanctions on targeted firms or banks after receiving treatment as compared to non-targeted banks and firms, we study the effects of sanctions on different parts of the population and firms. As Ahn and Ludema (2020) show, the Crimea-related sanctions forced targeted firms in Russia to reduce employment by 33% and led to a decline in total revenues by 25%. We, in turn, show that these effects are likely to be concentrated within a group of larger firms with higher levels of TFPs, whereas smaller firms with lower levels of TFPs were unlikely to be affected by the sanctions. Regarding the effects on households, Neuenkirch and Neumeier (2016) find that the sanctions lead to a rising poverty gap, which is very persistent over time. Our results for the cross-section of Russian households open a different angle regarding the effects of sanctions: we show that the real income of richer households is affected negatively by the sanctions, whereas that of poorer households grows first and then declines. An unintended consequence of the sanctions could be a reduction in economic inequality, conditional on the sanctioned government's support for the poorest.

Fourth, our results imply that credit supply shocks matter for the macroeconomy even after controlling for endogenous monetary policy responses. Schularick and Taylor (2012) and Mian, Sufi, and Verner (2017) establish a negative long-run effect of credit on output in the US and other major advanced countries. However, Brunnermeier et al. (2021) criticize these and related works for the absence of the monetary policy reaction to rising prices in the reduced-form equations used to establish the result. In our setting with the financial sanctions as episodes of negative (international) credit supply shocks, we show that Russia's industrial production declines by 1.78% in the VAR model containing domestic regulated interest rate and by 1.95% if the model would omit the interest rate variable (as in the previous literature). The price of omitting the accommodative effect of domestic monetary policy is thus significant but not very large.

The chapter is structured as follows. Section 2.2 describes the timing and types of sanctions. Section 2.3 discusses the methods: composition of the VAR model, sign restriction approach to capture the effect of sanctions, micro-level evidence for the sign restriction approach (syndicated loan deals), and the sources of data. Section 2.4 presents the macroeconomic estimates of the effects of sanctions and Section 2.5 further investigates the cross-sectional implications of sanctions. Section 2.6 contains our final remarks.

2.2 Timing of sanctions on Russia

The first wave of sanctions began in 2014 in response to the Russo-Ukrainian conflict: the annexation of Crimea, and the Russian support for separatist movements in Eastern Ukraine. These sanctions were imposed by the US in coordination with the EU and targeted the same entities (Welt et al. 2020). This allows us to focus on the timing of the US sanctions only. These sanctions are administered by the Treasury Department's Office of Foreign Assets Control (OFAC) and are divided into two groups: those blocking foreign assets of Specially Designated Nationals and Blocked Persons (SDNs) and those prohibiting lending, investment, and trading with entities on the Sectoral Sanctions Identifications (SSI) list. The latter—also called *sectoral sanctions*—is the primary object of our interest in this chapter because they effectively reduced the foreign borrowing capacity of Russian companies and banks.

The US Ukraine-related sanctions date back to March-December 2014 (executive orders 13660, 13661, 13662, and 13685; see Welt et al. 2020). As of 2022, before Russia launched a full-scale war in Ukraine, the sectoral sanctions remained in place and applied to new equity issuance and the loans of various maturities (more than 14-day for entities in the financial sector, more than 60-day lending for the energy sector, and more than 30-day lending for the defense sector). By 2022, OFAC included 13 Russian companies and banks and their 276 subsidiaries on the SSI list. The parent entities list includes the four largest state-owned banks, one development bank, seven major oil, gas, and pipeline companies, and one state-owned defense company.[8]

The second wave of sanctions dates back to 2017–2018 and was introduced in response to illicit cyber-enabled activities, electoral interference, and support for Syria. These sanctions were mostly imposed by the US with less support from the European Union (Welt et al. 2020). In August 2017, the US passed the Countering America's Adversaries Through Sanctions Act (CAATS), which included the Countering Russian Influence in Europe and Eurasia Act of 2017 (CRIEEA). The latter, among other measures, strengthened Ukraine-related sanctions and established several new sanctions. In particular, CRIEEA targeted a further reduction of foreign lending to the Russian financial and energy sectors. The new package also introduced mandatory sanctions (previously discretionary) against foreign financial institutions involved in "undesirable" transactions (weapons transfers, oil projects) with Russian entities, thus more strongly reducing Russian access to external financial infrastructure.

The third wave of sanctions appeared in February 2022 as President Putin's troops invaded the territory of Ukraine. As is widely discussed by Berner, Cecchetti, and Schoenholtz (2022), the sanctions were of unprecedented size and scope: roughly half of the total

[8] VTB Bank, Gazprombank, Rosselkhozbank, VEB, Rosneft, Gazpromneft, Transneft, Novatek, Rostec, Lukoil, Surgutneftegaz, and Gazprom.

international assets of the Central Bank of Russia (CBR) were frozen, private and corporate financial and real assets in Western countries were frozen, state-owned banks that were previously under less strict sectoral sanctions now faced fully blocking sanctions, many banks—including the largest privately-held—faced sanctions and were banned from using the SWIFT international payment system, and Russia's export and import operations were substantially banned. During the first weeks after these 'tsunami' sanctions, the financial sector in Russia seemed paralyzed with massive bank runs and the depreciation of the nominal exchange rate from 75 to roughly 140 rubles per dollar. However, CBR raised the interest rate from 9.5 to 20% and imposed various forms of capital controls. Ultimately, financial stability was restored within a month after the war started. However, as of the date of writing this text, the situation in the real sector of the economy remains highly uncertain due to the massive destruction of supply chains, Western corporate exodus, and concerns about Eastern countries (India, China, etc.) directly substituting Russia's lost imports.

In these circumstances, we need an empirical tool to quantify the effects of the financial and non-financial sanctions and analyze their variation in time, depending on the strength of the shock.

2.3 Methodology and data

2.3.1 A VAR model of the Russian economy

We perform our empirical exercises using vector autoregressive models (VARs). We consider the following (standard) VAR process with n variables and p lags:

$$y_t = A_1 y_{t-1} + ... + A_p y_{t-p} + u_t \tag{2.1}$$

where $y_t = (y_{1t}, y_{2t}, \ldots, y_{nt})'$ is a column vector containing the values of n variables at time t. Each matrix A_k comprises all unknown coefficients of each variable y_t taken with a lag j ($j = 1 \ldots p$) and thus has $n \times n$ dimension. $u_t = (u_{1t}, u_{2t}, \ldots, u_{nt})'$ is a column vector with reduced-form residuals, which are assumed to be normally distributed with a zero mean and covariance matrix $E(u_t u_t') = \Sigma_u$ of $n \times n$ size, $u_t \sim N(0, \Sigma_u)$.

Following Uribe and Yue (2006), Akinci (2013), and Ben Zeev, Pappa, and Vicondoa (2017), we include foreign and domestic variables in our VAR model. We consider three variables in the foreign block: commodity terms of trade (CTOT), the US corporate bond (Baa) spread, and the real US interest rate. *CTOT* captures movements in commodity exports that are crucial for Russia. Oil, gas, and their products account for 63% of total exports, and their exports to GDP ratio is as high as 27% (2010-2016 average). Further, numerous studies find that changes in world financial conditions are important for emerging economies. Early literature focused on the role played by world interest rates (Neumeyer and Perri 2005; Uribe and Yue 2006). However, a more recent study by Akinci (2013) finds that the contribution of world interest rates to business cycle fluctuations in emerging economies could be negligible—the major force is global financial shocks. Following these studies, our VAR model includes both the *Baa spread* as a measure of global financial risks[9] and *the real interest rate in the US economy* as a proxy for the world risk-free interest rate.

The composition of the domestic variables block builds upon the real sector variables that have theoretical counterparts in the real business cycle models, e.g., Neumeyer and Perri (2005), Garcia-Cicco, Pancrazi, and Uribe (2010), Chang and Fernandez (2013). We include *industrial production* (IP) as a proxy for domestic output, *private consumption* (C), *investment* (I), *trade balance* (TB)—all in constant rubles. We also include JP Morgan's EMBI+ *country spread* for Russia to proxy for the price of international borrowings in Russia (S)[10] and the *outstanding amount of Russia's corporate external debt* to capture the quantity of international borrowings in Russia (D, in US dollars, deflated by US CPI). Both S and D are central for the identification of the sanctions shock (see

[9]Another popular measure, the VIX index provided by CBOE, reflects global financial volatility and is also employed in the literature. We use this variable instead of the Baa spread in the robustness section.

[10]J.P. Morgan Emerging Markets Sovereign Bond Spread, EMBI+.

Section 2.3.3). Following recent studies by Ben Zeev, Pappa, and Vicondoa (2017) and Monacelli, Sala, and Siena (2023), we additionally include the *real effective exchange rate* (REER), which transmits the terms of trade shocks to the domestic economy.[11] Finally, we also consider the *regulated interest rate* in Russia (RIR, in real terms) to capture endogenous monetary policy responses to the sanctions shock. Although the inclusion of this variable is not directly dictated by the literature we follow, we argue that this is clearly important for our purposes. As Brunnermeier et al. (2021) show, omitting the regulator's reaction to economic shocks biases substantially the estimated effects of the shock and can thus deliver a misleading conclusion.

Ultimately, the vectors y_t and u_t can be represented as:

$$y_t = \Big[CTOT_t,\, RIR_t^{US},\, Baa_t^{US},\, IP_t,\, C_t,\, I_t,\, TB_t,\, D_t,\, S_t,\, REER_t,\, RIR_t\Big]' \tag{2.2}$$

$$u_t = \Big[u_t^{CTOT},\, u_t^{RIR^{US}},\, u_t^{Baa^{US}},\, u_t^{IP},\, u_t^{C},\, u_t^{I},\, u_t^{TB},\, u_t^{D},\, u_t^{S},\, u_t^{REER},\, u_t^{RIR}\Big]' \tag{2.3}$$

where variables 1–3 reflect external conditions (foreign block) and variables 4–11 internal conditions of the Russian economy (domestic block). To ensure that domestic variables do not affect external conditions, we impose the small open economy restrictions by setting to zero the coefficients on variables 4–11 in the equations in which variables 1–3 are dependent variables.

We estimate the VAR model (2.1) using the Bayesian methods in a framework suggested by Antolin-Diaz and Rubio-Ramirez (2018). The usage of the Bayesian methods is justified for the following reasons. First, reliable macroeconomic time series on the Russian economy cover at most the last two decades after the transformation and sovereign default crises of the 1990s (Svejnar 2002) and thus are relatively short—even if we consider monthly frequency. The Bayesian methods are shown to work well in the presence of short time series, by formulating a prior distribution of unknown parameters, and are widely exploited in the literature on macroeconometric forecasting (Koop and Korobilis

[11] Domestic production and absorption, and sectoral composition (though we do not consider sectoral outputs, to keep the model short).

2010; Banbura, Giannone, and Reichlin 2010; Koop 2013; Carriero, Clark, and Marcellino 2015). Second, as we discuss below, we employ sign restrictions to isolate the sanctions shocks after estimating the VAR model. As argued by Kilian and Lutkepohl (2017), the sign restrictions perform much better and are thus usually implemented under the Bayesian framework.

Since the Bayesian methods are appropriately designed for the models with nonstationary time series, we specify the VAR model (2.1) in *levels* instead of deviations from respective HP-trends. In the robustness section, we nonetheless compare the results obtained with the HP-detrended time series.

However, within the Bayesian methods, we apply only the *flat* (i.e., uninformative) prior to escape subjectivity that pertains to other forms of the priors (e.g., Minnesota, inverted-Wishart, etc.). In the baseline estimates, we set $p = 2$ months.[12]

2.3.2 The data

We collect monthly data on each of the 11 variables entering the VAR model (2.1) and listed in vector y_t (2.2). We focus on the period from January 2000, i.e., after the sovereign default crisis of the late 1990s, to December 2018, i.e., at least a year and a half after the second wave of sanctions on Russia (see Section 2.2). This gives us 208 observations on each variable in total.[13]

External variables. The data on the variables reflecting external conditions for the Russian economy (i.e., the variables 1–3 in the VAR model) comes from the following sources. CTOT data is retrieved from the IMF Commodity Terms of Trade Database, where it is readily available on a monthly basis. Note that Ben Zeev, Pappa, and Vicondoa (2017) constructed the commodity terms-of-trade index for each country themselves based on the IMF Primary Commodity Price data set and the country-specific weights

[12]In the sensitivity analysis, we vary the lag structure by considering different values of p.

[13]We also experimented with adding the data for each of the 12 months of 2019 and revealed no added value in terms of identification of the credit supply shocks related to the two waves of sanctions. The data from 2020 is ignored due to COVID-19 concerns.

of commodities in their exports. CTOT is a net export price index of Russia's commodity bundle, in which individual commodities are weighted by the ratio of net exports to GDP.[14] Further, the real interest rate in the US economy is calculated as the US CPI-adjusted nominal 3-month Treasury Bill rate (both series come from the IMF's International Financial Statistics database). The Baa spread for the US economy is retrieved from the St. Louis FRED database.

Domestic variables. Domestic real sector variables are constructed based on the datasets of the Federal State Statistics Service of the Russian Federation (Rosstat). Financial data, in turn, is obtained through the website of the Central Bank of Russia. Industrial production, consumption, and investment are constructed based on chain indices and the nominal values and re-expressed in constant 2010 prices.[15] Trade balance is calculated as the difference between the dollar value of Russia's exports and imports and deflated by US CPI (the data is taken from the IMF's International Financial Statistics database).

Data on corporate external debt in Russia is obtained from the website of the Central Bank of Russia.[16] We sum the banks' and other sectors' external debt and subtract debt owed by these sectors to direct investors.[17] We then linearly interpolate quarterly series to obtain monthly data and deflate it by the US CPI.

Following Uribe and Yue (2006), we compute the real interest rate as the sum of the US real interest rate and JP Morgan's EMBI country spread for Russia (J.P. Morgan Emerging Markets Sovereign Bond Spread, EMBI+). We obtain the REER variable

[14]The weighting scheme transforms the series into constant prices because import prices stand in the denominator. We also consider a deflated series: we divide the commodity export price index by the US import price index of manufactured goods from industrialized countries, similarly to Ben Zeev, Pappa, and Vicondoa (2017). The results did not change.

[15]Data source: Short-term economic indicators, see https://rosstat.gov.ru/compendium/document/50802.

[16]External Sector Statistics, see http://cbr.ru/eng/statistics/.

[17]A sizeable amount of Russia's corporate external debt falls into a category of debt to direct investors and direct investment enterprises. As of the end of 2013, the share of this type of corporate external debt amounted to 2% for Russian banks and 35% for Russian non-financial firms. This portion of debt is characterized by non-market behavior, as the creditors are tightly connected to the borrowers through a common ownership structure such as a group or consortium. Thus, these creditors are likely to extend debt repayment deadlines even under sanctions. We address this issue by excluding the debt to direct investors from the total stock of corporate external debt.

from the Bank of International Settlement (BIS) website. Following Ben Zeev, Pappa, and Vicondoa (2017), we re-express this series as an inverse of that reported by BIS to interpret a decrease in this variable as REER appreciation and an increase in it as depreciation.

We apply the seasonal adjustment procedure X13 to industrial production IP_t, consumption C_t, investment I_t, and trade balance TB_t. All variables are further transformed into logs. In the robustness section, we also apply an alternative approach to data transformation: we use HP-filter to compute deviations from the filtered ("long-run") values for each of the variables employed in the VAR model.

2.3.3 Identification of the financial sanction shock

Sign restriction scheme: sanctions as an international credit supply shock

Because the financial sanctions induce an increase in the country spread and a decrease in the amount of foreign debt simultaneously, we suggest treating them as realizations of negative international credit supply shocks (Cesa-Bianchi, Ferrero, and Rebucci 2018; Ben Zeev 2019; di Giovanni et al. 2021). It is thus natural to use a proper sign restrictions scheme that allows the separation of credit supply shocks from credit demand and other shocks (Eickmeier and Ng 2015; Gambetti and Musso 2017).

Formally, we first rewrite the reduced-form VAR model (2.1) in the companion form $Y_t = AY_{t-1} + u_t$ and then premultiply both sides by a matrix B_0 that is aimed at isolating the necessary shocks. This yields a structural representation of the VAR model:

$$B_0 Y_t = B_1 Y_{t-1} + \varepsilon_t \tag{2.4}$$

where ε_t is a vector of orthogonal structural shocks that are related to the original reduced-form residuals via $u_t = B_0^{-1} \varepsilon_t$.

Since an international credit supply shock is a movement of the quantity and price of foreign debt along the demand curve, whereas an international credit demand shock pushes the two along the supply curve, we thus impose the following sign restrictions to identify B_0^{-1}:

$$\begin{pmatrix} \vdots \\ u_t^{IP} \\ u_t^{C} \\ u_t^{I} \\ u_t^{TB} \\ u_t^{D} \\ u_t^{S} \\ u_t^{RIR} \\ u_t^{REER} \end{pmatrix} = \begin{pmatrix} \vdots & \vdots & \vdots & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & + & - & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & + & + & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \end{pmatrix} \begin{pmatrix} \vdots \\ \varepsilon_t^{4} \\ \varepsilon_t^{5} \\ \varepsilon_t^{6} \\ \varepsilon_t^{7} \\ \varepsilon_t^{Credit\ Demand} \\ \varepsilon_t^{Credit\ Supply} \\ \varepsilon_t^{10} \\ \varepsilon_t^{11} \end{pmatrix} \tag{2.5}$$

where "+" and "$-$" are the imposed signs that guarantee that D and S move in the same direction when a credit demand shock hits and in the opposite direction when a credit supply shock occurs. Further, "$\vdots$" are the cells that correspond to the three exogenous variables: they may affect each other, but they are not affected by the domestic variables (the small open economy restrictions; each 0 has a 3×1 dimension, for convenience reasons). Finally, "." means a non-empty (unrestricted) element.

Using the framework of Antolin-Diaz and Rubio-Ramirez (2018), we rotate candidates for the B_0^{-1} matrix until we obtain at least 10,000 successful draws from the posterior distribution that satisfy the imposed sign restrictions. For each successful draw, we compute the time series of the international credit supply shock $\hat{\varepsilon}_t^{Credit\,Supply}$ and the impulse responses (IRFs) of the domestic macroeconomic variables to this shock h periods ahead ($h = 1, 2...60$ months). The IRFs are normalized across all variables such that the shock is equivalent to a 1 pp increase in the country spread variable. We first plot the time evolution of the resultant empirical distribution of the international credit supply shock to analyze whether we identify significant spikes around the first and second sanction waves in 2014-2015 and 2017-2018, respectively.[18] If we do identify these, we then relate them to the financial sanctions and we eventually compute the average effects of the sanctions on the i-th domestic variable as the product of the peak magnitude of respective IRF and the size of the shock in the *50th %-tile* of the shock's distribution. For the first two waves of sanctions, we do it *in-sample*:

$$\Delta^{(J)}\hat{y}_i = \max_{t\in J}\left(\hat{\varepsilon}_t^{Credit\,Supply}\right) \times \max_h\left(\frac{\partial \hat{y}_{i,\tau+h}}{\partial \hat{\varepsilon}_\tau^{Credit\,Supply}}\right), \tag{2.6}$$

where $J = [Mar.2014\ldots Dec.2015]$ marks the first wave and $J = [Jun.2017\ldots Dec.2018]$ marks the second wave of sanctions. $\max_{t\in J}$ implies obtaining maximum value over the J-th wave of financial sanctions and $\max_h$ implies searching for such h at which respective IRF reaches its maximum. Therefore, $\Delta^{(J)}\hat{y}_i$ means the maximum predicted change of y_i caused by the international credit supply shock over the J-th wave of financial sanctions.

For the third wave—the 2022 war-related full-scale sanctions—we compute the *out-of-sample* predictions of the effects of sanctions. We assume that the (peak) IRFs did not change in time and the size of the shock is fully captured by the observed dramatic increase in the country spread during the first months of the war (recall Fig. 2.2):

$$\Delta^{(J)}\hat{y}_i = \max_{t\in J}\left(Spread_t\right)\Bigg|_{J=[Feb.22\ldots Apr.22]} \times \max_h\left(\frac{\partial \hat{y}_{i,\tau+h}}{\partial \hat{\varepsilon}_\tau^{Credit\,Supply}}\right)\Bigg|_{\tau\in[Jan.00\ldots Dec.18]}, \tag{2.7}$$

Overall, the sign restriction approach allows us to isolate international credit supply shocks while controlling for commodities terms-of-trade (first confounder) and domestic monetary policy responses to rising prices (second confounder).

[18] Similar procedures of relating the identified shocks to specific events that are generally attributed to the episodes of particular shocks are performed in, e.g., Antolin-Diaz and Rubio-Ramirez (2018) and Brunnermeier et al. (2021) to ensure credibility.

Microeconomic justification of the aggregate credit supply shock: evidence from syndicated loan data

We now provide evidence supporting our sign restrictions scheme at a more granular level. Specifically, we employ the data on syndicated loans in Russia that were issued between January 2011, i.e., three years before the sanctions, and December 2017, i.e., three years after. By matching banks and their corporate borrowers and employing the combinations of borrower*month fixed effects, this data enables us to separate supply from demand on loans. Of course, a typical drawback is that syndicated loans cover only a small portion of firms compared to all firms borrowing within a given country. However, these are typically very large firms that operate not only within the country but also abroad and attract loans from the syndicates of local and foreign banks. When we explore the effects of international sanctions, a decline in the supply of loans can stem from the foreign banks' decreased willingness to continue lending in the sanctioned country (Efing, Goldbach, and Nitsch 2023).

We obtain syndicated loan data from an international financial IT-company Cbonds.[19] We reveal 294 loans granted by the syndicates of Russian and foreign banks to non-financial firms and banks operated in Russia from January 2011 to December 2017. We observe that 148 loans were issued to firms and the other 146 were issued to banks. We also witness a decline in the number of loans as the economy switches from non- to the sanctions regime: 177 loans were granted before and only 117 loans after the Crimean sanctions. We also observe in the data that the average amount of loans declines (in real US dollars) whereas the average interest rate on those loans rises when we compare 'before' and 'after' the sanctions—a pattern that is already consistent with the supply-side effects (Table 2.1). We see that the share of the ever-sanctioned firms and banks in the total number of borrowers in the market declines by 10 pp in the sanctions regime. We finally reveal that the total amount of the 294 syndicated loans is equivalent to roughly 30% of the total banking system's credit to firms in Russia.

[19] See https://cbonds.com/.

Table 2.1: Descriptive statistics of the Russian syndicated loan market

Note: The table reports descriptive statistics for the variables employed in Equation (2.8). Real loans, the interest rate on loans, and the maturity of the loans match the syndicate of lending banks s, borrowing firm f (either a non-financial entity or the bank itself), and month t when the contract is signed. *Whether credit goes to ever-sanctioned firm*$_f$ is a binary variable equal to 1 if borrowing firm f ever faces sanctions after March 2014 and until the end of the sample period in 2020. Analogously, *Whether sanctioned banks in syndicate*$_{b,s}$ is a binary variable equal to 1 if bank b participating in syndicate s ever faces sanctions.

	Obs	Mean	SD	Min	Max
	(1)	(1)	(2)	(3)	(4)
Before the sanctions (Jan.2011–Feb.2014)					
Real Loan$_{s,f,t}$, USD bln 2015	177	0.762	1.450	0.006	13.152
Interest Rate$_{s,f,t}$, % annum	95	3.4	2.2	1.7	12.8
Whether credit goes to ever-sanctioned firm$_f$	177	0.2	0.4	0.0	1.0
Whether sanctioned banks in syndicate$_{b,s}$	177	0.2	0.4	0.0	1.0
Loan Maturity$_{s,f,t}$, months	177	53.9	38.4	6.0	240.0
After the sanctions (Mar.2014–Dec.2017)					
Real Loan$_{s,f,t}$, USD bln 2015	117	0.630	1.140	0.001	10.515
Interest Rate$_{s,f,t}$, % annum	34	3.6	2.4	1.2	12.8
Whether credit goes to ever-sanctioned firm$_f$	117	0.1	0.3	0.0	1.0
Whether sanctioned banks in syndicate$_{b,s}$	117	0.3	0.5	0.0	1.0
Loan Maturity$_{s,f,t}$, months	117	68.5	46.3	6.0	192.0

With this data at hand, we run the following difference-in-differences regression:

$$\begin{aligned} Y_{s,f,t} = \alpha_{i,t} &+ \beta_1\Big(SANCTIONED_f \times POST.March2014_t\Big) \\ &+ \beta_2\Big(SANCTIONED_f \times POST.Date_{f,t}\Big) + Controls + \varepsilon_{s,f,t} \end{aligned} \tag{2.8}$$

where $Y_{s,f,t}$ is the dependent variable—either the log of real loans issued by syndicate s to borrowing firm f in month t or the interest rate on this loan. $\alpha_{i,t}$ is a product of the firm's f industry fixed effects and year fixed effects.[20] This combination of fixed effects is

[20] Since we are rather restricted in the number of observations, we are not able to include firm*month fixed effects (Khwaja and Mian 2008), because that would require every single firm in the sample to have at least two different banks across time, which may not be the case in our situation. We instead have to aggregate the firms at their respective industry level and then multiply the industry dummies with the year, not month, indicator variables. In total, we have 11 industries and 7 years. We note, however, that following the firm- and industry*year fixed effects is in line with the approach proposed by Degryse et al. (2019), which works as an alternative to Khwaja and Mian (2008) in the absence of multiple-bank firms in the sample.

intended to capture demand on loans of the firms from the same industries, in the spirit of Degryse et al. (2019). $SANCTIONED_f$ is a binary variable that equals 1 during each month within 2011–2017 if firm f ever faces sanctions after March 2014 until the end of the sample period in 2020 and 0 if else. $POST.March2014_t$ and $POST.Date_{f,t}$ are the binary variables that mark 'before' and 'after': i.e., before and after the first sanction announcement that occurred in March 2014 (the first variable) and before and after each and every further sanction on Russian firms that appeared after March 2014 (the second variable). These two variables are inspired by the work of Mamonov, Pestova, and Ongena (2021) that reveals a strong information effect of sanctions after 2014: even if not-yet-sanctioned, potentially targeted firms (banks) adapted their international operations in advance. Thus, in Equation (2.8) we also separate the information and direct effects of sanctions. $Controls$ include the components of the two products, maturity of loans, and whether ever-sanctioned Russian banks participate in syndicate s.

We argue that, if a negative (international) credit supply shock leads to a declining amount and a rising price of syndicated loans, then we will obtain $\beta_k < 0$ in the regression of real loans and $\beta_k > 0$ in the regression of interest rates on those loans ($k = 1, 2$). The estimation results appear in Table 2.2.

As the results show, this is indeed the case: we obtain a negative and significant coefficient on the $SANCTIONED_f \times POST.March2014_t$ variable when the dependent variable is the log of real loans (column 1), whereas the coefficient turns positive and significant when we switch the dependent variable to the interest rate on those loans (column 2). This means that after March 2014, the syndicates of banks started to reduce the volume of new loans and raise the interest rates on those loans for the firms that were potentially targeted by the sanctions—state-owned or controlled corporates and banks—as compared to other firms. Economically, the effects are large: the average amount of loans was reduced by 72% ($e^{-1.354} - 1$) while the interest rate was raised by 1.4 pp. Strikingly, no such effects are obtained for any other sanction announcements once we control for the one associated with March 2014.

Table 2.2: Difference-in-differences estimation results:
Supply-side effects of sanctions at the syndicated loan level

Note: The table reports the estimates of Equation (2.8) with the dependent variable being either the log of the real amount of loan issued by syndicate s to borrowing firm f in month t (column 1) or the interest rate on this loan (column 2). $SANCTIONED_f$ is a binary variable that equals 1 during each month within 2011–2017 if firm f ever faces sanctions after March 2014 until the end of the sample period in 2020 and 0 if else. $POST.March2014_t$ and $POST.Date_{f,t}$ are the binary variables that mark 'before' and 'after': i.e., before and after the first sanction announcement that occurred in March 2014 (the first variable) and before and after each and every further sanction on Russian firms that appeared after March 2014 (the second variable). $Loan.Maturity_{s,f,t}$ is loan maturity, in months. *Whether sanctioned Russian banks in syndicate* is a binary variable equal to 1 if syndicate s contains Russian bank(s) under sanctions. *Industry* is a set of 11 binary variables equal to 1 if firm f belongs to the respective industry and 0 if else.

Dependent variable, $Y_{s,f,t}$:	$\ln(\text{Real.Loan})_{s,f,t}$	$\text{Interest.Rate}_{s,f,t}$
	(1)	(2)
$\text{SANCTIONED}_f \times \text{POST.March2014}_t$	–1.354***	+1.380***
	(0.548)	(0.397)
$\text{SANCTIONED}_f \times \text{POST.Date}_{f,t}$	0.516	–5.331
	(0.976)	(4.135)
SANCTIONED_f	1.612***	–0.095
	(0.277)	(0.397)
POST.March2014_t	0.830	2.959
	(0.569)	(3.720)
$\ln(\text{Loan.Maturity}_{s,f,t})$	0.078	–0.337
	(0.136)	(0.503)
Whether sanctioned Russian banks in syndicate	0.579***	2.711***
	(0.213)	(0.765)
Industry × Year FE	Yes	Yes
N obs	294	129
R^2	0.569	0.745

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the loan level and appear in the brackets under the estimated coefficients.

We therefore obtain microeconomic evidence that the sanctions led to a decrease in borrowings and an increase in the price of borrowed funds for the firms targeted by the sanctions. This evidence backs up our sign restrictions scheme introduced above (Section 2.3.3) and favors our usage of the concept of credit supply shocks at the aggregate level in the rest of this chapter.

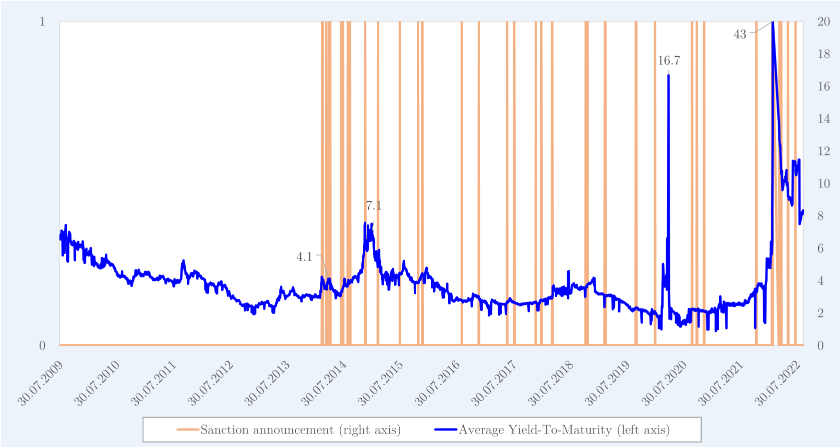
2.3.4 Identification of the overall sanction shock

We now employ the high-frequency identification (HFI) approach to uncover the effects of all sanctions, not only financial sanctions. HFI has been widely used to capture monetary policy shocks using the Fed's announcements on the interest rate (Gertler and Karadi 2015) and then, more recently, oil news shock using OPEC's announcements on oil extraction quotas (Kanzig 2021a), climate policy shocks using the EU's announcements on future CO_2 emission quotas (Kanzig 2021b), and policy shocks using the UK's Brexit announcements (Geiger and Guntner 2022).

We adopt HFI to identify the *sanctions news shocks* using daily dates on the OFAC/EU announcements of sanctions against Russia's politicians, state-connected businessmen, and corporations (both firms and banks) that fell on either the SDN or SSI lists. The idea is that we observe substantial spikes in the yield-to-maturity of Russia's US dollar-denominated sovereign bonds around sanction announcements because investors are likely to re-evaluate risks and start selling bonds once the bad news arrives. This is indeed what we can observe in Fig. 2.4, which presents the daily evolution of the yield-to-maturity averaged across 15 different (partly overlapping) issues of Russia's Ministry of Finance US dollar-denominated bonds on the background of more than 30 OFAC sanction announcement dates that occurred between 20 March 2014 and 21 July 2022.[21]

Importantly, the spikes in the sovereign bond spreads, induced by the leakages of information on upcoming sanctions, should reflect investors' expectations regarding the overall prospects of the sanctioned economy, not only its financial sector. The reason is that the government's ability to repay its debts clearly depends on taxes which are not only levied on the financial intermediaries but also on households and non-financial firms. If investors start massive sales of those bonds, they must believe that the government's ability to repay is seriously damaged because the income of key economic agents is likely to decline when the sanctions are enacted. Therefore, our measure may capture the effects

[21] Recall, however, that the macroeconomic data available for our VAR analysis is limited by the year 2019.



Note: The figure reports the average daily yield-to-maturity across 15 issues of Russia's US dollar-denominated sovereign bonds over 2001 to 2022 (*blue line*) and 31 OFAC daily announcements of sanctions against Russia's individuals and firms between 2014 and 2021 (SSI and SDN).

Figure 2.4: Average yield-to-maturity of Russia's US dollar-denominated sovereign bonds and the OFAC sanction announcements

of sanctions that go beyond the financial sector of the Russian economy.

Therefore, we can attribute (some of) the daily changes in the yield-to-maturity (YTM) to the announcements of sanctions, or anticipation of these announcements, and apply these sanction-driven changes as an instrument to isolate exogenous variation in the reduced-form residuals u of the country spread S regression at the first stage:

$$u_t^{(S)} = \alpha_k + \beta_k \cdot \Delta\overline{YTM}_{k,t} + \xi_{k,t}, \tag{2.9}$$

where $u_t^{(S)}$ is obtained from the VAR model (2.1), $\Delta\overline{YTM}_{k,t}$ is a cumulative within-month t sum of one-day changes in the average yield-to-maturity $\overline{YTM}$ of Russia's US dollar-denominated sovereign bonds around sanction announcement days, which is defined as:

$$\Delta\overline{YTM}_{k,t} = \sum_{\tau(t)=1}^{R_t} \Delta_1\overline{YTM}_{\tau(t)+k}, \tag{2.10}$$

where $\tau(t)$ is a day of sanction announcement within a month t and R_t is the total number of sanction announcements that occur within that month. $\Delta_1\overline{YTM}_{\tau(t)+k}$ is a one-day change in the average daily YTM that occurs $\tau(t) + k$ days before (if $k < 0$) or after (if

$k > 0$) the sanction announcement. The k parameter governs potential *leakage* of the information on upcoming sanctions that may appear shortly before the announcements (e.g., $-5 \leq k < 0$ days) or traces potential delays in the reaction of financial markets to the news on already announced sanctions (e.g., $0 \leq k \leq 5$). International media sources provide direct evidence on such leakages.[22] In turn, delays may take place because global investment funds may not be able (or not allowed) to sell all the bonds within one day, which is aimed at restricting the negative systemic effects on the financial markets that such sales could entail.

If our instrument works well in the first stage, we then proceed to the second stage of the HFI approach. Specifically, we apply Jorda (2005) local projection (LP) approach to build impulse responses of domestic macroeconomic variables to the sanctions shock, as measured with the fitted values $\widehat{u}_t^{(S)} = \widehat{\beta}_k \cdot \Delta_1 \overline{YTM}_{k,t}$ from the first stage. As discussed, e.g., in Mian, Sufi, and Verner (2017), Jorda's LP is more flexible in terms of control variables than VARs and is thus more robust to functional misspecification. We use the following regression form:

$$y_{i,t+h} = \omega_{i,h} + \gamma_{i,h} \cdot \widehat{u}_t^{(S)} + \delta'_{i,h}\mathbf{X}_t + \mu_{i,t+h} \tag{2.11}$$

where $y_{i,t}$ is ith ($i = 1, 2...8$) domestic macroeconomic variable considered in the VAR model (2.1) above, t is month from January 2000 to December 2018 and $h = 1, 2...36$ is prediction step ahead of the sanction shock. $\mathbf{X}_t$ contains control variables: all monthly lags of $\widehat{u}_t^{(S)}$ from $1st$ until $12th$, thus covering the whole previous year, and the current values and 12th month lagged values of each of the eleven variables in y_t.[23] Born et al.

[22] We run a series of Google searches of the following form: "[*Name of the media*] Russia sanctions" in a five-day time interval $[\tau - 5, \tau)$ across such medias as *The Guardian*, *Wall Street Journal*, *New-York Post*, *BBC*, *Bloomberg*, etc. In all cases, we find that the sanction announcements were highly expected one to five days in advance. Essentially, this is not surprising because an adverse action—another episode of Putin's aggression—and the response of the West to it—economic sanctions—are clearly separated in time. After the action and before the sanction announcement, the sanction's preparation stage takes place during which leakage may occur. see Appendix 2.B for examples of media reports on expected sanctions on the eve of the most important announcements in 2014: on March 17th (politicians responsible for the annexation of Crimea and the *Rossiya Bank*, the so-called "Putin's wallet"), July 16th (most of the largest state-owned banks, excluding the top-1, Sberbank), and September 12th, Sberbank).

[23] Recall that all variables in y_t are taken in levels so that it is enough to consider their $0th$ and $12th$

(2020) apply a similar procedure of unfolding the effects of spread shocks on macroeconomic variables.

2.4 Results: macroeconomic effects of sanctions

In this section, we present the macroeconomic estimates of the sanctions shock and its impact on the system of domestic macroeconomic variables. We begin with the effects of *financial sanctions* that we capture using the sign restrictions (SR) approach and the concept of international credit supply slump. We then turn to the effects of *all sanctions*, which include not only financial sanctions, but also restrictions on trade, politicians, and technology. For this purpose, we employ the high-frequency identification approach (HFI). We then summarize the effects we obtain under SR and HFI attributing the difference between them to the other (non-financial) sanctions.

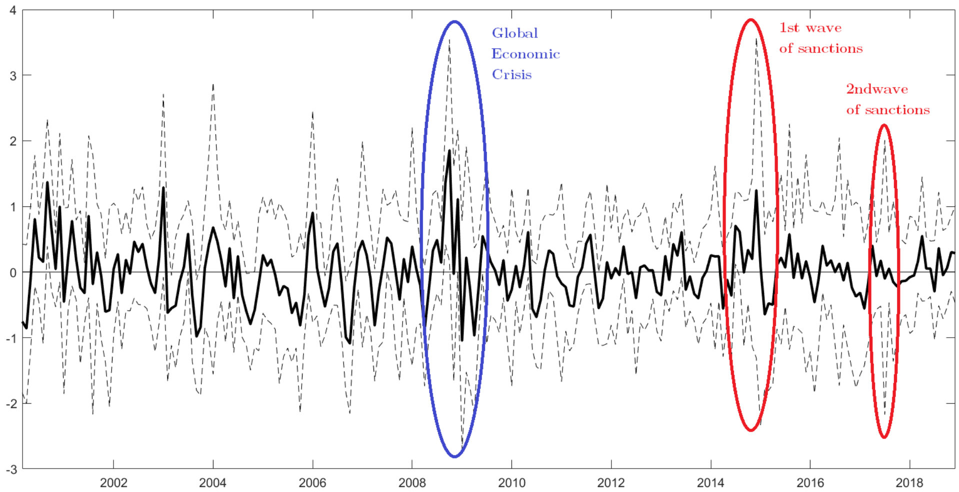
2.4.1 Sign restrictions: the effects of financial sanctions

We start with the preliminary results that we obtain from a version of the VAR model (2.1) with the domestic interest rate being dropped from the list of endogenous variables. Domestic monetary policy is typically ignored in the VAR models of EMEs because the literature assumes that local financial regulators simply follow world interest rate cycles determined by global central banks, see, e.g., Uribe and Yue (2006). Recall that we have the real interest rate in the US economy among the external variables in our models. In the next section, we add the domestic interest rate to close the model and reveal whether there is an added value in terms of the estimated effects of sanctions.

Using the sign restriction scheme (2.5), we first isolate a *negative* international credit supply (ICS) shock from the residuals of the VAR model (2.1) and we then analyze the time evolution of the isolated shock (Fig. 2.5). By construction, positive values of the

lags to cover the previous year. Our results remain the same if we include each lag from $1st$ until $12th$ of each of the eleven variables in y_t. We do not consider it a baseline because it is much less parsimonious than what is implied by Equation (2.11), given the relatively short time span that we have. We also stress that the results remain the same if we drop the $1st$ to $12th$ lags of $\widehat{u}_t^{(S)}$ from $\mathbf{X}_t$.

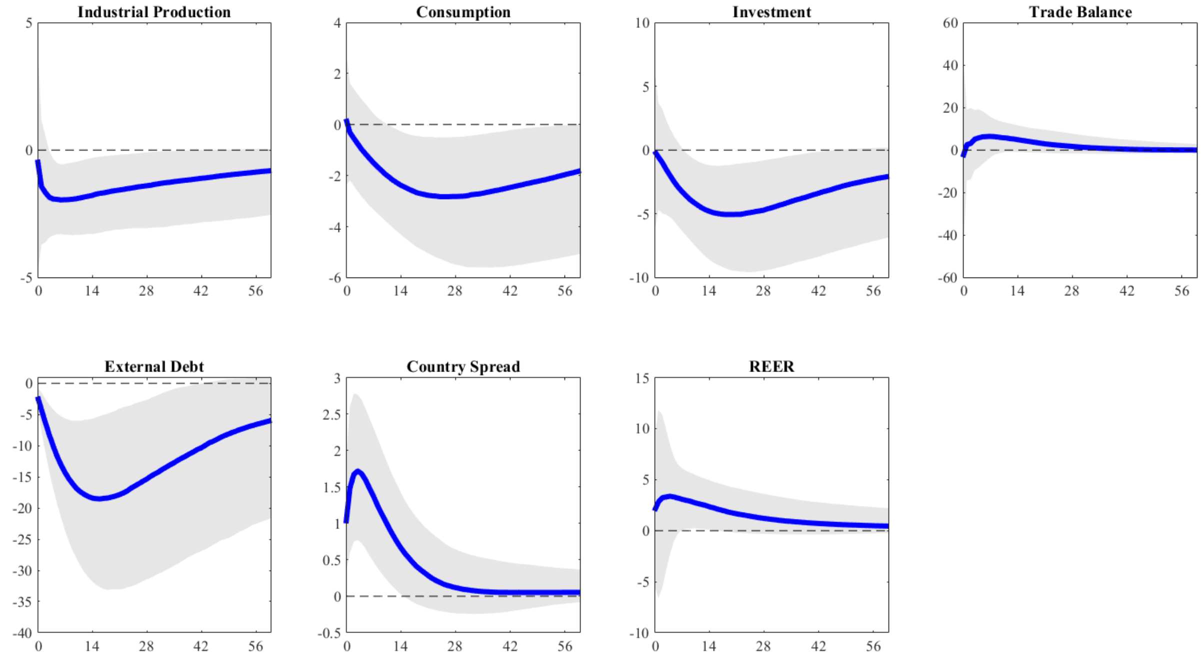
ICS shock correspond to unexpected declines in the supply of external borrowings, and negative values—to unexpected rises. We plot the median extraction from the posterior distribution of the estimated ICS shock and the conventional bands formed by the 16th and 84th %-tiles of the same distribution. We infer that the resultant time series contain substantial spikes around the first wave of the financial sanctions in 2014. The peak of these spikes is the largest one in the 2010s and is comparable to the maximum value of the estimated ICS shock—the one that corresponds to the global financial crisis of 2008–2009. Conversely, we observe no jumps around the second wave of the financial sanctions in 2017–2018. These results are in line with our expectations and the findings of Mamonov, Pestova, and Ongena (2021), which show that sanctioned banks in Russia adapted their international operations after 2014 but in advance of actually facing the restrictions.



Note: The figure reports the time evolution of the sanctions shock estimated with the BVAR model containing 10 variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported. Substantial spikes in the time series of the estimated shock are identified for the first but not for the second wave of sanctions at the end of 2014 and 2017, respectively. One more is identified for the period of the 2008–2009 global economic crisis and is reported for comparative reasons.

Figure 2.5: Time evolution of a negative shock to the international credit supply identified under the sign restrictions scheme

With a plausible estimate of the ISC shock, we now turn to analyze the responses of the domestic endogenous variables. The estimation results appear in Fig. 2.6 below. We report the estimated impulse responses over five years of the domestic variables to a *negative* ISC shock defined above. For the sake of representation, the shock is re-scaled to a +1 pp increase in the country spread on impact, and the responses are re-scaled accordingly.



Note: The figure reports the estimated IRFs of domestic macroeconomic variables to the sanction shock identified using the sign restrictions scheme as an international credit supply shock. The IRFs are re-scaled so that the shock is equivalent to a +1 pp rise of $Country.Spread_t$. The BVAR model contains 10 variables: external characteristics—commodity terms-of-trade ($CTOT_t$), the Baa corporate bond spread ($Baa.Spread_t$), the real interest rate in the US economy ($US.Real.Interest.Rate_t$); domestic indicators—industrial production (IP_t), private consumption ($Consum_t$), investments ($Invest_t$), trade balance (TB_t), corporate external debt ($ExtDebt_t$), Russia's country spread ($Country.Spread_t$), the real effective exchange rate ($REER_t$). Monetary policy reaction to the sanctions shock is ignored in this version of the model. The $Country.Spread_t$ variable is ordered second last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 2.6: Impulse response functions to the international credit supply shock identified under the sign restriction scheme

First, we find that after the initial impulse, the country spread's response peaks at +1.7 pp half a year after the ICS shock and then it attenuates towards zero in the following three years. We also find that corporate external debt, i.e., our second restricted variable, declines by 18 pp one year after the shock. Second, we obtain significantly negative and persistent reactions of the real economy to the ICS shock: industrial production declines by 1.95 pp within half a year after the shock, private consumption falls by 3 pp two years after the shock, and investments slump by 5 pp in the second year after the shock.[24] Third, the results show that the international trade balance, in contrast, reacts positively to the ICS shock, which may imply that imports decline by more than exports; however, the estimated reaction is barely significant. Finally, our estimates indicate that REER also rises in response to the ISC shock, peaking at +4 pp in a quarter after the shock. REER depreciates because Russia's economic agents are forced to repay their external debts, which means a greater demand for foreign currencies in Russia and their outflows abroad. Trade balance rises because agents need to earn enough income in foreign currencies to be able to repay their external debts.

Accounting for endogenous monetary policy responses

As argued by Brunnermeier et al. (2021), we can be sure that we capture the real effects of credit supply shocks only if we properly account for the monetary policy changes in response to such shocks. In our setting, the idea is that negative shocks to international credit supply can provoke rises of credit supply by domestic financial institutions, holding the demand on loans at the same level (substitution channel), which in turn can create upward pressure on domestic prices. Clearly, domestic financial regulators may step in and raise the interest rate to curb inflation. A well-known side effect of this policy is the depression of economic activity. Therefore, we eventually could have a double negative effect on the macroeconomy—one stemming from the international credit supply shock and the other from the monetary contraction. It is a-priori unclear which of the two negative effects dominates and how they relate to each other. To address these concerns, we add the domestic regulated interest rate (in real terms) to the list of endogenous variables employed in the VAR model (2.1) and re-run the same exercises as in the previous section.

As can be inferred from Fig. 2.1, the time evolution of the re-estimated ICS shock

[24]The more strong reaction of private consumption as compared to industrial production is consistent with the lack of consumption smoothing over the business cycle typically observed in EMEs Neumeyer and Perri (2005, Uribe and Schmitt-Grohe (2017).

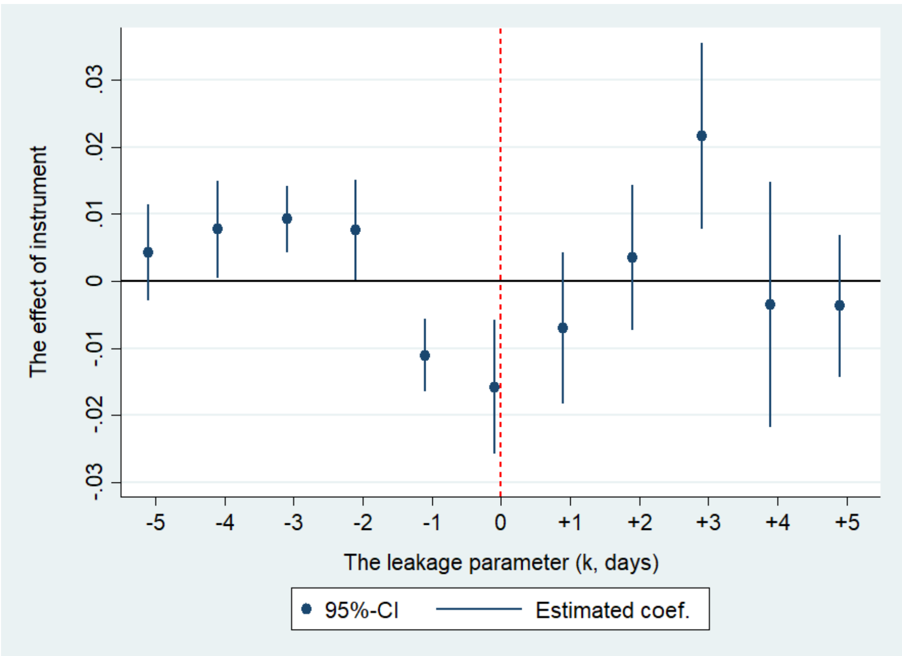
remains very close to the baseline (see Appendix 2.C). The re-estimated impulse responses show that the Central Bank of Russia indeed tends to raise the key domestic interest rate in response to negative ICS shocks (Fig. 2.2). The peak increase reaches +1.4 pp half a year after the ICS shock. However, this has only a minor quantitative impact on our previous results: we find that the estimated responses of the other domestic variables remain almost the same as before. For instance, industrial production declines by 1.78 pp at most, which is only 0.17 lower in magnitude than the respective estimate in the previous section, where the domestic regulated interest rate was ignored.

Overall, accounting for endogenous monetary policy reactions to negative ICS shocks leads to only a small reduction of the estimated responses of domestic macroeconomic variables to these shocks. As an alternative and more conventional approach, we also employ *recursive scheme* (Cholesky ordering) and analyze the effects of financial sanctions by isolating innovations to country spread (S) instead of international credit supply (ICS). The results are largely in line with what we obtain with the ICS shock and are reported in Appendix 2.D.

2.4.2 High-frequency identification (HFI) approach: the effects of all sanction packages

We report the first-stage estimation results in Fig. 2.7, as implied by Equation (2.9). Strikingly, we obtain positive and highly significant β_k estimate when the leakage parameter is set at three days *before* the sanction announcement ($k = -3$). Moreover, this is the only case when the associated first-stage F-statistic exceeds the threshold of 10 (13.5), meaning that the underlying instrument is not weak. For deeper values of the leakage parameter k we either obtain an insignificant estimate ($k = -5$) or still significant but the corresponding F-statistic falls largely below 10 ($k = -4$). For smaller lags, we either obtain an insignificant positive estimate ($k = -2$) or even a negative and highly significant one ($k = 0, 1$). The negative estimates may indicate a reversal from the (over)selling of bonds at deeper k's to buying those at smaller k's. Apparently, this implies that the financial markets expect harsher sanctions than they ultimately are.

With regard to the *after the announcement* days, we find that YTMs start rising during the first three days, and the associated effect that pertains to the third day ($k = 3$) becomes positive and highly significant. This effect is the highest across all days before and after the announcement, exceeding its counterpart that we find significant at $k = -3$ by a factor of 2. Interestingly, if one is willing to consider the average effect across all $k \in [-3, 3]$ to balance the different forces that take place before and after sanction announcements, then the resultant sum (0.0082) is surprisingly similar to the single effect at $k = -3$ (0.0092). Effectively, this means that the overall inference at the second stage would be the same. We thus stick to the β_{-3} case.

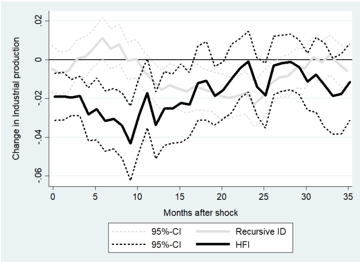


Note: The figure reports the estimation results from the first stage, as implied by Equation (2.9). A sanction announcement takes place on day 0 (*red line*). The estimated coefficients (*blue dots*) show the effect of sanction announcements on Russia's country spread at *monthly* frequency that runs through the changes in the average yield-to-maturity of Russia's US dollar-denominated sovereign bonds that occur k *days* prior to the sanction announcements ($-5 \leq k < 0$, *leakage*) or after it ($0 \leq k \leq 5$, *delay*).

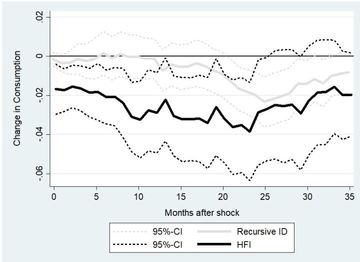
Figure 2.7: High frequency identification of the effects of sanctions: 1st stage estimation results

As for the second-stage results, we present the estimated impulse responses to the

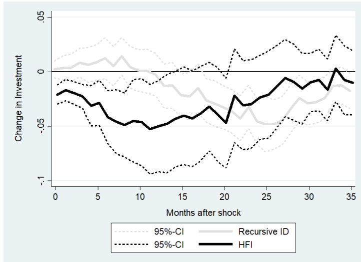
HFI-based sanction shock in Fig. 2.8(a)–(h), as implied by the local projection Equation (2.11). Each subfigure plots the time evolution of the estimated impulse responses $\gamma_{i,h}$ of a given variable $y_{i,t}$ to the HFI shock $\widehat{u}_t^{(S)}$ against the background of the recursively identified SVAR-based shock $\varepsilon_t^{(S)}$. The 95% confidence intervals are computed with bootstrap (500 draws, with replications) to account for the estimated nature of the shocks. The responses are re-scaled to a +1 pp rise in Russia's country spread variable.



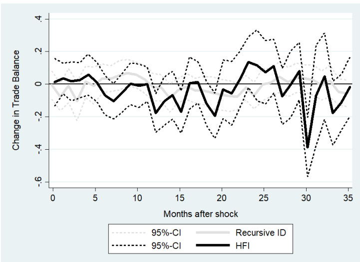
(a) Industrial production



(b) Consumption

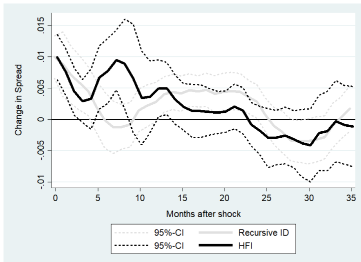


(c) Investment

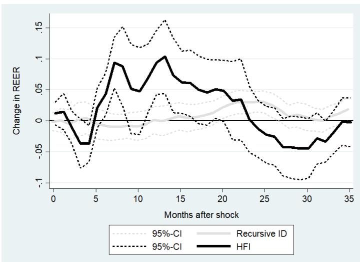


(d) Trade Balance

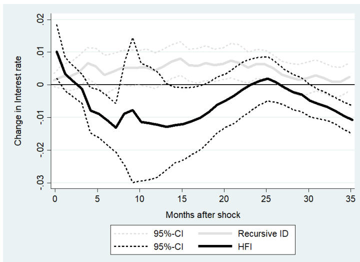
(e) External Debt



(f) Spread



(g) REER



(h) Interest rate

Note: The figure reports impulse responses to a positive country spread shock identified with the high-frequency approach (*HFI*) and recursive identification scheme (*Recursive ID*). The responses are obtained under Jorda's LP approach, as implied by $\beta_{j,h}$ in Equation (2.11). The 95% confidence intervals are computed with bootstrap (500 draws, with replications).

Figure 2.8: Impulse responses to the country spread shock identified under the high-frequency approach

We find that industrial production declines faster and two times more intensively in response to the HFI shock than to the country spread shock (a). The peak reaction to the HFI shock reaches –4 pp by the end of the first year after the shock hits (significant at 1%), whereas the maximal response to the country spread shock is only –2 pp that is reached by the end of the second year after the shock. A similar pattern holds for private consumption (b) and investment (c) whose declines reach 3.2 and 5 pp within a year after the shock. For the trade balance, we do not obtain significant results (d) under either the HFI or recursive identification. For external debt (e) we find that the peak reaction is comparable to what we get with the recursive identification (around –10 pp), but again this happens much faster—within the first year (HFI), not the second year (recursive). For the REER (g), we also obtain that Russia's ruble depreciates following the HFI-based sanction shock, as we get under the recursive scheme; however, the peak depreciation is larger, +10 pp, and this happens faster, within a year after the shock. Finally, we estimate that domestic monetary policy accommodates the sanction shock on impact—by raising the key interest rate by 1 pp—but then turns to easing, by 1.2 pp within half a year after the shock (HFI). Overall, under the HFI approach, we find that the reaction of macroeconomic variables to the sanction shock is much deeper and it materializes faster than under the SVAR-based approach.

2.4.3 Summary of macroeconomic estimates

We have so far isolated the trajectories of the shocks to country spread (S) and international credit supply (ICS) using the VAR model (2.1) and the sanctions news shock using the HFI approach (2.9)–(2.11). With these trajectories at hand, we then estimated the peak responses of domestic macroeconomic variables to these shocks and established the spikes in the shocks' trajectories around 2014–2015 (the first wave of sanctions), 2017–2018 (the second wave), and 2022 (the third wave). By exploiting the *peak responses* and the *sizes of the shocks*, we now compute the resultant macroeconomic effects of the financial sanctions using Expressions (2.6) for the first two waves of sanctions (in-sample) and (2.7) for the third wave (out-of-sample). We report the computation results in Table

2.3. The table compares the effects obtained under the sign restrictions (*Sign*) in an 11-variable VAR model and under the Jorda (2005) Local Projection (*LP*) and the HFI approach. For comparison, we also report the effects obtained under the recursive identification (*Recurs*). We treat the HFI approach as the one capturing the overall effects of sanctions, whereas *Sign* captures only the effects of financial sanctions.

Table 2.3: Macroeconomic effects of sanctions on Russia: Estimation summary

Note: The table contains the (median) estimates of the macroeconomic effects across three waves of financial sanctions, as implied by Expressions (2.6) and (2.7). The estimates are obtained with the use of either a structural VAR model or Jorda (2005) local projection (LP) and the HFI approach. Under the VAR model, the identification methods are: recursive ordering (*Recurs*) or sign restrictions (*Sign*). *Recurs* identifies a positive shock to Russia's country spread and *Sign* isolates a negative shock to the international credit supply. *HFI* identifies a sanctions news shock that pushes investors to sell Russia's sovereign bonds. D_t is Corporate external debt, IP_t is Industrial production, C_t is Final consumption, I_t is Investment, TB_t is Trade balance, $REER_t$ is Real effective exchange rate.

1^{st} and 2^{nd} wave estimates: *in-sample* predictions (Jan.2000–Dec.2018). 3^{rd} wave estimates: *out-of-sample* predictions (2022) based on (*i*) the realized shock to Russia's country spread during the first weeks of Russia's war over Ukraine and (*ii*) the impulse responses estimated for the period of Jan.2000–Dec.2018.

Sanction wave:	First (2014–2015)			Second (2017–2018)		Third (2022)		
Approach:	SVAR		HFI + Jorda LP	SVAR	HFI + Jorda LP	SVAR		HFI + Jorda LP
ID scheme:	Recurs	Sign		*both*		Recurs	Sign	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_t	–20.0	–11.9	–11.2	0	–1.7	$\sim$ –100	$\sim$ –100	–38.5
$REER_t$	+7.0	+2.0	+11.2	0	1.7	+61.3	+17.9	+38.5
TB_t	+4.0	+5.1	0	0	0	+35.0	+44.6	0
IP_t	–3.8	–1.2	–4.8	0	–0.7	–34.1	–11.7	–17.6
GDP_t	–2.5	–0.8	–3.2	0	–0.5	–22.9	–7.9	–11.8
C_t	–4.5	–1.5	–4.5	0	–0.7	–23.9	–8.2	–12.3
I_t	–5.4	–3.4	–5.6	0	–0.9	–51.2	–17.6	–26.5

Corporate external debt. We start the description of our results with the effect of sanctions on the targeted variable—(corporate) external debt (D_t) of the Russian economy. Our estimations indicate that this characteristic is the most responsive variable across all of Russia's macroeconomic variables. With the HFI approach, we estimate that the corporate external debt declines by 11% in response to the first wave of sanctions, as cumulative during 2014–2015.[25] This accounts for roughly half of the overall decline of corporate external debt during that period. Given that the strength of the second wave of sanctions is much lower, we get that its effect on the corporate external debt is just −1.7%.[26] During the third wave of sanctions, the shock is so large that our model predicts a 40% decline of corporate external debt in 2022 in response to the 'tsunami' sanctions. Notably, the estimates that we obtain under the *Recurs* and *Sign* approaches predict a complete shutdown of international borrowings for Russia's economic agents. This clearly speaks in favor of the HFI approach whose results are more realistic, given that Russian firms may still (though partly at best) substitute Western financial funds with those attracted from Asian financial markets.

International trade and exchange rate. Clearly, sanctions force Russia's economic agents to accelerate payments on their external debts. To be able to repay, the agents have to earn relatively more income from international trade—or the government has to support them directly—and then service the external debts. This must cause the outflows of foreign currencies from Russia and, eventually, lead to a depreciation of the ruble.

As our computations under the HFI approach show, the real effective exchange rate ($REER_t$) of the ruble depreciated by 11% in response to the first wave of sanctions in 2014–2015, by another 2% due to the second wave, and by roughly 40% in response to the sanctions news shock in February–March 2022. These estimates mirror those for the corporate external debt that we have just described above. As for the trade balance (TB_t), our HFI estimates produce a zero reaction to the sanctions news across all three waves, meaning that the associated declines in exports could be as large as the declines in imports. If so, then in order to repay external debts, the agents have to appeal for government support. As has been recently shown by Nigmatulina (2022), the government support channel was indeed strong over the above periods. We note, however, that the two other approaches we use, *Recurs* and *Sign*, deliver different results that are consistent

[25] The effect is computed as the product of the first stage coefficient (0.93), the cumulative sanctions news shock over the period (1.20), and the peak response estimated at the second stage (−10).

[26] The effect is computed similarly to the previous one, with the size of the sanctions news shock being replaced from 1.20 by 0.18.

with the agents' abilities to repay the debts using growing income from international trade. That is, under these two approaches, Russia's trade balance increased in response to the first and third waves of sanctions. We assume both channels were at work.

Industrial production and GDP growth. Given the depreciation of the ruble and the decline in international borrowings by Russian firms in response to the sanctions, we can anticipate real negative effects on the Russian economy.[27] Indeed, with our HFI approach, we estimate that industrial production in Russia could have lost nearly 5% in response to the first wave of sanctions cumulatively in 2014–2015.[28] This accounts for 63% of the overall decline in industrial production during that period (–7.6%). The effect of the second wave is much (six times) smaller and equals just 0.7% of lost industrial production in 2017–2018. This explains 32% of the overall decline in industrial production over the respective period (–2.3%). Conversely, when it comes to the third wave, the estimated effect turns dramatically high: minus 17.6% of losses in terms of industrial production dynamics in 2022, which is roughly four times larger than during the first wave and twenty-five times larger than in the second wave.[29] We stress that, by the construction of our local projection equation (2.11), these estimated effects go beyond the effects of CTOT movements and monetary policy responses to changing prices that occurred during the three waves of sanctions.[30]

Given that monthly data on GDP does not exist, we uncover the effects of the three

[27]The firms heavily relied on international borrowings as a source of funds: corporate external debt was equivalent to 30% of GDP in Russia on the eve of the first wave of sanctions.

[28]The effect is computed as the product of the first stage coefficient (0.93), the cumulative sanctions news shock over the period (1.20), and the peak response estimated at the second stage (-4.3).

[29]The estimate is built up similarly to the two previous ones, with the sanctions news shock being replaced by 4.14. The computation is $100\%\cdot\left(\left(1+\frac{1}{100}\cdot 0.0093\cdot 4.14\cdot(-4.3)\right)\cdot\left((1-0.007)(1-0.006)\right)-1\right) = -17.6\%$. Here, we have also accounted for the monthly growth rates of industrial production that we observe for the pre-war January and February 2022 (-0.7% and -0.6%, respectively) before the data was closed by the Russian government as the war raged.

[30]As an example of relative contribution during the first wave of sanctions, we find that the oil price slump and monetary contraction explain jointly 4.4 pp of the decline of industrial production. This means that together with the sanctions news effects (4.8 pp), the three shocks accommodate a 9.2 pp decline in industrial production over those times. Recall that the actual decline equals 7.6%. This implies that a conservative estimate of the strength of the government support channel (when the Russian government was supporting sanctioned firms, see Nigmatulina 2022) can be equivalent to at least 1.6 pp.

waves of sanctions on Russia's final output by using a simple linear mapping from industrial production to GDP estimated at the quarterly frequency (0.67, significant at 1%, see Appendix 2.L). With this mapping, we find that real GDP in Russia could have lost 3.2% during the first wave of sanctions in 2014–2015, 0.5% in 2017–2018, and that in could lose nearly 12% in 2022. Importantly, one should not confuse these estimates with the overall forecast of GDP dynamics in the respective years. Instead, these estimates capture the potential of initial sanctions shock: a pure sanction effect originating from the size of the sanctions news shock to Russia's US dollar-denominated sovereign bonds that had occurred on the eve of the sanction announcements. These estimates thus do not take into account responses to sanctions by the Russian government, the Central Bank of Russia, and the international partners across the world that help Russia to evade the universe of global restrictions.[31]

In almost all cases, our estimates exceed those in the literature (between 0% in Kholodilin and Netsunajev (2019) and –1.5% in Barseghyan 2019), analyst reports (–0.2% by the IMF 2015), and our own estimates obtained with the use of the VAR models under *Recurs* or *Sign*. For instance, under the *Sign* approach, we estimate that the effect of the ICS shock on GDP is 2.4 pp less strong during the first wave of sanctions and 3.9 pp less strong during the third wave.[32] We argue that these discrepancies in the estimated effects arise exactly because we use an innovative measure of the sanctions shock stemming from the news on upcoming sanctions which we accommodate with the HFI approach (*external instrument*) rather than using the ICS concept originating from the residuals of (VAR) regressions based on macroeconomic time series themselves.

[31] For the overall forecasts of Russia's GDP, one could be directed to the IMF predictions produced, e.g., in August 2022, according to which Russia's GDP could lose around 6% in 2022. At the moment of this text writing in mid-2023, the final figure for 2022 is just –2%. Therefore, one can compare our estimate of the sanction potential, –12%, and this final figure, –2%, and think of the difference, i.e., 10 pp, as of the effect of sanction evasion in 2022.

[32] Note that the size of the ICS shock is not observed in 2022 because we do not have the data on the amount of external debt decline. To overcome this issue, we assume that the ratio between the peak magnitudes of the country spread and ICS shocks remains constant in time—in 2014 when both are observed, and in 2022 when only the country spread is observed. This allows us to uncover the assumed size of the ICS shock in 2022 and compute the effect on industrial production and other macroeconomic variables.

Consumption and investment. With the HFI approach, we find that the sanctions news shock could have led to a decline in private consumption of 4.5% and investment of 5.6% during the first wave of sanctions, as cumulative over 2014–2015. This implies much stronger negative reactions than that of GDP which we described above—by 1.3 and 2.4 pp on magnitude, respectively. We then obtain the negative effects on consumption and investment turn substantially milder (five to six times) during the second wave in 2017–2018, being bounded by –1% and still exceeding the effect on GDP. Our computations then indicate that the sanctions news shock at the beginning of the third wave in February–March 2022 is able to trigger a slump in consumption of 12% and investment of more than 25%, which are comparable only to the effect of the USSR collapse in the early 1990s.[33]

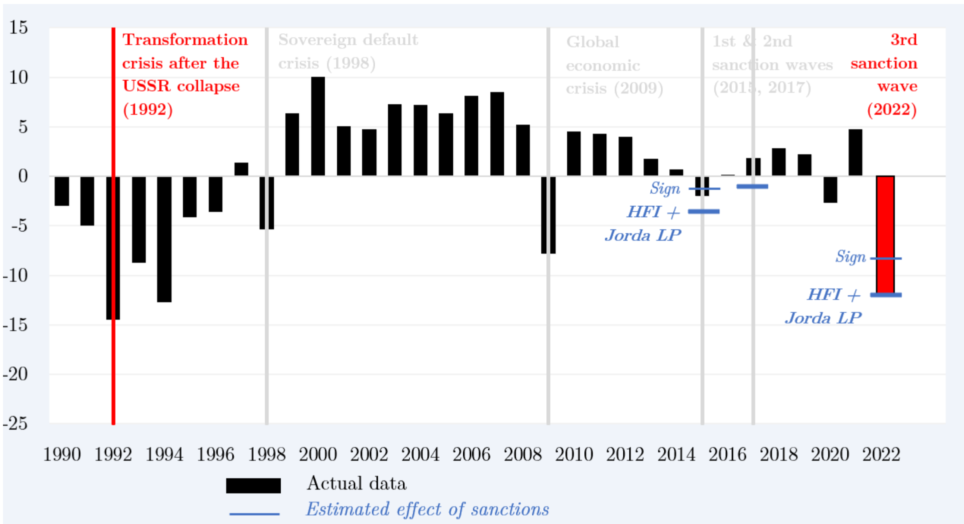
Again, as was the case with GDP, our estimates obtained with the *Sign* approach indicate a less strong reaction of both consumption and investment than those obtained with HFI. Clearly, this highlights the key difference between the two approaches: while *Sign* captures only the reduced *supply* of international funds, HFI encompasses both the reduced supply of these funds and the depressed *aggregate demand* in the economy due to negative feedback loops. Put differently, sanctions first shrink the supply of international finance—this then raises the likelihood that firms' and households' borrowing constraints become binding—this, in turn, forces consumption and investment to shrink, and thus the agents demand less in the economy than before the sanctions hit. We argue that the expectations of this chain of events by financial markets are included in the prices of Russia's sovereign bonds and thus are fully captured by our HFI approach, whereas *Sign*, by construction, ignores the demand side of the story.

Our results are partially consistent with the cross-country event-study estimates of Gutmann et al. (2021), who find that consumption falls by 2.2% during the first year after sanctions while investment decreases by 24% in two years after the sanctions. Our

[33]These out-of-sample computations exploit a linear mapping between private consumption and industrial production (0.69, see Appendix 2.L) and between investment and industrial production (1.5, see Appendix 2.L), respectively.

results point to more equal reactions of consumption and investment to the international financial sanctions.

Overall, we document that financial sanctions have multiple effects: they not only change the flows of international borrowing funds but also have significant real effects on the domestic (sanctioned) economy. These effects clearly depend on the size of the sanction shock and on whether and how much other shocks affect the economy at the same time. But even in 2014–2015, when the Russian economy encountered a deep negative oil price shock and similarly deep restrictive monetary response, we show that the sanctions were still responsible for at least 50% of the total decline in industrial production and GDP. In 2022, by contrast, external conditions were more than favorable but the sanction shock was unprecedentedly high causing the largest decline in the economy since the collapse of the USSR (Fig. 2.9). Of course, the latter estimate should be perceived as a pure effect of the sanctions *prior to* the Russian government's response to the shock, including the imposition of capital controls by the Central Bank of Russia in early March 2022.



Note: The figure reports the time evolution of real GDP growth rates over the last 30 years in Russia and marks the episodes of economic crises. *Sign*, *Jorda LP* and *Recurs* are the methods we apply to obtain the estimates of the effects of sanctions: sign restrictions (2.5), Jorda's local projection (2.11), and recursive identification (2.12).

Figure 2.9: Sanctions and the history of business cycles from the collapse of the USSR until the war in Ukraine, 1990–2022

2.4.4 Other robustness checks

The rest of the sensitivity analysis is devoted to understanding how much our estimated impulse responses depend on the modelling assumptions and data transformation.

First, instead of imposing the sign restrictions (2.5) on impact, we assume a wider time period during which the restrictions must hold. We consider 1, 2, and 3 months when estimating the VAR models using the approach of Antolin-Diaz and Rubio-Ramirez (2018). In all cases, we obtain virtually the same time series of the estimated ICS and country spread shocks and the patterns of impulse responses. The quantitative differences with respect to the baseline results are negligible (available upon request).

Second, the empirical macroeconomic literature that relies on frequentist (i.e., non-Bayesian) estimation methods typically exploits detrended time series to ensure stationarity and comparability with theoretical literature (Akinci 2013). Though we apply the Bayesian methods that are robust to non-stationarity in the data, we also perform a portion of VAR estimates with HP-detrended time series. The results obtained under the recursive identification scheme appear in Appendix 2.H and those under the sign restriction in Appendix 2.I. Qualitatively, we obtain the same results as in the baseline: real variables—industrial production, private consumption, investment—contract, trade balance improves, REER appreciates, and external debt and the domestic regulated interest rate rise. Only one exception is the response of investment in the recursive case, which turns positive but remains insignificant during the whole prediction horizon. Quantitatively, the recursive case delivers significant responses, whereas the sign restrictions produce mostly insignificant responses when the data is HP-detrended. Under the recursive case, interestingly, the estimated responses are 2 to 3 times lower in magnitude as compared with the baseline (Fig. 2.1), and the size of the shock in 2014 is also lower by 1 pp than in the baseline estimates (Fig. 2.2).

Third, we run a more parsimonious model—a five-variable VAR from Uribe and Yue (2006)—and perform the recursive identification of the country spread shock. We report the results in Appendix 2.J, which indicate that output falls by slightly more (–1.1 pp) than in our 11-variable VAR specification. Investment, by contrast, falls slightly less (–1.1 pp) than in the baseline. In this regard, the results are very much robust. However, with regard to the trade balance, we encounter a wedge in the results: in the five-variable VAR, we find that the trade balance reacts negatively, not positively, to a positive country-spread shock. This contradicts the theory that we use (Uribe and Yue 2006; Chang and Fernandez 2013; Uribe and Schmitt-Grohe 2017). Clearly, for an export-oriented economy like Russia, being strongly dependent on the export prices of fuel goods, omitting commodity terms-of-trade as well as REER may pose a serious challenge for recovering the full space of shocks. Nonetheless, even in this case the estimated time evolution of the identified shock to country spread still allows us to recognize a substantial spike in 2014 (the first wave of sanctions) and no significant shocks in 2017 (the second wave of sanctions).

Fourth, we use Jorda's LP approach to re-estimate the impulse responses obtained with our VAR model (2.1). The estimation results are reported in Fig. 2.1.(a)–(h) (see Appendix 2.K). Each subfigure plots the time evolution of the estimated impulse responses of a given variable $y_{i,t}$ to the shock $\hat{\varepsilon}_t^{(j)}$ that is computed either with the recursive or sign restriction schemes. We find that in most cases (except for industrial production), the results are quantitatively larger under the VAR than Jorda's LP methods but remain qualitatively the same. Therefore, we conclude that our baseline results are supported by Jorda's LP approach and are thus robust to misspecification.

2.5 Results: cross-sectional effects of sanctions

Having established significant macroeconomic implications of the financial sanctions for the Russian economy in 2014–2015 and 2022, we now ask how the aggregated sanctions shock affects the cross-sections of households and firms. We are specifically interested in the heterogeneity of the effects of sanctions. We may expect that the current sanctions have larger negative effects on the economic agents that are less likely to support the political regime in Russia: richer households in large cities, as they may have international assets and are more competitive in international labor markets, and more productive firms, as they are more likely to be well-integrated into the world economy. Conversely, the sanctions are less likely to hit the regime's proponents: poorer households in rural areas and local firms with lower levels of productivity.

2.5.1 Sanctions and the cross-section of firms

We collect firm-level data from the SPARK-Interfax database over the period from 2012 to 2018.[34] We require firms to simultaneously have non-missing non-negative values on total assets, total revenue, value added, number of employees and wages, capital and intermediate inputs (materials), and bank and non-bank borrowed funds. We also require the firms to operate for at least three consecutive years. The final sample consists of 7,460 large and small firms resulting in 40,381 firm–year observations over the period of 2012–2018.[35] The firms operate in as many as 16 different sectors of the Russian economy (two-digit classification) ranging from natural resources extraction to IT.

With this data at hand, we estimate the firms' TFPs by applying a popular methodology proposed by Wooldridge (2009) and Petrin and Levinsohn (2012). We assume a Cobb-Douglas production function with the real value added as the dependent variable and labor, capital, and materials as the explanatory variables. We also impose constant returns to scale. The summary statistics on the variables employed in the estimation and the estimates of firm productivity $TFP_{f,t}$ appear in Table 2.1 (see Appendix 2.M). The estimates show that the magnitude of productivity averaged across all firms and years equals 13.6, being bounded between 6.1 and 21.4 and thus indicating a large variation in

[34] See https://spark-interfax.com/.

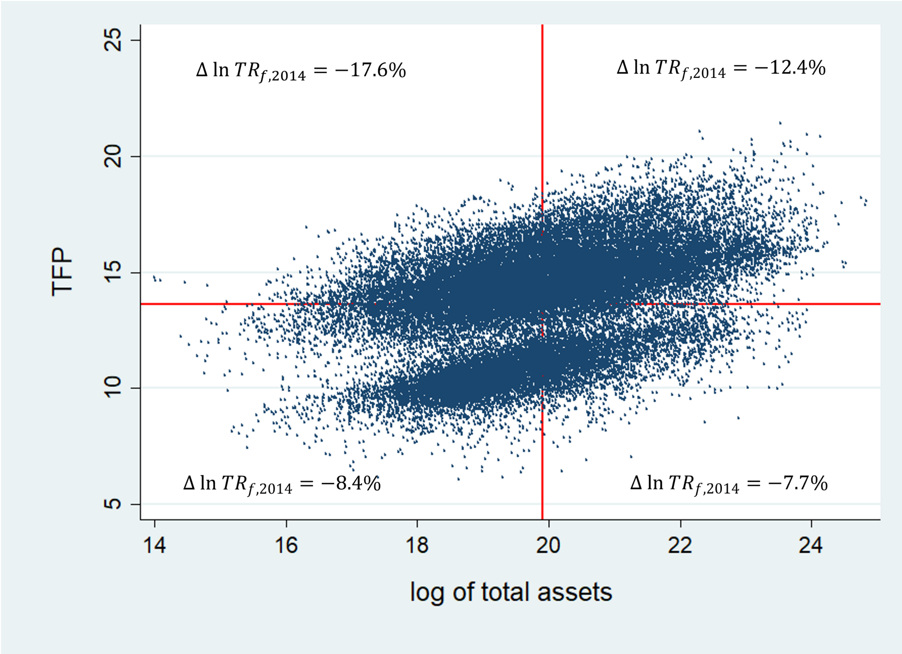
[35] Initial sample consists of roughly 300,000 firms. The substantial decline in the number of firms is caused by many missing values on employee and wage data in the firms' balance sheets and the requirement to work for at least three years in a row. We cannot remove the condition imposed on employees and wages because this data is essential for estimating TFP. If we remove the condition on at least three years of operations, then the number of firms rises to 32,790, i.e., by a factor of four, and the number of firm–year observations increases to 81,004. The results on the cross-sectional effects of sanctions do not change in this case (see below). We prefer to keep this condition to relax the "survivorship bias" problem.

firms' $TFP_{f,t}$ (note that the mean magnitude of the real value added is 18.5). Plotting the time evolution of the firms' distribution by $TFP_{f,t}$ and size, as proxied with the log of the firms' total assets (in constant prices) $\ln TA_{f,t}$, we observe a slightly positive trend in the firms' productivities, despite the sanctions shock in 2014, and a visible negative trend in the firms' size, especially during 2014 (see Fig. 2.1.(a) and (b) in Appendix 2.M). In both cases, the observed variation across firms remains large and stable over time.

Given the estimated firms' $TFP_{f,t}$ and sizes $\ln TA_{f,t}$, we divide our sample into four parts: (*i*) large firms with high TFP ($N\,obs = 14,126$), (*ii*) large firms with low TFP ($N\,obs = 5,198$), (*iii*) small firms with high TFP ($N\,obs = 9,684$), and (*iv*) small firms with low TFP ($N\,obs = 11,373$). We use the mean value of $\ln TA_{f,t}$ to separate 'large' and 'small' firms. 'High' and 'low' productivities are defined accordingly using the mean value of $TFP_{f,t}$. Fig. 2.10 visualizes the resultant four cells of firm–year observations and reports the growth rate of firms' total revenue (in constant prices) during the first year of the financial sanctions in each of the four cells. In line with the anecdotal evidence discussed above, we indeed observe that holding the firms' size constant, more productive firms faced larger declines in real revenues than less productive firms. Regarding large firms, more productive firms experienced a 12.4% decline in real revenues in 2014 while less productive firms encountered only a 7.7% drop during the same year. Concerning small firms, more productive firms reported a 17.6% slump in real revenues in 2014 while less productive firms experienced only an 8.4% reduction over the same period. These figures also imply that larger firms were able to better support their revenues than smaller firms, and more so for more productive firms.[36]

Clearly, the raw data shows that four groups of firms in our sample experienced a deterioration of their real revenues in the first year of the Crimea-related sanctions (Of course, certain firms in each of the four groups could have (and indeed had) positive growth rates of real income during 2014.). However, given the negative CTOT shock and

[36]An interesting side outcome from Fig. 2.10 is a clustering of the scatter-plot: there are two clusters of firms—more productive and less productive, given the same firm size. Probably, this could be related to the exporting statuses of the firms. Our data, however, does not allow us to elaborate more on this topic. We leave it for future research.



Note: The figure reports the scatter-plot of 40,381 firm–year observations (7,460 firms over the 2012–2018 period) on the log of total assets (in constant prices, X axis) and firms' TFPs, as estimated using the Wooldridge (2009) and Petrin and Levinsohn (2012) approach (Y axis). The horizontal and vertical red lines mark the mean levels of the firms' TFPs and total assets, respectively. For each of the four resultant cells, the figure also reports the growth rate of real total revenue $\Delta \ln TR_{f,t}$ that the firms reported in their balance sheets by the end of 2014, i.e., the first year of the Crimea-related sanctions.

Figure 2.10: Firm size, TFP, and decline in firms' real income during the first year of sanctions

restrictive monetary policy stance in 2014, we ask what part of the deterioration could be attributed to the sanctions. From the previous literature, we only know that the firms directly targeted by the sanctions encountered a more pronounced decline in employment and sales compared to non-targeted firms (Ahn and Ludema 2020; Crozet et al. 2021). We take a broader perspective and apply the Jorda (2005) LP approach to reveal the effects of sanctions in the four cells of firms outlined above.

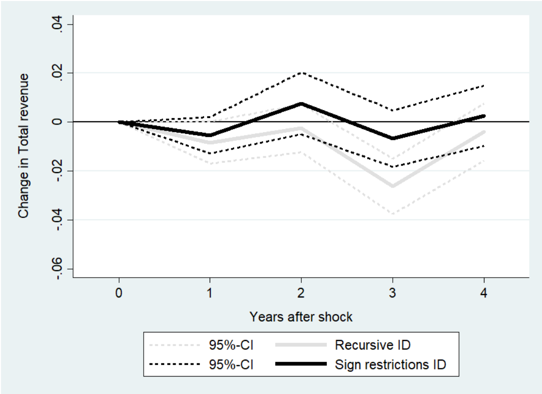
For the purpose of estimation, we employ the same regression (2.11) as before but adapted to the firm level. The dependent variable $y_{f,t+h}$ is the log of total revenue (in constant prices). The key explanatory variable remains the same—either the *ICS* or *country spread* shocks isolated from our VAR models. The monthly estimates of the shocks are aggregated to the annual level by means of summation within each year. We also adjust the control variables $\mathbf{X}_{f,t}$ so that they contain the current values of the total revenue, number of employees, investment, and, importantly, the interest expenses on the firms' loans from domestic banks and the CTOT variable. The two last variables are intended to capture the restrictive monetary policy of the Central Bank of Russia in 2014 and the tumble in world oil prices during the same period.

The estimation results appear in Fig. 2.11. The figure contains the same four cells of firms and in the same order as in Fig. 2.10 above. First, the estimates suggest that during the first year of the financial sanctions (*ICS*) shock, the real total revenue of the *large firms with high TFP* and *large firms with low TFP* both decline by 2.0–2.2 pp. These are equivalent to 16% and 29% of the total decline in real revenues of these firms, respectively.[37] This means the sanctions had economically significant effects on the performance of large firms in Russia beyond the effects of the oil price collapse and monetary contraction back in 2014. Interestingly, for the large firms with high TFPs, the effect of the sanctions turns to declining from the 2nd year, though still significant, whereas the same effect for the large firms with low TFPs continues to expand, reaching almost −4%. This implies that productivity matters for the absorption of the effect of the sanctions. Starting from the 3rd year, the effect on both types of firms attenuates to zero. We also note that the results remain the same, and even stronger quantitatively, if we consider *country spread* instead of ICS shock.

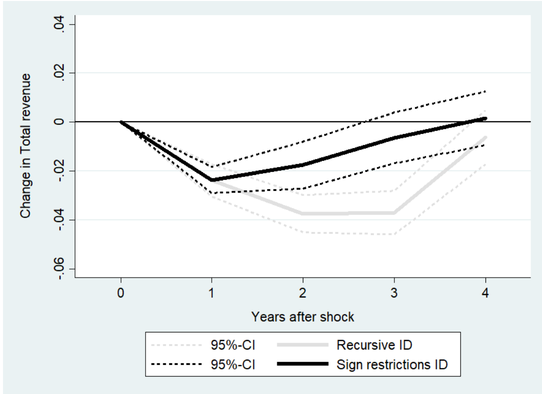
Second, the estimates indicate that, during the first year after the ICS shock, the real revenue of the *small firms with high TFP* decreases by 0.5 pp. This decrease, however, is insignificant. During the 2nd to 4th years after the ICS shock, the estimated response remains close to zero and insignificant. (Only in the case of the country spread shock does the response turn significant during the 3rd year, but we treat it with caution). Qualitatively, almost the same results pertain to the group of *small firms with low TFP*: during the first year after the ICS shock their real revenue declines by 1 pp (or 12% of the overall decline in 2014),[38] but this effect is much lower than for the large firms and it

[37] The shares are computed as $-\frac{2.0}{12.4} = -0.16$ and $-\frac{2.2}{7.7} = -0.29$.

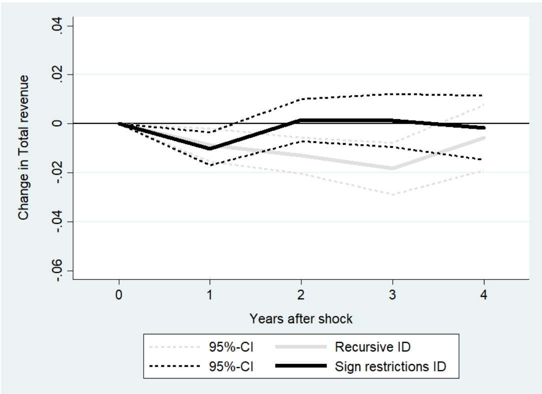
[38] The share is computed as $-\frac{1.0}{8.4} = -0.12$.



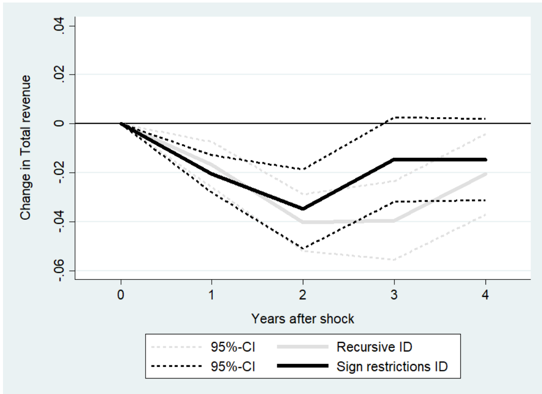
(a) Small firms with high TFP



(b) Large firms with high TFP



(c) Small firms with low TFP



(d) Large firms with low TFP

Note: The figure reports the impulse responses of the firms' total revenues (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using Jorda (2005) local projection approach. The sample contains 40,381 firm–year observations for 7,460 firms over the period of 2012–2018. The condition that the firms must operate for at least three consecutive years is imposed. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

Figure 2.11: The effects of the sanctions shock on the real total revenue in a cross-section of firms

turns virtually zero from the 2nd year onwards. Strikingly, the small firms experienced much larger total declines in their real revenue than did the large firms (recall Fig. 2.10) but, as our estimates suggest, these declines are barely explained by the sanctions.

Finally, we argue that the results remain the same if we drop our condition that the firms in the sample must operate for at least three years. Indeed, as can be inferred from Fig. 2.2, the sanctions shock negatively affects large firms, and less so if TFP is higher, and the shock has virtually no effect on smaller firms, regardless of their TFP (see Appendix 2.M).

2.5.2 Sanctions and the cross-section of households

To test the hypothesis that richer households in larger cities were more adversely affected by the sanctions than poorer households in rural areas we need appropriate survey data. This data comes from the RLMS-HSE database, a rich survey of 5,000 Russian households that the National Research University "Higher School of Economics" has been conducting across Russia since 1994.[39] We extract the data on income and consumption for the period from 2006 to 2018 and winsorize the data below 1 and above 99%-tiles, which resulted in 21,813 individuals from different households and 74,356 observations in total.

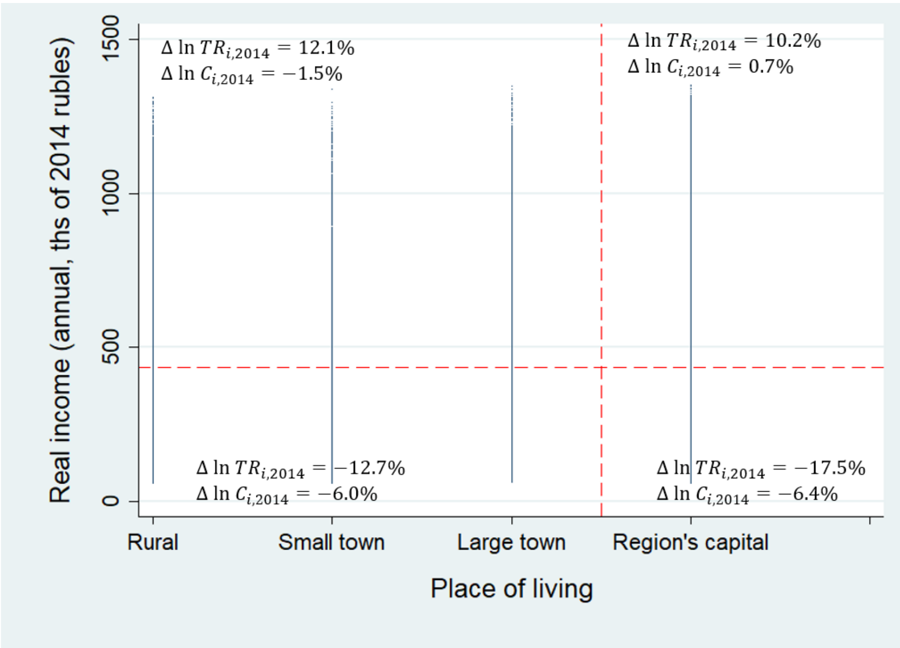
The data allows us to trace the place of living and total income of each individual, among other things. The breakdown of the 74,356 observations that we have for the analysis is as follows: 31,266 pertain to a region's capital city (*Region's capital*), 20,836 belong to large towns other than the capital (*Large town*), 4,460 are in smaller towns (*Small town*), and 17,794 are attributed to rural areas within a region (*Rural*). Mean annual income across the four locations is, respectively, 483.6, 416.7, 422.4, and 371.9 thousand rubles (in constant 2014 prices).[40]

With these preliminaries at hand, we divide all observations into four cells: (*i*) richer individuals residing in regions' capital city ($N\,obs = 31,266$), (*ii*) poorer individuals residing in regions' capital city ($N\,obs = 20,836$), (*iii*) richer individuals residing in regions' other locations ($N\,obs = 4,460$), and (*iv*) poorer individuals residing in regions' other locations ($N\,obs = 17,794$). Within these four cells, the raw data shows that richer households experienced growing, not declining, income during the first year of sanctions in 2014, whereas poorer households suffered from a substantial decline in income (Fig. 2.12). Indeed, the annual growth rate of real income equaled 10.2% and 12.1% for richer

[39]The data is representative and has been already used in many different areas of economics research, see, e.g., Yakovlev (2018).

[40]These numbers are equivalent to 11.6, 10.0, 10.2, and 8.9 thousand US dollars, assuming the exchange rate of 41.57 rubles per US dollar, as an average over 2006–2018.

households residing in regions' capital cities and other places, respectively. Conversely, the same figures for the poorer households were –17.5% and –12.7%, respectively. These numbers imply that the overall variation in real income rose dramatically during the first wave of the sanctions.[41] However, as was the case with firms in the previous section, the question arises as to whether we can fully attribute this increased variation in households' income to the effects of sanctions, given the other important adverse shocks hitting the households during the same period (oil price slump and monetary tightening).



Note: The figure reports the scatter-plot of 74,356 individual–year observations (21,813 individuals over 2006–2018) on the place of living (X axis) and annual real income (Y axis). The horizontal dashed red line marks the mean levels of the individuals' income. The vertical dashed red line separates observations on the individuals residing in a region's capital city from the others living in either rural areas, small or large towns different from the capital. For each of the four resultant cells, the figure also reports the growth rate of real income and total consumption by the end of 2014, i.e., the first year of the Crimea-related sanctions.

Figure 2.12: Individual income, consumption, and the place of living in a cross-section of households

Interestingly, the rise in real income of the richer households in 2014 was apparently not enough to sustain their consumption—the annual growth rates of real total consump-

[41]Though it is out of the scope of this chapter and our data does not allow us to explore this issue, we can cautiously assume that richer households possess substantially higher savings denominated in foreign currencies than poorer households back in 2014. The ruble lost 90% of its value against the US dollar during that year.

tion were either positive but low or even negative. For the poorer households, the growth rates of real total consumption were even much more negative, implying a substantial decline in their standards of living during the first year of sanctions (Fig. 2.12).[42] It is also important to understand how the financial sanctions impacted total consumption and its components, consumption of durables and non-durables. We report the descriptive statistics on these variables in Table 2.1 (see Appendix 2.N).

To answer the question as to how the financial sanctions affect different parts of the population, we again explot the *ICS* and *country spread* shocks and apply the Jorda (2005) LP approach, as we did for firms in the previous section. The estimation results appear in Fig. 2.13. The figure contains the same four cells of households and in the same order as in Fig. 2.12 above.

Richer households. The estimates suggest that the real income of richer households does not respond to the sanctions (*ICS*) shock during the first year after the shock occurs. However, during the second year after the shock, the real income declines by 1.5 pp if the households live in regions' capital cities, and by 2.0 pp if they live everywhere else (all estimates are significant at 5%). Interestingly, in regions' capital cities, the effect on real income persists in time, remaining negative and significant even during the 3rd and 4th years after the shock. For the other places of living, by contrast, the effect on real income weakens starting from the 3rd year. If we switch to the country spread shock, we get an even more pronounced contraction of income during already the 1st year after the shock. A further disaggregation analysis reveals that the effects on total consumption of the richer households are driven by the reduction in the consumption of non-durables, while the consumption of durables was barely affected by the sanctions (Fig. 2.1, see Appendix 2.N).

Poorer households. Strikingly, the estimates further indicate that the real income of

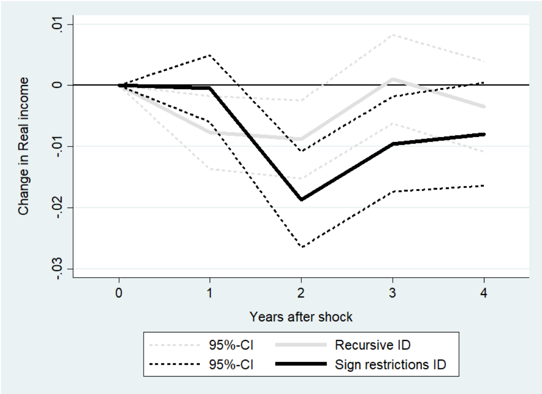
[42]Again, though our data does not contain this information, we can assume that all households had to substantially increase the interest payments on their loans, given that the key interest rate had been raised by the Central Bank of Russia from 5.5 to 17% during 2014. This dramatic rise in the price of money had negatively affected the households' consumption at that time, as the literature on consumption and monetary policy predicts (Cloyne, Ferreira, and Surico 2019).

poorer households responds positively, not negatively, to the ICS shock during the first year. The positive reactions are 1.2 pp for the poorer households residing in regions' capital cities and 1.1 pp for the poorer households everywhere else (all estimates are significant at 5%). However, during the second year after the ICS shock, the reactions flip the sign negative, reaching –1.5 and –2.1 pp, respectively (all estimates are significant at 5%). The 3rd and 4th years' reactions vanish and are insignificant. As is the case with richer households, our further disaggregation analysis shows that the positive effect of sanctions during the first year is triggered by rising consumption of non-durables (Fig. 2.1, see Appendix 2.N).

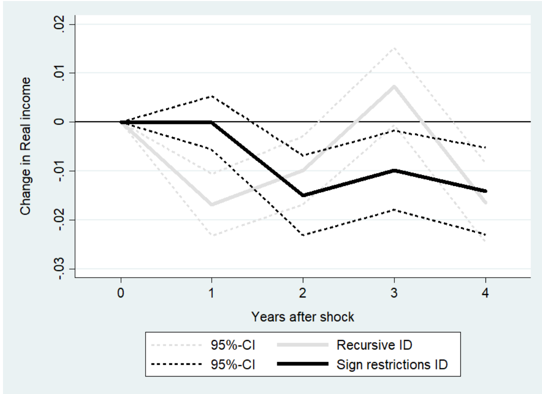
By pooling the results for richer and poorer households together, we argue that the financial sanctions could have the unintended effect of reducing income inequality. This is because the sanctions could have (partly) closed the doors for the international businesses of richer households while forcing the Russian government to support poorer households through the redistribution of income and taxes. The government support channel is established by the micro evidence from Mamonov, Pestova, and Ongena (2021) and Nigmatulina (2022).

Indeed, recall from our description of the raw data above that richer households enjoyed growing real income in 2014, whereas poorer households suffered from a slump in their income. As Ananyev and Guriev (2018) show, a decline in income causes the destruction of trust in the government in Russia. Moreover, as the findings of Simonov and Rao (2022) suggest, an average consumer of (state-owned) media news in Russia—at least back in the 2010s—has a distaste for pro-governmental ideology. This, when coupled with the declining income of poorer households, may have produced a large negative unintended effect on the Russian government, which is clearly not what the Kremlin's policy aims to achieve.

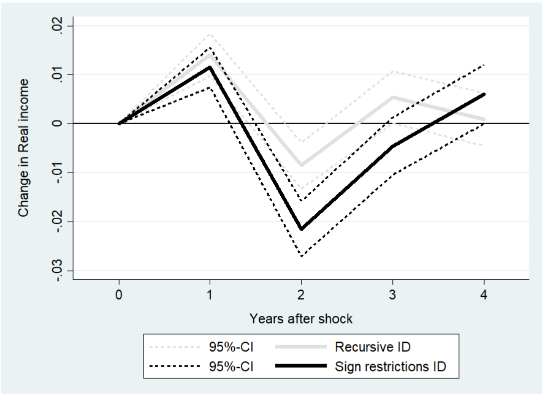
Our findings contrast with those of Neuenkirch and Neumeier (2016), who reveal that US sanctions typically led to a rising poverty gap in the sanctioned countries in the past, i.e., prior to the Crimea-related restrictions. The authors did not account for the potential



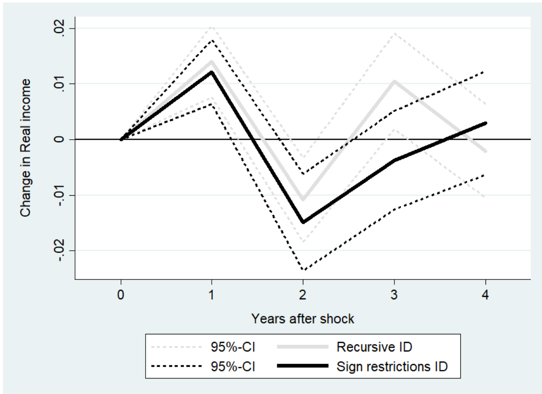
(a) High income, region's other places of living



(b) High income, region's capital city



(c) Low income, region's other places of living



(d) Low income, region's capital city

Note: The figure reports the impulse responses of the individuals' income (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using Jorda (2005) LP approach. The sample contains 74,356 individual–year observations for 21,813 individuals over the period of 2006–2018. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

Figure 2.13: The effects of the sanction shock on the real income in a cross-section of households

support for the poorer population by the sanctioned government. The unintended effect of reducing income inequality that we find is, however, unlikely to persist over time, since our estimates show that the positive effect on the poorer households lasted for only one year.

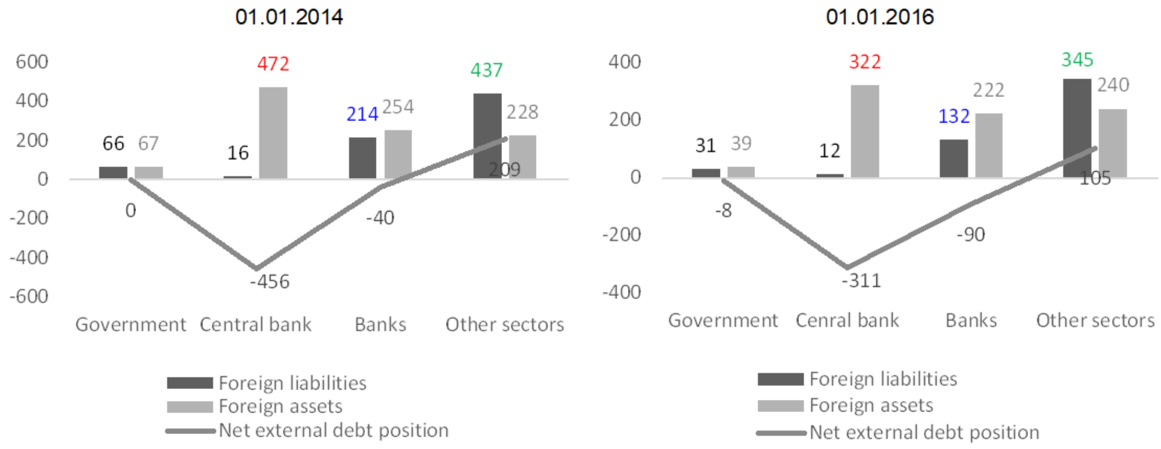
2.6 Conclusion

Our analysis shows that the effects of financial sanctions on the Russian economy in the 2010s were at best modest. In 2014-2015 (*the first wave of sanctions*), the economy would have fallen into recession—even without sanctions—due to the oil prices shock and endogenous monetary policy response. In 2017–2018 (*the second wave of sanctions*), the macro effects of sanctions were near zero.

However, in the 2020s, with Russia's invasion of the Eastern and Southern territories of Ukraine, the situation is dramatically different (*the third wave of sanctions*). The war and the sanctions, even absent of a potential oil and gas embargo, are likely to produce one of the deepest economic crises in Russia over the last three decades, most comparable to the transformation crisis (1992) that followed the Soviet Union's collapse and possessing some features of the sovereign default crisis (1998). The Russian economy will nonetheless continue to rely on the existing export model, which is difficult to change. The population will struggle with the â€new poor' who will be appealing to the mechanisms of household adaptation to deep crises widely employed in the 1990s (switching from the informal sector of the economy and turning to home production of food due to high inflation). As a negative unintended spillover effect, this will not only impact the Russian population but also households in many developing countries across the globe.

The key question, which is difficult to answer and quantify, is whether the international coordination of sanctions against Russia will be strong enough to combat Russia's attempts to restore broken supply chains through the use of 'gray' export-import arrangements with China and other Eastern countries that have not formally joined the Western sanctions.

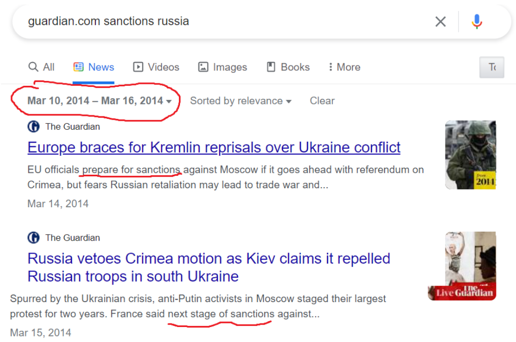
2.A Net foreign debt positions



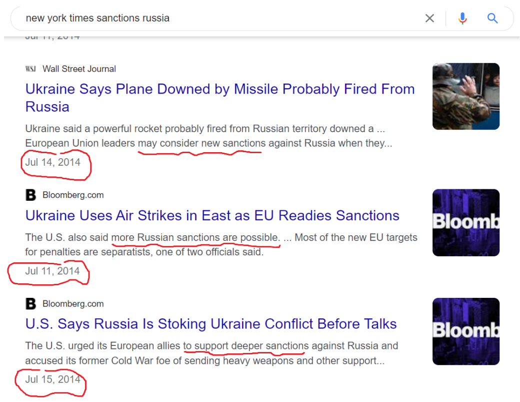
Source: The Central Bank of Russia

Figure 2.1: Net foreign debt position of different sectors of the Russian economy before and after the 2014 sanctions

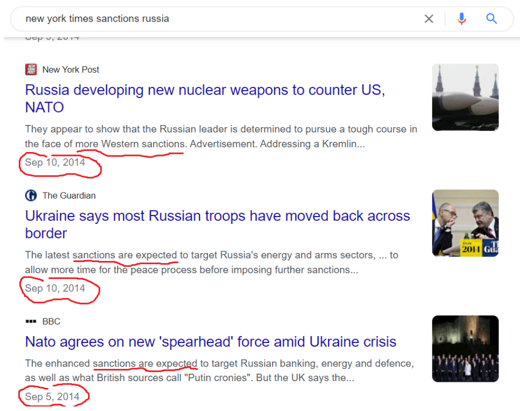
2.B News on upcoming sanctions



(a) Before 20 March 2014



(b) Before 16 July 2014

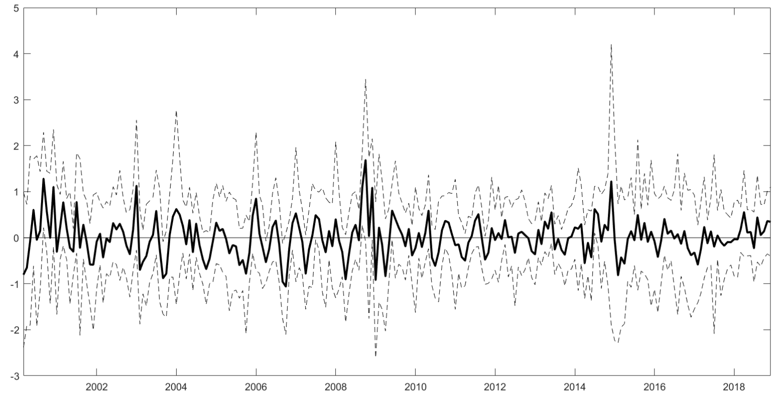


(c) Before 12 September 2014

Note: The figure reports news search results of the following form "[*Name of the media* Russia sanctions] in a five-day time interval before the sanction announcements by OFAC on 20 March 2014 (*a*, 16 July 2014 (*b*), and 12 September 2014 (*c*).

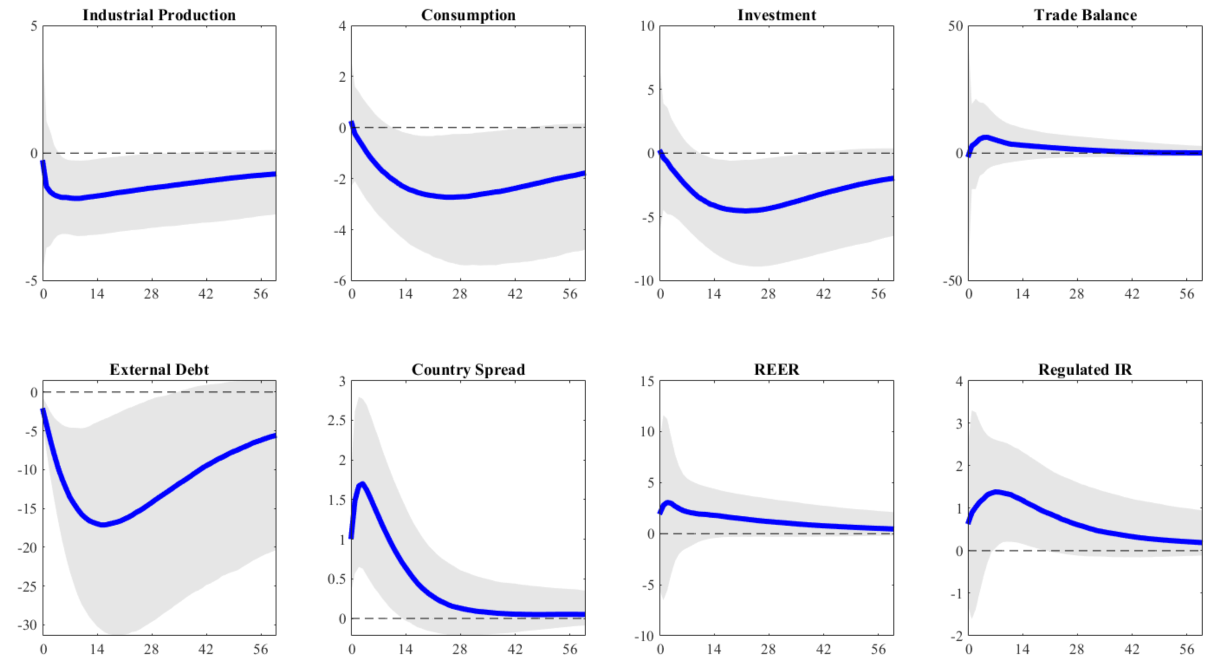
Figure 2.1: Anticipation of sanctions (informational leakage) on the eve of sanction announcements

2.C International credit supply shock in a VAR model with monetary policy



Note: The figure reports the time evolution of the estimated negative shock to the international credit supply (ICS) shock isolated with the use of 11-variable VAR model. Positive values of the shock variable reflect unexpected declines of ICS, and vice versa.

Figure 2.1: Time evolution of the international credit supply shock identified under the sign restrictions



Note: The figure reports the estimated IRFs of domestic macroeconomic variables to the sanction shock identified using the sign restrictions scheme as an international credit supply shock. The IRFs are re-scaled so that the shock is equivalent to a +1 percentage point rise of $Country.Spread_t$. The BVAR model contains 11 variables: external characteristics—commodity terms-of-trade ($CTOT_t$), the Baa corporate bond spread ($Baa.Spread_t$), the real interest rate in the U.S. economy ($US.Real.Interest.Rate_t$); domestic indicators—industrial production (IP_t), private consumption ($Consum_t$), investments ($Invest_t$), trade balance (TB_t), corporate external debt ($ExtDebt_t$), Russia's country spread ($Country.Spread_t$), real effective exchange rate ($REER_t$), and the regulated interest rate (real, $Regulated.IR_t$). The last variable captures the monetary policy reaction to the sanctions shock. The $Country.Spread_t$ variable is ordered third last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 2.2: Impulse response functions to the international credit supply shock identified under the sign restriction scheme that accounts for endogenous monetary policy reactions

2.D Back-up for the sign restrictions: recursive identification

We now discuss an alternative approach: instead of capturing a negative ICS shock using the sign restriction scheme (2.5) we isolate a positive shock to country spread—an unexpected rise in the country risk premium—by applying a more conventional recursive identification. First, the sign restriction scheme applied in the main text lacks identification of other important shocks (CTOT, monetary policy, etc.), which could also affect the economy during the sanctions shock (as it was in 2014). Second, there is ample literature arguing that country spread shocks account for a non-negligible part of business cycle fluctuations in EMEs (Uribe and Yue 2006; Garcia-Cicco, Pancrazi, and Uribe 2010; Chang and Fernandez 2013). Moreover, there is an established procedure for the identification of these shocks, which we follow to ensure comparability with the literature and support our baseline results from the previous section.[43]

To isolate a country spread shock using the recursive identification, the literature typically assumes that the *country spread* variable reacts to the shocks to other variables immediately, whereas a *shock* to the country spread affects other (domestic) variables only with a time lag. Put differently, the country spread variable is usually ordered last (Uribe and Yue 2006; Akinci 2013; Born et al. 2020; Monacelli, Sala, and Siena 2023).

Recall, however, that we study a larger VAR model than in prior studies: we include REER in the set of domestic variables as one of the channels through which the sanctions transmit to the economy. Monacelli, Sala, and Siena (2023) mention that there is a potential problem if the country interest rate (or spread) is ordered *after* REER: this would imply that REER does *not* react to innovations in domestic interest rate, which is clearly dubious. Therefore, in our recursive identification, we place REER *last*, the domestic regulated interest rate *second last*, and the country spread *third last*.[44] The matrix B_0^{-1} is thus assumed to have the following structure, being lower triangular with unit elements on the main diagonal:

[43] In addition, credible bands for the estimated impulse responses are likely to be more narrow under the recursive scheme (RS) as compared to the sign restrictions (SR). This is because RS uses just one structural model to identify shocks and impulse responses to them; conversely, SR effectively uses multiple models to produce generalized impulse responses.

[44] In the sensitivity analysis, we also consider the five-variable VAR of Uribe and Yue (2006) which does not contain REER or a foreign block, and in which we order country interest rate last.

$$
\begin{pmatrix} \vdots \\ u_t^{IP} \\ u_t^{C} \\ u_t^{I} \\ u_t^{TB} \\ u_t^{D} \\ u_t^{S} \\ u_t^{RIR} \\ u_t^{REER} \end{pmatrix}
=
\begin{pmatrix}
\ddots & & & & & & & & \\
\ldots & 1 & & & & & & & \\
\ldots & . & 1 & & & & & & \\
\ldots & . & . & 1 & & & & & \\
\ldots & . & . & . & 1 & & & & \\
\ldots & . & . & . & . & 1 & & & \\
\ldots & . & . & . & . & . & 1 & & \\
\ldots & . & . & . & . & . & . & 1 & \\
\ldots & . & . & . & . & . & . & . & 1
\end{pmatrix}
\begin{pmatrix} \vdots \\ \varepsilon_t^{IP} \\ \varepsilon_t^{C} \\ \varepsilon_t^{I} \\ \varepsilon_t^{TB} \\ \varepsilon_t^{D} \\ \varepsilon_t^{S} \\ \varepsilon_t^{RIR} \\ \varepsilon_t^{REER} \end{pmatrix}
\tag{2.12}
$$

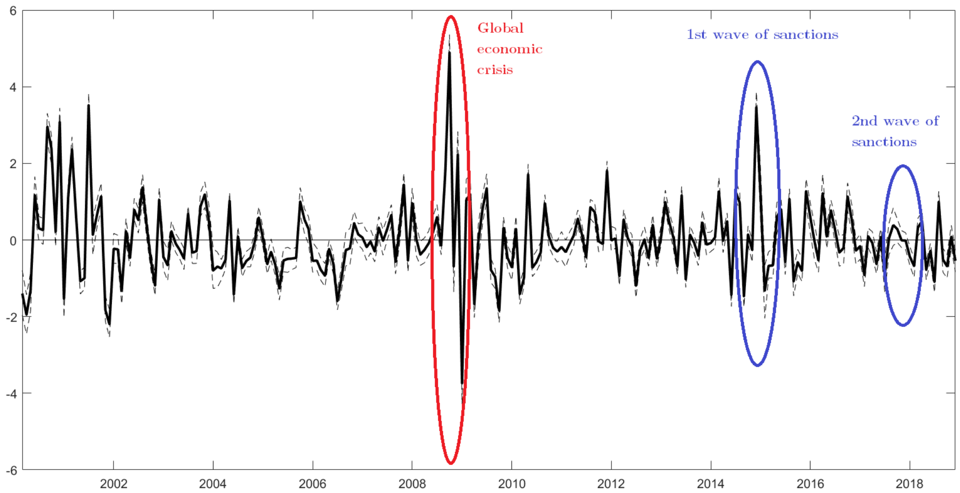
where "⋮" and "…" correspond to the three exogenous variables, "." implies a non-empty element, and the empty cells, by the construction of the lower triangular, contain zeros.

Differently from the sign restrictions scheme (2.5), we now have a shock to the country spread ε_t^S instead of a shock to ICS $\varepsilon_t^{Credit\,Supply}$. We also now obtain shocks to CTOT ε_t^{CTOT} and the domestic regulated interest rate ε_t^{RIR}. The latter allows us to be sure that we do not confuse the sanctions shock of 2014 with a slump in the world oil price and the dramatic rise of the domestic regulated interest rate that both occurred during the same time. However, the drawback of (2.12) is that it does not distinguish demand and supply-driven forces in the dynamics of external debt.

Let us now turn to the estimation results that we obtain under the recursive identification (2.12). As can be inferred from Fig. 2.1, the time evolution of the estimated country spread shock ε_t^S is remarkable. First, we observe a sharp spike at the end of 2014 which can clearly be attributed to the first wave of the financial sanctions. The size of the shock equals +4 p.p. which, according to our estimates, is the second strongest shock over the last two decades after the shock associated with the global economic crisis of 2007–2009 (+5 p.p., at the beginning of 2009). Further, when we turn to the second wave of sanctions, we recognize a credible positive shock in the second half of 2017, but its size is at least three times lower than during the first wave. This again implies that the second wave could have much less harmful macroeconomic effects. We also note that ε_t^S shocks of a size comparable to that of the second wave of sanctions occur very often, according to our estimates. Overall, we obtain qualitatively the same result for the dynamics of the country spread shock as the one under the sign restriction scheme presented in the main text (see Fig. 2.5).

The estimated impulse responses to the identified country spread shock appear in Fig. 2.2.[45] As before, all impulses were re-scaled to a +1 percentage point shock to the country spread. First, we obtain a negative and significant reaction of the volume of external debt, with the peak response being equal –5 p.p. On one hand, it implies

[45]Estimated responses from a recursive identification scheme that ignores monetary policy imply the same results and are reported in Appendix 2.E.



Note: The figure reports the time evolution of the sanctions shock estimated with the BVAR model containing 11 variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported. Substantial spikes in the time series of the estimated shock are identified for the first but not for the second wave of sanctions at the end of 2014 and 2017, respectively. One more is identified for the period of 2008–2009 global economic crisis and is reported for comparative reasons.

Figure 2.1: Time evolution of a positive shock to the country spread identified under the recursive identification scheme

that supply-driven forces dominate over demand for external debt. On the other hand, this peak reaction is at least three times lower in magnitude than the analogous estimate under the sign restriction scheme. This indirectly implies that demand also plays a large role in determining the inflows of external debt to Russia.

Second, the real economy also reacts negatively to the country spread shock: industrial production declines by almost 1 percentage point, private consumption by slightly more than 1 percentage point, and investment by 1.4 p.p. Strikingly, these estimates are two to three times lower in magnitude as compared to their analogs obtained under the sign restrictions in the previous section. However, the credible bands indeed become much more narrow than before. What is also remarkable is that the estimated responses are now much less persistent than before. Overall, the results obtained with the recursive identification are qualitatively the same as those achieved with the sign restrictions, thus supporting our baseline findings.

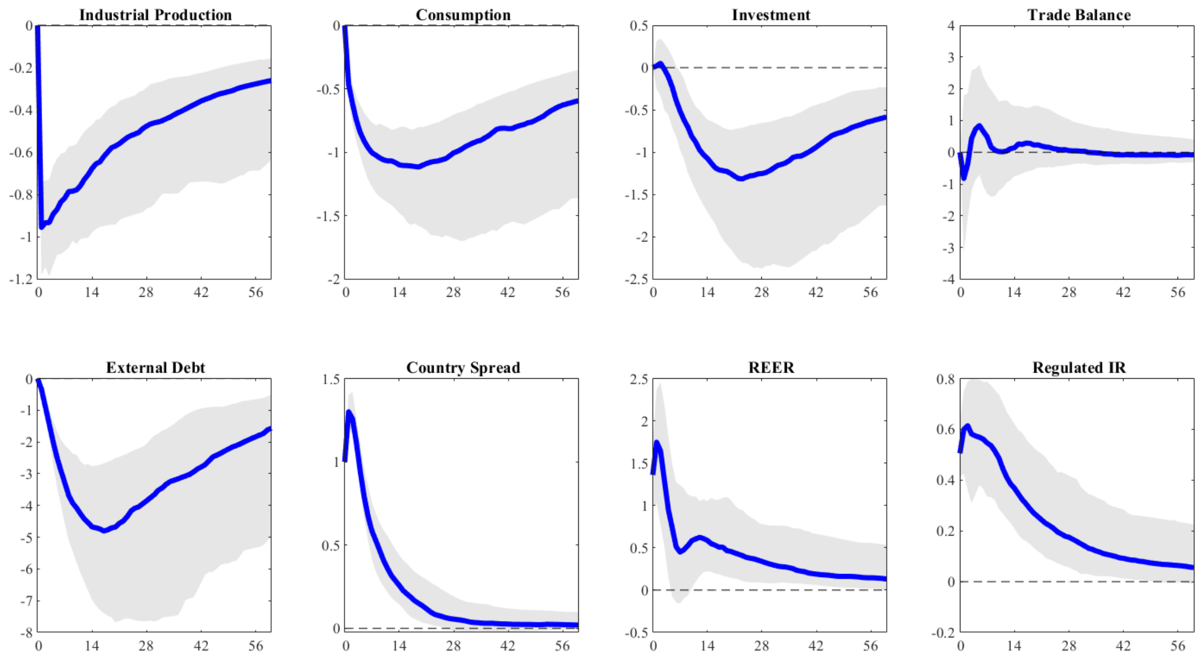
Third, trade balance tends to respond positively to the positive country spread shock, however, the response is again insignificant, as it was before. REER also reacts positively and, in turn, significantly, with a response peaking at +1.8 p.p. This is again lower than in the baseline, by a factor of two. As before, the depreciation of the Ruble in response to the positive country spread shock is justified by the necessity to repay external debts (i.e., the magnitude of the external debt's decline), which, in turn, becomes possible via (marginal) improvement of the trade balance.

Finally, the domestic regulated interest rate also reacts positively to the country's spread shock, thus accommodating the increased price of foreign borrowings. The estimated peak reaction equals +0.6 p.p., which is less than one and is lower than in the baseline by a factor of three.

Therefore, the results obtained under the recursive identification fully support our baseline results, although the estimated impulse responses differ quantitatively. Recall, however, that the estimated size of the ICS shock in 2014, i.e., during the first wave of the financial sanctions, is lower by a factor of three as compared to the size of the country's spread shock during the same time. This means that the resulting estimates of the effects of sanctions are expected to be comparable across the two SVAR-based identification schemes (see Section 2.4.3 in the main text).

One more issue which we elaborate on in this section is how important for the economy are the shocks to the country spread in comparison with the shocks to CTOT and domestic monetary policy (MP), as identified through the recursive scheme. Impulse responses of the domestic macroeconomic variables to the (positive) CTOT and (restrictive) MP shocks are reported in Appendices Appendix 2.F and Appendix 2.G, respectively. Recall that the country spread shock was set at +1 percentage point when we were computing the effects of this shock above. If we now re-scale both responses to the CTOT and MP shocks accordingly so that they are equivalent to +1 percentage point rises of the country spread, then we obtain the following result. Industrial production reacts negatively and significantly to both (negative) CTOT and (restrictive) MP shocks, with the peaks reaching –2 p.p. ($+0.4 \times (-5)$) and –2.4 p.p. (-0.47×5), respectively.[46] This result means that oil price drops, as captured by negative CTOT shocks, and rises in domestic interest rate, as captured by restrictive MP shocks, both force the Russian economy to decline deeper than the shocks to the country spread. This argument exhibits its importance in Section 2.4.3 of the main text where we compute the final effects of the financial sanctions.

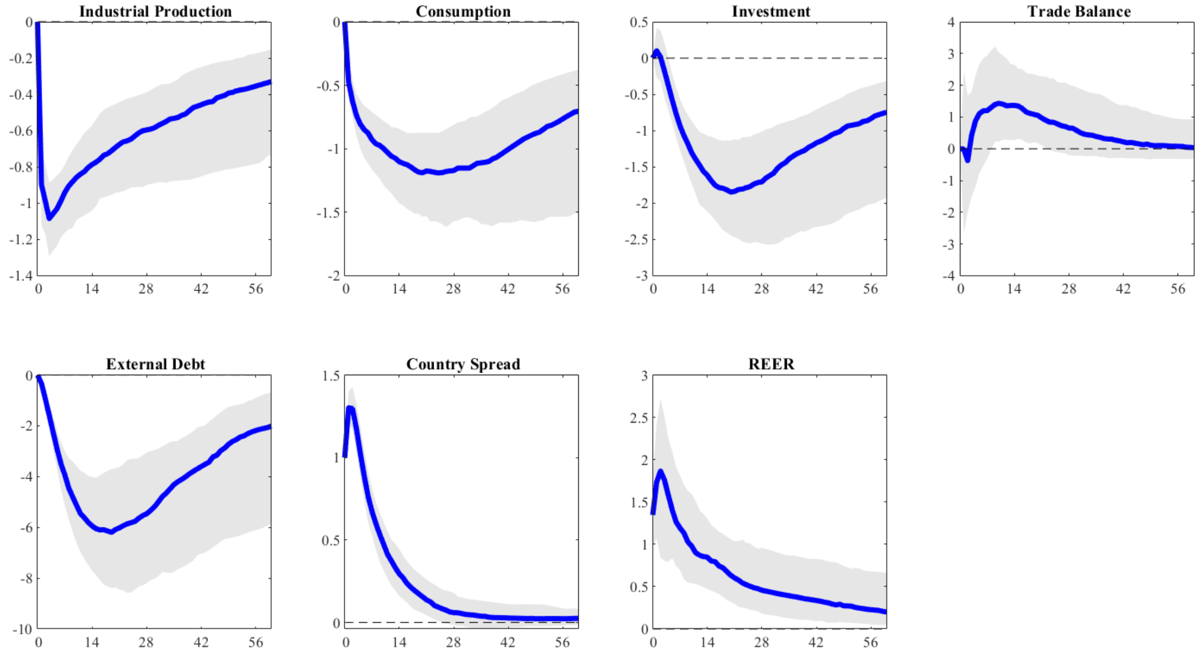
[46] In here, -5 and 5 are the re-scaling factors that force the country spread to reach a +1 percentage point rise in response to CTOT and MP shocks.



Note: The figure reports the estimated IRFs of domestic macroeconomic variables to the sanction shock identified using the recursive scheme as a shock to country spread. The IRFs are re-scaled so that the shock is equivalent to a +1 percentage point rise of $Country.Spread_t$. The BVAR model contains 11 variables: external characteristics—commodity terms-of-trade ($CTOT_t$), the Baa corporate bond spread ($Baa.Spread_t$), the real interest rate in the U.S. economy ($US.Real.Interest.Rate_t$); domestic indicators—industrial production (IP_t), private consumption ($Consum_t$), investments ($Invest_t$), trade balance (TB_t), corporate external debt ($ExtDebt_t$), Russia's country spread ($Country.Spread_t$), the real effective exchange rate ($REER_t$), and the regulated interest rate (real, $Regulated.IR_t$). The last variable captures the monetary policy reaction to the sanctions shock. The $Country.Spread_t$ variable is ordered third last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 2.2: Impulse response functions to the country spread shock identified under the recursive scheme that accounts for endogenous monetary policy reactions

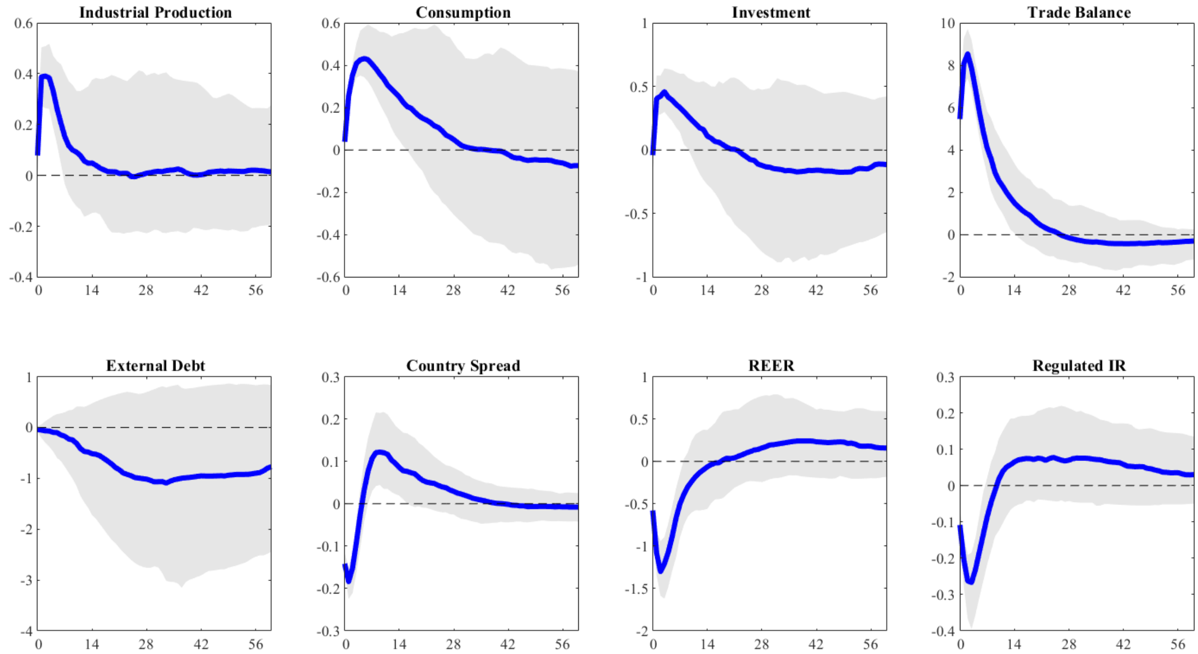
2.E Recursive identification of the country spread shock ignoring monetary policy



Note: The figure reports the estimated IRFs of domestic macroeconomic variables to the sanction shock identified using the recursive scheme as a shock to country spread. The IRFs are re-scaled so that the shock is equivalent to a +1 percentage point rise of $Country.Spread_t$. The BVAR model contains 10 variables: external characteristics—commodity terms-of-trade ($CTOT_t$), the Baa corporate bond spread ($Baa.Spread_t$), the real interest rate in the U.S. economy ($US.Real.Interest.Rate_t$); domestic indicators—industrial production (IP_t), private consumption ($Consum_t$), investments ($Invest_t$), trade balance (TB_t), corporate external debt ($ExtDebt_t$), Russia's country spread ($Country.Spread_t$), real effective exchange rate ($REER_t$). Monetary policy response to the sanctions shock is ignored in this version. The $Country.Spread_t$ variable is ordered third last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 2.1: Impulse responses to the country spread shock identified under the recursive scheme

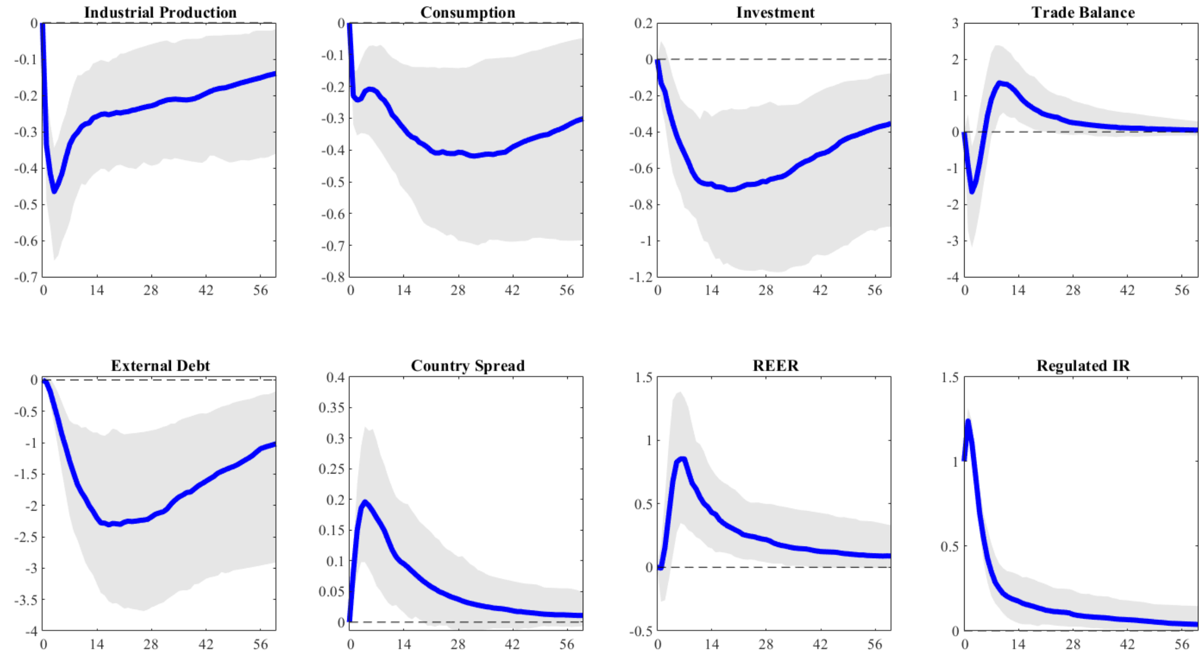
2.F The effects of a CTOT shock on domestic macroeconomic variables



Note: The figure reports estimated IRFs of domestic macroeconomic variables to a positive CTOT shock identified recursively. The BVAR model contains 11 variables. Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 2.1: Impulse responses to a *positive* CTOT shock under recursive identification

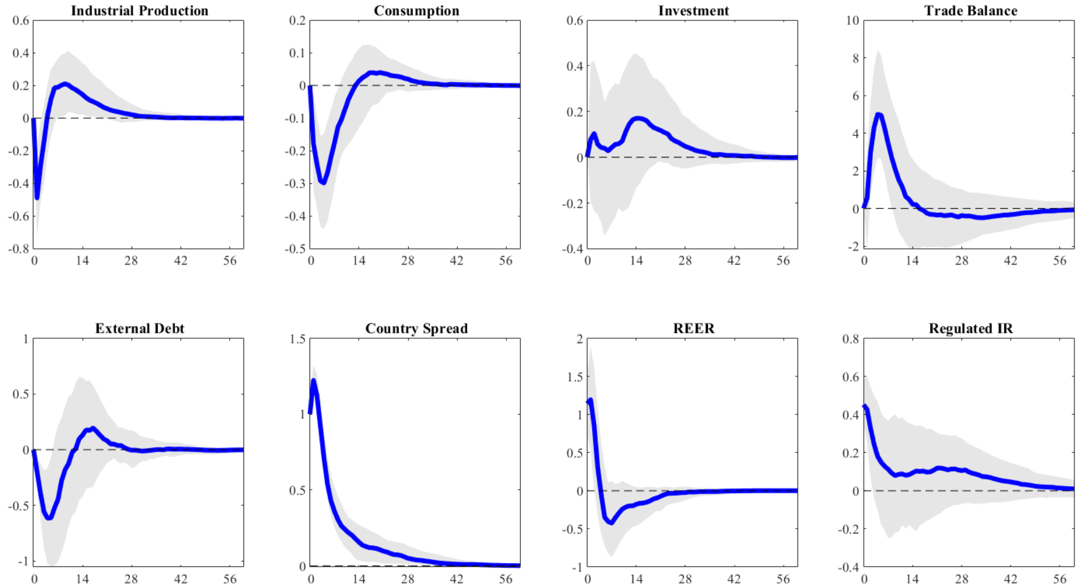
2.G The effects of a monetary policy shock on domestic macroeconomic variables



Note: The figure reports estimated IRFs of domestic macroeconomic variables to a restrictive monetary policy shock identified recursively. The shock is normalized to +1 p.p. rise of the regulated interest rate on impact. The BVAR model contains 11 variables. Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

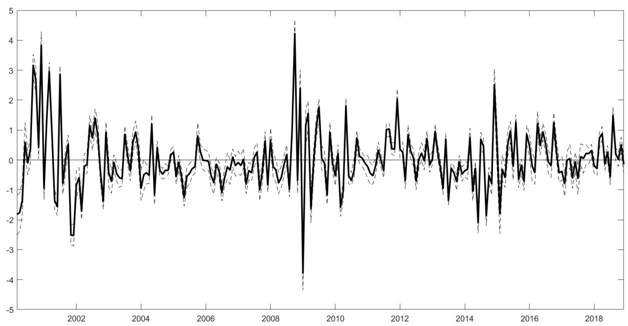
Figure 2.1: Impulse responses to a *restrictive* monetary policy shock under recursive identification

2.H Recursive identification with the HP-filtered time series



Note: The figure reports estimated impulse responses of domestic macroeconomic variables to a +1 percentage point shock in the country spread variable. The VAR model contains 11 variables, and the country spread variable is ordered third last, i.e., before the REER and domestic regulated interest rate variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated responses are reported (grey shaded area).

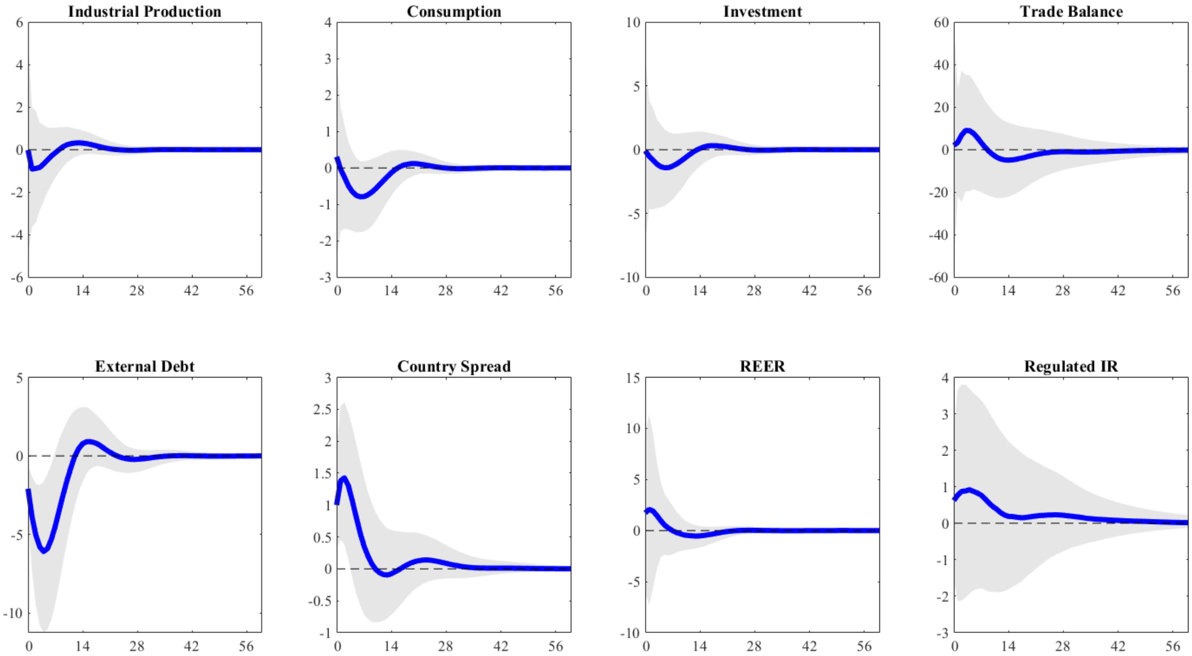
Figure 2.1: Impulse responses to the country spread shock identified under the recursive scheme



Note: The figure reports the time evolution of the positive shock to the country spread estimated with the 11-variable VAR model. Positive values of the shock variable reflect unexpected rises of Russia's country spread, and vice versa.

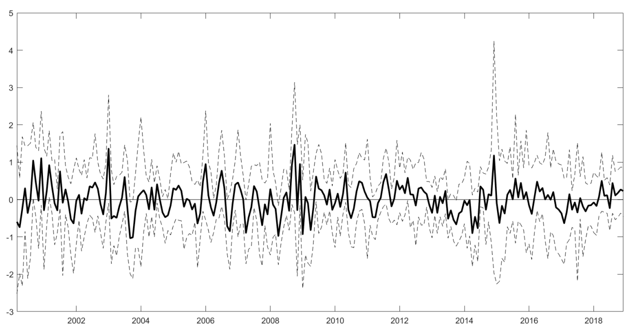
Figure 2.2: Time evolution of the country spread shock identified under the recursive scheme

2.I Sign restrictions with the HP-filtered time series



Note: The figure reports estimated impulse responses of domestic macroeconomic variables to a negative international credit supply (ICS) shock. The shock is re-scaled so that it is equivalent to a +1 percentage point rise in the country spread variable. The VAR model contains 11 variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated responses are reported (grey shaded area).

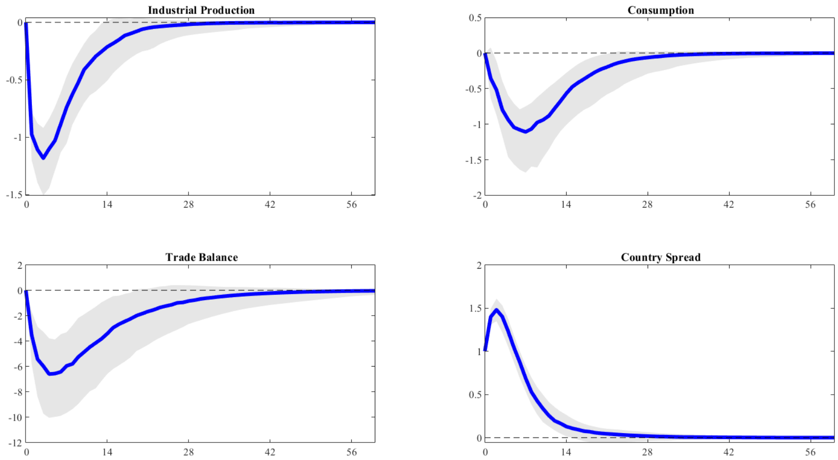
Figure 2.1: Impulse responses to the international credit supply shock identified under the sign restrictions



Note: The figure reports the time evolution of the estimated negative shock to the international credit supply (ICS) shock isolated with the use of the 11-variable VAR model. Positive values of the shock variable reflect unexpected declines in ICS and vice versa.

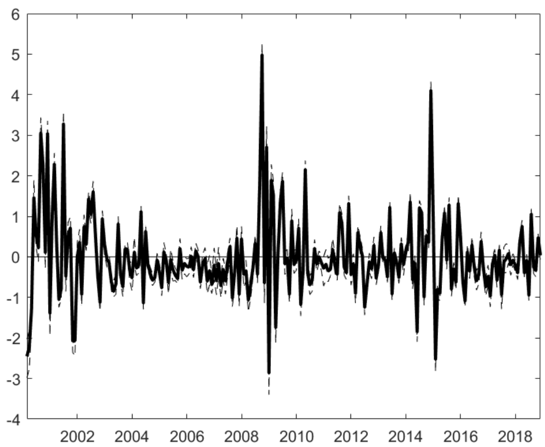
Figure 2.2: Time evolution of the international credit supply shock identified under the sign restrictions

2.J Recursive identification in the five-variable VAR model of Uribe and Yue (2006)



Note: The figure reports estimated impulse responses of domestic macroeconomic variables to a +1 percentage point shock in the country spread variable. The VAR model contains the five variables, as in Uribe and Yue (2006). The country spread variable is ordered last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

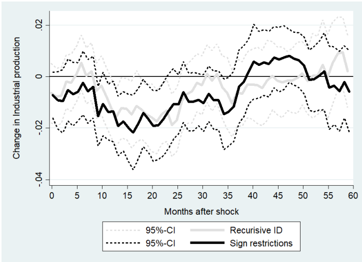
Figure 2.1: Impulse responses to the country spread shock identified under the recursive scheme in a five-variable VAR model



Note: The figure reports the time evolution of the positive shock to the country spread estimated with the five-variable VAR model. Positive values of the shock variable reflect unexpected rises of Russia's country spread, and vice versa.

Figure 2.2: Time evolution of the country spread shock identified under the recursive scheme in a five-variable VAR model

2.K Jorda local projection



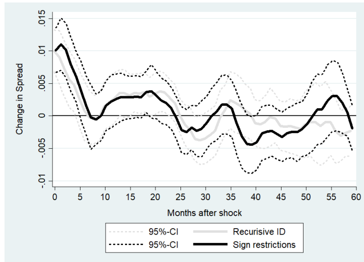
(a) Industrial production

(b) Consumption

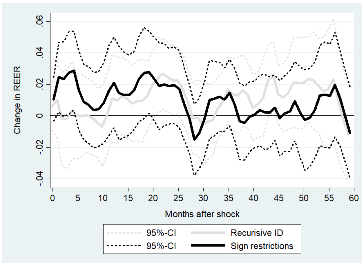
(c) Investment

(d) Trade Balance

(e) External Debt



(f) Spread



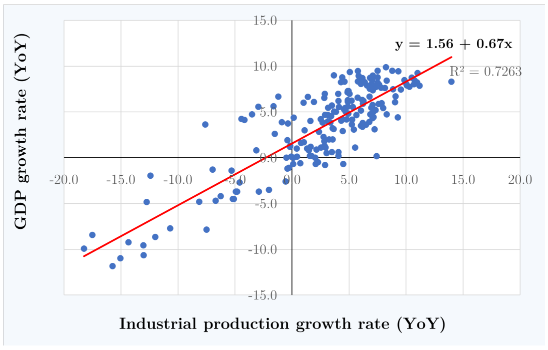
(g) REER

(h) Interest rate

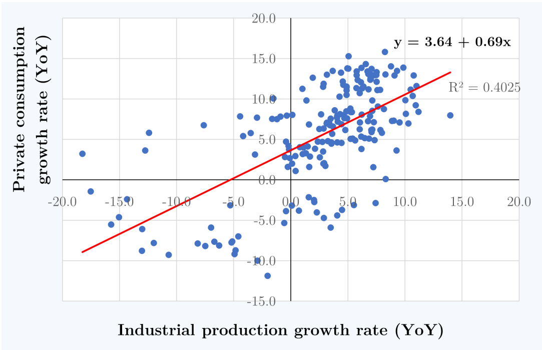
Note: The figure reports impulse responses to a positive country spread shock (*Recursive ID*) and a negative ICS shock (*Sign restrictions*). The responses are obtained under the Jorda's LP approach, as implied by $\beta_{j,h}$ in the following equation: $y_{i,t+h} = \alpha_{i,j,h} + \beta_{j,h} \cdot \hat{\varepsilon}_t^{(j)} + \gamma'_{i,j,h}\mathbf{X}_t + \mu_{i,j,t+h}$, where $y_{i,t}$ is ith domestic macroeconomic variable considered above ($i = 1, 2...8$), t is month from January 2000 to December 2018 and $h = 1, 2...60$ is prediction step ahead of the shock $\hat{\varepsilon}_t^{(j)}$, where $j = 1$ stands for the recursive identification (*country spread shock*) and $j = 2$ the sign restrictions scheme (*ICS shock*). $\mathbf{X}_t$ contains control variables: all monthly lags of $\hat{\varepsilon}_t^{(j)}$ from $1st$ till $12th$, thus covering the whole previous year, and the current values and 12th month lags of each of the eleven variables in y_t.. The 95% confidence intervals are computed with bootstrap (500 draws, with replications).

Figure 2.1: Impulse responses to the international credit supply shock and country spread shock estimated with the Jorda's local projection

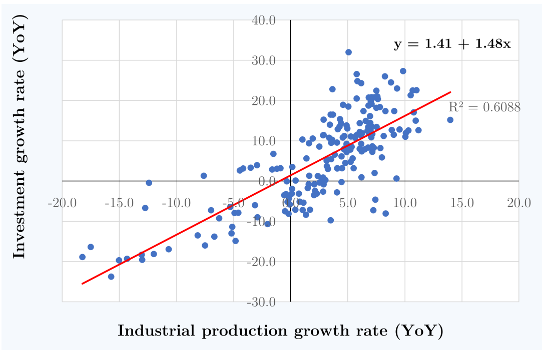
2.L Industrial production and GDP components



(a) GDP



(b) Private consumption



(c) Investment

Note: The figure reports empirical relationships between industrial production and various macroeconomic characteristics of the Russian economy. The data covers the period of January 2000 to December 2018.

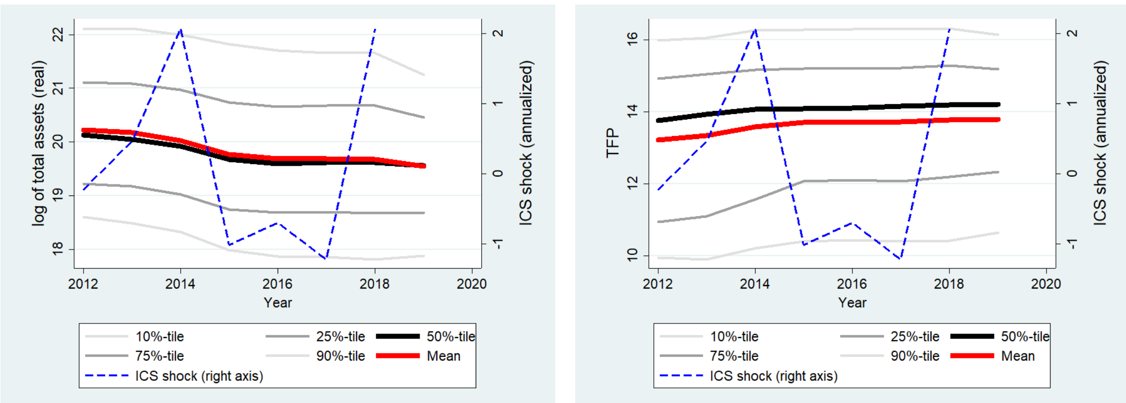
Figure 2.1: Relationship between industrial production and GDP components

2.M Details on the cross-section of firms

Table 2.1: Summary statistics for production function estimates

Note: The table reports the estimates of firms' total factor productivity, $TFP_{f,t}$, and the summary statistics for the variables employed in its estimation. We apply the methodology of Wooldridge (2009) and Petrin and Levinsohn (2012) to estimate a production function with the real value added $Y_{f,t}$ as a dependent variable and the number of employees $L_{f,t}$, capital (as proxied with fixed assets) $K_{f,t}$, and intermediate inputs (materials, as proxied with payments to suppliers) $M_{f,t}$. The estimation period is 2012–2019.

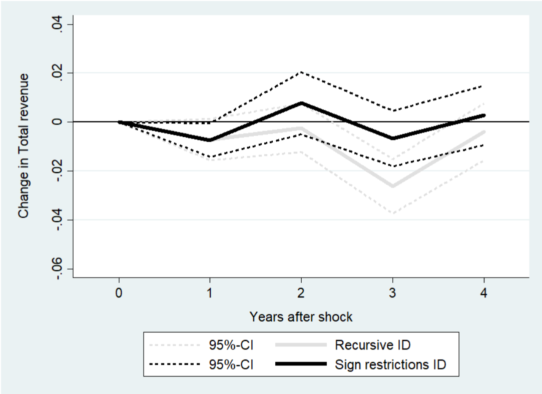
	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
$TFP_{f,t}$	40,381	13.6	2.2	6.1	21.4
$\ln Y_{f,t}$	40,381	18.5	1.5	12.2	23.2
$\ln L_{f,t}$	40,381	5.1	1.6	0.0	8.8
$\ln K_{f,t}$	40,381	18.3	2.0	8.9	23.8
$\ln M_{f,t}$	40,381	19.3	1.8	11.0	24.4



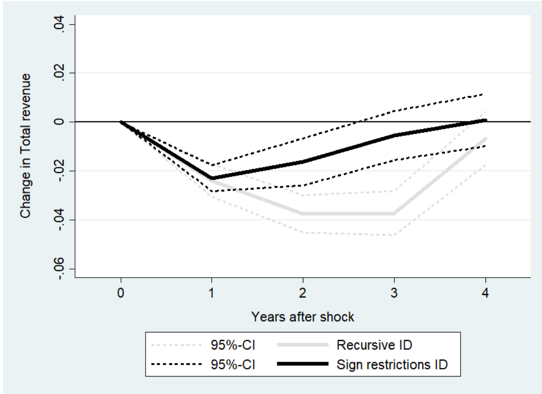
(a) Firm size $lnTA_{f,t}$

(b) Firm productivity $TFP_{f,t}$

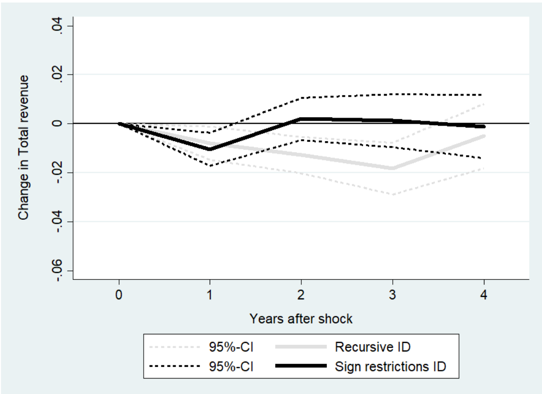
Figure 2.1: Firm size and productivity



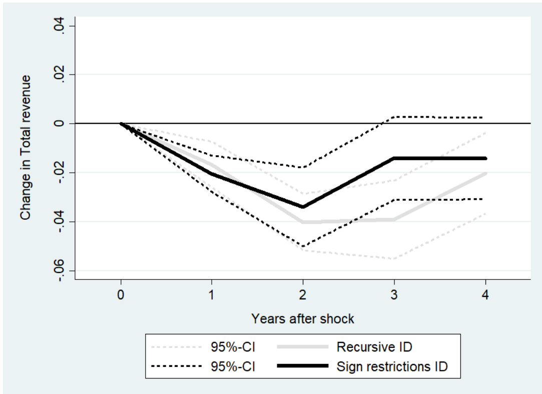
(a) Small firms with high TFP



(b) Large firms with high TFP



(c) Small firms with low TFP



(d) Large firms with low TFP

Note: The figure reports the impulse responses of the firms' total revenue (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using the Jorda (2005)'s LP approach. The sample contains 81,004 firm–year observations for 32,790 firms over the period of 2012–2018. The condition that the firms must operate for at least three consecutive years is not imposed. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

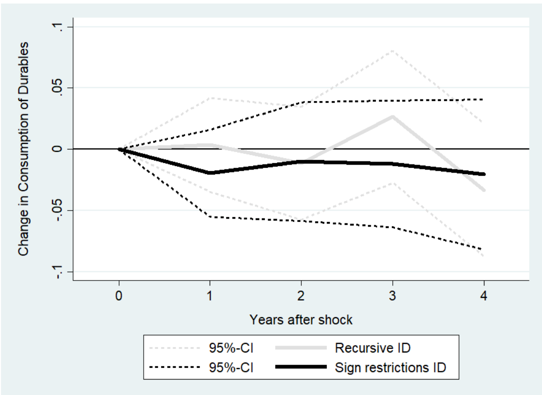
Figure 2.2: The effects of the sanctions shock on the real total revenue in a cross-section of firms

2.N Details on the cross-section of households

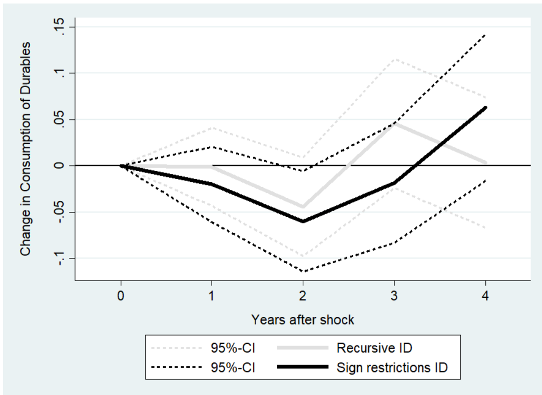
Table 2.1: Summary statistics for the sample of households

Note: The table reports the summary statistics on individuals' total income $Y_{i,t}$, total consumption $C_{i,t}$, consumption of durables $C^D_{i,t}$, and consumption of non-durables $C^N_{i,t}$, all in constant 2014 prices. The sample contains 21,813 individuals over the period of 2006–2018.

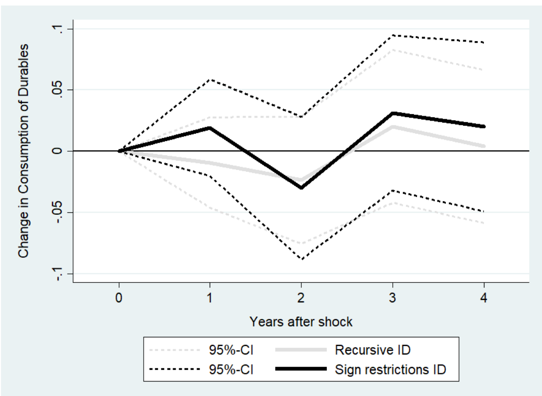
	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
$\ln Y_{h,t}$	74,356	5.9	0.6	4.1	7.2
$\ln C_{h,t}$	74,356	5.5	0.6	3.6	7.0
$\ln C^D_{h,t}$	74,356	3.1	1.7	–3.4	8.9
$\ln C^N_{h,t}$	74,356	5.3	0.7	–1.0	10.2



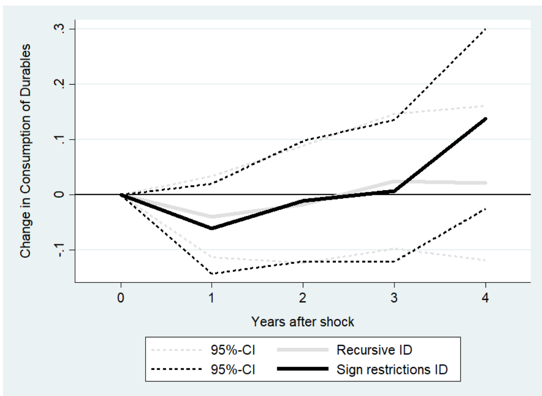
(a) High income, region's other places of living



(b) High income, region's capital city

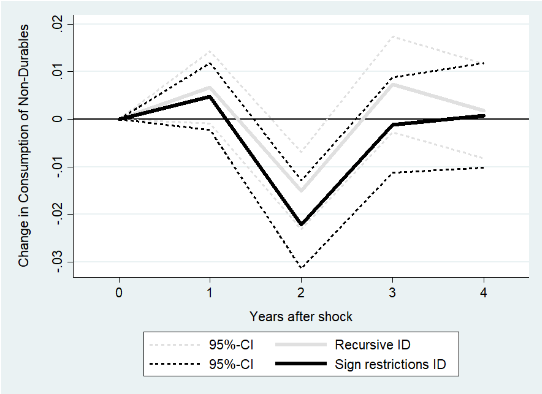


(c) Low income, region's other places of living

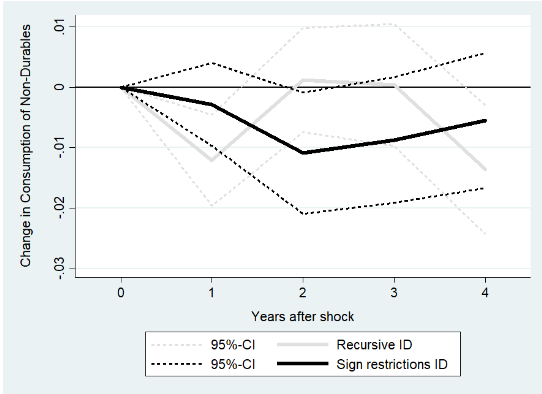


(d) Low income, region's capital city

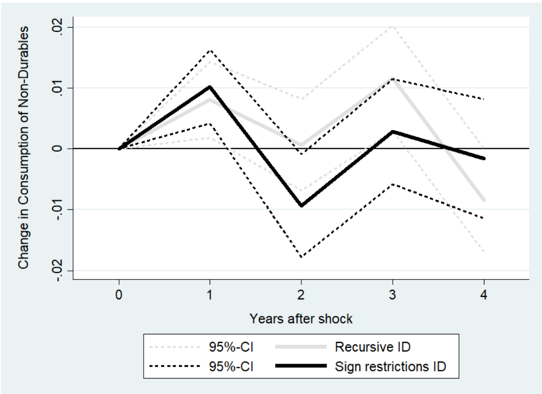
Figure 2.1: The effects of the sanctions shock on consumption of durables in a cross-section of households (*beginning*)



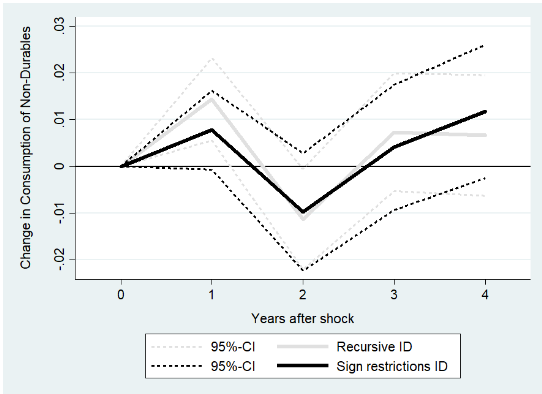
(e) High income, region's other places of living



(f) High income, region's capital city



(g) Low income, region's other places of living



(h) Low income, region's capital city

Note: The figure reports the impulse responses of the individuals' income (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using the Jorda (2005)'s LP approach. The sample contains 74,356 individual–year observations for 21,813 individuals over 2006–2018. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

Figure 2.1: The effects of the sanctions shock on consumption of durables in a cross-section of households (*ending*)

Chapter 3

Quo vadis? Evidence on new firm-bank matching and firm performance following "sin" bank closures

Co-authored with Roman Goncharenko (KU Leuven), Steven Ongena (University of Zurich, Swiss Finance Institute, KU Leuven, CEPR), Svetlana Popova (National Research University "Higher School of Economics", the Bank of Russia), and Natalia Turdyeva (the Bank of Russia)

Published as CEPR Discussion paper No 17015 and CERGE-EI Working Paper Series No 754

3.1 Introduction

Firms derive value from bank credit beyond the benefit of obtaining external financing. Banks can help mitigate information asymmetries between lenders and borrowers through screening (Leland and Pyle 1977), and to reduce moral hazard through monitoring (Holmstrom and Tirole 1997). Through a lending relationship with a borrowing firm, a bank can gain proprietary information about the firm and potentially influence decisions taken by the firm's management (Petersen and Rajan 1995), and the firm may expect support from the relationship bank in times of distress (Bolton et al. 2016; Schafer 2019). Thus, losing an established relationship with a bank due to the bank's failure can have negative effects on a firm. However, the negative consequences of such an event are less clear if banks are forcibly closed by a financial regulator due to fraudulent activities revealed. In this chapter, we explore the phenomenon of fraudulent ("sin") banks to understand what happens to firms borrowing from these banks after the financial regulator breaks the sin bank-firm relationships.

We analyze how firms match with new banks when their prior lenders are forcibly closed by the regulator. We examine what happens to the firms' performance during the transition period, i.e., after their sin banks are closed and before they match with new banks. Moreover, we investigate whether there are differences between poorly-performing, loss-making firms ("bad" firms, hereinafter) and well-performing, profitable firms ("good" firms) in this respect, given that both could have had relationships with the same closed bank.

How firms fare after the closure of their banks remains an open question of sizable academic and policy-making interest. Empirical studies have examined how firms are affected by negative credit supply shocks (Chodorow-Reich 2014; Gropp et al. 2018; Degryse et al. 2019; Greenstone, Mas, and Nguyen 2020), the closure of their bank branches (Bonfim, Nogueira, and Ongena 2020), and the failure of their distress banks (Liaudinskas and Grigaite 2021). However, to the best of our knowledge, there are no studies that examine the effect of pre-emptive regulatory closure of banks on firms' consequent matches with new banks and performance. It is vitally important to understand these effects to design optimal bank clean-up policies.

We analyze the recent—and rather dramatic—series of bank closures undertaken by the Central Bank of Russia (CBR), which began in 2013. Its intent was to clean up the banking system by closing banks engaged in fraudulent activities (see historical and institutional details in Section 3.2). This new regime of intensified fraud intolerance followed a period of widespread regulatory forbearance from 2006 to 2013. Over the seven years between 2013 and early 2020—which were characterized by primarily calm economic times—the CBR effectively revoked around two-thirds of all banking licenses in the country. As a result, almost 650 banks were briskly closed when fraud was detected in their operations.

Three characteristics of the policy make it particularly informative. First, the policy began unexpectedly following a prolonged period of regulatory forbearance, which resulted in a large fraction of the banking system being contaminated with fraudulent banks. Second, the active phase of the policy continued for over five years, which allows for the possibility of new matches between firms and not-yet-detected fraudulent banks following the closure of the firms' prior sin bank. Finally, the bulk of the policy was enacted during a period of mostly normal economic times, which provides a clean setting for identifying the real effects of the policy.[1]

We employ loan-level data provided by the Bureau of Credit History (BCH) from 2008 until 2018, and the CBR's credit register, which is available to us from 2017 onward. The former data contain a monthly firm-bank match and the number of days during which a firm was delinquent in its payment of interest and/or principal of a loan. Hereinafter, for simplicity, we refer to this indicator as the *days of NPLs* (Non-Performing Loans, DNPL). DNPL ranges between 0 and 200+, with 0s being interpreted as a "positive" outcome in the sense that a borrowing firm does not delay the interest payments (0s account for roughly 80% of the sample). These data are unique in their coverage and comprehensiveness and were made accessible to independent academic research for the first time in this study. We merge these data with the balance sheet characteristics of firms, taken from the SPARK-Interfax database, and of banks, as gleaned from the CBR website. We also manually collect data on all bank owners and directors during the last decade from a nationwide banking media source. We employ this information to assess

[1]The policy was launched in mid-2013—six months before the Russian economy entered another (local) recession and was hit by Western economic sanctions (Ahn and Ludema 2020). The recession was relatively mild, peaking at–3.1% of GDP growth by 2015Q2 (for comparison, during the world economic crisis of 2007-2009, the Russian economy declined by 11.2% at its peak in 2009Q2). The effect of the sanctions was muted by the preceding largely negative oil price shock in 2014 and because the targeted (state-owned or controlled) banks were largely supported by the government, which allowed them to simply reshuffle credit flows from firms to households (Mamonov, Pestova, and Ongena 2021).

whether firms, following the closure of their sin bank, match with a new bank that has the same or different owners as the closed bank.

We begin our empirical analysis by exploring the determinants of firms' matching with new banks following their prior banks' closures. Many such closures were motivated by the detection of bank fraud, and we apply a duration model to analyze if the closure of such sin banks results in firms of different quality engaging with banks of different standing. We proxy the quality of the firms with two variables: (i) whether the firms have negative profits (firm-level); or (ii) the number of days of NPLs firms had in closed banks (firm-bank level).[2] We find that the lower the quality of loans the firms had in the closed banks, the more likely it is that those firms again matched with (not-yet-detected) sin banks and the less likely that the firms end up at "saint" banks (all other peers survived till the end of the sin bank closure policy). The firms' profitability always has a positive effect on matching.

We also show that the average time to match with another sin bank equals 19 months, while the time to match with a saint bank takes much longer, at 42 months. Our duration regression analysis also shows that compared to a firm with 0 days of NPLs, a firm with 90 days of NPLs is 35% more likely to match with another sin bank and 16% less likely to join a saint bank.

We then investigate several channels through which firm-bank matching may work. First, with our unique data on bank owners and directors, we find that among the 956 banks present after 2010, as many as 238 banks have interlocks with other banks through their bank holding company and/or through owners and/or directors. Following sin bank closure, 50 to 75% of the bad firms again match with a sin bank owned by the *same* owners. It takes only a year and a half to establish such matches. In contrast,

[2]Another important variable that we could use in our analysis to distinguish between good and bad firms is liquidity holdings. As Beck, Da-Rocha-Lopes, and Silva (2020) show in their setting on Portuguese firms borrowing from a large bank that encountered a bail-in, these are exactly low-liquid firms that had to substantially shrink their employment after the credit supply squeeze caused by the bail-in. We, however, stick to our profit-based measures for the baseline estimates, because they provide an integral characteristic of firm performance, and profits affect the value of firms and their market-perceived perspectives.

establishing a new firm–saint bank match takes about three years.[3] Excluding banks with common ownership, we find that following sin bank closure, bad firms are *no more* likely to match with another (not-yet-detected) sin bank. In most instances, good firms match with a new saint bank, regardless of whether the new bank shares common owners and/or directors with the firms' closed bank.

Second, apart from common ownership, we hypothesize that not all sin bank closures are equally predictable by economic agents. Some may be more *predictable* than others, based on publicly observable data reported in the banks' balance sheets. If detection of bank fraud is predictable from its balance sheet we conjecture that the related bad firms will experience difficulties engaging a new bank, even if it is a sin bank. To assess this effect of *surprising* bank closures on firm-bank matching, we follow a two-stage procedure. In the first stage, we run a loop of logit regression models using a six-month rolling window to predict bank fraud detection and flexibly capture the regulator's learning about misreporting approaches employed by fraudulent banks. We sort the closed sin banks into two categories: *surprising* closures contain those with the predicted probability of fraud detection being below the unconditional threshold, and *expected* closures consist of those with the predicted probability being above the same threshold. In the second stage, we re-run the duration model for the two subsamples of firms: those that experienced surprising bank closures and those whose lenders' fraud detection was expected. Our results clearly show that new banks pay attention to where the firms come from: firms whose prior banks were predictably fraudulent do not match easily with new banks, and the sorting of bad firms to new (not-yet-detected) sin banks only works when closures of the bad firms' prior sin banks were surprising.

Third, we show that the concentration of regional credit markets matters for the matching of bad firms and saint banks. The higher the market concentration, the more likely a saint bank operating in this market will engage a bad firm coming from a closed

[3]After a sin bank is closed, its firms continue repaying loans to a receiver (CBR or the Deposit Insurance Agency) until the loans either mature or are sold to other entities (financial or non-financial). Firm-level data reveals that the treated firms with a single bank-firm relationship raise funding from other (non-banking) sources, e.g., through trade credit, before matching with a new bank.

sin bank. This result is consistent with the information acquisition hypothesis in Petersen and Rajan (1995), who argue that banks in more concentrated markets are more willing to finance opaque firms because future retention of the firm is more likely and therefore intertemporal subsidization is possible.

To confirm the validity of our estimates, we perform a placebo test to check whether firms switch from about-to-fail banks in advance. Importantly, our results show that bad firms do not increase their loan delinquencies, nor do they switch in advance from their current lenders.[4]

We proceed to a difference-in-differences analysis of firm performance conditional on sin bank closure. We examine whether the closure of sin banks results in deterioration of firm performance, which could be due to the destruction of the bank-firm match, or whether it leads to improvement of firm performance, which may happen due to the break-up of the lock-in effect (Liaudinskas and Grigaite 2021). Our estimation results show that the policy had a *cleansing* effect (Gropp et al. 2022) on the performance of good firms that experienced sin bank closures: firms' employment and total size increase, total revenues improve, default rates decrease. We find the opposite for bad firms after sin bank closures. Using credit register data on loan interest rates, we show that a potential explanation involves *credit risk underpricing* by sin banks, especially in the case of bad firms: bad firms enjoy a lower rate at a sin bank than they would at a saint bank. When the sin bank is closed, bad firms lose their "subsidized" loans, which in combination with the lack of opportunities and incentives to improve further deteriorates the state of bad firms.

This chapter contributes to several strands of the literature. First, to the literature that examines the effects of bank clean-up policies (Acharya, Berger, and Roman 2018; Cortes et al. 2020; Chopra, Subramanian, and Tantri 2020; Diamond and Rajan 2011; Philippon and Schnabl 2013). In advanced economies, clean-up policies often take the

[4] In general, the latter result is consistent with the literature highlighting a firm's cost of switching from one bank to another (Ioannidou and Ongena 2010; Bonfim, Nogueira, and Ongena 2020; Liaudinskas and Grigaite 2021).

form of a combination of capital infusions (Calomiris and Khan 2015), stress testing (Acharya, Berger, and Roman 2018), and/or asset quality reviews. Clean-up policies often take place as a response to a crisis.[5] To the best of our knowledge, our study is the first one to analyze the real effects of a clean-up policy that takes the form of many sin bank closures. This is of particular interest to emerging economies, which are more likely to suffer from widespread malpractice in their banking systems.

Second, this chapter contributes to the literature on the real effects of bank distress on firms (Chodorow-Reich 2014; Gropp et al. 2018; Degryse et al. 2019; Greenstone, Mas, and Nguyen 2020). A recent study by Bonfim, Nogueira, and Ongena (2020) shows, for example, that if firms purposely switch banks, unconditional on bank closure, they likely receive a lower interest rate on loan, i.e., a discount to the market price. However, if firms are forced to switch due to their current bank's decision to close the nearest-by branch, the firms receive no discount and pay the same interest as before. A study by Liaudinskas and Grigaite (2021) further documents that firms that had relationships with distressed banks that eventually failed were charged a higher loan rate than the competitive market price prior to the banks' failure (hence possibly locked-in by these banks). After the banks' failure, the firms generally benefit by obtaining a lower loan rate from a new bank. Yet, despite the impact of a branch or bank closure on loan rates, work by Greenstone, Mas, and Nguyen (2020) finds no significant impact of bank switching itself (which has been shown to involve costs) on the firms' employment, during crises or normal times. Our analysis shows that following the closure of a fraudulent sin bank, bad (good) firms are more likely to end up in a match with a sin (saint) bank, and that the performance of a bad (good firm) worsens (improves).

Third, our study contributes to the literature on regulatory forbearance (Acharya and Yorulmazer 2007; Brown and Dinc 2011; Morrison and White 2013; Agarwal et al. 2014; Kang, Lowery, and Wardlaw 2015; Gropp et al. 2022). The literature usually rationalizes the presence of regulatory myopia in closing distressed banks by the Too-Many-To-Fail

[5] A notable exception is the Indian Asset Quality Review program analyzed in Chopra, Subramanian, and Tantri (2020).

concerns (Acharya and Yorulmazer 2007; Brown and Dinc 2011), reputational contagion (Morrison and White 2013), competition between regulators at different levels (Agarwal et al. 2014), political pressure, and/or avoidance of damage to the local economy (Kang, Lowery, and Wardlaw 2015). Our results show that, through a well-designed closure policy (pre-emptive and exogenous with respect to bank and firm expectations), regulators can overcome reputational risk and the risk of declining economic activity when closing distressed banks, thus exhibiting a complete reversal of regulatory forbearance.

Fourth, we contribute to the literature on relationship lending (Degryse and Ongena 2005; Petersen and Rajan 1995; Bolton et al. 2016; Degryse et al. 2019; Schafer 2019). We show that a relationship can be caused by common ownership: following bank closures, firms can often establish new relationships with banks owned/governed by the same persons/entities. We show that this effect weakens as the concentration of local credit markets rises.

The rest of the chapter is structured as follows. Section 3.2 describes the policy experiment undertaken by CBR in mid-2013. Section 3.3 introduces the loan-level, firm- and bank-level data. In Section 3.4, we perform our duration analysis to investigate how bad and good firms switch to new sin or saint banks. In Section 3.5, we explore the channels of firm-bank matching. In Section 3.6, we present the difference-in-differences estimation of the real effects of sin bank closure on firm performance. Section 3.7 concludes.

3.2 Regulatory forbearance and bank clean-up policy in Russia

Following the collapse of the USSR in 1991, the centrally-planned Russian economy began to transition to a market economy. Russia witnessed rapid growth in the number of privately-owned banks.[6]

[6] During the Soviet time the banking system comprised the "Big-4" state banks. These are still operational, and even 30 years after the collapse of the USSR, they dominate the banking landscape of Russia, with a share of more than 50%.

During the "dashing" 1990s, the number of banks expanded to nearly 2,500. These were mainly very small credit institutions, short-lived, created to finance non-financial businesses of their owners ('pocket' banks) at lower interest rates than the market would otherwise offer, which was especially important during hyperinflation (Svejnar 2002). Many of these banks were involved either in outright criminal activities or employed questionable practices (Degryse, Karas, and Schoors 2019).

With the start of the new millennium, the number of operating banks shrank by half; nevertheless, many were still pursuing illegal or questionable practices. In 2006, the CBR attempted a clean-up of the banking system, which resulted in the closure of two large banks involved in illegal activities. However, the clean-up policy came to an effective halt with the assassination of the Deputy Head of the CBR, Andrey Kozlov, the key figure behind the clean-up policy. The so-called "Kozlov affair" shocked the banking community in Russia and led to an extreme form of regulatory forbearance: bank closures became rare and took place primarily when the owners of failed banks simply had no interest to continuing, irrespective of whether their business was fully legal or not.[7]

Until the global financial crisis of 2007–2009, the Russian banking system had been expanding at a two-digit growth a year per year, mainly due to expanding corporate and retail lending, thus satisfying a large demand for loans.[8] The financial crisis exposed serious inefficiencies in the Russian banking system and necessitated large-scale government interventions to provide support to the largest banks. The number of operating banks continued to decline after the crisis, to around 1,100 banks by the beginning of 2013. Overall, the regulatory stigma over auditing and closing fraudulent banks following the assassination of Andrey Kozlov remained, and the period between 2006-2013 was characterized by a large degree of regulatory forbearance.

This forbearance effectively ended in 2013 with the appointment of a new head of the Central Bank.[9] Though the intention to conduct an active clean-up of the banking

[7]See the history of the process at The Guardian's article: https://www.theguardian.com/business/2006/sep/14/russia.internationalnews.

[8]For example, commercial loans grew up by nearly 70% in 2007, on the eve of the crisis in Russia.

[9]The change of the head of the Bank was announced rather unexpectedly: Elvira Nabiullina, the head

system was not explicitly mentioned in the inauguration speech of the new head of the Bank, in a sequence of consequent interviews, the new head stressed her intention to tighten regulatory oversight over illegal and questionable banking practices.[10]

However, it soon became clear that the CBR had rather rapidly swung from its regulatory forbearance regime towards a strict intolerance of fraud. Overall, during the period of 2013–2020, the number of operating banks in Russia had declined from around 1,000 to about 350, due to the tightened policy (Figure 3.1). The average annual frequency of fraud-induced license revocation rose from 29 (on average during 2008–2013) to nearly 70 (on average during 2013–2020). The dramatic fall in the number of operating banks is nearly linear, irrespective of the changing phases of the business cycle during that time.[11] In February 2018, the CBR officially announced that the active phase of the cleansing policy was over, given the large number of fraudulent banks discovered and closed.

The geography of the cleansing policy is summarized in Figure 3.2. The policy was not limited to Moscow and Saint-Petersburg—where more than 75% of the banking system in terms of total asset size is concentrated—but in fact affected every region up to the Far East, with the largest number of license revocations taking place in the West and in the South, near the Black Sea. In almost every case, forced license revocations were associated with hidden negative capital revealed during on-site inspections of the banks, ranging between 10% and 50% of affected banks' total liabilities, throughout all of Russia.[12] As can be inferred from Figure 3.3, the bank-level data shows that during the active phase of the policy in 2013–2018, operating banks: (a) created additional loan loss reserves, (b)

of the Ministry of Economic Development, was to replace the head of the Bank, Sergey Ignatiev, who had held the post for the previous 13 years.

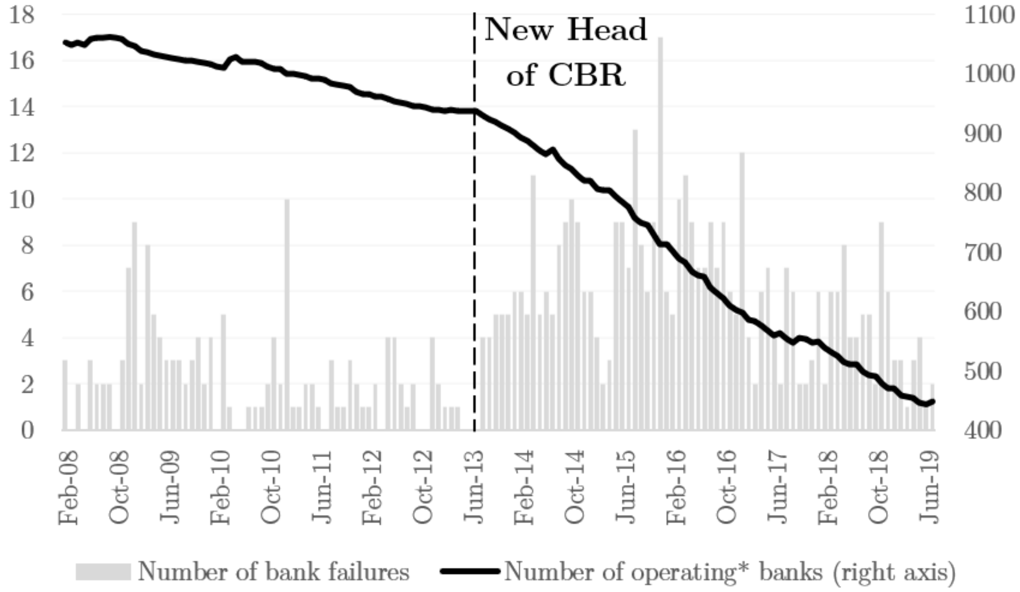
[10]In her inauguration speech, the new head of the CBR mainly stressed that the primary aims of the Bank would include switching from a fixed to a flexible exchange rate regulation and establishing an inflation targeting regime, in which the key instrument of monetary policy would be the regulated interest rate. The main purpose of the new policy, as the new head announced, was in curbing double-digit inflation in the country to the target of 4%. There was no apparent discontinuity over the policy following the appointment of the new head: for example, the previous head of the bank took up the post of the new head's adviser.

[11]The Russian economy experienced a local recession during 2014-2015 and subsequent recovery in 2016–2019.

[12]By negative capital, we mean negative owners' equity—that is, when the total value of a bank's assets is less than the sum total of its liabilities.

Figure 3.1: Bank Closures and the New Head of CBR

Note: This figure depicts the time series of monthly bank closures (the left y-axis) and the monthly number of operating banks (the right y-axis) during February 2008 and June 2019. The new head of the Central Bank of Russia (CBR) was appointed in Jun 2013.



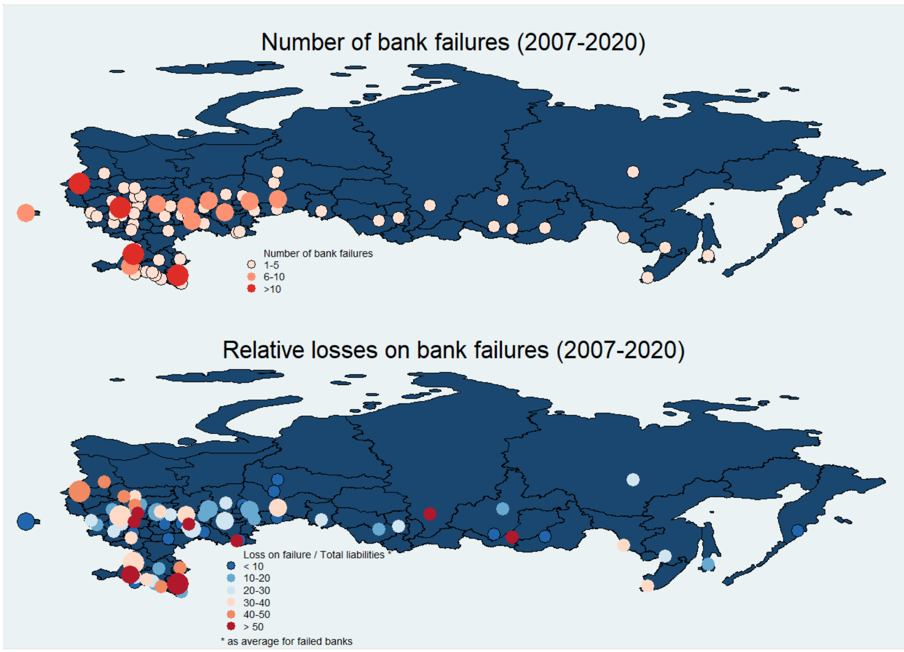
disclosed more NPLs in their loan portfolios, (c) reduced the stock of (possibly opaque) loans to firms, and (d) slowed down new loan issuance, compared to before the policy, and irrespective of the business cycle phase. Overall, despite closing 2/3rds of all operating banks, the policy did *not* lead to a shrinking of the financial system. According to the World Bank statistics, the ratio of domestic private credit to GDP increased from 81% in 2012 to 99% in 2020, i.e., the banking sector was rising rapidly during the years of the CBR's tight policy.[13]

3.3 Data

Our bank-firm level data come primarily from three sources. First, the annual frequency firm-level data covering the period from 2007 to 2020 come from financial statements provided in SPARK database.[14] Second, the monthly (balance sheets items) and quarterly (P&L account) frequency bank-level data come from the CBR's reporting forms 101 and

[13]See https://data.worldbank.org/indicator/FD.AST.PRVT.GD.ZS.
[14]https://spark-interfax.ru/.

Figure 3.2: Geography of bank fraud and bank closures



102, respectively, available from 2004 to 2021.[15] Third, to identify the bank-firm lending relationships, we employ monthly data from two Russian credit registries. For the period from July 2013 to December 2017, we use the Credit History Bureaus (CHB), which only provides data on the number of days during which the loans are overdue, the *days of NPLs*, while for the period from January 2018 to October 2020, we employ data from the credit registry of the CBR (reporting form No. 0409303).

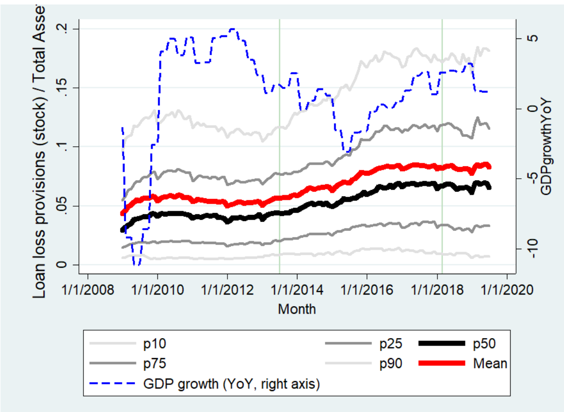
3.3.1 Credit history bureau and credit registry data

The Credit History Bureaus database (the CHB hereafter) is compiled from three credit history bureaus: the United Credit Bureau, the National Bureau of Credit Histories, and the Equifax Credit History Bureau. These three credit history bureaus are the largest of 14 bureaus registered with the State Register of Credit History Bureaus maintained by the CBR.[16] For each bank and each corporate borrower, the CHB contains information on the maximum number of days loan payment is overdue at the reporting date (the

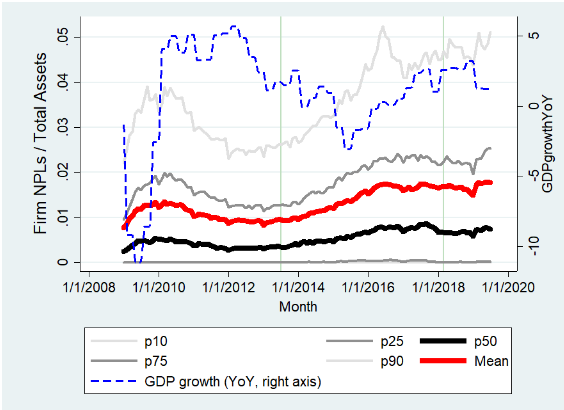
[15] https://www.cbr.ru/banking_sector/otchetnost-kreditnykh-organizaciy/.
[16] See https://www.cbr.ru/ckki/restr/.

Figure 3.3: Time evolution of selected bank variables before and during the active phase of the tight regulation policy (Jul.2013–Feb.2018)

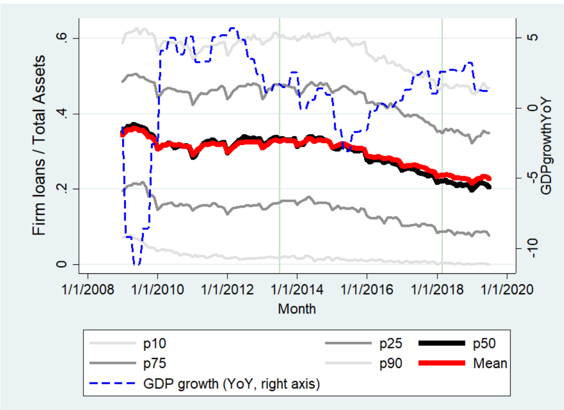
Note: The figure depicts the time evolution of selected bank characteristics at the bank-month level before, during, and after the active phase of the tight regulatory policy against the background of the annual GDP growth rates in Russia. The active phase of the policy is marked with two vertical green lines.



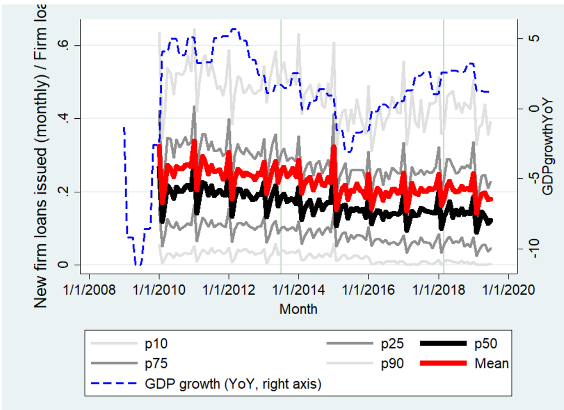
(a) Loan loss reserves



(b) NPLs of firms



(c) Loans to firms



(d) New loans issued

days of NPLs). That is, if a firm has multiple loans at a bank, the CHB provides the maximum number of days of payment overdue across these multiple loans (it is possible that only one of several loans is delinquent).

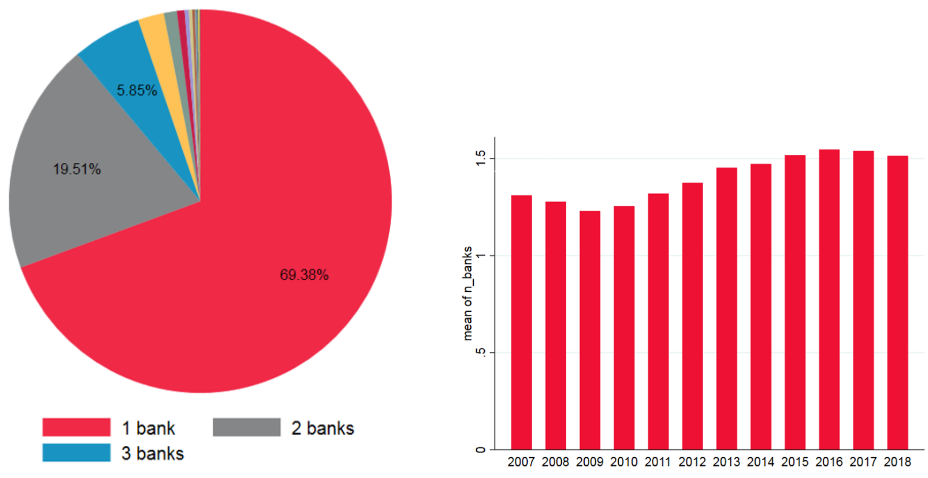
The days of NPLs indicator is a categorical variable denoting the time intervals of overdue dates. For example, days overdue is equal to 0 if there are no delayed payments, 30 for all delays in payments from 1 to 30 days, 60 for delays from 31 to 60 days, and so on. Loans with days overdue equal to 150 or 200 routinely include loans that were labeled as "hopeless," paid by collateral, contested in courts, or written off.

The CHB covers the period from 2007 to 2017. We use the CHB from July 2013 to 2017 to identify bank-firm relationships during the active phase of the cleansing policy. To identify the firm-bank relationships starting from 2017, we employ the credit registry database (Form 0409303). This database contains detailed information about credit: currency and amount of loans, lending rates, maturity, collateral attached, borrower-lender affiliation, and the amounts of debt repayment including interest payments and the amortization of the principal amount of debt. Here, we use the days of NPLs indicator.

Our database (CHB merged with credit registry) of matched bank-firm relationships initially consists of 655,300 firms and 906 banks at the start of the sample in July 2013 and covers almost 90% of Russian banks by net assets. More than 70% of firms in the CHB data are micro-firms (with fewer than 15 employees), another 20-25% are SMEs, while the rest are medium and large firms.

The majority of Russian firms obtain loans from just one bank. In 2017, the share of such single-bank firms equaled 69.4%, and another 19.5% of firms obtained loans from at least two banks (Figure 3.4). These patterns are similar to, e.g., Belgium where 84% of firms are single-bank firms (Degryse et al. 2019) but are different from, e.g., Spain, where 86% of loans are granted to multiple-bank firms (Jiménez et al. 2014).

Figure 3.4: Bank–firm relationships



(a) Distribution of bank-firm relationships (as of 2017)

(b) Time evolution of the mean number of bank-firm relationships, by year

3.3.2 Bank-level data

We merge the bank-level data from the banks' balance sheets and P&L accounts with the firm-bank relationships database (the CHB and credit registry). The bank-Level data is at the monthly frequency for balance sheet items and at the quarterly frequency for the P&L account. The data come from the CBR forms 101 and 102 and cover the period from 2004 to 2021.

As discussed earlier, around 650 banks were shut down by the regulator during the active phase of the cleansing policy (July 2013–February 2018), of which 85% were due to fraud revealed during the audit. We refer to those banks that had their licenses revoked due to fraud as sin banks, while those that were permitted to continue their activities we dub saint banks.

3.3.3 Firm-level data

The firm-level data includes information from firms' financial statements extracted from the SPARK database, which is provided by the Interfax Group. Matching the SPARK database with the firm-bank relationships database (CHB and credit registry) covers about 60% of the total number of firms operating in the Russian economy. For a detailed list of variables we use from firms' financial statements, refer to Table 3.1 in Appendix 3.A.

We refer to a firm as a "bad" firm if it suffered losses during at least the prior two years (three and four years for robustness). In other cases, we refer to a firm as a "good" firm. In addition, we proxy for the quality of firm by the days of NPLs variable from the CHB database.

3.3.4 Bank-firm relationships: descriptive statistics

We focus on the subset of firms that were borrowing from sin banks and thus experienced bank shutdowns during the cleansing period. Moreover, we also focus only on the first switching episodes, when the bank closure was really a shock to a borrowing firm, and remove all observations where firms did second, third, and so on (multiple) switching. We do this intentionally, because we want to see the real effects of switching when an affected firm has no experience in an unexpected credit supply squeeze caused by regulatory intervention. In our sample, we thus have only 13,373 firms that had relationships with at least one of the sin banks detected and closed by the CBR. Firm-level data are not available for 6,062 of these firms. After we trim our data for outliers (1 and 99 percentiles), we lose 80 more firms. Adjusting for a one-month lag of all regressors in our analysis, our effective sample consists of 262.6 thousand observations with 6,267 firms and 645 banks. If we focus on the case in which a firm has relationships with more than one bank, our sample includes 287.1 thousand observations with 6,061 firms.

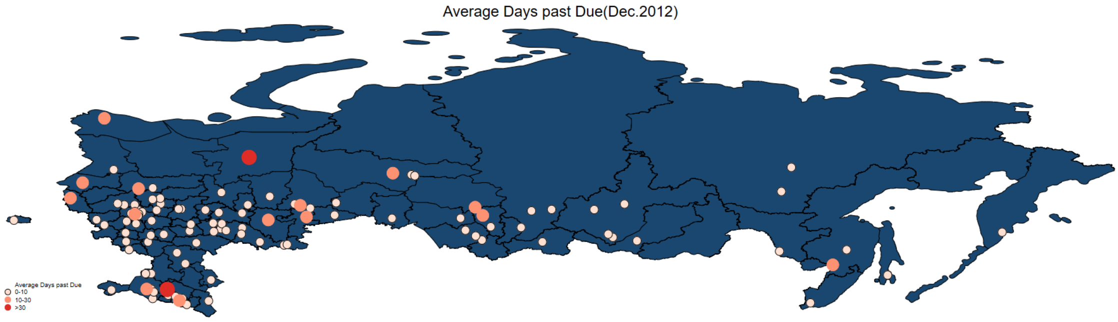
Regarding the geography of firm-bank relationships, our final dataset is representative, covering the whole territory of Russia, with the densest frequency of relationships observed in the west, central, and south regions of the country (Figure 3.5).

We present the descriptive statistics at the firm-bank-month and firm-year levels in Table 3.1. We consider three groups of switching firms: firms that switch to a saint bank, firms that switch to a sin bank, and firms that never switch (i.e., are not recorded in the credit register anymore) following their prior sin bank closures. Of 6,267 firms in our sample, the overwhelming majority (85%) are the never-switchers.[17] Those firms that manage to switch to a new bank (15%) mostly establish a connection with a saint bank (11% or 715 firms). The rest (3.2%) borrow from a new sin bank, which is not-yet-detected by the CBR. Firms that switch to a saint bank are generally in better financial shape, with an average ROA of 5%, smaller leverage, and higher liquidity ratios.

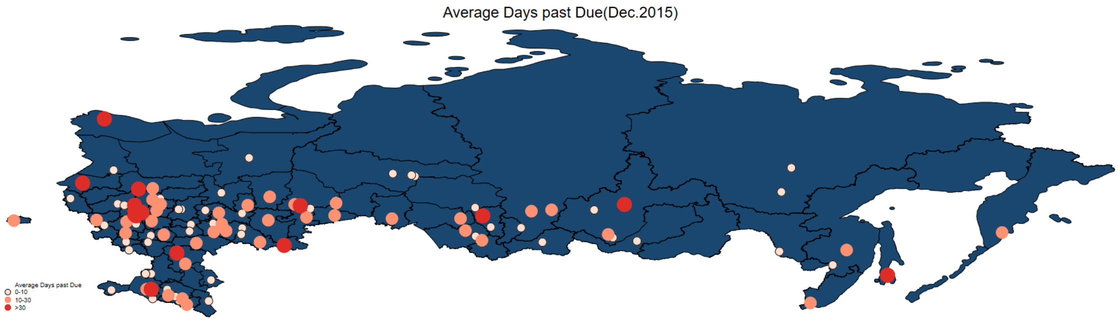
Though we observe that firms matching with new saint banks reported fewer days of NPLs in the prior sin banks than firms matching with new sin banks, the difference between the two is not large. This may indicate that both good and bad firms match with

[17] These firms generally rely on either their own funds, borrowings from other non-financial firms inside Russia, or from foreign banks abroad. We do not have detailed data on the breakdown of the firms' borrowing outside the Russian credit registry.

Figure 3.5: Geographical variation in the number of firm-bank relationships



(a) Before the regulatory shock (as of December 2012)



(b) After the regulatory shock (as of December 2015)

either of the two types of banks. However, we do observe that matching with new saint banks takes much longer (46 months) than matching with new sin banks (18 months). Apparently, it is less difficult to persuade a not-yet-detected sin bank to accept a firm than to persuade a saint bank. Another characteristic that delivers substantial differences across the three types of matching firms is their overall size (in terms of total assets). In contrast to an expectation that larger firms may find it easier to borrow from new saint banks, we observe a different picture in our data. The average size of a firm that switches to a sin bank is almost three times larger than the average size of a firm that switches to a saint bank (85 vs. 29 mln rubles), and almost two times larger than the average size of those that never switch (85 vs. 44 mln rubles). Thus, we can describe a firm that switches to a sin bank as a large financially constrained firm (higher leverage and lower liquidity than for an average firm that switches to a saint bank).

Table 3.2 describes the regional structure of our data. In more than half of the obser-

Table 3.1: Descriptive statistics for the firms matching with either *sin* or *saint* banks following closures of their prior banks

	Mean	Median	SD	Min	Max
Panel 1: Firms matching with new *saint* banks:					
Match with saint vs never match	0.25	0.00	0.43	0.00	1.00
Months in search	45.77	46.00	25.39	2.00	139.00
Days of NPLs in the closed sin bank	14.87	0.00	42.07	0.00	200.00
Whether had negative profit when the sin bank closed	0.05	0.00	0.23	0.00	1.00
Whether had a negative profit when matched with new bank	0.10	0.00	0.30	0.00	1.00
log of total assets	17.19	17.23	2.03	10.04	23.38
Leverage	0.75	0.73	0.80	0.00	9.78
Liquid assets	0.17	0.19	0.70	–8.57	1.00
Return on assets	0.05	0.03	0.23	–2.37	0.91
Panel 2: Firms matching with new *sin* banks:					
Match with sin vs never match	0.06	0.00	0.23	0.00	1.00
Months in search	17.86	13.00	14.34	1.00	73.00
Days of NPLs in the closed sin bank	15.73	0.00	41.75	0.00	200.00
Whether had negative profit when the sin bank closed	0.02	0.00	0.13	0.00	1.00
Whether had a negative profit when matched with new bank	0.15	0.00	0.35	0.00	1.00
log of total assets	18.26	18.45	2.07	9.39	23.44
Leverage	0.95	0.89	1.25	0.00	18.46
Liquid assets	0.06	0.12	0.90	–9.52	1.00
Return on assets	–0.02	0.00	0.29	–2.73	0.90
Panel 3: Firms that *never* match with new banks:					
Days of NPLs in the closed sin bank	14.19	0.00	39.52	0.00	200.00
Whether had negative profit when the sin bank closed	0.05	0.00	0.21	0.00	1.00
Whether had a negative profit when matched with new bank	0.12	0.00	0.33	0.00	1.00
log of total assets	17.60	17.71	2.52	9.31	23.63
Leverage	0.99	0.86	1.34	0.00	18.71
Liquid assets	0.03	0.14	1.01	–11.93	1.00
Return on assets	0.00	0.01	0.27	–3.14	0.91

vations, the firms that experienced bank closures were registered in the Central Federal District (FD), and observations with firms from Volga, Northwestern, and Siberian FDs account for 10% each. Ural, Southern, and Far Eastern FDs add another 15% together, and the rest of the observations (less than 1%) are for firms from the North Caucasian FD. The majority of observations (from 78 to 94%) contain no information about delays in credit payments. The only notable exception is the North Caucasian FD, where the share of "no delays" is less than 70%.

The regional dimension of our data allows us to look into the spatial concentration

Table 3.2: Regional structure of observations, by Federal Districts (FD)

	Sib.	Far East.	Volga	N-West.	N.Caucas.	Ural	Central	South	Total
Share of firms, %	9,47	2,27	10,45	10,13	0,66	6,49	54,70	5,84	100
The days of NPLs accumulated by firms in their sin banks in each FD:									
0	85,14	91,58	78,24	92,41	69,69	82,51	84,71	82,78	84,66
30	6,54	1,53	7,67	1,93	7,25	2,68	4,9	4,19	5,12
60	1,55	1,54	3,61	0,75	2,35	1,54	1,96	3,6	2,12
90	1,14	0,02	1,91	0,18	0,03	0,49	0,91	1,99	1,02
120	0,44	0,62	0,82	0,10	0,03	0,37	0,36	0,85	0,44
150	3,98	4,27	6,61	4,09	5,9	11,47	6,24	5,94	5,56
≥180	1,21	0,44	1,14	0,54	14,75	0,94	0,92	0,65	1,06
Mean HHI	1 265,3	1 822,9	1 457,5	1 651,6	1 885,5	1 763,9	1 205,8	1 769,2	1 371,5
SD HHI	459,8	796,0	1 051,6	596,7	485,7	995,6	737,3	821,7	802,5

of the Russian regional credit markets by calculating the Herfindahl-Hirschman index (HHI). We construct the index as the sum of squared shares of newly issued loans for firms in the region r by bank b in the total volume of new loans in the region r for each month t. We report means and standard deviations of HHI across federal districts in the lower panel of Table 3.2. We visualize regional concentration and days of NPL for each federal district in a scatter plot (Figure 3.6).

3.4 Firm-bank matching following sin bank closures

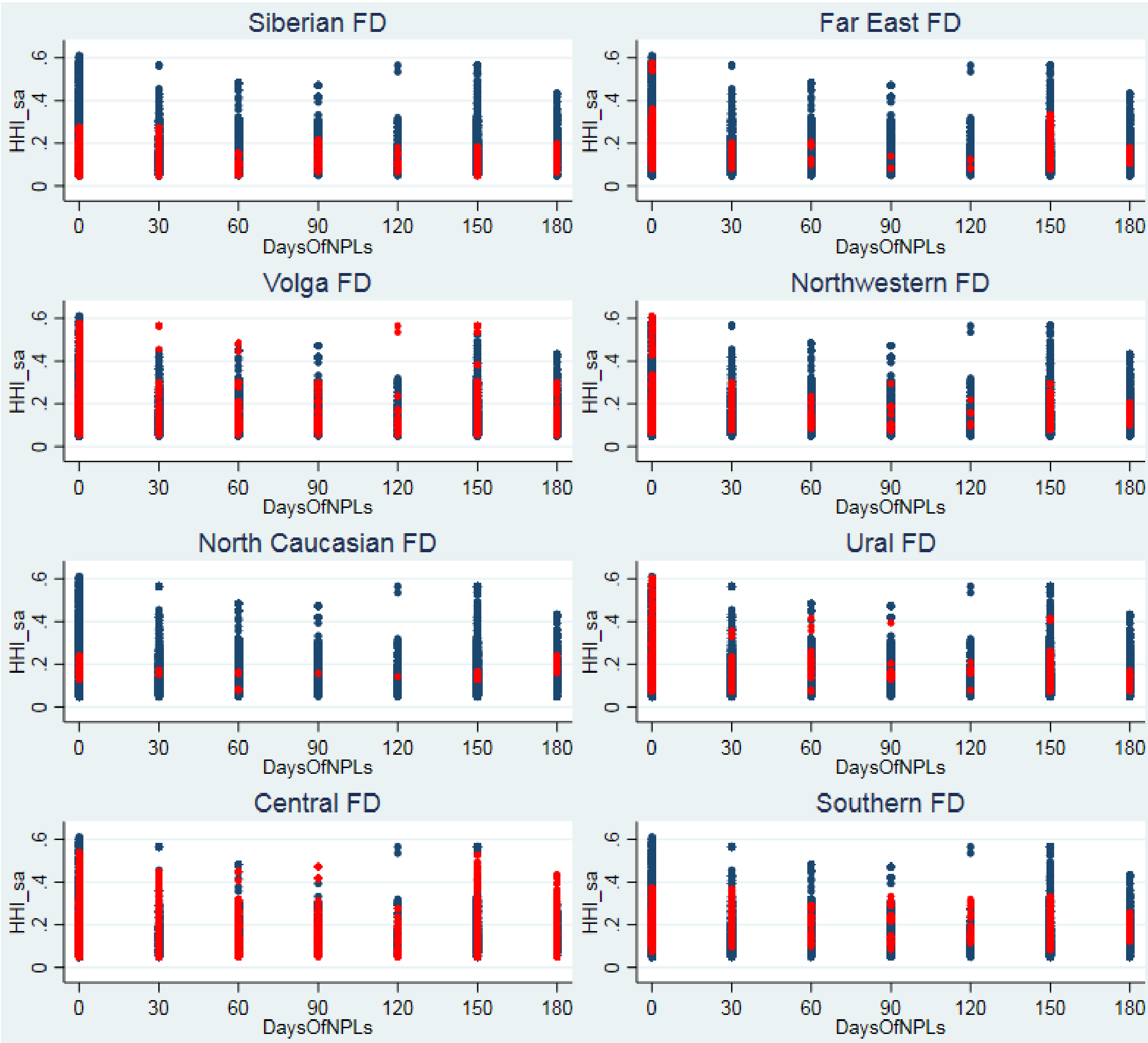
3.4.1 Baseline results

We begin our analysis by examining the determinants of a firm's matching with a new bank following the closure of the firm's prior sin bank, and conditional upon the firm's survival to the moment in time when the new match is established.[18] The duration regression approach ("survival" model) is a natural methodological framework for this analysis, because it takes into account the duration of the spell, i.e., the time it takes the

[18] As discussed in Section 3.3, we define a sin bank as a bank that is closed due to fraud at some later point in time in our sample.

Figure 3.6: Quality of loans and regional concentration of credit markets

Note: The figure depicts the days of NPLs accumulated by firms in the closed banks across the credit markets aggregated at the eight federal districts of Russia and characterized by different levels of concentration, as measured by the Herfindahl-Hirschman Index (HHI). HHI is computed using monthly bank branch-level data as the sum of squared shares of new issued loans for firms in region r by bank b in total volume of new loans in region r. Observations in each particular federal district are marked red.



firm to match with a new bank.[19] We focus on *single* firm-sin bank relationships, i.e., when a firm obtained loans from only one bank, which, at some point in time, is closed for fraud.[20] Appealing to the title of this chapter, we are interested in *where* the firm goes to after the closure of its prior sin bank: to another (not-yet-detected) sin bank,

[19]"Survival" regressions have been previously adopted to study bank failures in, e.g., (Brown and Dinc 2011).

[20]Recall from Section 3.3 that single firm-bank relationships cover 70% of the sample.

or to a saint bank. The rationale for focusing on single firm-bank relationships at the moment of sin bank closure is that the CBR's tight regulation policy is likely to affect single firm-bank pairs much more than firms with multiple-bank relationships, in which the firms may have more opportunities to substitute the flow of borrowed funds across existing lenders.

Among the determinants of new firm-bank matching, we focus on the *quality* of firms. One may expect that when a sin bank is closed, good firms have more chances to find new bank matches than bad firms. We start by employing a *single-failure duration analysis*, in which the duration of the spell for a firm f begins with the closure of its prior sin bank b at time t_f^* (t^*, for simplicity) and ends with the firm being matched with a new bank at time $t^* + k$, where k is the firm-specific duration of the spell (in the data mean $k = 35$ months). Following the standard terminology of duration analysis, we refer to the time $t^* + k$ event as a "failure." If $t^* + k$ is never observed in the sample—that is, if firm f never matches with a new bank—we treat the corresponding failure as right-censored, leaving all such firms in the sample. The instantaneous rate at which firms "exit," i.e., match with new banks conditional on survival to the current moment in time, is described by the following hazard function $\lambda(\cdot)$:

$$\lambda\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(\alpha + \alpha_{bc} + \alpha_r + \alpha_i + \text{Firm.Quality}_{f,t-1}\mathrm{B} + \mathbf{C}_{f,t-1}\Gamma\Big), \quad (3.1)$$

where $\text{Firm.Quality}_{f,t-1}$ is firm f quality proxy at time $t-1$, which is measured by either (i) the log of days of NPLs accumulated in the prior sin bank before its closure—that is, by $t^*_{f,b}$—or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. $\mathbf{C}_{f,t}$ is a set of control variables including the firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios (see definitions in Table 3.1; all controls are taken with a one year lag to eliminate simultaneity). $\alpha_{bc}, \alpha_r, \alpha_i$ are bank-closure event fixed effects, fixed effects of the region in which the firm operates, and industry fixed effects. Θ is the set of parameters to be estimated $(\alpha, \alpha_b c, \alpha_r, \alpha_i, B, \Gamma)$. $\lambda_0(t)$ is the baseline hazard function. We use the exponential distribution function to specify the baseline hazard: $\lambda_0(t) = \lambda > 0$.[21]

Table 3.3 reports the estimation results of equation (3.1). In columns (1)–(2) the firm quality measure is proxied by the log of days of NPLs the firm had accumulated in the closed sin bank prior to its closure at t^*, log $DNPL_{f,t^*}$. Here, the sample consists of 6,249 firms, 413 bank closures, and 915 "failures," i.e., new firm-bank matches. We obtain negative but largely insignificant estimates on the log $DNPL_{f,t^*}$ variable, moreover, the estimated coefficient is close to zero. Next, in columns (3)–(4) we replace this granular measure with the binary variable of whether a firm has negative profits, $Profit_{f,t^*} < 0$, at the bank closure date t^*. Due to limitations with firm-level data on profits, the sample slightly reduces. Similar to the previous case, we observe negative and largely insignificant estimates on the $Profit_{f,t^*} < 0$ variable.

Finally, in columns (5)–(6) we add an indicator variable of whether a firm had negative profits at the time it matched with a new bank, $Profit_{f,t^*+k} < 0$, to the specification considered in the two previous columns, because although a firm might suffer losses at t^* when its sin bank was closed, the firm might also have improved by the time it matched with a new bank at $t^* + k$. Indeed, while the estimates on the $Profit_{f,t^*} < 0$ variable are still insignificant, we find negative and highly significant estimates on the $Profit_{f,t^*+k} < 0$ variable. Economically, the underlying effect is sizeable: as compared to a profitable (good) firm, a firm that still has losses after the closure of its prior sin bank (bad firm) is 33.2% less likely to match with a new bank.[22]

The regression results suggest an absence of an empirical relationship between the time t^* measures of firm quality and the chances to match with a new bank in the future at some random time $t^* + k$. In other words, more severe loan payment delinquencies and low profitability when the firm's sin bank is closed do not predict whether the firm finds a new bank match in the future.

[21] Under the exponential distribution, the hazard does not change as time passes (the memoryless property of the exponential distribution function). We test the constant duration dependence using the Weibull distribution.

[22] The effect is computed as $exp(-0.403 * 1) - exp(-0.403 * 0) = -0.332$.

Table 3.3: Survival regression results: firm-bank match based on firm quality

Note: The table reports estimates of new firm-bank matching following the firms' f prior sin banks closure, as implied by equation (3.1). Dependent variable $\lambda\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms "exit," i.e., match with new sin or saint banks, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b}+k$. Other controls include firm size, as measured by the log of total assets and its square, leverage-to-total assets, and liquidity-to-total assets ratios. We perform the estimations for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7 till 2020M10. Coefficients are reported instead of subhazard ratios. The constant term is included but not reported to save space.

Firm.Quality$_{f,t}$:	*Days of NPLs at t^**		*Negative profit at t^**		*Negative profit at t^* and t^*+k*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Firm quality:						
log DNPL$_{f,t^*}$	–0.009	–0.024				
	(0.024)	(0.031)				
Profit$_{f,t^*} < 0$			–0.117	–0.240	0.046	–0.066
			(0.212)	(0.253)	(0.213)	(0.252)
Profit$_{f,t^*+k} < 0$					–0.391***	–0.403***
					(0.129)	(0.136)
Panel 2: Other controls:						
Firm size$_{f,t-1}$	1.600***	1.590***	2.053***	2.062***	2.071***	2.096***
	(0.261)	(0.290)	(0.304)	(0.335)	(0.306)	(0.338)
Firm size$^2_{f,t-1}$	–0.043***	–0.042***	–0.055***	–0.056***	–0.056***	–0.057***
	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)
Leverage$_{f,t-1}$	–0.271**	–0.342**	–0.479***	–0.597***	–0.482***	–0.607***
	(0.118)	(0.140)	(0.141)	(0.174)	(0.144)	(0.179)
Liquidity$_{f,t-1}$	–0.061	–0.088	–0.132	–0.142	–0.166	–0.188
	(0.105)	(0.123)	(0.121)	(0.143)	(0.124)	(0.147)
Bank closure event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs		Yes		Yes		Yes
N obs	262,648	262,648	182,197	182,197	182,120	182,120
N firm-bank new matches	915	915	705	705	705	705
N firms	6,249	6,249	4,280	4,280	4,277	4,277
log L	–4,015.3	–3,680.6	–3,096.5	–2,791.0	–3,091.4	–2,785.8

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

We further hypothesize that it may be important to distinguish cases in which the firm matches with a new sin bank—that has not yet been shut down—from those with a saint bank. We hypothesize that bad firms are more likely to be sorted to sin banks and good firms are more likely to match with saint banks. Because the CBR's cleansing policy stretched over five years, it gave firms that were separated from sin banks an opportunity to be matched again with another (not yet shut down) sin bank.

To test these hypotheses we slightly modify the duration regression we applied above. Specifically, we consider two hazard functions instead of one: $\lambda_1(\cdot)$ for the firm's decision to match with a new sin bank vis-a-vis never match and $\lambda_2(\cdot)$ for when the firm seeks to match with a new saint bank vis-a-vis never match:

$$\lambda_\jmath\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(\alpha_\jmath + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_\jmath + \mathbf{C}_{f,t-1}\Gamma_\jmath\Big), \tag{3.2}$$

where $j = 1$ stands for regression with sin bank matching and $j = 2$ for saint bank matching. Other notations, including sample size and time span, remain the same.

Table 3.4 reports the estimation results on the duration regressions with the sample split in equation (3.2). Columns (1)–(3) present the estimates from regressions of the matching with sin banks and columns (4)–(6) with saint banks, for different measures of firm quality. For the duration analysis of matching with sin banks, the sample consists of 6,069 firms and nearly 200 new sin matches, and the average duration of the spell changes from 35 months, which was true across all matches, to 18 months. For the matches with saint banks, the sample comprises 6,080 firms and 715 matches with new saint banks, and the average duration of the spell rises to 46 months. Note that the 200 new sin matches and 715 new saint matches constitute the 915 matches we considered above before splitting the sample.

Strikingly, our split estimates suggest that the insignificant effect of log $DNPL_{f,t^*}$ obtained above now turns *positive* and is highly significant in the regressions of matching with sin banks (column 1). Conversely, in the regressions of matching with saint banks, the respective estimate is negative and also highly significant (column 4). Jointly, these estimates support our hypothesis on endogenous sorting of firms: conditional on sin bank

Table 3.4: Survival regression results: splitting the firm-bank matches

Note: The table reports estimates of new firm-bank matching following the firms' f prior sin banks closure, as implied by equation (3.2). Dependent variable $\lambda_{\jmath}\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin ($\jmath = 1$) or saint ($\jmath = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure—that is, by $t^*_{f,b}$—or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include a firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. The sample includes those firms that have a *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7 to 2020M10. Coefficients instead of subhazard ratios are reported. The constant term is included but not reported to save space.

	Match with a sin bank			Match with a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Firm quality:						
log DNPL_{f,t^*}	0.155***			–0.091**		
	(0.058)			(0.037)		
$\text{Profit}_{f,t^*} < 0$		–1.742*	–1.475*		0.041	0.204
		(0.908)	(0.895)		(0.247)	(0.248)
$\text{Profit}_{f,t^*+k} < 0$			–0.534*			–0.384**
			(0.297)			(0.151)
Panel 2: Other controls:						
Firm $\text{size}_{f,t-1}$	2.627***	2.229***	2.263***	1.422***	2.036***	2.069***
	(0.760)	(0.783)	(0.786)	(0.313)	(0.371)	(0.374)
Firm $\text{size}^2_{f,t-1}$	–0.069***	–0.061***	–0.061***	–0.038***	–0.055***	–0.056***
	(0.020)	(0.021)	(0.021)	(0.009)	(0.010)	(0.010)
$\text{Leverage}_{f,t-1}$	–0.275	–0.289	–0.292	–0.353**	–0.730***	–0.745***
	(0.222)	(0.265)	(0.262)	(0.165)	(0.188)	(0.192)
$\text{Liquidity}_{f,t-1}$	–0.151	–0.248	–0.303	–0.057	–0.094	–0.135
	(0.208)	(0.243)	(0.251)	(0.144)	(0.164)	(0.168)
Bank closure event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N obs	257,190	178,447	178,372	257,681	178,833	178,758
N firm-bank new matches	200	168	168	715	537	537
N firms	6,069	4,198	4,195	6,080	4,203	4,200
log L	–1,066.0	–853.7	–851.8	–2,921.0	–2,169.1	–2,165.4

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

closure, bad firms tend to match with another (not-yet-detected) sin banks, while good firms are more likely to establish relationships with saint banks. Economically, both estimates imply large effects: compared to a firm with 30 days of NPLs, a firm with 90 days of NPLs is 31% more likely to match with another sin bank and 7% less likely to join a saint bank in the future.[23]

Next, we replace the log $DNPL_{f,t^*}$ variable by $Profit_{f,t^*} < 0$ to check whether having negative profits also predicts the sorting of bad firms to sin banks and good firms to saint banks, as we find above. However, as can be inferred from columns (2) and (5) of Table 3.4, this is not the case. Indeed, in the regression of matching with sin banks, we obtain a negative, not positive, coefficient on the $Profit_{f,t^*} < 0$ variable, meaning that firms that had negative profits at the moment their sin bank closed are not more likely to establish a match with another sin banks in the future. Economically, the underlying effect is very large: a firm with negative profits at t^* has a 77.1% smaller chance to match with another sin bank. However, we treat this result with caution: the estimated coefficient itself is only marginally significant, and thus uncertainty is large, as opposed to the highly significant coefficient on the loan payment delinquencies variable obtained above.

In the regression of matching with saint banks, we obtain a near zero and insignificant coefficient on the $Profit_{f,t^*} < 0$ variable, reflecting that firms that were facing losses during the closure of their sin banks are *not* less likely to match with saint banks in the future. This estimate is also in stark contrast to what we obtained for the loan delinquencies variable above.

Finally, we consider whether a firm had negative profits not only at t^* when the firm's sin bank fails but at $t^* + k$ when the firm matches with another sin bank, column (3), or with a saint bank, column (6). As can be observed from the two columns, we obtain negative and significant estimates in both cases. The underlying effects imply that a firm with negative profit at the moment of establishing a new match is 41.4% less likely to join a new sin bank and 31.9% less likely to join a saint bank, as compared to a profitable firm.

[23]The effects are computed as (i) $exp(0.155 \cdot \ln(90)) - exp(0.155 \cdot \ln(30)) = 0.31$ and (ii) $exp(-0.091 \cdot \ln(90)) - exp(-0.091 \cdot \ln(30)) = -0.07$.

3.4.2 Robustness checks

One concern regarding splitting duration regressions is that we separately study matching with sin and with saint banks. To address this concern, we run a *multinomial regression model* in which we have all three options for a firm: never switch (0), match with a sin bank (1), and match with a saint bank (2). As Table 3.1 in Appendix 3.B shows, the estimation results are qualitatively and even quantitatively very close to the baseline.[24]

Another concern is that we omit *macroeconomic and regional characteristics*, which both might affect the CBR's intention to close problem banks.[25] We thus include GDP growth rates (moving averages across four quarters) to capture the turning points of the business cycle and concentration of regional credit markets, as measured by the Herfindahl-Hirschman Index (HHI) using the bank branch-level data, to control for the observed differences in banks' market power across Russia. As we show in Table 3.1 in Appendix 3.C, neither of the two forces has an effect on our baseline results. This supports the view that the CBR conducted its tight policy exogenously, i.e., not because of the recession/sanctions and not because of the dramatically large concentration of regional credit markets that could have led to higher risk-taking by small banks.

Further, one could doubt that our baseline effects are valid only for firms that have single-bank relationships. We re-run our splitting duration regressions on the sample of firms that have *multiple bank relationships*, with at least one of them being a sin bank. Table 3.1 in Appendix 3.D clearly indicates that there are no significant effects of the firm quality on the likelihood, and *direction*, of new bank matching. The estimates on the log $DNPL_{f,t^*}$ and $Profit_{f,t^*} < 0$ are insignificant in both regressions of matching with sin and saint banks. The only effect that remains is the one describing the negative relationship between a firm's losses after the firm's prior bank is closed, i.e., at $t^* + k$, and the chance to match with a new saint bank on the same date. Jointly, these results

[24] The estimates are performed with the multinomial logit model instead of a competing risks duration model. This is because of the issues with the convergence of the likelihood function.

[25] In 2014–2015, the Russian economy experienced a double shock: internal factors led the economy into yet another recession, and external forces, e.g., deterioration of the commodities terms of trade and Western economic sanctions (Ahn and Ludema 2020), intensified the internal factors.

imply that firms behave *strategically*: if they establish multiple bank relationships, they may use sin banks to store the worst part of their debt and service the best parts in saint banks. When their sin banks are closed, the firms tend to substitute the lost credit at their other banks rather than searching for new lenders.

Finally, one could argue that not all days of NPLs are equally important, given the internationally applied 90-day threshold. Recall that the days of delinquencies in loan repayment reported for each firm-bank match at the Bureau of Credit History (BCH) varies from 0 to more than 200 days, thus covering qualitatively different cases. When choosing between two firms to establish a match, it is likely that new banks pay less attention to the cases when one firm had, say, 30 days and the other had 60 days—both are well below the threshold of 90 days. However, if one of the firms had, say, 120 days, not 30 or 60, then a saint bank may strongly prefer to reject the firm.

We begin with testing the 90 days threshold by substituting our initial variable log $DNPL_{f,t^*}$ with a binary version in which it equals 1 if $DNPL_{f,t^*} \geq 90$ and 0 if else. We find that the estimated coefficient on the new binary variable is insignificant for matching with sin banks and remains negative and highly significant for matching with saint banks.

We then re-categorize the $DNPL_{f,t^*}$ variable on the following seven bins: $0 \leq DNPL_{f,b,t} < 30$ (bin 1, reference), $30 \leq DNPL_{f,b,t} < 60$ (bin 2),..., $DNPL_{f,b,t} \geq 180$ (bin 7). The estimation results appear in Table 3.1 in Appendix 3.E. In column (1) where we analyze matching with new sin banks, the estimated coefficients on the categorical variables $30 \leq DNPL_{f,b,t} \leq 60$ (bin 2) and $60 \leq DNPL_{f,b,t} \leq 90$ (bin 3) are both positive and highly significant. The estimated coefficients for bins 4 and 5 are also positive but insignificant. Before categorizing, we were unable to see the striking estimated coefficient on $150 \leq DNPL_{f,b,t} \leq 180$ (bin 6) and $DNPL_{f,t^*} \geq 180$ (bin 7), which turns *negative* and also highly significant in the last case. Jointly, these results imply that *intensity really matters*: the effect of the days of NPLs on matching with new sin banks is positive for small and moderate magnitudes of loan delinquencies (below 90 days), but turns negative for very large delinquencies (above 150 days). Sin banks, despite being sin, are evidently unwilling to match with "hopeless" firms.

In column (2) with the results on matching with new saint banks, we obtain negative coefficients on mostly all categorical variables, with those for $30 \leq DNPL_{f,b,t} \leq 60$ (bin 2), $120 \leq DNPL_{f,b,t} \leq 150$ (bin 5), and $150 \leq DNPL_{f,b,t} \leq 180$ (bin 6) being significant. Therefore, saint banks really prefer to establish matches with firms that had virtually no bad debts in closed sin banks.

Regarding the other control variables at the firm level, our estimates indicate that all else being equal, size has a non-linear relationship with the likelihood of matching with both sin and saint banks, with mid-sized firms having the largest likelihoods.[26] We also find that more leveraged firms are less likely to find a new match, conditional on surviving to the moment, whereas liquidity seems to have no effect on the hazard rate.

Overall, our regression analysis has shown that firms with more days of NPLs accumulated when their sin bank is closed are *more* likely to match with another (not-yet-detected) sin banks and are *less* likely to establish relationships with saint banks. This favors endogenous firm-bank matching that appears under a stretched-in-time regulation policy targeting sin banks detection. Turning from the granular level, i.e., loan-month, to a more aggregated level, i.e., firm-year, does not lead to the same result. Firms with negative annual profits, either at the moment of sin bank closure or the moment of matching with new banks, are always *less* likely to establish new relationships with banks, regardless of their sin or saint type.

[26]This is consistent with the observation that small firms usually experience more problems obtaining credit, while large firms may either use their own sources of funds or substitute domestic credit with funds raised from international financial markets. Indeed, there is a large body of anecdotal evidence that during the 2010s largest Russian companies, mainly exporters of natural resources, reduced their demand for *domestic* loans and were actively using either international (at least before the Western sanctions in 2014) or local financial markets to place their debts. As is shown by Bruno and Shin (2017), borrowing from abroad is cheaper for large companies operating in EMEs than getting finance in domestic markets.

3.5 Cross-sectional variation in new firm-bank relationships

In this section, we examine how our baseline result on differential sorting of good and bad firms across sin and saint banks depends on common ownership between the old and new banks, how well anticipated the closures of sin banks are, and the concentration of regional credit markets.

3.5.1 Common bank ownership

One potential explanation for our baseline results is that having experienced the closure of their sin banks, bad firms consequently matched with another (not-yet-detected) sin banks that have *the same* directors or owners as the closed sin banks. Several banks may constitute a bank holding group, or the same individuals may appear on the board of directors in different (formally unrelated) banks. We refer to this channel as *common bank ownership*, for simplicity.[27]

To examine the efficacy of common bank ownership we re-estimate our duration regressions (3.2) on a subsample of firms that only match with those new banks that do not share common persons on the board of directors or owners with detected and closed sin banks. To construct such a subsample, we exploit the nation-wide banking media source banki.ru and manually collect data on the ownership structure of the banks that operated in the Russian banking system over the 2010s, which is publicly disclosed through this web-site. The data contains personal information on every member of the board of directors or owners of these banks, including name, surname, and the share in the capital owned.[28]

Overall, we find that among the 956 banks in our database, as many as 238 had

[27]Enikolopov and Stepanov (2013) provide an excellent description of the state of corporate governance in Russia, and Appendix 3.F provides an example of the ownership/governance structure of Alfa-Bank, Russia's major private bank from the top-10 in terms of total assets.

[28]We disclose our *common ownership database* through our websites for further research.

overlapping ownership or control structures. In fact, more than half of all firms that had relationships with sin banks later matched with another (not-yet-detected) sin banks owned or controlled by *the same* persons.

Table 3.5 presents estimation results of the duration regressions (3.2) on the reduced sample without common ownership of "old" (i.e., closed) and "new" (i.e., not-yet-detected) sin banks. Columns (1)-(3) summarize the results of matching with a new sin bank, while columns (4)-(6) present the results of matching with a new saint bank. As can be inferred from column 1 of Table 3.5, the estimated coefficient on the log $DNPL_{f,t^*}$ remains positive, as before, but the size of the coefficient drops by a factor of 2 and, more importantly, the estimate is no longer significant. This clearly indicates that the baseline result on the endogenous sorting of firm-bank matches is fueled by the common ownership phenomenon. It is remarkable that the estimated coefficient is still not negative, as one might expect. We think that this may reflect either inferior expertise in sin banks or the sin banks' exposure to adverse selection of borrowers, intentional or forced by the conduct of market rivals. Further, in columns (2) and (3), we find that the estimated coefficient on the $Profit_{f,t^*} < 0$ and $Profit_{f,t^*+k} < 0$ variables are also insignificant. By contrast, in columns (4)-(6) we show no qualitative differences with our baseline result; quantitatively, the estimates imply even stronger effects than in the respective part of the baseline result.

Overall, our estimation results highlight the importance of common ownership for matching between bad firms and (not-yet-detected) sin banks after the closure of the firms' prior sin banks. In fact, in the subsample without common ownership, our estimation no longer predicts that bad firms are more likely to end up in a new match with a (not-yet-detected) sin bank. In contrast, good firms are more likely to match with new saint banks regardless of whether there is common ownership between their old and new banks.

Table 3.5: Channels of endogenous firm-bank matching: Common bank group owners

Note: The table reports estimates of new firm-bank matching following the firms' f prior sin banks closure, as implied by equation (3.2) and conditional on the new bank not sharing owners or governors with the closed sin bank. Dependent variable $\lambda_j(t, \mathbf{X}_{f,t-1}; \Theta)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include a firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and its liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. The constant term is included but not reported to save space. Other firm controls, bank closure event FEs, regional FEs, and industry FEs are included in all specifications.

	Switch to a sin bank w/out common owners			Switch to a saint bank w/out common owners		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL$_{f,t^*}$	0.082			–0.125***		
	(0.082)			(0.046)		
Profit$_{f,t^*} < 0$		–0.886	–0.589		0.341	0.512
		(0.912)	(0.902)		(0.305)	(0.314)
Profit$_{f,t^*+k} < 0$			–0.483			–0.418**
			(0.384)			(0.202)
N obs	107,220	76,235	76,160	107,434	76,371	76,296
N firm-bank new matches	116	99	99	361	274	274
N firms	2,757	1,965	1,962	2,764	1,969	1,966
log L	–590.8	–471.9	–470.9	–1,489.1	–1,134.7	–1,132.2

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

3.5.2 Surprising bank closures

With such a large number of sin bank closures, it is natural that not all of them were perceived as equally likely to happen: while some were more predictable, others were more surprising. The more predictable closures are indicative of more severe or, at least, more transparent bank fraud. Intuitively, following a relatively more anticipated sin bank closure, the firms must have had a harder time finding new matches, regardless of their perceived quality based on their credit history or profitability. Therefore, we examine how our baseline results are affected by how well specific bank closures could have been predicted.

To capture the effect of the degree of surprise regarding closures of sin banks on new firm-bank matching, we apply the following two-stage approach. First, we run a simple predictive logit regression of bank closures and sort all failed banks by their respective predicted probabilities into two groups: well-predicted closures and surprise closures. Second, we re-estimate our duration regression (3.2) separately for these two groups of closures.

The details in the first stage—that is, the estimation of the predictive model—are presented in Table 3.1 in Appendix 3.G. We define well-predicted closures as those with a predicted probability of closure above threshold $\bar{p}$, while surprise closures are those with a predicted probability of closure below the threshold $\bar{p}$. We set the threshold $\bar{p} = 0.5\%$, which is the mean of the predicted monthly probability of bank closure in the sample.[29] As a result of this sorting, we classify about 250 bank closures as surprises and about 150 ones as well predicted. In Figure 3.7, we plot the evolution of predicted probabilities of closure in time. The predicted probabilities are close to zero prior to implementation of the policy, and increase dramatically during the active phase of the policy (i.e., between 2013M7 and 2018M2).[30]

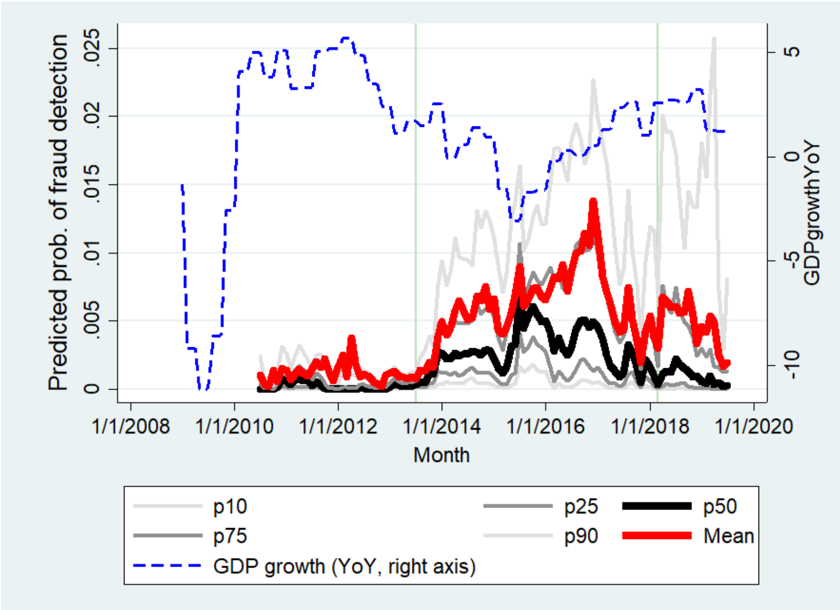
Table 3.6 presents the results of estimating the duration regression (3.2) separately for surprise closures in columns (1)-(2) and for well-predicted ones in columns (3)-(4). Panel 1 of Table 3.6 demonstrates that our baseline results are fully driven by the surprise bank closures. In the duration regressions of matching with new sin banks, the estimated coefficient on log $DNPL_{f,t^*}$ is positive and highly significant in the case of surprise closures (column 1) and is negative and insignificant in the case of well-predicted closures (column 3). Moreover, in the case of surprise closures, the magnitude of the effect rises by about 30% compared to the baseline estimation results. Similarly, in the duration

[29] Annualized, the average predicted probability of closure is about 6%. As a part of robustness checks, we also consider substantially higher values for the threshold $\bar{p}$ of 1% and 1.5%. Qualitatively, our results are robust to these higher values of the threshold.

[30] Note that the predicted probabilities are at the monthly frequency. It is also notable that the probabilities peak in 2016–2017, at least one year before the end of the active phase. We also observe no clear correlations between the predicted probabilities and annual real GDP growth rates. This suggests that the policy and macroeconomic conditions were fairly orthogonal to each other.

Figure 3.7: Time evolution of selected bank variables before and during the active phase of the tight regulation policy (Jul.2013–Feb.2018)

Note: The figure depicts the time evolution of the predicted probabilities of fraud detection at the bank-month level before, during, and after the active phase of the tight regulatory policy against the background of annual GDP growth rates in Russia. The active phase of the policy is marked with two vertical green lines.



regressions of matching with saint banks, the estimated coefficient on the log $DNPL_{f,t^*}$ variable is negative and highly significant under the surprise condition (column 2), but is insignificant under the other condition (column 4). Again, the magnitude of the coefficient increases by about 30% compared to the baseline estimation results.

In Panel 2 of Table 3.6, we replace the log $DNPL_{f,t^*}$ with the firm's profitability as an alternative proxy of its quality variable—that is, with $Profit_{f,t^*} < 0$ and $Profit_{f,t^*+k} < 0$. The estimation results in Panel 2 are consistent with those in Panel 1. We obtain significant coefficients on the negative profits variables only in the case of the surprise closures—columns (1) and (2).

Overall, our results suggest that when the closure of a sin bank is more predictable, the quality of its firms measured by accounting information does not help to predict the sorting of new firm-bank matches. If we interpret higher closure predictability as evidence of more severe fraud, then one potential explanation behind this result is that the firm's past accounting information is viewed by banks as being less reliable if it was generated

Table 3.6: Channels of endogenous firm-bank matching: surprising bank closures

Note: The table reports the estimates of new firm-bank matching following the firms' f prior sin banks closure, as implied by equation (3.2) and conditional on the sin bank closure being less predictable (*Surprise*) or more predictable (*Not a surprise*). Dependent variable $\lambda_j(t, \mathbf{X}_{f,t-1}; \Theta)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include a firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. *Surprise* indicates that the estimations are performed on the subsample of only those banks for which the predicted probability of fraud detection is *below* the unconditional threshold of 0.5% monthly (or 6% annually). *Not a surprise*, in contrast, means *above* the threshold. Details on modeling the probability of fraud detection are in Appendix 3.G. The sample includes those firms that have a *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. The constant term is included but not reported to preserve space. Other firm controls, bank closure event FEs, regional FEs, and industry FEs are included in all specifications.

Previous sin bank closure:	*Surprise*		*Not a surprise*	
Match with a new bank:	sin bank	saint bank	sin bank	saint bank
	(1)	(2)	(3)	(4)
Panel 1: Firm quality: Days of NPLs				
log DNPL_{f,t^*}	0.204***	–0.118***	–0.072	–0.019
	(0.065)	(0.044)	(0.143)	(0.069)
N obs	224,821	225,274	32,369	32,407
N new firm-bank matches	168	611	32	104
N firms	5,193	5,203	876	877
log L	–893.6	–2,479.7	–157.4	–428.7
Panel 2: Firm quality: Negative profits				
$\text{Profit}_{f,t^*} < 0$	–2.039*	0.124	0.385	0.443
	(1.202)	(0.279)	(1.822)	(0.701)
$\text{Profit}_{f,t^*+k} < 0$	–0.711**	–0.364**	0.095	–0.614
	(0.350)	(0.164)	(0.744)	(0.389)
N obs	154,007	154,382	24,365	24,376
N new firm-bank matches	143	459	25	78
N firms	3,545	3,551	650	649
log L	–719.0	–1,827.8	–112.5	–319.8

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

in a relationship with a more fraudulent bank.

3.5.3 Regional credit market concentration

Next, we examine the effect of regional bank market concentration on our baseline results. The CBR's cleansing policy was associated with a rising market concentration, because many banks were closed. Following a sin-bank closure, firms have fewer opportunities to find a new bank match. Bad firms become increasingly more restricted in their ability to match with *not-yet-detected* sin banks. Saint banks, however, may be less willing to lend to bad firms to protect their market power from the uncertainty associated with financing bad firms. Thus, in need of credit, bad firms could be effectively forced to improve to be accepted by saint banks.

To examine the effect of regional bank market concentration on our baseline results, we slightly modify our duration regressions by introducing a cross-product of the regional HHI concentration measure with a firm quality proxy:

$$\begin{aligned}\lambda_\jmath\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(&\alpha_\jmath + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \beta_{\jmath,1}\text{Firm.Quality}_{f,t-1} + \mathbf{C}_{f,t-1}\Gamma_\jmath \\ &+ \beta_{\jmath,2} HHI.credit_{r,t-1} + \beta_{\jmath,3} \cdot \text{Firm.Quality}_{f,t-1} \times HHI.credit_{r,t-1}\Big),\end{aligned} \tag{3.3}$$

The estimation results are presented in Table 3.7, where Panel 1 contains the results with firm quality proxied with the days of NPLs, while in Panel 2, firm quality is proxied with negative profits.

As can be seen in Panel 1, the estimated coefficient on the interaction of log $DNPL_{f,t^*}$ and $HHI.credit_{r,t-1}$ is insignificant in column (1) and positive and highly significant in column (2). Qualitative, the same result is in Panel 2. These results suggest that the rising concentration of credit markets was unlikely to prevent bad firms from matching with not-yet-detected sin banks but that it did facilitate new matches between bad firms and saint banks. One possible interpretation is that the saint banks could extract rent from relationships with bad firms by setting higher interest rates. Furthermore, saint banks operating in regions with highly concentrated credit markets may be more skilled

Table 3.7: Channels of new firm-bank matching: regional credit market concentration

Note: The table reports the estimates of new firm-bank matching following the firms' f prior sin banks closure, as implied by equation (3.3). Dependent variable $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms exit, i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. $HHI.credit_{r,t}$ is the Herfindahl-Hirschman index of regional credit market concentration. Other controls include a firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. Other firm controls, bank closure event FEs, regional FEs, and industry FEs are included in all specifications. The sample includes those firms that have a *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. The constant term is included but not reported to preserve space.

	Switch to a sin bank	Switch to a saint bank
	(1)	(2)
Panel 1: Firm quality: Days of NPLs		
log $\text{DNPL}_{f,t^*} \times \text{HHI.credit}_{r,t-1}$	0.501	1.430***
	(1.017)	(0.425)
log $\text{DNP}L_{f,t^*}$	0.223***	–0.107**
	(0.068)	(0.043)
$\text{HHI.credit}_{r,t-1}$	1.406	5.625***
	(1.405)	(0.649)
N obs	222,837	223,290
N firms	5,159	5,169
N new firm-bank matches	168	611
log L	–891.0	–2,434.6
Panel 2: Firm quality: Negative profits		
$\text{Profit}_{f,t^*} < 0 \times \text{HHI.credit}_{r,t-1}$	–6.860	4.364*
	(8.042)	(2.487)
$\text{Profit}_{f,t^*+k} < 0 \times \text{HHI.credit}_{r,t-1}$	2.946	2.399**
	(3.977)	(1.138)
$\text{Profit}_{f,t^*} < 0$	–2.221*	–0.036
	(1.143)	(0.311)
$\text{Profit}_{f,t^*+k} < 0$	–0.681*	–0.405**
	(0.349)	(0.171)
$\text{HHI.credit}_{r,t-1}$	–0.721	3.110***
	(2.036)	(0.811)
N obs	152,735	153,110
N firms	3,526	3,532
N new firm-bank matches	143	459
log L	–718.7	–1,808.0

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

in evaluating projects, and thus they might be able to provide valuable expertise to bad firms, helping them improve their fundamentals.[31]

3.6 Sin bank closure and firm performance

In this section, we examine the real effects of sin bank closures on firm performance and explore the corresponding mechanisms.

3.6.1 The real effects of sin bank closures

Closure of a (sin) bank can be viewed as a *credit supply shock*. The literature typically considers the effects of credit supply shocks on firm employment (Chodorow-Reich 2014), investment and sales (Gropp et al. 2018; Degryse et al. 2019; Chopra, Subramanian, and Tantri 2020), among other measures of performance. We employ similar characteristics of performance except for investment.[32] We also use firm profits and firm default rates as our measures of firm performance.

There is one potential issue with evaluating the effect of sin bank closures on firm performance. If firms anticipate the closure of their sin banks in advance, they could make preemptive adjustments. If so, the effect of a sin bank closure per se could be distorted by firm's preemptive actions. We conduct two tests to examine whether firms could anticipate the closure of their sin banks in advance. We test whether firms preemptively leave sin banks in anticipation of regulatory closure, and whether firms strategically delay loan repayments around the closure date. These tests are based on the conjectured heterogeneous responses of high and low-quality firms to the prospect of sin bank closure, and are presented in Appendix 3.H. These tests do not support the hypothesis that firms anticipated the closure of their sin banks.

[31] Political pressure to lend to specific firms, which may be bad, is an alternative explanation.

[32] Our firm-level data (provided by SPARK-Interfax) unfortunately contains a very large number of missing values on investment. Thus, using the data on investment will result in the total number of observations shrinking by a factor of 10, at least.

Specifically, we want to understand what happens to firm performance *after* the firms experience the closure of their prior sin banks, but *before* they find new bank matches. On the one hand, one might expect that firm performance deteriorates because, by losing their bank, firms become more financially constrained (Chodorow-Reich 2014; Chopra, Subramanian, and Tantri 2020). On the other hand, firm performance could improve due to the termination of the hold-up problem (Liaudinskas and Grigaite 2021).

To answer this question, we employ the difference-in-differences approach with the time-varying imposition of treatment (TV-DID, Goodman-Bacon 2021). The *treatment group* consists of all those firms, bad and good, that experienced sin bank closures at some point in time during 2013–2020. Specifically, we define treatment as the closure of sin bank b that affects firm f at time $t^*_{b,f}$. Thus, our treatment variable is $Sin.Bank_{b,f}$ which equals 1 if firm f's bank b is closed due to fraud detection at $t^*_{b,f}$. Furthermore, for each firm f, let $POST_{\{t \geq t^*_{b,f}\}}$ define an indicator variable equals 1 for all t following $t^*_{b,f}$, and 0 otherwise.

The *control group* is constructed by matching firms on the set of observable characteristics using the nearest neighborhood estimator of Abadie and Imbens (2011). The following set of observable characteristics is employed: firm size, leverage, liquidity, return on assets, and annual growth of total assets. We follow the so-called 1:4 rule of thumb and match firm f that has experienced bank closure at $t^*_{b,f}$ (i.e., a *treated* firm) with four similar (*control*) firms that (i) also have relationships with sin banks and (ii) have not experienced closures of their sin banks within two years before and after firm f.[33]

Acknowledging that any real effect of sin bank closure on a firm's performance can be mitigated if the firm has more than one bank relationship (i.e., borrows from more than one bank), we focus only on *single*-bank firms (Degryse et al. 2019). We thus run

[33] That is, we consider a moving window of $[t^*_{b,f} - 2, t^*_{b,f} + 2]$. Our treatment group includes firms that experienced closures of their sin banks between 2011 (beginning of the sample period) and 2018. Because our sample ends in 2020, the last treated firm appears at the end of 2018 since, by construction, we require that it is matched with four control firms that did not experience their sin bank closures between 2016 and 2020 (end of the sample period).

our TV-DID regressions for the subsamples of firms that had only one (sin) bank at the moment of the bank's closure.

Formally, we specify the following TV-DID regression:

$$\begin{aligned} Y_{f,t} = \alpha_f + \alpha_t + \beta_1 \Big(Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}} \Big) + \\ + \beta_2 \Big(Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t} \Big) \\ + X'_{f,b,t}\Psi + \varepsilon_{f,t}, \text{ if } t \in [t^*_{b,f} - 2, t^*_{b,f} + 2] \end{aligned} \tag{3.4}$$

where $Y_{f,t}$ is a measure of firm f's performance, among which we consider (i) firm size (the log of total assets), (ii) the ratio of debt to total assets, (iii) the ratio of total revenue to total assets, (iv) the ratio of the number of workers to total revenue, (v) the ratio of profit to total assets, and (vi) a binary variable which equals 1 if firm f defaults in year t and 0 otherwise. $X_{f,b,t}$ includes various control variables including firm size and its square, leverage, and liquidity ratios to total assets, where appropriate, to capture any residual differences between the treated and control firms remaining after the 1:4 nearest neighborhood matching. Equation (3.4) is estimated with logit when the dependent variable is binary (i.e., case (vi)) and with panel FE estimator otherwise (cases (i)-(v)).[34] We require firms to not default between $t^*_{b,f} - 2$ and $t^*_{b,f}$ in logit regression (case (vi)) and we require firms to survive until at least $t^*_{b,f} + 2$ in panel FE regressions (cases (i)-(v)) to control for the survivorship bias (Brown et al. 1992).

The estimation results of equation (3.4) are summarized in Table 3.8. After the nearest neighborhood matching and restricting the sample of firms by imposing condition $t \in [t^*_{b,f} - 2, t^*_{b,f} + 2]$ years, we have only about 10,745 to 18,613 observations at the firm-year level for different dependent variables.

Column (1) of Table 3.8 starts with firm size as the dependent variable in equation (3.4). The estimated coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ is positive and highly

[34]Those observations for which a new firm-bank match is created before $t^*_{b,f} + 2$ are censored, to ensure that we are analyzing firm performance before the firm finds a new bank.

Table 3.8: Difference-in-differences estimation results: firm performance after sin bank closures and before establishing new firm-bank relationships

Note: The table reports the estimates of firm performance after a firm experiences closure of its prior sin bank and before it establishes a relationship with a new bank, as implied by equation (3.4). Firm performance is proxied with the following dependent variables $Y_{f,t}$: firm size, as captured by the log of total assets (*log(TA)*, column 1), the ratio of borrowed funds to total assets (*Borrow/TA*, column 2), revenue to total assets (*Revenue/TA*, column 3), number of workers to total revenue ratio (*Employ/Revenue*, column 4), profit after taxes to total assets (*Profit/TA*, column 5), a binary indicator of whether a firm f defaults at year t (*Default=1*, column 6). $Sin.Bank_{b,f} = 1$ if bank b that has a relationship with firm f is closed for fraud at some point in time within the sample period, and 0 if it survives till the end of the sample. $POST_{\{t \geq t^*_{b,f}\}} = 1$ if $t \geq t^*_{b,f}$, and 0 if else. $Bad.Firm_{f,t}$ is a binary variable that equals 1 for firms with losses, and 0 for profitable firms. We run our regressions for $t \in [2011, 2020]$ on a panel of matched firms that experienced sin bank closures and only had relationships with a single bank at the moment of its forced closure. We also restrict the panel so that it only includes observations up to two years before and after $t^*_{b,f}$, i.e., firm-bank-specific windows $[t^*_{b,f} - 2, t^*_{b,f} + 2]$ years. We employ 1:4 nearest neighborhood matching of firms prior to $t^*_{b,f}$ using the five observables: firm size, leverage, liquidity, annual growth of total assets, and profitability. All regressions contain all necessary sub-products of the triple interaction variable $Sin.Bank_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$, firm and year fixed effects, and the set of firm controls to capture any residual differences across treated and control firms after 1:4 matching (firm size, except (1); leverage, except (2); and liquidity).

$\mathrm{Y}_{f,t} :=$	log(TA)	$\frac{Borrow}{TA}$	$\frac{Revenue}{TA}$	$\frac{Employ}{Revenue}$	$\frac{Profit}{TA}$	Default
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Focus variables:						
$\mathrm{Sin.Bank}_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}}$	0.205***	0.011	0.384***	–4.408*	0.006	–2.566*
	(0.043)	(0.018)	(0.140)	(2.365)	(0.017)	(1.468)
$\mathrm{Sin.Bank}_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times$	–0.320**	0.101*	–0.770**	10.553**	–0.017	n/a
$\times$ $\mathrm{Bad.Firm}_{f,t}$	(0.136)	(0.060)	(0.325)	(4.239)	(0.030)	
Panel 2: Key components of the triple interaction variable:						
$\mathrm{Sin.Bank}_{b,f}$	–0.091**	–0.009	–0.210**	1.994**	0.000	2.356***
	(0.040)	(0.011)	(0.101)	(0.921)	(0.014)	(0.319)
$\mathrm{POST}_{\{t \geq t^*_{b,f}\}}$	0.082**	–0.030	–0.184	4.940*	–0.028	0.595
	(0.037)	(0.025)	(0.162)	(2.852)	(0.018)	(1.500)
$\mathrm{Bad.Firm}_{f,t}$	–0.008	0.085***	–0.316***	8.703**	–0.180***	0.607
	(0.029)	(0.031)	(0.120)	(3.599)	(0.014)	(0.717)
N obs	17,174	18,861	17,835	11,683	18,613	10,745
N firms	3,226	3,261	3,234	2,869	3,258	3,237
R^2 (pseudo / LSDV)	0.3	0.7	0.1	0.0	0.1	0.1

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

statistically significant, whereas the coefficient β_2 on the $Sin.Bank_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ is negative and statistically significant at 5%. This implies that while good firms tend to grow in size following sin bank closure (but before they start borrowing from a new bank), bad firms tend to shrink in size. Economically, these effects and their differences are significant. Regressions in the next columns of the table shed light on why we obtain these differential effects.

In column (2) of Table 3.8, we turn to results on firm leverage. The estimated coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ is positive, but it is not statistically significant. Thus, we do not find any effect on a treated firm's leverage-to-total assets ratio. The estimated coefficient β_2 on the $Sin.Bank_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ is, however, positive and marginally significant. Therefore, our results suggest that the leverage of bad firms increases by as much as 10 p.p. relative to good firms. This is a sizeable effect, given that the mean leverage ratio of the firms in our sample lies between 75 and 95%. Further estimations show that the absolute amount of borrowing by low-quality treated firms declines, but by less than their total assets (see column (2) of Table 3.1 in Appendix 3.I).

Column (3) of Table 3.8 presents the result for firm total revenue. We obtain a positive and highly significant coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ and a negative and significant coefficient β_2 on $Sin.Bank_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$. In absolute terms, β_2 exceeds β_1 by a factor of 2, which implies a heterogeneous treatment effect on firm revenue-to-total assets depending on firm quality. Following their sin bank closures, good firms have rising revenue, while the revenue of bad firms declines, relative to the total assets of respective firms. The result on firm revenue is in line with the result on leverage. It provides evidence of a cleansing effect of sin bank closure: good firms improve while bad firms deteriorate.

In column (4) of Table 3.8, we present our results on firm employment. We obtain a negative and marginally significant coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ while coefficient β_2 on the $Sin.Bank_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ is estimated to be positive and significant. In absolute terms, β_2 exceeds β_1 by a factor of 2. That is, the treatment effect on firm employment-to-total revenue is also heterogeneous. Following their sin bank closures, good firms reduce their labor force to total revenue ratio, whereas bad firms expand the labor force loading on their total revenue, compared to control firms. Given that good firms also raise their total revenues—column (3)—we find that their revenues grow faster than the number of workers employed. This is also confirmed by our additional regressions, in which we replace the revenue-to-total assets ratio with the log of revenue and employment-to-revenue with the log of employment: the estimated semi-elasticity of revenue exceeds that of the number of workers by a factor of two (compare columns (3) and (4) of Table 3.1 in Appendix 3.I).

In column (5) of Table 3.8, we examine the effect on profits. We obtain a positive but insignificant coefficient β_1 on the $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ variable and negative and insignificant coefficient β_2 on the $Sin.Bank_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ variable. The signs are consistent with the firm improvement hypothesis for good firms and the firm deterioration hypothesis for bad firms. However, since the effects are insignificant, we interpret these results with caution.

Finally, column (6) presents the estimation result when the dependent variable is *firm defaults*. In this case, we obtain a negative and marginally significant coefficient on the $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ variable. That is, following the closure of a sin bank, the failure risk of a good firm *decreases*. At the same time, the variable $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t} = 1$ perfectly predicts firm defaults, and thus this variable is dropped from the estimations (marked as "n/a" in the table). Our results thus suggest that while the stability of high-quality firms improves following the closure of a sin bank, the opposite result holds for low-quality firms. This result is consistent with the findings in previous columns and indicates the cleansing effect of sin bank closure on firms.

3.6.2 Exploring the mechanisms: credit risk underpricing

Next, we investigate why good firms may improve while bad firms may deteriorate following their sin bank closures. We hypothesize that sin banks may underprice credit risk when lending to firms, thus effectively subsidizing firm credit. The loss of such a subsidy would naturally pose a larger problem to a bad firm. Thus, underpricing of the credit risk by sin banks could explain why bad firm performance is likely to deteriorate following the closure of its sin bank—the loss of the subsidy combined with fewer opportunities and incentives to improve will further deteriorate the state of bad firms. On the other hand, good firms have better incentives and abilities to improve following the closure of their sin banks, which helps to decrease their cost of credit in the future.

To test this hypothesis, we employ credit registry loan-level data on interest rates available from 2017 at a monthly frequency. The credit register contains data on loan contracts and includes the interest rate, loan amount, loan maturity, type of credit, and ex-ante assessment of the borrower's credit risk on a scale of 1 to 5, with 1 being the lowest risk and 5 being the highest.

First, we examine a linear regression model of the interest rate that a bank b sets to firm f at month t on the sin bank indicator variable (bank level), credit risk category from 1, lowest risk, to 5, highest risk (firm-bank-month level), and the product of the two, controlling for firm and firm*month fixed effects, log of loan volumes, loan maturities, relevant bank-level controls, and regional and macroeconomic characteristics:

$$r^L_{f,b,t} = \sum_{j=1}^{5} \beta_j \cdot \left(Sin.Bank_{b,f} \times Credit.Risk^{(j)}_{f,b,t} \right) + \gamma Sin.Bank_{b,f} + Loan.Control'_{f,b,t}\Xi$$

$$+ Bank.Control'_{b,t}\Psi + Macro.Control'_{t}\Phi + \alpha_f + \alpha_{f,t} + \epsilon_{f,b,t} \quad (3.5)$$

With this composition of variables, we have up to 1,774,379 loan-level observations. We obtain a positive and highly significant coefficient on the $Sin.Bank_{b,f}$ variable, meaning that sin banks charge 1.5 p.p. higher interest rates on loans than the saint banks (see Table 3.9). However, we further obtain a negative and highly significant coefficient on the interactions of the sin bank variable and credit risk category, and the magnitudes of the estimates range from –1.6 to –0.5 p.p. This means that within *the same* sin bank, firms with poorer quality *pay less* on their loans, while firms with better quality *pay more*. These results hold on the sample of all loans issued and for the subsample of multiple loans, i.e., for the firms that obtained at least two loans within the 2017-2020 period. The latter subsample allows us to shut down demand effects by including firm*month fixed effects. Overall, our regression results here are consistent with the hypothesis that sin banks underprice risk, especially in the case of low-quality firms.

Second, we proceed to a linear regression of the credit risk category on the bad firm and sin bank indicator variables, the product of the two, controlling for the same characteristics as in the interest rate regression above. The credit risk regression reads as:

$$\begin{aligned} Credit.Risk_{f,b,t} = {} & \beta \cdot \Big(Sin.Bank_{b,f} \times Bad.Firm_{f,t} \Big) + \gamma Sin.Bank_{b,f} + \delta Bad.Firm_{f,t} \\ & + Loan.Control'_{f,b,t}\Xi + Bank.Control'_{b,t}\Psi + Macro.Control'_{t}\Phi + \alpha_f + \alpha_{f,t} + \epsilon_{f,b,t} \end{aligned} \tag{3.6}$$

With this composition of variables, we have up to 1,263,970 loan-level observations. We obtain a negative and highly significant coefficient on the sin bank variable, meaning that the same borrower is given higher credit quality ex-ante assessment by sin banks than by saint banks (see Table 3.10). Further, we obtain a negative and highly significant coefficient on the interaction of the sin bank and bad firm indicators, which implies that within a given sin bank, bad firms receive relatively higher, not lower, credit quality assessments. As in the case of interest rate regressions, these results hold for both the sample of all loans and the subsample of multiple loans.

Table 3.9: Interest rates and amount of loans in sin banks

Note: The table reports the estimates of equation (3.5), where the dependent variable is either the loan interest rate (columns 1–2) or the log of the loan amount (columns 3–4) at the firm-bank-month level, $t \in$ 2017M1–2020M9. $Sin.Bank_{b,f}$ is an indicator variable of a sin bank, i.e., a bank that ever experienced license revocation due to fraud detection. $Credit.Risk_{f,b,t}$ is a categorical variable ranging from 1 (the lowest risk, or the best quality, reference) to 5 (the highest risk, or the worst quality) to reflect a bank's ex-ante assessment of the loan credit risk. $Loan.Control_{f,b,t}$ includes loan quality, log of the loan amount (columns 1–2), the interest rate on the loan (columns 3–4), maturity of the loan, and loan type (credit lines, overdraft, etc.). $Bank.Control_{b,t}$ includes the structure of bank assets (loans to firms, loans to households), the structure of bank liabilities (equity capital, deposits of firms, households, and government), all as % of bank total assets, bank size (log of total assets), and the ex-post quality of bank loans (NPL ratio), which are not reported to save space. α_f is firm fixed effects and $\alpha_{f,t}$ is firm*month fixed effect (capturing firm demand on loans). *All loans* means each and every loan from the credit register is included in the regression ($\alpha_{f,t}$ is not included), whereas *Multiple loans* involves a subsample of those firms that obtain credit at least twice in the time period considered ($\alpha_{f,t}$ is included). *n/a* means the effect is absorbed by (month) fixed effects.

	Interest rate on loan, $Interest.Rate_{f,b,t}$		log of loan amount, log $Loan_{f,b,t}$	
	All loans	Multiple loans	All loans	Multiple loans
	(1)	(2)	(3)	(4)
$\text{Sin.Bank}_{b,f}$	1.579***	1.519***	–0.104***	–0.270***
	(0.091)	(0.181)	(0.037)	(0.084)
$\text{Credit.Risk}_{f,b,t} = 1$ (*reference*)				
$\text{Credit.Risk}_{f,b,t} = 2$	0.029**	0.191***	–0.060***	–0.075***
	(0.012)	(0.023)	(0.006)	(0.015)
$\text{Credit.Risk}_{f,b,t} = 3$	0.593***	1.228***	–0.045***	–0.024
	(0.029)	(0.080)	(0.012)	(0.034)
$\text{Credit.Risk}_{f,b,t} = 4$	0.045	0.594***	–0.282***	–0.217**
	(0.036)	(0.127)	(0.026)	(0.105)
$\text{Credit.Risk}_{f,b,t} = 5$	–0.052	0.713***	–0.040	0.235*
	(0.069)	(0.257)	(0.045)	(0.135)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 2$	–0.502***	–0.786***	0.095***	0.259***
	(0.083)	(0.189)	(0.035)	(0.088)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 3$	–1.036***	–1.648***	0.104**	0.680***
	(0.116)	(0.307)	(0.050)	(0.154)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 4$	–0.718***	–0.194	0.310***	1.345***
	(0.149)	(0.826)	(0.092)	(0.484)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 5$	–0.750***	–0.930	–0.222	0.271
	(0.259)	(0.688)	(0.223)	(0.232)
log $\text{Loan}_{f,b,t}$	–0.056***	–0.032***		
	(0.002)	(0.005)		
$\text{Interest.Rate}_{f,b,t}$			–0.031***	–0.022***
			(0.001)	(0.004)
$\text{HHI.Credit}_{r,t}$	–0.001***	n/a	–0.000***	n/a
	(0.000)		(0.000)	
Firm FEs	Yes	Yes	Yes	Yes
Firm × month FEs	No	Yes	No	Yes
Obs	1,774,379	679,356	1,774,379	679,356
R^2 (adj.)	0.9	0.8	0.7	0.6

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank-firm level and appear in the brackets under the estimated coefficients.

Table 3.10: Loan quality in sin and saint banks: regression estimation results

Note: The table reports estimates of the following loan-level regressions: $Credit.Risk_{f,b,t} = \beta \cdot \left(Sin.Bank_{b,f} \times Bad.Firm_{f,t}\right) + \gamma Sin.Bank_{b,f} + \delta Bad.Firm_{f,t} + Loan.Control'_{f,b,t}\Xi + Bank.Control'_{b,t}\Psi + Macro.Control'_{t}\Phi + \alpha_f + \alpha_{f,t} + \epsilon_{f,b,t}$, where $Credit.Risk_{f,b,t}$ is a categorical variable ranging from 1 (the lowest risk, or the best quality, reference) to 5 (the highest risk, or the worst quality) to reflect a bank's ex-ante assessment of the loan credit risk, $t \in$ 2017M1–2020M9. $Sin.Bank_{b,f}$ is an indicator variable of a sin bank, i.e., a bank that ever experienced license revocation due to fraud detection. $Bad.Firm_{f,t}$ is a binary variable that equals 1 for firms with losses, and 0 for profitable firms. $Loan.Control_{f,b,t}$ includes the log of the loan amount, maturity of the loan, and loan type (credit lines, overdraft, etc.). $Bank.Control_{b,t}$ includes the structure of bank assets (loans to firms, loans to households), the structure of bank liabilities (equity capital, deposits of firms, households, and government), all as a % of bank total assets, bank size (log of total assets), and the ex-post quality of bank loans (NPL ratio), which are not reported to save space. $Macro.Control_t$ is GDP growth rate (YoY) and regional credit market concentration, as proxied with $HHI_{r,t}$. α_f is firm fixed effects and $\alpha_{f,t}$ is firm*month fixed effect (capturing firm demand on loans). *All loans* means each and every loan from the credit register is included in the regression ($\alpha_{f,t}$ is not included), whereas *Multiple loans* involves a subsample of firms that obtain credit at least twice in the time period considered ($\alpha_{f,t}$ is included). *n/a* means the effect is absorbed by (month) fixed effects.

$Y_{f,b,t} :=$	Loan quality	
	All loans	Multiple loans
	(1)	(2)
$\text{Sin.Bank}_{b,f}$	–0.073***	–0.062***
	(0.011)	(0.019)
$\text{Bad.Firm}_{f,t}$	0.026***	0.003
	(0.003)	(0.007)
$\text{Sin.Bank}_{b,f} \times \text{Bad.Firm}_{f,t}$	–0.047**	–0.192***
	(0.021)	(0.055)
$\log \text{Loan}_{f,b,t}$	–0.003***	–0.002***
	(0.000)	(0.001)
GDP.Growth_t	0.007***	n/a
	(0.000)	
$\text{HHI.Credit}_{r,t}$	0.000***	n/a
	(0.000)	
Firm FEs	Yes	Yes
Firm × month FEs	No	Yes
Obs	1,263,970	679,904
R^2 (adj.)	0.7	0.8

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank-firm level and appear in the brackets under the estimated coefficients.

3.7 Conclusion

Our study shows that following sin bank closures, bad firms are more likely to match with any remaining sin banks, especially if the banks are held by the same owners or operate in relatively less concentrated regional credit markets. Good firms on the other hand match with saint banks. The tight policy of the Central Bank of Russia in the 2010s had cleansing effects on the performance of firms during the transition period, i.e., after their current sin banks were closed and before they matched with any remaining banks.

Overall, our analysis of sin bank closures provides evidence of heterogeneous treatment effects on firm performance. Closing sin banks improves the state of good firms while having the opposite effect on bad firms. These heterogeneous effects on firm performance are channeled through credit risk underpricing by sin banks, especially in the case of low-quality firms.

3.A Description of the data

Table 3.1: The List of financial statement variables for the survival analysis and difference-in-difference analysis

Name	Definition	Source
Survival regression analysis		
Size	$\ln(\text{Total assets})$	Balance sheet
Leverage	$\frac{\text{Short-term liabilities} + \text{Long-term liabilities}}{\text{Total assets}}$	Balance sheet
Liquidity	$\frac{\text{Current liabilities} - (\text{Accounts payable} + \text{Short-term loans})}{\text{Total assets}}$	Balance sheet
Profit	Gross profit	Income statement
Difference-in-difference analysis		
Default	= 1 if firm is bankrupt at t	Register of Legal Entities
Employ	$\frac{\text{Number of workers}}{\text{Sales}}$	Balance sheet
Revenue	$\frac{\text{Sales}}{\text{Total assets}}$	Income statement, Balance Sheet
Profit	Gross profit	Income statement
Borrowed funds	Short-term liabilities+Long-term liabilities	Income statement
Total assets	Sum of all assets	Balance Sheet

3.B Three-outcomes bank-firm matching model

Table 3.1: Multinomial logit regression results: splitting the firm-bank matches

Note: The table reports estimates of a multinomial logit model of new firm-bank matching that follows the closure of firm f's prior sin bank b, as an analog to the duration regression (3.2). Differently from equation (3.2) that splits the match option to "match with sin bank" or "match with saint bank," conditional on surviving till month t, the multinomial regression here assembles all the three outcomes: never match, match with sin or saint banks ($\jmath = 0, 1, 2$): $\mathbf{Pr}(Match_{f,t} = \jmath \,|\, \mathbf{X}_{f,t-1}; \Theta) = \Lambda\Big(\alpha_\jmath + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_\jmath + \mathbf{C}_{f,t-1}\Gamma_\jmath\Big)$, where the dependent variable $Match_{f,t}$ is a categorical variable that equals zero if a firm that experienced closure of its prior bank never finds a new bank match (*reference*, 1 if a firm finds a new match with a *sin* bank (columns 1–3), 2 if with a *saint* bank (columns 4–6). Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure—that is, by $t^*_{f,b}$—or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include a firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of marginal effects are reported. Constant is included but not reported to save space.

	Switch to a sin bank			Switch to a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL$_{f,t^*}$	0.126***			–0.063**		
	(0.041)			(0.029)		
Profit$_{f,t^*} < 0$		–1.278*	–1.029		0.104	0.245
		(0.722)	(0.711)		(0.211)	(0.210)
Profit$_{f,t^*+k} < 0$			–0.568**			–0.328**
			(0.268)			(0.143)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank closure event FEs	No	No	No	No	No	No
Regional FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N obs	263,502	183,166	183,088	263,502	183,166	183,088
N firm-bank new matches	200	168	168	715	537	537
N firms	6,921	4,770	4,767	6,253	4,327	4,324
log L	–6,428	–4,879	–4,874	–6,428	–4,879	–4,874

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

3.C Bank-firm matching model with macroeconomic and regional credit market controls

Table 3.1: Survival regression results with aggregate controls: splitting the firm-bank matches

Note: The table reports estimates of new firm-bank matching that follows the closure of firm f's prior sin bank b, as implied by equation (3.2), with GDP growth rates (YoY) and concentration at regional credit markets (HHI) included as additional controls: $\lambda_j(t, \mathbf{X}_{f,t-1}; \Theta) = \lambda_0(t) \cdot \exp\Big(\alpha_j + \alpha_{j,bc} + \alpha_{j,r} + \alpha_{j,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_j + \mathbf{C}_{f,t-1}\Gamma_j + \delta_{j,1}GDP.growth_{t-1} + \delta_{j,2}HHI.credit_{r,t-1}\Big)$, where the dependent variable $\lambda_j(t, \mathbf{X}_{f,t-1}; \Theta)$ is an instantaneous rate at which firms exit, i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure—that is, by $t^*_{f,b}$—or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. The sample includes firms with a *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy in 2013M7 and till 2020M10. Coefficients are reported instead of subhazard ratios. Constant is included but not reported to preserve space.

	Switch to a sin bank			Switch to a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL$_{f,t^*}$	0.156***			–0.085**		
	(0.058)			(0.036)		
Profit$_{f,t^*} < 0$		–1.729*	–1.469*		0.021	0.182
		(0.893)	(0.882)		(0.245)	(0.246)
Profit$_{f,t^*+k} < 0$			–0.532*			–0.394***
			(0.298)			(0.150)
GDP.growth$_{t-1}$	0.141	–0.110	–0.107	–0.277***	–0.267***	–0.265***
	(0.230)	(0.200)	(0.198)	(0.062)	(0.071)	(0.071)
HHI.credit$_{r,t-1}$	1.187	–0.244	–0.245	4.900***	3.865***	3.935***
	(1.420)	(2.030)	(2.071)	(0.574)	(0.707)	(0.713)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N obs	255,152	177,121	177,046	255,643	177,507	177,432
N firm-bank new matches	200	168	168	715	537	537
N firms	6,034	4,178	4,175	6,045	4,183	4,180
log L	–1,065.0	–853.4	–851.5	–2,876.6	–2,149.4	–2,145.6

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

3.D Bank-firm matching model: multiple firm-bank relationships with at least one sin bank within

Table 3.1: Survival regression results with multiple firm-bank relationships: splitting the firm-bank matches

Note: The table reports estimates of new firm-bank matching that follows the closure of firm f's prior sin bank b, as implied by equation (3.2). Dependent variable $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms exit, i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure—that is, by $t^*_{f,b}$—or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. The sample includes those firms that have *multiple* bank relationships. The estimations are performed for the period starting with the active phase of the tight regulation policy in 2013M7 and till 2020M10. Coefficients are reported instead of subhazard ratios. Constant is included but not reported to preserve space.

	Switch to a sin bank			Switch to a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL$_{f,t^*}$	–0.013			–0.033		
	(0.068)			(0.039)		
Profit$_{f,t^*} < 0$		–0.816	–0.558		–0.771*	–0.576
		(0.727)	(0.759)		(0.428)	(0.423)
Profit$_{f,t^*+k} < 0$			–0.453			–0.344**
			(0.307)			(0.166)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N obs	235,231	160,843	160,802	235,562	161,124	161,082
N firm-bank new matches	171	142	142	502	423	422
N firms	5,368	3,671	3668	5,405	3,704	3,701
log L	–928.9	–722.1	–720.8	–2,259.4	–1,814.7	–1,808.1

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

3.E Bank-firm matching model: categorization of the loan quality in the closed banks

Table 3.1: Categorizing the days of non-performing loans: splitting the bank-firm matches

Note: The table reports estimates of new firm-bank matching that follows the closure of firm f's prior sin bank b, as implied by equation (3.2). Dependent variable $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms exit, i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by the log of days of NPLs accumulated in the closed sin bank before its closure—that is, by $t^*_{f,b}$. The days of NPLs were categorized into seven 30-day bins: $0 \leq DNPL_{f,t-1} \leq 30$ (*reference*), $30 < DNPL_{f,t-1} \leq 60$, ..., $DNPL_{f,t-1} > 180$. Other controls include firm size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. The sample includes firms with a *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy in 2013M7 and till 2020M10. Coefficients are reported instead of subhazard ratios. Constant is included but not reported to preserve space.

	Switch to a sin bank	Switch to a saint bank
	(1)	(2)
Bin 1: $0 < DNPL_{f,t-1} \leq 30$ (*reference)*		
Bin 2: $30 < DNPL_{f,t-1} \leq 60$	0.779***	–0.377*
	(0.302)	(0.212)
Bin 3: $60 < DNPL_{f,t-1} \leq 90$	1.425***	0.053
	(0.421)	(0.286)
Bin 4: $90 < DNPL_{f,t-1} \leq 120$	0.016	–0.616
	(0.991)	(0.502)
Bin 5: $120 < DNPL_{f,t-1} \leq 150$	0.193	–0.653**
	(0.483)	(0.287)
Bin 6: $150 < DNPL_{f,t-1} \leq 180$	–0.910	–17.399***
	(1.086)	(1.431)
Bin 7: $DNPL_{f,t-1} > 180$	–16.140***	–0.721
	(0.770)	(1.090)
N obs	257,190	257,681
N firms	6,069	6,080
N new firm-bank matches	200	715
log L	–1,060.1	–2,918.9

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

3.F Single bank owners database: an example

banki.ru Contributions Loans Cards Mortgage Insurance Investments Business news More

Home • Banks of Russia • Alfa-Bank

Альфа-Банк

Alfa Bank CHANGE BANK

license No. 1326

OGRN 1027700067328

ABOUT THE BANK | REVIEWS

Detailed background information, rating, services and offers.

ENG

Alfa-Bank JSC is one of the largest universal banks in Russia, owned by the Alfa-Group consortium. Alpha's positions are strong in almost all segments of the banking market. The key sources of the bank's funding are equally the funds of corporate clients and the population.

As of November 1, 2020, the bank's net assets amounted to RUB 4.6 trillion, and its own funds - RUB 0.6 trillion. In January-October 2020, the credit institution demonstrates a profit of 186.3 billion rubles.

Subdivision network:
head office (Moscow);
7 branches;
316 additional offices;
295 credit and cash offices;
174 operating offices;
4 operating cash desks outside the cash register.

Owners:
Mikhail Fridman - 32.86%;
German Khan - 20.97%;
Alexey Kuzmichev - 16.32%;
Petr Aven - 12.40%;
UniCredit SpA (Italy) - 9.90%;
Charitable Trust The Mark Foundation for Cancer Research (Cayman Islands) - 3.87%;
Andrey Kosogov - 3.67%.

100% of the bank through AB Holding JSC is controlled by ABH HOLDINGS SA (Luxembourg), the ultimate beneficiaries of which are the aforementioned co-owners of Alfa Group * and other persons.

Board of Directors: Petr Aven (Chairman), Marat Atnashev, Andrew Baxter, Vladimir Verkhoshinsky, Artem Leontiev, Andrey Kosogov, Alexey Marey, Oleg Sysuev, Mikhail Fridman, Oscar Hartmann, Alexander Galitsky, Sergey Matsotsky.

Management Board: Andrey Sokolov (Chairman), Vladimir Verkhoshinsky, Alexey Chukhlov, Mikhail Grishin, Michael Touch, Andrew Chulak, Denis Osin, Vladimir Voyekov, Ivan Pyatkov, Sergey Polyakov.

Альфа-Банк

Alfa Bank

license No. 1326

BANK PRODUCTS

Loan selection	3
Contributions	4
Mortgage	eight
Credit cards	eleven

FIRST PERSONS

Petr Aven

Chairman of the Board of Directors

All first persons

together with FINPARTY

RUSSIAN BANKS

Enter the name of the | TO FIND

3.G In-advance detection of sin banks

When developing a logit model of bank failures to capture bank fraud, we need to account for the following stylized facts. A large body of anecdotal evidence, as well as our consultations with the Central Bank of Russia, shows that gambling banks, having observed that the regulator switched to the tight regime in mid-2013, turned to permanently update their tools for balance sheet falsification (artificially raising the quality of their assets to lower loan loss provisions and keep the capital above the regulatory threshold).[35] the Central Bank of Russia itself was, and is, constantly learning these tools through the process of revoking sin banks' licenses. Thus, we need to account for updating of falsification schemes and the regulator's learning process in our logit models. In addition, our models have to accommodate not only standard bank failure determinants, as captured by CAMELS (see, e.g., DeYoung and Torna 2013), but also fraud-specific indicators.

We account for fraudulence updating and the regulator's learning processes by running a loop of logit regressions on a 6-month rolling window starting from 2010M6, i.e., three years before the regulator switched to the tight regime, to 2020M12, i.e., nearly three years after the announcement of the end of the active phase of the tight policy (see the description of the timing of the policy in Section 3.2).

As for fraud-specific indicators, after our consultations with the Central Bank of Russia, we choose (i) a variable that captures those situations in which a bank has higher-than-average loan loss reserves but lower-than-average NPLs of firms (both as % of the bank's total assets), (ii) a variable that captures the cases in which a bank has a large portion of assets in corresponding accounts of banks outside Russia (greater than 30%, for concreteness) and no operations with these funds, (iii) a variable that captures the cases when a bank predominantly attracts funds from households and lends them to non-financial firms rather than to households.

As for the variables within the CAMELS approach, we use (i) capital adequacy ratio (C), NPL ratios in the loans to firms and to households, loan loss reserves to total assets ratio, growth of total assets and its square (A), operating cost-to-income ratio (M), the annual return on total assets (E), the ratio of cash and government securities in total assets (L), net inter-bank exposure in the domestic banking system and net foreign assets abroad, both as % of total assets (S). We also include bank size to control for too-big-to-fail considerations.

We also incorporate macroeconomic controls to account for the state of the business cycle, cross-regional differences in bank competition, and distance from a bank headquarters to the center of Moscow to capture geographical differences across banks.

[35] See an early review of these falsification tools here: https://www.banki.ru/news/daytheme/?id=6609791 (In Russian; for English, one may use automated web-translation tools).

The 6-month rolling window logit estimates appear in Table 3.1.[36] The table contains a snapshot of results extracted for the following four sub-periods: before the tight policy, during the first months of the tight policy (2013M7), at the midpoint of the policy (2016M1), and around the end of the policy (2018M2). The dependent variable is a binary variable that equals 1 if a bank b was shut down at month t for fraud.[37] All explanatory variables are taken with a one-month lag.

The logit estimation results show that, depending on the sub-period, banks with greater capital, lower NPL ratios, higher returns, and greater net inter-bank loans were less likely to be closed for fraud. These are within the CAMELS approach. With our fraud-specific indicators, we find strong evidence that greater LLR together with lower NPLs is a significant predictor of fraud in the near future. Regarding regional controls, we find that banks operating in regions with higher regional bank concentration, as measured by the regional Herfindahl-Hirschman Index (HHI), are less likely to be closed for fraud. This can be viewed as reminiscent of the "market power–stability" concept (see, e.g., Keeley 1990). At the macro level, we find that banks are less likely to be closed for fraud during an expansionary phase of the business cycle. Overall, the results are in line with the broad literature on bank failures.

Regarding the in-sample quality of the estimated logit models, we compute two ROC curves—one for models with only CAMELS variables and the other for models in which we add our fraud-specific variables. The results are reported in Fig. 3.1. The area under the ROC curve equals 0.78 for the models with CAMELS and 0.88 for models with added fraud indicators. This indicates the high in-sample quality of the models and a high added value of the fraud indicators.

[36] We also tested the 12-month window and found no qualitative changes compared to the baseline.

[37] The data on fraud-related closures come from the Central Bank of Russia's official press releases from 2010 to 2020.

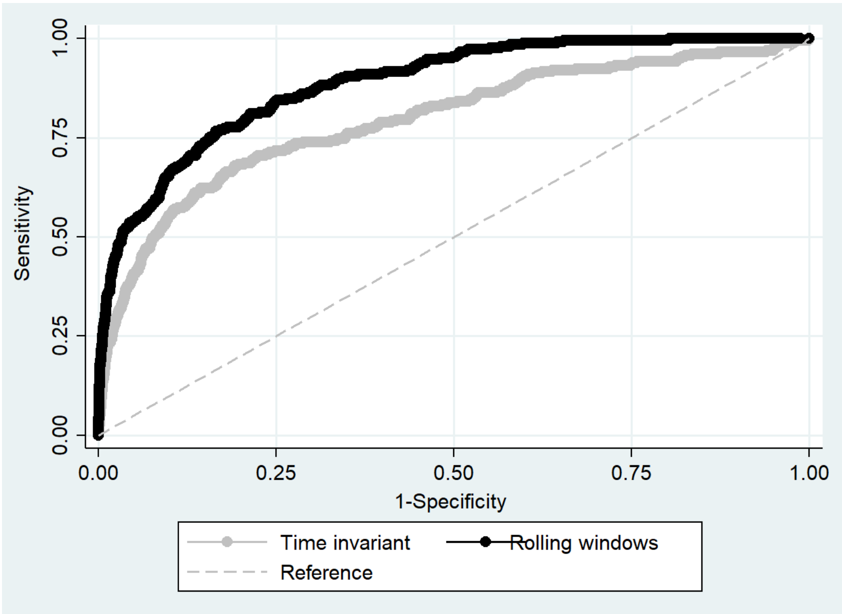
Table 3.1: Probability of sin banks detection and closure: logit regression results

Note: The table reports estimates of the following logit model: $Pr\left(Fraud.Detection_{b,t} = 1 \mid \mathbf{X}_{b,t-1}\right) = \Lambda\Big(\mathbf{X}'_{b,t-1}\Psi$, where the dependent variable $Pr\left(Fraud.Detection_{b,t} = 1 \mid \mathbf{X}_{b,t-1}\right)$ is a binary variable that equals 1 if an operating bank b is closed for fraud at month t, and 0 if the bank continues to operate. $\mathbf{X}_{b,t-1}$ includes capital adequacy ratio (CAR), the NPL ratios in the credit to households and credit to firms, return-to-assets (ROA), cash and reserves at the corresponding accounts at the Central Bank of Russia to total assets ratio (liquidity), growth of total assets (YoY) and its square, inter-bank loans minus inter-bank debts to total assets ratio, foreign assets minus foreign liabilities to total assets ratio, log of total assets, a censored variable equals loan loss reserves (LLR) if LLR exceeds median across all banks at a given month and equals 0 if else, the product of the censored variable and NPLs of firms, the distance of bank headquarters location to Moscow, regional credit market concentration (HHI), and GDP growth rates (YoY). The estimations are performed using 6-month rolling windows starting from 2010M1, i.e., before the active phase of the tight regulation policy began, and finishes at the end of the sample period in 2019M6. The constant term is not reported.

Period:	Before the policy	During the active phase of the policy		
		≤2013M7	≤2016M1	≤2018M2
	(1)	(2)	(3)	(4)
CAR	–0.003	0.003	–0.002	–0.021**
	(0.018)	(0.018)	(0.008)	(0.010)
NPLs households	–2.660	24.488***	–1.337	–4.167
	(11.869)	(8.027)	(6.085)	(4.414)
NPLs firms	5.943	–22.104	9.264	8.187**
	(4.146)	(104.406)	(7.044)	(3.382)
ROA	–7.664***	–35.742***	–8.069***	–10.415***
	(2.053)	(9.724)	(2.981)	(1.852)
Liquidity	–1.376	3.422	–1.375	–2.863*
	(1.681)	(5.235)	(1.475)	(1.490)
Growth of total assets	–0.946	–0.664	–1.053	–0.575
	(0.775)	(3.559)	(0.666)	(0.490)
Growth of total assets ^{2}	0.545*	0.448	0.467*	0.348*
	(0.295)	(1.311)	(0.252)	(0.185)
Net inter-bank loans	–3.342***	3.878	–3.632***	–3.852***
	(0.845)	(3.695)	(1.399)	(0.848)
Net Foreign assets	0.165	5.464**	1.040	0.038
	(1.077)	(2.402)	(1.124)	(0.865)
Bank size	–0.614**	–0.049	–0.416***	–0.525***
	(0.294)	(0.413)	(0.122)	(0.098)
LLR > 50%$tile$	7.367***	–3.977	5.654***	6.497***
	(1.781)	(7.210)	(1.393)	(0.910)
LLR > 50%$tile$ × NPLs firms	–22.147	–66.891	–63.950**	–53.920***
	(16.286)	(476.620)	(27.815)	(16.016)
Distance to Moscow	0.000	–0.000	–0.000	–0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Regional HHI		0.001	–0.000	–0.000*
		(0.000)	(0.000)	(0.000)
Annual GDP growth	0.083	–1.038	–0.158**	–0.143***
	(0.110)	(0.682)	(0.077)	(0.055)
N obs	37,889	1,550	19,568	31,836
R^2-pseudo	0.117	0.274	0.080	0.120

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Figure 3.1: The in-sample quality of logit models (Area under ROC-curves): CAMELS alone and with fraudulent indicators



3.H Did firms anticipate closures of their sin banks?

In this section, we examine whether firms anticipated the closure of their sin banks. We consider two possible endogenous adjustments by firms as evidence of such anticipation. First, we conjecture that firms could preemptively leave sin banks in anticipation of their closure. Second, we conjecture that firms, especially low-quality ones, could delay their loan repayments.

3.H.1 Preemptive switching

There are at least three reasons why firms could consider leaving their about-to-fail sin bank preemptively. First, a firm obtaining loans from an about-to-fail bank may decide to leave the bank preemptively to *signal* to other banks that it seeks long-term stable relationships with its lender(s). Second, if the firm does not switch to a different bank in advance, its payment obligation can be transferred to a new bank through an auction during the resolution process of the sin bank, in which case the firm has no control over which new bank this may be (Granja, Matvos, and Seru 2017). Third, the closure of the firm's bank can have a disruptive effect on a firm's day-to-day operations.

On the other hand, even if the firms obtaining loans from an about-to-fail sin bank wanted to preemptively switch to a new bank, they may actually do so. The empirical literature provides evidence of the existence of switching costs, which leads to a hold-up problem—a situation in which the firm stays with its current bank despite being able to obtain better conditions at another one.[38]

We are interested in whether firm quality determines preemptive switching to a new bank. One could expect that, because of higher outside options, better quality firms are less subject to the hold-up problem and, thus, are more likely to switch to a new bank preemptively. Likewise, lower-quality firms are likely to be more constrained by the hold-up problem and, thus, are less likely to leave the about-to-fail bank preemptively.

To find whether firm quality helps to explain preemptive switching, we examine if firms switch to a new bank within some time period h before the sin bank closure date. We define the indicator variable $Switch_{f,t}$ which equals 1 if firm f switches to a new bank during the time interval $[t^*_{f,b} - h, t^*_{f,b})$, where $t^*_{f,b}$ is sin bank b closure date, and zero otherwise.[39] We set $h = 6$—that is, we consider a time interval of 6 months to identify

[38] Ioannidou and Ongena (2010) provide evidence on the existence of switching costs. Bonfim, Nogueira, and Ongena (2020) show that switching costs are primarily due to information asymmetries. Liaudinskas and Grigaite (2021) also provide the estimates of switching cost and evidence on the hold-up problem.

[39] A firm may decide to switch in advance from an about-to-fail bank *occasionally*, because the firm's loan will mature at some time $\tilde{t} \in [t^*_{f,b} - h, t^*_{f,b})$ and the firm is simply not willing to continue with the same bank. Unfortunately, with information only on the days of NPLs at the loan lever and no access to either maturity or other relevant information until 2017—we cannot distinguish these cases from the

evidence of preemptive switching. We then estimate the following logit model:

$$\mathbf{Pr}(Switch_{f,t} = 1 \,|\, \mathbf{X}_{f,t-1}; \Theta) = \Lambda\Big(\alpha_{\jmath} + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_{\jmath} + \mathbf{C}_{f,t-1}\Gamma_{\jmath}\Big). \tag{3.7}$$

The estimation results are reported in Table 3.1. Our sample now consists of only about 30,000 firm-month observations, which is less than in the reference by a factor of 10. We have about 3,190 firms and the number of preemptive switches is about 1,950. Estimation of the preemptive switching regressions delivers no significant coefficients on the log $DNPL_{f,t^*-6}$ or $Profit_{f,t^*-6} < 0$ variables. This is true for switching to new (not-yet-detected) sin banks in columns 1 and 2, and switching to saint banks regressions in columns 3 and 4. The signs of the estimated coefficients are reversed compared to the baseline.

Regarding the other firm controls, we find that the coefficients on firm size and its square are insignificant, meaning that *larger* and *smaller* firms are not more likely to switch preemptively. The estimated coefficient on firm leverage is negative and significant in the case of in-advance switching to saint banks. Finally, liquidity negatively and significantly affects the likelihood of in-advance switching to sin banks.

Overall, the logit estimation results reveal that firms' preemptive switching from about-to-fail banks is not affected by firms' quality. One potential interpretation of this is the lack of evidence that firms could easily anticipate bank closures. That is, preemptive switching is more likely to take place for other common reasons (expiration/full repayment of loans, etc.). An alternative explanation is that firms could anticipate closures, but the hold-up problem was strong enough even for high-quality firms.

3.H.2 Strategic loan repayment delay

An alternative way to examine whether firms anticipate sin bank closures is to investigate loan repayments during the run-up to sin bank closure. Troubled firms that struggle more to meet their loan obligations may find it optimal to delay their payments if they anticipate that their bank is about to fail. In this case, they can be transferred to a new creditor, thus, opening up the possibility for debt restructuring.

We hypothesize that bad firms, as proxied with negative profits, may act strategically and thus raise loan delinquencies. We explore empirically if a change in loan repayment delay relates negatively to the firm's quality proxy during some time period before the

in-advance switching based on information leakages.

Table 3.1: Logit regression results: do firms switch to sin or saint banks in anticipation of their current sin bank closure?

Note: The table reports estimates of the logit model (3.7) of new firm-bank matching *prior* to the closure of firm f's current sin bank b, as an analog to the duration regression (3.2) that considers the matching *after* the sin bank closure. The dependent variable $\mathbf{Pr}(Switch_{f,t} = 1 \,|\, \mathbf{X}_{f,t-1}; \Theta)$ is the indicator variable which equals 1 if firm f switches to a new bank during the time interval $[t^*_{f,b} - h, t^*_{f,b})$, where $t^*_{f,b}$ is sin bank b closure date, and zero otherwise. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure—that is, by $t^*_{f,b}$—or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include a firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets, and liquidity-to-total assets ratios. The sample includes firms with a *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of marginal effects are reported. The constant term is included but not reported to preserve space.

	Match with a sin bank		Match with a saint bank	
	(1)	(2)	(3)	(4)
Panel 1: Firm quality:				
log DNPL$_{f,t^*-6}$	0.010		0.095	
	(0.080)		(0.131)	
Profit$_{f,t^*-6} < 0$		0.362		0.035
		(0.267)		(0.069)
Profit$_{f,t} < 0$		0.038		0.052
		(0.153)		(0.047)
Panel 2: Other controls:				
Firm size$_{f,t-1}$	0.406	0.521	0.013	0.074
	(0.417)	(0.431)	(0.134)	(0.145)
Firm size$^2_{f,t-1}$	–0.012	–0.015	0.000	–0.001
	(0.011)	(0.011)	(0.004)	(0.004)
Leverage$_{f,t-1}$	–0.264	–0.293	–0.252***	–0.228***
	(0.194)	(0.203)	(0.066)	(0.068)
Liquidity$_{f,t-1}$	–0.411**	–0.361**	–0.090	–0.049
	(0.166)	(0.172)	(0.059)	(0.061)
Bank closure event FEs	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
N obs	4,645	4,253	26,287	25,519
N firm-bank new matches	619	606	1,331	1,317
N firms	854	818	2,336	2,314
log L	–2,916	–2,676	–16,010	–15,557
R^2 (pseudo)	0.035	0.034	0.006	0.005

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

sin bank closure. Specifically, we estimate the following model:

$$\Delta DNPL_{f,b,t} = \alpha_f + \alpha_b + \alpha_{b,f} + \alpha_t + \alpha_r + \alpha_i + \alpha_{bc} + \beta \cdot \mathbf{1}\{Profit_{f,t^*-h} < 0\} + \varepsilon_{f,b,t}, \tag{3.8}$$

where $\Delta DNPL_{f,b,t}$ is a one-month change in the days of NPLs reported by a firm f that has a relationship with (not-yet-detected) sin bank b at month $t \in [t^*-h, t^*)$, $h = 12, 9, 6, 3$ months prior to the bank b closure. α_f, α_b, $\alpha_{b,f}$, α_t, α_r, α_i, α_{bc} are respectively FEs for firm, bank, firm*bank (relationship), month, region, industry, and bank closure events.

We aim to capture the effect of $Profit_{f,t^*-h} < 0$ on $\Delta DNPL_{f,b,t}$ that works *beyond* those stemming from intrinsic features of the firm's and bank's business models, the $bank \times firm$ relationships, aggregate shocks affecting the economy of the whole country or its particular regions, industry-specific shocks that may force even a profitable firm to delay repayment on loans, and the cascade of bank closures witnessed in the active phase of the tight policy.

The estimation results of regression (3.8) are presented in Panel 1 of Table 3.2. We do not find any statistical evidence that a firm's quality relates to the delay of loan repayments—that is, the estimated coefficient on $Profit_{f,t^*-h} < 0$ is insignificant at any considered horizon h prior to the bank closures. Thus, we do not find evidence that bad firms increase their loan delinquencies before the closures of their sin banks.

Table 3.2: Panel estimation results: do bad firms increase delays in repaying loans before their banks are closed?

Note: The table reports estimates of 1-month changes in the days of NPLs prior to sin bank closure, as implied by equation (3.8), where the dependent variable $\Delta DNPL_{f,b,t}$ is a one-month change in the days of NPLs a firm f has in bank b at month t. The estimations are performed in a window of h months before a sin bank closure, i.e., $t \in [t^*_{f,b} - h, t^*_{f,b})$, where $t^*_{f,b}$ is firm-specific date of ending a relationship with the firm f's current sin bank b and h is set at 6 months. $Profit_{t^*-h}$ is the binary variable of whether the firm had negative profits at $t^*_{f,b} - h$. *Single "firm–sin bank"* indicates those cases in which a firm has a relationship only with one bank and this bank is a sin bank. *Multiple "firm–(sin) bank"* indicates cases in which a firm has relationships with more than one bank and (at least) one of these banks is a sin bank. All regressions include bank, firm, firm*month, month, regional, industry, and bank closure events fixed effects.

Months h before sin bank closure:	$h = 12$	$h = 9$	$h = 6$	$h = 3$
	(1)	(2)	(3)	(4)
Panel 1: single "firm–sin bank" relationship (*baseline*)				
$\text{Profit}_{f,t^*-h} < 0$	1.063	0.761	1.197	–0.711
	(0.720)	(0.949)	(1.174)	(1.565)
N obs	78,645	62,519	44,749	24,768
R^2 (within)	0.091	0.111	0.143	0.255
Panel 2: multiple "firm–(sin) bank" relationship				
$\text{Profit}_{f,t^*-h} < 0$	0.219	0.414	0.937	0.379
	(0.450)	(0.786)	(0.591)	(0.500)
$\text{Sin.Bank}_b \times \text{Profit}_{f,t^*-h} < 0$	0.111	–0.644	–1.165	–1.589
	(0.791)	(1.200)	(1.367)	(1.955)
N obs	213,229	163,688	111,957	60,140
R^2 (within)	0.081	0.100	0.135	0.232

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

Panel 2 of Table 3.2, further presents the estimation results of equation (3.8) when we allow for *multiple* firm-bank relationships.[40] The results are qualitatively similar in that we do not find evidence that firm quality affects loan delinquencies before the closures of their sin banks.

Overall, our results show little evidence that firms anticipated sin bank closures. The firms neither left their sin banks preemptively nor did they engage in strategic loan repayments delay.

[40] For this purpose, equation (3.8) is modified so that a firm may have relationships with at least one (not-yet-detected) sin bank and at least one saint bank simultaneously. In this case, the variable of interest is not only $Profit_{f,t^*-h} < 0$, but also its product with the sin bank indicator variable, $Sin.Bank_b$, which is equal to 1 if a bank ever fails due to fraud, and 0 if survives till the end of the sample. For strategic reasons, firms are likely to hold the worst part of their debts in sin banks and serve their best-quality debts in saint banks. If firms anticipate sin bank closures, then bad firms could start to increase loan delinquencies in the sin banks rather than saint ones.

3.I Firm performance: additional results

Table 3.1: Difference-in-differences estimation results: firm performance after sin bank closures and before establishing new firm-bank relationships

Note: The table reports estimates of firm performance after firms experience closures of their prior sin banks and before they match with new banks, as implied by equation (3.4). Firm performance is proxied with the following dependent variables $Y_{f,t}$: firm size, as captured by the log of total assets (*log(TA)*, column 1), log of borrowed funds (*log(Borrow)*, column 2), log of total revenue (*log(Revenue)*, column 3), log of number of workers (*log(Employ)*, column 4), log of profit after taxes (*log(Profit)*, column 5). $Sin.Bank_{b,f} = 1$ if bank b that has a relationship with firm f ever fails for fraud, and 0 if it survives till the end of the sample. $POST_{\{t \geq t^*_{b,f}\}} = 1$ if $t \geq t^*_{b,f}$, and 0 if else. $Bad.Firm_{f,t}$ is a binary variable that equals 1 for firms with losses, and 0 for profitable firms. The estimations are performed for $t \in [2011, 2020]$ on a panel of matched firms that experienced sin bank closures during the sample period and only had single bank relationships at $t^*_{b,f}$, and the panel is restricted so that it includes the observations in only up to two years before and after $t^*_{b,f}$, i.e., firm-bank-time specific windows $[t^*_{b,f} - 2, t^*_{b,f} + 2]$ years. 1:4 nearest neighborhood matching of firms is performed prior to $t^*_{b,f}$ using the five observables: firm size, leverage, liquidity, annual growth of total assets, and profitability. All regressions contain all necessary sub-products of the triple interaction variable $Sin.Bank_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$, firm and year fixed effects, and the set of firm controls to capture any residual differences across treated and control firms after 1:4 matching (firm size, except (1); leverage, except (2); and liquidity). The sample includes those firms that have a *single* bank relationship.

$Y_{f,t}$:=	log(TA)	log(Borrow)	log(Revenue)	log(Employ)	log(Profit)
	(1)	(2)	(3)	(4)	(5)
Panel 1: Focus variables:					
$\text{Sin.Bank}_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}}$	0.205***	0.164***	0.342***	0.158**	0.267***
	(0.043)	(0.058)	(0.063)	(0.069)	(0.080)
$\text{Sin.Bank}_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times$	–0.320**	–0.259**	–0.028	0.307	n/a
$\times$ $\text{Bad.Firm}_{f,t}$	(0.136)	(0.121)	(0.168)	(0.197)	
Panel 2: Key components of the triple interaction variable:					
$\text{Sin.Bank}_{b,f}$	–0.091**	–0.045	–0.129***	–0.120**	–0.093
	(0.040)	(0.052)	(0.045)	(0.060)	(0.059)
$\text{POST}_{\{t \geq t^*_{b,f}\}}$	0.082**	0.122**	–0.072	–0.131*	0.006
	(0.037)	(0.054)	(0.068)	(0.068)	(0.082)
$\text{Bad.Firm}_{f,t}$	–0.008	0.099**	–0.390***	–0.041	n/a
	(0.029)	(0.043)	(0.081)	(0.056)	
N obs	17,174	17,065	16,344	10,336	13,016
N firms	3,226	3,225	3,190	2,647	2,932
R^2 (pseudo / LSDV)	0.3	0.2	0.2	0.3	0.1

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the firm level and appear in the brackets under the estimated coefficients.

Chapter 4

Measuring fraud in banking and its impact on the economy: a quasi-natural experiment

Published as CERGE-EI Working Paper Series No 755

4.1 Introduction

Central banks are typically perceived as planners that can prevent financial crises by setting proper bank regulation, thus avoiding associated welfare losses. However, in practice, central banks usually do not achieve this ideal picture due to a myriad of confounders, including (*i*) uncertainty regarding the banks' assets choices, which undermines planners' ability to recognize problem banks (Boot and Thakor 1993), (*ii*) reputational issues when problem banks are detected and must be closed (Morrison and White 2013), (*iii*) a lack of commitment to optimal policy per se (Acharya and Yorulmazer 2007), and (*iv*) inconsistency in bank closure decisions at different levels of regulation (Agarwal et al. 2014). These issues cause not only the "too big to fail" problem (O'Hara and Shaw 1990) but also lead to *regulatory forbearance* in bank closure decisions. Regulatory forbearance is shown to be pervasive both in developed countries (Wheelock and Wilson 2000; Kang, Lowery, and Wardlaw 2015) and in emerging economies (Brown and Dinc 2011). Though regulatory forbearance can be optimal in specific situations (Morrison and White 2013;

Kang, Lowery, and Wardlaw 2015), it can also be costly for society (Cole and White 2017). Regulatory forbearance adds to banks' incentives to misreport losses when they face negative shocks to their assets, which results in greater fraud in banking (James 1991; Nagel and Purnanandam 2019). In this chapter, I suggest a novel approach to measure bank fraud and its implications for the real economy. I explore a unique example of what can happen to privately-held operating banks when regulatory forbearance suddenly disappears.

The example comes from the Russian banking system in the 2010s, when substantial organizational changes were made in the structure and responsibility of its key regulatory authority, the Central Bank of Russia (CBR). In mid-2013, a new head of the CBR, Elvira Nabiullina, launched an aggressive policy of fraud detection and license revocation to deal with a large body of asset falsifications inherited from the past.[1] This policy resulted in forced bank closures of more than 600 of 950 privately-held financial institutions during the following six years.[2] Before 2013, the number of banks had also been permanently decreasing, but at a much lower rate, and due to more market-based reasons (losing market shares during the global recession of 2007–2009, the exit of a number of foreign banks, and others) than to changes in bank regulation. Conversely, starting from exactly mid-2013, the rate of license revocation increased dramatically and remained very high throughout the next six years.[3]

[1]The myopia of the CBR before 2013 has roots in 2006, when the first deputy chairman of the CBR, Andrei Kozlov, was murdered after he revealed and blocked an illegal withdrawal of funds from Russia by a coalition of domestic and foreign subsidiary banks, see https://www.theguardian.com/business/2006/sep/14/russia.internationalnews. This episode provoked a large depressive effect on the subsequent quality of prudential regulation in Russia and stimulated not only further expansion of illegal activities, but also less stringent credit risk management by Russian banks and more bank misreporting.

[2]In 2015, *Euromoney* awarded Nabiullina a top central banker award, after the Reserve Bank of India's governor Raghuram Rajan was awarded one year before; see https://www.forbes.com/sites/kenrapoza/2015/09/16/and-the-worlds-best-central-banker-is-not-yellin/#4223182550d3. In 2016, Nabiullina received another award, by *The Banker*. See also an overview of the Nabiullina's license revocation policy by *Bloomberg* via https://www.bloomberg.com/news/features/2017-02-14/putin-s-central-banker-purges-100-banks-a-year-in-epic-crackdown.

[3]Note that this decreasing trend materialized at least six months before the Russian economy entered the (local) recession of 2014–2015, and at least three quarters before the first wave of financial sanctions against Russian banks were imposed (in March 2014); see Ahn and Ludema (2020) and Mamonov, Pestova, and Ongena (2021).

During the period of 2013-2018, in addition to regular on-site inspections of each bank every two years, the CBR was conducting *unscheduled* in-site inspections of suspicious banks and reporting the size of losses discovered in the closed banks' assets, i.e., *hidden negative capital* (HNC).[4] According to the CBR official press releases, in the majority of cases, banks were closed for illegal activities (e.g., money laundering) and excessive risk-taking that resulted in large-scale asset losses that were artificially hidden by means of balance sheet falsifications.[5] Importantly, the CBR was not publicly disclosing its upcoming targets after discovering and closing fraudulent banks: uncertainty remained as to which banks can be inspected next, and when this could happen. This environment provides a rich, unique laboratory to test the effects of declining regulatory forbearance on the operations of and risk perception by "gambling" commercial banks.

More formally, my research questions are as follows. First, what is the *empirical rule* according to which the central bank distinguishes those banks engaged in misreporting from those that report the state of their balance sheets truthfully?[6] Put differently, I suggest an empirical approach to capturing which banks are likely to be *suddenly* inspected by a regulator. Second, do likely-to-be-inspected banks increase or decrease their equity capital, and do they shrink their liabilities and assets after the empirical rule signals that they are in the red zone? Specifically, I am interested in whether such banks reduce their deposits from households and non-financial firms and whether they decrease lending to the economy. I refer to these as *scale effects* of tightened regulation. Third, what are the *composition effects* of tightened regulation, i.e., whether likely-to-be-inspected banks change the structure of their balance sheets towards specific type(s) of liabilities and assets (more or less prone to falsification and opaqueness, in the spirit of Song and Thakor

[4]For convenience reasons, and because the banks were forcibly closed due to (eventually) revealed misreporting, I refer to this measure of losses as "hidden negative capital".

[5]Typically, after facing negative idiosyncratic shocks to their assets, the banks turned to falsify the actual quality of these assets to prevent accruing additional loss reserves, so that the falsified capital adequacy ratios still satisfy official requirements. Before 2016, the regulatory threshold on the minimal capital to risk-weighted assets equaled 10%, and after 2016—8%, plus a counter-cyclical component depending on the state of the business cycle (as a part of the Basel III recommendations).

[6]In this respect, I act similarly to empirical macroeconomists who proxy for monetary policy rule with actual data on inflation and GDP.

2007)? Fourth, what happens to the prices these banks set for their services? Finally, what are the macroeconomic implications of tightened prudential regulation? If the identified misreporting banks reduce their credit supply to the economy, how large could it be economically?

The first challenge I face is how to identify misreporting banks that are likely to be inspected by the regulator in an unscheduled mode. Note that these are not-yet-failed credit institutions—they continue their operations until they either recover their financial health (by drawing a positive idiosyncratic shock) or they are detected by the regulator. Thus, standard logit/probit analysis applied in the literature on bank failures is not appropriate here. One option is to compute some balance sheet characteristics that reflect bank risk exposure, rank the banks, and identify those at the bottom of the list, as is done, e.g., in DeYoung and Torna (2013). Though I also follow this direction, I argue that there is a more appropriate alternative. Specifically, if a regulator publishes official reports on the reasons for closing failed banks, one can extract the necessary information from these reports. As noted, the CBR publishes detailed reports, from which I obtain all cases of bank misreporting—fair evaluation of (remaining) assets, and the actual size of HNC.[7] I thus can keep track of not only whether a bank was closed for misreporting or not (*extensive margin*), but also the size of the losses on the closed banks' assets (*intensive margin*). The press releases containing these data, "*Vestniki Banka Rossii*", are irregular, and I manually collect them from 2007 till the end of 2019, case by case. I construct a binary indicator that equals one if a bank appears in press releases as closed for misreporting, and I use the difference between remaining assets and liabilities as a measure of losses associated with misreporting. For the rest, I rely on the monthly balance sheets and quarterly profit and loss accounts of Russian banks disclosed publicly through the CBR official website from January 2004 till February 2022, when the data was closed due to Russia's invasion of Ukraine.

[7]Typically, the CBR inspection committee works for 1-2 months evaluating the real quality of assets reported by the banks on the eve of license revocation. Further, all the necessary asset loss provisions are accrued, and the remaining equity capital (usually negative) is reported as the difference between remaining assets and liabilities.

I first identify likely-to-be-inspected banks among those not yet detected by the CBR using *the Heckman selection approach* (Heckman 1979), which encompasses both the binary indicator of misreporting bank closure and the size of HNC in a tractable way.[8] I know which banks were already forcibly closed for misreporting, and I use this information to estimate (i) the probability that a given operating bank is likely to be inspected in the next quarter for misreporting and (ii) the size of HNC conditional on misreporting being detected by the CBR.

When identifying misreporting banks, the idea is that a researcher does not know *how* a regulator makes decisions on whether to audit a suspicious bank or not, and thus she is agnostic regarding *which part* of the banking system the regulator inspects each and every period.[9] To formalize this idea, I assume the regulator inspects a bank if the predicted probability of misreporting reaches the red zone. For convenience, I assume that the threshold between green and red zones is the median value across all banks in the respective period (quarter). I also modify this assumption in several directions and discuss it in the robustness section.

Further, the researcher may also be agnostic about *the degree of regulatory suspicion*, i.e., for how long a bank with misreporting detected at a given date is treated by the regulator as continuing its misreporting practices afterward. It is natural to assume that under declining regulatory forbearance, once detected, a bank could operate under the watchful eye of the regulator for longer than just one quarter. In my regression analysis, I nonetheless start with one quarter, then proceed with four quarters, and finally assume that a suspicious bank remains under the regulator's control forever. Therefore, I construct various versions of the treatment group by assuming that different parts of the banking system will be inspected, and by changing the presumed degree of regulatory suspicion. Technically, from the standpoint of a standard difference-in-differences imple-

[8]I am not the first to exploit this technique in banking studies. Jiménez et al. (2014) also apply the Heckman selection approach when analyzing which loan applications were approved and which were rejected, and how an otherwise standard bank lending channel of monetary policy works when it is conditioned on approved loan applications.

[9]As I mention above in the case of Russia, the CBR does not disclose this information.

mentation, it is important that the treated objects remain in the treated group during the whole estimation window. In my case, this holds if I assume suspicious banks remain under the regulator's control forever. However, this does not hold in the other two cases. However, I show that the results are qualitatively the same across all these cases—though they are stronger for the 'forever' assumption.

The control group includes all not-treated banks in my baseline estimates, i.e., the banks in the green zone. In additional estimates, I reduce the control group by using the bias-adjusted matching estimator of Abadie and Imbens (2011) to find the nearest neighbors to treated banks. For this purpose, I employ certain bank-specific characteristics (asset size, structure of assets and liabilities, quality of assets, profitability, etc.), as suggested by Gropp et al. (2018).

Given the estimated nature of my constructed treatment and control groups, I next follow a "fuzzy" difference-in-differences approach (de Chaisemartin and D'Haultfoeuille 2017) to estimate whether the tightened prudential regulation shrank the size of treated banks after mid-2013 and forced them to adjust their assets and liabilities, compared to control banks. I then analyze the role of aggregate banking sector concentration (Boyd and De Nicolo 2005) and cross-sectional variation in bank risk-taking (Laeven and Levine 2009), as proxied by non-performing loan (NPL) and bank equity capital ratios, in propagating the effects of tightened regulation. Finally, I aggregate the microeconomic estimates to the macroeconomic level. I estimate the elasticity of GDP with respect to loan volumes during periods of loan supply shocks using a VAR model of the Russian economy with the sign restrictions scheme developed by Gambetti and Musso (2017) and the narrative sign restrictions approach of Antolin-Diaz and Rubio-Ramirez (2018).

In a nutshell, my results indicate that the CBR policy was efficient in restricting the scope and structure of activities of treated banks in the 2010s, i.e., before the war against Ukraine in 2022, on both intensive and extensive margins. My estimates suggest that in one quarter after the predicted probability of unscheduled in-site inspection hits the red zone, the treated banks reduced loans to households by 3.9 billion rubles and to non-financial firms by another 3.0 billion rubles, on average.[10] These are the estimated amounts of credit that could have been granted to borrowers if the banks continued to overstate their creditworthiness after 2013. This shows that tightened regulation can have considerable scale effects, echoing the result obtained by Kupiec, Lee, and Rosenfeld (2017), who show that lower ratings assigned by regulators to weak banks led to a significant decline in these banks' lending to the economy. At the same time, treated banks raised the share of (expensive) household deposits by 2.3 p.p. of their total liabilities and increased the share of (cheaper) firm credit by the same amount. In other words, they became more dependent on the fully insured funds and more specialized on informationally opaque assets, thus engaging in a greater asset-liability mismatch (Song and Thakor 2007)—an unintended effect of the CBR policy. This also proves that tightened bank regulation can entail unintended composition effects. I then show that the banking sector concentration, which was rising in the 2010s due to the growing share of state-owned banks, was amplifying the scale effects of the tightened regulation. My cross-sectional estimates also show that the scale effects were larger for the banks with larger NPLs and lower equity capital. From the VAR analysis, I infer that the policy-induced reduction of credit to households and to non-financial firms by the banks in the red zone could entail a decrease in GDP by 4.1% and 3.2%, respectively. These are the estimated macroeconomic effects of the policy-induced negative credit supply shock, which are clearly large.

My results survive a battery of robustness checks, including variations of the regulation rule and the degree of regulatory suspicion, applying the bias-adjusted matching estimator of Abadie and Imbens (2011) to construct a matched sample of treated and control banks, modifications to the composition of the Heckman selection model (Lennox, Francis, and Wang 2012), and applying a popular measure of a bank in distress (Z-score) to constructing the treatment group, as, e.g., in DeYoung and Torna (2013).

This chapter contributes to several strands of the literature. First, I suggest an empirical approach to capture a prudential regulation rule setting unscheduled on-site

[10] For comparison reasons, these are equivalent to 79 and 61 million US dollars, respectively (applying the average US dollar-to-ruble exchange rate for 2014–2016).

inspections of potentially fraudulent banks. My approach is based on a combination of the Heckman selection model and fuzzy difference-in-differences. It is applicable to many banking systems that are subject to bank fraud, and it requires only standard bank balance sheet characteristics rather than proprietary loan-level data (Blattner, Farinha, and Rebelo 2023). The approach complements traditional ways of measuring bank risk exposures usually applied in the literature—Z-score of the distance to default (Beck, De Jonghe, and Schepens 2013) and logit/probit-based probabilities of default that exploit CAMELS indicators (DeYoung and Torna 2013). As stated by Nagel and Purnanandam (2019), "*solvency problems may not be immediately apparent when bad shocks are realized. Deterioration in asset values may be hidden for a while, perhaps facilitated by regulatory forbearance, and short-term debt may be rolled over even if the bank is actually insolvent.*" My approach, distinct from Z-scores and predicted probabilities of default, is able to capture hidden deterioration in asset values.

Second, I add to studies on regulatory forbearance (Acharya and Yorulmazer 2007; Brown and Dinc 2011; Morrison and White 2013; Kang, Lowery, and Wardlaw 2015). The Russian banking system provides an empirical example of the theory of optimal regulatory forbearance developed by Morrison and White (2013). Although as many as 600 of 950 banks were closed by the CBR within six years after the appointment of a new head in mid-2013, there were no systemic episodes of contagious runs on other (healthy) banks, which could have potentially been initiated by banks' creditors because of the overall loss of trust. The reputation of the CBR after detecting and closing misreporting banks was not diminished. Self-interested regulation (Boot and Thakor 1993) seems also not to have played a role. As can be inferred from the figures, the "too many to fail" effect (Acharya and Yorulmazer 2007; Brown and Dinc 2011) was absent in the Russian banking system. To make things even more complicated, the "too big to fail" effect (O'Hara and Shaw 1990) was also rather limited, because the CBR refused to forbear losses of a bank from the top-30 in terms of assets (Bank Trust) and revoked its license after discovering that the bank had hidden negative capital.[11]

Third, my results contribute to the literature on relationship lending and asset-liability mismatch (Song and Thakor 2007) by showing that misreporting banks tend to increase the relative weights of less monitored funding (from the liability side) and more informationally opaque lending (from the asset side).

Finally, given Russia's war against Ukraine in 2022, it is important to understand the strength of the Russian banking system. This is a key sector that transmits financial sanctions to the rest of the economy (Mamonov, Pestova, and Ongena 2021). My results indicate that due to the CBR policy, the banking sector became stronger than before in terms of the degree of fraud but, at the same time, it also became more state-oriented. Lower fraud can reduce the overall impact of sanctions due to more trust from local investors, whereas larger government ownership can increase the impact of sanctions through capital misallocation (Nigmatulina 2022).

The remainder of the chapter is organized as follows. Section 4.2 presents the empirical design of the chapter. In Section 4.3, I describe the bank-level data. Section 4.4 then presents the baseline estimation results. I perform sensitivity analysis in Section 4.5, and Section 4.6 concludes.

4.2 Empirical design and hypotheses

I first discuss how I suggest proxy the regulatory rule to inspect a suspicious bank with the Heckman selection approach. I then move to describe the construction of the treatment group, i.e., the banks that are suspected by the CBR of misreporting, and the control group. With these two groups, I further introduce the baseline difference-in-differences (DID) specification, in which I test the scale and composition effects of tightened regula-

[11]The "too big to fail" effect appeared in 2017 when the CBR detected hidden negative capital in three banks from the top-10 or top-20 in terms of assets (Binbank, Promsvyazbank, and Otkrytie, the so-called banks of the "*Moscow Gold Ring*"), and initiated their resolution through the Banking sector consolidation fund rather than closing them; see https://www.rbc.ru/finances/02/07/2019/5d1b858c9a7947ed0ee3c54f.

tion using an estimation window of ±3 years around the regulatory change in mid-2013. I then introduce the channels of tightened regulation transmission to the bank balance sheets. Finally, I describe a VAR analysis suitable for uncovering the macroeconomic implications of tightened bank regulation.

4.2.1 Identifying fraudulent banks: the Heckman selection approach

I do not know the rule that the Central Bank of Russia (the CBR) uses to determine suspicious banks. However, I can assume that the CBR predicts the financial conditions of banks using certain econometric techniques and the banks' balance sheets. Since the CBR faces a large body of bank misreporting, in which the banks falsified the actual level of the funds they own (capital), I need to account for this phenomenon when attempting to mimic the CBR rule. Thus, as a baseline technique, I apply the Heckman selection model (Heckman 1979), which allows me, all else being equal, to predict (i) whether a given bank is practicing misreporting (*extensive margin*) and (ii) the size of any hidden negative capital (HNC) conditional on misreporting (*intensive margin*). In the robustness checks, I switch to a simpler alternative and compute the rankings of bank soundness using the Z-score of banking stability, as in DeYoung and Torna (2013). Thus, my baseline specification is comprised of a selection equation, determining a bank's state (misreporting or not), and an outcome equation, defining the size of HNC conditional on the bank's state:

$$\mathrm{s}_{it} = \mathbb{1}\left(\mathrm{HNC}_{it} = a_1 + \sum_{j=1}^{M} c_{1,j}\mathrm{BSF}_{j,it-k} + \overline{\psi}\mathrm{Size}_{it-k} + \varepsilon_{1,it} > 0\right), \tag{4.1}$$

$$\mathrm{HNC}_{it} = a_2 + \sum_{j=1}^{5} c_{2,j}\mathrm{BSF}_{j,it-k} + \gamma\lambda\left(a_1 + \sum_{j=1}^{M} c_{1,j}\mathrm{BSF}_{j,it-k} + \overline{\psi}\mathrm{Size}_{it-k}\right) + \varepsilon_{2,it}. \tag{4.2}$$

where s_{it} is a respective binary indicator and HNC_{it} is the conditional size of hidden negative capital (as % of bank total assets) of bank i at time t. $\mathrm{BSF}_{j,it-k}$ is a j^{th} bank-specific control variable ($j = 1...M$) stemming from the literature on bank failures and reflecting bank asset structure, liability structure, quality of assets, growth of assets, inter-bank linkages, etc. (see discussion of details in Section 4.3). I consider one-quarter lag $k = 1$ in my baseline estimations. Further, Size_{it-k} is the log of bank total assets. As is well-known, the selection equation must contain at least one variable identifying selection and not affecting the outcome. As shown by Lennox, Francis, and Wang (2012), empirical literature applying the Heckman selection approach most commonly uses the size variable for this purpose. Finally, $\lambda(\cdot)$ is the *Heckman's lambda* (the ratio of c.d.f. to p.d.f. at the respective point) aimed to capture the selection bias, and $\varepsilon_{1,it}$ and $\varepsilon_{2,it}$ are the selection and outcome regression errors.

There are several alternatives for the choice of the variable that may identify a bank's selection into the treated group. First, the *bank size* variable: below I show statistically that this characteristic is highly negatively correlated with the selection and exhibits no correlation with the *relative* HNC size (recall that I normalize HNC with bank total assets). However, choosing bank size would imply that large banks are less likely to be selected, i.e., inspected by the regulator, than small banks, which is arguable. Second, an indicator variable of whether a bank has *negative profit* at date t. If the bank faces losses, it has to reclassify its assets and accrue additional loss reserves; however, the latter could threaten the bank by pulling its capital to risk-weighted assets below the regulatory threshold. If a bank expects that its continuation value *in* the banking sector will be larger than the *outside* option, the bank is likely to opt to misreport. Thus, to be in line with the literature, I follow the size variable in my baseline estimates and then, in robustness checks, switch to the indicator of negative profits.

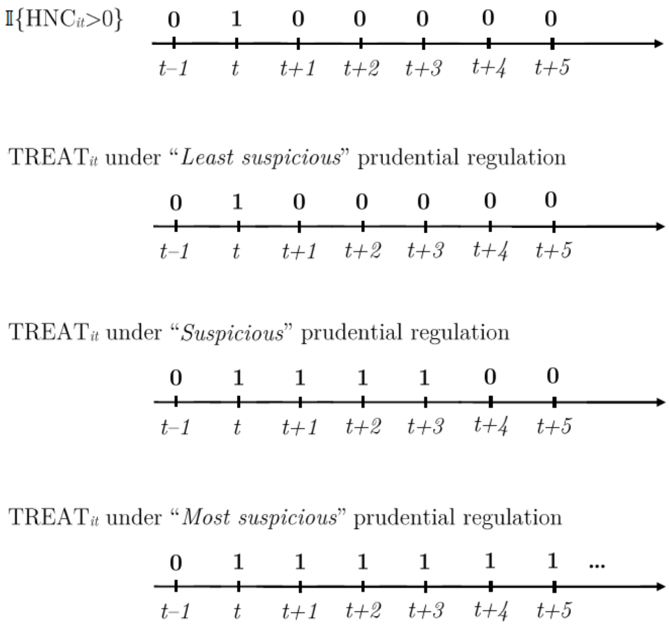
I estimate equations (4.1)–(4.2) for each date $t = 1...T$ separately to account for changing the regulatory framework. I perform the estimates with Heckman's two-step efficient estimator. I next compute the fitted values of the two respective dependent variables at each date t and obtain their time-specific distributions across banks in the sample. The costly state verification problem (Townsend 1979) applies to central banks, and it is unlikely that the CBR inspects all the banks in the system in every period. In the baseline estimates, I am agnostic about which part is inspected. Thus, for each t, I compute the median value of the fitted selection variable. I assume that this is the borderline $\hat{s}^*_t$, above which the CBR treats the banks as misreporting and applies tightened regulation. In robustness checks, I change the borderline from the median to the 25^{th} and then to the 75^{th} percentiles to check whether my results hold if I assume a *more tolerant* or *less tolerant* regulator. Regarding the HNC fitted values, I assume that, if $\hat{s}_{it} > \hat{s}^*_t$, $\widehat{HNC}_{it} > 0$ (*misreporting banks*); if else, then the CBR pays no attention to the estimated size of HNC (*non-misreporting bank*).

Overall, I approximate the regulatory rule of detecting misreporting banks with the Heckman selection model (4.1)–(4.2). I next make additional assumptions regarding the dynamic nature of how the CBR treats banks it has discovered to be misreporting.

4.2.2 Regulatory tightening and the operations of fraudulent banks: a difference-in-differences approach

I am agnostic about how the CBR treats detected banks and thus consider several options (Fig. 4.1). Suppose that a bank i was detected as misreporting at date t, i.e., $\mathbb{1}\big(\text{HNC}_{it} > 0\big) = 1$, but the formal rule indicated that the bank had recovered in the subsequent periods, i.e., $\mathbb{1}\big(\text{HNC}_{it+k} > 0\big) = 0$ for any $k = 1...T$.

The first (baseline) option is that, having detected a fraudulent bank i at time t (by the rule $\hat{s}_{it} \geq \hat{s}^*_t$), the CBR tightens the regulation of all such banks by setting an appropriate range of activity restrictions at t and removing these restrictions as soon as the rule shows the banks are no longer misreporting. For instance, if, in the following period $t+1$ the bank i has improved its financial health—which is reflected in $\hat{s}_{it+1} < \hat{s}^*_{t+1}$—the CBR does not treat this bank as misreporting any longer. I refer to this scenario as "*the least suspicious*" regulation (the CBR fully trusts the rule). I can formalize the construction



Note: HNC is hidden negative capital. I assume three types of prudential regulation that the Central Bank of Russia (the CBR) could follow after the change of its head in mid-2013. Mid-2013 is a borderline that has marked a switch from an HNC-tolerant to an HNC-intolerant regime of prudential regulation. Fraudulent banks, when they are detected by the CBR, are subject to various forms of activity restrictions (e.g., a ban on attracting new deposits, a ban on granting new loans, etc.). The assumed three types of regulation deal with the time span of activity restrictions applied to fraudulent banks. First, by "*Least suspicious*" regulation I assume that the CBR allows fast recovery of fraudulent banks, i.e., that a bank detected by the CBR as fraudulent at t is able to fully recover at $t+1$ (able to switch from the treatment to control group). Second, by "*Suspicious*" regulation I assume that having detected a fraudulent bank at t, the CBR still believes the bank is misreporting until $t+4$ even if the rule $\mathbb{1}\{HNC_{it+1} > 0\}$ shows the bank has recovered from $t+1$ on. This partially accounts for the possibility of fraudulent banks' misreporting in the future. Third, by "*Most suspicious*" regulation I assume the CBR believes that a misreporting bank never recovers and, once detected, must always be treated (until the bank fails). This fully accounts for the possibility of continuing misreporting in the future.

Figure 4.1: Assumed types of differential prudential regulation

of the treatment group in this scenario as follows:

$$TREAT_{it}^{(1)} = \begin{cases} 1, & \text{if } \widehat{HNC}_{it} > 0 \\ 0, & \text{if } \widehat{HNC}_{it} = 0 \end{cases} \tag{4.3}$$

However, I cannot exclude that the CBR may continue to scrutinize banks that have been identified as misreporting at date t. The idea is that being detected as misreporting does not automatically entail license revocation, and the detected banks may either recover and stop misreporting, or continue misreporting in a different manner, pretending they have fully recovered.[12] The banks have incentives to mimic recovery to enjoy the removal of the CBR restrictions on their activities. To account for these features (i.e., expanding falsification practices by banks and the CBR attention to it), I introduce second and third options for the CBR regulation. The second option implies that the CBR remains suspicious and maintains restrictions on a bank's activities for at least one year after it detects misreporting. The third option implies that restrictions are maintained forever. I refer to these options as "*suspicious*" and "*most suspicious*" regulations. I check these two options in the robustness section. Formally, under these options, the treatment group is constructed as follows:

$$TREAT_{it}^{(j)} = \begin{cases} 1, & \text{if} \quad \widehat{HNC}_{it} > 0 \quad \text{or} \quad \widehat{HNC}_{it-1} > 0 \quad \dots \quad \text{or} \quad \widehat{HNC}_{it-p} > 0 \\ 0, & \text{if} \quad \forall p = 0, 1 \dots P \quad \widehat{HNC}_{it-p} = 0 \end{cases} \tag{4.4}$$

where I set $P = 4$ for the "suspicious" type ($j = 2$) and $P = \tilde{t}$ for the "most suspicious" type ($j = 3$), where $\tilde{t}$ is the first quarter in which the rule (4.1)–(4.2) detects misreporting by bank i.

I further define the indicator variable reflecting switching to the tightened regulation regime after the appointment of the new CBR head in mid-2013, which divides my sample period into two parts: before the change, when the enforcement was soft, and after the change, when the enforcement tightened. Formally, the indicator variable reads as:

$$REG.CHANGE_t = \begin{cases} 1, & \text{if } t \geq 2013Q2 \\ 0, & \text{if else} \end{cases} \tag{4.5}$$

Having defined the division of banks into treatment and control groups and given the time of regulatory change, I proceed to regression analysis. The concepts in this analysis

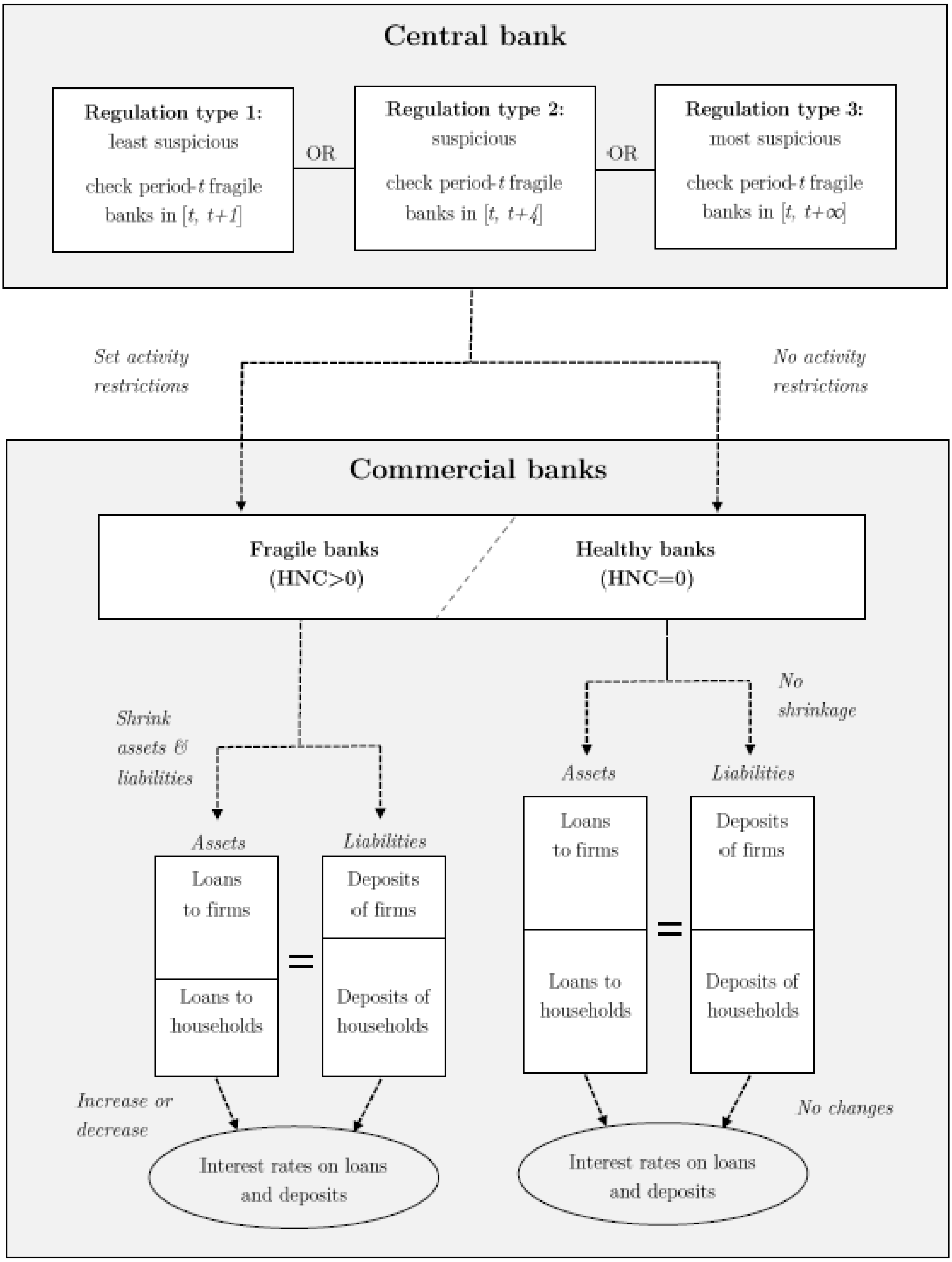
[12]This is in line with a large body of anecdotal evidence that operating banks modify their falsification schemes as soon as the CBR reveals existing schemes and includes them in its current regulations, see, e.g., an analytical report in https://www.banki.ru/news/daytheme/?id=6609791.

appear in Fig. 4.2 below. The central bank sets regulation rules according to which it inspects banks each and every period but does not disclose the rules publicly. When inspecting banks, the central bank reveals a regulation type $j = 1, 2, 3$. The threshold between misreporting and non-misreporting banks is contingent and thus requires inspection. When it detects a misreporting bank, the central bank sets activity restrictions on the bank. Formally, I do not observe any connection between the CBR and a misreporting bank i until the CBR revokes its license and reports the reasons for doing so. However, I can observe that a fraudulent bank i, while still operating in the market, does or does not start to shrink its activities *more* than others after mid-2013 (within some specific window, during which one can be relatively certain that there were no other factors affecting the bank's decisions). If a shrinkage of the bank's i activities indeed takes place, it is likely to affect both sides of the bank's balance sheet and P&L accounts. Therefore, I expect that the bank i will (i) decrease its borrowed funds (relative to owned funds, or capital) and reduce its risk-bearing assets, e.g., loans to corporations and households (relative to total assets), *in absolute terms*, (ii) adapt the structure of its liabilities and assets *in relative terms*, and (iii) adapt expenses on its borrowed funds and face changing returns on its risk-bearing assets compared to other banks that have not been detected by the central bank.

My treatment and control groups of banks are fuzzy by construction (de Chaisemartin and D'Haultfoeuille 2017). The imposition of treatment varies over time, which requires one to control for time fixed effects to make the treatment effects comparable across times Goodman-Bacon (2021). I formalize these ideas in the following fuzzy time-varying difference-in-differences (DID) regression:

$$
\begin{aligned}
Y_{it}^{(n)} = {} & \beta_1 TREAT_{it}^{(j)} + \beta_2 REG.CHANGE_t + \beta_3 \Big(TREAT_{it}^{(j)} \times REG.CHANGE_t \Big) \\
& + \sum_{m=1}^{M} \delta_m \mathrm{BSF}_{m,it} + \alpha_i + \gamma_t + \varepsilon_{it}
\end{aligned}
\tag{4.6}
$$

where for bank i at time t $Y_{it}^{(n)}$ is n^{th} dependent variable from one of two categories:



Note: HNC is hidden negative capital. I assume three types of prudential regulation that the Central Bank of Russia (the CBR) could follow after mid-2013. Mid-2013 is a borderline that marked a switch from an HNC-tolerant to an HNC-intolerant regime of prudential regulation.

Figure 4.2: Differential prudential regulation and its effects on banks

assets and liabilities in absolute terms, assets and liabilities in relative terms (see below). Further, $\text{BSF}_{m,it}$ is m^{th} bank-specific control variable, as suggested by Gropp et al. (2018); α_i and γ_t represent bank and time fixed effects, and ε_{it} is the regression error.

Regarding the choice of $Y_{it}^{(n)}$, the first of the two categories includes the sizes of assets, equity capital, deposits of households, deposits of non-financial firms, loans to households, and loans to non-financial firms. The null hypothesis reads as $\beta_3 < 0$ and is statistically significant. This would indicate declining regulatory forbearance, thus implying less CBR tolerance of misreporting banks after mid-2013.

The second category of dependent variables considers variables from the first category to be ratios to total assets (except the assets themselves). The null hypothesis implies that β_3 is statistically significant, though its sign is ambiguous. The data show whether and how treated banks adjust the structure of their balance sheets.

I estimate regression (4.6) with a robust two-way fixed effects estimator. The estimation window for the baseline estimates is set to ±3 years around the regulatory change in mid-2013. In the robustness section, I check the sensitivity of my results to shrinking the length of the window. BSF, bank, and time fixed effects are aimed to capture observable differences across banks and in time. In the robustness section, I also reduce the sample size by applying the bias-adjusted matching estimator of Abadie and Imbens (2011) and re-running regression (4.6) on a matched sample.

4.2.3 Transmission channels

I further explore potential channels that can amplify the effects of tightened regulation on bank balance sheets. I consider a growing concentration in the banking system across the 2010s, deteriorating loan quality (rising NPLs), and declining equity capital to total assets ratios (rising leverage). To test these channels, I modify the DID regressions (4.6) from the previous section by including the respective transmission variable to the product of treatment and regulatory change binary indicators:

$$
\begin{aligned}
Y_{it}^{(n)} &= \beta_1 TREAT_{it}^{(j)} + \beta_2 REG.CHANGE_t + \beta_3\Big(TREAT_{it}^{(j)} \times REG.CHANGE_t\Big) \\
&\quad + \beta_4\Big(TREAT_{it}^{(j)} \times REG.CHANGE_t \times TRANSMIT_{it}\Big) \\
&\quad + \sum_{m=1}^{5} \delta_m \mathrm{BSF}_{m,it} + \alpha_i + \gamma_t + \varepsilon_{it} \qquad (4.7)
\end{aligned}
$$

where $TRANSMIT_{it}$ is either banking sector concentration (as measured by the Herfindahl-Hirschman index, HHI) at t, bank i's NPLs at t, or bank i's equity capital to total assets ratio at t. All the rest is the same as before. All possible subproducts of the three variables entering the triple interaction are included but not fully reported to save space.

Banking sector concentration (HHI). On the one hand, the literature suggests that it may be easier for a central bank to monitor a banking system with fewer players and therefore less risk of financial contagion (Allen and Gale 2000). In this case, I expect $\beta_4 < 0$. On the other hand, banks that survive in a more concentrated system could possess more bargaining power with the central bank and/or could have more scope for falsification practices. In this case $\beta_4 > 0$. The data will show which force dominates.

Bank loan quality (NPLs). I expect that banks that report greater NPLs are more likely to be inspected by the central bank and, if misreporting is detected, to face larger activity restrictions, i.e., $\beta_4 < 0$.

Bank equity capital. Banks with lower reported equity capital to total assets ratios are also likely to be inspected and to face activity restrictions if misreporting is detected, i.e., $\beta_4 > 0$.

4.2.4 Macroeconomic implications of tightened regulation: a SVAR analysis

In the absence of access to matched bank-borrower data of the CBR, I turn to alternative ways to evaluate the macroeconomic effects of tightened prudential regulation in the Russian economy. I aggregate the microeconomic estimates of credit reductions by misreporting banks to the macroeconomic level by applying a SVAR model with five endogenous variables, including output, CPI inflation, risk-free interest rate, composite bank lending rate, and the volume of bank loans in the economy, following Gambetti and Musso (2017). I employ the authors' sign restriction approach and identify credit supply shock as a shock that simultaneously causes the lending rate to fall and loan volumes to rise (on-impact), and output, prices, and the risk-free rate to also rise (on-impact). I identify the other shocks—monetary, aggregate demand (AD), and aggregate supply (AS)—to separate them from the credit supply shock and avoid the "masquerading" of shocks (Wolf 2020).

Following recent trends in the SVAR literature, I also apply the narrative sign restriction approach, suggested by Antolin-Diaz and Rubio-Ramirez (2018). I account for the fact that December 2014 is perceived as a time of dramatically restrictive monetary policy shock in Russia. That is, during the "Black Monday" of December 15, the CBR suddenly raised the key rate by 6.5 percentage points (from 10.5 to 17% per annum), which raised fears of further credit declines in the economy.

I then compute the impulse response functions of all endogenous variables to the identified credit supply shock and the implied elasticity of output with respect to credit at exactly the time a credit supply shock occurred. Tightened regulation has nothing to do with demand-side factors affecting the credit and thus can be understood as a force underlying negative credit supply shocks.

4.3 Data description

4.3.1 Bank-level data

I use several sources of statistics at the bank level. First, data on bank misreporting come from official press releases of the CBR ("*Vestniki Banka Rossii*") from 2007 till mid-2019. These data deliver information on which banks were closed by the CBR for misreporting and what size of associated losses (HNC) that entailed. Second, I collect all relevant data on bank assets and liabilities from monthly balance sheets ("*Form 101*") and data on bank income and expenses from quarterly profit and loss accounts ("*Form 102*"), which were freely disclosed through the CBR website from 2004 to 2022.[13] Publishing these forms is not mandatory for banks; however, from 2007 (the beginning of my sample due to the availability of the data on misreporting), these forms covered about 95% of the Russian banking system's total assets.

I exclude from the sample the top-5 largest banks in the Russian banking system in terms of assets because these are state-owned national giants that are unlikely to be inspected.[14] I also exclude the subsequent 6 banks in the asset ranking, because they—together with the top-5—are officially recognized by the CBR as SIFI, i.e., systemically important financial institutions, and thus are also unlikely to be closed.[15] For each relative bank-specific indicator discussed in the previous section (except bank size as measured by the log of total assets), I winsorize all observations below the 1st and above the 99th percentiles in respective distribution, by each quarter. Overall, I have 925 banks that reported their forms publicly in January 2007 (the beginning of the sample), 937 banks in June 2013 (the time of Nabiullina's appointment as the head of the CBR), and 448 banks in June 2019 (the end of the sample).

I do not report the descriptive statistics of the full sample of banks here. I first run the Heckman selection model and, based on the results, construct treatment and control groups, and then, before proceeding to the DID analysis, I report descriptive statistics for the two groups in comparison.

4.3.2 Macroeconomic data

For the SVAR analysis, I gather monthly data on output, CPI inflation, risk-free rate, composite lending rate, and the volume of loans to households and non-financial firms (see Fig. 1.1 in Appendix 1.H). The data are collected from the official databases of the

[13]The forms can be accessed through https://www.the CBR.ru/banking_sector/otchetnost-kreditnykh-organizaciy/.

[14]These include (in order of size): Sberbank, VTB, Gazprombank, Russian Agricultural Bank, and the Bank of Moscow.

[15]See https://www.the CBR.ru/press/PR/?file=14102019_191000ik2019-10-14T19_03_50.htm.

Federal State Statistic Service (Rosstat) and the CBR.

The data show that output grew 1.5 times over the period, exhibiting strong cyclical features (especially before the global financial crisis of 2007–2009) and clearly slowed after the recession of 2014–2015. Prices during the same period more than tripled. Loan volumes substantially outpaced the growth of output and prices, increasing approximately 17 times. This is a typical feature of emerging economies. Risk-free and lending rates vary considerably, between 5 to 15% and 10 to 20% per annum, respectively, also exhibiting strong pro-cyclical features.

4.4 Estimation results

4.4.1 Identification of treatment and control groups of banks

This section describes the construction of the treatment and control groups of banks using the Heckman selection model. I also provide a descriptive analysis of the two groups.

The Heckman selection model

As noted, data on bank closures due to detected misreporting started to appear publicly at the beginning of 2007. Because the subsequent DID analysis is based on six-year estimation window around mid-2013, it is enough to run the Heckman selection model starting from 2010. Hence, I estimate selection and outcome equations (4.1) and (4.2) quarter-by-quarter from 2010 Q1 till 2019 Q2. To account for the past experience of the CBR in detecting misreporting, when estimating the equations for a given quarter t, I include all banks that were closed for misreporting from 2007 till t ($s_{it} = 1$ in selection equation and $HNC_{it} > 0$ in outcome equation). To keep the sample balanced between closed and operating banks at each quarter t, when estimating the equations for the quarter t I include all operating banks active at this quarter ($s_{it} = 0$ in selection equation and $HNC_{it} = 0$ in outcome equation), not from 2007 till t as in the case of closed banks.

Table 4.1 below reports a snapshot of the estimation results for the key quarters in the sample: three years before, the time of, and three years after the Nabiullina's arrival to the CBR.[16] At these three points in time, the number of banks that were closed due to misreporting increased at least sevenfold.

Results from the Heckman selection estimates indicate that the CBR regulation rule could have been constantly updated since 2010. This comes from the fact that almost all of the estimated coefficients change considerably across time. Of course, some of these changes could be attributed to the fact that, from quarter to quarter, the size of the subsample of banks that were closed for misreporting is growing.

Nonetheless, *first*, I observe that, as time passes, the likelihood of being detected by the CBR becomes lower for banks with more equity capital and, if detected, the size of revealed HNC becomes smaller. This result is consistent with the literature on the losses associated with bank closures (James 1991; Schaeck 2008; Kang, Lowery, and Wardlaw 2015; Cole and White 2017).

Second, NPLs seem not to be very important when the CBR decides whether to inspect a suspicious bank or not, which is indicated by mostly insignificant coefficients. This is likely to reflect the regulator's mistrust of the reported quality of the loans to the Russian economy.

Third, regarding the structure of assets, I observe that banks with more loans to firms are more likely to be inspected and, if misreporting is detected, to exhibit larger losses of closure. This is very much in line with the theory of Song and Thakor (2007). Further, holding more liquidity (in the form of cash and reserves) increases the size of losses, if a bank was inspected and misreporting was detected. The growth of total assets per se seems not to play a large role in the decision to inspect a suspicious bank. What could be important instead is how fast the loans—especially household loans—are turned over. Hereinafter, by turnover of a bank's asset (liability) we mean an inflow of this asset (liability) between $t-1$ and t (note that growth rate equals the difference between inflow

[16] Full results are available upon request.

Table 4.1: Cross-sectional Heckman selection estimates: ±3 years around the regulatory tightening in mid-2013 [a]

	3 years before 2013Q2		2013Q2		3 years after 2013Q2	
	Out	Sel	Out	Sel	Out	Sel
	(1)	(2)	(3)	(4)	(5)	(6)
Equity capital / Total assets	–0.023	–0.009	–0.165	–0.018**	–0.829***	–0.036***
	(0.379)	(0.009)	(0.523)	(0.007)	(0.265)	(0.006)
NPLs on firm loans	0.128	–0.001	–1.117	–0.004	–0.007	–0.009*
	(0.499)	(0.010)	(1.026)	(0.010)	(0.286)	(0.005)
NPLs on household loans	–0.016	0.004	0.599	0.012**	0.006	0.001
	(0.218)	(0.006)	(0.476)	(0.005)	(0.193)	(0.004)
Liquid assets / Total assets	0.188	0.004	1.560***	0.013**	0.496**	–0.007*
	(0.231)	(0.007)	(0.502)	(0.006)	(0.220)	(0.004)
ROA (annualized)	–1.000	–0.067**	–2.116	–0.059***	1.310**	0.044***
	(1.300)	(0.030)	(1.711)	(0.019)	(0.538)	(0.013)
Growth of total assets	–0.020	–0.005**	0.102	0.001	0.064	0.002*
(annualized)	(0.120)	(0.003)	(0.079)	(0.001)	(0.040)	(0.001)
Net interbank loans	–0.066	–0.020*	0.865	–0.010	0.902***	–0.009*
/ Total assets	(0.541)	(0.010)	(0.651)	(0.008)	(0.257)	(0.005)
Household deposits	0.135	0.009**	0.098	0.007**	–0.018	0.003
/ Total assets	(0.210)	(0.004)	(0.267)	(0.003)	(0.132)	(0.003)
Loans to firms	0.323	0.014***	0.637*	0.009**	0.744***	0.016***
/ Total assets	(0.261)	(0.005)	(0.372)	(0.004)	(0.187)	(0.003)
Turnover of house.loans	–1.640	–0.014	8.050***	0.025	5.388***	0.036
/ Total assets	(1.026)	(0.031)	(1.793)	(0.024)	(1.145)	(0.023)
Turnover of firms.loans	0.303	0.016*	0.214	0.008	–0.214	–0.002
/ Total assets	(0.379)	(0.009)	(0.703)	(0.008)	(0.338)	(0.007)
log of total assets		–0.180**		–0.270***		–0.332***
		(0.075)		(0.065)		(0.045)
Constant	–11.792	–2.239***	–120.474*	–1.587***	–33.478*	0.174
	(53.505)	(0.474)	(61.923)	(0.353)	(19.666)	(0.295)
N obs.	886		899		852	
N censored / observed	844 / 42		814 / 85		567 / 285	
Wald χ^2	8.786		33.272***		47.555***	
ρ	0.350		0.834**		0.852***	

Note: The table reports efficient two-step estimates of the Heckman selection model for the three specific periods: 2013Q2, i.e., the time of regulatory change in the Central Bank of Russia, and three years before and after this date (recall that the estimation window in the baseline version of the difference-in-differences estimates equals ±3 years around mid-2013). Dependent variables are (i) an indicator variable of whether hidden negative capital, HNC, was detected by the CBR (columns "*Sel*") and (ii) the ratio of HNC to the equity capital reported one quarter before the closure (columns "*Out*"). *Sel* and *Out* are selection and outcome equations of the Heckman model. All explanatory variables are taken with a one-quarter lag. ρ is correlation between the regression errors of *Sel* and *Out*. *Wald* χ^2 is the Wald statistic that tests the null hypothesis that all coefficients equal zero simultaneously. N *censored* reflects all banks operating in the respective quarter for which the estimate is done. N *observed* accumulates all banks with HNC detected from the beginning of the sample, 2010Q2, to the respective quarter for which I perform an estimate.

[a] The rest of the estimates (i.e., for the other 44 quarters in the sample, 2010Q2 to 2019Q2) are not reported for the sake of space and are available upon request

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Robust standard errors appear in the brackets under the estimated coefficients.

and outflow). For example, turnover of loans means newly issued loans. The estimates suggest that faster turnovers, though not associated with a greater probability of being detected, are positively associated with losses in case of closure.[17] Finally, granting more loans in the inter-bank market is associated with larger losses, if a bank was detected for misreporting. This may indicate that either the bank attempted to withdraw funds in a coalition with other banks or that it lent to fraudulent banks.

Fourth, as for the liability structure, I observe that relying more on insured deposits (of households) is not a panacea per se and is unlikely to drive the CBR decision to inspect and the losses conditional on being audited.

Fifth, the profitability of bank assets (ROA) seems to play opposite roles before and after mid-2013. Before, as expected, a greater ROA reduced the likelihood of being audited, whereas after, less expectedly, the situation reverted and a higher ROA might have attracted the attention of the regulator. Though I regard this observation cautiously, it could reflect cyclical movements of profits and losses along with the business cycle phases.[18] Put differently, if a bank has a lower ROA during the positive phase of the business cycle, it is likely to induce the central banks to inspect it, and, conversely, if a bank reports profits when others tend to report losses, this could also raise suspicion.

Sixth, bank size is negatively related to the probability of the CBR inspection and is highly statistically significant in the selection equation, as expected. This could also reflect the "too big to fail" problem and as I discussed, I check the robustness of the findings by switching to an indicator of negative profits.

Finally, the estimated correlation between the errors from the selection and outcome equations is positive and rather large, exhibiting statistical significance at least from mid-2013. This indicates that selection issues are indeed present in the data.

Overall, the Heckman selection estimates deliver insight into what type of regulation

[17] Though indirectly, this conclusion is in line with the findings of Mian and Sufi (2009), which establish that a too-fast expansion of loans to households triggers financial crises in the future. Note that no such effects are observed in the estimates of the turnover of corporate loans, which is also in line with Mian, Sufi, and Verner (2017).

[18] Mid-2013 witnessed a move of the economy towards another recession, while mid-2016 was a period of recovering from that recession.

rule a central bank may follow. In the robustness section, I turn to a very different approach to capturing such a rule, based on rankings by the Z-score of bank stability, as in DeYoung and Torna (2013).

I next predict the fitted values of both selection and outcome equations and report the results in Table 4.2 below. The table contains two groups of columns, one for banks closed for misreporting (1–5) and the other for the rest of the banks, which continued their operations and which could be misreporting but not yet detected by the CBR (6–10).

Table 4.2: In-sample predictions after the Heckman selection estimates: descriptive statistics, ±3 years around the regulatory tightening in mid-2013

	Banks failed with $HNC_{it} > 0$					Operating banks with $\widehat{HNC}_{it} > 0$				
	Obs	Mean	SD	Min	Max	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HNC / Total assets (actual data)	371	35.7	36.8	0.6	413.9					
HNC / Total assets (predicted, baseline)	371	35.7	12.5	0.0	112.0	18,585	38.0	29.8	0.0	444.4
$Pr\left[\widehat{HNC}_{it} > 0\right]$ (predicted, baseline)	371	0.7	0.2	0.0	1.0	18,585	0.1	0.1	0.0	0.9

Note: The table reports descriptive statistics pooled across the periods from 2010 Q2 to 2016 Q2 for (i) banks closed for misreporting ($HNC_{it} > 0$) and (ii) the other operating banks with misreporting not-yet-detected ($\widehat{HNC}_{it} > 0$). Note that the Heckman selection model itself assigns a strictly positive probability of misreporting to all of them.

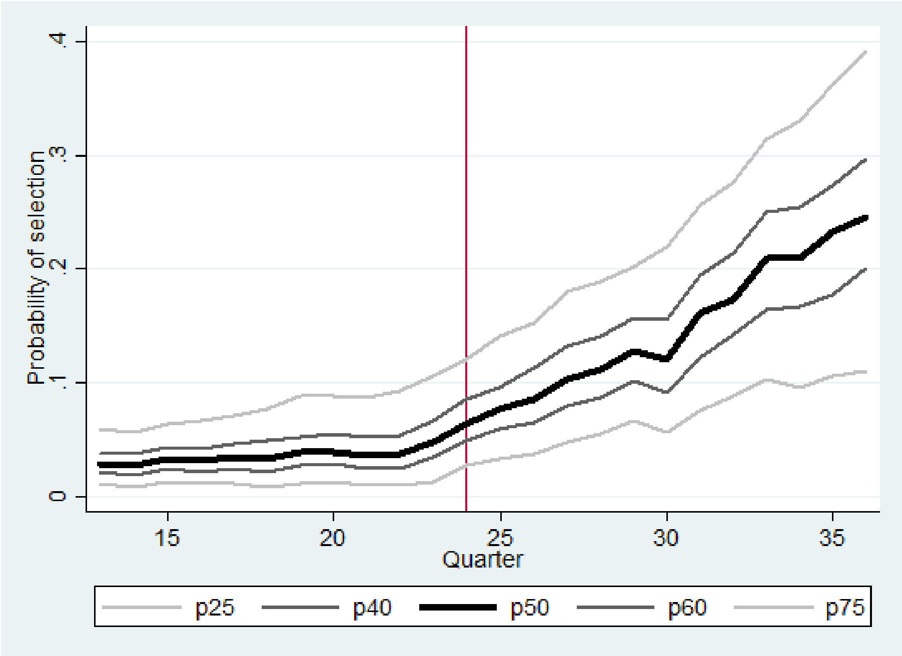
These pooled fitted values suggest that *first*, the model works well to capture the in-sample mean size of HNC but under-predicts (by about 4 times) the size of losses in the upper tail of the failed banks' distribution by HNC. Indeed, within the six years between 2010 Q2 and 2016 Q2, the final sample contains 371 banks closed for misreporting, with average losses (HNC) amounting to 36% of total assets reported on the eve of closure, and extreme losses largely exceeding assets.[19] The latter pertains to the cases in which banks were hiding illegal active operations from their balance sheets. *Second*, for operating banks, one can observe that the model predicts almost the same mean size of HNC as

[19]Notably, the mean losses are about 1.25 times larger than those reported by James (1991) for the US banking system when it suffered losses from the S&L crisis in the 1980s and approximately 1.5 times larger than the losses of US banks during and after the Great Recession, as reported by Cole and White (2017).

for the closed banks, i.e., slightly above one-third of their total assets. I note that these banks were still operating but not-yet-detected by the CBR within the period considered. It is even more notable that the model predicts huge losses in the upper tail, which more than four times exceed the assets (just as in the actual data of closed banks). The model's minimal fitted value is effectively zero. Thus, the model does work to distinguish between operating banks with potentially high and potentially low losses conditional on being detected. *Third*, regarding the probability of being detected itself, as expected, the model assigns a large mean value for closed banks and a small value for operating banks, with the difference amounting to a factor of 7. Note that the range of fitted probabilities is also wide, from almost zero to almost unity for both closed and operating banks.

I next plot the time evolution of the probability s_{it} of being detected that I predict for operating banks. Fig. 4.3 below illustrates the results. The figure contains 25, 40, 50, 60, and 75th percentiles of the operating banks' distribution by $\hat{s}_{it}$ for each quarter t within ± 3 years around the mid-2013. Before Nabiullina's appointment at the CBR, the probability of being detected was rather stable, though it started to increase slightly two quarters prior to mid-2013. The median levels amounted to about 4-5% and the interquartile range was almost always below 10%. With Nabiullina's tightened regulation, the probability of being detected continued to grow along an almost linear trend so that, by mid-2016, the median probability increased to 25%, and the interquartile range was bounded between 11 and 40%.

As discussed in the previous section, in the baseline estimations below I am agnostic about which part of the banking system the CBR inspects each period and I set the threshold $\hat{s}^*_{it}$ to the median at each quarter. That is, I assume that every quarter the CBR audits each bank with $\hat{s}_{it}$ above the black solid line depicted in Fig. 4.3. I use θ to denote the regulation rule, with $\theta \in (0, 1)$, and thus refer to the baseline case as $\theta = 0.5$. In the robustness section, I recompute all results for the cases when the regulator is more concerned ($\theta = 0.25$, for concreteness) and less concerned ($\theta = 0.75$) about bank misreporting.



Note: The figure reports selection predictions after estimating the Heckman selection model. The predictions are performed at the bank-quarter level and then, for the sake of representation, averaged across the banks in the sample in each quarter. The vertical red line crosses the 24th quarter of the sample, which stands for 2013 Q2, i.e., the point at which the CBR shifted from soft to tight prudential regulation.

Figure 4.3: Predicted probability of fraud being detected at the bank-quarter level

Descriptive analysis of the treatment and control groups

Having defined the agnostic regulation rule ($\theta = 0.5$) in the previous section, I now analyze the descriptive statistics of resultant groups of banks in Table 4.1 (see Appendix 4.A). In the table, columns 1 to 5 represent statistics for the control group, and columns 6 to 9 for the treatment group. As discussed above, in the baseline estimates, I use the time-varying median level of the fitted values of the probability of being detected, as implied by the selection equation (4.1). Thus, with $\hat{s}_t^*$ representing the time-varying medians, banks with $\hat{s}_{it} < \hat{s}_t^*$ are included in the control group and banks with $\hat{s}_{it} \geq \hat{s}_t^*$ in the treatment group. Panel 1 then reports the data for the dependent variables used in the family of equations (4.6) and (4.7) and Panel 2 contains the data on the explanatory variables for the same equations. All the data are computed for the six years horizon with the center in mid-2013, as in my DID regressions to follow. I drop observations below the 1st and above the 99th percentiles of the distribution by each explanatory variable (except the log of total assets) for both the control and treatment groups.

The descriptive statistics suggest that, first, even though I exclude SIFI, the six-year average size of total assets in the control group is 9 times larger than that in the treatment group, signaling that larger banks are still less likely to be inspected by the CBR. The same holds for all scale variables presented. Second, in relative terms, important differences arise in the structure of assets and liabilities between the groups. While banks in the control group have more or less similar weights on the funds attracted from households (insured) and non-financial firms (uninsured), banks in the treatment group are much more oriented toward insured funds.[20] Further, with these insured funds, treated banks are much more specialized in lending to corporations rather than households than are the control banks.[21] Third, with these differences in asset-liability structures, I observe further that treated banks pay more on both corporate and retail deposits and earn more interest on corporate loans than do the control banks. Fourth, treated banks are relatively less capitalized, report two times lower NPLs on corporate loans, hold more cash and reserves, provide fewer loans in the inter-bank market, have higher turnovers on corporate loans compared to the control banks, and exhibit lower returns on assets (ROA).[22] Fifth, regarding the growth rate of total assets, both groups are more or less similar, except that treated banks are less volatile in this respect. Overall, the treatment group has a more risky profile compared to the control group.

[20]This recalls moral hazard issues going back (at least) to the theory of Keeley (1990) and cross-country empirical evidence by Demirguc-Kunt and Detragiache (2002).

[21]This indirectly speaks to the theory of Song and Thakor (2007), which shows negative consequences for the stability of banks that rely predominantly on informationally opaque corporate (relationship) loans funded with insured deposits, which are likely to be less monitored.

[22]An important question arises: Could lower equity as a result of regime shift in treated banks also be due to recognition of losses enforced by the regulator during the on-site inspection? In general, this could, and should, be the case if the regulation is successful. However, I construct the treatment group using the Heckman selection model, in which all explanatory variables enter the selection equation with a one-quarter lag. The resultant selection variable should thus measure the ex-ante riskiness of a bank, including capitalization. It should be prior to the on-site inspection by the regulator

4.4.2 The effects of tightened regulation on not-yet-detected misreporting banks

Having constructed and discussed the treatment and control groups of banks, I now present my baseline DID regression results on the scale and composition effects of declining regulatory forbearance.

Scale effects

Table 4.3 below reports the results of estimating the DID regressions (4.6) with the set of scale-dependent variables under the regulation rule $\theta = 0.5$. The table contains two panels: one for the estimates at extensive margin ($TREAT_{it}$ is the binary indicator of bank misreporting) and the other for intensive margin ($TREAT_{it}$ equals the predicted value of losses in case of detection, i.e., $\widehat{HNC}_{it}$, if $\hat{s}_{it} \geq \hat{s}^*_{it}$, and zero, if-else). All regressions contain the full set of bank FEs, quarter FEs, and bank-specific control variables, which are not reported to preserve space.[23] In all regressions, I use the estimation window which is ± 3 years around the regulatory change in mid-2013.

Several outcomes emerge from the estimated scale effects.

First, in all six cases, the effects are negative and highly statistically significant on both the extensive and intensive margins, meaning that the tightened regulation after mid-2013 shrank the balance sheets of potentially misreporting banks, including the most important types of liabilities and assets, as compared to the other (non-misreporting / not inspected) banks.

Second, the estimated effects are economically very large, and in most cases exceed the average size of the respective operation in the treatment group, thus indicating high efficiency of the tightened regulation. For instance, the estimates suggest that, on the extensive margin, the average bank from the treatment group was forced to reduce its total assets by 18 billion rubles, which is 3.5 times more than the actual size of its total assets (Table 4.1 above). On the intensive margin, the average estimate decreases by

[23] The full results are available upon request.

Table 4.3: Scale effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

Dependent variable	TA_{it}	EQ_{it}	$DEPf_{it}$	$DEPh_{it}$	$LNSf_{it}$	$LNSh_{it}$
$Y_{it}^{(n)}$ $(n = 1...6)$:	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: On extensive margin (the size of HNC does not matter)						
TREAT×REGIME	–18.521***	–1.176***	–3.278***	–5.032***	–3.001***	–3.922***
	(2.824)	(0.210)	(0.885)	(0.746)	(0.736)	(0.661)
TREAT	6.735***	0.405***	1.192***	2.089***	1.292***	1.694***
	(1.369)	(0.091)	(0.423)	(0.393)	(0.479)	(0.296)
REGIME	32.034***	1.573***	1.070	4.104***	0.569	1.887***
	(4.536)	(0.421)	(1.010)	(1.108)	(1.445)	(0.530)
N Obs.	17,696	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910	910
R^2_{within}	0.095	0.087	0.053	0.175	0.121	0.090
Panel 2: On intensive margin (the size of HNC may matter)						
TREAT×REGIME	–0.303***	–0.019***	–0.057***	–0.081***	–0.043***	–0.064***
	(0.053)	(0.004)	(0.017)	(0.015)	(0.015)	(0.012)
TREAT	0.078***	0.004***	0.011**	0.020***	0.012**	0.015***
	(0.019)	(0.001)	(0.005)	(0.005)	(0.005)	(0.004)
REGIME	27.465***	1.264***	0.263	2.750***	–0.329	0.862*
	(3.981)	(0.402)	(0.914)	(1.013)	(1.344)	(0.495)
N Obs.	17,696	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910	910
R^2_{within}	0.083	0.080	0.050	0.165	0.117	0.079

Note: The table contains difference-in-differences estimates of regression (4.6) with dependent variables $Y_{it}^{(j)}$ reflecting the size of total assets TA_{it} $(n = 1)$, equity capital EQ_{it} $(n = 2)$, deposits of non-financial firms $DEPf_{it}$ $(n = 3)$, deposits of households $DEPh_{it}$ $(n = 4)$, loans to non-financial firms $LNSf_{it}$ $(n = 5)$, loans to households $LNSh_{it}$ $(n = 6)$. All regressions include full sets of bank FE, quarter FE, and bank control variables, which are not reported for the sake of space and are available upon request. Mid-2013 marks the transition of the CBR to a new prudential regulation regime, in which the CBR is no longer tolerant of fraudulent banks. The treatment group consists of all banks which are likely to be treated as fraudulent by the CBR (the treatment rule is proxied with the Heckman selection model (4.1)–(4.2)). The composition of the treatment and control groups varies in time depending on the application of the treatment rule in each quarter (see Section 4.2.2 for details).

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

two times but still exceeds the actual size.[24] Recall that the maximal value of total

[24]The average predicted HNC is 38% of total assets (see Table 4.2), and the estimated respective scale effect is –0.303, which results in a decline of total assets by 11.5 billion rubles, more than 2 times larger

assets in the treatment group is 265 billion rubles. The estimated scale effects lie well between the mean and maximal values, which implies that the CBR tended to eventually close smaller banks detected in misreporting and to allow larger banks to continue their operations (possibly after requiring them to clean poor quality assets from their balance sheets).

Third, The treated banks were likely to reduce lending to both households and non-financial firms. This could have macroeconomic implications and I apply the estimated scale effects to trace this in Section 4.4.4.

One could argue that the chosen estimation window ±3 years around the regulatory tightening in mid-2013 is too wide and possibly includes other important events (sanctions, the crisis of 2014–2015), and thus relying on it could be misleading. Though I control for any macroeconomic shocks that could affect the results, by including quarter FEs, I re-estimated all the scale effects under narrower windows. The results of this exercise appear in Fig. 4.1 (see Appendix 4.B). The figure enlarges all six regressions from Panel 1 of Table 4.3 by shifting to the following estimation windows centered at 2013 Q2: ±1 quarter, ±4 quarters, ±8 quarters, ±12 quarters (the baseline), and the full sample (for comparative reasons). Specifically, the figure reports only the estimated coefficient on $TREAT_{it} \times REG.CHANGE_t$ and its associated 95% confidence intervals in each of the six cases. It is clear from the figure that, qualitatively, the estimated scale effects are the same across the different estimation windows (perhaps, except ±1 quarter, which could be too narrow for the effect to materialize). As the window expands, each of the estimated effects tends to increase in magnitude, pointing to the persistency of the differences between treated and non-treated banks in time.[25] Notably, if I consider the ±4 quarters estimation window, I would obtain the scale effect on total assets equaled

than its actual size.

[25]Though I do not explore this directly, it is possible that the CBR may indeed be rather suspicious and scrutinized recovered banks in the future solely because they were engaged in misreporting in the past. An alternative explanation is that having been detected by the CBR for misreporting, a treated bank that continues its operations loses a part of its market share and never catches up with its rivals in the future. Though indirectly, this is also consistent with the findings of Berger and Bouwman (2013), who show that the inability of (some) US banks to raise their capital resulted in a partial waste of market shares during the Great Recession.

approximately –5 billion rubles (significant at 1%), i.e., exactly covering the sample mean.

Overall, the results indicate that, with the launching of tightened prudential regulation in mid-2013, banks engaging in misreporting were more likely to be inspected (and detected) by the CBR and to face substantial balance sheet shrinkage (Fig. 4.2 above).

Composition effects: asset and liability structure

Having established the existence and negativity of the scale effects of tightened prudential regulation, I now turn to the testing of its composition effects, i.e., whether the tightened regulation pushed fraudulent banks to adapt their liability and asset structures. I run the same DID regressions (4.6), as in the previous section, but now with dependent variables being scaled by total assets. The estimation results appear in Table 4.4 below.

The estimation results on the composition effects of tightened regulation suggest that, compared to non-inspected (control) banks, banks from the treated group in fact did significantly restructure their borrowed and owned funds and assets. In particular, they tended to decrease equity capital on the intensive margin, i.e., facing larger potential losses associated with being detected. This could be an unintended negative consequence of the tightened regulation because it implies that treated banks were more willing to withdraw their owned funds when facing the CBR inspection (and after) than to raise capital.[26] In addition, the estimates indicate that treated banks also tended to reduce their borrowing from non-financial firms (i.e., uninsured funds) on the intensive margin. Having reduced their owned funds and deposits from firms, the treated banks substituted them with household deposits (i.e., insured funds), on both the extensive and intensive margins.[27] With these new funds from households, the treated banks further expanded their lending to corporations, not to households, on both the extensive and intensive

[26]Similar results appear in Gropp et al. (2018), who show in a quasi-natural experiment framework that after banking authorities in the EU differentially applied tighter capital regulation to targeted banks in 2011, the treated banks tended to decrease their capital rather than to reduce their risk-weighted assets.

[27]In Russia, a system of partial deposit insurance was established in 2004. In the 2010s, the deposit coverage amounted to 1.4 million rubles, which effectively encompasses as much as 99% of the quantity of all deposit accounts, which, however, cover only 55% of the total volume of individual deposits (data from the Deposit Insurance Agency of the Russian Federation; see https://www.asv.org.ru/agency/annual/2018_full/report2018/ru/page2_1_6.html).

Table 4.4: The assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

Dependent variable $Y_{it}^{(n)}$ $(n = 1...5)$:	EQ_{it}/TA_{it} (1)	$DEPf_{it}/TA_{it}$ (2)	$DEPh_{it}/TA_{it}$ (3)	$LNSf_{it}/TA_{it}$ (4)	$LNSh_{it}/TA_{it}$ (5)
Panel 1: On extensive margin (the size of HNC does not matter)					
TREAT×REGIME	–0.467	–0.569	2.270***	2.250***	–0.500
	(0.309)	(0.463)	(0.463)	(0.471)	(0.371)
TREAT	–1.374***	–1.080***	1.871***	4.910***	0.499**
	(0.265)	(0.361)	(0.340)	(0.395)	(0.234)
REGIME	6.225***	–7.463***	5.584***	–4.699***	0.490
	(0.626)	(0.819)	(0.882)	(0.928)	(0.646)
N Obs.	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910
R^2_{within}	0.257	0.211	0.171	0.205	0.174
Panel 2: On intensive margin (the size of HNC may matter)					
TREAT×REGIME	–0.014**	–0.019**	0.053***	0.064***	–0.001
	(0.007)	(0.009)	(0.010)	(0.010)	(0.007)
TREAT	–0.011**	0.005	0.012*	0.054***	0.006
	(0.005)	(0.007)	(0.006)	(0.008)	(0.005)
REGIME	6.315***	–7.568***	5.909***	–4.743***	0.209
	(0.616)	(0.799)	(0.877)	(0.921)	(0.620)
N Obs.	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910
R^2_{within}	0.251	0.209	0.159	0.177	0.173

Note: The table contains difference-in-differences estimates of regression (4.6) with dependent variables $Y_{it}^{(n)}$ reflecting the composition of a bank i balance sheet from the liabilities and assets sides: the ratio of equity capital to total assets EQ_{it}/TA_{it} $(n = 1)$, deposits of non-financial firms to total assets $DEPf_{it}/TA_{it}$ $(n = 2)$, deposits of households to total assets $DEPh_{it}/TA_{it}$ $(n = 3)$, loans to non-financial firms to total assets $LNSf_{it}/TA_{it}$ $(n = 4)$, loans to households to total assets $LNSh_{it}/TA_{it}$ $(n = 5)$. All regressions include full sets of bank FE, quarter FE, and bank control variables, which are not reported to save space and are available upon request. Mid-2013 marks the transition of the CBR to a new prudential regulation regime in which the CBR was no longer tolerant of fraudulent banks. The treatment group consists of all banks which are likely to be treated as fraudulent by the CBR (the treatment rule is proxied with the Heckman selection model (4.1)–(4.2)). The composition of the treatment and control groups varies in time depending on the application of the treatment rule in each quarter (see Section 4.2.2 for details).

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

margins.[28] Notably, household deposits increased by the same 2.3 percentage points of the treated banks' total assets, as did loans to firms within the three years after mid-2013. Finally, loans to households remain unaffected by the composition effects. It seems that the risk profile of the treated banks indeed rose, as was previously suggested by the descriptive analysis (Section 4.4.1).

As in the previous section, I again address the concern regarding the chosen length of the estimation window. Fig. 4.2 in Appendix 4.B presents the results obtained under different estimation windows (on extensive margins).[29] As can be inferred from the figure, my results are qualitatively robust to choosing different lengths of estimation windows. For the deposits of households, I observe that the effect becomes significant even within 1 quarter after the regulatory tightening, increases in magnitude during the next 11 quarters, and remains there till the end of the sample period in 2019 Q2. For loans to non-financial firms, the effect becomes significant starting 8 quarters after the tightening and reaches the peak under the baseline window.

Overall, the results of this section indicate that the tightened prudential regulation launched in mid-2013 forced inspected banks to restructure their assets and liabilities. However, this restructuring was likely to increase the banks' risk exposure, because they tended to decrease owned funds, rely more on insured deposits of households, and provide relatively more loans to firms.

4.4.3 Channels of the effects of tightened regulation

I now investigate potential channels through which tightened bank regulation could have affected the scale and composition of bank balance sheets after mid-2013. As discussed in the methodology section, I consider three possible channels that were active in the 2010s in Russia: the increasing concentration of the banking system, measured by HHI

[28] As suggested by Song and Thakor (2007), this implies engaging more in the asset-liability mismatch in terms of added value.

[29] The figure with the associated results on intensive margin is not reported to save space, and it is available upon request.

(the industry level), and decreasing capitalization and increasing NPLs (the treated bank level); see Fig. 4.1 in Appendix 4.C.[30] I run the DID regressions (4.7) in which I include all three possible channels simultaneously, first to test the scale effects and then the composition effects.

Channels of the scale effects

The estimation results on the channels of the scale effects appear in Table 4.5.

The results indeed suggest that all three channels could have transmitted the scale effects of tightened regulation after mid-2013. *First*, I still find that the mean scale effects remain negative and statistically significant (except for loans to firms), as before. *Second*, the coefficient on the triple interaction of the treatment indicator, regime indicator, and NPLs on household loans is negative and significant in three of six cases (columns 1, 4, and 6, that is, for regressions of total assets, deposits of and loans to households, respectively). This means that an increase in a treated bank's household NPL ratio further amplifies the negative scale effect of tightened regulation.[31] *Third*, the coefficient on the triple interaction of the main two binary indicators and bank equity capital is always positive and highly statistically significant (except for column 2 in which there is no such triple interaction because equity capital is the dependent variable). This implies that greater bank capital diminishes the negative mean scale effect of tightened regulation. *Fourth*, the HHI indicator delivers mixed predictions when interacting with the two main binary indicators. On the one hand, a denser concentration of the banking system amplifies the mean scale effect of tightened regulation on treated banks' total assets (column 1) and deposits of firms (column 3), which supports the idea that a regulator's ability to monitor a more concentrated banking system is higher than a less concentrated one.[32] However,

[30]Note that at the control bank level, the opposite trends were in play during the same time: increasing capitalization and decreasing NPLs.

[31]The estimation results with firm NPL ratio reveal no significant triple effects.

[32]An important concern here would be that concentration should go up as more bank licenses are being withdrawn. Recall in this regard that we study what happens to a bank's operations at t if the bank falls in the treatment group at t. The latter means that the regulator also inspects the bank at t, and formally, the bank's license is not yet revoked, and the bank's assets still contribute to the formation of HHI at t, as it was before. At $t+1$, however, the bank's license may be revoked, and this would then

Table 4.5: Channels of the scale effects of declining regulatory forbearance: ± 3 years around the regulatory tightening in mid-2013

Dependent variable $Y_{it}^{(n)}$ $(n = 1...6)$:	TA_{it} (1)	EQ_{it} (2)	$DEPf_{it}$ (3)	$DEPh_{it}$ (4)	$LNSf_{it}$ (5)	$LNSh_{it}$ (6)
TREAT×REGIME	–7.327***	–0.341**	–1.145**	–1.875***	–0.500	–0.660**
	(1.751)	(0.164)	(0.466)	(0.572)	(0.495)	(0.330)
TREAT×REGIME×NPLh	–0.553**	0.001	–0.008	–0.225**	0.008	–0.125**
	(0.258)	(0.021)	(0.046)	(0.097)	(0.045)	(0.059)
TREAT×REGIME×EQ	0.858***		0.177***	0.205***	0.123***	0.158***
	(0.158)		(0.053)	(0.038)	(0.043)	(0.028)
TREAT×REGIME×HHI	–723.931***	–2.559	–227.774**	–34.868	–68.483	268.387***
	(266.503)	(18.223)	(98.235)	(100.737)	(87.477)	(94.942)
N Obs.	17,696	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910	910
R^2_{within}	0.184	0.093	0.065	0.218	0.138	0.117

Note: The table contains difference-in-differences estimates of regression (4.6) with dependent variables $Y_{it}^{(n)}$ reflecting the size of total assets TA_{it} $(n = 1)$, equity capital EQ_{it} $(n = 2)$, deposits of non-financial firms $DEPf_{it}$ $(n = 3)$, deposits of households $DEPh_{it}$ $(n = 4)$, loans to non-financial firms $LNSf_{it}$ $(n = 5)$, loans to households $LNSh_{it}$ $(n = 6)$. All regressions include full sets of bank FE, quarter FE, bank control variables, and all possible combinations of $TREAT_{it}$, $REGIME_t$, and either of the three-channel variables considered, i.e., the ratio of non-performing loans in loans to households $NPLh_{it}$, equity capital to total assets ratio EQ_{it} (except column 2), or the banking systems concentration measured by the Herfindahl-Hirschman index HHI_t based on banks' total assets. All respective coefficients are not reported for the sake of space and are available upon request. Mid-2013 marks the transition of the CBR to a new prudential regulation regime in which the CBR was no longer tolerant of fraudulent banks. The treatment group consists of all banks which are likely to be treated as fraudulent by the CBR (the treatment rule is proxied with the Heckman selection model (4.1)–(4.2)). The composition of the treatment and control groups varies in time depending on the application of the treatment rule in each quarter (see Section 4.2.2 for details).

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

this does not hold for regressions of equity capital, deposits of households, or loans to firms (columns 2, 4, and 5, respectively). Moreover, in the regression of household loans, the triple interaction with HHI switches the sign from negative to positive and appears to be highly significant (column 6). I thus treat the results associated with banking system concentration with caution.

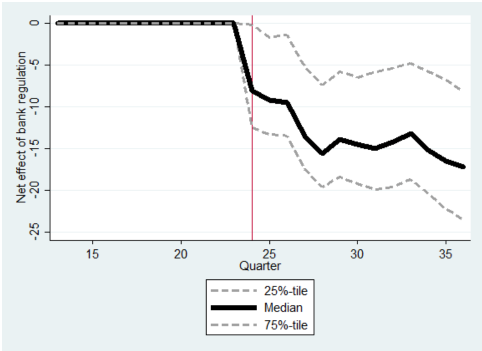
Because each of the three channels has time variation, the triple interactions also vary

contribute to a policy-induced rise in HHI.

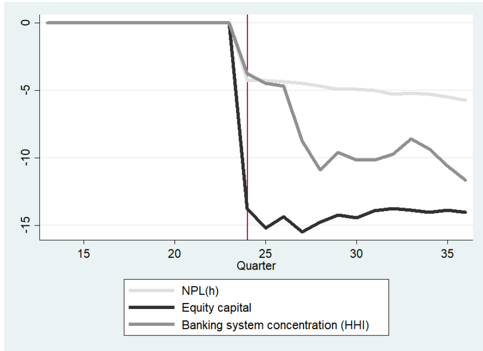
in time, thus allowing me to decompose the total effects and to rank the three channels by economic significance. I first plot the time evolution of the total effect of tightened regulation on the treated banks' total assets (Fig. 4.4.*a*), loans to non-financial firms (Fig. 4.4.*c*), and loans to households (Fig. 4.4.*e*). I then plot the time evolution of the respective total effect decomposed by the three channels (Fig. 4.4.*b*, Fig. 4.4.*d*, and Fig. 4.4.*f*). I choose these three variables, because, in the subsequent section, I focus on the macroeconomic implication of reductions in treated banks' credit to the economy. The results for the other three variables—equity capital, household deposits, and firm deposits—are presented in Fig. 4.1 in Appendix 4.D.

Overall, the decomposition exercise indicates that the scale effects of tightened regulation reveal a large degree of heterogeneity across banks, and that bank capital plays the most prominent role in transmitting the effects on treated banks. First, I find that the effects on the treated banks' total assets and firm loans were rising in magnitude during the first year since mid-2013 and stabilized afterwards, remaining negative within the interquartile range (Fig. 4.4.*a* and Fig. 4.4.*c*); the effect on household loans was also large and negative during the first year, but then it soon diminished (Fig. 4.4.*e*). Second, in cases of total assets (Fig. 4.4a-b) and firm loans (Fig. 4.4c-d), I find that the decrease in treated banks' capital was the main factor pushing the effect downwards, i.e., to be more negative, and thus efficient from the standpoint of the CBR. Growing banking sector concentration was also efficient in helping the CBR shrink the activities of fraudulent banks, but less than the bank capital channel and can thus be ranked second. The growing NPLs of treated banks ranked third and thus were the least efficient. Finally, in the case of household loans (Fig. 4.4e-f), the bank capital channel is still the most efficient, but the near-zero role of NPLs and rising concentration made the overall effect low (recall a positive rather than negative sign of the coefficient on HHI in the respective regression).

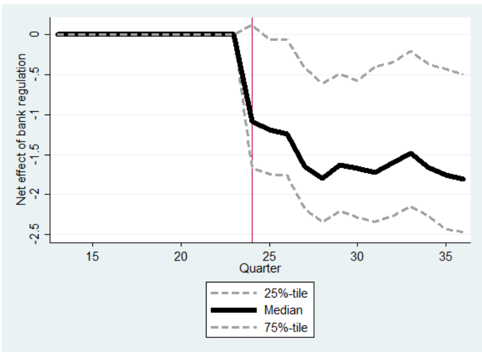
Quantitatively, the exercise shows that, during the three years after the regulatory change, the median effect on the total assets of treated banks could have doubled (from



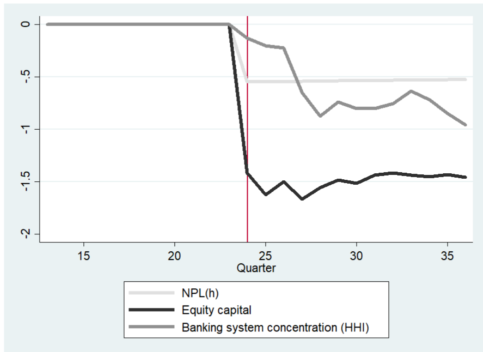
(a) Total assets (*aggregated effect*)



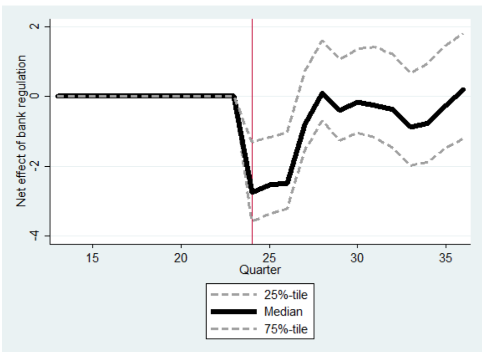
(b) Total assets (*disaggregated effects*)



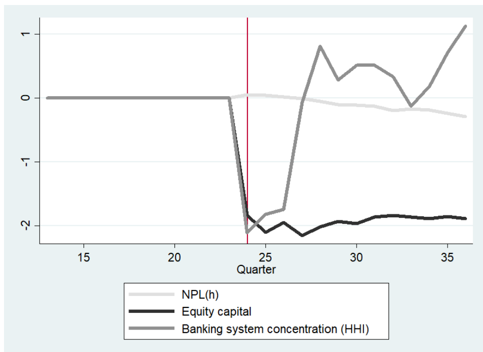
(c) Loans to firms (*aggregated effect*)



(d) Loans to firms (*disaggregated effects*)



(e) household loans (*aggregated effect*)



(f) household loans (*disaggregated effects*)

Figure 4.4: Time evolution of selected scale effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

–9 to –17 billion rubles), the median effect on their firm loans could have also increased by roughly a factor of 2 (from –1 to –2 billion rubles), and the median effect on household loans diminished (from –2 billion rubles at the beginning to zero in the end).

Channels of the composition effects

I now turn to the composition effects of tightened regulation and analyze the same three channels. Table 4.6 below reports the estimation results for the composition effects on the treated banks' structure of assets and liabilities.

Table 4.6: Channels of the assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

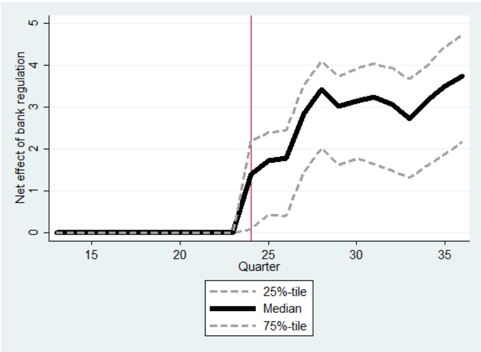
Dependent variable	EQ_{it}/TA_{it}	$DEPf_{it}/TA_{it}$	$DEPh_{it}/TA_{it}$	$LNSf_{it}/TA_{it}$	$LNSh_{it}/TA_{it}$
$Y_{it}^{(n)}$ $(n = 1...5)$:	(1)	(2)	(3)	(4)	(5)
Panel 1: On extensive margin (the size of HNC does not matter)					
TREAT×REGIME	–0.254	–1.891***	1.864***	1.563**	0.392
	(0.347)	(0.546)	(0.474)	(0.613)	(0.383)
TREAT×REGIME×NPLh	0.007	0.003	0.062	0.073	–0.030
	(0.034)	(0.047)	(0.045)	(0.048)	(0.030)
TREAT×REGIME×EQ		0.033	–0.056*	–0.149***	0.069***
		(0.034)	(0.033)	(0.041)	(0.023)
TREAT×REGIME×HHI	14.830	–370.618***	203.627***	175.281**	109.689*
	(50.596)	(77.819)	(69.500)	(83.850)	(56.111)
N Obs.	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910
R^2_{within}	0.257	0.215	0.175	0.215	0.183

Note: The table contains difference-in-differences estimates of regression (4.6) with dependent variables $Y_{it}^{(n)}$ reflecting the composition of a bank i balance sheet from the liabilities and assets side: the ratio of equity capital to total assets EQ_{it}/TA_{it} $(n = 1)$, deposits of non-financial firms to total assets $DEPf_{it}/TA_{it}$ $(n = 2)$, deposits of households to total assets $DEPh_{it}/TA_{it}$ $(n = 3)$, loans to non-financial firms to total assets $LNSf_{it}/TA_{it}$ $(n = 4)$, loans to households to total assets $LNSh_{it}/TA_{it}$ $(n = 5)$. All regressions include full sets of bank FE, quarter FE, bank control variables, and all possible combinations of $TREAT_{it}$, $REGIME_t$, and either of the three-channel variables considered, i.e., the ratio of non-performing loans in loans to households $NPLh_{it}$, equity capital to total assets ration EQ_{it} (except column 2), and the concentration of the banking system measured by the Herfindahl-Hirschman index HHI_t based on banks' total assets. All respective coefficients are not reported for the sake of space and are available upon request. Mid-2013 marks the transition of the CBR to a new prudential regulation regime in which the CBR was no longer tolerant of fraudulent banks. The treatment group consists of all banks which are likely to be treated as fraudulent by the CBR (the treatment rule is proxied with the Heckman selection model (4.1)–(4.2)). The composition of the treatment and control groups varies in time depending on the application of the treatment rule in each quarter (see Section 4.2.2 for details).

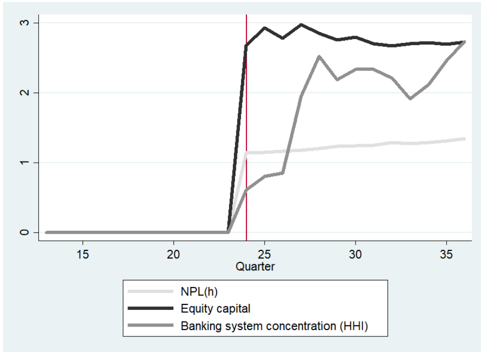
***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Several outcomes emerge from the regression analysis. *First*, I obtain a negative and significant coefficient on the $TREAT_{it} \times REG.CHANGE_t$ variable in the case of firm deposits (column 2) and positive and significant coefficients in the cases of household deposits and loans to firms (columns 3 and 4). These estimates confirm my previous findings that the mean composition effects of tightened regulation were such that treated banks were reducing borrowed funds from corporations (uninsured) and increasing those from households (insured), and lending more to corporations than to households. *Second*, NPLs on household loans were unlikely to be a channel for those effects, because the respective coefficients on the $TREAT_{it} \times REG.CHANGE_t \times NPLh_{it}$ variable are never significant. In other words, although treated banks with relatively more NPLs were decreasing the absolute size of their operations more in response to the tightened regulation, greater NPLs *per se* were not pushing them to adjust the structure of these operations. *Third*, as opposed to NPLs, bank capital again plays a role in channeling the regulatory effect: treated banks with relatively less capital were raising their funding from uninsured sources by more than did relatively more capitalized treated banks. The same holds for lending to firms. The respective coefficients on the $TREAT_{it} \times REG.CHANGE_t \times EQ_{it}$ variable are negative and significant. Regarding household loans, I obtain the opposite result: treated banks with relatively less capital—while increasing loans to firms—were decreasing loans to households by more than did treated banks with relatively more capital. *Fourth*, regarding the banking system concentration, I again obtain mixed evidence, as in the previous section. However, now the sign of the coefficient on the $TREAT_{it} \times REG.CHANGE_t \times HHI_t$ variable coincides with the sign of respective mean effect, implying that the observed increase of the banking system concentration was amplifying the treated banks' reduction in firm deposits and expansion of household deposits and loans to firms.

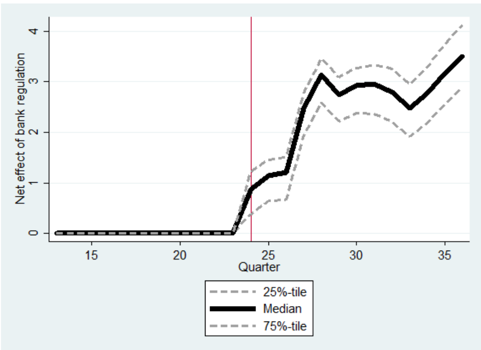
I plot the time evolution of the estimated composition effects and perform the decomposition exercise. The full results on the time evolution are reported in Appendix 4.D (see Fig. 4.2). Below, I analyze only the two most important effects—on treated banks' household deposits and firm loans (see Fig. 4.5).



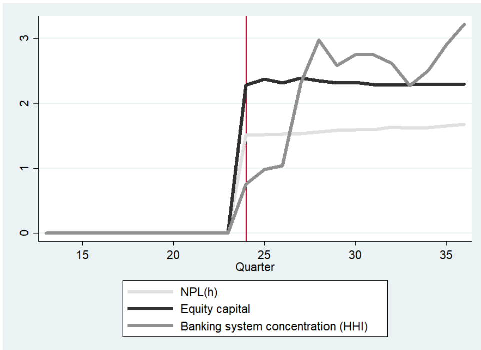
(a) Loans to non-financial firms / total assets (*aggregated effect*)



(b) Loans to non-financial firms / total assets (*disaggregated effects*)



(c) Deposits of households / total assets (*aggregated effect*)



(d) Deposits of households / total assets (*disaggregated effects*)

Figure 4.5: Time evolution of the assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

Overall, as in the previous section with channels of the scale effects, I again observe the same two results. First, the composition effects of tightened regulation on treated banks' household deposits and firm loans strengthened in time after mid-2013 (see Fig. 4.5a,c). Second, bank capital plays either the most important role in transmitting these effects (4.5b) or, at least, is as important as banking concentration (4.5d). Quantitatively, the exercise demonstrates that during the three years after the regulatory tightening, treated banks might have increased the share of firm loans in their total assets by as much as 4 percentage points, and increased the share of household deposits in their total liabilities by 3.5 percentage points (median estimates).

4.4.4 Macroeconomic implications of tightened bank regulation

Having established that tightened bank regulation had significant scale and composition effects at the treated (misreporting) bank level, I now evaluate the macroeconomic implications of these effects. The range of SVAR-estimated elasticities of output with respect to loan volumes—1.52 to 1.86 (see Appendix 1.H)—provides a bridge between the micro part of the chapter and evaluation of the macroeconomic implications of the tightened bank regulation. Recall from the estimated scale effects of the tightened regulation that the treated banks might have reduced their supply of loans to households by as much as 3.9 billion rubles and to firms by 3.0 billion rubles within the three years after mid-2013 (see Table 4.3 in Section 4.4.2). Recall also that I applied an agnostic regulation rule ($\theta = 0.5$), according to which the CBR audits half of the banking system each quarter: the banks with estimated probabilities of being audited ($\hat{s}_{it}$ from selection equation (4.1) of the Heckman model) exceeding the median at each respective quarter. This results in 455 banks being audited each quarter.

To evaluate the macroeconomic effects of tightened bank regulation, I multiply the estimated elasticities by the average credit supply reductions and by the average number of banks to be audited, and obtain the following results. First, Russia's GDP might have contracted by 2.6–3.2% (or by 2,075–2,539 billion rubles) through the channel of *corporate* credit supply reduction by fraudulent banks.[33] Second, Russia's GDP might have contracted by another 3.2–4.1% (or by 2,697–3,301 billion rubles) through the channel of *household* credit supply decline by fraudulent banks. Needless to say, these are considerable numbers, reflecting the price of removing fraud from the banking system.

In reality, no contractions occurred, however, and this is likely because of the positive effects I am not estimating here. These effects are related to the potential acceleration of operations by those banks that survived after the regulatory inspections. If a bank survives, outside investors are likely to be more confident in this bank and supply more

[33]The average volume of nominal GDP in 2014–2016 equaled 80,180 billion rubles. This is equivalent to 1,618 billion US dollars (using the average dollar-to-ruble exchange course for the same period, 49.57).

funds at lower prices. This may boost the growth of the bank's assets, including credit, and compensate for the lost credit and the negative effects on macroeconomic dynamics due to the policy-induced shrinkage in the number of operating banks. Suffice it to mention at this stage that the ratio of the total banking system's assets to GDP increased, not decreased, over the years of the tight policy from 70 to 90%.

4.5 Sensitivity analysis

I run a battery of robustness checks, including a variation of the regulation rule applied ($\theta = 0.25$ and $\theta = 0.75$), changing the implied regulation type (degree of regulatory suspicion), matching treated banks with nearest neighbors within non-treated banks, simplifying the Heckman selection model to achieve greater generalizability, and finally switching from the Heckman model of the regulation rule to an alternative based on a popular statistical measure of bank soundness extensively used in banking literature (Z-score). In each case, I re-run all DID regressions and thus re-estimate every scale and composition effect of tightened regulation. Overall, the results survive.

4.5.1 Regulation rule

In the main text, I was agnostic regarding the fraction of banks the regulator audits each period, i.e., I set $\theta = 0.5$, meaning that banks with estimated probabilities of being audited above the median (across all banks in a given quarter) are treated as potentially misreporting by the regulator and are thus under threat of activity restrictions. In this section, I deviate from this rule by first decreasing the fraction of audited banks and then by increasing it. Because it is difficult to justify any particular number, I choose $\theta = 0.25$ in the first case and $\theta = 0.75$ in the second, which together embrace the standard interquartile range. When $\theta = 0.25$, it means that the regulator is more concerned with the state of misreporting in the system and audits a bank i if $\hat{s}_{it}$ is greater than the 25th percentile of the banks' distribution by $\hat{s}$ in a given quarter. $\theta = 0.75$ thus implies a less concerned regulator auditing a bank i if respective $\hat{s}_{it}$ is greater than the 75th percentile.

The estimation results on the scale effects of tightened regulation appear in Table 4.1 (see Appendix 4.E). In this table, I report only the estimated coefficients on the $TREAT_{it} \times REG.CHANGE_t$ variable (the rest of the controls used are the same as in the main text, but are not reported to save space). The first three columns report the results with $\theta = 0.25$, $\theta = 0.50$, and $\theta = 0.75$ on the extensive margin, and the last three columns do the same on the intensive margin. By rows, the table contains six panels, one for each of the scale-dependent variables. Overall, the estimates suggest that my results are robust to varying regulations. All estimated DID coefficients remain negative, implying that tightened regulation forces treated banks to shrink their activities in absolute terms, and are highly statistically significant in almost all cases. Qualitatively, I obtain the result that the more concerned the regulator is (i.e., the lower the θ), the greater the shrinkage of the treated banks' balance sheets will be.

Table 4.2 reports the estimation results on the composition effects of tightened regulation on the asset and liability structure of the treated banks' balance sheets. I again observe that the effects concentrate in the panels with household deposits to total assets ratio and the firm loans to total assets ratio. Both ratios are rising, as in the main text, irrespective of the choice of θ. Again, the more concerned the regulator is, the stronger the composition effect will be.

4.5.2 Regulation type

In the main text, I assumed that when running its prudential regulation, the CBR is not suspicious (has no negative memory) in the sense that if the regulation rule applied in period $t+1$ shows that a period-t misreporting bank is no longer identified as misreporting, the regulator has no reason to audit the bank. In this section, I deviate from this image of regulation to those implying more suspicion from the regulator's side. I assume that a period-t misreporting bank, despite no longer being identified by the formal rule as misreporting at period $t + 1$ onwards, is still treated by the regulator as misreporting for at least four periods in the future (up to $t+4$, "suspicious regulation") or forever (to the end of the sample period, for concreteness; "most suspicious regulation"), and that all activity restrictions remain in place.

The re-estimated scale effects of tightened regulation appear in Table 4.1 (see Appendix 4.F). The table has fully the same structure as in the previous section, except now I place the assumed regulatory suspicion by columns from least to most suspicious. The estimated DID coefficients are all negative, as in the main text, and statistically significant. Quantitatively, the more suspicious the regulator could be, the stronger the negative scale effect becomes. The results are robust to a particular assumption on the degree of regulatory's suspicion.

The re-estimated composition effects of tightened regulation are reported in Table 4.2 for the asset-liability structure of the treated banks' operations. Nothing new appears in these tables. Irrespective of the degree of assumed regulatory suspicion, the treated banks increase both their household deposits-to-assets ratios and firm loans-to-assets ratios, and decrease their firm deposits-to-assets ratios. The more suspicious the regulator is, the greater the effect.

4.5.3 Matching

In the main text, I run DID regressions on an unmatched sample of treated and control banks. Given the chosen baseline regulatory rule $\theta = 0.5$, this unmatched sample consists of almost the same quantity of treated and control banks. Though I control my regression estimates on a large set of bank-specific characteristics and bank and quarter FEs, some important differences could still exist. In this section, I apply the bias-adjusted matching estimator of Abadie and Imbens (2011), with which I construct 1-to-1 matched samples of banks. Because in the baseline estimates with $\theta = 0.5$ I have a slightly larger number of control banks, the first matched sample is constructed under the $\theta = 0.5$ rule. The second matched sample is then constructed under an assumption of a less concerned regulator, i.e., $\theta = 0.75$, which effectively shrinks the sample size in the DID regression by twofold.

Matching under the third rule considered, i.e., = 0.25, is impossible for obvious reasons. I re-run all DID regressions on the two constructed matched samples.

The estimation results with the scale effects of tightened regulation appear in Table 4.1 (see Appendix 4.G). The structure of the table is again the same as in the two previous sections, except that now I locate unmatched regression results in the first column (for comparisons) and matched regression results under $\theta = 0.5$ and $\theta = 0.75$ in the second and third columns, respectively. The results clearly show that, again, nothing changes qualitatively. This is expected in the $\theta = 0.5$ case, but not that much under the = 0.75 case, due to a substantially smaller number of observations. I again find that the less concerned the regulator is, the less strong the scale effect becomes, in each of the six panels of the table, though it remains significant.

When I consider the composition effects of tightened regulation, I again find no qualitative changes—for the asset-liability structure of the treated banks' balance sheets (Table 4.2).

Overall, the matching exercise confirms the results from the main text.

4.5.4 A more parsimonious Heckman selection model and a different identification of selection

In the main text, I specify an extended version of the Heckman selection model, i.e., I consider not only standard predictors of the bank in distress, such as capitalization, liquidity, profitability, etc. (in line with the CAMEL approach), but more specific characteristics of bank business profile (inter-bank market, rollovers of various types of loans, and so on). In this section, I step back to a more traditional set of determinants and re-estimate the Heckman selection model and all DID regressions covering the scale and composition effects of declining regulatory forbearance.

The estimation results on the more compact version of the Heckman selection model appear in Table 4.1 (see Appendix 4.H). Qualitatively, I still observe that, across all periods of estimation, bank capital reduces both the probability of being audited and the size of losses, as measured by HNC, conditional on being detected. Other variables still deliver mixed effects, depending on the particular quarter of estimation. The bank size variable delivers a negative sign, statistically significant, across all periods, as in the main text. The estimated ρ coefficient, reflecting the correlation between the selection and outcome regressions' errors is positive and significant, but only after mid-2013, while in the main text, it was significant in mid-2013 as well.

With this re-estimated version of the Heckman selection model, I further report the re-estimated DID regressions, in which I assume the same structure of regulatory decision-making (i.e., $\theta = 0.5$ and no negative memory of the regulator), as in the main text. Table 4.2 in Appendix 4.H reports the results on the scale effect of tightened regulation. The structure of the table is again the same as in the previous section, but now the columns compare the baseline estimates with those obtained here. For instance, the newly estimated coefficient on the $TREAT_{it} \times REG.CHANGE_t$ variable implies that treated banks could be forced to reduce their total assets by 21 billion rubles compared to non-treated banks on average within the three years after mid-2013 (significant at 1%). This is quantitatively similar to those obtained with the baseline specification. The same applies to the rest of the five scale variables in the table. Overall, the estimated scale effects are larger than those in the main text.

I next shift to the re-estimated composition effects on the asset and liability structure of the treated banks' balance sheets (see Table 4.3 in Appendix 4.H), and find that all the results from the main text are still confirmed. Moreover, with the compact version of the Heckman model, I obtain significant effects on variables that were insignificant before. In particular, treated banks were likely to reduce their owned funds as a share of total assets both on extensive and intensive margins, which implies a negative consequence of tightened regulation. Further, unlike in the main text, treated banks could turn to decreasing the weights of firm deposits and household loans, again on extensive and intensive margins.

Finally, as discussed in the methodology section, the bank size variable was replaced with the binary indicator of whether a bank has losses in quarter t to identify selection. The estimated coefficient appears to be positive and highly statistically significant, implying that losses attract regulatory attention and thus the probability of being audited rises. The re-estimated DID regressions deliver no qualitative changes. The results are not reported to save space and are available upon request.

4.5.5 Why Heckman and not Z-score?

In the main text, I assume the regulator applies the Heckman selection model to detect misreporting banks. One could argue that there are more straightforward ways to meet this purpose. In particular, a very popular metric, the Z-score of bank soundness, could be applied to separate fraudulent from healthy banks, as is done in, e.g., DeYoung and Torna (2013). The Z-score is measured as a sum of the bank capital-to-assets ratio and monthly profit-to-assets ratio (ROA) divided by the standard deviation of ROA (three years moving average suggested by the literature). The Z-score is an upper-bound measure of a bank's overall stability that equals the number of deviations by which the bank's ROA should fall so that the resultant losses world fully destroy the bank's capital. The indicator stems from applying Chebyshev's inequality to measure the probability of a bank facing negative capital.

My essential reason for choosing Heckman's approach instead of the Z-score is that I can adjust it to account for the bank misreporting phenomenon so that I can trust a bank balance sheet's information only until the bank is not selected into the group of misreporting banks (the treatment group). In this respect, the Z-score could be less preferable simply because it does not contain any information on already revealed misreporting, only the information from (possibly falsified) balance sheets. Nevertheless, here I apply the Z-score metric and (i) compute an alternative to Heckman's approach bank treatment indicator based on the Z-score, (ii) show the relationships between both versions of the treatment indicator, and (iii) re-run DID regressions with the treatment indicator based on the Z-score.

Having computed the Z-scores for each bank and each quarter in my sample and, based on that, having ranked the banks by their Z-score at each quarter, I begin reporting comparative descriptive statistics in Table 4.1 (see Appendix 4.I). By rows, in the first three panels of the table, I compare the treatment indicators based on the Z-score with those from the main text, i.e., based on the Heckman approach, for each of the three regulation rules considered ($\theta = [0.25, 0.50, 0.75]$). Here, I basically show that the number of treated banks is very much similar across the treatment indicators within each regulation rule.

Panel 4 presents the Z-score and size-adjusted Z-score for the full sample of banks.[34] I prefer the size-adjusted Z-score because the size and the Z-score are negatively associated, thus exhibiting the "too big to fail" phenomenon; because I aim to capture misreporting, which could be applied irrespective of bank size, I need to eliminate this concern. I find in this panel that, on average, a decline of monthly ROA by 50 standard deviations is able to fully deplete bank capital. Size adjustment renders the Z-score negative, and so I cannot interpret its levels in terms of standard deviations anymore, but I can still interpret its dynamics (the more the better). Finally, Panels 5 to 7 report the Z-scores themselves, the size-adjusted Z-scores, and (for subsequent comparisons) predicted losses, as measured with HNC, for treated banks across the three regulation rules. Across these three panels, the added value of adjusting Z-scores by bank size is clear: as θ grows, i.e., as the central bank checks banks with lower values of the Z-score, the mean value of size-adjusted Z-score declines from –3.2 in Panel 5 to –8.0 in Panel 7, whereas the mean value of the Z-score itself declines only marginally, if at all.

I further test the relationship between the size-adjusted Z-score and the baseline treatment indicator from the main text. The results appear in Table 4.2 (see Appendix 4.I). This table contains two panels by rows, one with results on the extensive margin and the other with those on the intensive margin. In the first panel, I perform probit estimates

[34] I run a regression of Z-scores on the bank size variables and bank FEs and quarter FEs and extract the estimated residuals. I find the coefficient on the size variable to be negative and highly significant.

with the baseline treatment indicator as the dependent variable. Columns (1)–(3) contain the marginal effects of the size-adjusted Z-score on the probability of being treated under the three regulation rules, $\theta = [0.25, 0.5, 075]$, respectively. Each of the three marginal effects is multiplied by a one standard deviation of the size-adjusted Z-score (36.95, in the full sample). As expected, banks with higher Z-scores are less likely to be treated under the baseline definition (significant at 1% for the $\theta = 0.50$ and $\theta = 0.75$ regulation rules). I further transform the size-adjusted Z-score into a binary indicator that equals 0 for banks with the highest 25%, 50%, or 75% of all observable values of Z-scores in a given quarter and 1 for the rest of the banks, respectively. Columns (4)–(6) present the marginal effects of being treated under the Z-score definition on the probability of being treated under Heckman's (*baseline*) definition. In these columns, the banks that are likely to be treated under the Z-score definition are also more likely to be treated under Heckman. It is clear that the two approaches are consistent with each other in the full sample within the six years around the regulatory tightening. The results in the second panel of the table provide qualitatively the same conclusions.

Finally, I re-run the DID regressions, estimating the scale effects of regulatory tightening (see Table 4.3 in Appendix 4.I) as well as the composition effects (Table 4.4). Overall, I still achieve the same outcomes as in the main text, with somewhat lower magnitudes of the scale effects than in the main text and sometimes lower or larger composition effects compared to the baseline. This exercise largely supports the use of the Heckman selection approach to determining misreporting banks.

4.6 Conclusion

The results indicate that central banks can effectively detect banks engaged in misreporting their balance sheets and restrict their activities on both the extensive and intensive margins. Banks are likely to pursue a misreporting strategy when they are experiencing negative shocks, e.g., to the quality of their assets, that are sufficient to push their capital down to well below the minimum levels required by the central bank. The banks thus artificially increase the quality of their assets to avoid additional losses, and to continue to satisfy the capital regulation constraint. Of course, the banks pursue this strategy only if they evaluate their continuation value in the banking system as being greater than the outside option. Central banks understand this logic and may exercise forbearance of the losses of such banks in the future, in anticipation that the banks will experience positive shocks. This gives rise to a large degree of regulatory forbearance on the part of the central banks of advanced and emerging economies. This chapter provides a unique example of an emerging economy (Russia), in which the central bank, after a decade of excessive forbearance, switched to a very tight regulation policy of detecting misreporting banks and revoking their licenses, thus cleaning the banking system. I also show that this policy had a meaningful macroeconomic effect: by forcing fraudulent banks to stop their lending to the economy, Russia's GDP might have lost roughly 7% in a three-year horizon. This is the price the economy has to pay for removing fraud.

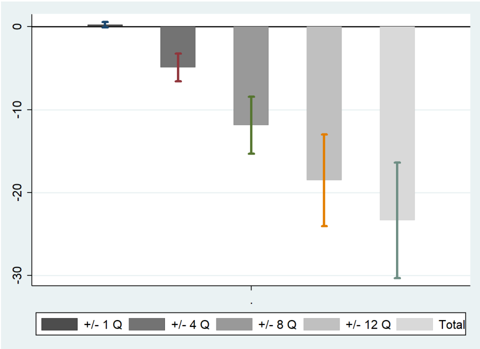
These results can provide input for a new theory of bank regulation that would bring together the possibility of rapidly declining regulatory forbearance and the risk of the regulator's reputation declining. Kang, Lowery, and Wardlaw (2015) show that a central bank could force active license revocation if the incurred monetary (short-run) and non-monetary (long-run) losses associated with a bank's closure are small enough. On the other hand, Morrison and White (2013) suggest that it is important to take the reputation risk of the central bank itself into consideration, to prevent contagion caused by runs of distrustful bank creditors. Finding a bridge between the two studies and my work here could be an important avenue for future research.

4.A Descriptive statistics at the bank level

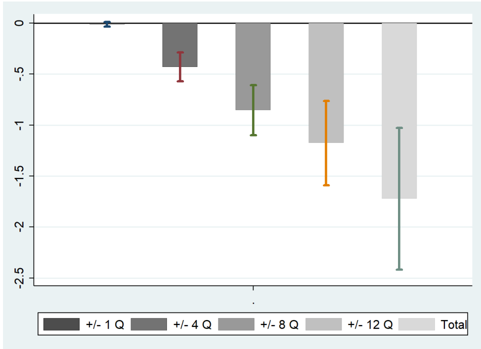
Table 4.1: Descriptive statistics: ±3 years around the regulatory tightening in mid-2013

Regulation type	Control group					Treatment group				
	N	Mean	SD	Min	Max	*N*	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel 1: The set of dependent variables										
Scale variables (billion rubles):										
Total assets	9,005	44.2	105.4	0.1	1415.5	8,691	5.5	10.1	0.1	264.5
Equity capital	9,005	4.6	10.2	–105.9	116.2	8,691	0.7	1.0	–1.1	31.4
firm deposits	9,005	9.9	29.2	0.0	622.1	8,691	1.2	2.8	0.0	120.5
Household deposits	9,005	12.7	31.0	0.0	364.6	8,691	2.3	4.2	0.0	69.0
Loans to firms	9,005	13.2	36.5	0.0	550.9	8,691	2.6	6.0	0.0	169.8
Loans to households	9,005	8.3	25.5	0.0	304.2	8,691	0.6	1.4	0.0	63.4
Composition variables (% of total assets):										
firm deposits	9,005	26.1	17.5	0.0	90.2	8,691	26.4	15.4	0.0	93.2
Household deposits	9,005	26.5	20.2	0.0	87.1	8,691	38.0	20.6	0.0	85.5
Loans to firms	9,005	27.6	17.4	0.0	92.7	8,691	42.1	18.5	0.0	96.2
Loans to households	9,005	16.6	17.2	0.0	94.8	8,691	15.2	12.7	0.0	87.8
% paid on firm deposits	7,692	6.5	2.9	0.1	19.7	7,660	7.0	3.0	0.1	19.7
% paid on household deposits	7,811	8.0	2.3	0.3	15.1	7,915	8.9	2.1	0.3	15.3
% received from loans to firms	8,799	13.5	3.8	2.6	32.9	8,669	14.9	2.9	2.9	32.6
% received from loans to households	8,865	15.8	5.0	3.5	43.6	8,625	15.6	4.0	3.2	43.7
Panel 2: The set of explanatory variables										
Equity capital / Total assets (%)	9,005	21.4	16.6	–19.0	97.6	8,691	18.1	11.4	–17.9	87.2
NPLs on firm loans (%)	9,005	6.9	14.3	0.0	100.0	8,691	3.1	5.5	0.0	100.0
NPLs on household loans (%)	9,005	6.0	9.7	0.0	100.0	8,691	7.0	11.7	0.0	100.0
Liquid assets / Total assets (%)	9,005	14.5	13.7	0.0	92.8	8,691	16.1	13.0	0.1	94.7
ROA (annualized, %)	9,005	1.8	2.9	–47.5	66.8	8,691	1.3	2.2	–16.2	26.7
Net interbank loans / Total assets (%)	9,005	2.8	12.2	–74.3	89.0	8,691	1.1	7.3	–71.6	62.2
Turnover of house.loans / Total assets (%)	9,005	2.1	3.2	0.0	61.8	8,691	2.0	2.6	0.0	52.7
Turnover of firms.loans / Total assets (%)	9,005	7.4	8.1	0.0	157.1	8,691	9.9	8.6	0.0	193.8
Growth of total assets (annualized, %)	9,005	24.6	54.7	–94.0	1540.5	8,691	22.9	40.7	–74.2	485.8
log of total assets	9,005	2.1	1.9	–2.5	7.3	8,691	1.0	1.1	–2.8	5.5

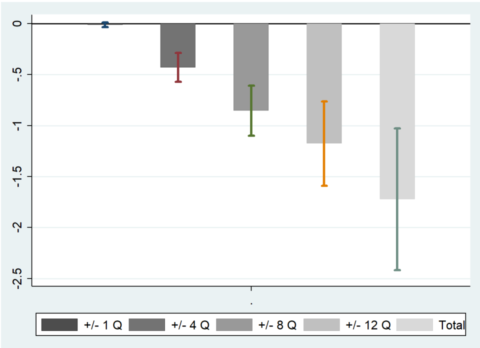
4.B Difference-in-differences estimates at different time windows



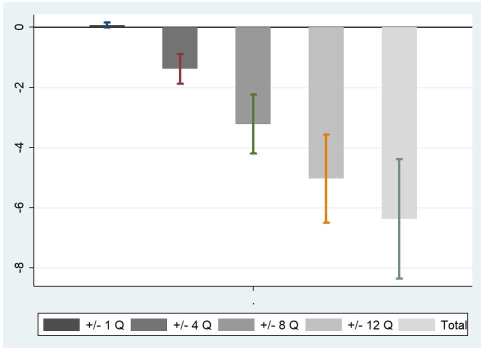
(a) Total assets TA_{it} $(n = 1)$



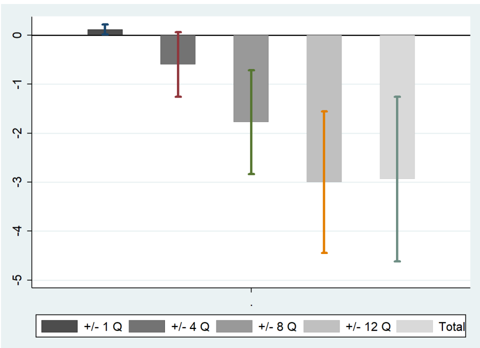
(b) Equity capital EQ_{it} $(n = 2)$



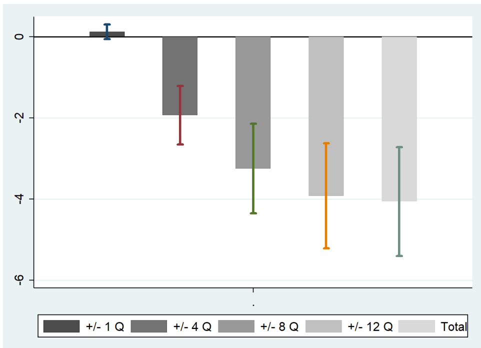
(c) Deposits of firms $DEPf_{it}$ $(n = 3)$



(d) Deposits of households $DEPh_{it}$ $(n = 4)$

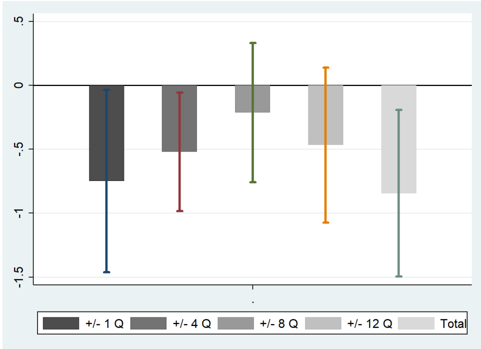


(e) Loans to firms $LNSf_{it}$ $(n = 5)$

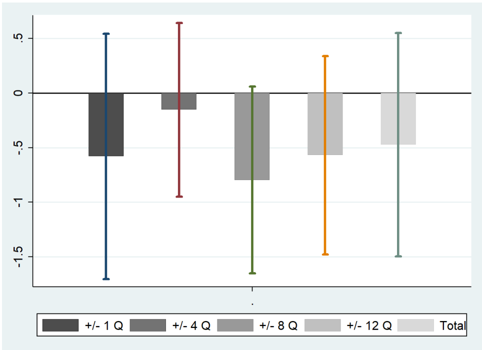


(f) Loans to households $LNSh_{it}$ $(n = 6)$

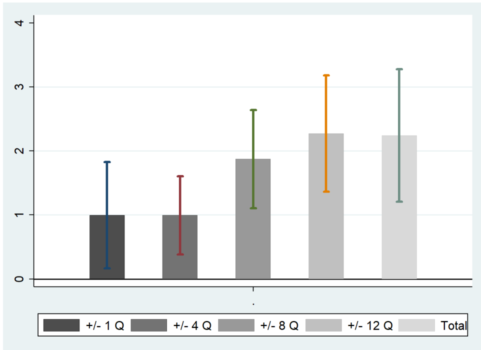
Figure 4.1: The scale effects of tightened prudential regulation over different estimation windows in DID regressions



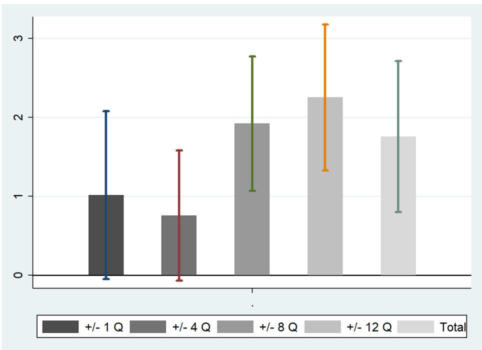
(a) Equity capital to total assets EQ_{it}/TA_{it} $(n = 1)$



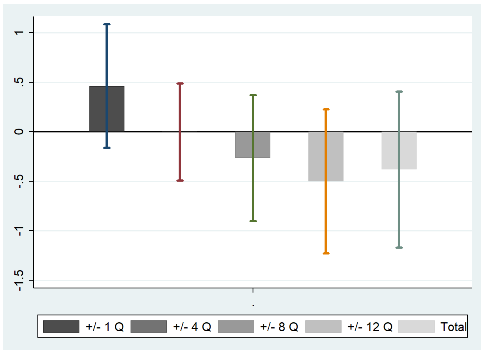
(b) Deposits of non-financial firms to total assets $DEPf_{it}/TA_{it}$ $(n = 2)$



(c) Deposits of households to total assets $DEPh_{it}/TA_{it}$ $(n = 3)$



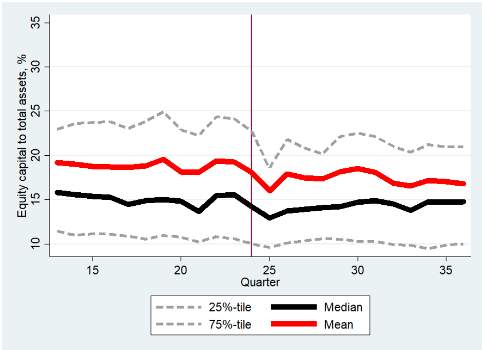
(d) Loans to non-financial firms to total assets $LNSf_{it}/TA_{it}$ $(n = 4)$



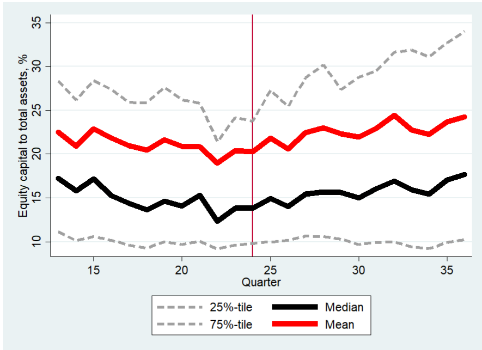
(e) Loans to households to total assets $LNSh_{it}/TA_{it}$ $(n = 5)$

Figure 4.2: The assets and liability composition effects of tightened prudential regulation over different estimation windows in DID regressions

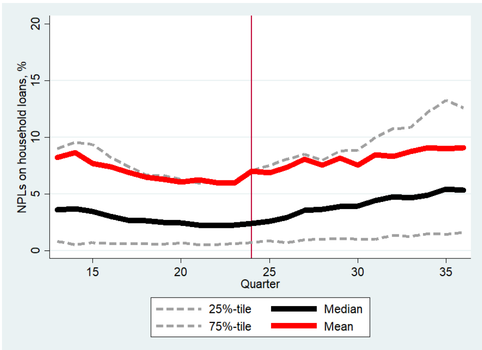
4.C Trends in the data on bank capital, NPLs, and banking system concentration



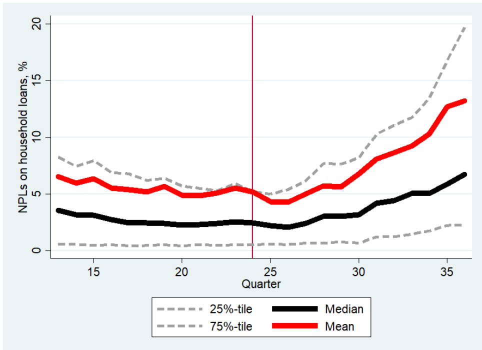
(a) Treated banks: Equity capital to total assets



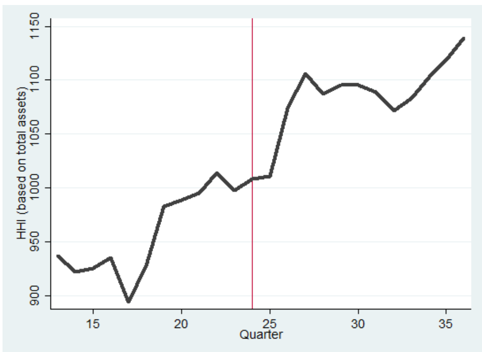
(b) Control banks: Equity capital to total assets



(c) Treated banks: NPLs on household loans



(d) Control banks: NPLs on household loans

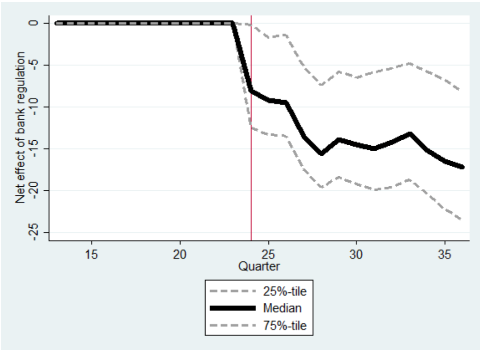


(e) Herfindahl-Hirschman index (HHI)

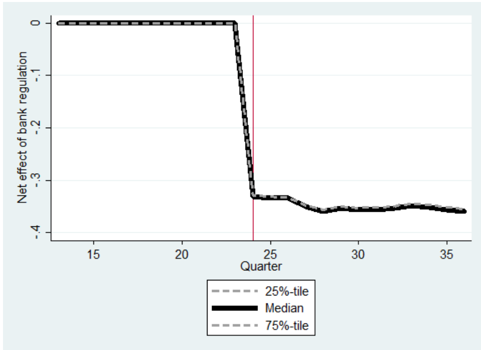
Note: Vertical red line crosses the 24th quarter of our sample, which corresponds to 2013 Q2, i.e., the beginning of the Nabiullina's tightened prudential regulation.

Figure 4.1: Equity capital to total assets ratio, NPLs on household loans, and the banking system's concentration (HHI) around the regulatory change in mid-2013

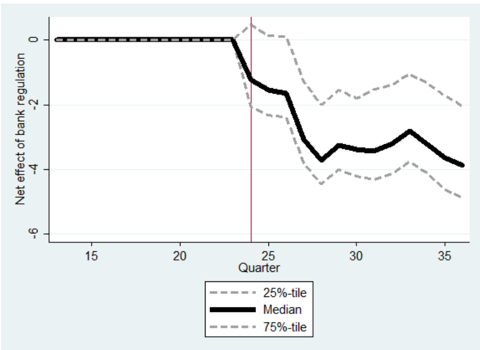
4.D Channels of the effects of tightened bank regulation



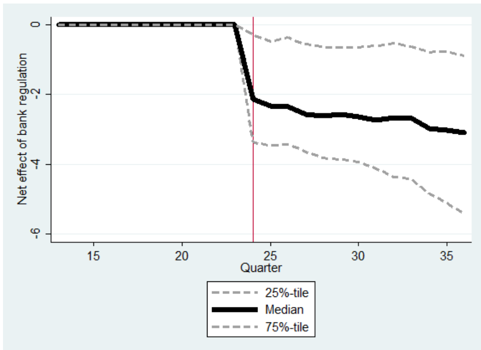
(a) Total assets TA_{it} ($n = 1$)



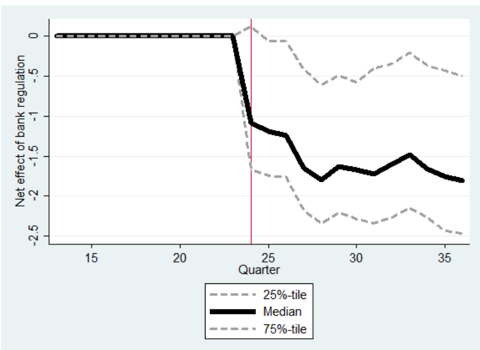
(b) Equity capital EQ_{it} ($n = 2$)



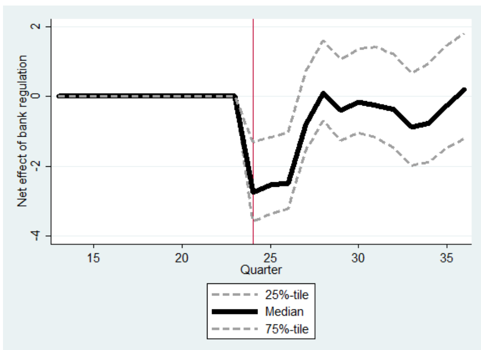
(c) Deposits of firms $DEPf_{it}$ ($n = 3$)



(d) Deposits of households $DEPh_{it}$ ($n = 4$)

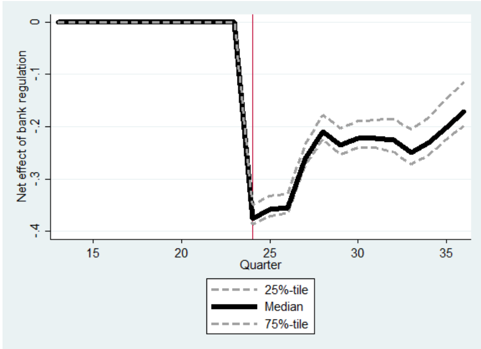


(e) Loans to firms $LNSf_{it}$ ($n = 5$)

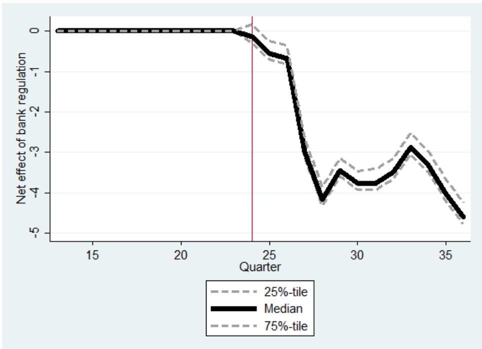


(f) Loans to households $LNSh_{it}$ ($n = 6$)

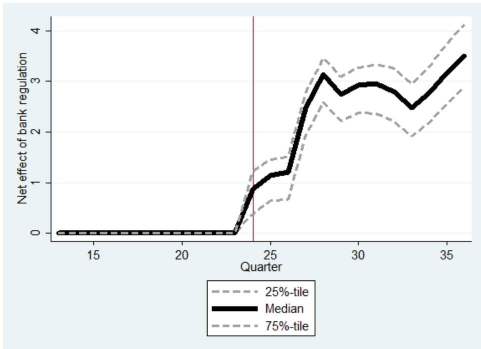
Figure 4.1: Time evolution of the scale effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013



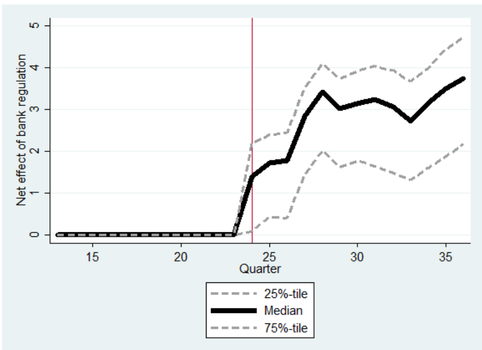
(a) Equity capital to total assets EQ_{it}/TA_{it} $(n = 1)$



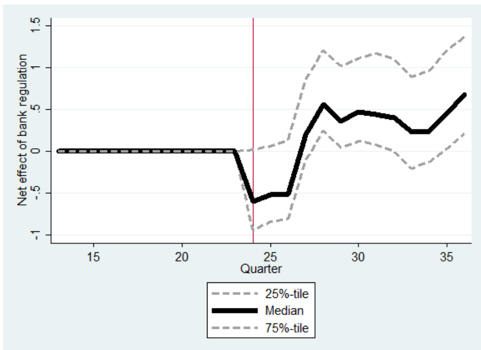
(b) Deposits of non-financial firms to total assets $DEPf_{it}/TA_{it}$ $(n = 2)$



(c) Deposits of households to total assets $DEPh_{it}/TA_{it}$ $(n = 3)$



(d) Loans to non-financial firms to total assets $LNSf_{it}/TA_{it}$ $(n = 4)$



(e) Loans to households to total assets $LNSh_{it}/TA_{it}$ $(n = 5)$

Figure 4.2: Time evolution of the assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

4.E Regulation rule

Table 4.1: Scale effects of declining regulatory forbearance:
±3 years around the regulatory tightening in mid-2013

	Extensive margin			Intensive margin		
	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = the absolute size of a bank's total assets TA_{it}						
TREAT×REGIME	–28.601***	–18.521***	–13.674***	–0.278***	–0.303***	–0.261***
	(4.921)	(2.824)	(2.039)	(0.066)	(0.053)	(0.045)
Panel 2: Dependent variable = the absolute size of a bank's equity capital EQ_{it}						
TREAT×REGIME	–1.912***	–1.176***	–0.975***	–0.019***	–0.019***	–0.019***
	(0.356)	(0.210)	(0.166)	(0.005)	(0.004)	(0.004)
Panel 3: Dependent variable = the absolute size of a bank's deposits of non-financial firms $DEPf_{it}$						
TREAT×REGIME	–5.304***	–3.278***	–2.584***	–0.059***	–0.057***	–0.053***
	(1.632)	(0.885)	(0.666)	(0.022)	(0.017)	(0.015)
Panel 4: Dependent variable = the absolute size of a bank's deposits of households $DEPh_{it}$						
TREAT×REGIME	–7.313***	–5.032***	–4.175***	–0.067***	–0.081***	–0.080***
	(1.266)	(0.746)	(0.610)	(0.018)	(0.015)	(0.014)
Panel 5: Dependent variable = the absolute size of a bank's loans to non-financial firms $LNSf_{it}$						
TREAT×REGIME	–3.757***	–3.001***	–2.798***	–0.018	–0.043***	–0.050***
	(1.312)	(0.736)	(0.649)	(0.018)	(0.015)	(0.015)
Panel 6: Dependent variable = the absolute size of a bank's loans to households $LNSh_{it}$						
TREAT×REGIME	–6.613***	–3.922***	–2.908***	–0.072***	–0.064***	–0.057***
	(1.168)	(0.661)	(0.487)	(0.014)	(0.012)	(0.010)

Note: The table re-performs the estimations reported in Table 4.3 from the main text with the use of different regulation rules, as reflected in $\theta = [0.25, 0.5, 0.75]$. For example, $\theta = 0.25$ implies that the regulator applies the cut-off threshold equal to 25% for the estimated probability of being selected into the group of misreporting banks: below the threshold, the banks are treated as healthy (non-misreporting), above it—as fraudulent (misreporting). All the notes from the reference table apply. In all regressions, N obs. = 17,696 and N banks = 910.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Table 4.2: The assets and liabilities composition effects of declining regulatory forbearance:
±3 years around the regulatory tightening in mid-2013

	Extensive margin			Intensive margin		
	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = equity capital to total assets ratio EQ_{it}/TA_{it}						
TREAT×REGIME	–0.547	–0.467	–0.544*	–0.007	–0.014**	–0.020**
	(0.400)	(0.309)	(0.324)	(0.006)	(0.007)	(0.009)
Panel 2: Dependent variable = deposits of non-financial firms to total assets ratio $DEPf_{it}/TA_{it}$						
TREAT×REGIME	–1.188**	–0.573	0.288	–0.013	–0.018**	–0.003
	(0.532)	(0.463)	(0.519)	(0.009)	(0.009)	(0.012)
Panel 3: Dependent variable = deposits of households to total assets ratio $DEPh_{it}/TA_{it}$						
TREAT×REGIME	2.447***	2.280***	1.776***	0.034***	0.053***	0.051***
	(0.526)	(0.462)	(0.531)	(0.009)	(0.010)	(0.012)
Panel 4: Dependent variable = loans to non-financial firms to total assets ratio $LNSf_{it}/TA_{it}$						
TREAT×REGIME	2.830***	2.259***	2.440***	0.046***	0.063***	0.082***
	(0.498)	(0.470)	(0.546)	(0.009)	(0.010)	(0.015)
Panel 5: Dependent variable = loans to households to total assets ratio $LNSh_{it}/TA_{it}$						
TREAT×REGIME	–0.378	–0.494	–0.775**	0.001	–0.001	–0.011
	(0.441)	(0.372)	(0.388)	(0.007)	(0.007)	(0.010)

Note: The table re-performs the estimations reported in Table 4.4 from the main text with the use of different regulation rules, as reflected in $\theta = [0.25, 0.5, 0.75]$. For example, $\theta = 0.25$ implies that the regulator applies the cut-off threshold equal to 25% for the estimated probability of being selected into the group of misreporting banks: below the threshold, the banks are treated as healthy (non-misreporting), above it—as fraudulent (misreporting). All the notes from the reference table apply. In all regressions reported, N obs. = 17,696 and N banks = 910.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

4.F Regulation type

Table 4.1: Scale effects of declining regulatory forbearance: $\pm$3 years around the regulatory tightening in mid-2013

Regulator's suspicion within	Extensive margin $[t, t+1]$	Extensive margin $[t, t+4]$	Extensive margin $[t, t+\infty]$	Intensive margin $[t, t+1]$	Intensive margin $[t, t+4]$	Intensive margin $[t, t+\infty]$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = the absolute size of a bank's total assets TA_{it}						
TREAT×REGIME	–18.521***	–22.839***	–26.689***	–0.303***	–0.284***	0.021
	(2.824)	(3.455)	(5.617)	(0.053)	(0.052)	(0.098)
Panel 2: Dependent variable = the absolute size of a bank's equity capital EQ_{it}						
TREAT×REGIME	–1.176***	–1.498***	–1.661***	–0.019***	–0.020***	0.003
	(0.210)	(0.242)	(0.405)	(0.004)	(0.004)	(0.007)
Panel 3: Dependent variable = the absolute size of a bank's deposits of non-financial firms $DEPf_{it}$						
TREAT×REGIME	–3.278***	–3.915***	–3.814***	–0.057***	–0.052***	0.020
	(0.885)	(1.038)	(1.239)	(0.017)	(0.017)	(0.032)
Panel 4: Dependent variable = the absolute size of a bank's deposits of households $DEPh_{it}$						
TREAT×REGIME	–5.032***	–6.599***	–8.685***	–0.081***	–0.083***	–0.019
	(0.746)	(0.975)	(1.711)	(0.015)	(0.015)	(0.025)
Panel 5: Dependent variable = the absolute size of a bank's loans to non-financial firms $LNSf_{it}$						
TREAT×REGIME	–3.001***	–3.523***	–3.145**	–0.043***	–0.039***	0.028
	(0.736)	(0.881)	(1.423)	(0.015)	(0.014)	(0.024)
Panel 6: Dependent variable = the absolute size of a bank's loans to households $LNSh_{it}$						
TREAT×REGIME	–3.922***	–5.199***	–7.771***	–0.064***	–0.067***	–0.043**
	(0.661)	(0.903)	(1.647)	(0.012)	(0.012)	(0.018)

Note: The table re-performs the estimations reported in Table 4.3 from the main text with the use of different regulation types, as reflected in a horizon within which the regulator audits a fragile bank: $[t, t+1]$ (baseline, least suspicious regulation), $[t, t+4]$ (suspicious regulation) or $[t, t+\infty]$ (most suspicious regulation). All the notes from the reference table apply. In all regressions, N obs. = 17,696 and N banks = 910.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Table 4.2: The assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

	Extensive margin			Intensive margin		
Regulator's suspicion within	$[t, t+1]$	$[t, t+4]$	$[t, t+\infty]$	$[t, t+1]$	$[t, t+4]$	$[t, t+\infty]$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = equity capital to total assets ratio EQ_{it}/TA_{it}						
TREAT×REGIME	–0.467	0.099	0.870	–0.014**	0.000	0.023***
	(0.309)	(0.370)	(0.548)	(0.007)	(0.007)	(0.007)
Panel 2: Dependent variable = deposits of non-financial firms to total assets ratio $DEPf_{it}/TA_{it}$						
TREAT×REGIME	–0.569	–1.252**	–1.470*	–0.019**	–0.025***	0.002
	(0.463)	(0.546)	(0.808)	(0.009)	(0.009)	(0.011)
Panel 3: Dependent variable = deposits of households to total assets ratio $DEPh_{it}/TA_{it}$						
TREAT×REGIME	2.270***	2.353***	2.117***	0.053***	0.043***	0.013
	(0.463)	(0.531)	(0.793)	(0.010)	(0.009)	(0.010)
Panel 4: Dependent variable = loans to non-financial firms to total assets ratio $LNSf_{it}/TA_{it}$						
TREAT×REGIME	2.250***	2.296***	0.838	0.064***	0.055***	–0.004
	(0.471)	(0.523)	(0.710)	(0.010)	(0.010)	(0.010)
Panel 5: Dependent variable = loans to households to total assets ratio $LNSh_{it}/TA_{it}$						
TREAT×REGIME	–0.500	–0.345	–0.813	–0.001	0.001	0.008
	(0.371)	(0.436)	(0.631)	(0.007)	(0.007)	(0.009)

Note: The table re-performs the estimations reported in Table 4.4 from the main text with the use of different regulation types, as reflected in a horizon within which the regulator audits a fragile bank: $[t, t+1]$ (baseline, least suspicious regulation), $[t, t+4]$ (suspicious regulation) or $[t, t+\infty]$ (most suspicious regulation). All the notes from the reference table apply. In all regressions, N obs. = 17,696 and N banks = 910.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

4.G Matching of treated and control banks

Table 4.1: Scale effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

	Extensive margin			Intensive margin		
	Unmatched	Matched		Unmatched	Matched	
	$\theta = 0.5$	$\theta = 0.5$	$\theta = 0.75$	$\theta = 0.5$	$\theta = 0.5$	$\theta = 0.75$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = the absolute size of a bank's total assets TA_{it}						
TREAT×REGIME	–18.521***	–17.091***	–12.393***	–0.303***	–0.269***	–0.200***
	(2.899)	(3.288)	(2.304)	(0.054)	(0.059)	(0.045)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875
Panel 2: Dependent variable = the absolute size of a bank's equity capital EQ_{it}						
TREAT×REGIME	–1.176***	–1.322***	–0.972***	–0.019***	–0.022***	–0.017***
	(0.216)	(0.266)	(0.186)	(0.004)	(0.005)	(0.004)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	905	888	910	905	888
Panel 3: Dependent variable = the absolute size of a bank's deposits of non-financial firms $DEPf_{it}$						
TREAT×REGIME	–3.278***	–4.089**	–2.620***	–0.057***	–0.069**	–0.047**
	(0.908)	(1.935)	(0.952)	(0.017)	(0.033)	(0.018)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875
Panel 4: Dependent variable = the absolute size of a bank's deposits of households $DEPh_{it}$						
TREAT×REGIME	–5.032***	–4.489***	–3.506***	–0.081***	–0.071***	–0.056***
	(0.766)	(0.767)	(0.680)	(0.015)	(0.015)	(0.013)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875
Panel 5: Dependent variable = the absolute size of a bank's loans to non-financial firms $LNSf_{it}$						
TREAT×REGIME	–3.001***	–3.774***	–2.972***	–0.043***	–0.054***	–0.043***
	(0.756)	(1.051)	(0.770)	(0.016)	(0.020)	(0.015)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875
Panel 6: Dependent variable = the absolute size of a bank's loans to households $LNSh_{it}$						
TREAT×REGIME	–3.922***	–2.533***	–1.886***	–0.064***	–0.038***	–0.031***
	(0.678)	(0.467)	(0.436)	(0.012)	(0.008)	(0.007)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875

Note: The table re-performs the estimations reported in Table 4.3 from the main text with the use of unmatched (baseline) and 1-to-1 matched samples of banks. Two matched samples are considered: one for the regulation rule $\theta = 0.5$ and the other for the rule $\theta = 0.75$. In the first case, half of all banks are in the treatment group, and I match each such bank with one counterpart from the control group. In the second case, only 25% of banks are treated (i.e., those with the probability of being selected above the 25%-tile of respective distribution), and I again find exactly one match from the control group. I perform matching using the Mahalanobis distance. I employ five bank-specific characteristics to match banks: (i) equity capital to total assets ratio (except Panel 2), (ii) NPLs ratio on loans to non-financial firms, (iii) NPLs ratio on loans to households, (iv) liquid assets to total assets ratio, and (v) ROA (annualized). In the rest, all the notes from the reference table apply.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Table 4.2: The assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

	Extensive margin			Intensive margin		
	Unmatched	Matched		Unmatched	Matched	
	$\theta = 0.5$	$\theta = 0.5$	$\theta = 0.75$	$\theta = 0.5$	$\theta = 0.5$	$\theta = 0.75$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Dependent variable = equity capital to total assets ratio EQ_{it}/TA_{it}						
TREAT×REGIME	–0.467	–0.739**	–0.408	–0.014*	–0.019**	–0.017*
	(0.317)	(0.351)	(0.404)	(0.007)	(0.007)	(0.009)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	905	888	910	905	888
Panel 2: Dependent variable = deposits of non-financial firms to total assets ratio $DEPf_{it}/TA_{it}$						
TREAT×REGIME	–0.573	0.114	0.630	–0.018*	–0.009	0.005
	(0.475)	(0.609)	(0.668)	(0.010)	(0.011)	(0.014)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875
Panel 3: Dependent variable = deposits of households to total assets ratio $DEPh_{it}/TA_{it}$						
TREAT×REGIME	2.280***	1.323***	1.328**	0.053***	0.034***	0.034***
	(0.474)	(0.495)	(0.610)	(0.010)	(0.010)	(0.012)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875
Panel 4: Dependent variable = loans to non-financial firms to total assets ratio $LNSf_{it}/TA_{it}$						
TREAT×REGIME	2.259***	2.288***	2.460***	0.063***	0.067***	0.067***
	(0.483)	(0.558)	(0.673)	(0.010)	(0.012)	(0.016)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875
Panel 5: Dependent variable = loans to households to total assets ratio $LNSh_{it}/TA_{it}$						
TREAT×REGIME	–0.494	–1.030***	–0.722*	–0.001	–0.011	–0.005
	(0.382)	(0.367)	(0.406)	(0.007)	(0.007)	(0.010)
N obs	17,696	17,382	8,540	17,696	17,382	8,540
N banks	910	902	875	910	902	875

Note: The table re-performs the estimations reported in Table 4.4 from the main text with the use of unmatched (baseline) and 1-to-1 matched samples of banks. Two matched samples are considered: one for the regulation rule $\theta = 0.5$ and the other for the rule $\theta = 0.75$. In the first case, half of all banks are in the treatment group, and I match each such bank with one counterpart from the control group. In the second case, only 25% of banks are treated (i.e., those with the probability of being selected above the 25%-tile of respective distribution), and I again find exactly one match from the control group. I perform matching using the Mahalanobis distance. I employ five bank-specific characteristics were used to match banks: (i) equity capital to total assets ratio (except Panel 2), (ii) NPLs ratio on loans to non-financial firms, (iii) NPLs ratio on loans to households, (iv) liquid assets to total assets ratio, and (v) ROA (annualized). In the rest, all the notes from the reference table apply.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

4.H A more compact version of the Heckman selection model of bank misreporting

Table 4.1: Cross-sectional Heckman selection estimates: ±3 years around the regulatory tightening in mid-2013 [a]

	3 years before 2013Q2		2013Q2		3 years after 2013Q2	
	Out	Sel	Out	Sel	Out	Sel
	(1)	(2)	(3)	(4)	(5)	(6)
Equity capital / Total assets	–0.045	–0.011*	–0.316	–0.019***	–0.623***	–0.037***
	(0.197)	(0.006)	(0.377)	(0.006)	(0.204)	(0.005)
NPLs on firm loans	–0.436	–0.010	–1.216	–0.006	–0.087	–0.015***
	(0.373)	(0.008)	(0.790)	(0.008)	(0.277)	(0.004)
NPLs on household loans	0.050	0.000	0.186	0.008*	–0.033	0.001
	(0.188)	(0.006)	(0.396)	(0.005)	(0.172)	(0.004)
Liquid assets / Total assets	–0.148	–0.013**	1.245***	0.002	0.291	–0.019***
	(0.197)	(0.006)	(0.327)	(0.005)	(0.196)	(0.004)
ROA (annualized)	–0.830	–0.033	–0.467	–0.046**	1.136**	0.040***
	(0.908)	(0.026)	(1.337)	(0.018)	(0.476)	(0.012)
Growth of total assets	–0.025	–0.007***	0.040	0.001	0.056	0.002*
	(0.113)	(0.002)	(0.068)	(0.001)	(0.036)	(0.001)
log of total assets		–0.175***		–0.255***		–0.354***
		(0.060)		(0.054)		(0.041)
Constant	8.789	–0.703***	–21.032	–0.599***	20.700***	1.215***
	(20.611)	(0.227)	(30.852)	(0.184)	(7.652)	(0.171)
N obs.	943		932		872	
N censored / observed	888 / 55		833 / 99		573 / 299	
Wald χ^2	3.160		17.455***		23.114***	
ρ	0.496		0.516		0.548***	

Note: The table reports efficient two-step estimates of the Heckman selection model for the three specific periods: 2013Q2, i.e., the time of regulatory change in the Central Bank of Russia, and three years before and after this date (recall that the estimation window in the baseline version of the difference-in-differences estimates equals ±3 years around mid-2013). Dependent variables are (i) an indicator variable of whether hidden negative capital, HNC, was detected by the CBR (columns "*Sel*") and (ii) the ratio of HNC to the equity capital reported one quarter before the closure (columns "*Out*"). *Sel* and *Out* are selection and outcome equations of the Heckman model. All explanatory variables are taken with a one-quarter lag. ρ is correlation between the regression errors of *Sel* and *Out*. *Wald* χ^2 is the Wald statistic that tests the null hypothesis that all coefficients equal zero simultaneously. *N censored* reflects all banks operating in the respective quarter for which the estimate is done. *N observed* accumulates all banks with HNC detected from the beginning of the sample, 2010Q2, to the respective quarter for which I perform an estimate.

[a] The rest of the estimates (i.e., for the other 44 quarters in the sample, 2010Q2 to 2019Q2) are not reported for the sake of brevity and are available upon request

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Robust standard errors appear in the brackets under the estimated coefficients.

Table 4.2: Scale effects of declining regulatory forbearance:
±3 years around the regulatory tightening in mid-2013

	Extensive margin		Intensive margin	
Heckman model:	Baseline	Additional	Baseline	Additional
	(1)	(2)	(3)	(4)
Panel 1: Dependent variable = the absolute size of a bank's total assets TA_{it}				
TREAT×REGIME	–18.521***	–20.846***	–0.303***	–0.520***
	(2.824)	(2.944)	(0.053)	(0.079)
Panel 2: Dependent variable = the absolute size of a bank's equity capital EQ_{it}				
TREAT×REGIME	–1.176***	–1.371***	–0.019***	–0.034***
	(0.210)	(0.211)	(0.004)	(0.006)
Panel 3: Dependent variable = the absolute size of a bank's deposits of non-financial firms $DEPf_{it}$				
TREAT×REGIME	–3.278***	–3.291***	–0.057***	–0.086***
	(0.885)	(0.844)	(0.017)	(0.024)
Panel 4: Dependent variable = the absolute size of a bank's deposits of households $DEPh_{it}$				
TREAT×REGIME	–5.032***	–5.873***	–0.081***	–0.149***
	(0.746)	(0.758)	(0.015)	(0.021)
Panel 5: Dependent variable = the absolute size of a bank's loans to non-financial firms $LNSf_{it}$				
TREAT×REGIME	–3.001***	–4.169***	–0.043***	–0.106***
	(0.736)	(0.754)	(0.015)	(0.021)
Panel 6: Dependent variable = the absolute size of a bank's loans to households $LNSh_{it}$				
TREAT×REGIME	–3.922***	–3.873***	–0.064***	–0.094***
	(0.661)	(0.644)	(0.012)	(0.016)

Note: The table compares the estimations reported in Table 4.3 from the main text (*Baseline*) with those obtained after switching to a more compact version of the Heckman selection model of bank misreporting (*Additional*). All the notes from the reference table apply. In all regressions, N obs. = 17,696 and N banks = 910.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Table 4.3: The assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

Heckman model:	Extensive margin		Intensive margin	
	Baseline	Additional	Baseline	Additional
	(1)	(2)	(3)	(4)
Panel 1: Dependent variable = equity capital to total assets ratio EQ_{it}/TA_{it}				
TREAT×REGIME	–0.467	–1.788***	–0.014**	–0.036***
	(0.309)	(0.312)	(0.007)	(0.009)
Panel 2: Dependent variable = deposits of non-financial firms to total assets ratio $DEPf_{it}/TA_{it}$				
TREAT×REGIME	–0.569	–0.800*	–0.019**	–0.029**
	(0.463)	(0.453)	(0.009)	(0.014)
Panel 3: Dependent variable = deposits of households to total assets ratio $DEPh_{it}/TA_{it}$				
TREAT×REGIME	2.280***	2.086***	0.053***	0.058***
	(0.462)	(0.486)	(0.010)	(0.013)
Panel 4: Dependent variable = loans to non-financial firms to total assets ratio $LNSf_{it}/TA_{it}$				
TREAT×REGIME	2.259***	0.995*	0.063***	0.022*
	(0.470)	(0.509)	(0.010)	(0.013)
Panel 5: Dependent variable = loans to households to total assets ratio $LNSh_{it}/TA_{it}$				
TREAT×REGIME	–0.494	–0.818**	–0.001	–0.021**
	(0.372)	(0.344)	(0.007)	(0.009)

Note: The table compares the estimations reported in Table 4.4 from the main text (*Baseline*) with those obtained after switching to a more compact version of the Heckman selection model of bank misreporting (*Additional*). All the notes from the reference table apply. In all regressions, N obs. = 17,696 and N banks = 910.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

4.I Relationship with bank Z-score

Table 4.1: Descriptive statistics of treatment group: ±3 years around the regulatory tightening in mid-2013

	N obs	N banks	Mean	SD	Min	Max
Panel 1: Regulation rule: $\theta = 0.25$						
Heckman-based treatment indicator	16,845	906	0.24	0.43	0	1
Z-score-based treatment indicator	16,845	906	0.22	0.42	0	1
Panel 2: Regulation rule: $\theta = 0.5$						
Heckman-based treatment indicator (*baseline*)	16,845	906	0.49	0.50	0	1
Z-score-based treatment indicator	16,845	906	0.49	0.50	0	1
Panel 3: Regulation rule: $\theta = 0.75$						
Heckman-based treatment indicator	16,845	906	0.74	0.44	0	1
Z-score-based treatment indicator	16,845	906	0.75	0.43	0	1
Panel 4 (for comparisons): the sample of all banks (treated and control)						
Z-score	16,845	906	48.64	43.82	0.22	272.28
Z-score, adjusted to bank size	16,845	906	–1.01	36.95	–65.62	405.68
HNC to total assets (predicted), $\text{HNC}_{0,1}$	16,845	906	15.64	21.77	0.00	444.44
Panel 5 (for comparisons): the subsample of treated banks under regulation rule $\theta = 0.25$						
Z-score	12,467	837	50.21	44.84	0.22	272.28
Z-score, adjusted to bank size	12,467	837	–3.17	35.67	–65.62	372.16
HNC to total assets (predicted), HNC_1	12,467	837	33.98	22.67	0.02	444.44
Panel 6 (for comparisons): the subsample of treated banks under regulation rule $\theta = 0.5$						
Z-score	8,313	733	49.65	44.78	0.46	272.28
Z-score, adjusted to bank size	8,313	733	–5.42	35.08	–65.62	372.16
HNC to total assets (predicted), HNC_1	8,313	733	31.70	21.26	0.05	444.44
Panel 7 (for comparisons): the subsample of treated banks under regulation rule $\theta = 0.75$						
Z-score	4,075	543	48.14	44.70	0.46	272.28
Z-score, adjusted to bank size	4,075	543	–8.02	35.44	–65.62	282.16
HNC to total assets (predicted), HNC_1	4,075	543	30.41	21.26	0.05	444.44

Note: The table contains descriptive statistics of (i) various versions of a binary indicator of the treatment group of banks (Panels 1–3) and (ii) Z-scores, both commonly used and adjusted for bank size, and predicted values of HNC computed for the full sample of banks (Panel 4) and for the three subsamples of treated banks corresponding to the regulation rules considered in the main text: $\theta = [0.25, 0.5, 075]$ (Panels 5–7). The Heckman-based treatment indicator relies on the "*hidden negative capital*" (HNC) concept and follows the Heckman selection model (4.1)–(4.2), the baseline approach in the text. The Z-score-based treatment indicator is based on bank rankings on their respective Z-scores, as in (DeYoung and Torna 2013), and additionally adjusted for bank size. The competing treatment indicators are reported for the three regulation rules. For example, for the Heckman-based treatment indicator, $\theta = 0.25$ implies that the regulator applies the cut-off threshold equaled 25% of the estimated probability of being selected into the group of misreporting banks: below the threshold, the banks are treated as healthy (non-misreporting), above it—as fragile (misreporting). For the Z-score-based treatment indicator, $\theta = 0.25$ means that, in a given quarter, all banks with the highest 25% of all values of Z-score are treated as healthy (non-misreporting) and the rest of banks—as fragile (misreporting).

Table 4.2: Comparison of HNC and Z-scores: complements?
±3 years around the regulatory tightening in mid-2013

Panel 1: Extensive margin

Dependent variable:	TREAT (HNC, *baseline*)					
Key explanatory variable X_{it}:	Z-score			TREAT (Z-score)		
Regulation rule:	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$
	(1)	(2)	(3)	(4)	(5)	(6)
X_{it}	–0.010	–0.019***	–0.022***	0.032***	0.010	0.029***
	(0.007)	(0.008)	(0.007)	(0.012)	(0.013)	(0.011)
N Obs.	16,800	16,800	16,800	16,845	16,845	16,845
N banks	906	906	906	906	906	906
Wald χ^2	210.9***	286.6***	324.5***	215.4***	277.3***	315.6***
log Likelihood	–5,660.1	–7,207.0	–5,711.5	–5,682.4	–7,236.4	–5,733.5

Panel 2: Intensive margin

Dependent variable:	Full sample: $HNC_{0,1}$			Subsample of treated banks: HNC_1		
Key explanatory variable X_{it}:	Z-score			Z-score		
Regulation rule:	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$	$\theta = 0.25$	$\theta = 0.5$	$\theta = 0.75$
	(1)	(2)	(3)	(4)	(5)	(6)
X_{it}	0.185	–0.074	–0.370	0.713*	0.912**	1.418*
	(0.443)	(0.406)	(0.333)	(0.428)	(0.421)	(0.744)
N Obs.	16,800	16,800	16,800	12,446	8,299	4,066
N banks	906	906	906	837	733	543
F-test	28.1***	12.3***	5.8***	53.1***	28.7***	9.9***
R^2_{within}	0.133	0.061	0.026	0.278	0.205	0.145

Note: The table contains regressions reflecting relationships between the Z-score adjusted for bank size and the estimated HNC at the bank level.

On the extensive margin (whether a bank is treated or not), in Panel 1 we perform probit estimates in columns (1)–(6). Columns (1)–(3) contain the marginal effects of the Z-score on the probability of being treated under the three regulation rules, $\theta = [0.25, 0.5, 075]$, respectively. Each of the three marginal effects is multiplied by the Z-score's one standard deviation (36.95, in the full sample). In columns (4)–(6), we further transform the Z-score into a binary indicator which equals 0 for the banks with the highest 25%, 50%, or 75% of all values of the Z-score in a given quarter and 1 for the respective rest of banks. We then present in columns (4)–(6) the marginal effects of being treated under the Z-score's definition of bank instability on the probability of being treated under the HNC (*baseline*) definition.

On the intensive margin (the size of HNC conditional on being treated), in Panel 2 we carry out two-way FE estimates in columns (1)–(6). Columns (1)–(3) show the relationship between the Z-score and the estimated HNC to total assets ratio in the full sample (all banks, i.e., treated and control), while columns (4)–(6) do the same for the subsample of treated banks only. The coefficients were multiplied by the Z-score's one standard deviation in the respective subsample.

All regressions include the full set of bank fixed effects (FE), quarter FE, and bank-specific characteristics.

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Table 4.3: Scale effects of declining regulatory forbearance: ± 3 years around the regulatory tightening in mid-2013

Dependent variable	TA_{it}	EQ_{it}	$DEPf_{it}$	$DEPh_{it}$	$LNSf_{it}$	$LNSh_{it}$
$Y_{it}^{(j)}$ $(j = 1...6)$:	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: treatment based on Z-score (adjusted on bank size)						
TREAT×REGIME	–10.881***	–1.075***	–2.713***	–3.565***	–2.993***	–2.371***
	(2.821)	(0.218)	(1.005)	(0.780)	(0.806)	(0.599)
TREAT	1.960	0.245*	0.977*	1.283**	0.924*	1.221***
	(1.799)	(0.136)	(0.535)	(0.535)	(0.549)	(0.408)
REGIME	27.601***	1.421***	0.499	2.988***	0.303	0.800
	(4.268)	(0.459)	(1.046)	(1.101)	(1.492)	(0.584)
N Obs.	17,696	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910	910
Panel 2: treatment based on HNC (baseline, for comparison)						
TREAT×REGIME	–18.521***	–1.176***	–3.278***	–5.032***	–3.001***	–3.922***
	(2.824)	(0.210)	(0.885)	(0.746)	(0.736)	(0.661)
TREAT	6.735***	0.405***	1.192***	2.089***	1.292***	1.694***
	(1.369)	(0.091)	(0.423)	(0.393)	(0.479)	(0.296)
REGIME	32.034***	1.573***	1.070	4.104***	0.569	1.887***
	(4.536)	(0.421)	(1.010)	(1.108)	(1.445)	(0.530)
N Obs.	17,696	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910	910

Note: The table contains difference-in-differences estimates of regression (4.6) with dependent variables $Y_{it}^{(j)}$ reflecting the size of total assets TA_{it} $(j = 1)$, equity capital EQ_{it} $(j = 2)$, deposits of non-financial firms $DEPf_{it}$ $(j = 3)$, deposits of households $DEPh_{it}$ $(j = 4)$, loans to non-financial firms $LNSf_{it}$ $(j = 5)$, loans to households $LNSh_{it}$ $(j = 6)$. All regressions include full sets of bank FE, quarter FE, and bank control variables, which are not reported for the sake of space and are available upon request. Mid-2013 marks the transition of the CBR to a new prudential regulation regime in which the CBR was no longer tolerant of fraudulent banks. The treatment group consists of all banks which are likely to be treated as fraudulent by the CBR (the treatment rule is proxied with the Heckman selection model (4.1)–(4.2)). The composition of the treatment and control groups varies in time depending on the application of the treatment rule in each quarter (see Section 4.2.2 for details). All regressions reflect results on the extensive margin (the size of HNC does not matter).

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Table 4.4: The assets and liabilities composition effects of declining regulatory forbearance: ±3 years around the regulatory tightening in mid-2013

Dependent variable	EQ_{it}/TA_{it}	$DEPf_{it}/TA_{it}$	$DEPh_{it}/TA_{it}$	$LNSf_{it}/TA_{it}$	$LNSh_{it}/TA_{it}$
$Y_{it}^{(j)}$ $(j = 1...5)$:	(1)	(2)	(3)	(4)	(5)
Panel 1: treatment based on Z-score (adjusted on bank size)					
TREAT×REGIME	0.098	–1.825***	1.109**	1.402**	–0.553
	(0.158)	(0.539)	(0.535)	(0.580)	(0.385)
TREAT	–1.741***	1.439***	0.128	–1.117**	–0.624**
	(0.161)	(0.427)	(0.445)	(0.488)	(0.308)
REGIME	0.864***	–7.059***	6.123***	–3.931***	0.415
	(0.298)	(0.824)	(0.897)	(0.941)	(0.642)
N Obs.	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910
Panel 2: treatment based on HNC (baseline, for comparison)					
TREAT×REGIME	–0.467	–0.569	2.270***	2.250***	–0.500
	(0.309)	(0.463)	(0.463)	(0.471)	(0.371)
TREAT	–1.374***	–1.080***	1.871***	4.910***	0.499**
	(0.265)	(0.361)	(0.340)	(0.395)	(0.234)
REGIME	6.225***	–7.463***	5.584***	–4.699***	0.490
	(0.626)	(0.819)	(0.882)	(0.928)	(0.646)
N Obs.	17,696	17,696	17,696	17,696	17,696
N banks	910	910	910	910	910

Note: The table contains difference-in-differences estimates of regression (4.6) with dependent variables $Y_{it}^{(j)}$ reflecting the composition of a bank i balance sheet from the liabilities and assets side: the ratio of equity capital to total assets EQ_{it}/TA_{it} $(j = 1)$, deposits of non-financial firms to total assets $DEPf_{it}/TA_{it}$ $(j = 2)$, deposits of households to total assets $DEPh_{it}/TA_{it}$ $(j = 3)$, loans to non-financial firms to total assets $LNSf_{it}/TA_{it}$ $(j = 4)$, loans to households to total assets $LNSh_{it}/TA_{it}$ $(j = 5)$. All regressions include full sets of bank FE, quarter FE, and bank control variables, which are not reported for the sake of space and are available upon request. Mid-2013 marks the transition of the CBR to a new prudential regulation regime in which the CBR was no longer tolerant of fraudulent banks. The treatment group consists of all banks which are likely to be treated as fraudulent by the CBR (the treatment rule is proxied with the Heckman selection model (4.1)–(4.2)). The composition of the treatment and control groups varies in time depending on the application of the treatment rule in each quarter (see Section 4.2.2 for details). All regressions reflect results on the extensive margin (the size of HNC does not matter).

***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the bank level and appear in the brackets under the estimated coefficients.

Bibliography

Abadie, Alberto, and Guido W. Imbens. 2011. "Bias-Corrected Matching Estimators for Average Treatment Effects." *Journal of Business & Economic Statistics* 29 (1): 1–11.

Acharya, Viral, and Tanju Yorulmazer. 2007. "Too many to fail–An analysis of time-inconsistency in bank closure policies." *Journal of Financial Intermediation* 16 (1): 1–31.

Acharya, Viral V., Allen N. Berger, and Raluca A. Roman. 2018. "Lending Implications of U.S. Bank Stress Tests: Costs or Benefits?" *Journal of Financial Intermediation* 34:58–90. Assessing Banking Regulation During the Obama Era.

Agarwal, Sumit, David Lucca, Amit Seru, and Francesco Trebbi. 2014. "Inconsistent Regulators: Evidence from Banking." *The Quarterly Journal of Economics* 129 (2): 889–938 (03).

Ahn, Daniel P., and Rodney Ludema. 2020. "The sword and the shield: The economics of targeted sanctions." *European Economic Review* 130 (C): 103587.

Aizenman, Joshua, Yothin Jinjarak, and Mark Spiegel. 2022. "Fiscal stimulus and commercial bank lending under COVID-19." *NBER Working Paper*, vol. 29882.

Akinci, Ozge. 2013. "Global financial conditions, country spreads and macroeconomic fluctuations in emerging countries." *Journal of International Economics* 91 (2): 358–371.

Allen, Franklin, and Douglas Gale. 2000. "Financial Contagion." *Journal of Political Economy* 108 (1): 1–33.

Ananyev, Maxim, and Sergei Guriev. 2018. "Effect of Income on Trust: Evidence from the 2009 Economic Crisis in Russia." *The Economic Journal* 129 (619): 1082–1118 (06).

Antolin-Diaz, Juan, and Juan Rubio-Ramirez. 2018. "Narrative Sign Restrictions for SVARs." *American Economic Review* 108 (10): 2802–29 (October).

Artavanis, Nikolaos, Adair Morse, and Margarita Tsoutsoura. 2016. "Measuring Income Tax Evasion Using Bank Credit: Evidence from Greece." *The Quarterly Journal of Economics* 131 (2): 739–798 (02).

Baker, Andrew, David Larcker, and Charles Wang. 2022. "How much should we trust staggered difference-in-differences estimates?" *Journal of Financial Economics* 144 (2): 370–395.

Banbura, Marta, Domenico Giannone, and Lucrezia Reichlin. 2010. "Large Bayesian vector auto regressions." *Journal of Applied Econometrics* 25 (1): 71–92.

Barseghyan, Gayane. 2019. "Sanctions and counter-sanctions: What did they do?" Bofit discussion papers 24/2019, Bank of Finland, Institute for Economies in Transition.

Beck, Thorsten, Samuel Da-Rocha-Lopes, and Andre F Silva. 2020. "Sharing the Pain? Credit Supply and Real Effects of Bank Bail-ins." *The Review of Financial Studies* 34 (4): 1747–1788 (06).

Beck, Thorsten, Olivier De Jonghe, and Glenn Schepens. 2013. "Bank competition and stability: Cross-country heterogeneity." *Journal of Financial Intermediation* 22 (2): 218–244.

Belin, Matej, and Jan Hanousek. 2021. "Which sanctions matter? Analysis of the EU/Russian sanctions of 2014." *Journal of Comparative Economics* 49 (1): 244–257.

Ben Zeev, Nadav. 2019. "Global credit supply shocks and exchange rate regimes." *Journal of International Economics* 116:1–32.

Ben Zeev, Nadav, Evi Pappa, and Alejandro Vicondoa. 2017. "Emerging economies business cycles: The role of commodity terms of trade news." *Journal of International Economics* 108:368–376.

Berger, Allen, and Christa Bouwman. 2013. "How does capital affect bank performance during financial crises?" *Journal of Financial Economics* 109 (1): 146–176.

Berner, Richard, Stephen Cecchetti, and Kim Schoenholtz. 2022. "Russian sanctions: Some questions and answers." *VoxEU.org*, March.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-In-Differences Estimates?*." *The Quarterly Journal of Economics* 119 (1): 249–275 (02).

Besedes, Tibor, Stefan Goldbach, and Volker Nitsch. 2017. "You're banned! The effect of sanctions on German cross-border financial flows." *Economic Policy* 32 (90): 263–318.

Bircan, Cagatay, and Ralph De Haas. 2019. "The Limits of Lending? Banks and Technology Adoption across Russia." *The Review of Financial Studies* 33 (2): 536–609 (06).

Blanchard, Olivier J., Jean-Paul L'Huillier, and Guido Lorenzoni. 2013. "News, Noise, and Fluctuations: An Empirical Exploration." *American Economic Review* 103 (7): 3045–70 (December).

Blattner, Laura, Luisa Farinha, and Francisca Rebelo. 2023. "When Losses Turn into Loans: The Cost of Weak Banks." *American Economic Review* 113 (6): 1600–1641 (June).

Bolton, Patrick, Xavier Freixas, Leonardo Gambacorta, and Paolo Emilio Mistrulli. 2016. "Relationship and Transaction Lending in a Crisis." *The Review of Financial Studies* 29 (10): 2643–2676 (06).

Bonfim, Diana, Gil Nogueira, and Steven Ongena. 2020. ""Sorry, We're Closed" Bank Branch Closures, Loan Pricing, and Information Asymmetries." *Review of Finance* 25 (4): 1211–1259 (11).

Boot, Arnoud, and Anjan Thakor. 1993. "Self-Interested Bank Regulation." *American Economic Review* 83 (2): 206–12.

Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer. 2018. "Diagnostic Expectations and Credit Cycles." *The Journal of Finance* 73 (1): 199–227.

Born, B., G. Muller, J. Pfeifer, and S. Wellmann. 2020. "Different No More: Country Spreads in Advanced and Emerging Economies." *CEPR Discussion Paper*, no. 14392.

Boyd, John, and Gianni De Nicolo. 2005. "The Theory of Bank Risk Taking and Competition Revisited." *The Journal of Finance* 60 (3): 1329–1343.

Brown, Craig O., and I. Serdar Dinc. 2005. "The politics of bank failures: Evidence from emerging markets." *The Quarterly Journal of Economics* 120 (4): 1413–1444 (11).

———. 2011. "Too Many to Fail? Evidence of Regulatory Forbearance When the Banking Sector Is Weak." *The Review of Financial Studies* 24 (4): 1378–1405 (04).

Brown, Stephen, William Goetzmann, Roger Ibbotson, and Stephen Ross. 1992. "Survivorship Bias in Performance Studies." *The Review of Financial Studies* 5 (4): 553–580.

Brunnermeier, Markus, Darius Palia, Karthik A. Sastry, and Christopher A. Sims. 2021. "Feedbacks: Financial Markets and Economic Activity." *American Economic Review* 111 (6): 1845–79 (June).

Bruno, Valentina, and Hyun Song Shin. 2017. "Global Dollar Credit and Carry Trades: A Firm-Level Analysis." *The Review of Financial Studies* 30 (3): 703–749 (02).

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-Differences with multiple time periods." *Journal of Econometrics* 225 (2): 200–230. Themed Issue: Treatment Effect 1.

Calomiris, Charles, and Stephen Haber. 2014. "Fragile by design: The political origins of banking crises and scarce credit." *Princeton; Oxford: Princeton University Press.*

Calomiris, Charles W., and Urooj Khan. 2015. "An Assessment of TARP Assistance to Financial Institutions." *Journal of Economic Perspectives* 29 (2): 53–80 (May).

Carriero, Andrea, Todd E. Clark, and Massimiliano Marcellino. 2015. "Bayesian VARs: Specification Choices and Forecast Accuracy." *Journal of Applied Econometrics* 30 (1): 46–73.

Cesa-Bianchi, Ambrogio, Andrea Ferrero, and Alessandro Rebucci. 2018. "International credit supply shocks." *Journal of International Economics* 112:219–237.

Cespedes, L., and A. Velasco. 2012. "A. Macroeconomic Performance During Commodity Price Booms and Busts." *IMF Economic Review* 60:570–599.

Chang, Roberto, and Andres Fernandez. 2013. "On the sources of aggregate fluctuations in emerging economies." *International Economic Review* 54 (4): 1265–1293.

Chernykh, Lucy, and Rebel Cole. 2011. "Does deposit insurance improve financial intermediation? Evidence from the Russian experiment." *Journal of Banking & Finance* 35 (2): 388–402.

Chodorow-Reich, Gabriel. 2014. " The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-9 Financial Crisis." *The Quarterly Journal of Economics* 129 (1): 1–59 (10).

Chopra, Yakshup, Krishnamurthy Subramanian, and Prasanna L Tantri. 2020. "Bank Cleanups, Capitalization, and Lending: Evidence from India." *The Review of Financial Studies*, 10. hhaa119.

Cipriani, Marco, Linda Goldberg, and Gabriele La Spada. 2023. "Financial Sanctions, SWIFT, and the Architecture of the International Payment System." *Journal of Economic Perspectives* 37 (1): 31–52 (February).

Claessens, Stijn, Erik Feijen, and Luc Laeven. 2008. "Political connections and preferential access to finance: The role of campaign contributions." *Journal of Financial Economics* 88 (3): 554–580.

Cloyne, James, Clodomiro Ferreira, and Paolo Surico. 2019. "Monetary Policy when Households have Debt: New Evidence on the Transmission Mechanism." *The Review of Economic Studies* 87 (1): 102–129 (01).

Cole, Rebel, and Lawrence White. 2017. "When time is not on our side: The costs of regulatory forbearance in the closure of insolvent banks." *Journal of Banking & Finance* 80 (C): 235–249.

Cole, Shawn. 2009. "Fixing Market Failures or Fixing Elections? Agricultural Credit in India." *American Economic Journal: Applied Economics* 1 (1): 219–50 (January).

Cortes, Kristle R., Yuliya Demyanyk, Lei Li, Elena Loutskina, and Philip E. Strahan. 2020. "Stress Tests and Small Business Lending." *Journal of Financial Economics* 136 (1): 260–279.

Crozet, Matthieu, and Julian Hinz. 2020. "Friendly fire: The trade impact of the Russia sanctions and counter-sanctions." *Economic Policy* 35 (101): 97–146.

Crozet, Matthieu, Julian Hinz, Amrei Stammann, and Joschka Wanner. 2021. "Worth the pain? Firms' exporting behaviour to countries under sanctions." *European Economic Review* 134 (02): 103683.

D'Acunto, Francesco, Michael Weber, and Jin Xie. 2019. "Punish One, Teach A Hundred: The Sobering Effect of Punishment on the Unpunished." *CESifo Working Paper*, vol. 7512.

de Chaisemartin, Clement, and X. D'Haultfoeuille. 2017. "Fuzzy Differences-in-Differences." *The Review of Economic Studies* 85 (2): 999–1028 (08).

de Chaisemartin, Clement, and Xavier D'Haultfoeuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review* 110 (9): 2964–2996 (September).

Degryse, Hans, Olivier De Jonghe, Sanja Jakovljevic, Klaas Mulier, and Glenn Schepens. 2019. "Identifying credit supply shocks with bank-firm data: Methods and applications." *Journal of Financial Intermediation* 40 (C): 100813.

Degryse, Hans, Alexei Karas, and Koen Schoors. 2019. "Relationship lending during a trust crisis on the interbank market: A friend in need is a friend indeed." *Economics Letters* 182 (C): 1–4.

Degryse, Hans, and Steven Ongena. 2005. "Distance, Lending Relationships, and Competition." *Journal of Finance* 60 (1): 231–266.

Delis, Manthos, Iftekhar Hasan, and Steven Ongena. 2020. "Democracy and credit." *Journal of Financial Economics* 136 (2): 571–596.

Demirguc-Kunt, Asli, and Enrica Detragiache. 2002. "Does deposit insurance increase banking system stability? An empirical investigation." *Journal of Monetary Economics* 49 (7): 1373–1406.

DeYoung, Robert, and Gokhan Torna. 2013. "Nontraditional banking activities and bank failures during the financial crisis." *Journal of Financial Intermediation* 22 (3): 397–421.

Diamond, Douglas W., and Raghuram G. Rajan. 2011. " Fear of Fire Sales, Illiquidity Seeking, and Credit Freezes." *The Quarterly Journal of Economics* 126 (2): 557–591 (05).

di Giovanni, Julian, Sebnem Kalemli-Ozcan, Mehmet Fatih Ulu, and Yusuf Soner Baskaya. 2021. "International Spillovers and Local Credit Cycles." *The Review of Economic Studies* 89 (2): 733–773 (10).

Dreger, Christian, Konstantin A. Kholodilin, Dirk Ulbricht, and Jarko Fidrmuc. 2016. "Between the hammer and the anvil: The impact of economic sanctions and oil prices on Russia's ruble." *Journal of Comparative Economics* 44 (2): 295–308.

Drott, Constantin, Stefan Goldbach, and Volker Nitsch. 2022. "The effects of sanctions on Russian banks in TARGET2 transactions data." *Deutsche Bundesbank Discussion Paper*, vol. 38.

Duflo, Esther, Pascaline Dupas, and Michael Kremer. 2011. "Peer Effects, Teacher Incentives, and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya." *American Economic Review* 101 (5): 1739–1774 (August).

Efing, Matthias, Stefan Goldbach, and Volker Nitsch. 2023. "Freeze! Financial sanctions and bank responses." *Review of Financial Studies, forthcoming.*

Eickmeier, Sandra, and Tim Ng. 2015. "How do US credit supply shocks propagate internationally? A GVAR approach." *European Economic Review* 74 (C): 128–145.

Enikolopov, Ruben, and Sergey Stepanov. 2013, 07. "Corporate Governance in Russia." In *The Oxford Handbook of the Russian Economy.* Oxford University Press.

Etkes, Haggay, and Assaf Zimring. 2015. "When trade stops: Lessons from the Gaza blockade 2007â€"2010." *Journal of International Economics* 95 (1): 16–27.

Faccio, Mara. 2006. "Politically Connected Firms." *American Economic Review* 96 (1): 369–386 (March).

Felbermayr, Gabriel, Aleksandra Kirilakha, Constantinos Syropoulos, Erdal Yalcin, and Yoto Yotov. 2020. "The global sanctions data base." *European Economic Review* 129 (C): 103561.

Fisman, Raymond. 2001. "Estimating the Value of Political Connections." *American Economic Review* 91 (4): 1095–1102 (September).

Gambetti, Luca, and Alberto Musso. 2017. "Loan Supply Shocks and the Business Cycle." *Journal of Applied Econometrics* 32 (4): 764–782.

Garcia-Cicco, Javier, Roberto Pancrazi, and Martin Uribe. 2010. "Real Business Cycles in Emerging Countries?" *American Economic Review* 100 (5): 2510–31 (December).

Geiger, Martin, and Jochen Guntner. 2022, May. "The Chronology of Brexit and UK Monetary Policy." Economics working papers 2022-06, Department of Economics, Johannes Kepler University Linz, Austria.

Gertler, Mark, and Peter Karadi. 2015. "Monetary Policy Surprises, Credit Costs, and Economic Activity." *American Economic Journal: Macroeconomics* 7 (1): 44–76 (January).

Gissler, Stefan, Jeremy Oldfather, and Doriana Ruffino. 2016. "Lending on hold: Regulatory uncertainty and bank lending standards." *Journal of Monetary Economics* 81:89–101. Carnegie-Rochester-NYU Conference Series on Public Policy "Monetary Policy: An Unprecedented Predicament" held at the Tepper School of Business, Carnegie Mellon University, November, 2015.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225 (2): 254–277. Themed Issue: Treatment Effect 1.

Gopinath, Gita, Sebnem Kalemli-Ozcan, Loukas Karabarbounis, and Carolina Villegas-Sanchez. 2017. "Capital Allocation and Productivity in South Europe." *The Quarterly Journal of Economics* 132 (4): 1915–1967 (06).

Granja, Joao, Gregor Matvos, and Amit Seru. 2017. "Selling Failed Banks." *The Journal of Finance* 72 (4): 1723–1784.

Greenstone, Michael, Alexandre Mas, and Hoai-Luu Nguyen. 2020. "Do Credit Market Shocks Affect the Real Economy? Quasi-experimental Evidence from the Great Recession and "Normal" Economic Times." *American Economic Journal: Economic Policy* 12 (1): 200–225 (February).

Gropp, Reint, Thomas Mosk, Steven Ongena, and Carlo Wix. 2018. "Banks Response to Higher Capital Requirements: Evidence from a Quasi-Natural Experiment." *The Review of Financial Studies* 32 (1): 266–299 (04).

Gropp, Reint, Steven Ongena, Joerg Rocholl, and Vahid Saadi. 2022. "The cleansing effect of banking crises." *Economic Inquiry*, vol. Forthcoming.

Gutmann, J., M. Neuenkirch, , and F. Neumeier. 2021. "The Economic Effects of International Sanctions: An Event Study." *CESifo Working Paper No.*, vol. 9007.

He, Zhiguo, and Asaf Manela. 2016. "Information Acquisition in Rumor-Based Bank Runs." *The Journal of Finance* 71 (3): 1113–1158.

Heckman, James. 1979. “Sample Selection Bias as a Specification Error.” *Econometrica* 47 (1): 153–61.

Holmstrom, Bengt, and Jean Tirole. 1997. “Financial Intermediation, Loanable Funds, and the Real Sector.” *The Quarterly Journal of Economics* 112 (3): 663–691.

IMF. 2015. “Staff Report for the Article IV Consultation: Russian Federation.” *International Monetary Fund,*, vol. Washington DC.

Ioannidou, Vasso, and Steven Ongena. 2010. “â€śTime for a Changeâ€ť: Loan Conditions and Bank Behavior when Firms Switch Banks.” *The Journal of Finance* 65 (5): 1847–1877.

Iyer, Rajkamal, and Manju Puri. 2012. “Understanding Bank Runs: The Importance of Depositor-Bank Relationships and Networks.” *American Economic Review* 102 (4): 1414–1445 (June).

Jaimovich, Nir, and Sergio Rebelo. 2009. “Can News about the Future Drive the Business Cycle?” *American Economic Review* 99 (4): 1097–1118 (September).

James, Christopher. 1991. “The Losses Realized in Bank Failures.” *The Journal of Finance* 46 (4): 1223–1242.

Jimenez, Gabriel, Luc Laeven, David Martinez-Miera, and Jose-Luis Peydro. 2022. “Public guarantees, relationship lending and bank credit: Evidence from the COVID-19 crisis.” *CEPR Discussion Paper 17110.*

Jiménez, Gabriel, Steven Ongena, José-Luis Peydró, and Jesús Saurina. 2014. “Hazardous times for monetary policy: What do twenty-three million bank loans say about the effects of monetary policy on credit risk-taking?” *Econometrica* 82 (2): 463–505.

Jorda, Oscar. 2005. “Estimation and Inference of Impulse Responses by Local Projections.” *American Economic Review* 95 (1): 161–182 (March).

Kang, Ari, Richard Lowery, and Malcolm Wardlaw. 2015. “The costs of closing failed banks: A structural estimation of regulatory incentives.” *The Review of Financial Studies* 28 (4): 1060–1102.

Kanzig, Diego R. 2021a. “The Macroeconomic Effects of Oil Supply News: Evidence from OPEC Announcements.” *American Economic Review* 111 (4): 1092–1125 (April).

———. 2021b. “The Unequal Economic Consequences of Carbon Pricing.” *SSRN Working Paper No*, no. 3786030 (February).

Karas, Alexei, William Pyle, and Koen Schoors. 2013. “Deposit Insurance, Banking Crises, and Market Discipline: Evidence from a Natural Experiment on Deposit Flows and Rates.” *Journal of Money, Credit and Banking* 45 (1): 179–200.

Karas, Alexei, and Andrei Vernikov. 2019. “Russian bank data: Birth and death, location, acquisitions, deposit insurance participation, state and foreign ownership.” *Data in Brief* 27:104560.

Keeley, Michael C. 1990. “Deposit Insurance, Risk, and Market Power in Banking.” *American Economic Review* 80 (5): 1183–1200.

Keerati, Ritt. 2022. "The Unintended Consequences of Financial Sanctions." *SSRN Working Paper*, vol. 4049281 (March 3).

Kempf, Elisabeth, Mancy Luo, Larissa Schafer, and Margarita Tsoutsoura. 2022. "Political Ideology and International Capital Allocation." *Becker Friedman Institute for Economics Working Paper*, no. 2022-27.

Kholodilin, Konstantin, and Aleksei Netsunajev. 2019. "Crimea and punishment: The impact of sanctions on Russian economy and economies of the Euro area." *Baltic Journal of Economics* 19 (1): 39–51.

Khwaja, Asim Ijaz, and Atif Mian. 2005. "Do lenders favor politically connected firms? Rent provision in an emerging financial market." *The Quarterly Journal of Economics* 120 (4): 1371–1411 (11).

———. 2008. "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market." *American Economic Review* 98 (4): 1413–42 (September).

Kilian, L., and H. Lutkepohl. 2017. "Structural Vector Autoregressive Analysis." *Cambridge University Press.*

Koetter, Michael, and Alexander Popov. 2020. "Political cycles in bank lending to the government." *The Review of Financial Studies* 34 (6): 3138–3180 (10).

Koop, Gary. 2013. "Forecasting with Medium and Large Bayesian VARs." *Journal of Applied Econometrics* 28 (2): 177–203.

Koop, Gary, and Dimitris Korobilis. 2010. "Bayesian Multivariate Time Series Methods for Empirical Macroeconomics." *Foundations and TrendsÂ® in Econometrics* 3 (4): 267–358.

Korhonen, Iikka, and Svetlana Ledyaeva. 2010. "Trade linkages and macroeconomic effects of the price of oil." *Energy Economics* 32 (4): 848–856. Policymaking Benefits and Limitations from Using Financial Methods and Modelling in Electricity Markets.

Kupiec, Paul, Yan Lee, and Claire Rosenfeld. 2017. "Does bank supervision impact bank loan growth?" *Journal of Financial Stability* 28:29–48.

Laeven, Luc, and Ross Levine. 2009. "Bank governance, regulation and risk taking." *Journal of Financial Economics* 93 (2): 259–275.

La Porta, Rafael, Florencio Lopez-de Silanes, and Andrei Shleifer. 2002. "Government Ownership of Banks." *The Journal of Finance* 57 (1): 265–301.

Laudati, Dario, and Hashem Pesaran. 2021. "Identifying the Effects of Sanctions on the Iranian Economy Using Newspaper Coverage." *CESifo Working Paper*, no. 9217 (August).

Leland, Hayne E., and David H. Pyle. 1977. "Informational Asymmetries, Financial Structure, and Financial Intermediation." *The Journal of Finance* 32 (2): 371–387.

Lennox, Clive S., Jere R. Francis, and Zitian Wang. 2012. "Selection Models in Accounting Research." *The Accounting Review* 87 (2): 589–616 (03).

Leung, Michael P. 2020. "Treatment and Spillover Effects Under Network Interference." *The Review of Economics and Statistics* 102 (2): 368–380 (05).

Levy, Philip I. 1999. "Sanctions on South Africa: What Did They Do?" *American Economic Review* 89 (2): 415–420 (May).

Liaudinskas, Karolis, and Kristina Grigaite. 2021. "Estimating firms' bank-switching costs." *Norges Bank Working Paper* 4/2021 (4): 1–57.

Mamonov, Mikhail, and Anna Pestova. 2022. "The Price of War: Macroeconomic and Cross-sectional Effects of Sanctions on Russia." *SSRN Working Paper*, vol. 4190655 (August 24).

Mamonov, Mikhail, Anna Pestova, and Steven Ongena. 2021. ""Crime and Punishment?" How Russian Banks Anticipated and Dealt with Global Financial Sanctions." *CEPR Discussion paper*, vol. 16075.

Martinez Peria, Maria, and Sergio Schmukler. 2001. "Do Depositors Punish Banks for Bad Behavior? Market Discipline, Deposit Insurance, and Banking Crises." *Journal of Finance* 56 (3): 1029–1051.

Mian, Atif, and Amir Sufi. 2009. "The Consequences of Mortgage Credit Expansion: Evidence from the U.S. Mortgage Default Crisis." *The Quarterly Journal of Economics* 124 (4): 1449–1496.

Mian, Atif, Amir Sufi, and Emil Verner. 2017. "Household Debt and Business Cycles Worldwide*." *The Quarterly Journal of Economics* 132 (4): 1755–1817 (05).

Monacelli, Tommaso, Luca Sala, and Daniele Siena. 2023. "Real interest rates and productivity in small open economies." *Journal of International Economics* 142:103746.

Morrison, Alan, and Lucy White. 2013. "Reputational contagion and optimal regulatory forbearance." *Journal of Financial Economics* 110 (3): 642–658.

Nagel, Stefan, and Amiyatosh Purnanandam. 2019. "Banks' Risk Dynamics and Distance to Default." *The Review of Financial Studies* 33 (6): 2421–2467 (10).

Neuenkirch, Matthias, and Florian Neumeier. 2016. "The impact of US sanctions on poverty." *Journal of Development Economics* 121 (C): 110–119.

Neumeyer, Pablo A., and Fabrizio Perri. 2005. "Business cycles in emerging economies: the role of interest rates." *Journal of Monetary Economics* 52 (2): 345–380 (March).

Nigmatulina, Dzhamilya. 2022. "US Sanctions against Russia Helped the Very Companies They Intended to Target. But Russia Still Lost." *Mimeo.*

O'Hara, Maureen, and Wayne Shaw. 1990. "Deposit Insurance and Wealth Effects: The Value of Being "Too Big to Fail"." *Journal of Finance* 45 (5): 1587–1600 (December).

Petersen, Mitchell A., and Raghuram Rajan. 1995. "The Effect of Credit Market Competition on Lending Relationships." *The Quarterly Journal of Economics* 110 (2): 407–443.

Petrin, Amil, and James Levinsohn. 2012. "Measuring aggregate productivity growth using plant-level data." *The RAND Journal of Economics* 43 (4): 705–725.

Philippon, Thomas, and Philipp Schnabl. 2013. "Efficient Recapitalization." *The Journal of Finance* 68 (1): 1–42.

Schaeck, Klaus. 2008. “Bank Liability Structure, FDIC Loss, and Time to Failure: A Quantile Regression Approach.” *Journal of Financial Services Research* 33 (3): 163–179 (June).

Schafer, Larissa. 2019. “"Forgive but Not Forget": The Behavior of Relationship Banks When Firms Are in Distress.” *Review of Finance* 23 (6): 1079–1114 (10).

Schularick, Moritz, and Alan M. Taylor. 2012. “Credit Booms Gone Bust: Monetary Policy, Leverage Cycles, and Financial Crises, 1870-2008.” *American Economic Review* 102 (2): 1029–61 (April).

Shleifer, Andrei, and Robert Vishny. 2011. “Fire Sales in Finance and Macroeconomics.” *Journal of Economic Perspectives* 25 (1): 29–48 (March).

Simonov, Andrey, and Justin Rao. 2022. “Demand for Online News under Government Control: Evidence from Russia.” *Journal of Political Economy* 130 (2): 259–309.

Slemrod, Joel. 2007. “Cheating Ourselves: The Economics of Tax Evasion.” *Journal of Economic Perspectives* 21 (1): 25–48 (March).

Song, Fenghua, and Anjan Thakor. 2007. “Relationship Banking, Fragility, and the Asset-Liability Matching Problem.” *Review of Financial Studies* 20 (6): 2129–2177.

Sun, Liyang, and Sarah Abraham. 2021. “Estimating dynamic treatment effects in event studies with heterogeneous treatment effects.” *Journal of Econometrics* 225 (2): 175–199. Themed Issue: Treatment Effect 1.

Svejnar, Jan. 2002. “Transition Economies: Performance and Challenges.” *Journal of Economic Perspectives* 16 (1): 3–28 (March).

Townsend, Robert. 1979. “Optimal contracts and competitive markets with costly state verification.” *Journal of Economic Theory* 21 (2): 265–293 (October).

Uribe, Martin, and Stephanie Schmitt-Grohe. 2017. “Open Economy Macroeconomics.” *Princeton University Press.*

Uribe, Martin, and Vivian Z. Yue. 2006. “Country spreads and emerging countries: Who drives whom?” *Journal of International Economics* 69 (1): 6–36. Emerging Markets.

Welt, C., K. Archick, R. Nelson, and D. Rennack. 2020. “U.S. Sanctions on Russia.” *Congressional Research Service Report No.*, no. R45415.

Wheelock, David, and Paul Wilson. 2000. “Why do Banks Disappear? The Determinants of U.S. Bank Failures and Acquisitions.” *The Review of Economics and Statistics* 82 (1): 127–138.

Wolf, Christian. 2020. “SVAR (Mis)identification and the Real Effects of Monetary Policy Shocks.” *American Economic Journal: Macroeconomics* 12 (4): 1–32 (October).

Wooldridge, Jeffrey M. 2009. “On estimating firm-level production functions using proxy variables to control for unobservables.” *Economics Letters* 104 (3): 112–114.

Yakovlev, Evgeny. 2018. “Demand for Alcohol Consumption in Russia and Its Implication for Mortality.” *American Economic Journal: Applied Economics* 10 (1): 106–49 (January).