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Quo Vadis? Evidence on New Firm-Bank Matching and Firm Performance Following Bad Bank Closures*

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ABSTRACT

This study investigates the effects of bank closure policy on firms and banks. Following an extended period of regulatory forbearance on bank misreporting, the Central Bank of Russia (CBR) adopted in 2013 a regime of tight bank supervision and intolerance to weak, non-transparent and fraudulent banks, which resulted in license revocation of the two-thirds of operating banks over the period between 2013-2020. We analyze unique loan-level data from the Russian credit register and show that, following bad bank closures, bad firms go to another (still operating) bad banks and good firms go to good banks. The matching of bad firms and bad banks is fueled through common ownership structure and weakens when concentration at local credit markets rise. We show that neither bad nor good firms possessed information on the CBR's actions (no anticipation of bad bank closures). We reveal that the policy had cleansing effect on the structure of the economy: after bad banks closure and before finding new banks, good firms improve their performance (default rates drop, employment and income rise), whereas bad firms further deteriorate. Finally, we find that the policy was pro-active: still operating bad banks turned to reducing their corporate and retail lending, creating more loan loss reserves and disclosing more non-performing loans.

JEL: G21, G28

Keywords: Bank clean-ups, Regulatory forbearance, Firm-bank matching, Common ownership, Loan delinquencies, Duration models, Difference-in-differences.

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1. INTRODUCTION

Firms derive the value from bank lending beyond the benefit of merely obtaining external financing. Banks are able to mitigate information asymmetries between borrowers and savers through screening (Leland and Pyle, 1977), and to reduce moral hazard through monitoring (Holmstrom and Tirole, 1997). During a lending relationship with a firm, a bank can gain proprietary information about the borrowing firm and influence decisions taken by the firm's management (Petersen and Rajan, 1995) while the firm may expect support by the relationship bank in times of distress (Bolton et al., 2016; Schäfer, 2019). Thus, the loss of an established lending relationship with a bank due to the bank's failure may have a negative effect on firms. But the negative consequences of such an event are less clear if the bank is actually closed by its regulator due to loss of capital.[1] In this paper, we analyze a large scale closure policy of weak, non-transparent and fraudulent (WNF) banks in Russia to understand what happens to firms that face the closure of their bank due to fraud detection.[2]

Specifically, we want to analyze how firms match with a new bank when their current bank fails? And what happens to the firms' performance during the transition period, i.e., after facing the bank's failure and before the firms are able to match with a new bank? Are there differences between non-performing, loss-making, "bad" firms and performing, profitable, "good" firms in this respect, given that both could have had a relationship with the failed bank?

How firms fare after the closure of their bank 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., 2019a; Greenstone et al., 2020), the closure of their bank branches (Bonfim et al., 2020), or 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 pro-active regulatory closure of bad banks on firms' consequent matches with new banks and performance. Yet, it is vitally important to understand these effects for the design of the optimal bank clean-up policy. Hereinafter, by bad banks we understand the banks engaged

[1] Official statements issued by the Bank of Russia cited the following reasons for a bank license revocation: "the loss of capital due to excessive credit risk, insufficient reserves and/or involvement in questionable transactions, which also led to the loss of capital".

[2] For the sake of this paper we use term "fraud" to describe bank behaviour that resulted in a loss of capital and could be a reason for revocation of the banking license.

in fraudulent activities, financially weak and non-transparent banks.

We therefore analyze a recent (rather dramatic) series of bank closures undertaken by the Central Bank of Russia (CBR), which has begun in 2013. Its intent has been to clean up the banking system by closing financially weak, non-transparent and fraudulent banks (see historical and institutional details in Section 2). This new regime of an intense fraud intolerance[3] followed a period of wide-spread regulatory forbearance lasting during 2006-2013. Over the period of seven years between 2013 and 2020 the CBR effectively revoked around two-third of all banking licenses in the country. Importantly, the policy began before the recession of 2014–2015 and before Western sanctions, i.e., during normal times. After seven years of the policy, almost 650 banks were briskly closed following fraud detection during these years. Three aspects make the settings of this policy particularly unique. First, the policy begins unexpectedly following a prolonged period of regulatory forbearance, which resulted in a large fraction of the banking system contaminated with bad banks. Second, the active phase of the policy continued for over 5 years, which allows for a possibility of new matches between firms and still operating bad banks following the closure of the firms' bad bank. Finally, the bulk of the policy was conducted during the period of primarily normal economic times, which provides better setting for identifying the real effects of the policy.[4]

To perform our study 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 of NPLs*.[5] The latter data are unique in their coverage and comprehensiveness and are opened to independent academic research for the first time. We merge these data with 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 nation-wide banking media source. We employ this information to assess if firms, following the closure of their bad bank, match with a new bank that have the same or different owners as in the

[3]Bank of Russia's regime of an intense fraud intolerance encompasses strict supervision and closures of financially weak, non-transparent and fraudulent banks.

[4]The policy was launched in mid-2013—half a year before the Russian economy entered another (local) recession and experienced economic sanctions of the West (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 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 supported by the government so that they simply reshuffled credit from firms to households (Mamonov et al., 2021).

[5]For simplicity, by *days of NPLs* we imply days during which the payment on a loan had been delinquent by firm.

closed bank.

We begin our empirical analysis by exploring the determinants of firms' matching with new banks following their current banks' closures. Many of such closures were motivated by the presence of bank fraud and we apply a duration model to analyze if the closure of such bad banks results in bad firms switching to other bad banks while good firms may end up at good banks. We proxy the quality of the firms with two variables: (i) whether the firms have negative profits (firm-level); or (ii) the number of the days of NPLs they had in the closed banks (firm-bank level). We find that the lower the quality of loans the firms had in the closed banks the more likely that these firms again match with (still operating) bad banks and the less likely that the firms end up at good banks. The firms' profitability has always positive effect on matching.

We also show that the average time to match with another bad bank equals 19 months while the time to match with a good bank equals 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 bad bank and 16% less likely to join a good 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 bad bank closure 50 to 75% of the bad firms match again with a bad bank owned by *the same* owners. Excluding banks with common ownership we find that following bad bank closure bad firms are *no more* likely to match with another (still operating) bad bank. In all instances good firms match with a new good bank, no matter if the latter share common owners and/or directors with the firms' closing bank.

Second, apart from common ownership, we hypothesize that not all bad bank closures are equally predictable by general public.[6] Some of the closures may be more *predictable* than others, based on for example publicly observable data reported in the banks' balance sheets. If a bank's fraud detection is predictable from its balance sheet then, we conjecture, the related "bad" firms will face difficulties engaging a new bank, even it is a bad 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

[6]There is a usual information asymmetry between owners and managers of a bank on one side and the general public on the other.

a 6-month rolling window with a logit model explaining bank fraud detection to flexibly capture the regulator's learning about the current and updated misreporting approaches by bad banks. We sort the failed banks into two categories: those with predicted probabilities below the unconditional threshold (which equally 0.5% monthly) are classified as surprising failures, while those with the predicted probabilities above the threshold are considered expected failures. In the second stage we then re-run our 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 that were related to banks where fraud detection was predictable do not match easily with a new bank and the bad firm to new bad-bank move only works for closures that were surprising (i.e., fraud detection was difficult).

Third, we show that concentration of regional credit markets matters for the matching of bad firms and good banks. The higher the market concentration, the more likely a good bank operating in this market will engage a bad firm coming from a closed bad bank. This results 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 retention of the firm is more likely and therefore intertemporal subsidization possible.

To confirm the validity of the estimates, we then perform a placebo test which checks whether firms switch from about-to-fail banks in advance. Importantly, our results show that bad firms neither raise their loan delinquencies nor do they switch in advance from their current lenders.[7]

With these findings at hand, we proceed to the difference-in-differences analysis of firm performance conditional on bad bank closure. We examine whether the closure of bad bank results in the deterioration of firm performance—which could be due to the destruction of the bank-firm match—or its improvement—for example, due to the break-up of the modulation of the lock-in effect (Liaudinskas and Grigaite, 2021). The estimation results show that the policy had a *cleansing* effect on the performance of good firms that faced bad banks' closures: firm default rates decrease and firms' total revenues improve. These findings are consistent with the interpretation that bad banks provide inferior expertise and charge good firms too high interest rates. The policy of closure thus unlocked good firms freeing them to improve their operations. Bad firms, in contrary, face

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

higher default risk after their bad banks closes. This is also consistent with the cleansing effect that the policy might have had on the structure of the real sector of the Russian economy.

A final empirical exercise is aimed at answering the question whether still operating bad banks adapt their balance sheets in advance or only when threatened by a regulatory unscheduled on-site inspection. Our estimation results indicate that the threat of an unscheduled inspection was *pro-active*, possesses heterogeneous ex-ante effects on bad banks' lending behavior, and effectively removes the differences among remaining banks as time passes.

As of 2021, it is clear that the bad banks closure policy of the CBR can be classified as successful, despite all the concerns listed above: the banking system continues steadily growth, banks make historically large profits, and there is little evidence that the policy itself resulted in a significant contraction of credit supply.

Our paper contributes to the several strands of the literature. First, our paper contributes to the literature that examines the effect of bank clean-up policies (Acharya et al., 2018; Cortés et al., 2020; Chopra et al., 2020; Diamond and Rajan, 2011; Philippon and Schnabl, 2013). In advanced economies, the clean-up policies often take the form of a combination of capital infusions (Calomiris and Khan, 2015), stress testing (Acharya et al., 2018), and asset quality reviews. Also, such clean-up policies often take place as a response to a crisis.[8] To the best of our knowledge, our paper is the first one to analyze the real effects of a clean-up policy that takes the form of bad bank closure. Such a clean-up policy is of a particularly interest to emerging economies, which are likely to suffer more from widespread malpractice in banking system.

Second, our paper contributes to the literature on the real effect of bank distress on firm (Chodorow-Reich, 2014; Gropp et al., 2018; Degryse et al., 2019a; Greenstone et al., 2020). A recent study by Bonfim et al. (2020) for example shows that if firms purposely switch banks, unconditional on bank closure, they receive a lower loan rate, i.e., a "discount" compared to what they have received otherwise. 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. A recent study by Liaudinskas and Grigaite (2021) further documents that firms that had a relationship with a distressed bank that eventually failed were prior to failure charged a higher loan rate (hence possible locked-in by these banks). After failure the firms then benefit by obtaining a lower loan rate from a new bank.

[8] A notable exception is the Indian Asset Quality Review program analyzed in Chopra et al. (2020).

Yet despite the impact of branch or bank closure on loan rates, work by Greenstone et al. (2020) finds no significant impact of the switching itself (shown to involve costs) on the firms' employment, neither during crises nor normal times. Our analysis shows that following closure of a bad bank bad (good) firms are more likely to end up in a match with a bad (good) bank and that the performance of a bad (good) firm worsens (increases).

Third, our paper constitutes to the literature on regulatory forbearance (Acharya and Yorulmazer, 2007; Brown and Dinç, 2011; Morrison and White, 2013; Agarwal et al., 2014; Kang et al., 2014). The literature usually rationalizes introduction or presence of "regulatory myopia" in closing distressed banks as caused by for example 'too-many-to-fail' concerns (Acharya and Yorulmazer, 2007; Brown and Dinç, 2011), reputational contagion (Morrison and White, 2013), competition among regulators at different levels (Agarwal et al., 2014), political pressure and/or avoidance of damage to the local economy (Kang et al., 2014). Our results show that, by a proper design of the closure policy (pro-activity and exogeneity with respect to banks' and firms' expectations), the regulator is able to overcome the reputational risk and the risk of declining economic activity when closing distressed banks, thus exhibiting the complete reversal of regulatory forbearance.

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

The rest of the paper is structured as follows. Section 2 describes the policy undertaken by the Central Bank of Russia in mid-2013. Section 3 introduces the loan-level, firm- and bank-level data. In Section 4, we perform our duration analysis to investigate how bad and good firms match with new bad or good banks. In Section 5, we explore the channels of firm-bank matching. In Section 6, we run placebo exercises answering the question whether firms could anticipate bank failures. In Section 7, we present the difference-in-differences estimation results on the firms' performance conditional on bad bank closures. In Section 8, we run a bank-level analysis to show whether still operating bad banks adapt their balance sheets in-advance of the central bank's on-site inspections. Section 9 concludes.

2. REGULATORY FORBEARANCE AND BANK CLEAN-UP POLICY IN RUSSIA

Following the collapse of the USSR in 1991, the-then Russian central planned economy began its transition to market-based economy. As such, Russia had witnessed a rapid growth of privately-owned banks.[9]

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 the non-financial businesses of their owners ('pocket' banks) at lower interest rates than the market would otherwise offer, which was especially important during the hyperinflation times (Svejnar, 2002). In addition, a great number of these banks were involved either in outright criminal activities or employed questionable practices (Degryse et al., 2019b).

With the start of the new millennium, the number of operating banks shrank to a half, nevertheless, many of these banks were still pursuing illegal or questionable practices. The Central Bank of Russia attempted a clean-up of the banking system, which resulted in the closure of two large banks, which were involved in illegal activities, in 2006. However, the clean-up policy came effectively to a halt with the assassination of the Deputy Head of the Central Bank of Russia, A. Kozlov, who was the key figure behind the clean-up policy implementation. The so-called "Kozlov affair" had 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 continue with the business, irrespective of whether this business was legal or not.[10]

Up until the global financial crisis of 2007–2009 the Russian banking system had been growing at a two-digit growth a year per year, mainly due to expanding corporate and retail lending thus satisfying a large demand on loans.[11] The financial crisis had exposed large inefficiencies in the Russian banking system and necessitated large-scale government interventions to provide support to the largest banks. Consequently, the number of operating banks continued to decline at a smooth

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

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

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

pace after the crisis to around 1,100 banks by the beginning of 2013. Overall, the regulatory stigma to audit and close fraudulent banks following the assassination of A. Kozlov was still there, and the period between 2006-2013 is characterized by a large degree of regulatory forbearance.

The regulatory forbearance effectively ended in 2013 with the appointment of the new head of the Central bank.[12] While the intention to conduct an active clean-up of the banking 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 of the Bank stressed her intention to tighten regulatory oversight over illegal and questionable banking practices.[13]

However, soon it became clear that the Central Bank of Russia had rather rapidly swung from the regulatory forbearance regime towards a strict intolerance towards financial weakness, lack of transparency and fraud. Overall, during the period of 2013–2020, the number of operating banks in Russia had declined from around 1,000 to nearly 350, which is by 85% due to the tight policy (see Fig. 1). The average annual frequency of fraud-induced license revocation had risen from 29 (on average during the years of 2008–2013 first half) to nearly 70 (on average during 2013 second half–2020). The dramatic negative trend in the number of operating banks is nearly linear, irrespective of the changing phases of the business cycle during that time.[14] In February 2018, the Bank has officially announced that the active phase of the cleansing policy was over amid the great body of WNF banks being revealed and closed.

The geography of the cleansing policy is summarized in Figure 2. The policy was not limited only to Moscow and Saint-Peterburg—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 Western part and in the South, near the Black Sea. In almost every case, forced license revocations were associated with hidden negative capital revealed during the on-site inspections of the banks, ranging between 50% and 10% of

[12]The change of the head of the Bank was announced rather unexpectedly: E. Nabiullina, the-then head of the Ministry of Economic Development, was to replace the current head of the Bank S. Ignatiev, who held the post for the last 13 years

[13]In her inauguration speech, the new head of the bank mainly stressed that the great efforts of the Bank would be devoted to switching from a fixed to flexible exchange rate regulation and establishing an inflation targeting regime, in which the key instrument of the monetary policy is going to be the regulated interest rate. The main purpose of the new policy, as the new head announced, was curbing the two-digit inflation in the country to the target of 4%. 3. Moreover, there seemed to be 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.

[14]The Russian economy had experienced a local recession during 2014-2015 and the subsequent recovery in 2016-2019.

affected banks' total liabilities, again spreading through the whole territory of Russia.[15] As can be inferred from Figure 3, the bank-level data shows that during the active phase of the policy in 2013–2018 operating banks had raised loan loss reserves (a), disclosed more NPLs in their loan portfolios (b), reduced the stock of (possibly opaque) loans to firms (c) and slowed down new loan issuance (d), as compared to before the policy and irrespective of the phase of the business cycle. Overall, despite closing 2/3rds of all operating banks, the policy did *not* lead to a shrink of financial system. According to the World Bank statistics, the ratio of credit to domestic private sector to GDP increased from 81% in 2012 to 99% in 2020, i.e., during the years of the CBR's tight policy the banking sector was growing rapidly.[16]

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 the financial statements provided in SPARK database.[17] Second, the monthly (balance sheets items) and quarterly (P/L account) frequency bank-level data come from the Bank of Russia's reporting forms 101 and 102, respectively, and available from 2004 to 2021.[18] Third, to identify the bank-firm lending relationships, we employ the monthly data from the Russian credit registries. For the period from July 2013 to December 2017, we use the data from the Credit History Bureaus (CHB), which provides the data on the number of days during which the loans are overdue, while from for the period from January 2018 to October 2020, we employ the data from the credit registry of the Bank of Russia (Bank of Russia reporting form No. 0409303).

3.1 Credit History Bureau and Credit Registry Data

The Credit History Bureaus database (the CHB hereafter) consists of monthly data on the number of days bank loan payments are overdue–including the information on the loans which are not overdue, in which case the number of days the loan is overdue is reported as zero.[19] For each bank

[15] By negative capital, we mean the negative owners' equity—that is, the situation when the total value of a bank's assets is less than the sum total of its liabilities.

[16] See https://data.worldbank.org/indicator/FD.AST.PRVT.GD.ZS.

[17] https://spark-interfax.ru/.

[18] https://www.cbr.ru/banking_sector/otchetnost-kreditnykh-organizaciy/.

[19] The CHB is compiled from three credit history bureaus: United Credit Bureau, National Bureau of Credit Histories and Equifax Credit History Bureau. These three credit history bureaus are the biggest of 14 bureaus

and each corporate borrower, the CHB contains the information on the maximum number of days the loan payment is overdue at the reporting date. That is, if a firm has multiple loans at a bank, the CHB provides the the maximum number of days of payment overdue across these multiple loans (it is possible that only one of these several loans is delinquent).

Number of days overdue in the CHB is a categorical variable denoting the time intervals of the 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 day overdue 150 or 200 could include loans that were labeled as "hopeless", payed by collateral, contested in courts, or written off.

The CHB covers the time period from 2007 to 2017. In our analysis, we use the CHB from July 2013 to 2017 to identify bank-firm relationships during the active phase of the cleansing policy. To identify firm-bank relationship 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, the amounts of debt repayment (including interest payments and the amortisation of the principal amount of debt). Here we use days of non-performing loans.

Our database (CHB + credit registry) of matched bank-firm relationships consists initially of 655.3 thousand firms and 906 banks at the start of the sample in July 2013. Our sample covers almost 90% of Russian banks by net assets. More then 70% of firms in the CHB's data are micro-firms (with less then 15 employees), another 20-25% are SMEs, while the rest are medium and large firms.

The majority of Russian firms take out loans at one bank only. In 2017 the share of firms that took out loans at one bank only was 69.4%, and another 19.5% took out loans at two banks (Fig. 4). These patterns are different from those observed in studies using similar data for developed economies. For example, Spanish firms with multiple bank relationships account for 86% of all business loans and employ on average three banks (Jiménez et al., 2014).

registered with the State Register of Credit History Bureaus maintained by the Bank of Russia (https://www.cbr.ru/ckki/restr/).

3.2 Bank-Level Data

Finally, 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 quarterly frequency for the P&L account. The data come from the Bank of Russia's reporting forms 101 and 102 and cover the time period from 2004 to 2021.

As discussed in the previous section, around 650 banks had been shut down by the regulator during the active phase of the cleansing policy (July 2013-February 2018), of which 85% are due to fraud revealed during audit. In our study, we refer to those banks that had their licenses revoked due to fraud as bad banks, while those that were permitted to pursue their activities we dub as good banks.

3.3 Firm-Level Data

The firm-level data, which include the data from firms' financial statements comes from SPARK database, provided by the Interfax Group. Matching SPARK database with the firm-bank relationships database (the CHB and credit registry) provides the data on about 60% of firms. For the detailed list of variables that we use in our analysis from firms' financial statement refer to Table A.I.

Throughout the paper, we refer to a firm as "bad" firm if the firm suffer losses during the past two years. In addition, we proxy the quality of the firm by the days of NPLs reported in the CHB.

3.4 Bank-Firm Relationships: Descriptive Statistics

In our analysis, we focus on the subset of firms that were borrowing from a bad bank and, thus, had their bank shut down during the cleansing period. In our sample, there are 13,373 firms that had a relationship with one of the bad banks. The firm-level data are not available for 6,062 of these firms. Furthermore, after treaming our data for outliers (1 and 99 percentiles), we lose 80 more firms. Adjusting for 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 then our sample includes 287.1 thousand observation with 6,061 firms.

As for the geography of firm-bank relationships, our final dataset is representative covering the whole territory of Russia, with most dense frequency of relationships being observed in the Western, Central, and Southern parts of the country (see Fig. 5).

Turning to the differences in terms of the days of NPLs ($DNPL_{f,b,t}$), we first observe that a quarter of all firm-bank matches report a good quality of loans with $DNPL_{f,b,t} \leq 30$ days, see Fig. 6.(a). Certain spikes are observed around 30 and then 150 days of delinquencies. As compared to good banks, bad banks have expectedly lower quality of loans, see Fig. 6.(b). And in comparison to profitable firms, firms suffering losses also report larger days of NPLs, see Fig. 6.(c).

Firms' descriptive statistics are presented in Table 1. Three groups of firms are presented: firms that match with a good bank, firms that match with a bad bank, and those who never match. Out of 6,267 firms in our sample overwhelming majority of firms (85%) never find a new bank to borrow from. Those who manage to match with a new bank (15%) mostly establish a connection with a good bank (11% or 715 firms). The rest (3.2%) borrow from a new bad bank. Firms that match with a good bank are generally in a better financial shape with average ROA of 5%, smaller leverage and higher liquidity then the rest.

We cannot distinguish firms by the days of NPL, since this variable is not significantly different between the groups. Though, one characteristics stands out - the size of a firm. Contrary to a natural guess that the bigger the firm the easier it will be for it to borrow from a good bank, we observe the inverse picture in our data. Average size of a firm that matches with a bad bank is almost 3 times higher then average size of a firm that switches to a good bank (85 mln vs. 29 mln Rub), and almost two times higher then average size of those who never match (85 mln vs. 44 mln Rub). Thus, we can describe a firm that match with a bad bank as a large financially constrained firm (higher leverage, lower liquidity then for an average firm that matches with a good bank). Table 2 describes regional structure of our data. In more than a half of observations firms that had faced bank closure are registered in the Central FD, observations with firms from Volga, Northwestern, and Siberian FDs are about 10% for each district. Ural, Southern, and Far Eastern FDs add another 15% together, the rest of observations (less then 1%) are with firms from North Caucasian FD. Overwhelming majority of observations (from 78 to 94%) contains no information about delays in credit payments. The only notable exception is North Caucasian FD, where share of no delays is less then 70%, but given the small share of observations from this FD in our sample it's difficult to draw any conclusions. Regional dimension of our data allows us to look into spatial concentration of the Russian regional credit markets by calculating Herfindahl-Hirschman index. We construct the index 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 for each month. Mean values of the index as well as standard deviation are presented in Table 2. We visualize regional concentration and days of NPL for each federal district in a scatter plot (see Fig. 7).

4. FIRM-BANK MATCHING FOLLOWING BAD BANK CLOSURES

4.1 Baseline Results

We begin our analysis by examining the determinants of a firm's matching with a new bank following the policy induced closure of the firm's current bank (bad bank) and conditional on the firm's survival to the moment in time when the new match is established.[20] A natural methodological framework for this analysis is the duration regression approach ("survival" model) which takes into account duration of the spell, i.e., the time it takes the firm to match with a new bank.[21] In our analysis, we focus on *single* firm–bad bank relationships, i.e., those cases when a firm obtained loans from only one bank which, at some point in time, is closed for fraud.[22] We are interested in *where* the firm goes next, i.e., after the closure of its bad bank: to another (still operating) bad bank or to good bank. The rationale for focusing on single firm-bank relationships at the moment of bad bank detection is that the CBR's tight regulation policy is likely to affect single firm-bank pairs by more than multiple relationships within which the firms have more opportunities to substitute the flaw of borrowed funds across existing banks.

Among the determinants of new firm-bank matching we focus on the *quality* of firms. One may expect that, conditional on bad bank closures, good firms have more chances to find new bank matches than bad firms. With these considerations at hand, we start with employing a *single-failure duration analysis* in which the duration of the spell for a firm f begins with the failure of its current bad bank b at time t_f^* (t^*, for simplicity) and ends with the firm being matched with a

[20] As is discussed in Section 3, we define bad bank as a bank that is closed due to fraud at some later point in time in our sample.

[21] "Survival" regressions were previously adopted to study bank failures in, e.g., (Brown and Dinç, 2011).

[22] Recall from the Section 3, that single firm-bank relationships cover 70% of the full sample.

new bank at time $t^* + k$, where k is the duration of the spell (recall that 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 the firm f never matches with a new bank — then 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 $\lambda(\cdot)$:

$$\lambda\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(\alpha + \alpha_{bc} + \alpha_r + \alpha_i + \text{Firm.Quality}_{f,t-1}\text{B} + \mathbf{C}_{f,t-1}\Gamma\Big), \quad (1)$$

where $\text{Firm.Quality}_{f,t-1}$ is firm f quality proxy at time $t-1$, which is measured by either (i) the days of NPLs accumulated in the closed bad bank before the closure–that is, by t^*–or (ii) the binary variables of whether the firm had negative profits at t^* or $t^* + k$. $\mathbf{C}_{f,t}$ 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 and liquidity ratios (all are taken with 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 $(\alpha, \alpha_b c, \alpha_r, \alpha_i, B, \Gamma)$. $\lambda_0\left(t\right)$ is the baseline hazard function. We use the exponential distribution function to specify the the baseline hazard: $\lambda_0\left(t\right) = \lambda > 0$.[23]

Table 3 reports the estimation results of equation (1). In columns (1)–(2) the firm quality measure is proxied by the days of NPLs the firm had accumulated in the closed bad bank by the moment of closure t^*, $DNPL_{f,t^*}$. Here, the sample consists of 6,267 firms, 413 bank closures, and 934 "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 by 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 moment of matching with a new bank, $Profit_{f,t^*+k} < 0$, to the specification considered in the

[23]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.

two previous columns. The idea behind including this variable is as follows. Although a firm might suffer losses at t^* when its bad bank was closed, the firm might also have improved by the time it is matched with a new bank at $t^* + k$. Indeed, while the estimates on the $Profit_{f,t^*} < 0$ variable are still insignificant, we reveal negative and highly significant estimates on the $Profit_{f,t^*+k} < 0$ variable. Economically, the underlying effect is sizeable: as compared to a profitable firm, the firm with losses reported at the moment of new matching has a 33.2% lower chance for this match.[24] Recall that the average duration of the spell, i.e., the time it takes to establish a new firm-bank match, equals 35 months in our sample.

The regression results above suggest an absence of 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 bad bank is closed do not predict whether the firm finds a new bank match in the future.

We further hypothesize that it may be important to distinguish the cases in which the firm matches with a new bad bank–that has not yet had a chance to be shut down–from those with a good bank. Put differently, we hypothesize that bad firms are more likely to be sorted to bad banks whereas good firms are more likely to match with good banks. Because the CBR's cleansing policy stretched in time for over five years, it gave the firms that were separated from bad banks an opportunity to be matched again with with another (not yet shut down) bad 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 bad bank vis-a-vis never match and $\lambda_2(\cdot)$ for the case when the firm seeks to match with a new good bank vis-a-vis never match:

$$\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(\alpha_j + \alpha_{j,bc} + \alpha_{j,r} + \alpha_{j,i} + \text{Firm.Quality}_{f,t-1}\text{B}_j + \mathbf{C}_{f,t-1}\Gamma_j\Big), \qquad (2)$$

where $j = 1$ stands for regression with bad bank matching and $j = 2$ for good bank matching. Other notations, as well as sample size and time span, remain the same.

Table 4 reports the estimation results on the duration regressions with the sample split in equation (2). Columns (1)–(3) present the estimates from regressions of the matching with bad

[24] The effect is computed as $exp(-0.403 * 1) - exp(-0.403 * 0) = -0.332$.

banks and columns (4)–(6) with good banks, for different measures of firm quality. For the duration analysis of matching with bad banks the sample consists of 6,222 firms and nearly 200 new bad matches, and the average duration of the spell changes from 35 months, which was true across all matches, to 19 months. For the matches with good banks, the sample comprises of 5,551 firms and 715 new good matches, and the average duration of the spell rises to 42 months. Note that the 200 new bad matches and 715 new good 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 flips its sign and turns *positive* and highly significant in the regressions of matching with bad banks (column 1). Conversely, in the regressions of matching with good banks respective estimate is negative and also highly significant (column 4). Jointly, these estimates support our hypothesis on endogenous sorting of firms: conditional on bad bank closure, bad firms match with another (still operating) bad banks whereas good firms establish relationships with good banks. Economically, both estimates imply large effects: as compared to a firm with 0 days of NPLs, a firm with 90 days of NPLs is by 35.4% more likely to match with another bad bank and by 16.2% less likely to join a good bank in the future.[25]

Next, we replace the log $DNPL_{f,t^*}$ variable by $Profit_{f,t^*} < 0$ to check whether having negative profits also predicts sorting of bad firms to bad banks and good firms to good banks, as we reveal above. However, as can be inferred from columns (2) and (5) of Table 4, this is not the case. Indeed, in the regression of matching with bad banks, we obtain negative, not positive, coefficient on the $Profit_{f,t^*} < 0$ variable, meaning that firms that had negative profits at the moment of their bad bank closures, are *not* more likely to establish a match with another bad banks in the future. Economically, the underlying effect is very large: a firm with negative profits at t^* has a 77.1% less chance to match with another bad 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 good banks, we get near zero and insignificant coefficient on the $Profit_{f,t^*} < 0$ variable, reflecting that firms that were facing losses during the closure of their

[25] The effects are computed as (i) $exp(0.155 \cdot 90) - exp(0.155 \cdot 0) = 0.354$ and (ii) $exp(-0.091 \cdot 90) - exp(-0.091 \cdot 0) = -0.162$.

bad banks are *not* less likely to match with good 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 bad bank fails but at $t^* + k$ when the firm matches with another bad bank, column (3), or with a good 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 new match is by 41.4% less likely to join a new bad bank and by 31.9% less likely to join a good bank, as compared to a profitable firm.

4.2 Robustness Checks

One concern towards our splitting duration regressions is that we separately study matching with bad and matching with good banks. To address this concern, we run a *multinomial regression model* in which we have all three options for a firm: never match (0), match with a bad bank (1) and match with a good bank (2). As Table B.I show, the estimation results are qualitatively and even quantitatively very close to the baseline.[26]

Another concern is that we omit *macroeconomic and regional characteristics*, which both might affect the CBR's intention to close problem banks.[27] 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 C.I, 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 dramatically large concentration of regional credit markets that could led to higher risk-taking by small banks.

Further, one could doubt that the baseline effects are valid only for the firms that have single bank relationships. We thus re-run our splitting duration regressions on the sample of firms that have *multiple bank relationships*, with at least one of them being bad. Table D.I clearly indicates

[26] The estimates are performed with the multinomial logit model instead of competing risks duration model. This because of the issues with the convergence of the likelihood function.

[27] In 2014–2015, the Russian economy had experienced a double shock: internal factors led the economy to yet another recession and external forces, e.g., deterioration of the commodities terms of trade and the Western economic sanction (Ahn and Ludema, 2020), had strengthened the internal ones.

that there are no significant effects of the firm quality on the likelihood, and *direction*, of new bank matching. Th estimates on the log $DNPL_{f,t^*}$ and $Profit_{f,t^*} < 0$ are insignificant in both regressions of matching with bad and good banks. The only effect that preserves is the one describing the negative relationship between a firm's losses at the moment of switch, i.e., $t^* + k$, and the chance to match with a good bank. Jointly, these results imply that firms behave *strategically*: if they establish multiple bank relationships, they may use bad banks to 'store' the worst part of their debt while servicing the best part of their debt in good banks. When their bad banks fail, the firms tend to substitute the lost part of credit at existing 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-days threshold. Recall that 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. It is likely that new banks, when choosing between two firms to establish a match, 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. Distinctly, if one of the firms had, say, 120 days, not 30 or 60, then a good 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 obtain that the estimated coefficient on the new binary variable is insignificant for matching with bad banks and remains negative and highly significant for matching with good banks.

We then go further and 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 E.I. In column (1) where we analyze matching with new bad 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. Strikingly, and we were not able to see it before categorizing, the estimated coefficient on $150 \leq DNPL_{f,b,t} \leq 180$ (bin 6) and $DNPL_{f,t^*} \geq 180$ (bin 7) 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 bad banks is positive for small and moderate magnitudes of loan delinquencies (below 90 days) but turns negative for very large delinquencies (above 150 days). bad banks, despite being bad, are not willing to accept the hopeless firms.

In column (2) with the results on matching with new good 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, good banks really prefer to establish matches with the firms that had virtually no bad debts in the closed bad 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 bad and good banks, with mid-sized firms revealing the largest likelihoods.[28] We also obtain that more levered firms are less likely to find a new match, conditional on surviving to the moment, whereas liquidity seems having no effect on the hazard rate.

Overall, our regression analysis has shown that firms with more days of NPLs accumulated by the moment of their bad bank closures are *more* likely to match with another (still operating) bad banks and are *less* likely to establish relationships with good banks. This favors endogenous firm-bank matching that appears under a stretched-in-time regulation policy targeting bad banks detection. Turning from granular level, i.e., loan-month, to more aggregated level, i.e., firm-year, does not allow us to obtain the same result. Firms with negative annual profits, either at the moment of bad bank closure or the moment of matching with new banks, are always *less* likely to establish new relationships with banks, no matter of bad or good type.

5. AUTOPSY OF NEW BANK-FIRM MATCHES

Next, we explore some possible channels behind our baseline finding that a bad firm is more likely to match with a bad bank following the closure of its old bad bank, while a good firm is likely to end up in a match with a good bank.

[28] This is consistent with an observation that small firms usually face more problems with getting credit while large firms may either use their own sources of funds or substitute domestic credit by the 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 on *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 **?**, the borrowing abroad is cheaper for large companies operating in EMEs than getting finance at home markets.

5.1 Channel 1: Single bank group ownership

One possible explanation of our baseline results is that bad firms, having faced their current bad banks closure, simply matched with another (still operating) bad bank that has *the same* owner. More generally, several banks may constitute a bank holding group, or the same persons may appear in the board of directors in different (formally not related) banks. We refer to these issue as the *single bank ownership*, for simplicity.

Our aim in this section is thus to capture the effects of firm quality on the firms' matching with single-owned banks, and compare it with the baseline result. For this purpose, we divide the sample into two parts: one with the firms that simply move within the banks owned / governed by the same persons, and the other with the firms that match with really new banks. Recall that single bank ownership does not necessarily imply bad banks, though intuitively this makes a sense. good banks may also be gathered in a group owned / governed by the same persons, e.g., as a result of M&A.

To do so, we need the information on bank holding groups, bank M&As, and names, surnames and other relevant personal information on each and every member of the board of directors of each and every bank operated(-ing) in the Russian banking system over the last decade, at least before the active phase of the tight regulation policy undertaken by the Bank of Russia in mid-2013. Of course, there is no readily available database that meets our demand.

Fortunately, such information can be manually collected through the web-site of nation-wide banking media resource banki.ru. This web-site provides the real-time information on each and every bank operated in Russia at least from the beginning of 2000s. The information is structured in alphabetical order by the banks' names and cities where they operate, thus simplifying the navigation and searching of desirable information.[29] When clicking on a bank's name, one obtains the full information on the history of the bank (when it was created, by whom, for which business purposes, etc.), list of its current operations and cities where it operates, real-time updated daily news, and, what is particularly important for us, the bank's *owners* (with per cent shares in equity capital) and *board of directors*.[30] The banks that failed are located in a separate page called

[29] As an example, here comes the list of banks operating in Moscow: https://www.banki.ru/banks/moskva/list/?letter=%C0.

[30] For illustrative purposes, we provide an example of *Alfa-bank*, the Russia's largest non-state owned bank inside the top-10 banks by the size of assets: https://www.banki.ru/banks/bank/alfabank/ (In Russian). Overall, the

"*memory book*".[31]

With these detailed bank-level information at hand, we have covered all banks operated in Russia from 2010 till 2021 and create a unique database.[32] The algorithm is simple: collect manually all persons' names and surnames and / or the name of the banks, if it is explicitly stated that the banks own a given bank, and check whether they appear in *different* banks: if yes, drop from further survival regressions; if not, keep in.

The main challenge, however, is as follows. To simplify things, there are situations in which a firm that faced closure of a bank A, which in turn together with some bank B constituted group AB, then matches with a new bank C, which enters a group CD jointly with a bank D. We thus create a *categorical* variable $Single.Group_{b,t}$ which equals 1 for the AB group, 2 for the CD group, ... n for the last revealed group, and 0 for all those banks for which we were not able to find intersections with other banks. Note that a *binary* variable (1 if single, 0 if not) would not work in our further regressions because, for instance, a firm could match with C or D after A, which are in a different banking groups.

Overall, we discovered that among the 956 banks presented in the database from 2010, as many as 238 banks have overlappings with other banks entering the same banking holding companies or owned / governed by the same persons. When we then merge our main database with the resultant categorical variable $Single.Group_{b,t}$, we reveal that from 50 to 75% of firms that ever had relationships with bad banks matched with another (still operating) bad banks owned / governed by *the same* persons. These are dramatically large numbers which may substantially affect our results.

We re-run the splitting duration regressions (2) with all different bank groups being dropped, i.e., under the condition that $Single.Group_{b,t} = 0$. The estimation results appear in Table 5. In columns (1)–(3) with the results of matching with really new bad banks, we have the number of observations being dropped from 257,190 to 107,220. Number of firms also reduces, from 6,069 to 2,757. In columns (4)–(6) describing the results of matching with really new good banks, the

largest privately-owned bank in Russia owns / controls at least four other commercial banks in different cities. The bank constitutes on of the 12 systemically important financial institutions (SIFI) and continues its operations as of 2021.

[31] https://www.banki.ru/banks/memory/.

[32] The database is represented as an MS Excel file which we refer to as the *common ownership database*. We disclose the database through our web-site and believe it could be useful in further research.

number of observations (firms) drops in a similar manner—from 257,681 to 107,434 (6,080 to 2,764). As can be inferred from column 1 of Table 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. What is interesting is that the estimated coefficient is not negative, as one might expect. We think that it may reflect either inferior expertise in bad banks or the bad banks' intentional or forced (by market's rivals conduct) exposure to adverse selection of borrowers. Further in columns (2) and (3), we obtain 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 then reveal no qualitative differences with our baseline result; quantitatively, the estimates imply even stronger effects than in respective part of the baseline result.

Overall, our estimation results so far have shown that, following bad bank closures, bad firms are *more* likely to match with another (still operating) bad banks, especially if the later are owned / governed by the same persons or entities, and they do it *faster* than good firms. Good firms, in turn, are more likely to match with new good banks, no matter if the later share the same owners / governors.

5.2 Channel 2: Surprising bank closures

Another idea for capturing the channels of new firm-bank matches is that not all bad bank closures are equal: in the presence of bank misreporting on the actual quality of assets, some closures may be treated as *surprising* and the others as *expected*. We call surprises those cases in which a bank's fraud detection and subsequent policy-induced closure at t^* was *not* predictable from the bank's balance sheet data before t^*. If, by contrast, the bank's closure was predictable before t^*, we refer to it as expected closure. The point is that, at $t^* + k$ when a firm is willing to match with a new bank, the bank may suspect the firm is of bad type if the policy-induced closure of the firm's previous lender was predictable. In this case, the bank—even if this is a still operating bad bank—may be less likely to accept the firm.[33]

[33] Recall that our regression analysis in the previous sections has shown that, following the bad bank closures, firms with losses are less likely to be matched with new banks, no matter bad or good.

To capture the effect of surprising bank closures on endogenous firm-bank matching we suggest the following *two-stage approach*. At the first stage, we run a standard logit regression of bank closures and sort all failed banks by their predicted probabilities into two groups: below and above a certain threshold ("low" and "high" probabilities of closure). We construct an indicator variable $Surprise.Closure_{b,t}$ that equals 1 if predicted probability of a bank's b closure at month t was a surprise (i.e., below the threshold), and 0 if else. At the second stage we run two versions of the splitting duration regression: one under condition that $Surprise.Closure_{b,t} = 1$, i.e., for surprising bank closures, and the other under condition $Surprise.Closure_{b,t} = 0$, i.e., for expected bank closures.

The first stage results: predicted probabilities of fraud-related bank closures. To preserve space, we present and describe in detail the logit estimation results in Appendix F, see Table F.I. Here, we focus on the logit post-estimation results by plotting the time evolution of the predicted probabilities of bank closures in different percentiles of the banks' distribution, see Fig. 11. As can be inferred from the figure, the predicted probabilities are evolving at close-to-zero levels before the policy change in 2013M7. But then they turn rising sharply during the active phase of the policy, i.e., between 2013M7 and 2018M2. During the active phase, the probabilities varied from almost zero to as much as 71% with the mean equaled to 0.5%.[34]. It is also notable that the probabilities are peaking in 2016–2017, at least a 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 is in line with our findings above that the policy and macroeconomic conditions were fairly orthogonal to each other.

We then choose a simple unconditional threshold equaled 0.5% of the monthly probability of being closed to disentangle surprising and expecting bank failures.[35] This gives us nearly 250 bank failures below the threshold (the surprises) and 150 above.

The second stage results: sample split regressions. The estimation results appear in Table 6. Columns (1) and (2) contain the results obtained under condition $Surprise.Closure_{b,t} = 1$, namely, for the subsample of surprising bank closures. Columns (3) and (4) then report the results with the alternative condition that $Surprise.Closure_{b,t} = 0$, that is, for the *expected* bank closures. Under

[34]Note that this is measured at the month-level, so that if we aggregate it to the annual level (mimicking the use of annual data), it would be equal to 6%)

[35]We also apply 1% and 1.5% thresholds for robustness checks. The results do not change qualitatively.

the surprise condition, we have 168 new firm–bad bank matches and 611 new firm–good bank matches, whereas under the alternative condition we have only 32 and 104 such matches. This means that the share of surprise cases dominate expected bank closures, and firm are 3-4 times more frequently match with good banks than with bad banks.

The estimates presented in Panel 1 of Table 6 demonstrate that our baseline results are fully driven by the surprising bank closures. In the duration regressions of matching with new bad banks, the estimated coefficient on the log $DNPL_{f,t^*}$ variable is positive and highly significant under the surprise condition (column 1) but negative and insignificant under the expected bank closure condition (column 3). Moreover, under the surprise condition the magnitude of the effect rises by one third as compared to the baseline. Similarly, in the duration regressions of matching with good banks, the estimated coefficient on the log $DNPL_{f,t^*}$ variable is negative and highly significant under the surprise condition (column 2) but renders insignificant under the other condition (column 4). Again, the magnitude of the coefficient increased by also one third comparing to the baseline.

Further, in Panel 2 we replace the log $DNPL_{f,t^*}$ variable with the $Profit_{f,t^*} < 0$ and $Profit_{f,t^*+k} < 0$ counterparts, reflecting whether firms had negative profits at the moment of bad bank closure or when they match with new banks. The estimation results are fully consistent with our idea on surprising bank closures. As one can observe, we obtain significant coefficients on the negative profits variables only in columns (1) and (2), where we impose the surprise condition, whereas in columns (3) and (4) respective coefficients are never significant.

Overall, these results support our hypothesis that new banks pay attention to where the firms come from: *predictably* bad banks or the bad banks whose closures were a surprise.

5.3 Regional credit markets concentration

Another potential channel fueling our baseline results on endogenous firm-bank matching is variation in regional credit markets concentration. It is clear that the CBR's cleansing policy was associated with rising concentration because many banks were closed by the regulator. After each and every next bad bank closure firms have less opportunities to find a new bank match. In particular, bad firms become increasingly more restricted in their abilities to match with *still operating* bad banks. If the firms really need bank credit, this rising restriction may force them to improve in order to be accepted by good banks. Good banks, however, may be less willing to do so to protect their market power from the uncertainty associated with financing the projects of bad firms. Recall Fig. 7 which illustrates a tendency that more concentrated regional credit markets reveal less days of NPLs than less concentrated markets (abstracting from the Central Federal District for which it is not true).

To test these hypotheses empirically, we slightly modify our duration regressions by introducing a cross-product of the regional HHI concentration measure and a proxy for firms' quality:

$$\lambda_{\jmath}\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(\alpha_{\jmath} + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \beta_{\jmath,1}\text{Firm.Quality}_{f,t-1} + \mathbf{C}_{f,t-1}\Gamma_{\jmath}$$
$$+ \beta_{\jmath,2} HHI.credit_{r,t-1} + \beta_{\jmath,3} \cdot \text{Firm.Quality}_{f,t-1} \times HHI.credit_{r,t-1}\Big), \tag{3}$$

The estimation results emerge in Table 7, where Panel 1 contains the results with firm quality measured as the days of NPLs, and Panel 2 as 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). Notably, the same qualitative result follows from Panel 2 where we replace the days of NPLs with the variables of negative profits. Indeed, the coefficients on the cross-products of $HHI.credit_{r,t-1}$ with either $Profit_{f,t^*} < 0$ or $Profit_{f,t^*+k}$ are both positive and significant.

These estimates are unexpected and mean that rising credit markets concentration observed in 2010s was *unlikely* to prevent bad firms from matching with still operating bad banks but, at the same time, it *could* facilitate new matches between bad firms and good banks. One possible interpretation is that the good banks were willing to extract a rent from relationships with bad firms by setting higher interest rates, managing the higher risk–larger profit trade-off. Another is that the good banks operating in the regions with highly concentrated credit markets possess more developed skills in evaluating projects. If so, the good banks may provide a valuable expertise for bad firms and thus help them to improve.

6. DID FIRMS ANTICIPATE BAD BANK CLOSURES?

Our results in previous sections are conditional on the moment t^* when a firm f faces a policy-induced closure of its bad bank. We were implicitly assuming that the bad bank closure is a *shock* to the firm. However, one's concern could be that it *might* not be a shock: the firm could possess some information about the upcoming closure at $t^* - h$, where h should be reasonably small (few months before the failure). Preparation to close a bad bank takes time, and there could be information leakage from the regulator to the market participants. And if a firm indeed possesses such information then it might be reasonable for the firm to *switch* from the about-to-fail bank to a new lender in advance.

There are at least two reasons why it makes sense for the firm to switch in advance from the about-to-fail bank b_i to some other bank(s) b_j $(j \neq i)$. First is that the firm may use the informational advantage to *signal* to other banks that it is not willing to continue with the about-to-fail bank because the firm seeks for long-run stable relationships with its lender(s). Second, the firm may also realize that, if not switching in advance, its debts are likely to be sold at auctions during the period of the bad bank receivership (Granja et al., 2017), in which case the firm has to deal with a bank b_j or other agents (competing firms, collector agencies, households) that buy the firm's debts.

Of course, the firm may also decide to switch in advance from the about-to-fail bank *occasionally*, that is, not because of knowing something about the upcoming bank closure but just because the firm's loan is maturing at $t^* - h$ and the firm is not willing to continue with the bank. Unfortunately, with the data at hand (only the days of NPLs, no maturity or other relevant information at the loan level are available up until 2017), we cannot distinguish these cases from the in-advance switching based on information leakages.

Note also that in this section we turn using the term "switching" instead of "matching". This is because decisions to break the current relationship with the about-to-fail bad bank, if any, and searching for new creditors is likely to be done by the firms, not by the new creditors (no auctions on the firms' debts before t^* are possible).

As natural placebo tests on the validity of our baseline results we run two different regression exercises: whether firms switch in advance or, if not, whether the firms start to delaying loan payments within a reasonably short period *before* the banks' closures.

6.1 An in-advance switching?

We modify our duration regression analysis as follows. We now consider a period of $[t^* - h, t^*)$, i.e., h months before the closure of firm's f bank b_i, and appeal to the logit instead of duration model.

The composition of the right-hand side variables remains the same. We choose $h = 6$ months, for concreteness. We also change accordingly the definition of our dependent variable. Now, it is a binary variable $Switch_{f,t}$ that equals 1 if a firm f switches in-advance to some new bank $b_j \neq b_i$ at $t \in [t^* - h, t^*)$, and zero if the firm continues with the current bank until the bank is detected for fraud and closed. As before, $j = 1$ reflecting a switching to another bad bank and $j = 2$ a switching to good bank. Regression reads as:

$$\mathbf{Pr}(Switch_{f,t} = \jmath \,|\, \mathbf{X}_{f,t-1}; \Theta) = \Lambda\Big(\alpha_\jmath + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_\jmath + \mathbf{C}_{f,t-1}\Gamma_\jmath\Big), \qquad (4)$$

The estimation results are reported in Table 8. We now have only about 30,000 firm-month observations, which is less than in the reference by a factor of 10. Number of firms equals 3,190 and the number of in-advance switches reaches 1,950. As can be inferred from the table, estimation of the in-advance switching regressions deliver no significant coefficients on the log $DNPL_{f,t^*-6}$ or $Profit_{f,t^*-6} < 0$ variables. This is true for both switching to bad banks regressions (columns 1 and 2) and switching to good banks regressions (columns 3 and 4). Moreover, the signs of the estimated coefficients are flipped compared to the baseline (except for the one in column 1).

Regarding the other firm controls, we obtain that the coefficients on firm size and its square are insignificant, meaning that *larger* and *smaller* firms are not more likely to switch in advance. The estimated coefficient on firm leverage is negative and significant in the case of in-advance switching to good banks. Finally, liquidity negatively and significantly affect the likelihood of in-advance switching to bad banks.

Overall, the logit estimation results reveal that firms' in-advance switching from about-to-fail banks occur not because the firms are bad or good, in terms of loan quality and / or profitability, and not because the firms know something about the upcoming closures of their current bad banks. The switchings, if any, are more likely to take place for common reasons (expiration / full repayment of loans, etc.).

6.2 Delaying loan repayment before the bad bank closures?

Although firms are unlikely to switch from about-to-fail banks in advance, the firms may still possess an information on the upcoming bank closures and start delaying the payments on their loans. The later may make sense because the monitoring ability of the about-to-fail bank declines and the firms can (partly) re-direct funds to other purposes.[36] More generally, the policy of active bank license revocation may itself create an expectation among firms that their banks can also be closed for fraud. If so, this can affect the firms' decision on the schedule of loan repayments.

In case of single firm–(bad) bank relationships, we hypothesize that bad firms, as proxied with negative profits, may act strategically and thus raise loan delinquencies:

$$\Delta DNPL_{f,b,t} = \alpha_f + \alpha_b + \alpha_{b,f} + \alpha_t + \alpha_r + \alpha_i + \alpha_{bc} + \beta \cdot \mathbf{1}\{Profit_{f,t^*-h} < 0\} + \varepsilon_{f,b,t} \tag{5}$$

where $\Delta DNPL_{f,b,t}$ is a one-month change in the days of NPLs reported by a firm f that has relationship with (still operating) bad 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. With the battery of the fixed effects employed, we are aimed at capturing 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*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.

We present the estimates of regression (5) in Panel 1 of Table 9. Strikingly, we obtain that the estimated effect of the variable $Profit_{f,t^*-h} < 0$ on 1-month changes in $DNPL_{f,b,t}$ is insignificant at any small horizon prior to bank closures. This means that bad firms *do not* begin to raise loan delinquencies before the closures of their bad banks.

Let's now consider the case of *multiple* firm-bank relationships. We need to modify equation (5) so that a firm may have relationships with at least one (still operating) bad bank and at least one good bank simultaneously. The focus explanatory variables here are not only $Profit_{f,t^*-h} < 0$, as before, but also its product with the bad bank indicator variable, $Bad.Bank_b$. This variable equals 1 if a bank ever fails due to fraud revealed, and 0 if survives till the end of the sample. We hypothesize that, for strategic reasons, firms may hold the worst part of its debts in bad banks and serve their best-quality debts in good banks. And if the firms possess information on the upcoming

[36] Of course, the firms realize that, after the banks are closed, they will be forced to re-new payments to receiver. But until then they could exploit their informational advantage.

bad bank closures, we may hypothesize further that bad firms could start to raise loan delinquencies in the bad banks. The resultant equation reads as:

$$
\begin{aligned}
\Delta DNPL_{f,b,t} = \alpha_f + \alpha_b + \alpha_{b,f} + \alpha_t + \alpha_r + \alpha_i + \alpha_{bc} + \beta_1 \cdot \mathbf{1}\{Profit_{f,t^*-h} < 0\} \\
+ \beta_2 \Big(\mathbf{1}\{Profit_{f,t^*-h} < 0\} \times Bad.Bank_b \Big) + \varepsilon_{f,b,t}
\end{aligned} \tag{6}
$$

The estimation results on regression (6) emerge in Panel 2 of Table 9. The estimated coefficients on $Profit_{f,t^*-h} < 0$ and its product with $Bad.Bank_b$ are both insignificant at any small horizon h prior to bad bank closures. We therefore cannot say that (i) firms, bad or good, turn delaying loan payments in their bad banks before the banks' closures and (ii) bad firms do so by more than good firms.

Overall, our estimates in this section show that bad firms *do not switch in advance* and continue with their about-to-fail banks till the end. When continuing, bad firms do not raise loan delinquencies—neither in absolute terms nor as compared to good firms.

7. EFFECT OF BAD BANK CLOSURE ON FIRM PERFORMANCE

Our placebo test has shown that it is unlikely that the firms switch from their current bad banks in advance of the banks' closure; instead, the firms stay with the bad banks till the end. We now ask what happens to the firms' performance *after* they face their current bank closures and *before* they find a new bank match. Does the firms' performance deteriorate (because less credit is now available, as is shown by Chodorow-Reich, 2014 for crisis times or by Chopra et al., 2020 for normal times) or does the firms' performance improve (due to breaking the negative lock-up effect, as suggested by Liaudinskas and Grigaite, 2021 in case of closing distressed banks)?

To answer this question, we appeal to the difference-in-differences approach with time-varying imposition of treatment (TV-DID, Goodman-Bacon, 2021). Let's define treatment as a bad bank b closure which affects a firm f at time t^*. Given this timing, let's further define an indicator variable $POST_{\{t \geq t^*\}}$ that equals 1 after the firm's f bad bank closure, and 0 before. To proxy for treatment, we use the indicator variable $Bad.Bank_b$ which equals 1 if a bank b is ever closed due to fraud (we employed this variable in our anticipation exercises above). Thus, *the treatment group* consists of all those firms, bad and good, that faced their bad bank closures at different points in time during 2013–2020.

To construct *the control group*, we match firms on the set of observable characteristics using the nearest neighborhood estimator of Abadie and Imbens (2011). The characteristics are as follows: firm size, leverage, liquidity, return on assets, and annual growth of total assets. We follow the suggested "1:4 rule of thumb" and match a firm f that has faced its bank closure at t^* ("treated") with four similar firms that (i) also have relationships with bad banks and (ii) have not faced closures of their bad banks within 2 years before and after the firm f ("controlled"). That is, we consider a moving window $[t^* - 2, t^* + 2]$, and thus our treatment group includes the firms that faced their bad banks' closures at most in 2018, because our sample ends in 2020 (i.e., last treated firm appears in the end of 2018 since, by construction, we require it is matched with four control firms that must face their bad bank closures within 2019–2020).

Bad bank closure may be viewed as a *credit supply shock* to firm performance. In choice of the measures of firm performance, we are motivated by rapidly growing literature on the real effects of financial shocks. This literature typically considers the effects of credit supply shocks on firms' employment (Chodorow-Reich, 2014), investment and sales (Gropp et al., 2018; Degryse et al., 2019a; Chopra et al., 2020), among others. We employ those variables, except for investment,[37] in our analysis because the firms that faced bad bank closures could be restricted in obtaining new loans which they used to launch projects and support employment. We also add firms' defaults rates which are likely to react on changes in sales and employment. Given that the firms' current bad banks are closed and new banks have not yet emerged, we conjecture the firms have to substitute the lost credit from the bad banks by borrowing from other sources (other firms, placing bonds at financial markets, etc.). Finally, we consider the firms' profits, because changing sales, employment, and leverage are likely to adjust the firms' net income.[38]

Importantly, we realize that it could be rather hard to reveal any effect stemming from the firm-bank-month level to the firm-year level since firms may have multiple relationships with banks and may simply substitute lost credit in bad banks by additional borrowings in other banks. As,

[37]The firm-level data on investment provided by SPARK-Interfax, the database we use, contains a very large number of missing values on investment so that, if we would use it, the total number of observation would fall by a factor of 10, at least.

[38]Recall the limitations of the loan-level data provided by the Bureau of Credit History (BCH) for the period of 2008–2018 which covers the active phase of the CBR's policy. Due to these limitations, we are not able to study the effects of bad bank closures at the firm-bank level—i.e., on interest rates, amount of loans, maturity, etc., as is done in many studies, e.g., Chopra et al. (2020); Bonfim et al. (2020), among others.

however, Degryse et al. (2019a) show, turning to *single* firm-bank relationships allows for revealing the desired real effects. In what follows, we thus run our TV-DID regressions for the subsamples of firms that had only one (bad) bank at the moment of the bank's closure. Recall that the baseline results from our duration regressions of firm-bank matching are also pertain to single firm-bank relationships (for multiple relationships the baseline results do not hold).

To formalize the ideas, we specify the following TV-DID regression:

$$\begin{aligned} Y_{f,t} = \alpha_f + \alpha_t + \beta_1\Big(Bad.Bank_b \times POST_{\{t \geq t^*\}}\Big) + \\ + \beta_2\Big(Bad.Bank_b \times POST_{\{t \geq t^*\}} \times Bad.Firm_{f,t}\Big) \\ + X'_{f,b,t}\Psi + \varepsilon_{f,t}, \text{ if } t \in [t^* - 2, t^* + 2] \end{aligned} \tag{7}$$

where $Y_{f,t}$ is a measure of a firm's f performance, among which we consider (i) a binary variable of whether a firm defaults in year t, (ii) leverage (bank and non-bank debts) to total assets ratio, (iii) number of workers to total revenue ratio, (iv) total revenue to total assets ratio, (v) profit to total assets ratio. $X_{f,b,t}$ includes the rest of the subproducts between the $Bad.Bank_b$, $POST_{\{t \geq t^*\}}$ and $Bad.Firm_{f,t}$ variables, as well as firm size and its square, leverage, and liquidity to capture any residual differences between the "treated" and "control" firms remaining after the 1:4 nearest neighborhood matching. In case (i), we run a panel logit estimator and in cases (ii)-(v) we perform panel FE estimations. We also censor those observations when $t^* + 2$ overlaps with establishing a new firm-bank match to insure we are analyzing firm performance before the firm finds a new bank.

A-priori we are agnostic about whether the closure of bad banks have positive or negative effects on firm performance. We formulate the following two alternative hypotheses. The *firm deterioration hypothesis* states that bad bank closures lead to deterioration of firm performance, and the more so for bad firms. The mechanism runs through termination of credit flows to the firms. There are switching costs that the firms have to pay to find new lenders (recall that the average time it takes a firm to match with a new bank is 35 months).

Alternatively, the *firm improvement* hypothesis implies that firms' performance improve after the bad bank closure, but the less so for bad firms. The mechanism is that policy-induced bad bank closure is an exogenous break of the firms' lock-ups in bad banks. Firm-bad bank matching is not random, and is likely to be explained through inferior firm characteristics that did not allow the firm to match with a good bank in the past. When joining the bad bank, the firm might suffer from either (i) higher interest rates on loans or (ii) inferior expertise of the bad bank on what concerns perspectives of the firm's projects. An exogenous break of this negative lock-ups allows the firm to renegotiate its loan conditions and find an appropriate bank expertise.

The estimation results of equation (7) appear in Table 10. After the nearest neighborhood matching and restricting the sample of firms by imposing condition $t \in [t^* - 2, t^* + 2]$ years, we have only about 10,000 to 19,000 observations at the firm-year level. In column (1), where the dependent variable is *firm defaults*, we obtain negative, not positive, and at least marginally significant coefficient β_1 on the $Bad.Bank_b \times POST_{\{t \geq t^*\}}$ variable. This means that the firms that have already faced their bad bank closures ("treated") turn to reducing their risk exposures before establishing new bank matches, as compared to the firms that have not yet experienced their bad bank closures ("control"). Further, it appeared that $Bad.Bank_b \times \text{POST}_{\{t \geq t^*\}} \times Bad.Firm_{f,t} = 1$ perfectly predicts the firms' defaults, and thus the variable was dropped from the estimations (marked as "n/a" in the table). We thus obtain two opposing outcomes: an empirical evidence favoring the firm improvement hypothesis for good firms and, conversely, an indirect evidence supporting the firm deterioration view for bad firms. And this strikingly echoes the baseline result from the duration analysis above that bad firms go to another bad banks while good firms match with good banks following bad bank closures.

In an attempt to understand why good "treated" firms might reduce their risk exposures, we appeal to the credit registry loan-level data on interest rates available from 2017 at monthly frequency. In a regression of interest rates on the bad bank indicator variable (bank level), loan quality indicator variable (firm-bank-month level), and the product of the two we obtain negative and highly significant coefficient on the interaction of bad banks and loan quality. This means that, within *the same* bad bank, firms with poorer quality *pay less* on their loans while firms with better quality *pay more*. Further, in a simple regression of loan quality on bad firms we show that bad firms are assigned to poorer quality categories, meaning that there is a clear mapping between the two (see Table 12). Overall, that the bad firms have price discounts in bad banks is absolutely consistent with sharing common owners who thus support the non-financial businesses of the bad firms.

Firms' leverage, column (2) of Table 10. We obtain a positive but insignificant estimate on the

$Bad.Bank_b \times POST_{\{t \geq t^*\}}$ variable. This implies that the "treated" firms sustain, not reduce, their leverage ratios at the *same* levels after facing their bad bank closures.[39] Further, the estimated coefficient on the $Bad.Bank_b \times \mathrm{POST}_{\{t \geq t^*\}} \times Bad.Firm_{f,t}$ variable is also positive and at least marginally significant. This indicates that bad "treated" firms, as compared to good "treated" firms, raise their borrowings from other firms by as much as 10 percentage points of their total assets. This is a sizeable effect, given that the mean leverage ratios of the firms in our sample are bounded between 75 and 95%. Overall, being a firm that suffer losses but raise borrowings again supports the firm deterioration hypothesis for bad firms.

Total revenue, column (3) of Table 10. We obtain a positive and highly significant coefficient β_1 on the $Bad.Bank_b \times POST_{\{t \geq t^*\}}$ variable and negative and significant (at 5% level) coefficient β_2 on the $Bad.Bank_b \times \mathrm{POST}_{\{t \geq t^*\}} \times Bad.Firm_{f,t}$ variable. In absolute terms, β_2 exceeds β_1 by a factor of 2. This means that we have heterogeneous treatment effects on the firms' revenues in our sample: following their bad bank closures, good "treated" firms are able to increase their income, whereas bad "treated" firms are not, as compared to the "control" firms. Assuming the firms face the same prices on their outputs, our results here may imply that good "treated" firms increase the quantity of their goods / services produced, and bad "treated" firms reduce their quantities of outputs. Though we cannot test it directly, it seems that, following their bad bank closures, good "treated" firms start using their inputs more efficiently while bad "treated" firms less efficiently. Why efficiency rises in the first case and shrinks in the second? It should be the case that bad banks lacked behind good banks in terms of assessing the firms' projects perspectives. Bad firms were likely to match with bad banks in the past due to sharing the same, or common, owners who might support the businesses of the bad firms. Closure of bad banks is thus a negative shock to the bad firms' income. On contrary, good firms were unlucky to match with bad banks in the past. And thus good firms might misallocate credit resources across their projects due to inferior expertise of their bad banks. Closure of bad banks is a positive shock to the good firms' income. Overall, our results are likely to support the firm improvement hypothesis for good firms and firm deterioration hypothesis for bad firms.

Employment, column (4) of Table 10. We obtain a negative and marginally significant coefficient

[39] This, in turn, means that those firms that lost the *lines* of credit are substituting them by borrowings from other (non-financial) firms. Or it may also be case that the firms are not borrowing anymore and just repay their existing loans to the bad banks' receivers. With the data at hand, we are not able to distinguish between these cases.

β_1 on the $Bad.Bank_b \times POST_{\{t \geq t^*\}}$ variable and positive and significant (at 5% level) coefficient β_2 on the $Bad.Bank_b \times \mathrm{POST}_{\{t \geq t^*\}} \times Bad.Firm_{f,t}$ variable. In absolute terms, β_2 exceeds β_1 by a factor of 2. This means that we have heterogeneous treatment effects on employment in our sample: following their bad bank closures, good "treated" firms turn to reducing their labor force to total revenue ratios, whereas bad "treated" firms expand the labor force loading on their total revenue, as compared to the "control" firms. Given that good "treated" firms also raise their total revenues (recall the results from column (3)), we obtain that their revenues grow faster than the number of workers employed. The productivity of good "treated" firms is therefore also rising, thus once again supporting the firm improvement hypothesis for good firms. Conversely, bad "treated" firms experience decline in productivity since income is falling and employment is rising, which favors the firm deterioration hypothesis for bad firms.

Profit, column (5) of Table 10. We obtain positive but insignificant coefficient β_1 on the $Bad.Bank_b \times POST_{\{t \geq t^*\}}$ variable and negative and insignificant coefficient β_2 on the $Bad.Bank_b \times \mathrm{POST}_{\{t \geq t^*\}} \times Bad.Firm_{f,t}$ variable. The signs are consistent with the firm imrovement hypothesis for good "treated" firms and firm deterioration hypothesis for bad "treated" firms. However, since the effects are insignificant, we interpret these results with a caution.

Overall, the results in this section deliver an interesting insight into the effects of the tight regulation policy that the Central Bank of Russia launched in mid-2013. That is, the policy had a *cleansing* effect on the performance of good firms that faced their bad banks' closures. It is consistent with the view that distressed banks overcharged the good firms with higher interest rate markups, thus locking up the firms. The policy was therefore a source of unlocking the good firms and letting them improve their operations. Bad firms, on contrary, faced larger default risks after their bad banks were closed by the regulator. This is also consistent with the cleansing effect that the policy might have on the structure of the real sector of the Russian economy.

8. STILL OPERATING BAD BANKS: IMPROVE OR FURTHER DETERIORATE?

In the final section, we explore the *ex-ante* effects of the tight regulation policy initiated by the Central Bank of Russia in mid-2013 on bank behavior, namely, whether *still operating* bad banks adapt their operations in advance.[40] Since the policy was stretched in time and lasted for at least five years, at each month of the policy the still operating bad banks was observing policy-induced closures of their rivals for revealed fraud. The banks thus had at least some time to adjust their balance sheets in a threat of the regulatory *unscheduled* on-site inspection.[41] The CBR does not disclose the information on unscheduled inspections, i.e., *which* banks are to be on-site inspected *beyond* the plan and *when* they are inspected.[42]

We attempt to unveil the curtain and make inference regarding the ex-ante effects of the policy using an empirical approach which mechanically resembles difference-in-differences (DID). In our approach, we have *fuzzy* "treated" banks and the time line divided by the introduction of the policy in 2013M7 into *before* and *during* the policy. Although all banks are treated by the policy, we argue that not all banks are treated equally.[43] We argue that the treatment is likely to be dependent on *the strength of signal* each bank sends to the regulator. If the signal (on fraud) is strong enough, the bank may be on-site inspected off the planned schedule, and vice versa. This enables comparisons of banks before and after the policy began, and across the banks that are more or less likely to be inspected at each point in time. All we are interested in is whether it is harder for WNF banks to *pursue the same* business model in the new state (tight policy, after 2013M7) compared to the previous state (regulatory forbearance, before 2013M7) in the banking system. The regression

[40] Recall from Section 2 that in 2014, i.e., one year after the policy was launched, the Head of the CBR announced that the policy should be *pro-active* and force bad banks to improve before CBR's arrival. In addition, CBR conducted preventive negotiations with the managers / owners of bad banks aimed at forcing them to create more reserves on losses and raise capitalization to prevent failures.

[41] According to the federal law 86-FZ "On the Central Bank of the Russian Federation (the Bank of Russia)" (from 7 July 2002), the Bank of Russia conducts both regular and unscheduled on-site inspections of each bank operating in the system. Regular inspections take place each 24 months, whereas unscheduled inspections appear when the Bank of Russia suspects a bank in engaging into illegal activities, capital wastage, misreporting. If the suspicions are justified, then the Bank of Russia imposes standard restrictions on the bank's new operations from both asset and liabilities sides and requires creating additional loan loss reserves / raising more capital to satisfy the regulatory capital adequacy ratio and other obligatory requirements.

[42] Absence of such information disclosure is due to obvious reasons, e.g., preventing panics within the banks' creditors, potential financial contagion, and possible mistakes due to unfair competition of the bank's rivals.

[43] Somewhat similar idea is applied in, e.g., **?** when sorting banks into "treatment" and "control" groups based on the median exposure of the banks to the Lehman collapse on the eve of the Great Recession.

specification reads as follows:

$$Y_{b,t} = \alpha_b + \gamma_t + \xi_1\Big(\text{MIDDLE}_{b,t-3} \times POLICY_t\Big) + \xi_2\Big(\text{EXTREME}_{b,t-3} \times POLICY_t\Big) \\ + \rho_1\text{MIDDLE}_{b,t-3} + \rho_2\text{EXTREME}_{b,t-3} + \tau POLICY_t \\ + \mathbf{X}'_{b,t}\mathbf{\Psi} + \varepsilon_{b,t} \text{ if } t \in [t_0 - k, t_0 + k] \tag{8}$$

where $Y_{b,t}$ is dependent variable reflecting bank b operations or risk exposures at t: lending to firms and to households, NPLs, loan loss reserves, capital, personnel expenses, and exposure to panics from private depositors. α_b is bank fixed effect, γ_t is month fixed effect. $\text{MIDDLE}_{b,t-3}$ and $\text{EXTREME}_{b,t-3}$ are indicator variables of *moderate* and *extreme* fraud: they equal 1 if the predicted probability that bank b engaged in fraud in $t-3$ is greater than the 50%- or 90%-tiles of banks' distribution in respective month. The estimates are obtained with our logit model of bank fraud (Appendix F and Fig. 11).[44] $POLICY_t$ is an indicator variable that equals 1 during the active phase of the policy (2013M7–2018M2), and 0 before the policy. $\mathbf{X}_{b,t}$ is a set of bank-level control variables which depends on the choice of $Y_{b,t}$ but basically reflects bank size, growth of total assets, and the structure of liabilities and other (not loans-related) assets. $\varepsilon_{b,t+h}$ is the regression error. Parameter k governs the length of the estimation window and runs from 1st (2013M7) to 56th (2018M2) months of the active policy. By expanding k and plotting the time evolution of estimated ξ_1 and ξ_2 we capture the *still operating* banks' adaptation to the policy.

With equation (8) we test the following three hypotheses:

1. the policy is pro-active: $\xi_j \neq 0$ for $j = 1, 2$;
2. intensity of fraud matters (heterogeneity): $\xi_1 \neq \xi_2$;
3. as time passes during the active phase of the policy, the banking system cleans up so that the differences between remaining "likely-treated" and "likely-control" banks vanish, thus rationalizing the announcement on the end of the policy on February 2018: $\xi_k \to 0$ as $k \to 56$.

Our estimation results are as follows.

[44] One potential objection to our sorting rules is that they may lead to *too frequent switches* of banks from "treatment" to "control" groups. However, we observe in the data that banks remain within one of the two groups for a sufficient period of time. For example, 97% of "treated" banks under the middle rule remain in the group for *at least three consecutive months*. This is enough for our purposes because, as we discus above, it takes at most two months (by the law) for the regulator to process the signals on suspicious banks.

Bank lending, Fig. 12. For $Y_{b,t}$ equals log of loans we obtain that moderately WNF banks were not reducing their lending to firms and households during the active phase of the policy, whereas extremely WNF banks did squeeze both types of loans by about 7 to 12 percentage points during the first two years of the policy (the estimates are significant at 1% level, see Fig. 12.a,c).[45] In the second half of the policy the effects attenuate. These results support all three hypotheses: the policy is pro-active, possesses heterogeneous ex-ante effects on WNF banks' lending behavior, and effectively cleans the differences among remaining banks as time passes.

Further, for $Y_{b,t}$ equals loans to total assets ratio (LTA) our estimates suggest that moderately WNF banks were raising their total assets while keeping loans constant, but this occurred only during the first year of the policy (LTA declined significantly at $k \in [0, 10]$ months, 12.b,d). Conversely, extremely WNF banks sustained constant LTAs (insignificant estimates, see the same subfigures).

Non-performing loans (NPLs), Fig. 13.a,b. For moderately WNF banks, we obtain insignificant estimates for the firms' NPLs during the whole span of the policy and negative and significant (at 5% level) estimates for the households' NPLs during the second half of the policy. For extremely WNF banks our results are different: the estimates for the firms' NPLs are positive and significant (at 5 or 10% levels), peaking at +1 percentage points of their total assets, and those for households' NPLs are also positive and highly significant, peaking at +1.5 percentage points of their total assets. Remarkably, both effects on the extremely WNF banks materialize during the first half of the policy. These results imply that moderately WNF banks were much less responsive to the ex-ante effects of the policy, whereas extremely WNF banks acted accordingly to the CBR's policy objectives: they started to (partly) disclose in their balance sheets the bad loans accumulated in the past.

Bank personnel expenses, Fig. 13.c. One could fairly anticipate that during the active phase of the policy gambling banks have to reduce wage bills they pay to their workers and dividends they pay to their owners, to raise cost efficiency under the pressure of rising disclosures of NPLs. However, our estimates deliver different results. For both moderately and extremely WNF banks, we obtain close to zero and insignificant estimates of the ex-ante effects of the CBR's policy. Probably, this just reflects that the banks are price-takers on the labor market and have to offer competitive wages to hold on their workers.

[45]This result echoes the one obtained by Kupiec et al. (2017) who show that poor examination rating significantly reduces subsequent dynamics of bank lending,controlling for the demand and other characteristics. In our case, we capture the supply-side effects by construction.

Exposure to panics from private depositors, Fig. 13.d. One concern on the policy is that it could trigger a panic runs on the banks. This could be so if the news on an scheduled on-site inspection of suspicious banks becomes available to the bank's creditors. However, our results do not support this concern. Although we obtain negative estimates for both moderately and extremely WNF banks consistent with the panics view, the estimates are insignificant during the whole span of the policy. This indicates that the policy design was effective in the sense that informational leakages regarding the policy actions were unlikely to take place.

Loan loss reserves (LLR). Fig. 14.a. For both moderately and extremely WNF banks we obtain positive and significant estimates (at 5 to 10% levels) of the ex-ante effects of the policy, thought the timing is different. Again, we reveal that extremely WNF banks response to the policy during the first half of the policy span while moderately WNF banks do so during the second half of the policy. For the first group of "targeted" banks the effect peaks at +0.7 percentage points of their total assets, and for the second group at +0.5 percentage points of their total assets. These results support the idea that the policy forces WNF banks to create additional loan loss reserves. From one side, this is likely to raise the banks' insurance against credit risks. From the other, it may also lead to additional pressure on the banks' capital adequacy ratios.

Bank equity capital. Fig. 14.d. If the problem banks have to disclose more NPLs and create additional reserves on loan losses in response to the pro-active policy, one's concern could be that the banks will face declining capital adequacy ratios (CAR). However, our results support a different view. For both moderately and extremely WNF banks we obtain insignificant estimates of the ex-ante effects of the CBR's policy. This means that the banks' risk-bearing assets and capital were changing in the same direction (decreasing) but with same speed. Indirectly, this may indicate that, though additional reserves were reducing capital, the banks' performing assets were enough to generate profits to at least partially offset the negative effects of creating additional reserves. Therefore, we argue that the policy did *not* lead to declining capital ratios of targeted banks.[46]

[46] When analyzing the capital exercise launched by the ECB in 2011, Gropp et al. (2018) come to the opposite conclusion. After stress-testing of potentially weak banks, the shareholders decided to reduce lending rather that raise capital to sustain CARs.

9. CONCLUSION

Our study shows that, following bad bank closures, bad firms are more likely to match with new bad banks, especially if the banks are run by common owners, whereas good firms match with good banks. The tight policy of the Central Bank of Russia had cleansing effects on the performance of firms during the transition period, i.e., after their current bad banks were closed and before they matched with new banks. Our analysis also supports the view that the tight policy possessed pro-active features: it forced the likely-gambling banks to adapt their balance sheets in advance, i.e., before the regulator arrives with unscheduled on-site inspections.

What happens with the firms after they match with new banks in terms of loan conditions and firm-level characteristics, we leave to future work.

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FIGURES

Figure 1. Bank Closures and the New Head of CBR

This figure depicts the time series of monthly bank closures (the left y-axis) and monthly number of operating banks (the right y-axis) during the period between February 2008 and June 2019. The new head of the Central Bank of Russia (CBR) was appointed in Jun 2013.

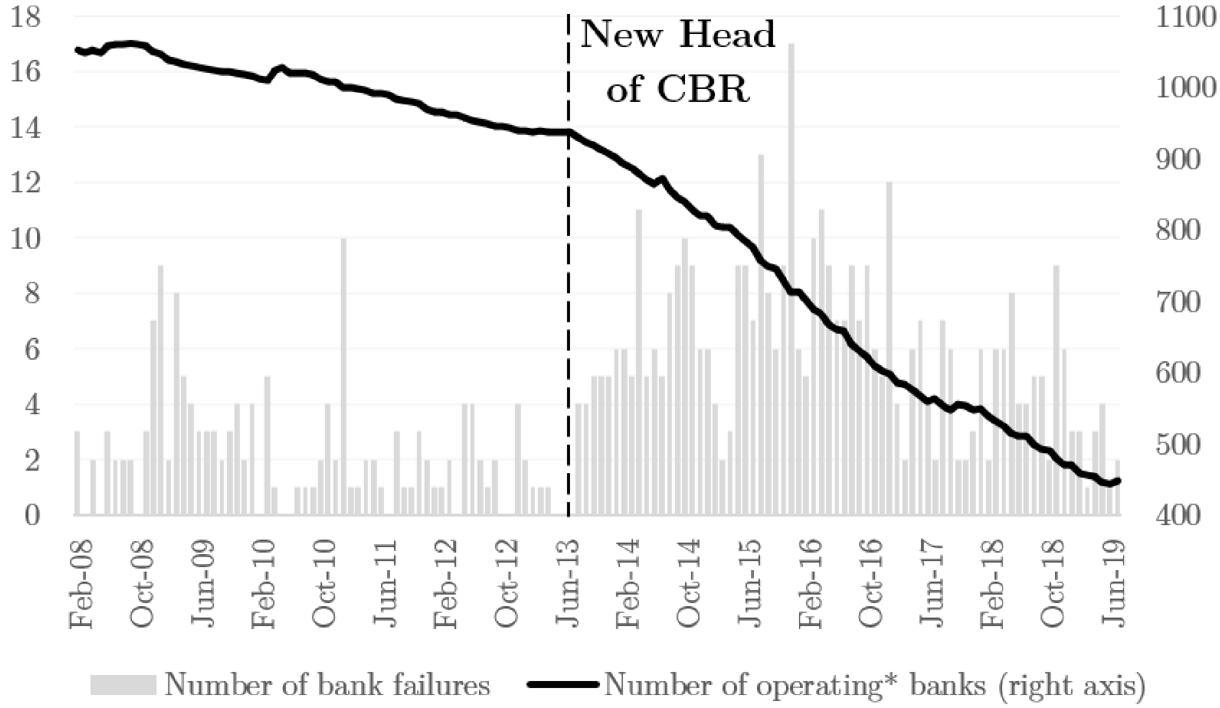


Figure 2. Geography of bank fraud and bank closures

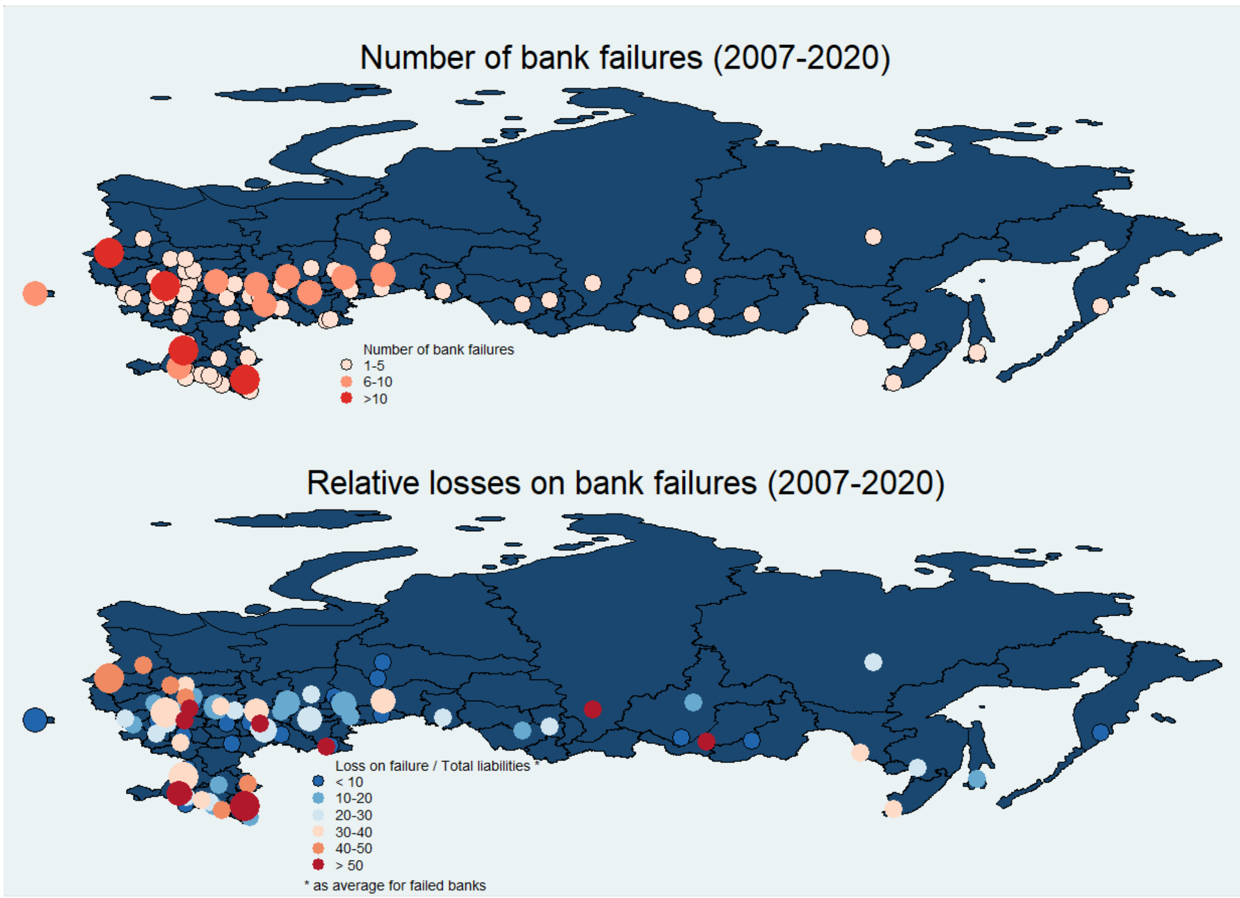
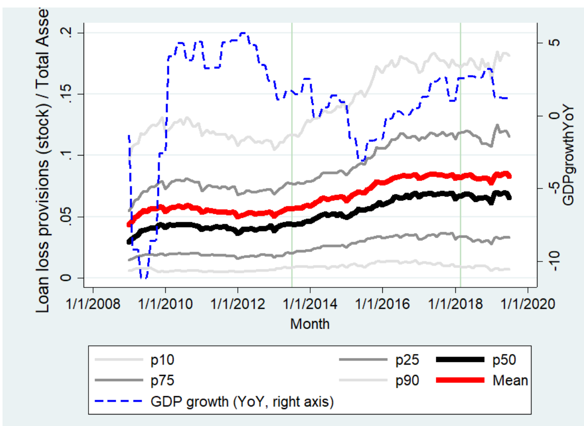
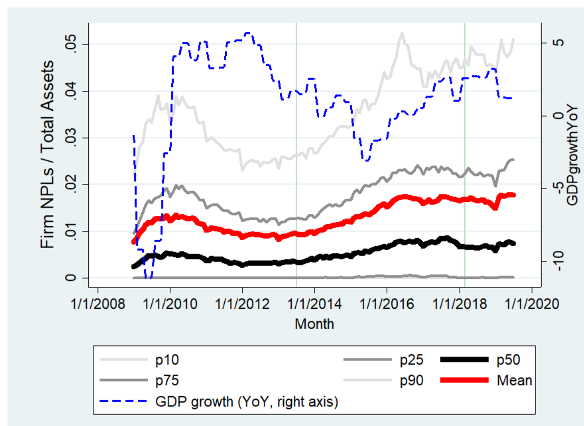


Figure 3. Time evolution of selected bank variables before and during the active phase of the tight regulation policy (Jul.2013–Feb.2018)

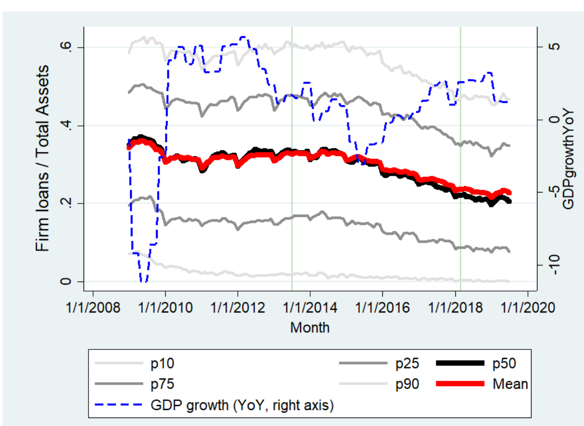
Note: The figure depicts time evolution of selected bank characteristics at the bank-month level before, during, and after the active phase of the tight regulatory policy against the background of the annual GDP growth rates in Russia. The active phase of the policy is marked with two vertical green lines.



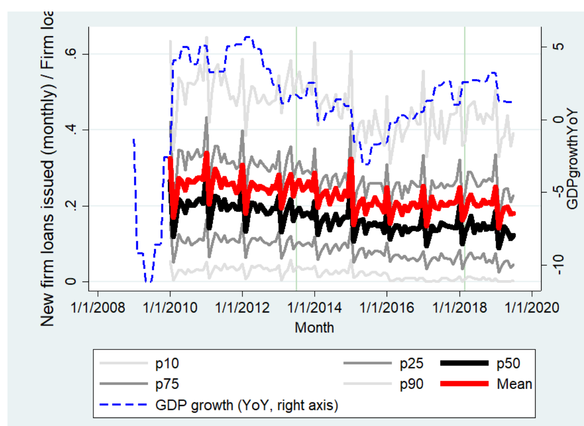
(a) Loan loss reserves



(b) NPLs of firms

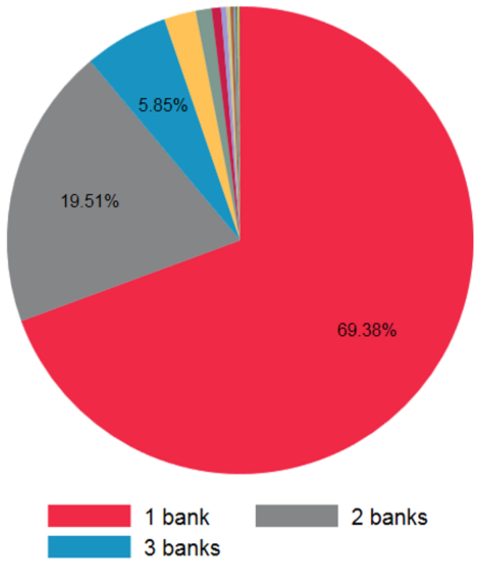


(c) Loans to firms

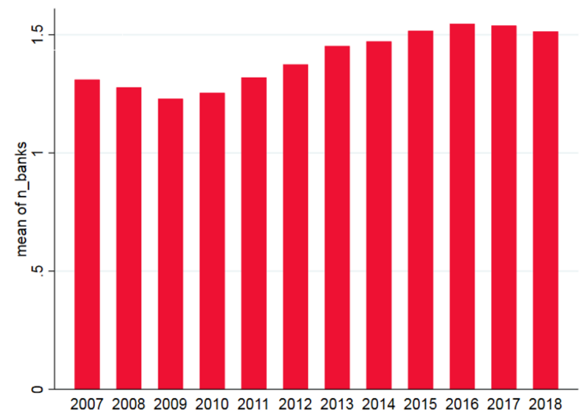


(d) New loans issued

Figure 4. Bank–firm relationships

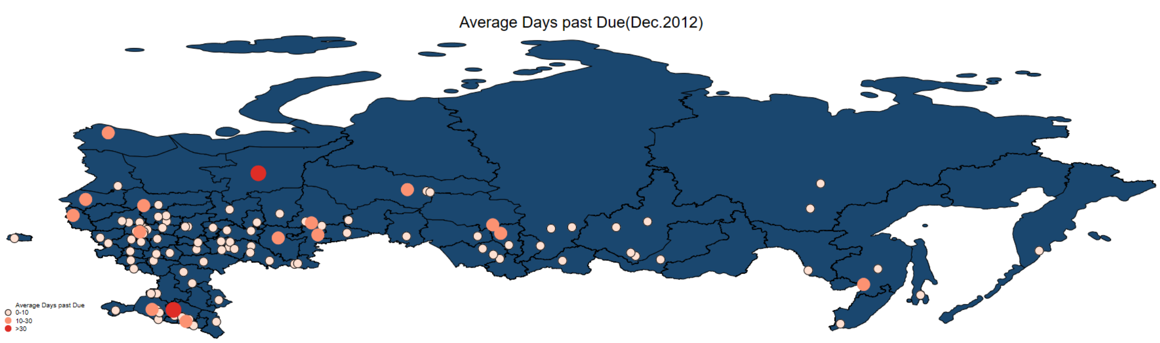


(a) Distribution of bank-firmrelationships (as of 2017)

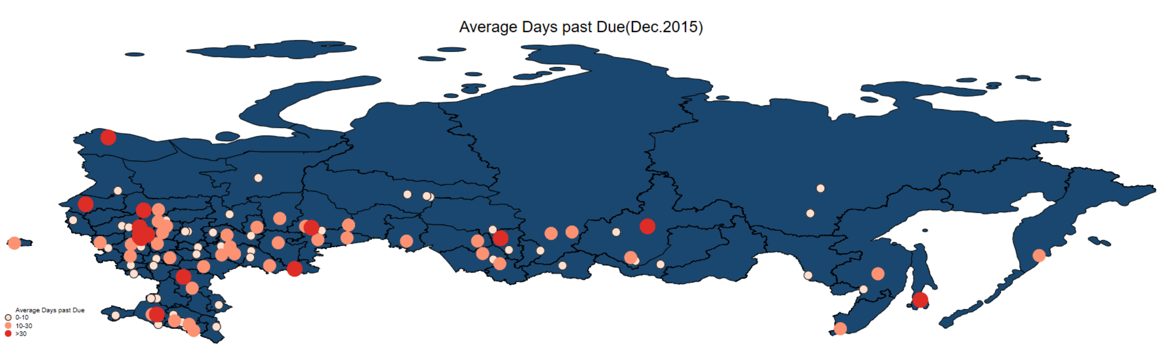


(b) Time evolution of the mean number of bank-firm relationships, by year

Figure 5. Geographical variation in the number of "bank–firm" matches

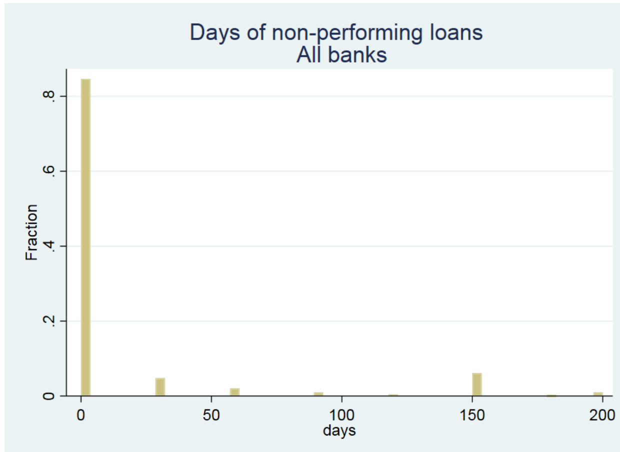


(a) Before the regulatory shock (as of December 2012)

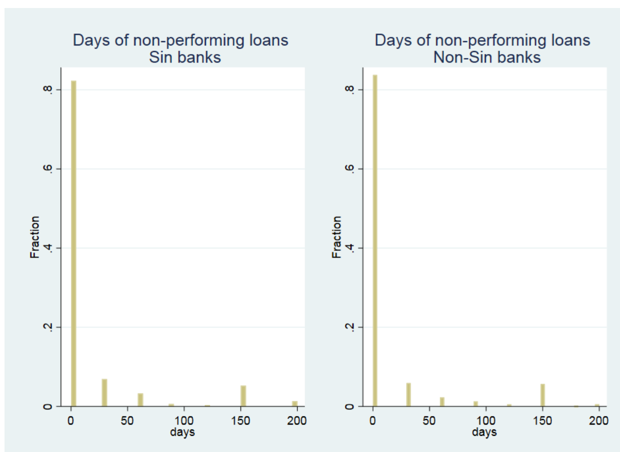


(b) After the regulatory shock (as of December 2015)

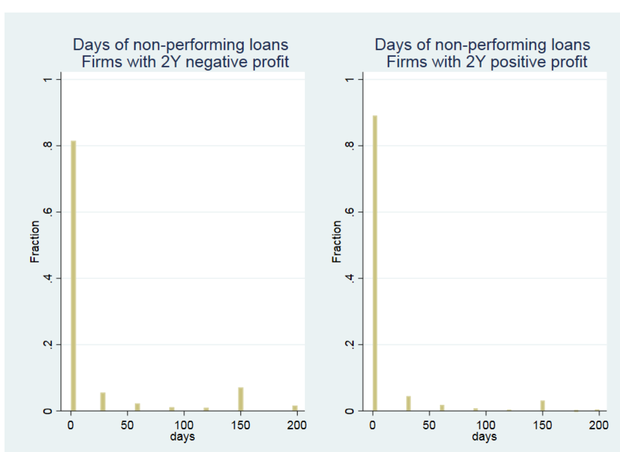
Figure 6. Frequency of the days of NPLs reported in the Bureau of Credit Histories (BCH), by "bank–firm" matches



(a) Full sample



(b) Bad versus good banks



(c) Profitable versus non-profitable firms

Figure 7. 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.

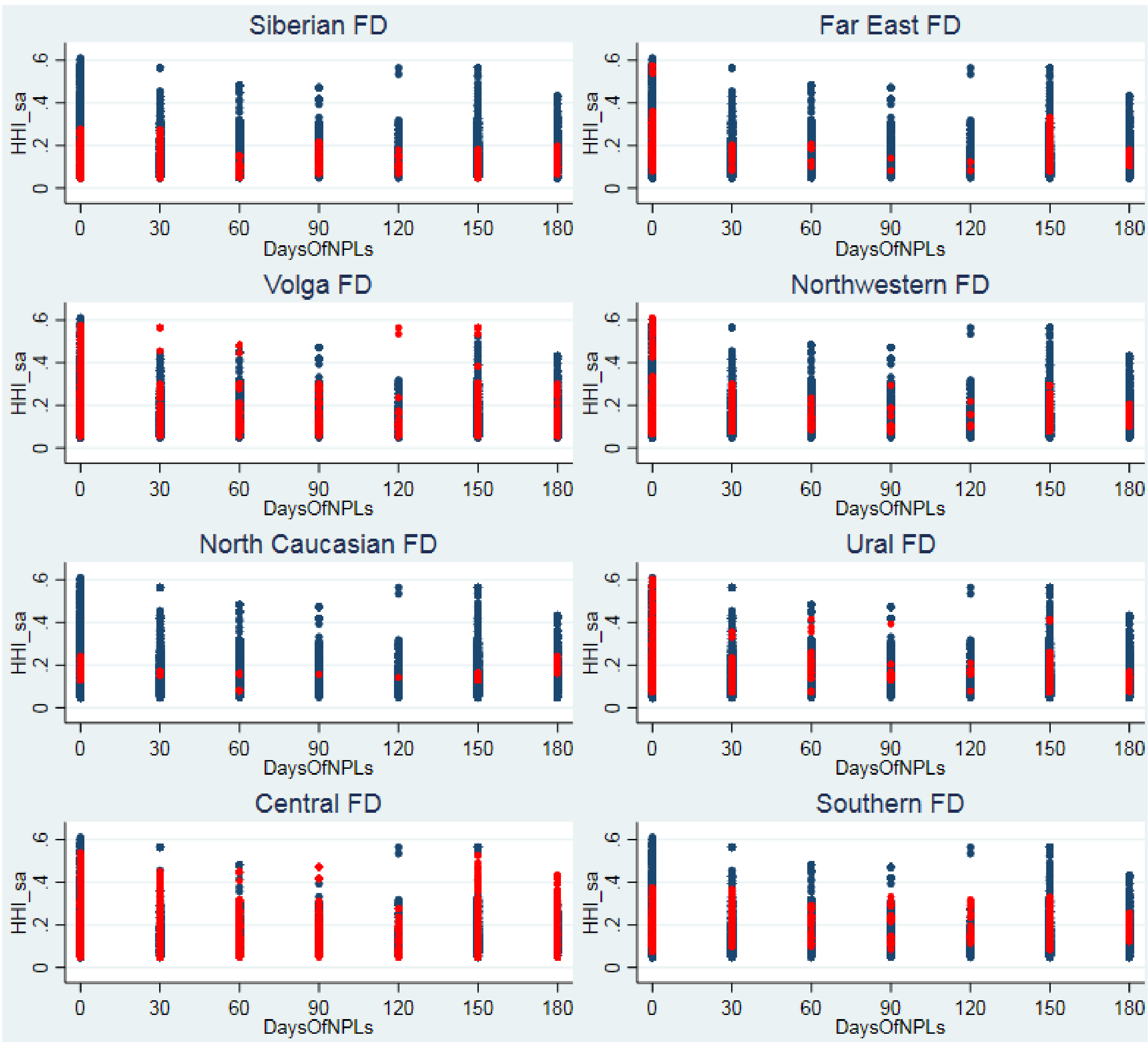


Figure 8. Time it takes for establishing new firm-bank matches after closure of weak, non-transparent and fraudulent banks

Note: The figure depicts empirical densities of time it takes for firms to match with new banks after their current banks are detected for fraud and closed by the regulator (in the tight regulation policy regime, i.e., from 2013M7).

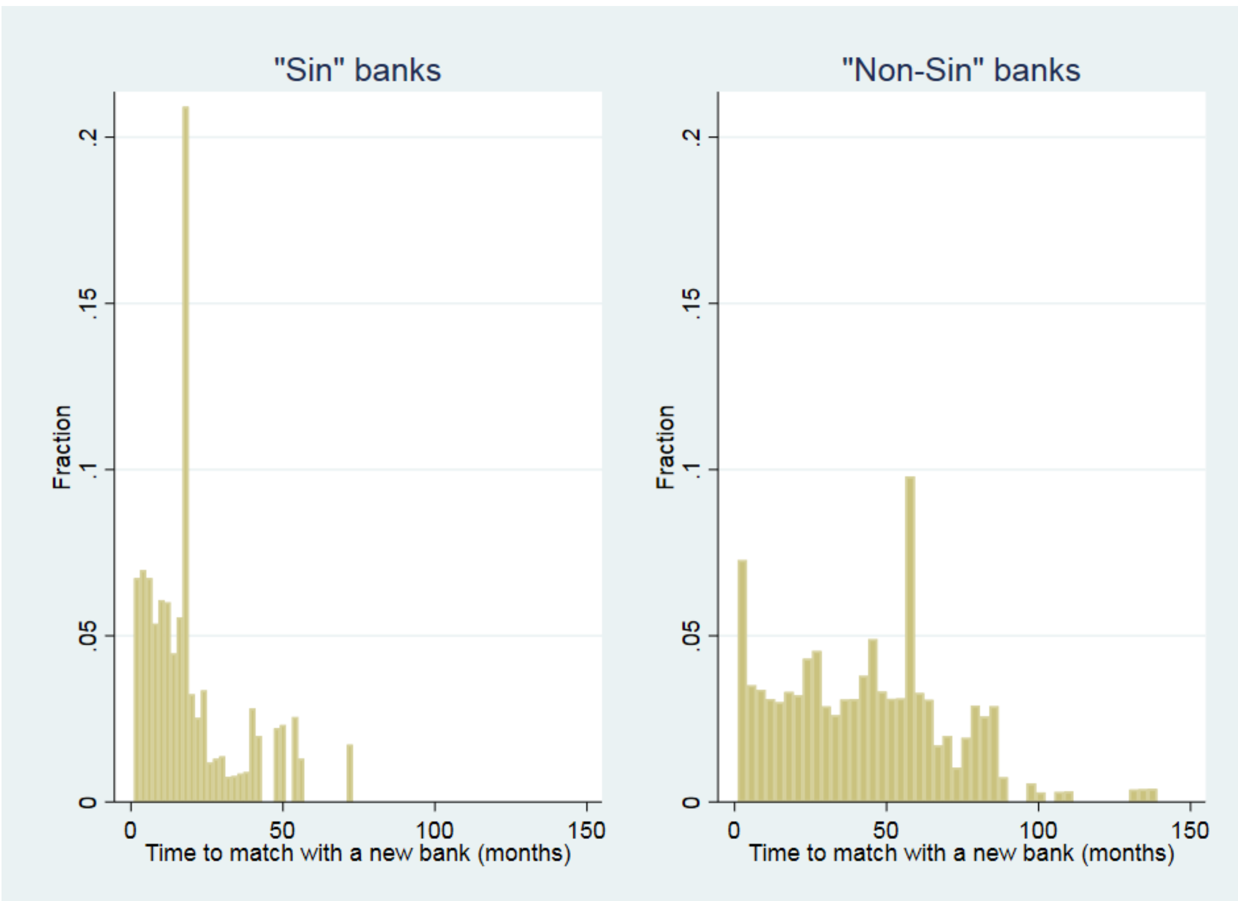


Figure 9. Time it takes for establishing new firm-bank matches after closure of weak, non-transparent and fraudulent banks and the quality of loans in the closed banks

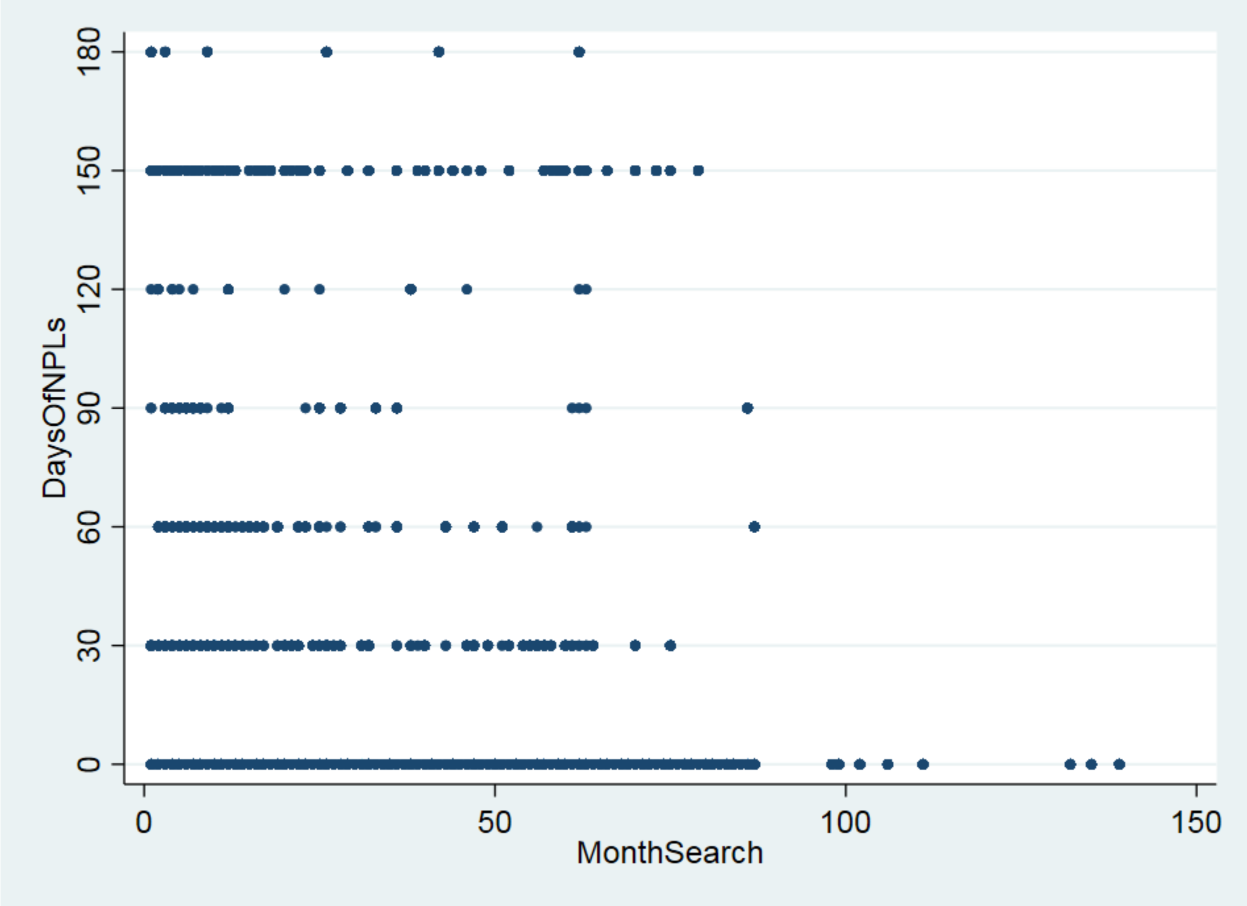


Figure 10. Loan quality at the firm-bank-month level: 1 (best) to 5 (worst) categories

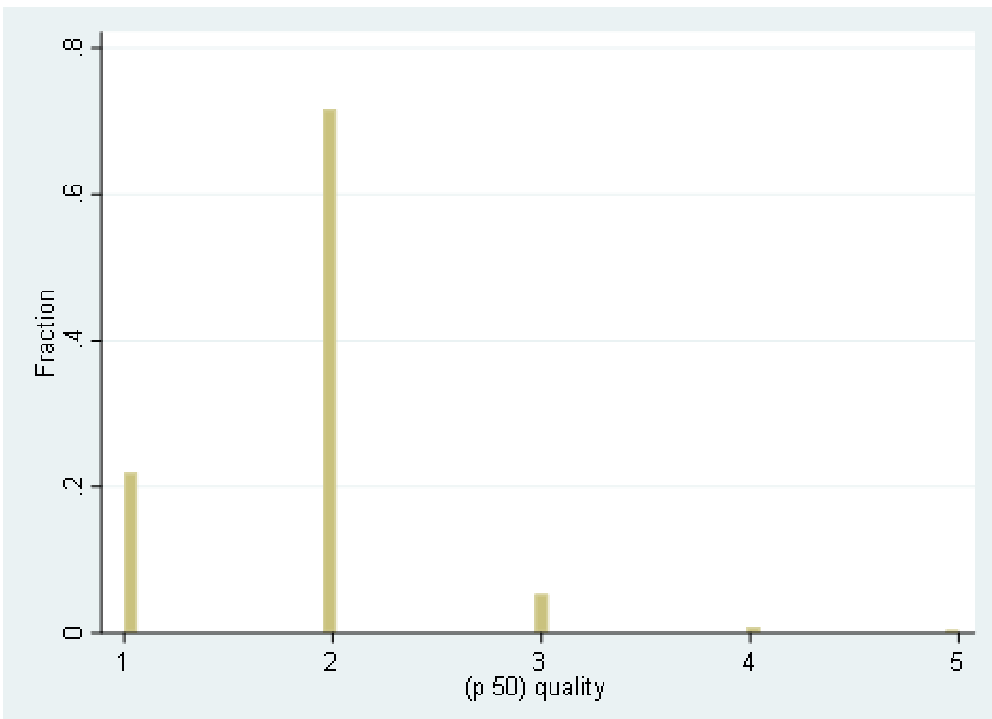


Figure 11. Time evolution of selected bank variables before and during the active phase of the tight regulation policy (Jul.2013–Feb.2018)

Note: The figure depicts 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 the annual GDP growth rates in Russia. The active phase of the policy is marked with two vertical green lines.

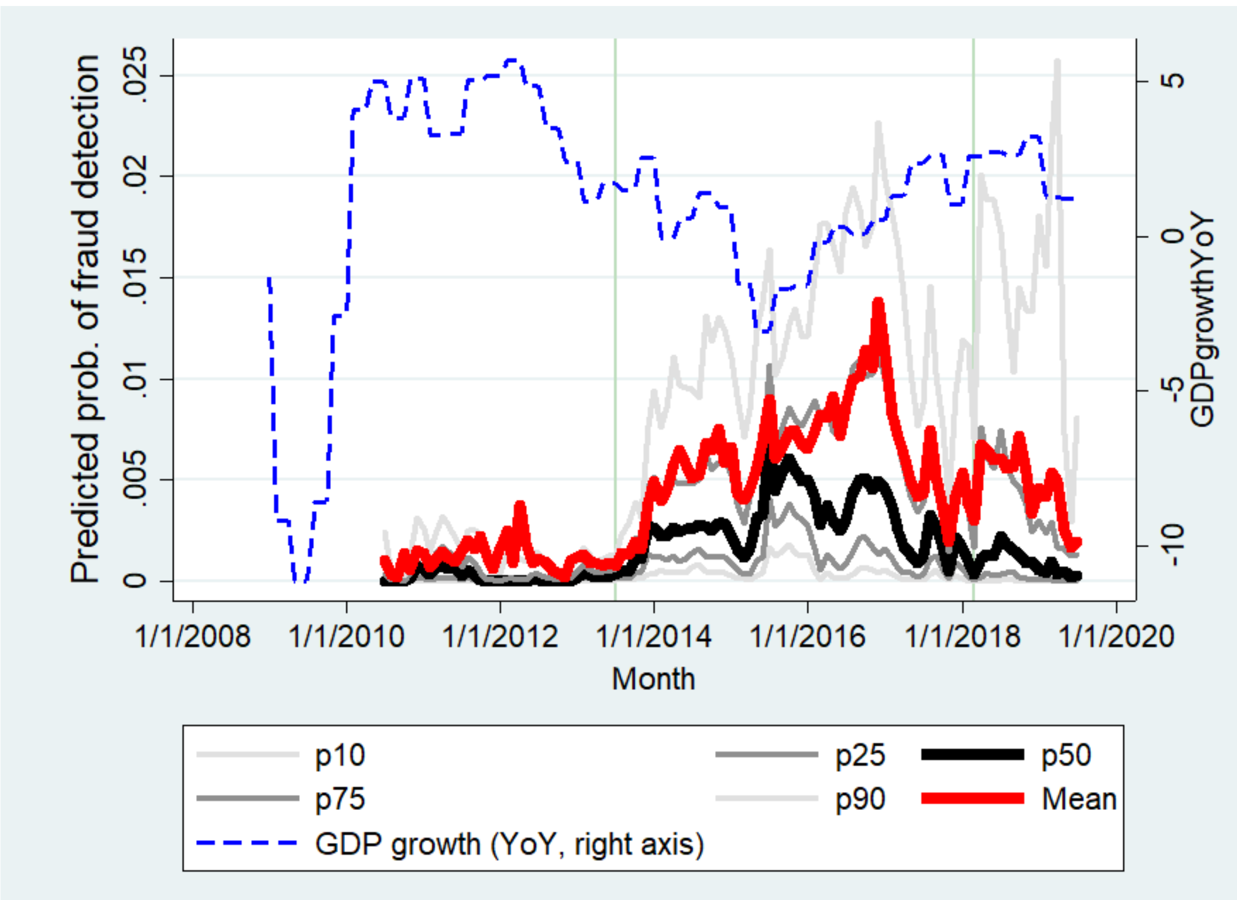
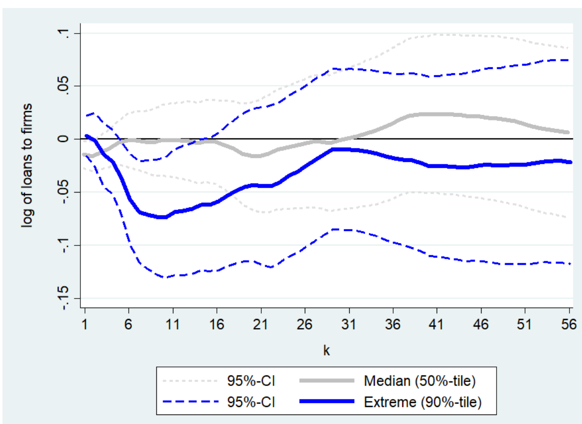
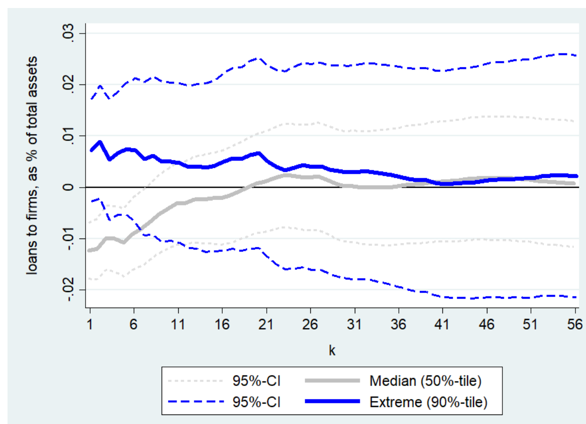


Figure 12. ***Ex-ante*** **effects of the CBR's tight regulation policy on bank lending**

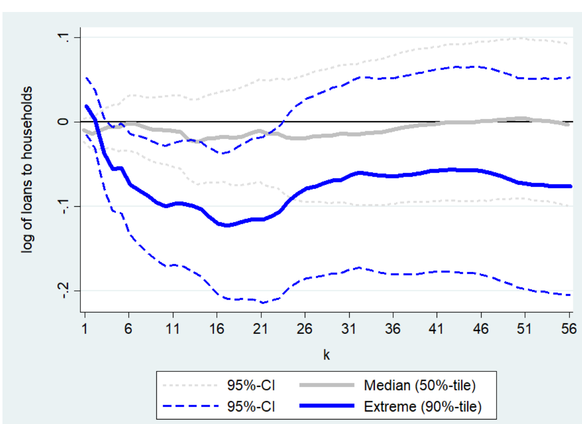
Note: The figure depicts time evolution of the estimated *ex-ante* effects of the CBR's tight regulation policy launched in mid-2013 on bank lending. Time evolution is obtained by running a loop of regressions on expanding windows $[2013M7-k, 2013M7+k]$, where $k = 1, 2...56$ governs the window length, with $k = 1$ reflecting the first and $k = 56$ the last month of the active phase of the policy. The estimates capture differences between *not yet detected* bad banks and good banks during the active phase of the policy, as well as adaptation of the bad banks to the policy as time passes. For concreteness, and by assumption, a bank i is defined as *bad* at month $t-1$ if the predicted probability of fraud detection in it is *greater* than a given threshold across all operating banks at $t-1$. We consider two thresholds: *median* (grey lines) to reflect moderate gambling and *90%*-tile (blue lines) to capture extreme gambling. Being bad in this definition implies common knowledge because the prediction is made on the publicly available balance sheet data at $t-1$. Thus, we assume that being bad also raises the probability of the bank's on-site inspection by the regulator from t onward. If the policy is pro-active, as the Head of the Central Bank of Russia claimed, the bank would adapt its characteristics in an anticipation of the regulator's arrival.



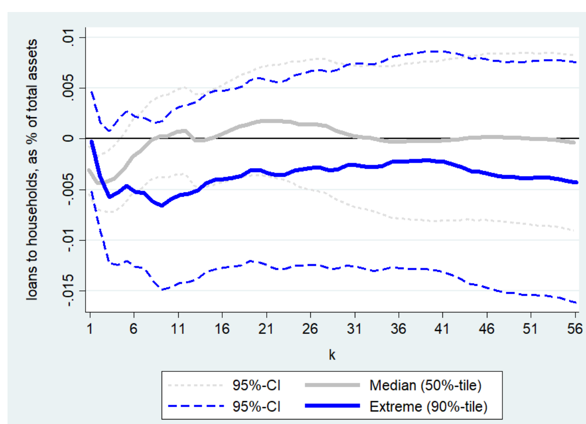
(a) Log of firms' loans



(b) Loans of firms to total assets



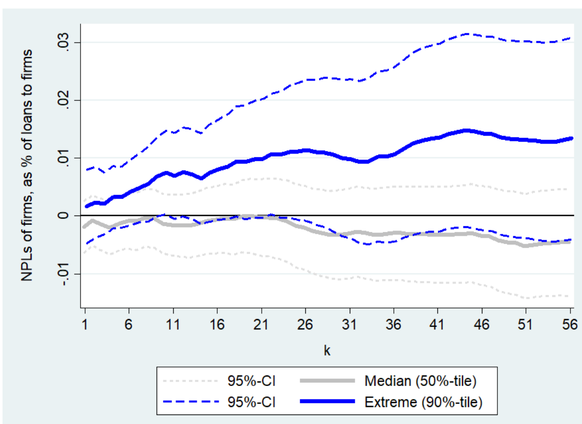
(c) Log of households' loans



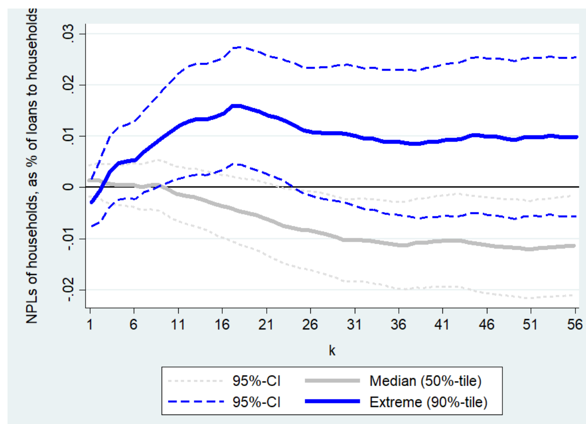
(d) Loans of households to total assets

Figure 13. ***Ex-ante* effects of the CBR's tight regulation policy on bank disclosure of NPLs, personnel expenses and exposures to private deposits withdrawals**

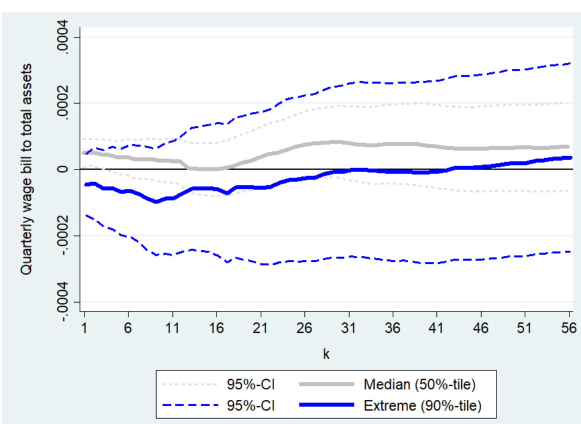
Note: The figure depicts time evolution of the estimated *ex-ante* effects of the CBR's tight regulation policy launched in mid-2013 on banks' disclosure of NPLs, the banks' personnel expenses and exposures to private deposits withdrawals. Time evolution is obtained by running a loop of regressions on expanding windows $[2013M7-k, 2013M7+k]$, where $k = 1, 2...56$ governs the window length, with $k = 1$ reflecting the first and $k = 56$ the last month of the active phase of the policy. The estimates capture differences between *not yet detected* bad banks and good banks during the active phase of the policy, as well as adaptation of the bad banks to the policy as time passes. For concreteness, and by assumption, a bank i is defined as *bad* at month $t-1$ if the predicted probability of fraud detection in it is *greater* than a given threshold across all operating banks at $t-1$. We consider two thresholds: *median* (grey lines) to reflect moderate gambling and *90%*-tile (blue lines) to capture extreme gambling. Being bad in this definition implies common knowledge because the prediction is made on the publicly available balance sheet data at $t-1$. Thus, we assume that being bad also raises the probability of the bank's on-site inspection by the regulator from t onward. If the policy is pro-active, as the Head of the Central Bank of Russia claimed, the bank would adapt its characteristics in an anticipation of the regulator's arrival.



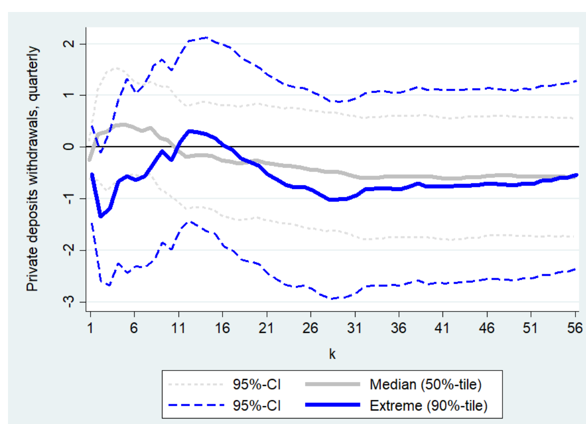
(a) Firms' NPLs, as % of total assets



(b) Households' NPLs, as % of total assets



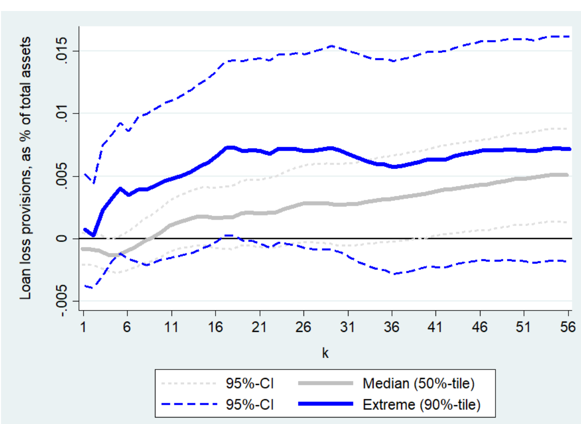
(c) Personnel expenses, as % of total assets



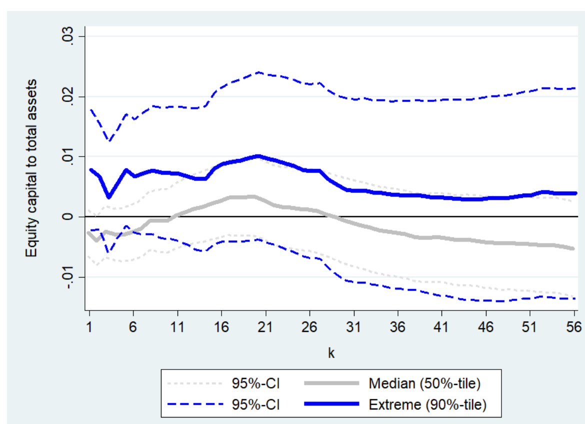
(d) Private deposits withdrawals, quarterly, as % of private deposits

Figure 14. ***Ex-ante*** **effects of the CBR's tight regulation policy on bank loan loss reserves and equity capital**

Note: The figure depicts time evolution of the estimated *ex-ante* effects of the CBR's tight regulation policy launched in mid-2013 on banks' disclosure of loan loss reserves (LLR) and the banks' equity capital to total assets ratios. Time evolution is obtained by running a loop of regressions on expanding windows $[2013M7-k, 2013M7+k]$, where $k = 1, 2...56$ governs the window length, with $k = 1$ reflecting the first and $k = 56$ the last month of the active phase of the policy. The estimates capture differences between *not yet detected* bad banks and good banks during the active phase of the policy, as well as adaptation of the bad banks to the policy as time passes. For concreteness, and by assumption, a bank i is defined as *bad* at month $t-1$ if the predicted probability of fraud detection in it is *greater* than a given threshold across all operating banks at $t-1$. We consider two thresholds: *median* (grey lines) to reflect moderate gambling and *90%*-tile (blue lines) to capture extreme gambling. Being bad in this definition implies common knowledge because the prediction is made on the publicly available balance sheet data at $t-1$. Thus, we assume that being bad also raises the probability of the bank's on-site inspection by the regulator from t onward. If the policy is pro-active, as the Head of the Central Bank of Russia claimed, the bank would adapt its characteristics in an anticipation of the regulator's arrival.



(a) Loan loss reserves to total assets



(b) Equity capital to total assets

TABLES

Table 1. Descriptive statistics: firms matching with new banks after their current (bad) banks fail

	Mean	Median	SD	Min	Max
Panel 1: Firms matching with new *good* banks:					
Match with good 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 failed bad bank	14.87	0.00	42.07	0.00	200.00
Whether had negative profit when the bad bank failed	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 *bad* banks:					
Match with bad 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 failed bad bank	15.73	0.00	41.75	0.00	200.00
Whether had negative profit when the bad bank failed	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 failed bad bank	14.19	0.00	39.52	0.00	200.00
Whether had negative profit when the bad bank failed	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

Table 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 bad 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

Table 3. Survival regression results: firm-bank match based on the firm quality

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:						
$\ln DNPL_{f,t^*}$	–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

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new bank match (establishes new relationship with a bank), and zero if the firm never finds the match. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Table 4. Survival regression results: splitting the firm-bank matches

	Match with a bad bank			Match with a good bank		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Firm quality:						
log $DNPL_{f,t^*}$	0.155***			–0.091**		
	(0.058)			(0.037)		
$Profit_{f,t^*} < 0$		–1.742*	–1.475*		0.041	0.204
		(0.908)	(0.895)		(0.247)	(0.248)
$Profit_{f,t^*+k} < 0$			–0.534*			–0.384**
			(0.297)			(0.151)
Panel 2: Other controls:						
Firm 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 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)
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)
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

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new match with a *bad* bank (columns 1–3) or with a *good* bank (columns 4–6), and zero if the firm never finds the match. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Table 5. Channels of endogenous firm-bank matching: single bank group owners

	Match with a bad bank w/out single owners			Match with a good bank w/out single 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)
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	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

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new match with a *bad* bank (columns 1–3) or with a *good* bank (columns 4–6), and zero if the firm never finds the match. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Table 6. Channels of endogenous firm-bank matching: surprising bank closures

Previous bad bank closure:	*Surprise*		*Not surprise*	
Match with a new bank:	bad bank	good bank	bad bank	good 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)
Other firm controls	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
N obs	224,821	225,274	32,369	32,407
N firm-to-bank switches	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				
$Profit_{f,t^*} < 0$	–2.039*	0.124	0.385	0.443
	(1.202)	(0.279)	(1.822)	(0.701)
$Profit_{f,t^*+k} < 0$	–0.711**	–0.364**	0.095	–0.614
	(0.350)	(0.164)	(0.744)	(0.389)
Other firm controls	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
N obs	154,007	154,382	24,365	24,376
N firm-to-bank switches	143	459	25	78
N firms	3,545	3,551	650	649
log L	–719.0	–1,827.8	–112.5	–319.8

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new match with a *bad* bank (columns 1, 3) or with a *good* bank (columns 2, 4), and zero if the firm never finds the match. "*Surprise*" indicates that the estimations are performed on the subsample of only those banks for which predicted probability of fraud detection is *below* the unconditional threshold of 0.5% monthly (or 6% annually). "*Not surprise*", on contrary, means *above* the threshold. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Table 7. Channels of endogenous firm-bank matching: regional credit market concentration

	Match with a bad bank	Match with a good bank
	(1)	(2)
Panel 1: Firm quality: Days of NPLs		
log $DNPL_{f,t^*} \times HHI.credit_{r,t-1}$	0.501	1.430***
	(1.017)	(0.425)
log $DNPL_{f,t^*}$	0.223***	–0.107**
	(0.068)	(0.043)
$HHI.credit_{r,t-1}$	1.406	5.625***
	(1.405)	(0.649)
N obs	222,837	223,290
N firms	5,159	5,169
N firm-to-bank switches	168	611
log L	–891.0	–2,434.6
Panel 2: Firm quality: Negative profits		
$Profit_{f,t^*} < 0 \times HHI.credit_{r,t-1}$	–6.860	4.364*
	(8.042)	(2.487)
$Profit_{f,t^*+k} < 0 \times HHI.credit_{r,t-1}$	2.946	2.399**
	(3.977)	(1.138)
$Profit_{f,t^*} < 0$	–2.221*	–0.036
	(1.143)	(0.311)
$Profit_{f,t^*+k} < 0$	–0.681*	–0.405**
	(0.349)	(0.171)
$HHI.credit_{r,t-1}$	–0.721	3.110***
	(2.036)	(0.811)
N obs	152,735	153,110
N firms	3,526	3,532
N firm-to-bank switches	143	459
log L	–718.7	–1,808.0

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new match with a *bad* bank (columns 1, 3) or with a *good* bank (columns 2, 4), and zero if the firm never finds the match. "*Surprise*" indicates that the estimations are performed on the subsample of only those banks for which predicted probability of fraud detection is *below* the unconditional threshold of 0.5% monthly (or 6% annually). "*Not surprise*", on contrary, means *above* the threshold. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Table 8. Logit regression results: do firms break switch to bad or good banks in anticipation of their current banks closure?

	Switch to a bad bank		Switch to a good 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

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new match with a *bad* bank (columns 1–3) or with a *good* bank (columns 4–6), and zero if the firm never finds the match. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Table 9. Panel estimation results: do bad firms increase delays in repaying loans before their banks are closed?

Months h before bad bank closure:	$h = 12$	$h = 9$	$h = 6$	$h = 3$
	(1)	(2)	(3)	(4)
Panel 1: single "firm–bad bank" relationship (*baseline*)				
$Profit_{f,t^*-h} < 0$	1.063	0.761	1.197	–0.711
	(0.720)	(0.949)	(1.174)	(1.565)
Bank FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Firm × FEs	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes
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–(bad) bank" relationship				
$Profit_{f,t^*-h} < 0$	0.219	0.414	0.937	0.379
	(0.450)	(0.786)	(0.591)	(0.500)
$Bad.Bank_b \times Profit_{f,t^*-h} < 0$	0.111	–0.644	–1.165	–1.589
	(0.791)	(1.200)	(1.367)	(1.955)
Bank FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Firm × FEs	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes
N obs	213,229	163,688	111,957	60,140
R^2 (within)	0.081	0.100	0.135	0.232

Note: 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 bad bank closure, i.e., $t \in [t^* - h, t^*)$, where t^* is firm-specific date of breaking relationship with the firm's current bad bank.

Single "firm–bad bank" indicates those cases in which a firm has relationship only with one bank and this bank is a bad bank.

Multiple "firm–(bad) bank" indicates those cases in which a firm has relationships with more than one bank and (at least) one of these banks is a bad bank.

***, **, * 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 10. Difference-in-differences estimation results: firm performance after bad bank closures

$Y_{f,t} :=$	Default	Leverage	Revenue	Employ	Profit
	(1)	(2)	(3)	(4)	(5)
Panel 1: Focus variables:					
$Bad.Bank_b \times \text{POST}_{\{t \geq t^*\}}$	–2.566*	0.011	0.384***	–4.408*	0.006
	(1.468)	(0.018)	(0.140)	(2.365)	(0.017)
$Bad.Bank_b \times \text{POST}_{\{t \geq t^*\}} \times Bad.Firm_{f,t}$	n/a	0.101*	–0.770**	10.553**	–0.017
		(0.060)	(0.325)	(4.239)	(0.030)
Panel 2: The rest of triple interaction components:					
$Bad.Bank_b$	2.356***	–0.009	–0.210**	1.994**	0.000
	(0.319)	(0.011)	(0.101)	(0.921)	(0.014)
$\text{POST}_{\{t \geq t^*\}}$	0.595	–0.030	–0.184	4.940*	–0.028
	(1.500)	(0.025)	(0.162)	(2.852)	(0.018)
$Bad.Firm_{f,t}$	0.607	0.085***	–0.316***	8.703**	–0.180***
	(0.717)	(0.031)	(0.120)	(3.599)	(0.014)
$Bad.Bank_b \times Bad.Firm_{f,t}$	0.625	–0.077*	0.336	–6.685*	–0.009
	(0.849)	(0.045)	(0.262)	(3.457)	(0.026)
$\text{POST}_{\{t \geq t^*\}} \times Bad.Firm_{f,t}$	–0.586	–0.051	0.131	–10.557**	0.024
	(0.854)	(0.050)	(0.210)	(4.344)	(0.021)
Firm controls	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
N obs	10,745	18,861	17,835	11,683	18,613
N firms	3,237	3,261	3,234	2,869	3,258
R^2 (pseudo / LSDV)	0.1	0.7	0.1	0.0	0.1

Note: Dependent variables $Y_{f,t}$ are as follows: a binary indicator of whether a firm f defaults at year t (*Default*, column 1), the ratio of borrowed funds to total assets (*Leverage*, column 2), revenue to total assets (*Revenue*, column 3), number of workers to total revenue ratio (*Employ*, column 3), profit after taxes to total assets (*Profit*, column 5). $Bad.Bank_b = 1$ if a bank b that has relationship with a firm f ever fails for loss of capital (and its license was revoked with one of the following formulations: "the loss of capital due to excessive credit risk, insufficient reserves and/or involvement in questionable transactions, which also led to the loss of capital") , and 0 if survives till the end of the sample. $POST_{\{t \geq t^*\}} = 1$ if $t \geq t^*$, 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 on a panel of matched firms that ever faced bad bank closures, and the panel is restricted so that it includes the observations in only two years before and after t^*, i.e., firm-time-varying windows $[t^* - 2, t^* + 2]$ years. 1:4 nearest neighborhood matching of firms is performed prior to t^* using the five observables: firm size, leverage, liquidity, annual growth of total assets, and profitability.

***, **, * 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 11. Interest rates and amount of loans in bad banks: regression estimation results

	Interest rate on loan, $Interest.Rate_{f,b,t}$			log of loan amount, log $LNS_{f,b,t}$		
	FEs	FEs + Bank	FEs + Bank + Macro	FEs	FEs + Bank	FEs + Bank + Macro
	(1)	(2)	(3)	(4)	(5)	(6)
$Bad.Bank_b$	3.557***	–0.444	–0.373	–0.231***	0.422***	0.430***
	(0.089)	(0.284)	(0.275)	(0.036)	(0.119)	(0.119)
$Loan.Quality_{f,b,t}$ = 1 (*reference*)						
$Loan.Quality_{f,b,t}$ = 2	0.109***	–0.013	0.021	–0.088***	–0.060***	–0.060***
	(0.012)	(0.014)	(0.013)	(0.006)	(0.007)	(0.007)
$Loan.Quality_{f,b,t}$ = 3	0.765***	0.189***	0.223***	–0.098***	–0.018	–0.017
	(0.028)	(0.027)	(0.026)	(0.011)	(0.015)	(0.015)
$Loan.Quality_{f,b,t}$ = 4	0.154***	–0.185***	–0.114**	–0.290***	–0.447***	–0.444***
	(0.034)	(0.046)	(0.046)	(0.026)	(0.034)	(0.034)
$Loan.Quality_{f,b,t}$ = 5	–0.045	–0.193*	–0.150	–0.089**	–0.327***	–0.326***
	(0.071)	(0.111)	(0.110)	(0.044)	(0.078)	(0.078)
$Bad.Bank_b$ × $Loan.Quality_{f,b,t}$ = 2	–0.640***	–0.125	–0.185	0.124***	–0.142	–0.145
	(0.086)	(0.208)	(0.201)	(0.036)	(0.101)	(0.101)
$Bad.Bank_b$ × $Loan.Quality_{f,b,t}$ = 3	–1.342***	–2.408***	–2.301***	0.154***	–0.487***	–0.491***
	(0.118)	(0.362)	(0.346)	(0.050)	(0.181)	(0.181)
$Bad.Bank_b$ × $Loan.Quality_{f,b,t}$ = 4	–1.050***	–1.210***	–1.304***	0.352***	0.004	0.001
	(0.155)	(0.462)	(0.422)	(0.092)	(0.258)	(0.259)
$Bad.Bank_b$ × $Loan.Quality_{f,b,t}$ = 5	–1.184***	–0.261	–0.106	–0.174	0.816***	0.817***
	(0.290)	(0.235)	(0.229)	(0.225)	(0.128)	(0.128)
log $LNS_{f,b,t}$		–0.080***	–0.078***			
		(0.003)	(0.003)			
$Interest.Rate_{f,b,t}$					–0.051***	–0.051***
					(0.002)	(0.002)
$Maturity_{f,b,t}$		–0.000***	–0.000***		0.000***	0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
GDP growth (YoY)			–0.226***			0.009***
			(0.002)			(0.002)
HHI (regional level)			–0.001***			–0.000***
			(0.000)			(0.000)
Obs	2,273,667	1,263,327	1,263,254	2,279,708	1,263,327	1,263,254
R^2 (adj.)	0.8	0.8	0.8	0.7	0.7	0.7

Note: The table reports estimates of the regressions of interest rate on loans (columns 1–3) and log of loan amounts (columns 4–6) at the firm-bank-month level, 2017M1–2019M12. Bank controls are included but reported. FEs are fixed effects of firms, banks, firm*bank, and months. $Loan.quality_{f,b,t}$ is a categorical variable ranging from 1 (the best quality, reference) to 5 (the worst quality). $Bad.Bank_b$ is an indicator variable of bad bank, i.e., a bank fails for loss of capital (and its license was revoked with one of the following formulations: "the loss of capital due to excessive credit risk, insufficient reserves and/or involvement in questionable transactions, which also led to the loss of capital").

***, **, * 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 12. Loan quality in bad banks: regression estimation results

$Y_{f,b,t}$:=	Loan quality
$Bad.Bank_b$	–0.169***
	(0.035)
$Bad.Firm_{f,t}$	0.031***
	(0.004)
$Bad.Bank_b \times Bad.Firm_{f,t}$	–0.037
	(0.028)
Constant	1.821***
	(0.051)
Obs	1,263,970
R^2 (adj.)	0.7

Note: The table reports estimates of the regressions of loan quality at the firm-bank-month level on bad firms indicator variable and bad banks indicator variable, 2017M1–2019M12. Bank controls are included but reported. FEs are fixed effects of firms, banks, firm*bank, and months. $Loan.quality_{f,b,t}$ is a categorical variable ranging from 1 (the best quality, reference) to 5 (the worst quality). $Bad.Bank_b$ is an indicator variable of bad bank, i.e., a bank fails for loss of capital (and its license was revoked with one of the following formulations: "the loss of capital due to excessive credit risk, insufficient reserves and/or involvement in questionable transactions, which also led to the loss of capital"). $Bad.Firm_{f,t}b$ is an indicator variable of negative profits.

***, **, * 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.

APPENDIX

Appendix A. DESCRIPTION OF THE DATA

Table A.I. List of financial statement variables used in survival analysis and difference-in-difference nalysis

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
ROA	$\frac{\text{Net profit}}{\text{Total assets}}$	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
Taxes	$\frac{\text{Income Tax}}{\text{Total assets}}$	Income statement, Balance Sheet

Appendix B. THREE-OUTCOMES BANK-FIRM MATCHING MODEL

Table B.I. Multinomial logit regression results: splitting the firm-bank matches

	Match with a bad bank			Match with a good 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

Note: The table reports multinomial logit model with the three outcomes: never match, match with bad or good banks ($\jmath = 0, 1, 2$): $\mathbf{Pr}(New.Match_{f,t} = \jmath \,|\, \mathbf{X}_{f,t-1}; \Theta) = \Lambda\Big(\alpha_\jmath + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_\jmath + \mathbf{C}_{f,t-1}\Gamma_\jmath\Big)$ Dependent variable $New.Match_{f,t}$ is a categorical variable that equals zero if a firm that faced closure of its previous bank in the past never finds a new bank match (*reference*, 1 if a firm finds the new match with a *bad* bank (columns 1–3), 2 if with a *good* bank (columns 4–6). The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of marginal effects are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Appendix C. BANK-FIRM MATCHING MODEL WITH MACROECONOMIC AND REGIONAL CREDIT MARKET CONTROLS

Table C.I. Survival regression results with aggregate controls: splitting the firm-bank matches

	Match with a bad bank			Match with a good 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

Note: The table reports two-outcomes survival model with annual GDP growth rates and credit market concentration at the regional-level: $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(\alpha_j + \alpha_{j,bc} + \alpha_{j,r} + \alpha_{j,i} + \text{Firm.Quality}_{f,t-1}\text{B}_j + \mathbf{C}_{f,t-1}\Gamma_j + \delta_{j,1}GDP.growth_{t-1} + \delta_{j,2}HHI.credit_{r,t-1}\Big)$

Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new bank match with a *bad* bank (columns 1–3) or with a *good* bank (columns 4–6), and 0 if the firms never finds a match. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Appendix D. BANK-FIRM MATCHING MODEL: MULTIPLE FIRM-BANK RELATIONSHIPS WITH AT LEAST ONE BAD BANK WITHIN

Table D.I. Survival regression results with multiple firm-bank relationships: splitting the firm-bank matches

	Match with a bad bank			Match with a good 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

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new match with a *bad* bank (columns 1–3) or with a *good* bank (columns 4–6), and zero if the firm never finds the match. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

***, **, * 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.

Appendix E. BANK-FIRM MATCHING MODEL: CATEGORIZATION OF THE LOAN QUALITY IN THE CLOSED BANKS

Table E.I. Categorizing the days of non-performing loans: splitting the bank-firm matches

	Match with a bad bank	Match with a good 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 firm-to-bank matches	200	715
log L	–1,060.1	–2,918.9

Note: Dependent variable $New.Match_{f,t}$ is a binary variable that equals 1 if a firm that faced closure of its previous bank in the past finds a new match with a *bad* bank (columns 1–3) or with a *good* bank (columns 4–6), and zero if the firm never finds the match. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space. Firm controls include firm size, leverage, and liquidity (not reported).

***, **, * 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.

Appendix F. IN-ADVANCE DETECTION OF BAD BANKS

When developing a logit model of bank failures that has 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 Bank of Russia, shows that gambling banks, having observed the regulator's switching to tight regime in mid-2013, turned to permanently updating 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 thresholds).[47] The Bank of Russia itself was, and is, constantly learning these tools when revoking bad banks' licenses. Thus, we need to account for falsification schemes updating and the central bank 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 the fraudulence updating and the central bank's learning processes by running a loop of separate logit regressions on a 6-months rolling window starting from 2010M6, i.e., three years before the regulator's switching 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 description of the timing of the policy in Section 2).

As for fraud-specific indicators, and after our consultations with the 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 large portion of assets at 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 lend them to non-financial firms rather than to households.

As for the variables within the CAMELS approach, we use (i) capital adequacy ratio (C), NPLs 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), Annual return on total assets (E), the ratio of cash and government securities in total assets (L), Net inter-bank exposure at domestic banking system and net foreign assets abroad, both as % of total assets (S). We also include bank size to control for the 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 headquarter of a bank to the center of Moscow to capture geographical differences across banks.

The 6-months rolling window logit estimates appear in Table F.I.[48] 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 mid of the policy (2016M1), and around the end of the policy (2018M2). Dependent variable is a binary variable that equals 1 if a bank b was shut

[47] See an early review of these falsification tools here: https://www.banki.ru/news/daytheme/?id=6609791 (In Russian; for switching to English, one may use the automated web-translation tools).

[48] We also tested 12-months windows and found no qualitative changes compared to the baseline.

down at month t by the regulator due to falsifications revealed during the on-site inspections.[49] All explanatory variables are lagged one month.

The logit estimation results show that, depending on the sub-period considered, banks with greater capital, lower NPLs ratios, higher returns, and greater net inter-bank loans were less likely to be those that were closed by the Bank of Russia for the reasons of fraud detected. These are within the CAMELS approach. What concerns our fraud-specific indicators, we find a strong evidence that greater LLR together with lower NPLs are a significant predictor of fraud detection in the near future. Regarding regional controls, we find that banks operating in the regions with higher regional bank concentration, as measured by regional Herfindahl-Hirschman Index (HHI), are less likely to be closed for fraud. This can be viewed as a 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 the 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 the models with only CAMELS variables and the other for the models in which we add our fraud-specific variables. The results are reported in Fig. F.I. The area under ROC-curve equals 0.78 for the models with CAMELS and 0.88 for the models with those and fraud indicators. This indicates high in-sample quality of the models and a great added value of the fraud indicators.

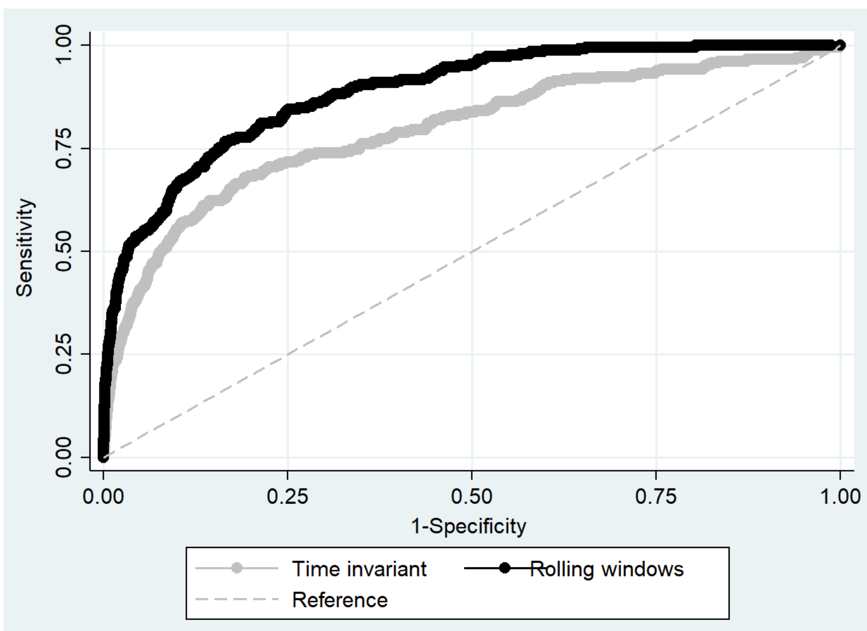


Figure F.I. The in-sample quality of logit models (Area under ROC-curves): CAMELS alone (*blue line*) and with fraud indicators (*red line*)

[49]The data on fraud-related closures come from the Bank of Russia's official press-releases during 2010 to 2020.

Table F.I. Probability of bad banks detection and closure: logit regression results

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 assets	–0.946	–0.664	–1.053	–0.575
	(0.775)	(3.559)	(0.666)	(0.490)
Growth of 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)
LLP > 50%$tile$	7.367***	–3.977	5.654***	6.497***
	(1.781)	(7.210)	(1.393)	(0.910)
LLP > 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

Note: Dependent variable $\widehat{Pr}\,(Fraud.Detection_{jt} = 1 \,|\, Controls_{jt-1})$ is a binary variable that equals 1 if an operating bank is closed for fraud revealed by the Central Bank of Russia, and 0 if a bank continues. The estimations are performed using 12-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. Constant not reported.

***, **, * 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.