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Quo Vadis? Evidence on New Firm-Bank Matching and Firm Performance Following 'Sin' Bank Closures

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Abstract

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JEL Classification: G21, G28

Keywords: Bank clean-ups, Regulatory forbearance, Firm-bank matching

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February 7, 2022

ABSTRACT

In 2013, the Central Bank of Russia started revoking licenses from fraudulent banks. By 2020, two-thirds of all operating banks had been shuttered. We analyze this unique period in history with credit register data. Following "sin" bank closure, poorly-performing "bad" firms rush to other (not yet detected) "sin" banks, while "good" firms transfer to "saint" banks. The "bad-sin" coupling more frequently occurs when "sin" is commonly owned or when the local banking market is unconcentrated. Before bank closure, firms remain unaffected; after bank closure, "bad" firms worsen in resiliency and profitability while "good" firms strengthen.

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1. INTRODUCTION

Firms derive value from bank credit beyond the benefit of merely obtaining external financing. Banks are able to mitigate information asymmetries between lenders and borrowers 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 from the relationship bank in times of distress (Bolton et al., 2016; Schäfer, 2019). Thus, the loss of an established relationship with a bank due to the bank's failure may have a negative effect on the firm. But the negative consequences of such an event are less clear if the bank is closed by its regulator due to fraudulent activities. In this paper, we analyze a large-scale bank closure policy in Russia to understand what happens to firms that face the closure of their "sin" bank due to fraud detection.

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 closure 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 closed 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 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 policies.

We, therefore, analyze a recent (rather dramatic) series of bank closures undertaken by the Central Bank of Russia (CBR), which began in 2013. Its intent was to clean up the banking system by closing banks engaged in fraudulent activities (see historical and institutional details in Section 2). This new regime of intensified fraud intolerance followed a period of widespread regulatory forbearance which lasted from 2006 to 2013. Over the period of seven years between 2013 and 2020—which were characterized primarily by normal economic times—the CBR effectively revoked around two-third of all banking licenses in the country. As a result, during these years almost 650 banks were briskly closed following fraud detection.

Three ingredients make this policy and its settings particularly informative. First, the policy begins unexpectedly following a prolonged period of regulatory forbearance, which resulted in a large fraction of the banking system being contaminated with fraudulent banks. Second, the active phase of the policy continued for over 5 years, which allows for a possibility of new matches between firms and not yet detected fraudulent banks following the closure of the firms' fraudulent bank. Finally, the bulk of the policy was conducted during the period of primarily normal economic times, which provides a better setting for identifying the real effects of the policy.[1]

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 the loan is a non-performing loan (NPL).[2] The latter data are unique in their coverage and comprehensiveness and were made accessible to independent academic research for the first time in this study. We merge these data with balance sheet characteristics of firms, taken from the SPARK-Interfax database, and of banks, as gleaned from the CBR website. We also manually collect data on all bank owners and directors during the last decade from a nationwide banking media source. We employ this information to assess if firms, following the closure of their sin bank, match with a new bank that has the same or different owners as in the 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 sin banks results in firms of different quality to engage with banks of different standing. We proxy the quality of the

[1] 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).

[2] We count the number of days during which the firm has been delinquent in its payment of interest and/or principal of a loan.

firms with two variables: (i) whether the firms have negative profits (firm-level); or (ii) the number of 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 these firms again match with (not yet detected) sin banks and the less likely that the firms end up at (similarly not yet identified all other) "saint" banks. The firms' profitability always has a positive effect on matching.

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

We then investigate several channels through which firm-bank matching may work. First, with our unique data on bank owners and directors, we find that among the 956 banks present after 2010 as many as 238 banks have interlocks with other banks through their bank holding company and/or through owners and/or directors. Following sin bank closure, between half and three quarter of the bad firms match again with a sin bank owned by the *same* owners. It takes only a year and a half to establish such matches, which is at least two times faster than on average. Conversely, establishing a new firm–saint bank match takes about three years.[3] Excluding banks with common ownership we find that following sin bank closure bad firms are *no more* likely to match with another (not yet detected) sin bank. In all instances, good firms match with a new saint bank, no matter if the latter shares common owners and/or directors with the firms' closing bank.

Second, apart from common ownership, we hypothesize that not all sin bank closures are equally predictable by economic agents. Some of the closures may be more *predictable* than others, based on for example publicly observable data reported in the banks' balance sheets. If the detection of bank fraud is predictable from its balance sheet then, we conjecture, the related "bad" firms will face difficulties engaging a new bank, even it is a sin bank. To assess this effect of *surprising* bank closures on firm-bank matching, we follow a two-stage procedure. In the first stage, we run a 6-month rolling window with a logit model explaining bank fraud detection to flexibly capture the regulator's learning about the current and updated fraudulent banks misreporting approaches. We

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

sort the failed banks into two categories: those with predicted probabilities below the unconditional threshold 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 the duration model for the two subsamples of firms: those that experienced surprising bank closures and those whose lenders' fraud detection was expected. Our results clearly show that new banks pay attention to where the firms come from: firms that were related to banks where fraud detection was predictable do not match easily with a new bank and the bad-firm-to-new-sin-bank move only works for closures that were surprising (i.e., when detection of fraud at the bank was difficult).

Third, we show that the concentration of regional credit markets matters for the matching of bad firms and saint banks. The higher the market concentration, the more likely a saint bank operating in this market will engage a bad firm coming from a closed sin bank. This result is consistent with the information acquisition hypothesis in Petersen and Rajan (1995) who argue that banks in more concentrated markets are more willing to finance opaque firms because future retention of the firm is more likely and therefore intertemporal subsidization is possible.

To confirm the validity of 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.[4]

With these findings at hand, we proceed to the difference-in-differences analysis of firm performance conditional on sin bank closure. We examine whether the closure of sin banks 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 lock-in effect (Liaudinskas and Grigaite, 2021). The estimation results show that the policy had a *cleansing* effect (Gropp et al., 2022) on the performance of good firms that faced sin bank closures: firms' employment and total size increase, total revenues improve, default rates decrease. We find the opposite for bad firms after sin bank closures. Using the credit register data on loan interest rates, we show that a potential explanation involves credit risk underpricing by sin banks, especially in the case of bad firms: bad firms enjoy a lower rate at a sin bank than they would at a saint bank. When the sin bank is closed bad firms lose their "subsidized" loans, which in combination with the lack

[4] In general, the latter result 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).

of opportunities and incentives to improve further deteriorates the state of bad firms.

Our paper contributes to 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/or asset quality reviews. Also, such clean-up policies often take place as a response to a crisis.[5] 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 many sin bank closures. Such a clean-up policy is of particularly interest to emerging economies, which are likely to suffer more from widespread malpractice in their banking system.

Second, our paper contributes to the literature on the real effects of bank distress on firms (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 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 possibly locked-in by these banks). After failure, the firms then benefit by obtaining a lower loan rate from a new bank. Yet despite the impact of a branch or bank closure on loan rates, work by Greenstone 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 the closure of a fraudulent sin bank, bad (good) firms are more likely to end up in a match with a sin (saint) bank and that the performance of a bad (good firm) worsens (improves).

Third, our 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; Gropp et al., 2022). The literature usually rationalizes the 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

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

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 (Degryse and Ongena, 2005; Petersen and Rajan, 1995; Bolton et al., 2016; Degryse et al., 2019a; Schäfer, 2019). We show that a 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 the concentration at local credit markets rises.

The rest of the paper is structured as follows. Section 2 describes the policy experiment 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 switch to new sin or saint banks. In Section 5, we explore the channels of firm-bank matching. In Section 6, we present the difference-in-differences estimation of the real effects of sin bank closure on firm performance. Finally, Section 7 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 a market-based economy. As such, Russia had witnessed a rapid growth of privately-owned banks.[6]

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

[6] 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%.

(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, Andrey Kozlov, who was the key figure behind the clean-up policy implementation. The so-called "Kozlov affair" shocked the banking community in Russia and led to an extreme form of regulatory forbearance: bank closures became rare and took place primarily when the owners of failed banks simply had no interest to continue with the business, irrespective of whether this business was legal or not.[7]

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 for loans.[8] The financial crisis 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 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 Andrey 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.[9] 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.[10]

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

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

[9] The change of the head of the Bank was announced rather unexpectedly: Elvira Nabiullina, the-then head of the Ministry of Economic Development, was to replace the then head of the Bank Sergey Ignatiev, who held the post for the last 13 years

[10] 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%. Moreover, there seemed to be no apparent discontinuity over the policy following the appointment of the new head:

However, soon it became clear that the Central Bank of Russia had rather rapidly swung from the regulatory forbearance regime towards a strict fraud intolerance. 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.[11] In February 2018, the Bank has officially announced that the active phase of the cleansing policy was over amid the great body of fraudulent 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-Petersburg—where more than 75% of the banking system in terms of total asset size is concentrated—but in fact affected every region up to the far East, with the largest number of license revocations taking place in the 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 affected banks' total liabilities, again spreading through the whole territory of Russia.[12] 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: (a) had raised loan loss reserves, (b) disclosed more NPLs in their loan portfolios, (c) reduced the stock of (possibly opaque) loans to firms, and (d) slowed down new loan issuance, 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 shrinking of the 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 rising rapidly.[13]

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for example, the previous head of the bank took up the post of the new head's adviser.

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

[12] 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.

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

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.[14] 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.[15] 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.[16] For each bank 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).

The 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 days overdue equal to 150 or 200 routinely include loans that were labeled as "hopeless", payed by collateral, contested in courts, or written off.

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

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

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

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 655,300 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).

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 were 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 "sin" banks, while those that were permitted to pursue their activities we dub as "saint" 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 sin 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 sin banks. The firm-level data are not available for 6,062 of these firms. Furthermore, after trimming 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 saint banks, sin banks have as expected 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 switch to a saint bank, firms that switch to a sin bank, and those who never switch. Out of 6,267 firms in our sample the overwhelming majority of firms (85%) never find a new bank to borrow from. Those who manage to switch to a new bank (15%) mostly establish a connection with a saint bank (11% or 715 firms). The rest (3.2%) borrow from a new sin bank. Firms that switch to a saint bank are generally in a better financial shape with average ROA of 5%, smaller leverage and higher liquidity than 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 saint bank, we observe the inverse picture in our data. Average size of a firm that switches to a sin bank is almost 3 times higher then average size of a firm that switches to a saint bank (85 mln vs. 29 mln Rub), and almost two times higher then average size of those who never switch (85 mln vs. 44 mln Rub). Thus, we can describe a firm that switches to a sin bank as a large financially constrained firm (higher leverage, lower liquidity then for an average firm that switches to a saint bank).

Table 2 describes regional structure of our data. In more than a half of the 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 than 1%) are with firms from North Caucasian FD. The 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.

The regional dimension of our data allows us to look into the spatial concentration of the Russian regional credit markets by calculating a 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 SIN 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 (sin bank) and conditional on the firm's survival to the moment in time when the new match is established.[17] 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.[18] In our analysis, we focus on *single* firm–sin 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.[19] We are interested in *where* the firm goes next, i.e., after the closure of its sin bank: to another (not yet detected) sin bank or to saint bank. The rationale for focusing on single firm-bank relationships at the moment of sin 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 sin 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 sin bank b at time t^*_f (t^*, for simplicity) and ends with the firm being matched with a new bank at time $t^* + k$, where k is the 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

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

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

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

on survival to the current moment in time, is described by the following hazard function $\lambda(\cdot)$:

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

where Firm.Quality$_{f,t-1}$ is firm f quality proxy at time $t-1$, which is measured by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b}+k$. $\mathbf{C}_{f,t}$ is a set of control variables including the firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios (see definitions in Table A.I; all controls 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: $\lambda_0\left(t\right) = \lambda > 0$.[20]

Table 3 reports the estimation results of equation (1). In columns (1)–(2) the firm quality measure is proxied by the log of days of NPLs the firm had accumulated in the closed sin bank by the moment of closure t^*, log $DNPL_{f,t^*}$. Here, the sample consists of 6,249 firms, 413 bank closures, and 915 "failures," i.e., new firm-bank matches. We obtain negative but largely insignificant estimates on the log $DNPL_{f,t^*}$ variable, moreover, the estimated coefficient is close to zero. Next, in columns (3)–(4) we replace this granular measure 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 two previous columns. The idea behind including this variable is as follows. Although a firm might suffer losses at t^* when its sin 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

[20] 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.

with losses reported at the moment of new matching has a 33.2% lower chance for this match.[21] 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 sin 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 sin bank–that has not yet had a chance to be shut down–from those with a saint bank. Put differently, we hypothesize that bad firms are more likely to be sorted to sin banks whereas good firms are more likely to match with saint banks. Because the CBR's cleansing policy stretched in time for over five years, it gave the firms that were separated from sin banks an opportunity to be matched again with with another (not yet shut down) sin bank.

To test these hypotheses we slightly modify the duration regression we applied above. Specifically, we consider two hazard functions instead of one: $\lambda_1(\cdot)$ for the firm's decision to match with a new sin bank vis-a-vis never match and $\lambda_2(\cdot)$ for the case when the firm seeks to match with a new saint bank vis-a-vis never match:

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

where $j = 1$ stands for regression with sin bank matching and $j = 2$ for saint 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 sin banks and columns (4)–(6) with saint banks, for different measures of firm quality. For the duration analysis of matching with sin banks the sample consists of 6,069 firms and nearly 200 new sin matches, and the average duration of the spell changes from 35 months, which was true across all matches, to 19 months. For the matches with saint banks, the sample comprises of 6,080 firms and 715 new "saint" matches, and the average duration of the spell rises to 42 months. Note that the

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

200 new sin matches and 715 new saint matches constitute the 915 matches we considered above before splitting the sample.

Strikingly, our split estimates suggest that the insignificant effect of log $DNPL_{f,t^*}$ obtained above now flips its sign and turns *positive* and highly significant in the regressions of matching with sin banks (column 1). Conversely, in the regressions of matching with saint banks respective estimate is negative and also highly significant (column 4). Jointly, these estimates support our hypothesis on endogenous sorting of firms: conditional on sin bank closure, bad firms match with another (not yet detected) sin bank whereas good firms establish a relationship with a saint bank. 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 sin bank and by 16.2% less likely to join a saint bank in the future.[22]

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 sin banks and good firms to saint 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 sin 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 sin bank closures, are *not* more likely to establish a match with another sin banks in the future. Economically, the underlying effect is very large: a firm with negative profits at t^* has a 77.1% less chance to match with another sin bank. However, we treat this result with caution: the estimated coefficient itself is only marginally significant, and thus uncertainty is large, as opposed to the highly significant coefficient on the loan payment delinquencies variable obtained above.

In the regression of matching with saint banks, we 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 sin banks are *not* less likely to match with saint banks in the future. This estimate is also in stark contrast to what we obtained for the loan delinquencies variable above.

Finally, we consider whether a firm had negative profits not only at t^* when the firm's sin bank fails but at $t^* + k$ when the firm matches with another sin bank, column (3), or with a saint bank, column (6). As can be observed from the two columns, we obtain negative and significant

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

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 sin bank and by 31.9% less likely to join a saint 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 sin and matching with saint banks. To address this concern, we run a *multinomial regression model* in which we have all three options for a firm: never switch (0), match with a sin bank (1) and match with a saint bank (2). As Table B.I show, the estimation results are qualitatively and even quantitatively very close to the baseline.[23]

Another concern is that we omit *macroeconomic and regional characteristics*, which both might affect the CBR's intention to close problem banks.[24] 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 sin. Table D.I clearly indicates 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 sin and saint 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 switch to a saint bank. Jointly, these results imply that firms behave *strategically*: if they establish

[23] 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.

[24] 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.

multiple bank relationships, they may use sin banks to 'store' the worst part of their debt while servicing the best part of their debt in saint banks. When their sin 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 saint bank may strongly prefer to reject the firm.

We begin with testing the 90 days threshold by substituting our initial variable log $DNPL_{f,t^*}$ with a binary version in which it equals 1 if $DNPL_{f,t^*} \geq 90$ and 0 if else. We obtain that the estimated coefficient on the new binary variable is insignificant for matching with sin banks and remains negative and highly significant for matching with saint banks.

We then 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 sin banks, the estimated coefficients on the categorical variables $30 \leq DNPL_{f,b,t} \leq 60$ (bin 2) and $60 \leq DNPL_{f,b,t} \leq 90$ (bin 3) are both positive and highly significant. The estimated coefficients for bins 4 and 5 are also positive but insignificant. 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 'sin' banks is positive for small and moderate magnitudes of loan delinquencies (below 90 days) but turns negative for very large delinquencies (above 150 days). Sin banks, despite being "sin", are not willing to accept the hopeless firms.

In column (2) with the results on matching with new saint banks, we obtain negative coefficients on mostly all categorical variables, with those for $30 \leq DNPL_{f,b,t} \leq 60$ (bin 2), $120 \leq DNPL_{f,b,t} \leq 150$ (bin 5), and $150 \leq DNPL_{f,b,t} \leq 180$ (bin 6) being significant. Therefore, saint banks really prefer to establish matches with the firms that had virtually no bad debts in the closed sin banks.

Regarding the other control variables at the firm-level, our estimates indicate that, all else being equal, size has a non-linear relationship with the likelihood of matching with both sin and saint banks, with mid-sized firms revealing the largest likelihoods.[25] 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 sin bank closures are *more* likely to match with another (not yet detected) sin banks and are *less* likely to establish relationships with saint banks. This favors endogenous firm-bank matching that appears under a stretched-in-time regulation policy targeting sin banks detection. Turning from 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 sin bank closure or the moment of matching with new banks, are always *less* likely to establish new relationships with banks, no matter of sin or saint type.

5. FURTHER ANALYSIS OF NEW BANK-FIRM MATCHES

In this section, we further explore the formation of new bank-firm matches following the closure of a sin bank. In particular, we examine how our baseline result depend on the common ownership between the old and new sink bank, the degree of how well anticipated the closure of a sink bank was, and regional credit markets concentration.

5.1 Common Bank Ownership

One potential explanation of our baseline results is that having faced the closure of their sin banks, bad firms consequently matched with another (not yet detected) sin bank that has *the same* owner. More generally, several banks may constitute a bank holding group, or the same individuals may appear on the board of directors in different (formally not related) banks. We refer to this channel as the *common bank ownership*, for simplicity.

[25] 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.

In this section, we examine whether our baseline results are driven by the common control/ownership of the firm's old and new sin banks. For this purpose, we re-estimate our duration regressions (2) on a subsample of those firms that match with a new bank that is unrelated to the closed sin bank through either ownership or control structure.

In order to be able to construct such a subsample, we collect the data on the ownership structure of Russian banks as well as personal information on every member of the board of directors of the banks operated(-ing) in the Russian banking system over the last decade.[26] These detailed bank-level data cover all banks that operated in Russia from 2010 till 2021.[27]

Overall, we find that among the 956 banks in our database as many as 238 banks have overlapping ownership or control structures. In fact, more than half of all firms that had relationships with sin banks matched with another (not yet detected) sin banks owned/controlled by *the same* persons.

Table 5 present the estimation results of the duration regressions (2) on the reduced sample without common ownership between the old and new sink banks. Columns (1)-(3) summarize the results of matching with a new sin bank, while columns (4)-(6) present the results of matching with a new saint bank. As can be inferred from column 1 of Table 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 sin banks or the sin 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 the respective part of the baseline result.

Overall, our estimation results highlight the importance of common ownership for the matching between bad firms and (not yet detected) sin banks after the closure of the old sin bank. In fact, on

[26] This data is manually extracted from the nation-wide banking media resource banki.ru.

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

the subsample without common ownership, our estimation no longer predicts that that bad firms are more likely to end up in a new match with a (not yet detected) sin bank In contrast, good firms, are more likely to match with new saint banks regardless whether there is common ownership between the old and new banks.

5.2 Surprising Bank Closures

With such a large number of sin bank closures naturally not all of them were perceived as equally likely to happen: while some were more predictable the others were more surprising. The more predictable closures are indicative of a more severe or, at least, more transparent bank fraud. Intuitively, following a relatively more anticipated sin bank closure, the firms must have had a harder time finding new matches regardless of their perceived quality based on their credit history or profitability. Thus, we examine how our baseline results are affected by how well the bank closures could have been predicted.

To capture the effect of the surprise in a sin bank's closure on the new endogenous firm-bank matching, we proceed with the following two-stage approach. First, we run a simple predictive logit regression of bank closures and sort all failed banks by their respective predicted probabilities into the two groups of "well-predicted closures" and "surprise closures." Second, we re-estimate our duration regression (2) separately for these two groups of closures.

The details on the first stage—that is, the estimation of the predictive model—are presented in Appendix F and in Table F.I. We define "well-predicted closures" as those with the predicted probability of closure above threshold $\bar{p}$, while "surprise closures" are those with the predicted probability of closure below the threshold $\bar{p}$. We set the threshold $\bar{p} = 0.5\%$, which is the mean of the predicted monthly probability of bank closure in the sample.[28] As a result of this sorting, we classify about 250 bank closures as surprises and about 150 ones as well predicted. In Figure 11, we plot the evolution of predicted probabilities of closure in time. The predicted probabilities are close to zero prior to the policy and increase dramatically during the active phase of the policy (i.e., between 2013M7 and 2018M2).[29]

[28] Annualized, the average predicted probability of closure is about 6%. As a part of robustness checks, we also consider substantially higher values for the threshold $\bar{p}$ of 1% and 1.5%. Qualitatively, our results are robust to these higher values of the threshold.

[29] Note that the predicted probabilities are month frequency. 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

Table 6 presents the results of estimating the duration regression (2) separately for "surprise" closures in columns (1)-(2) and for "well-predicted" ones in columns (3)-(4). Panel 1 of Table 6 demonstrates that our baseline results are fully driven by the "surprise" bank closures. In the duration regressions of matching with new sin banks, the estimated coefficient on log $DNPL_{f,t^*}$ is positive and highly significant in the case of "surprise" closures (column 1) and is negative and insignificant in the case of "well-predicted" closures (column 3). Moreover, in the case of "surprise" closures, the magnitude of the effect rises by about 30% as compared to the baseline estimation results. Similarly, in the duration regressions of matching with saint banks, the estimated coefficient on the log $DNPL_{f,t^*}$ variable is negative and highly significant under the surprise condition (column 2) but is insignificant under the other condition (column 4). Again, the magnitude of the coefficient increases by about 30% compared to the baseline estimation results.

Further, in Panel 2 of Table 6, we replace the log $DNPL_{f,t^*}$ with the firm's profitability as an alternative proxy of its quality variable—that is, with $Profit_{f,t^*} < 0$ and $Profit_{f,t^*+k} < 0$. The estimation results in Panel 2 are consistent with those in Panel 1. We obtain significant coefficients on the negative profits variables only in the case of the "surprise" closures—columns (1) and (2).

Overall, our results suggest that when the closure of a sin bank is more predictable then the quality of its firms measured by accounting information does not help to predict the sorting of the new firm-bank matches. If we interpret higher closure predictability as the evidence of a more severe fraud then one potential explanation behind this result is that the firm's past accounting information is viewed by banks as less reliable if it was generated in the relationship with a more fraudulent bank.

5.3 Regional Credit Markets Concentration

Next, we examine the effect of regional bank market concentration on our baseline results. The CBR's cleansing policy was associated with a rising market concentration because many banks were closed. Following a sin-bank closure, firms have fewer opportunities to find a new bank match. In particular, bad firms become increasingly more restricted in their abilities to match with *not yet detected* sin banks. Saint banks, however, may be less willing to lend to bad firms to protect

the predicted probabilities and annual real GDP growth rates. This suggests that the policy and macroeconomic conditions were fairly orthogonal to each other.

their market power from the uncertainty associated with financing of bad firms. Thus, in need of credit, bad firms could be effectively forced to improve to be accepted by a saint bank. Saint banks, however, may be less willing to lend to bad firms to protect their market power from the uncertainty associated with financing the projects of bad firms.

To examine the effect of regional bank market concentration on our baseline results, we slightly modify our duration regressions by introducing a cross-product of the regional HHI concentration measure with a firm quality proxy:

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

The estimation results are presented in Table 7, where Panel 1 contains the results with firm quality proxied with the days of NPLs while in Panel 2 firm quality is proxied with negative profits.

As can be seen in Panel 1, the estimated coefficient on the interaction of log $DNPL_{f,t^*}$ and $HHI.credit_{r,t-1}$ is insignificant in column (1) and positive and highly significant in column (2). Qualitative, the same result is in Panel 2. Our results suggest that rising credit markets concentration observed was unlikely to prevent bad firms from matching with not yet detected sin banks but, at the same time, it facilitated new matches between bad firms and saint banks. One possible interpretation is that the saint banks could extract rent from relationships with bad firms by setting higher interest rates. Furthermore, saint banks operating in the regions with highly concentrated credit markets could possess more developed skills in evaluating projects, and thus they might be able to provide valuable expertise to bad firms helping them to improve.

6. SIN BANK CLOSURE AND FIRM PERFORMANCE

In this section, we examine the real effects of sin bank closures on firm performance. Firms derive value from bank credit beyond the benefit of merely obtaining external financing. Thus, the loss of an established relationship with a bank due to the bank's failure may harm the firm. However, if the closed bank in question is engaged in fraudulent activities then the negative consequence of its closure on firm performance is less obvious. One could even argue it could benefit better quality firms.

There is one potential issue with evaluating the effect of sin bank closures on firm performance. If firms anticipate the closure of their sin banks in advance then they could make preemptive adjustments already before the closure. If so, the effect of a sin bank closure per se could be distorted by the firm's preemptive actions. We conduct two tests to examine whether firms could anticipate the closure of their sin banks in advance. Specifically, we test whether firms preemptively leave sin banks in anticipation of regulatory closure and whether firms strategically delay loan repayments around the closure date. These tests are based on the conjectured heterogeneous responses of high and low-quality firms to the prospect of sin bank closure and are presented in Appendix G. These tests do not support the hypothesis that firms anticipated the closure of their sin banks.

Specifically, we want to understand what happens to firm performance *after* the firms are confronted with the closure of their current (sin) bank but *before* they find a new bank match. On the one hand, one might expect that firm performance deteriorates because by losing their bank, albeit a sin one, firms become more financially constrained (Chodorow-Reich, 2014; Chopra et al., 2020). On the other hand, firm performance could improve due to the termination of the hold-up problem (Liaudinskas and Grigaite, 2021).

To answer this question, we employ the difference-in-differences approach with the time-varying imposition of treatment (TV-DID, Goodman-Bacon, 2021). The *treatment group* consists of all those firms, bad and good, that faced their sin bank closures at some point in time during 2013–2020. Specifically, we define treatment as the closure of sin bank b that affects firm f at time $t^*_{b,f}$. Thus, our treatment variable is variable $Sin.Bank_{b,f}$ which equals 1 if firm f's bank b is ever closed due to fraud detection. Furthermore, for each firm f, let $POST_{\{t \geq t^*_{b,f}\}}$ define an indicator variable equals 1 for all t following $t^*_{b,f}$, and 0 otherwise.

The *control group* is constructed by matching firms on the set of observable characteristics using the nearest neighborhood estimator of Abadie and Imbens (2011). The following set of observable characteristics is employed: firm size, leverage, liquidity, return on assets, and annual growth of total assets. We follow the so-called "1:4 rule of thumb" and match firm f that has faced its bank closure at $t^*_{b,f}$ (i.e., a "treated" firm) with four similar ("control") firms that (i) also have relationships with sin banks and (ii) have not faced closures of their sin banks within two years before and after firm f.[30]

[30] That is, we consider a moving window of $[t^*_{b,f} - 2, t^*_{b,f} + 2]$. Thus, our treatment group includes firms that faced

The closure of a (sin) bank can be viewed as a *credit supply shock*. The literature typically considers the effects of credit supply shocks on firm employment (Chodorow-Reich, 2014), investment and sales (Gropp et al., 2018; Degryse et al., 2019a; Chopra et al., 2020), among other measures of performance. In our analysis, we employ similar measures of performance except for investment.[31] Additionally, we also use firm profits and firm default rates as our measures of firm performance.

Finally, acknowledging that any real effect of sin bank closure on a firm's performance can be mitigated if the firm has more than one bank relationship (i.e., borrows from more than one bank), following Degryse et al. (2019a) we focus only on those firms that have *single* bank relationship. In what follows, we thus run our TV-DID regressions for the subsamples of firms that had only one (sin) bank at the moment of the bank's closure.

Formally, we specify the following TV-DID regression:

$$\begin{aligned} Y_{f,t} = \alpha_f + \alpha_t + \beta_1 \Big(Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}} \Big) + \\ + \beta_2 \Big(Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t} \Big) \\ + X'_{f,b,t} \Psi + \varepsilon_{f,t}, \text{ if } t \in [t^*_{b,f} - 2, t^*_{b,f} + 2] \end{aligned} \tag{4}$$

where $Y_{f,t}$ is a measure of firm f's performance, among which we consider (i) firm size (the log of total assets), (ii) the ratio of debt to total assets, (iii) the ratio of total revenue to total assets, (iv) the ratio of number of workers to total revenue, (v) the ratio of profit to total assets, and (vi) a binary variable which equals 1 if firm f defaults in year t and 0 otherwise. $X_{f,b,t}$ includes various control variables such as firm size and its square, leverage and liquidity ratios to total assets, where appropriate, to capture any residual differences between the treated and control firms remaining after the 1:4 nearest neighborhood matching. Equation (4) is estimated with logit when the dependent variable is binary (i.e., case (vi)) and with panel FE estimator otherwise (cases (i)-(v)).[32] We require firms not to default between $t^*_{b,f} - 2$ and $t^*_{b,f}$ in logit regression (case (vi)) and

the closure of their sin banks at most until the end of 2018. As our sample ends in 2020, the last treated firm appears at the end of 2018 since, by construction, we require that it is matched with four control firms that did not face their sin bank closures within 2016-2020).

[31] Our firm-level data (provided by SPARK-Interfax) unfortunately contains a very large number of missing values on investment. Thus, using the data on investment will result in the total number of observations shrinking by a factor of 10, at least.

[32] Those observations for which the new firm-bank match is created before $t^*_{b,f} + 2$ are censored to insure we are analyzing firm performance before the firm finds a new bank.

we require firms to survive until at least $t^*_{b,f} + 2$ in panel FE regressions (cases (i)-(v)) to escape "survivorship" bias.

The estimation results of equation (4) are summarized in Table 8. After the nearest neighborhood matching and restricting the sample of firms by imposing condition $t \in [t^*_{b,f} - 2, t^*_{b,f} + 2]$ years, we have only about 10,745 to 18,613 observations at the firm-year level for different dependent variables.

Column (1) of Table 8 starts with firm size as dependent variable in equation (4). The estimated coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ is positive and highly statistically significant, whereas the coefficient β_2 on the $Sin.Bank_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ is negative and statistically significant at 5%. This implies that while good firms tend to grow in size following sin bank closure (but before they they start borrowing from a new bank), bad firms tend to shrink in size. Economically, these effects and their differences are significant. Regressions in the next columns of the table shed light on why we may obtain these differential effects.

In column (2) of Table 8, we turn to results on firm leverage. The estimated coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ is positive but it is not statistically significant. Thus, we do not find any effect on a treated firm's leverage-to-total assets ratio. The estimated coefficient β_2 on the $Sin.Bank_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ is, however, positive and marginally significant. Therefore, our results suggest that the leverage of bad treated firms increases by as much as 10 percentage points relative to good treated firms. This is a sizeable effect, given that the mean leverage ratio of the firms in our sample lies between 75 and 95%. Further estimations show that the absolute amount of borrowing by low-quality treated firms declines but less than their total assets (see column (2) of Table H.I in Appendix H).

Column (3) of Table 8 presents the result for firm's total revenue. We obtain a positive and highly significant coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ and a negative and significant coefficient β_2 on $Sin.Bank_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$. In absolute terms, β_2 exceeds β_1 by a factor of 2, which implies a heterogeneous treatment effect on firm revenue-to-total assets depending on firm quality. Following their sin bank closures, good treated firms have their revenues going up, while the revenues of bad treated firms decline, relative to the firms' total assets. The result on firm revenue is in line with that on leverage. It provides evidence of a cleansing effect of sin bank closure: good treated firms improve while bad treated firms deteriorate.

In column (4) of Table 8, we present our results on firm employment. We obtain a negative and marginally significant coefficient β_1 on $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ while coefficient β_2 on the $Sin.Bank_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ is estimated positive and significant. In absolute terms, β_2 exceeds β_1 by a factor of 2. That is, the treatment effect on firm employment-to-total revenue is also heterogeneous. Following their sin bank closures, good treated firms reduce labor force to total revenue ratio, 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—column (3)—we obtain that their revenues grow faster than the number of workers employed. This is also confirmed by our additional regressions in which we replace revenue-to-total assets ratio with the log of revenue and employment-to-revenue with the log of employment: the estimated semi-elasticity of revenue exceeds that of the number of workers by a factor of two (compare columns (3) and (4) of Table H.I in Appendix H).

In column column (5) of Table 8, we examine the effect on profits. We obtain positive but insignificant coefficient β_1 on the $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ variable and negative and insignificant coefficient β_2 on the $Sin.Bank_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$ variable. The signs are consistent with the firm improvement 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.

Finally, column (6) presents the estimation result when the dependent variable is *firm defaults*. In this case, we obtain a negative and marginally significant coefficient on the $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}}$ variable. That is, following the closure of a sink bank the failure risk of a good treated firm *decreases*. At the same time, the variable $Sin.Bank_{b,f} \times POST_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t} = 1$ perfectly predicts firm defaults, and thus this variable is dropped from the estimations (marked as "n/a" in the table). Our results, thus, suggest that while the stability of good quality firms improves following the closure of a sin bank, the opposite result holds for low-quality firms. This result is consistent with the findings in previous columns and indicates the cleansing effect of sin bank closure on firms.

Next, we attempt to shed some light on why good treated firms would improve while bad treated firms would deteriorate following their sin bank closures. We hypothesize that sin banks may underprice credit risk when lending to firms thus effectively subsidizing firm credit. The loss of such a subsidy would naturally pose a bigger problem to a bad rather than a good firm. Thus, the underpricing of the credit risk by sin banks could explain why bad firm performance is likely to deteriorate following the closure of its sin bank—the loss of the subsidy combined with the lack of opportunities and incentives to improve will further deteriorate the state of bad firms. On the other hand, good firms have better incentives and abilities to improve following the closure of their sin banks, which would help to decrease their cost of credit in the future.

To test this hypothesis, we employ the credit registry loan-level data on interest rates available from 2017 at a monthly frequency. The credit register contains data on loan contracts and includes interest rate, loan amount, loan maturity, type of credit, and the ex-ante assessment of the borrower's credit risk on the scale of 1 to 5 with 1 being the lowest risk and 5 being the highest risk.

First, we examine a linear regression model of the interest rate that a bank b sets to firm f at month t on the sin bank indicator variable (bank level), credit risk category from 1, lowest risk, to 5, highest risk (firm-bank-month level), and the product of the two, controlling for firm and firm*month fixed effects, log of loan volumes, loan maturities, relevant bank-level controls, and regional and macroeconomic characteristics:

$$\begin{aligned} r^L_{f,b,t} = \sum_{j=1}^{5} \beta_j \cdot \left(Sin.Bank_{b,f} \times Credit.Risk^{(j)}_{f,b,t} \right) + \gamma Sin.Bank_{b,f} + Loan.Control'_{f,b,t}\Xi \\ + Bank.Control'_{b,t}\Psi + Macro.Control'_{t}\Phi + \alpha_f + \alpha_{f,t} + \epsilon_{f,b,t} \end{aligned} \tag{5}$$

With this composition of variables, we have up to 1,774,379 loan-level observations. We obtain a positive and highly significant coefficient on the $Sin.Bank_{b,f}$ variable, meaning that sin banks charge 1.5 percentage points higher interest rates on loans than do the saint banks (see Table 9). However, we further obtain a negative and highly significant coefficients on the interactions of the sin bank variable and credit risk category, and the magnitudes of the estimates range from –1.6 to –0.5 percentage points. This means that, within *the same* sin bank, firms with poorer quality *pay less* on their loans while firms with better quality *pay more*. These results hold on the sample of all loans issued and for the subsample of multiple loans, i.e., for the firms that obtained at least two loans within the 2017-2020 period. The latter subsample allows us to shut down demand effects by including firm*month fixed effects. Overall, our regression results here are consistent with the hypothesis that sin banks underprice risk, especially in the case of lower quality firms.

Second, we proceed to a linear regression of the credit risk category on the bad firm and sin bank indicator variables, the product of the two, controlling for the same characteristics as in the interest rate regression above. The credit risk regression reads as:

$$\begin{aligned} Credit.Risk_{f,b,t} = \beta \cdot \Big(Sin.Bank_{b,f} \times Bad.Firm_{f,t} \Big) + \gamma Sin.Bank_{b,f} + \delta Bad.Firm_{f,t} \\ + Loan.Control'_{f,b,t}\Xi + Bank.Control'_{b,t}\Psi + Macro.Control'_{t}\Phi + \alpha_f + \alpha_{f,t} + \epsilon_{f,b,t} \end{aligned} \tag{6}$$

With this composition of variables, we have up to 1,263,970 loan-level observations. We obtain a negative and highly significant coefficient on the sin bank variable, meaning that the same borrower gets higher credit quality ex-ante assessment by sin banks as compared to saint banks (see Table 10). Further, we obtain negative and highly significant coefficient on the interaction of the sin bank and bad firm indicators, which implies that, within a given sin bank, bad firms receive a relatively higher, not lower, credit quality assessment. As in the case of interest rate regressions, these results hold for both the sample of all loans and the subsample of multiple loans.

Overall, our analysis of sin bank closures provides evidence of heterogeneous treatment effects on firm performance. Closing sin banks improves the state of good firms while having the opposite effect on bad firms. Our further empirical findings suggest that this result is due to credit risk underpricing by sin banks, especially in the case of low quality firms.

7. CONCLUSION

Our study shows that, following sin bank closures, bad firms are more likely to match with any remaining sin banks, especially if the banks are held by the same owners. Good firms on the other hand match with saint 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 sin banks were closed and before they matched with any remaining banks.

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FIGURES

Figure 1. Bank Closures and the New Head of CBR

Note: This figure depicts the time series of monthly bank closures (the left y-axis) and 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.

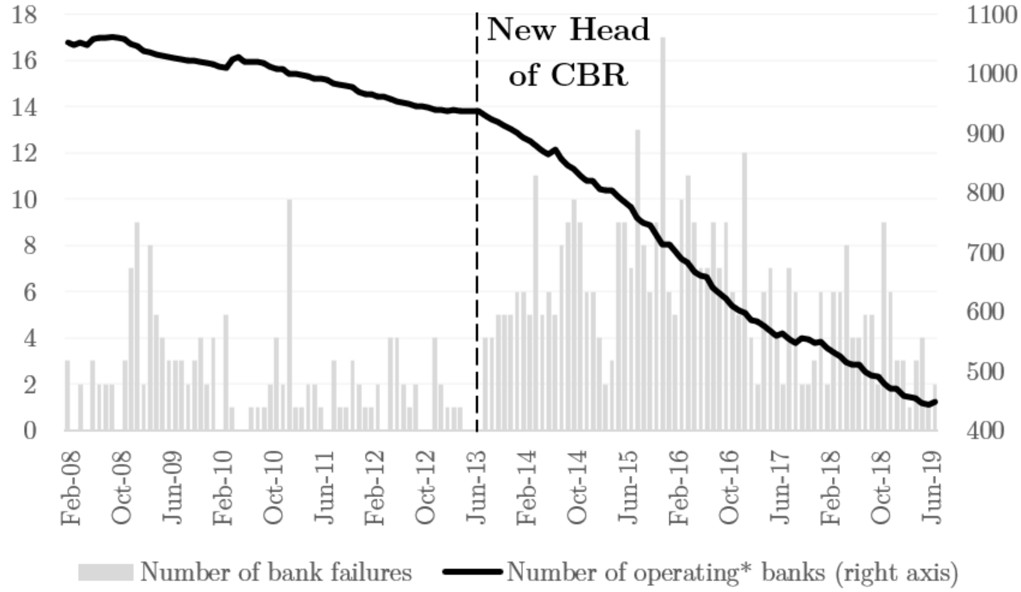


Figure 2. Geography of bank fraud and bank closures

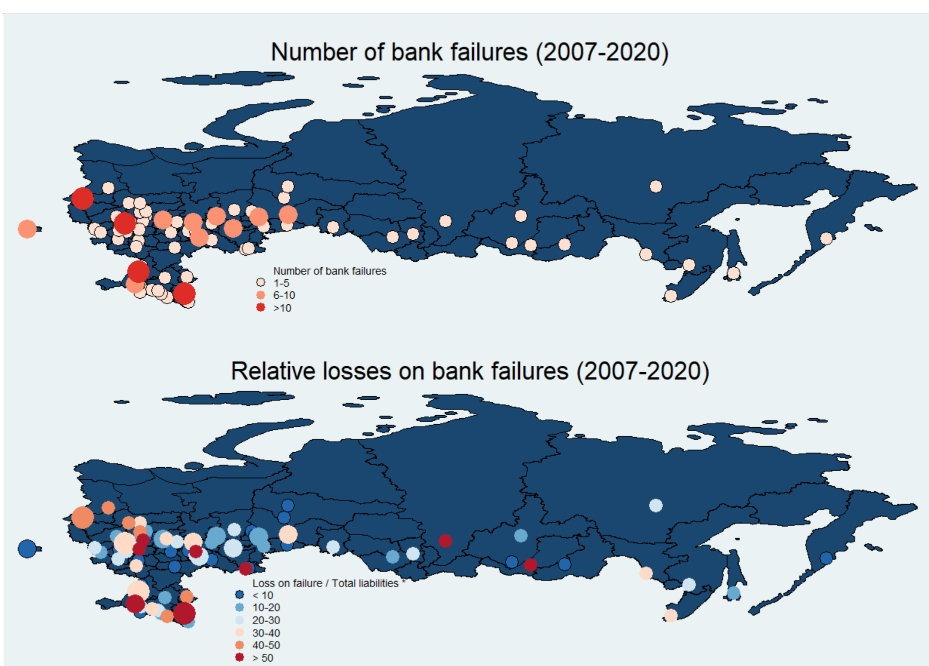
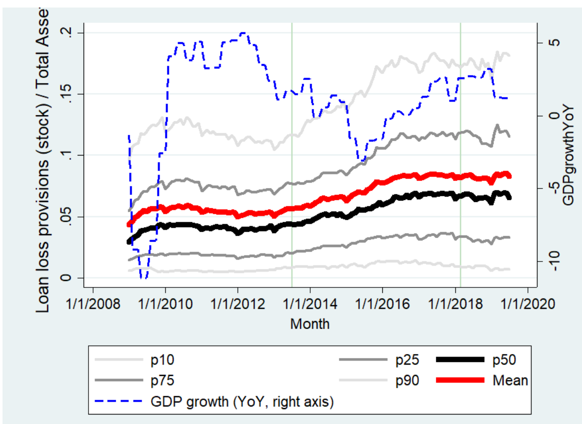
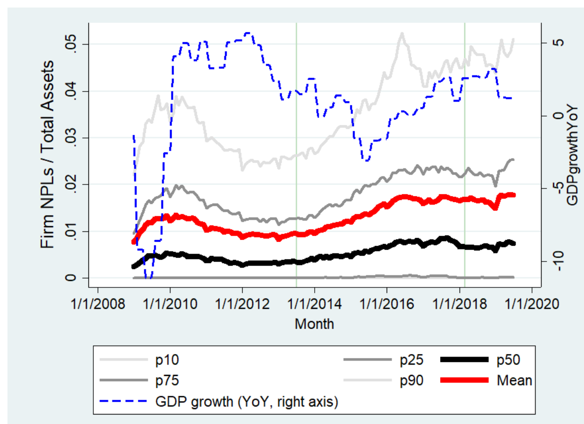


Figure 3. Time evolution of selected bank variables before and during the active phase of the tight regulation policy (Jul.2013–Feb.2018)

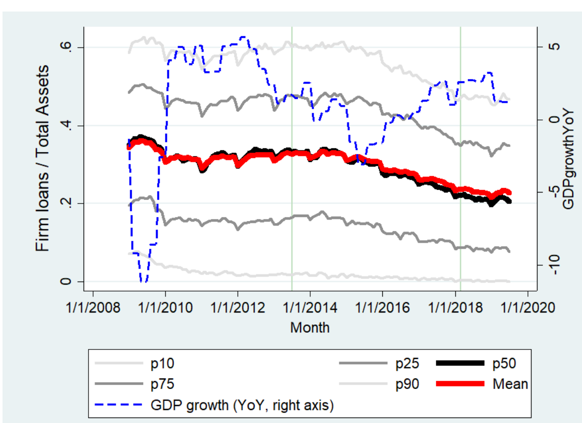
Note: The figure depicts time evolution of selected bank characteristics at the bank-month level before, during, and after the active phase of the tight regulatory policy against the background of the annual GDP growth rates in Russia. The active phase of the policy is marked with two vertical green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



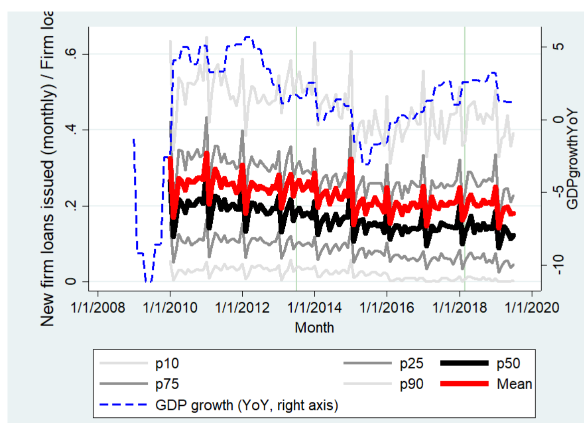
(a) Loan loss reserves



(b) NPLs of firms

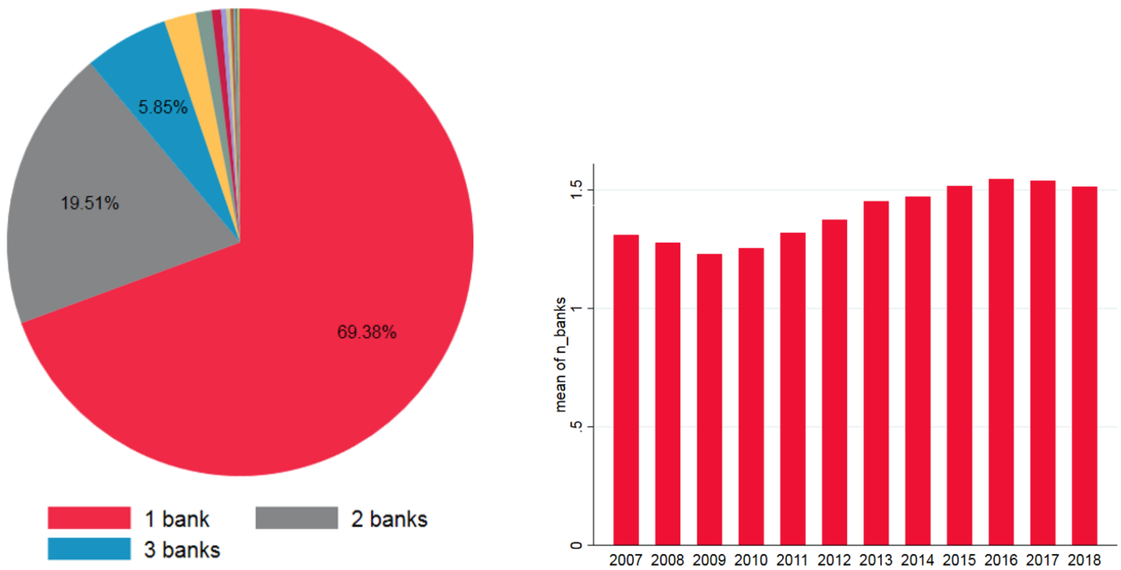


(c) Loans to firms



(d) New loans issued

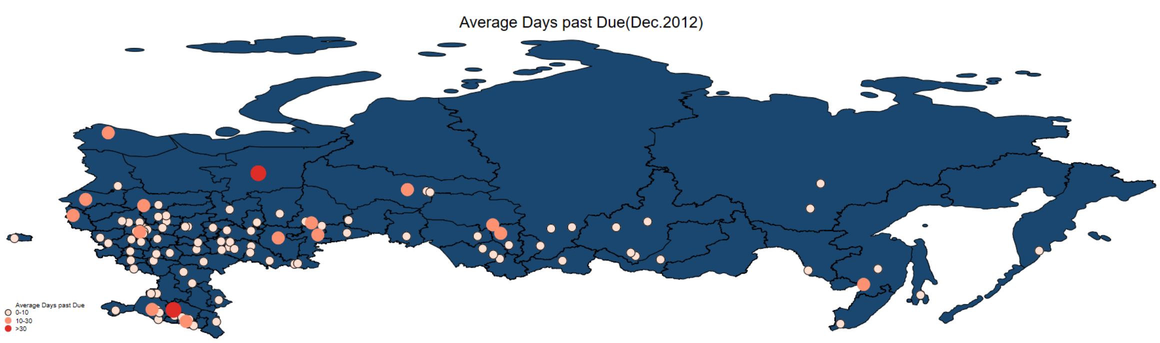
Figure 4. Bank–firm relationships



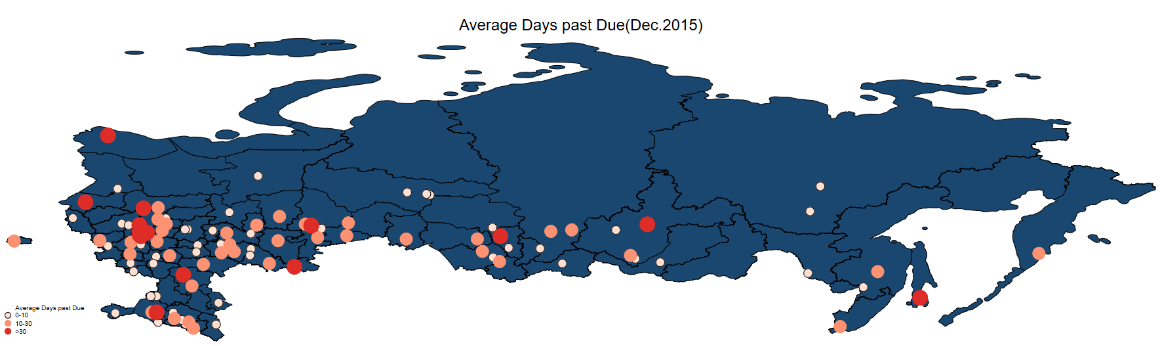
(a) Distribution of bank-firmrelationships (as of 2017)

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

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

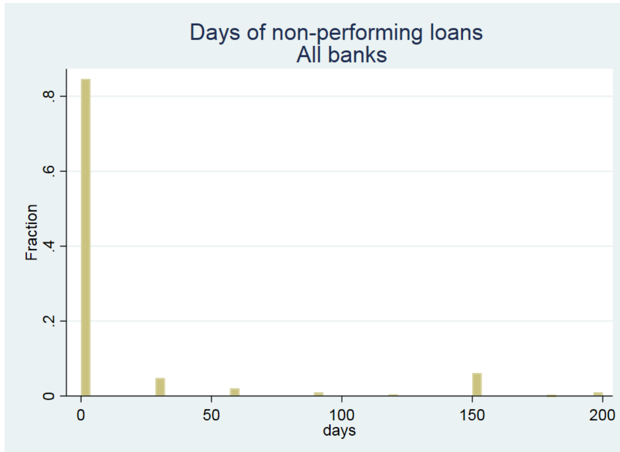


(a) Before the regulatory shock (as of December 2012)

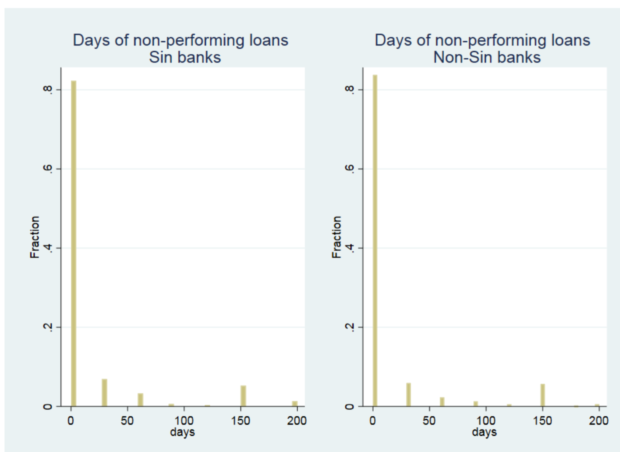


(b) After the regulatory shock (as of December 2015)

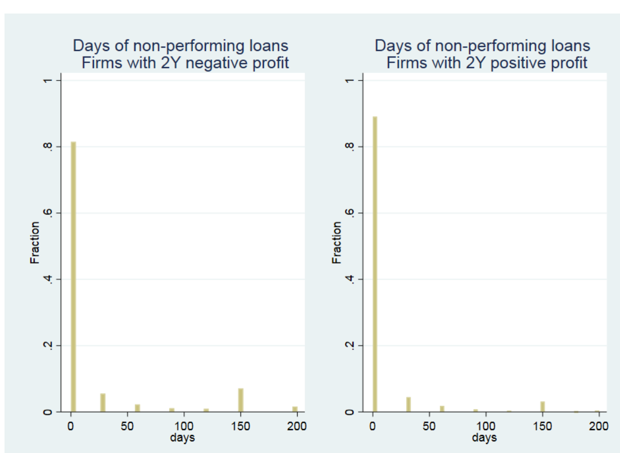
Figure 6. Frequency of the days of NPLs reported in the Bureau of Credit Histories (BCH), by "bank–firm" matches



(a) Full sample



(b) 'Sin' versus 'non-sin' 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.

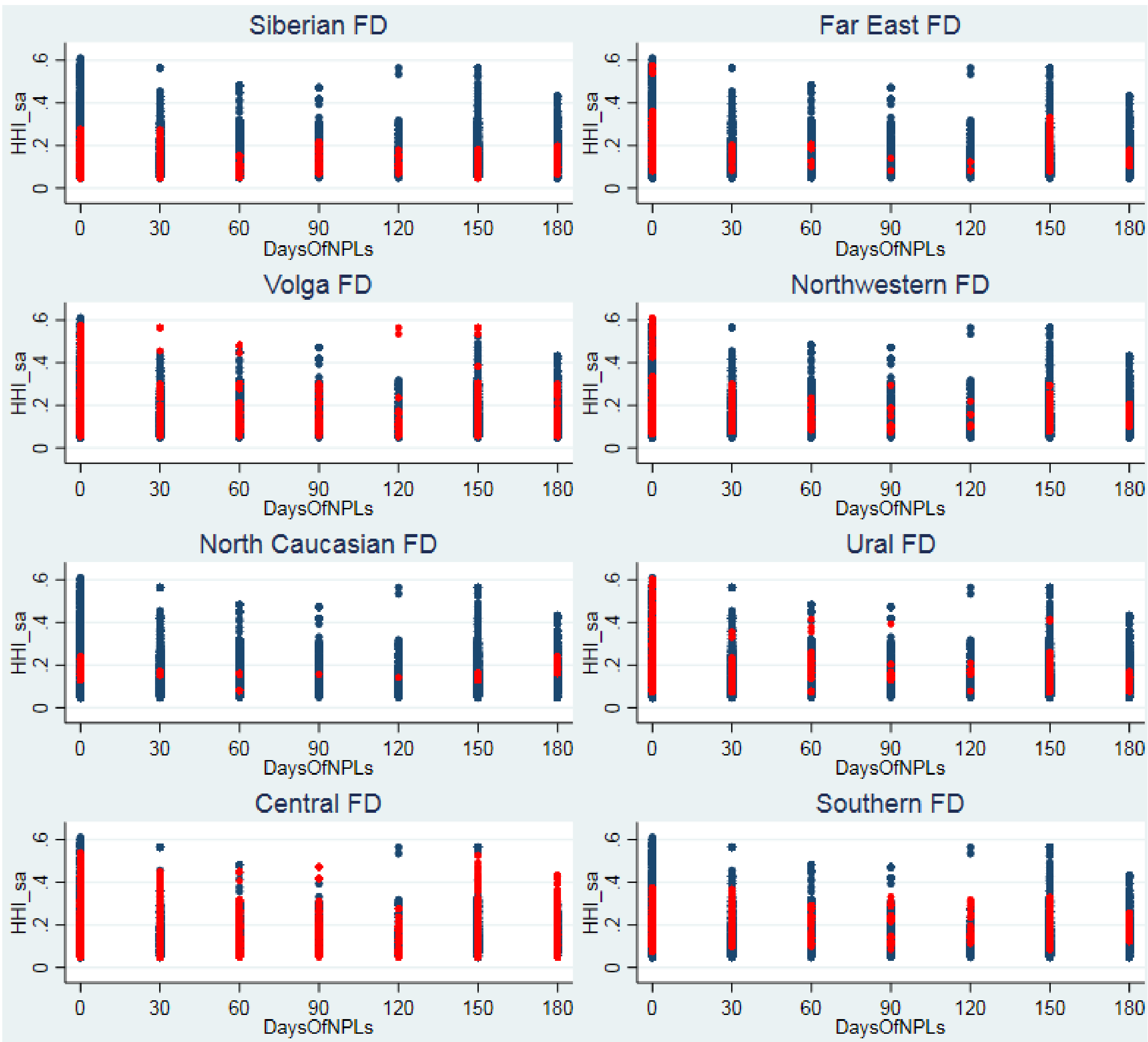


Figure 8. Time it takes for establishing new firm-bank matches after closure of fraudulent banks

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).

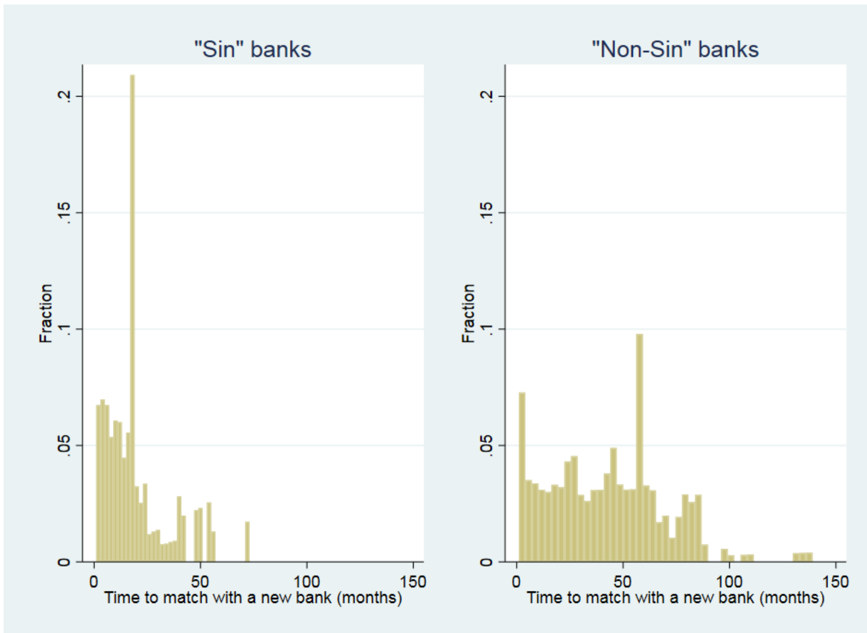


Figure 9. Time it takes for establishing new firm-bank matches after closure of fraudulent banks and the quality of loans in the closed banks

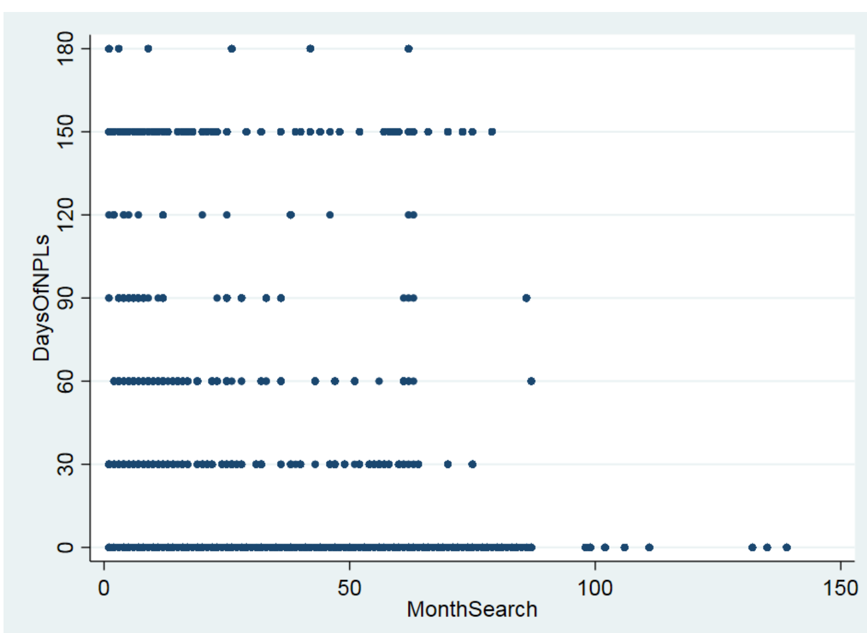


Figure 10. Loan quality at the firm-bank-month level: 1 (best) to 5 (worst) categories

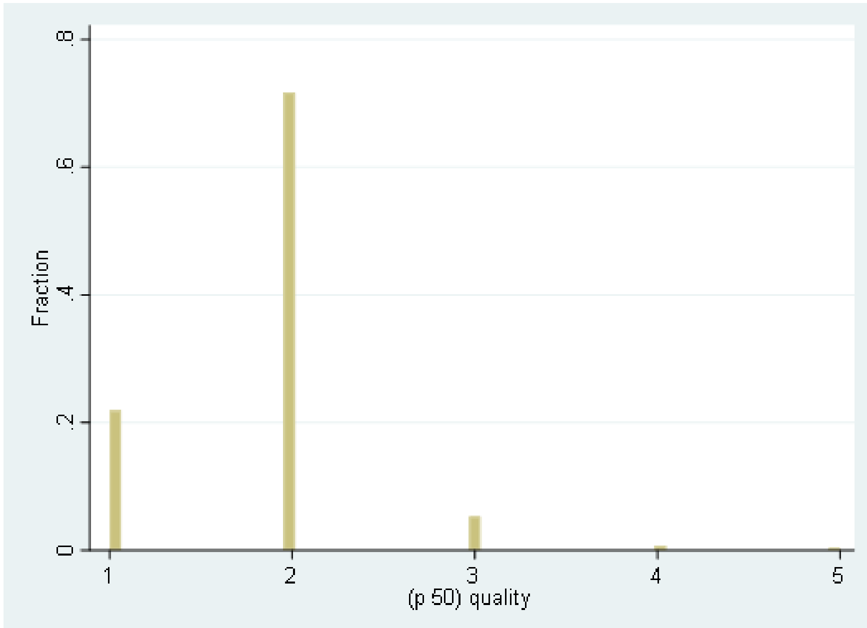
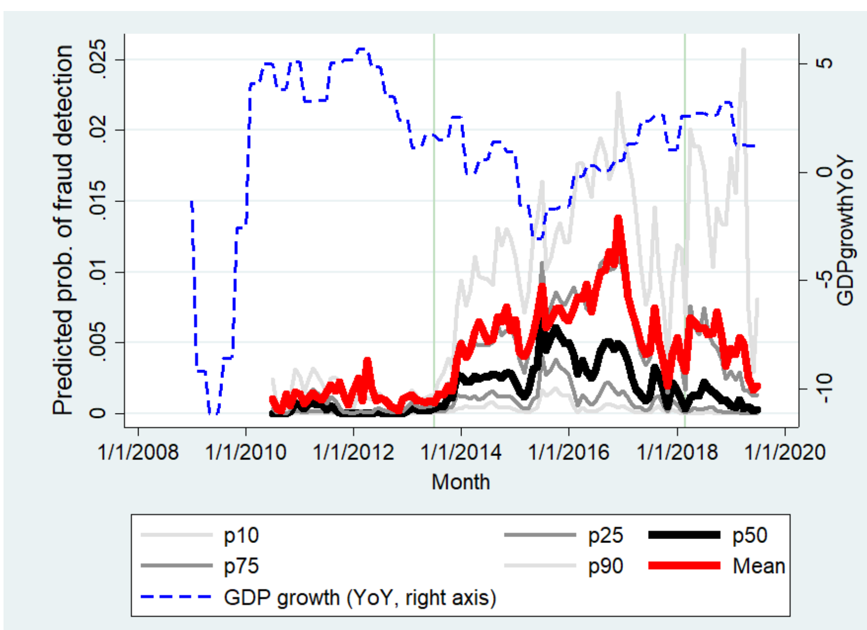


Figure 11. Time evolution of selected bank variables before and during the active phase of the tight regulation policy (Jul.2013–Feb.2018)

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.



TABLES

Table 1. Descriptive statistics: firms matching with new banks following their sin banks closure

	Mean	Median	SD	Min	Max
Panel 1: Firms matching with new *saint* banks:					
Match with saint vs never match	0.25	0.00	0.43	0.00	1.00
Months in search	45.77	46.00	25.39	2.00	139.00
Days of NPLs in the failed sin bank	14.87	0.00	42.07	0.00	200.00
Whether had negative profit when the sin 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 *sin* banks:					
Match with sin vs never match	0.06	0.00	0.23	0.00	1.00
Months in search	17.86	13.00	14.34	1.00	73.00
Days of NPLs in the failed sin bank	15.73	0.00	41.75	0.00	200.00
Whether had negative profit when the sin 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 sin bank	14.19	0.00	39.52	0.00	200.00
Whether had negative profit when the sin 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 sin banks in each FD:									
0	85,14	91,58	78,24	92,41	69,69	82,51	84,71	82,78	84,66
30	6,54	1,53	7,67	1,93	7,25	2,68	4,9	4,19	5,12
60	1,55	1,54	3,61	0,75	2,35	1,54	1,96	3,6	2,12
90	1,14	0,02	1,91	0,18	0,03	0,49	0,91	1,99	1,02
120	0,44	0,62	0,82	0,10	0,03	0,37	0,36	0,85	0,44
150	3,98	4,27	6,61	4,09	5,9	11,47	6,24	5,94	5,56
≥180	1,21	0,44	1,14	0,54	14,75	0,94	0,92	0,65	1,06
Mean HHI	1 265,3	1 822,9	1 457,5	1 651,6	1 885,5	1 763,9	1 205,8	1 769,2	1 371,5
SD HHI	459,8	796,0	1 051,6	596,7	485,7	995,6	737,3	821,7	802,5

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

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (1). Dependent variable $\lambda\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, no matter sin or saint, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b}+k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

Firm.Quality$_{f,t}$:	*Days of NPLs at t^**		*Negative profit at t^**		*Negative profit at t^* and t^*+k*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Firm quality:						
log DNPL$_{f,t^*}$	–0.009	–0.024				
	(0.024)	(0.031)				
Profit$_{f,t^*} < 0$			–0.117	–0.240	0.046	–0.066
			(0.212)	(0.253)	(0.213)	(0.252)
Profit$_{f,t^*+k} < 0$					–0.391***	–0.403***
					(0.129)	(0.136)
Panel 2: Other controls:						
Firm size$_{f,t-1}$	1.600***	1.590***	2.053***	2.062***	2.071***	2.096***
	(0.261)	(0.290)	(0.304)	(0.335)	(0.306)	(0.338)
Firm size$^2_{f,t-1}$	–0.043***	–0.042***	–0.055***	–0.056***	–0.056***	–0.057***
	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)
Leverage$_{f,t-1}$	–0.271**	–0.342**	–0.479***	–0.597***	–0.482***	–0.607***
	(0.118)	(0.140)	(0.141)	(0.174)	(0.144)	(0.179)
Liquidity$_{f,t-1}$	–0.061	–0.088	–0.132	–0.142	–0.166	–0.188
	(0.105)	(0.123)	(0.121)	(0.143)	(0.124)	(0.147)
Bank closure event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs		Yes		Yes		Yes
N obs	262,648	262,648	182,197	182,197	182,120	182,120
N firm-bank new matches	915	915	705	705	705	705
N firms	6,249	6,249	4,280	4,280	4,277	4,277
log L	–4,015.3	–3,680.6	–3,096.5	–2,791.0	–3,091.4	–2,785.8

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

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

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (2). Dependent variable $\lambda_j(t, \mathbf{X}_{f,t-1}; \Theta)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

	Match with a sin bank			Match with a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Firm quality:						
log DNPL$_{f,t^*}$	0.155***			–0.091**		
	(0.058)			(0.037)		
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

***, **, * 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: Common bank group owners

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (2) and conditional on new bank is not sharing the same owners / governors with the closed sin bank. Dependent variable $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin (j = 1) or saint (j = 2) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b}+k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

	Switch to a sin bank w/out common owners			Switch to a saint bank w/out common owners		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL_{f,t^*}	0.082			–0.125***		
	(0.082)			(0.046)		
$\text{Profit}_{f,t^*} < 0$		–0.886	–0.589		0.341	0.512
		(0.912)	(0.902)		(0.305)	(0.314)
$\text{Profit}_{f,t^*+k} < 0$			–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

***, **, * 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

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (2) and conditional on the sin bank closure being hardly predictable ('surprise') or not ('not a surprise'). Dependent variable $\lambda_j(t, \mathbf{X}_{f,t-1}; \Theta)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. *Surprise* indicates that the estimations are performed on the subsample of only those banks for which predicted probability of fraud detection is *below* the unconditional threshold of 0.5% monthly (or 6% annually). *Not a surprise*, on contrary, means *above* the threshold. Details on modeling the probability of fraud detection are in Appendix F. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

Previous sin bank closure:	*Surprise*		*Not a surprise*	
Match with a new bank:	sin bank	saint bank	sin bank	saint bank
	(1)	(2)	(3)	(4)
Panel 1: Firm quality: Days of NPLs				
log DNPL$_{f,t^*}$	0.204***	–0.118***	–0.072	–0.019
	(0.065)	(0.044)	(0.143)	(0.069)
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

***, **, * 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

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (3). Dependent variable $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin (j = 1) or saint (j = 2) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b}+k$. $HHI.credit_{r,t}$ is the Herfindahl-Hirschman index of regional credit market concentration. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

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

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

Table 8. Difference-in-differences estimation results: firm performance after 'sin' bank closures

Note: The table reports the estimates of firm performance after firms face their 'sin' banks closure and before they match with new banks, as implied by equation (4). Firm performance is proxied with the following dependent variables $Y_{f,t}$: firm size, as captured by the log of total assets (*log(TA)*, column 1), the ratio of borrowed funds to total assets (*Borrow/TA*, column 2), revenue to total assets (*Revenue/TA*, column 3), number of workers to total revenue ratio (*Employ/Revenue*, column 4), profit after taxes to total assets (*Profit/TA*, column 5), a binary indicator of whether a firm f defaults at year t (*Default=1*, column 6). $Sin.Bank_{b,f}$ = 1 if bank b that has relationship with firm f ever fails for fraud, and 0 if survives till the end of the sample. $POST_{\{t \geq t^*_{b,f}\}}$ = 1 if $t \geq t^*_{b,f}$, and 0 if else. $Bad.Firm_{f,t}$ is a binary variable that equals 1 for firms with losses, and 0 for profitable firms. The estimations are performed for $t \in [2011, 2020]$ on a panel of matched firms that ever faced sin bank closures, and the panel is restricted so that it includes the observations in only up to two years before and after $t^*_{b,f}$, i.e., firm-time-varying windows $[t^*_{b,f} - 2, t^*_{b,f} + 2]$ years. 1:4 nearest neighborhood matching of firms is performed prior to $t^*_{b,f}$ using the five observables: firm size, leverage, liquidity, annual growth of total assets, and profitability. All regressions contain all necessary sub-products of the triple interaction variable $Sin.Bank_{b,f}$ × $\text{POST}_{\{t \geq t^*_{b,f}\}}$ × $Bad.Firm_{f,t}$, firm and year fixed effects, and the set of firm controls to capture any residual differences across treated and control firms after 1:4 matching (firm size, except (1); leverage, except (2); and liquidity). The sample includes those firms that have *single* bank relationship.

$Y_{f,t}$:=	log(TA)	$\frac{Borrow}{TA}$	$\frac{Revenue}{TA}$	$\frac{Employ}{Revenue}$	$\frac{Profit}{TA}$	Default
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Focus variables:						
$\text{Sin.Bank}_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}}$	0.205***	0.011	0.384***	–4.408*	0.006	–2.566*
	(0.043)	(0.018)	(0.140)	(2.365)	(0.017)	(1.468)
$\text{Sin.Bank}_{b,f} \times \text{POST}_{\{t \geq t^*_{b,f}\}} \times \text{Bad.Firm}_{f,t}$	–0.320**	0.101*	–0.770**	10.553**	–0.017	n/a
	(0.136)	(0.060)	(0.325)	(4.239)	(0.030)	
Panel 2: Key components of the triple interaction variable:						
$\text{Sin.Bank}_{b,f}$	–0.091**	–0.009	–0.210**	1.994**	0.000	2.356***
	(0.040)	(0.011)	(0.101)	(0.921)	(0.014)	(0.319)
$\text{POST}_{\{t \geq t^*_{b,f}\}}$	0.082**	–0.030	–0.184	4.940*	–0.028	0.595
	(0.037)	(0.025)	(0.162)	(2.852)	(0.018)	(1.500)
$\text{Bad.Firm}_{f,t}$	–0.008	0.085***	–0.316***	8.703**	–0.180***	0.607
	(0.029)	(0.031)	(0.120)	(3.599)	(0.014)	(0.717)
N obs	17,174	18,861	17,835	11,683	18,613	10,745
N firms	3,226	3,261	3,234	2,869	3,258	3,237
R^2 (pseudo / LSDV)	0.3	0.7	0.1	0.0	0.1	0.1

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

Table 9. Interest rates and amount of loans in sin banks: regression estimation results

Note: The table reports the estimates of the following loan-level regressions: $y_{f,b,t} = \sum_{j=1}^{5} \beta_j \cdot \left(Sin.Bank_{b,f} \times Credit.Risk^{(j)}_{f,b,t} \right) + \gamma Sin.Bank_{b,f} + Loan.Control'_{f,b,t}\Xi + Bank.Control'_{b,t}\Psi + Macro.Control'_t\Phi + \alpha_f + \alpha_{f,t} + \epsilon_{f,b,t}$, where $y_{f,b,t}$ is loan interest rate (columns 1–2) or log of the loan amount (columns 3–4) at the firm-bank-month level, $t \in$ 2017M1–2020M9. $Sin.Bank_{b,f}$ is an indicator variable of sin bank, i.e., a bank that ever faced license revocation due to fraud detection. $Credit.Risk_{f,b,t}$ is a categorical variable ranging from 1 (the lowest risk, or the best quality, reference) to 5 (the highest risk, or the worst quality) to reflect a bank's ex-ante assessment of the loan credit risk. $Loan.Control_{f,b,t}$ includes loan quality, log of loan amount (columns 1–2), interest rate on loan (columns 3–4), maturity of loan, loan type (credit lines, overdraft, etc.). $Bank.Control_{b,t}$ includes the structure of bank assets (loans to firms, loans to households), the structure of bank liabilities (equity capital, deposits of firms, households, and government), all as % of bank total assets, bank size (log of total assets), and the ex-post quality of bank loans (NPL ratio), which are not reported to preserve space. $Macro.Control_t$ is GDP growth rate (YoY) and regional credit market concentration, as proxied with $HHI_{r,t}$. α_f is firm fixed effects and $\alpha_{f,t}$ is firm*month fixed effect (capturing firm demand on loans). *All loans* means each and every loan from the credit register is included in the regression ($\alpha_{f,t}$ is not included), whereas *Multiple loans* involves a subsample of those firms that demand credit at least twice per the time period considered ($\alpha_{f,t}$ is included). *n/a* means the effect is absorbed by (month) fixed effects. The sample includes those firms that have *single* bank relationship.

	Interest rate on loan, $Interest.Rate_{f,b,t}$		log of loan amount, log $Loan_{f,b,t}$	
	All loans	Multiple loans	All loans	Multiple loans
	(1)	(2)	(3)	(4)
$\text{Sin.Bank}_{b,f}$	1.579***	1.519***	–0.104***	–0.270***
	(0.091)	(0.181)	(0.037)	(0.084)
$\text{Credit.Risk}_{f,b,t} = 1$ (*reference*)				
$\text{Credit.Risk}_{f,b,t} = 2$	0.029**	0.191***	–0.060***	–0.075***
	(0.012)	(0.023)	(0.006)	(0.015)
$\text{Credit.Risk}_{f,b,t} = 3$	0.593***	1.228***	–0.045***	–0.024
	(0.029)	(0.080)	(0.012)	(0.034)
$\text{Credit.Risk}_{f,b,t} = 4$	0.045	0.594***	–0.282***	–0.217**
	(0.036)	(0.127)	(0.026)	(0.105)
$\text{Credit.Risk}_{f,b,t} = 5$	–0.052	0.713***	–0.040	0.235*
	(0.069)	(0.257)	(0.045)	(0.135)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 2$	–0.502***	–0.786***	0.095***	0.259***
	(0.083)	(0.189)	(0.035)	(0.088)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 3$	–1.036***	–1.648***	0.104**	0.680***
	(0.116)	(0.307)	(0.050)	(0.154)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 4$	–0.718***	–0.194	0.310***	1.345***
	(0.149)	(0.826)	(0.092)	(0.484)
$\text{Sin.Bank}_{b,f} \times \text{Credit.Risk}_{f,b,t} = 5$	–0.750***	–0.930	–0.222	0.271
	(0.259)	(0.688)	(0.223)	(0.232)
log $\text{Loan}_{f,b,t}$	–0.056***	–0.032***		
	(0.002)	(0.005)		
$\text{Interest.Rate}_{f,b,t}$			–0.031***	–0.022***
			(0.001)	(0.004)
GDP.Growth_t	–0.229***	n/a	0.005***	n/a
	(0.002)		(0.001)	
$\text{HHI.Credit}_{r,t}$	–0.001***	n/a	–0.000***	n/a
	(0.000)		(0.000)	
Firm FEs	Yes	Yes	Yes	Yes
Firm × month FEs	No	Yes	No	Yes
Obs	1,774,379	679,356	1,774,379	679,356
R^2 (adj.)	0.9	0.8	0.7	0.6

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

Table 10. Loan quality in sin and saint banks: regression estimation results

Note: The table reports the estimates of the following loan-level regressions: $Credit.Risk_{f,b,t} = \beta \cdot \Big(Sin.Bank_{b,f} \times Bad.Firm_{f,t}\Big) + \gamma Sin.Bank_{b,f} + \delta Bad.Firm_{f,t} + Loan.Control'_{f,b,t}\Xi + Bank.Control'_{b,t}\Psi + Macro.Control'_{t}\Phi + \alpha_f + \alpha_{f,t} + \epsilon_{f,b,t}$, where $Credit.Risk_{f,b,t}$ is a categorical variable ranging from 1 (the lowest risk, or the best quality, reference) to 5 (the highest risk, or the worst quality) to reflect a bank's ex-ante assessment of the loan credit risk, $t \in$ 2017M1–2020M9. $Sin.Bank_{b,f}$ is an indicator variable of sin bank, i.e., a bank that ever faced license revocation due to fraud detection. $Bad.Firm_{f,t}$ is a binary variable that equals 1 for firms with losses, and 0 for profitable firms. $Loan.Control_{f,b,t}$ includes log of loan amount, maturity of loan, loan type (credit lines, overdraft, etc.). $Bank.Control_{b,t}$ includes the structure of bank assets (loans to firms, loans to households), the structure of bank liabilities (equity capital, deposits of firms, households, and government), all as % of bank total assets, bank size (log of total assets), and the ex-post quality of bank loans (NPL ratio), which are not reported to preserve space. $Macro.Control_t$ is GDP growth rate (YoY) and regional credit market concentration, as proxied with $HHI_{r,t}$. α_f is firm fixed effects and $\alpha_{f,t}$ is firm*month fixed effect (capturing firm demand on loans). *All loans* means each and every loan from the credit register is included in the regression ($\alpha_{f,t}$ is not included), whereas *Multiple loans* involves a subsample of those firms that demand credit at least twice per the time period considered ($\alpha_{f,t}$ is included). *n/a* means the effect is absorbed by (month) fixed effects. The sample includes those firms that have *single* bank relationship.

$Y_{f,b,t}$:=	Loan quality	
	All loans	Multiple loans
	(1)	(2)
Sin.Bank$_{b,f}$	–0.073***	–0.062***
	(0.011)	(0.019)
Bad.Firm$_{f,t}$	0.026***	0.003
	(0.003)	(0.007)
Sin.Bank$_{b,f}$ × Bad.Firm$_{f,t}$	–0.047**	–0.192***
	(0.021)	(0.055)
log Loan$_{f,b,t}$	–0.003***	–0.002***
	(0.000)	(0.001)
GDP.Growth$_t$	0.007***	n/a
	(0.000)	
HHI.Credit$_{r,t}$	0.000***	n/a
	(0.000)	
Firm FEs	Yes	Yes
Firm × month FEs	No	Yes
Obs	1,263,970	679,904
R^2 (adj.)	0.7	0.8

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

Internet Appendix

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Quo Vadis? Evidence on New Firm-Bank Matching and Firm Performance Following "Sin" Bank Closures

Roman Goncharenko, Mikhail Mamonov, Steven Ongena, Svetlana Popova, and Natalia Turdyeva

APPENDIX

Appendix A. DESCRIPTION OF THE DATA

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

Name	Definition	Source
Survival regression analysis		
Size	$\ln(\text{Total assets})$	Balance sheet
Leverage	$\frac{\text{Short-term liabilities} + \text{Long-term liabilities}}{\text{Total assets}}$	Balance sheet
Liquidity	$\frac{\text{Current liabilities} - (\text{Accounts payable} + \text{Short-term loans})}{\text{Total assets}}$	Balance sheet
Profit	Gross profit	Income statement
Difference-in-difference analysis		
Default	= 1 if firm is bankrupt at t	Register of Legal Entities
Employ	$\frac{\text{Number of workers}}{\text{Sales}}$	Balance sheet
Revenue	$\frac{\text{Sales}}{\text{Total assets}}$	Income statement, Balance Sheet
Profit	Gross profit	Income statement
Borrowed funds	Short-term liabilities+Long-term liabilities	Income statement
Total assets	Sum of all assets	Balance Sheet

Appendix B. THREE-OUTCOMES BANK-FIRM MATCHING MODEL

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

Note: The table reports the estimates of a multinomial logit model of new firm-bank matching following the firms' f current sin banks closure, as an analog to the duration regression (2). Differently from equation (2) that splits the match option to 'match with sin bank' or 'match to saint bank', conditional on surviving till month t, the multinomial regression here assembles all the three outcomes: never match, match with sin or saint banks ($\jmath = 0, 1, 2$): $\mathbf{Pr}(Match_{f,t} = \jmath \,|\, \mathbf{X}_{f,t-1}; \Theta) = \Lambda\Big(\alpha_\jmath + \alpha_{\jmath,bc} + \alpha_{\jmath,r} + \alpha_{\jmath,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_\jmath + \mathbf{C}_{f,t-1}\Gamma_\jmath\Big)$, where the dependent variable $Match_{f,t}$ is a categorical variable that equals zero if a firm that 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 *sin* bank (columns 1–3), 2 if with a *saint* bank (columns 4–6). Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of marginal effects are reported. Constant is included but not reported to preserve space.

	Switch to a sin bank			Switch to a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL$_{f,t^*}$	0.126***			–0.063**		
	(0.041)			(0.029)		
Profit$_{f,t^*}$ < 0		–1.278*	–1.029		0.104	0.245
		(0.722)	(0.711)		(0.211)	(0.210)
Profit$_{f,t^*+k}$ < 0			–0.568**			–0.328**
			(0.268)			(0.143)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank closure event FEs	No	No	No	No	No	No
Regional FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N obs	263,502	183,166	183,088	263,502	183,166	183,088
N firm-bank new matches	200	168	168	715	537	537
N firms	6,921	4,770	4,767	6,253	4,327	4,324
log L	–6,428	–4,879	–4,874	–6,428	–4,879	–4,874

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

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

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (2), with GDP growth rates (YoY) and concentration at regional credit markets (HHI) being included as additional controls: $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right) = \lambda_0\left(t\right) \cdot \exp\Big(\alpha_j + \alpha_{j,bc} + \alpha_{j,r} + \alpha_{j,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_j + \mathbf{C}_{f,t-1}\Gamma_j + \delta_{j,1}GDP.growth_{t-1} + \delta_{j,2}HHI.credit_{r,t-1}\Big)$, where the dependent variable $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin (j = 1) or saint (j = 2) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

	Switch to a sin bank			Switch to a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL_{f,t^*}	0.156***			–0.085**		
	(0.058)			(0.036)		
$\text{Profit}_{f,t^*} < 0$		–1.729*	–1.469*		0.021	0.182
		(0.893)	(0.882)		(0.245)	(0.246)
$\text{Profit}_{f,t^*+k} < 0$			–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)
$\text{HHI.credit}_{r,t-1}$	1.187	–0.244	–0.245	4.900***	3.865***	3.935***
	(1.420)	(2.030)	(2.071)	(0.574)	(0.707)	(0.713)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N obs	255,152	177,121	177,046	255,643	177,507	177,432
N firm-bank new matches	200	168	168	715	537	537
N firms	6,034	4,178	4,175	6,045	4,183	4,180
log L	–1,065.0	–853.4	–851.5	–2,876.6	–2,149.4	–2,145.6

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

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

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

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (2). Dependent variable $\lambda_j\left(t, \mathbf{X}_{f,t-1}; \Theta\right)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin (j = 1) or saint (j = 2) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *multiple* bank relationships. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

	Switch to a sin bank			Switch to a saint bank		
	(1)	(2)	(3)	(4)	(5)	(6)
log DNPL$_{f,t^*}$	–0.013			–0.033		
	(0.068)			(0.039)		
Profit$_{f,t^*}$ < 0		–0.816	–0.558		–0.771*	–0.576
		(0.727)	(0.759)		(0.428)	(0.423)
Profit$_{f,t^*+k}$ < 0			–0.453			–0.344**
			(0.307)			(0.166)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank closure event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N obs	235,231	160,843	160,802	235,562	161,124	161,082
N firm-bank new matches	171	142	142	502	423	422
N firms	5,368	3,671	3668	5,405	3,704	3,701
log L	–928.9	–722.1	–720.8	–2,259.4	–1,814.7	–1,808.1

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

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

Note: The table reports the estimates of new firm-bank matching following the firms' f current sin banks closure, as implied by equation (2). Dependent variable $\lambda_j(t, \mathbf{X}_{f,t-1}; \Theta)$ is an instantaneous rate at which firms "exit," i.e., match with new banks, sin ($j = 1$) or saint ($j = 2$) vis-a-vis never match, conditional on survival to the current month t. Firm quality is proxied by the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$. The days of NPLs variable was categorized into 7 30-days bins: $0 \leq DNPL_{f,t-1} \leq 30$ (*reference*), $30 < DNPL_{f,t-1} \leq 60$, ..., $DNPL_{f,t-1} > 180$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of subhazard ratios are reported. Constant is included but not reported to preserve space.

	Switch to a sin bank	Switch to a saint bank
	(1)	(2)
Bin 1: $0 < DNPL_{f,t-1} \leq 30$ (*reference)*		
Bin 2: $30 < DNPL_{f,t-1} \leq 60$	0.779***	–0.377*
	(0.302)	(0.212)
Bin 3: $60 < DNPL_{f,t-1} \leq 90$	1.425***	0.053
	(0.421)	(0.286)
Bin 4: $90 < DNPL_{f,t-1} \leq 120$	0.016	–0.616
	(0.991)	(0.502)
Bin 5: $120 < DNPL_{f,t-1} \leq 150$	0.193	–0.653**
	(0.483)	(0.287)
Bin 6: $150 < DNPL_{f,t-1} \leq 180$	–0.910	–17.399***
	(1.086)	(1.431)
Bin 7: $DNPL_{f,t-1} > 180$	–16.140***	–0.721
	(0.770)	(1.090)
N obs	257,190	257,681
N firms	6,069	6,080
N firm-to-bank switches	200	715
log L	–1,060.1	–2,918.9

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

Appendix F. IN-ADVANCE DETECTION OF SIN 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).[33] The Bank of Russia itself was, and is, constantly learning these tools when revoking 'sin' 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.[34] 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

[33] 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).

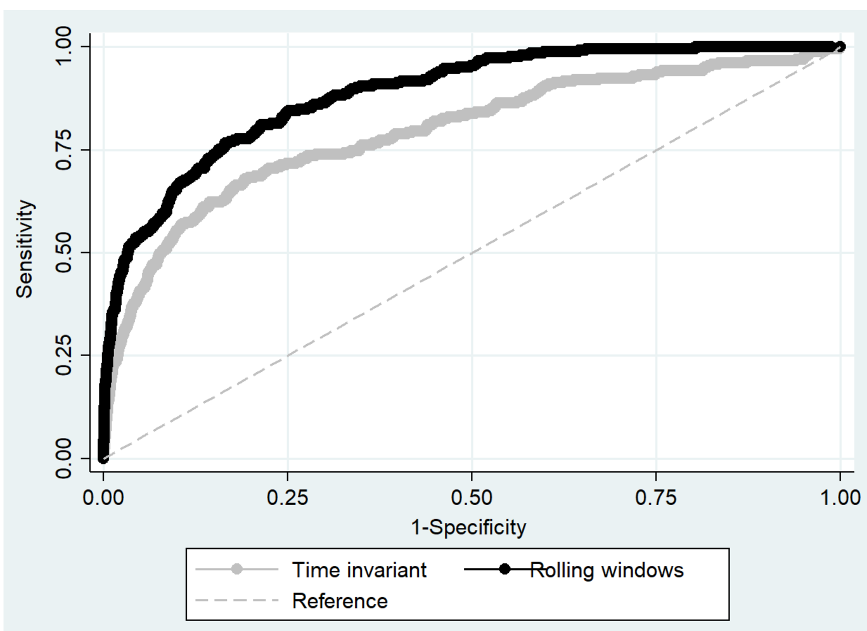
[34] We also tested 12-months windows and found no qualitative changes compared to the baseline.

down at month t by the regulator due to falsifications revealed during the on-site inspections.[35] 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 and with fraudulent indicators



[35]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 sin banks detection and closure: logit regression results

Note: The table reports the estimates of the following logit model: $Pr\left(Fraud.Detection_{b,t} = 1 \mid \mathbf{X}_{b,t-1}\right) = \Lambda\Big(\mathbf{X}'_{b,t-1}\Psi$, where the dependent variable $Pr\left(Fraud.Detection_{b,t} = 1 \mid \mathbf{X}_{b,t-1}\right)$ is a binary variable that equals 1 if an operating bank b is closed for fraud revealed by the Central Bank of Russia at month t, and 0 if the bank continues. $\mathbf{X}_{b,t-1}$ includes capital adequacy ratio (CAR), the NPLs ratios in the credit to households and credit to firms, return-to-assets (ROA), cash and reserves at the corresponding accounts at the Central Bank of Russia to total assets ratio (liquidity), growth of total assets (YoY) and its square, inter-bank loans minus inter-bank debts to total assets ratio, foreign assets minus foreign liabilities to total assets ratio, log of total assets, a censored variable equals loan loss reserves (LLR) if LLR exceeds median across all banks at a given month and equals 0 if else, the product of the censored variable and NPLs of firms, distance of bank's headquarter location to Moscow, regional credit market concentration (HHI), and GDP growth rates (YoY). The estimations are performed using 6-month rolling windows starting from 2010M1, i.e., before the active phase of the tight regulation policy began, and finishes at the end of the sample period in 2019M6. Constant is not reported.

Period:	Before the policy	During the active phase of the policy		
		≤2013M7	≤2016M1	≤2018M2
	(1)	(2)	(3)	(4)
CAR	–0.003	0.003	–0.002	–0.021**
	(0.018)	(0.018)	(0.008)	(0.010)
NPLs households	–2.660	24.488***	–1.337	–4.167
	(11.869)	(8.027)	(6.085)	(4.414)
NPLs firms	5.943	–22.104	9.264	8.187**
	(4.146)	(104.406)	(7.044)	(3.382)
ROA	–7.664***	–35.742***	–8.069***	–10.415***
	(2.053)	(9.724)	(2.981)	(1.852)
Liquidity	–1.376	3.422	–1.375	–2.863*
	(1.681)	(5.235)	(1.475)	(1.490)
Growth of total assets	–0.946	–0.664	–1.053	–0.575
	(0.775)	(3.559)	(0.666)	(0.490)
Growth of total assets ^{2}	0.545*	0.448	0.467*	0.348*
	(0.295)	(1.311)	(0.252)	(0.185)
Net inter-bank loans	–3.342***	3.878	–3.632***	–3.852***
	(0.845)	(3.695)	(1.399)	(0.848)
Net Foreign assets	0.165	5.464**	1.040	0.038
	(1.077)	(2.402)	(1.124)	(0.865)
Bank size	–0.614**	–0.049	–0.416***	–0.525***
	(0.294)	(0.413)	(0.122)	(0.098)
LLR > 50%*tile*	7.367***	–3.977	5.654***	6.497***
	(1.781)	(7.210)	(1.393)	(0.910)
LLR > 50%*tile* × NPLs firms	–22.147	–66.891	–63.950**	–53.920***
	(16.286)	(476.620)	(27.815)	(16.016)
Distance to Moscow	0.000	–0.000	–0.000	–0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Regional HHI		0.001	–0.000	–0.000*
		(0.000)	(0.000)	(0.000)
Annual GDP growth	0.083	–1.038	–0.158**	–0.143***
	(0.110)	(0.682)	(0.077)	(0.055)
N obs	37,889	1,550	19,568	31,836
R^2-pseudo	0.117	0.274	0.080	0.120

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

Appendix G. DID FIRMS ANTICIPATE THE CLOSURE OF THEIR SIN BANKS?

In this section, we examine whether firms anticipated the closure of their sin banks. We consider two possible endogenous adjustments by firms as evidence of such anticipation. First, we conjecture that firms could preemptively leave sin banks in anticipation of their closure. Second, we conjecture that firms, especially the low-quality ones, could delay the loan repayments in anticipation of sin bank closure.

G.1 Preemptive Switching

There are at least three reasons why firms could consider leaving their about-to-fail sin bank preemptively. First, the firm of an about-to-fail bank may leave the bank preemptively to *signal* to other banks that it seeks for long-term stable relationships with its lender(s). Second, if the firm does not switch to a different bank in advance, its payment obligation can be transferred to a new bank through an auction during the resolution process of the sin bank, in which case the firm has no control over what this new bank can be (Granja et al., 2017). Third, the closure of the firm's bank can have a disruptive effect on the firm's day-to-day operations.

On the other hand, even if the firms of an about-to-fail sin bank wanted to preemptively switch to a new bank does not mean they would do it. The empirical literature provides evidence on the existence of switching cost–the cost firms incur when deciding to switch to a new bank–which leads to a hold-up problem–a situation in which the firm stays with its old bank despite being able to obtain better conditions at another one.[36]

We are interested in whether firms' quality determines preemptive switching to a new bank. One could expect that because of higher outside options the better quality firms face are less subject to the hold-up problem and, thus, are more likely to switch to a new bank preemptively. Likewise, the lower quality firms are likely to be more constrained by the hold-up problem and, thus, are less likely to leave the about-to-fail bank preemptively.

To find whether firms' quality help to explain preemptive switching, we examine if firms switch to a new bank within some time period h before the sin bank closure date. That is, we define the indicator variable $Switch_{f,t}$ which equals 1 if firm f switches to a new bank during the time interval $[t^*_{f,b} - h, t^*_{f,b})$, where $t^*_{f,b}$ is sin bank b closure date, and zero otherwise.[37] We set $h = 6$—that is, we consider a time interval of 6 months to identify evidence of preemptive switching. We then estimate the following logit model:

$$\mathbf{Pr}(Switch_{f,t} = 1 \,|\, \mathbf{X}_{f,t-1}; \Theta) = \Lambda\Big(\alpha_J + \alpha_{J,bc} + \alpha_{J,r} + \alpha_{J,i} + \text{Firm.Quality}_{f,t-1}\mathrm{B}_J + \mathbf{C}_{f,t-1}\Gamma_J\Big). \qquad (7)$$

[36] Ioannidou and Ongena (2010) provide evidence on the existence of switching costs. Bonfim et al. (2020) show that switching costs are primarily due to information asymmetries. Liaudinskas and Grigaite (2021) the quantitative estimated of switching cost and evidence on the hold-up problem.

[37] A firm may also decide to switch in advance from the about-to-fail bank *occasionally* merely because the firm's loan is happened to mature at some time $\tilde{t} \in [t^*_{f,b} - h, t^*_{f,b})$ and the firm is simply not willing to continue with the same bank. Unfortunately, with the data at hand—that is, with information only on the days of NPLs at the loan lever and no access to either maturity or other relevant information until 2017—we cannot distinguish these cases from the in-advance switching based on information leakages.

Table G.I. Logit regression results: do firms break switch to sin or saint banks in anticipation of their current banks closure?

Note: The table reports the estimates of the logit model (7) of new firm-bank matching *prior* to the firms' f current sin banks closure, as an analog to the duration regression (2) that considers the matching *after* the sin bank closure. Dependent variable $\mathbf{Pr}(Switch_{f,t} = 1 \,|\, \mathbf{X}_{f,t-1}; \Theta)$ is the indicator variable which equals 1 if firm f switches to a new bank during the time interval $[t^*_{f,b} - h, t^*_{f,b})$, where $t^*_{f,b}$ is sin bank b closure date, and zero otherwise. Firm quality is proxied by either (i) the log of days of NPLs accumulated in the closed sin bank before the closure–that is, by $t^*_{f,b}$–or (ii) the binary variables of whether the firm had negative profits at $t^*_{f,b}$ or $t^*_{f,b} + k$. Other controls include firm's size, as measured by the log of total assets and its square, the firm's leverage-to-total assets and liquidity-to-total assets ratios. The sample includes those firms that have *single* bank relationship. The estimations are performed for the period starting with the active phase of the tight regulation policy, i.e., from 2013M7, and till 2020M10. Coefficients instead of marginal effects are reported. Constant is included but not reported to preserve space.

	Match with a sin bank		Match with a saint bank	
	(1)	(2)	(3)	(4)
Panel 1: Firm quality:				
log DNPL_{f,t^*-6}	0.010		0.095	
	(0.080)		(0.131)	
$\mathrm{Profit}_{f,t^*-6} < 0$		0.362		0.035
		(0.267)		(0.069)
$\mathrm{Profit}_{f,t} < 0$		0.038		0.052
		(0.153)		(0.047)
Panel 2: Other controls:				
Firm $\mathrm{size}_{f,t-1}$	0.406	0.521	0.013	0.074
	(0.417)	(0.431)	(0.134)	(0.145)
Firm $\mathrm{size}^2_{f,t-1}$	–0.012	–0.015	0.000	–0.001
	(0.011)	(0.011)	(0.004)	(0.004)
$\mathrm{Leverage}_{f,t-1}$	–0.264	–0.293	–0.252***	–0.228***
	(0.194)	(0.203)	(0.066)	(0.068)
$\mathrm{Liquidity}_{f,t-1}$	–0.411**	–0.361**	–0.090	–0.049
	(0.166)	(0.172)	(0.059)	(0.061)
Bank closure event FEs	Yes	Yes	Yes	Yes
Regional FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
N obs	4,645	4,253	26,287	25,519
N firm-bank new matches	619	606	1,331	1,317
N firms	854	818	2,336	2,314
log L	–2,916	–2,676	–16,010	–15,557
R^2 (pseudo)	0.035	0.034	0.006	0.005

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

The estimation results are reported in Table G.I. Our sample now consists of only about 30,000 firm-month observations, which is less than in the reference by a factor of 10. We have about 3,190 firms and the number of preemptive switches is about 1,950. As can be inferred from the table, estimation of the preemptive switching regressions delivers no significant coefficients on the log $DNPL_{f,t^*-6}$ or $Profit_{f,t^*-6} < 0$ variables. This is true for switching to the new (not-yet-identified) sin banks in columns 1 and 2 and switching to saint banks regressions in columns 3 and 4. Moreover, the signs of the estimated coefficients are flipped compared to the baseline.

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 preemptively. The estimated coefficient on firm leverage is negative and significant in the case of in-advance switching to saint banks. Finally, liquidity negatively and significantly affects the likelihood of in-advance switching to sin banks.

Overall, the logit estimation results reveal that firms' preemptive switching from the about-to-fail banks is not affected by firms' quality. One potential interpretation of this is the lack of evidence that firms could easily anticipate bank closures. That is, the preemptive switching is more likely to take place for other common reasons (expiration/full repayment of loans, etc.). An alternative explanation is that firms could anticipate closures but the hold-up problem was strong enough even for good quality firms.

G.2 Strategic Loan Repayment Delay

An alternative way to examine whether firms anticipate sin bank closures is to investigate loan repayments during the run-up to sin bank closure. Troubled firms, which struggle more with meeting their loan obligations, may find it optimal to delay their payments if they anticipate that their bank is about to fail. In this case, they can be transferred to a new creditor, thus, opening up a possibility for debt restructuring.

Thus, we hypothesize that bad firms, as proxied with negative profits, may act strategically and thus raise loan delinquencies. Thus, we explore empirically if a change in the loan repayment delay relates negatively to the firm's quality proxy during some time period before the sin bank closure. Specifically, we estimate the following model:

$$\Delta DNPL_{f,b,t} = \alpha_f + \alpha_b + \alpha_{b,f} + \alpha_t + \alpha_r + \alpha_i + \alpha_{bc} + \beta \cdot \mathbf{1}\{Profit_{f,t^*-h} < 0\} + \varepsilon_{f,b,t}, \tag{8}$$

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 (not yet detected) sin bank b at month $t \in [t^* - h, t^*)$, $h = 12, 9, 6, 3$ months prior to the bank b closure. α_f, α_b, $\alpha_{b,f}$, α_t, α_r, α_i, α_{bc} are respectively FEs for firm, bank, firm*bank (relationship), month, region, industry, and bank closure events.

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 $bank \times firm$ relationships, aggregate shocks affecting the economy of the whole country or its particular regions, industry-specific shocks that may force even a profitable firm to delay repayment on loans, and the cascade of bank closures witnessed in the active phase of the tight policy.

The estimation results of regression (8) are presented in Panel 1 of Table G.II. We do not find any statistical evidence that the firm's quality relates to the change in the delay of loan repayments—that is, the estimated coefficient on $Profit_{f,t^*-h} < 0$ is insignificant at any considered horizon h prior to the bank closures. Thus, we do not find evidence that the bad firms raise loan delinquencies before the closures of their sin banks.

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

Note: The table reports the estimates of 1-month changes in the days of NPLs prior to sin bank closure, as implied by equation (8), where the dependent variable $\Delta DNPL_{f,b,t}$ is a one-month change in the days of NPLs a firm f has in bank b at month t. The estimations are performed in a window of h months before a sin bank closure, i.e., $t \in [t^*_{f,b} - h, t^*_{f,b})$, where $t^*_{f,b}$ is firm-specific date of breaking relationship with the firm's current sin bank and h is set at 6 months. $Profit_{t^*-h}$ is the binary variable of whether the firm had negative profits at $t^*_{f,b} - h$. *Single "firm–sin bank"* indicates those cases in which a firm has relationship only with one bank and this bank is a sin bank. *Multiple "firm–(sin) bank"* indicates those cases in which a firm has relationships with more than one bank and (at least) one of these banks is a sin bank. All regressions include bank, firm, firm*month, month, regional, industry, and bank closure events fixed effects.

Months h before sin bank closure:	h = 12	h = 9	h = 6	h = 3
	(1)	(2)	(3)	(4)
Panel 1: single "firm–sin bank" relationship (*baseline*)				
$\text{Profit}_{f,t^*-h} < 0$	1.063	0.761	1.197	–0.711
	(0.720)	(0.949)	(1.174)	(1.565)
N obs	78,645	62,519	44,749	24,768
R^2 (within)	0.091	0.111	0.143	0.255
Panel 2: multiple "firm–(sin) bank" relationship				
$\text{Profit}_{f,t^*-h} < 0$	0.219	0.414	0.937	0.379
	(0.450)	(0.786)	(0.591)	(0.500)
$\text{Sin.Bank}_b \times \text{Profit}_{f,t^*-h} < 0$	0.111	–0.644	–1.165	–1.589
	(0.791)	(1.200)	(1.367)	(1.955)
N obs	213,229	163,688	111,957	60,140
R^2 (within)	0.081	0.100	0.135	0.232

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

Panel 2 of Table G.II, further presents the result of estimation equation (8) when we allow for *multiple* firm-bank relationships.[38] The results are qualitatively similar in that we do not find evidence that the firm's quality affects loan delinquencies before the closures of their sin banks.

Overall, our results show little evidence that firms anticipated sin bank closures. The firms neither left their sin banks preemptively nor did they engage in strategic loan repayments delay.

[38] For this purpose, equation (8) is modified so that a firm may have relationships with at least one (not yet detected) sin bank and at least one saint bank simultaneously. In this case, the variable of interest is not just $Profit_{f,t^*-h} < 0$, but also its product with the sin bank indicator variable, $Sin.Bank_b$, which is equal to 1 if a bank ever fails due to fraud revealed, and 0 if survives till the end of the sample. For strategic reasons, firms are likely to hold the worst part of their debts in sin banks and serve their best-quality debts in saint banks. If firms anticipate sin bank closures, then bad firms could start to raise loan delinquencies in the sin banks rather than saint ones.

Appendix H. FIRM PERFORMANCE: ADDITIONAL RESULTS

Table H.I. Difference-in-differences estimation results: firm performance after 'sin' bank closures

Note: The table reports the estimates of firm performance after firms face their 'sin' banks closure and before they match with new banks, as implied by equation (4). Firm performance is proxied with the following dependent variables $Y_{f,t}$: firm size, as captured by the log of total assets (*log(TA)*, column 1), log of borrowed funds (*log(Borrow)*, column 2), log of total revenue (*log(Revenue)*, column 3), log of number of workers (*log(Employ)*, column 4), log of profit after taxes (*log(Profit)*, column 5). $Sin.Bank_{b,f} = 1$ if bank b that has relationship with firm f ever fails for fraud, and 0 if survives till the end of the sample. $POST_{\{t \geq t^*_{b,f}\}} = 1$ if $t \geq t^*_{b,f}$, and 0 if else. $Bad.Firm_{f,t}$ is a binary variable that equals 1 for firms with losses, and 0 for profitable firms. The estimations are performed for $t \in [2011, 2020]$ on a panel of matched firms that ever faced sin bank closures, and the panel is restricted so that it includes the observations in only up to two years before and after $t^*_{b,f}$, i.e., firm-time-varying windows $[t^*_{b,f} - 2, t^*_{b,f} + 2]$ years. 1:4 nearest neighborhood matching of firms is performed prior to $t^*_{b,f}$ using the five observables: firm size, leverage, liquidity, annual growth of total assets, and profitability. All regressions contain all necessary sub-products of the triple interaction variable $Sin.Bank_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times Bad.Firm_{f,t}$, firm and year fixed effects, and the set of firm controls to capture any residual differences across treated and control firms after 1:4 matching (firm size, except (1); leverage, except (2); and liquidity). The sample includes those firms that have *single* bank relationship.

$\mathrm{Y}_{f,t} :=$	log(TA)	log(Borrow)	log(Revenue)	log(Employ)	log(Profit)
	(1)	(2)	(3)	(4)	(5)
Panel 1: Focus variables:					
$\mathrm{Sin.Bank}_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}}$	0.205***	0.164***	0.342***	0.158**	0.267***
	(0.043)	(0.058)	(0.063)	(0.069)	(0.080)
$\mathrm{Sin.Bank}_{b,f} \times \mathrm{POST}_{\{t \geq t^*_{b,f}\}} \times \mathrm{Bad.Firm}_{f,t}$	–0.320**	–0.259**	–0.028	0.307	n/a
	(0.136)	(0.121)	(0.168)	(0.197)	
Panel 2: Key components of the triple interaction variable:					
$\mathrm{Sin.Bank}_{b,f}$	–0.091**	–0.045	–0.129***	–0.120**	–0.093
	(0.040)	(0.052)	(0.045)	(0.060)	(0.059)
$\mathrm{POST}_{\{t \geq t^*_{b,f}\}}$	0.082**	0.122**	–0.072	–0.131*	0.006
	(0.037)	(0.054)	(0.068)	(0.068)	(0.082)
$\mathrm{Bad.Firm}_{f,t}$	–0.008	0.099**	–0.390***	–0.041	n/a
	(0.029)	(0.043)	(0.081)	(0.056)	
N obs	17,174	17,065	16,344	10,336	13,016
N firms	3,226	3,225	3,190	2,647	2,932
R^2 (pseudo / LSDV)	0.3	0.2	0.2	0.3	0.1

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