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Abstract

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JEL Classification: D82, G21, J21

Keywords: Access to finance

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Information Sharing, Access to Finance, Loan Contract Design, and the Labor Market

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Abstract

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JEL Classification: D82, G21, J21
Keywords: Access to finance, borrower type, information sharing, labor markets, loan contracts, small businesses

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1. Introduction

Information asymmetries and the resulting adverse selection and moral hazard problems are one of the biggest obstacles in credit markets (Stiglitz and Weiss, 1981). Credit information sharing through public credit registries and private credit bureaus is seen as a critical policy tool to overcome the ensuing market frictions and inefficiencies and has been advocated by international financial institutions over the past two decades (CGAP & IFC, 2011). By reducing information asymmetries between borrowers and lenders, credit information sharing can improve lenders' screening process of borrowers ex-ante and tighten repayment discipline ex-post (Pagano and Jappelli, 1993). Credit information sharing also allows borrowers to build up reputation collateral, thus reducing information rents captured by lenders and increasing competition, and improves access to credit for creditworthy borrowers (Sharpe, 1990; Rajan, 1992; Padilla and Pagano, 1997). However, these different hypotheses have differential and sometimes contrasting implications for different borrowers and ambiguous aggregate effects on overall lending, financial stability, and real outcomes.

This paper uses the inclusion of a new borrower group into the Brazilian credit registry in January 2012, as the reporting threshold was lowered from 5,000 BRL (approximately 2,000 USD at the time) to 1,000 BRL (approximately 400 USD at the time), to test for the effect of information sharing. Unlike previous studies, we gauge the impact of credit information sharing on the extensive and intensive margins of lending, loan conditionality, loan performance and firm employment at the same time. Unlike other studies, we can also differentiate not only across borrowers with different risk profiles, but also banks of different ownership and size and thus different lending technology. Our study thus speaks not only to the literature on the effects of credit information sharing, but also the literature on lending techniques and banking market structure.

We document an increase in external financing for previously invisible borrowers, lower interest rates, a lower collateralization ratio, but also shorter loan maturities. We also find important differences across borrowers with different risk-profiles, with risky borrowers benefiting from better access to external funding sources, while relatively safe and safe borrowers experience lower interest rates. We find different behavior by incumbent and new lenders, with expansion of funding primarily coming from new lenders while the reduction in interest rates is driven by incumbent lenders. There are also distinguishing reactions by different ownership types of lenders, with privately-owned banks reacting most strongly to the expansion

of the credit registry. Finally, we show that while loan quality for riskier treated borrowers declines, the improved information sharing results in higher employment.

Theory has focused on different effects of credit information sharing. Padilla and Pagano (1997, 2000) focus on the disciplining effect of credit information sharing: by sharing default information with other lenders, banks can incentivize borrowers to reduce default probability. At the same time, information from credit registries can improve the screening process of banks (Kallberg and Udell, 2003). Sharing also positive (in addition to default) information about borrowers can increase competition between lenders and increase overall lending if asymmetric information is very pronounced (Pagano and Japello, 1993). This competition effect can also reduce hold-up problems for opaque borrowers limited to one specific lender (Sharpe, 1990; Rajan, 1992; von Thadden, 2004).

Theory also predicts an important heterogeneity effect across borrowers of different quality: while high-quality borrowers should benefit from information sharing, low-quality borrowers might see a reduction in access to credit or significantly adverse loan contract terms. It is therefore ex-ante not clear whether the sharing of information will, on average and in the aggregate, result in an increase in lending and an easing of loan contract terms such as a reduction in interest rates or collateral requirements.

We exploit an exogenous shock to borrower-specific information available to lenders due to a change in the threshold amount above which financial institutions have to report loan-level information to the Central Bank of Brazil (BCB)'s credit information system. This change was due to technological improvements in data storage and thus exogenous to firm borrowing. It therefore allows us to compare a group of small and very small businesses that appeared for the first time in the credit registry after the change with a control group of such firms that were already visible there, conditional on both groups having been borrowing before the change and controlling for general changes and time trends before and after the change. Given this setting, we are able to compare loans that became visible after the threshold change and had been granted in an environment with no information sharing to those granted after information was shared among lenders and to loans to borrowers whose borrowing information was already available before the change. The standard difference-in-differences (DiD) approach we apply combined with the exogeneity of the increase in available information about some firms enable us to obtain causal effects of an increase of available borrower-specific information induced by information sharing among lenders.[1]

[1] We also confirm this interpretation with a placebo test in Section 4.5.

In our regression analyses, we first explore the effect of information sharing on extensive and intensive lending margins. Relative to pre-treatment trends and control groups, riskier borrowers increase their number of lenders. The results suggest that privately-owned lenders use the newly available information to start relationships with risky and relatively safe borrowers (referred to as good borrowers in the remainder of the paper and in the results tables), while foreign-owned and Top-5 lenders use the newly available information to start relationships with risky, but not with safe (referred to as prime), or good borrowers. As regards the extensive margin, it is thus risky customers who were able to increase credit options relatively more from the inclusion in the credit registry than prime or good borrowers (unlike findings in previous studies for other countries, e.g., De Haas et al., 2021; Liberti et al., 2022).

While the economic effect of information sharing on new relationships is relatively small, we find a large effect on average loan amounts. We find increases of the average loan amount of 74% for risky, 109% for good and 68% for prime borrowers after inclusion in the credit registry relative to already visible borrowers, resulting in an inverted U-shaped effect. This increase comes primarily from new rather than existing lending relationships. Together, these results suggest an easing of financing constraints for firms newly included in the credit registry, though in different forms across borrowers of different risk profile. This first set of results is not necessarily consistent with the screening hypothesis, as it is riskier borrowers that benefit more than prime borrowers, but it is consistent with the competition hypothesis of borrowers having better access to external funding once information about them becomes available to other lenders.

We next explore the effect of information sharing on loan contract terms. We find that newly included borrowers experience lower interest rates, with the relationship being U-shaped in risk profile, that is, good borrowers seeing the largest decline, followed by prime and then risky borrowers. The lower interest rates are mostly driven by existing lending relationships, pointing again to competition effects of information sharing. At the same time, we find that good and prime borrowers see a large reduction in interest rates if they switch lenders, i.e., end an incumbent and start a new lending relationship, in line with Ioannidou and Ongena (2010). In sum, while for risky borrowers the higher competition from inclusion in the credit registry and their access to more funding sources do not come with lower interest rates from new lenders (unless these lenders are government-owned or small), good and prime borrowers benefit from higher competition in the form of lower interest rates, both from incumbent and new lenders. Furthermore, we document that most of the interest rate-reducing effect is driven by firms

borrowing from government-owned or -dominated lenders, while interest rates increase for risky and good borrowers if they borrow from a new Top-5 lender.

We further find that following inclusion in the credit registry, borrowers face shorter maturity loans from incumbent lenders, consistent with a disciplining effect, i.e., as information sharing allows borrowers access to more lenders, incumbent lenders react by reducing loan maturity. Concerning collateralization, we find that incumbent lenders relax the demand of collateral, in line with the findings for interest rates. This finding could be interpreted as a protection device from competition used by incumbent lenders, suggesting that they substitute physical collateral with the disciplining tool of information sharing. Here the effect is strongest for prime borrowers, while good borrowers face higher collateral requirements when switching lenders and with certain types of new lenders.

Finally, we find that while there is a relatively small decline in default probability on average, there are large differences between incumbent and new lenders and between borrowers of different risk types. This is most striking for risky borrowers who show a decline in default probability on loans with incumbent lenders and an increase of default probability on loans with new lenders. Finally, our findings show an increase in employment, especially for good and risky borrowers with new lenders that are private banks or credit unions. The expansion of credit availability due to better information availability therefore has positive labor market implications.

Taken together these results have several important implications that have, to the best of our knowledge, not yet been shown in the literature. First, the benefits of information sharing for lenders stem from their improved ability to screen and monitor borrowers' creditworthiness, which translates into better loan terms for safer borrowers. But all borrowers are affected in some instance, as we find evidence of improved access to credit and real effects in the labor markets especially for riskier borrowers. There is also evidence that information sharing benefits competition, as borrowers get better loan terms from their incumbent lenders.

Second, our results indicate that the effects differ by lender ownership type, which suggests that lenders use different credit technologies and this translates into heterogeneous effects associated with an increase of available information about borrowers. We show that risky borrowers can benefit from credit information sharing in a market with financial institutions that use the credit registry to expand their lending portfolio, even if this comes at the risk of higher loan default. Finally, we document that these effects translate into important labor market effects, through an increase in the number of employees of treated firms, with the effect being stronger for good borrowers and firms that have loans from either a new private

lender or a credit union. This may be a direct consequence of the findings about access to finance and loan contract terms. To the best of our knowledge, all these results for small and very small firms are important and novel in the literature.

Our paper is related to the theoretical and empirical literature on information sharing and the literature on access to finance. Padilla and Pagano (1997, 2000) and Pagano and Japelli (1993) show theoretically the stability-enhancing role of credit registries but also the increase in competition that they might trigger. Bennardo et al. (2015) show that loan performance improves because borrower screening becomes more efficient and overborrowing declines. In the empirical literature and using data from a lender in Guatemala, De Janvry et al. (2010) show that, after an increase in available information about borrowers, lenders realize efficiency gains and good borrowers are rewarded with better loan terms, while Luoto et al. (2007) show a decline in default rates and late payments, pointing to both screening and disciplining effects. Choudhary and Jain (2020) exploit the effects of an exogenous change to available information about firms in the Pakistani credit registry on adverse selection in the credit market and show that information from preexisting relationships cannot make up for information obtained through a credit registry. Doblas-Madrid and Minetti (2013) use the staggered entry of lenders into a credit bureau in the U.S. and find that information sharing reduces loan defaults, especially for informationally opaque firms. Degryse et al. (2016) use data from a Swedish bank to show that when a previously exclusive firm obtains a loan from another bank, the initial bank decreases its internal limit, suggesting that information sharing allows lenders to condition their terms on loans from others.[2] Finally, our results are related to Liberti et al. (2022), who study how the introduction of a commercial credit bureau in the U.S. affects competition and access to credit. Our paper adds to the literature by assessing the impact of credit information sharing on access to funding, loan contracting, loan performance and employment at the same time, while being able to differentiate across borrowers with different risk profile and banks with different lending techniques. This not only allows us to test for different theories related to credit information sharing, but also speaks to the literature on lending techniques and market structure at the same time. To our best knowledge, no other paper in the literature has done so.

[2] Other papers include Albertazzi et al. (2017) who investigate the effects of sharing information about loan rejections among lenders in Italy and De Haas et al. (2021) who show that after the introduction of a credit registry in Bosnia-Herzegovina more loans are rejected in competitive credit markets, lending standards tighten, and loan quality improves. Kallberg and Udell (2003) show that information sharing can help to predict future borrower defaults and Cheng and Degryse (2010) show that positive information sharing increases credit card lending volume in China. Finally, Boyd et al. (2020) show that higher information sharing can have similar effects as greater levels of creditor rights, arguing that poor creditor rights can be substituted by improved information sharing.

Our approach is quite similar in spirit to the approach used by Hertzberg et al. (2011), who show Argentine lenders coordinate when they get access to information about lending relationships with other lenders, but we are interested in distinctively different research questions.[3]

Specifically, while Hertzberg et al. (2011) gauge coordination effects of lenders after gaining access to information, we focus on the direct effect of information sharing on access to finance, loan contract terms, and loan performance. Furthermore, we explore the implications of information sharing for the labor market. While Hertzberg et al. (2011) exploit a reduction in the reporting threshold in Argentina from 200,000 USD to zero and thus the impact on relatively larger firms, we focus on small and very small firms for which there is less of a coordination issue but mainly a lending decision challenge.

Our empirical tests also speak to the literatures on small and medium enterprise (SME) lending techniques and on bank ownership. Specifically, the literature distinguishes between relationship-based and transaction-based lending, where the former relies more on repeated interactions between lenders and borrowers and soft information, while the latter relies more on hard information and assets (Berger and Udell, 2006). Typically, lenders rely on soft information to assess micro and small firms' credit risk. Empirical evidence has shown that larger and foreign-owned lenders are more likely to use transaction-based lending techniques (Beck et al., 2018, provide evidence using data from Bolivia). Our results are in line with these findings, as we see an increase in lending both along the extensive and intensive margin mostly for foreign-owned and larger banks that used access to hard information from the credit registry to expand their loan portfolios. At the same time, Beck et al. (2011) documented that privately- and foreign-owned banks are more likely to report the use of credit registry information in their lending decisions than government-owned banks, again in line with our findings.

The remainder of this paper is structured as follows. The next section presents the institutional background, the data and the empirical setup. Section 3 discusses the empirical findings for access to finance, while Section 4 explores the results for loan contracts. Section 5 explores the labor market implications of the credit registry expansion and Section 6 concludes.

2. Institutional Background, Data and Methodology

This section first describes the institutional background underlying the change in the Brazilian credit registry in January 2012. It then presents in detail the different data sources and how we

[3] It is also similar to the approach used by Gianetti et al. (2017). While those authors focus on internal rating changes that banks undertake before having to share information with a credit registry, we focus on the response of banks vis-à-vis their borrowers after information is being shared.

match them as well as the variables used in the empirical analyses. We further present descriptive sample statistics in this section. Finally, we introduce the different specifications that are used in the regression analyses.

2.1. Institutional Background about the Brazilian Credit Registry

The BCB created its own credit registry in 1997, with the objective to support banking supervision. The original credit registry has evolved over time and has been called Credit Information System (*Sistema de Informações de Crédito - SCR,* in Portuguese) since 2003. Every financial institution in Brazil must report monthly to the SCR detailed information on all outstanding loans of firms and individuals if the total exposure of each firm or individual is above a certain threshold.

As technological solutions have developed and become less costly, the BCB started communicating with financial institutions in 2009 about its ability to reduce the reporting threshold from 5,000 BRL to 1,000 BRL. The new regulation (Circular 3567 of December 12, 2011) established April and July 2012 as the deadlines for banks and credit unions, respectively, to send information in accordance with this new threshold. The largest banks in the country were swift to respond, and 90% of the increase in the pool of borrowers in the credit registry happened in January 2012.

Despite being a confidential dataset of the BCB, a firm or individual may grant permission to share part of their data registered in the SCR with other financial institutions.[4] There are 12 reports per borrower available for query[5], one for each of the last 12 months. They present data aggregated across all the financial institutions that have extended loans and include: the date that the borrower opened its first account in the financial system, the number of outstanding loans, the number of lenders, the number of loans with disagreements or under judice, co-obligations, and the amounts due, in arrears and in losses by loan type (e.g., working capital, real estate, auto loans) and currency denomination.[6]

As financial institutions must report the loans outstanding in each month, they include those granted in the current and past months. The reduction of the new threshold to 1,000 BRL allows us to identify firms in the SCR in 2012 that were not visible before and observe their

[4] Account opening forms and loan applications typically have a checkbox that allows the financial institution to access the client's data in the SCR.

[5] The query is usually made in batches, in which case the financial institution sends to the BCB a list with many firms and individuals (using a nationwide unique identifier), but it could be one by one. There is a very small fee for this service, negligible for our research questions.

[6] The new regulation added the aggregate credit limit to the list of data shared among financial institutions.

loans granted in an environment with no information sharing.[7] This identification strategy is similar in spirit to the identification used in, e.g., Hertzberg et al. (2011) and Gianetti et al. (2017).

2.2. Data Sources, Sample Construction, and Variables

The dataset used in the empirical analyses is constructed by merging three data sources, made possible because every firm in Brazil has a unique nationwide identifier, the CNPJ (*Cadastro Nacional da Pessoa Jurídica*). The main data source is the credit registry SCR of the BCB.[8] The proprietary and confidential SCR data identify both the lender and the borrower of each loan and include a large set of information such as the interest rate, the date the loan was granted and its due date, the amount outstanding, the amount in arrears, and whether the loan is collateralized or not.

We use another dataset managed by the BCB, *UNICAD*, which allows us to map each lender in the credit registry to its financial conglomerate. This is important because all the empirical tests are done at the financial conglomerate level, assuming lenders have access to the same pool of information when they belong to the same conglomerate. For the remainder of the paper, we will continue referring to financial institution or lender rather than financial conglomerate, unless otherwise noted. The *UNICAD* data also identify lender type and ownership (e.g., banks, credit unions, privately-owned, government-owned or foreign) and allow us to identify if a borrower has a new lender, i.e., one that would not have access to information privately gathered. For example, if firm X's unique lender in 2011 is Bank A and in 2012 it borrows from Bank B, we consider the latter a new lender only if A and B do not belong to the same financial conglomerate.

The third source of data is the Brazilian Ministry of Labor's employer-employee dataset Annual Social Information System (*Relação Anual de Informações Sociais* – RAIS). RAIS is a mandatory survey filled out annually by all firms (formal businesses) in Brazil. Using worker level data, we construct the number of employees each firm had in December 2011 and

[7] For example, suppose firm A has two loans from one lender. The amounts outstanding in December 2011 are 3,000 BRL and 2,500 BRL. Firm A can be found in the SCR that month because the total amount owned (5,500 BRL) is higher than the 5,000 BRL threshold. By contrast, suppose firm B also has two loans from one lender with amounts outstanding of 3,000 BRL and 1,600 BRL in December 2011. Firm B cannot be found in the SCR that month because the total amount owned (4,600 BRL) is lower than the 5,000 BRL threshold. In January 2012, the amounts outstanding are 700 BRL and 500 BRL and firm B can be found in the SCR because the sum of them (1,200 BRL) is above the new 1,000 BRL threshold. This type of dynamic led to a substantial increase in the number of firms visible in the SCR in 2012.

[8] Because the confidential version of the Credit Information System identifies both lender and borrower, the collection and manipulation of the individual loan-level data were conducted exclusively by the staff of the Central Bank of Brazil.

December 2012 and merge it to our dataset. Our analyses are restricted to firms with non-missing number of employees.

We use data from January 2011 to December 2012[9]. The period January-December 2011 is defined as the pre-period, and the period between January and December 2012 is defined as the post-period because, as mentioned before, the new regulation that lowered the reporting threshold almost doubled the number of borrowers registered in the SCR in January 2012.

To restrict the data to small and very small firms we rely on a three-step procedure. First, we remove firms that have more than 50 employees at any time in the period 2011 through 2014, based on the EU recommendation 2003/261, while at the same time dropping firms with no employees (i.e., self-employed individuals) and individual microentrepreneurs (*Microempreendedor Individual, MEI*).[10] Second, we remove firms that have at least one loan with an amount granted larger than 50,000 BRL (approximately 25,000 USD in the sample period) any time in the period 2011 through 2014. Finally, we drop firms which had a yearly mean of their original loan amounts lower than 100 BRL (approximately 50 USD in the sample period). While loans with a size of 100 BRL would not be included by themselves in the credit registry, if a firm has 10 or more of those loans outstanding, the information for all loans would be included if all loans had been made by the same lender after the reduction of the reporting threshold. This latter step removes from the sample firms that hardly take on credit.

The firms included for the first time in the SCR in 2012[11] with loans outstanding that had been granted in 2011 are considered our treatment group. These are firms which have had loans granted in an environment with no information sharing before the new reporting threshold was implemented and had their credit history become visible in 2012. The other firms are our control group. We compare firm- and loan-level characteristics of treated and control firms (first difference) in the periods before and after the regulatory change (second difference). As mentioned before, our difference-in-differences approach enables us to obtain causal effects of

[9] Garber et al. (2022) document that Brazil initiated a significant household credit expansion through the government-owned lenders in 2011. In our sample of small firms, we also observe that credit from government-owned lenders grew faster than that from private domestic and foreign owned lenders. However, when we break down the sample of treated and control firms, we see that the treated to control ratio of credit growth for private domestic and foreign lenders is more than 10 times greater than that for government owned lenders, suggesting that the credit expansion by government owned lenders is not driving our results.

[10] The business category MEI was created by law in 2008 with the objective to reduce the number of informal self-employed workers and increase contributions to the public pension system. Albeit considered firms for tax purposes, MEIs are not in fact business organizations. To remove MEIs from our analyses, we take advantage of their nondiscretionary naming. The name of a MEI is defined by this rule: the name of the owner, followed by his or her unique 11-digit-number nationwide individual identifier, CPF (*Cadastro de Pessoas Físicas*). To identify a MEI in the credit registry we extract all firms whose names finish with an 11-digit-number and check if the number is an individual's CPF.

[11] These firms did not appear in the registry in the previous three years.

the increase of available borrower-specific information induced by information sharing among lenders.

To analyze the effects of information sharing on loan terms, we focus on working capital and overdraft loans denominated in domestic currency (Brazilian Real) that have fixed interest rates and are not earmarked.[12] These are by far the two biggest loan categories included in the credit registry and the most prevalent among small and very small firms.[13] The final sample used in the baseline analyses consists of 351,074 observations of 175,537 firms, of which 7,915 are in the treatment group whereas 167,622 firms are in the control group.

To assess the differential effects of credit information sharing on borrowers with different risk profiles, we build two firm-level variables (*Prime* and *Good*) based on data shared among financial institutions via the SCR: loan amounts past due by several time frames. By retrieving the previous 12 months of data, lenders can build the firm's credit risk history and assess if the firm has had loans past due – and for how long they have been in arrears – over time. We take a similar approach and define *Prime* as a dummy variable that takes on the value of one if a firm only has loans that were never past due for longer than 14 days in 2011 and zero otherwise. A firm that has *Prime* equal to one is what we consider a safe borrower. *Good* is a dummy that takes on the value of one if the firm has at least one loan that was past due between 15 and 90 days in 2011 and zero otherwise. A firm that has *Good* equal to one is what we consider a relatively safe borrower. By contrast, the risky firms are those with at least one loan that was past due for 90 days or more in 2011. Therefore, the variables *Prime* and *Good* not only identify the safe and relatively safe borrowers, but also allow to gauge the use of the arrear's information by lenders via information sharing.

We use the following dependent variables in the firm-level empirical analyses. The variable *Number of lenders* indicates how many financial conglomerates[14] a firm borrowed from in the pre- and post-period. The variable *New lender* is a dummy variable that takes on the value of one if a firm borrowed from at least one new lender in the post-period, and zero otherwise. We also use variations of *New lender* by most common types of financial institutions: *New private lender*, *New government lender*, *New foreign lender*, and *New credit union lender*. We also distinguish whether the new lender is among the group of lenders which have assets larger than 10% of the financial system, and more than 1,000 branches (*Top-5 lenders* – Banco do

[12] Earmarked loans are subject to legally pre-determined interest rates or other legal restrictions.
[13] Most variables used in the analyses are constructed before dropping all other loan categories to reflect the full credit history of each firm.
[14] Some lenders are not part of a financial conglomerate, but they are accounted for as if there were a financial conglomerate comprised of only one financial institution.

Brasil, Bradesco, Caixa Econômica Federal, Itaú, and Santander)[15], or a *New small lender*. The variable *Average loan amount* is the mean of the largest loan amount outstanding (which proxies for the loan face value). Because for treated firms we only observe the loans granted in 2011 that were still outstanding in 2012, we restrict the analyses of the *Average loan amount* to loans granted in November or December of each year to avoid biasing the results. For a final set of tests, we employ the variable *Number of employees*. This variable indicates the number of employees per firm in either December 2011 or 2012.

We use the following dependent variables in the loan-level empirical analyses. *Interest rate* is the annual interest rate on the loan in percentage. *Collateral* is a dummy variable that takes on the value of one if the loan is secured by collateral, and zero otherwise.[16] *Loan maturity* is defined as the number of months between the loan approval date and the loan due date, and we restrict the analyses of pre-treatment loans to loans granted in November or December 2011, because only the loans granted to treated firms in 2011 with long enough maturity show up in the credit registry in 2012, which would introduce an upward bias in maturity. Finally, *Arrears > 30 days* is a dummy variable that takes on the value of one if the loan has been in arrears for more than 30 days at least once in the sample period.

For some of our tests, we distinguish between loan contract terms and performance of loans given by incumbent lenders and loans given by new lenders. In our loan-level sample, we have a total of 1,299,753 observations, 985,962 of which are from incumbent lenders that also give loans in the post period, and 236,052 from new lenders. Among loans by incumbent lenders, we distinguish between loans to borrowers with no new lending relationships (717,930) and loans to borrowers with at least one new lending relationship (268,032), which allows us to test for differences according to the competitive environment. Among the loans from new lenders, we have 130,247 observations referring to firms that switched lenders. Finally, we differentiate between lenders of different ownership and of different size as in the firm-level analysis. Specifically, we distinguish between loans given by new privately-owned lenders (39,126), new government-owned lenders (148,637) and new foreign-owned lenders (24,433), as well as between loans given by new big lenders (189,355) and new small lenders (46,697).

2.3. Descriptive Statistics

[15] The size distinction is based on lenders having more than 10% market share. Splits based on equity > BRL 15 million, or on more than 1,000 branches yield the same grouping. In the sample period, the Top 5 had a combined market share of around 80%, both in terms of assets and deposits.

[16] The reporting of collateralization is voluntary in the cases of loans without collateral. In these cases, lenders can report zero or not provide any information. In our baseline analysis we choose a conservative approach and we drop observations with no collateral information.

Table 1, Panel A presents descriptive statistics of our sample observations on the firm-level, considering the pool of firms that have loans outstanding in both the pre- and post-periods. The average number of lenders is slightly above one lender (1.25) and the average number of employees is about 7 employees per firm. The average loan amount between November 2011 and December 2012 per sample firm is 14,857 BRL (approximately 7,400 USD). All these numbers indicate we are indeed exploring the effects of information sharing for small and very small enterprises. 38% of the firms are prime borrowers and 52% are good borrowers, considering only loans from the pre-period. Table 1 also shows that 4% of the firms that have loans in both periods are defined as treated. For firms that add at least one new lender in the post-period, the values for number of lenders (1.44), number of employees (7.84), good borrower (55%), and average loan amount (16,414 BRL) are slightly higher. The share of prime borrowers, on the other hand is slightly lower (36%). The share of treated firms is the same with 4%.

In Panel B of Table 1, we present descriptive statistics on the loan-level. The average interest rate per loan and year is 62.81%, with a 99-percentile of 207%. While these numbers are high by developed country standards, they are in line with the interest rate regime used by Brazilian lenders in 2011 and 2012.[17] The average loan maturity is close to 17 months and 72% of all loans are collateralized.[18] The share of loans that go into arrears for more than 30 days at least once in the sample period is 11%. The average firm had about 20 loans in the sample period. Loan maturity (20.46), ratio of collateralized loans (73%), and arrear occurrence (12%) are slightly higher, while the interest rate is substantially lower (53%) for firms that added at least one loan from a new lender in the post-period.

2.4. Empirical Approach

In our main empirical analyses, we use a difference-in-differences approach to assess the effect of the reduction in the reporting threshold to the BCB credit registry SCR on the firm and loan-level outcomes. The small and very small businesses in both treatment and control groups have loans granted in the years 2011 and 2012. The difference between them is that financial institutions could not find the treated firms in the credit registry before 2012, which means the lending decisions in 2011 relied on privately acquired information or information acquired from

[17] The average interest rate during 2011-2012 was approximately 166% per year for overdraft facility, and 21% per year for working capital loans.

[18] There is information available on the collateral value in the SCR, but we choose not to use it because in our sample period the values were not frequently audited by the supervision department and therefore not entirely reliable.

other sources than the SCR. By contrast, nothing changed for the firms in the control group regarding information availability because of the reduction of the minimum amount to be reported to the credit registry.[19]

As the firms in both groups are small and very small firms, and the definition of when and how the threshold should change was based on technological advances in the BCB, this setting provides a clean identification and enables us to make causal inferences. Additionally, strategic behavior (or precise manipulation) of either firms or lenders to cluster on one side of the threshold is highly unlikely. On the one hand, small and very small firms are typically financially constrained, and would prefer having access to more credit. Going to several lenders to remain below the threshold also seems unlikely because of search costs and more complex management processes and higher transaction costs when having multiple lenders. On the other hand, lenders would not be willing to offer a larger loan amount to borrowers just to have them included in the credit registry because it would expose them to higher risk and they would have to share the borrower information with other financial institutions. In addition, lenders should be unwilling to lend less and avoid sharing information because that would result in foregoing the profits of larger credit amounts.

We compare the difference in the outcome variables between the treatment and the control group (first difference) before and after the reduction of the minimum amount (second difference). This is possible because at the end of each month in 2012 lenders reported all loans from firms which had a total amount outstanding above 1,000 BRL, including the loans granted in the previous years that did not show up in the credit registry in 2011 because the firms' total loan amount outstanding per financial conglomerate was lower than 5,000 BRL. This type of backfilling allows us to learn about the loans granted to firms in the absence of shared information from other lenders. We can plausibly assume that for those loans information asymmetries were (relatively) higher, while after the reduction of the loan amount threshold, loan decisions could be made with additional borrower-specific information available in the SCR. This setup gives us a clean empirical setting to explore what effects additional information about borrowers have on access to finance, loan contract terms, loan quality, and firms' labor market activities.

[19] In our approach, which is quite similar in spirit to the approach used by Hertzberg et al. (2011), who gauge coordination effects of lenders after gaining access to information, we focus on the direct effect of information sharing on access to finance, loan contract terms, and loan performance." While they exploit a reduction in the reporting threshold in Argentina from 200,000 USD to zero and thus the impact on relatively larger firms, we focus on small and very small firms for which there is less of a coordination issue but mainly a lending decision challenge. A similar identification strategy was used by Gianetti et al. (2017).

Our main firm-level regression specification is defined in the following way and estimated using OLS:

$$y_{i,t} = \delta Post_t * Treatment_i + \varphi_i + \tau_t + \varepsilon_{i,t}, \qquad (1)$$

where subscripts *i* and *t* denote borrower and pre-/post-period, respectively. *Post* is a dummy variable that takes on the value of one if the observation is from 2012, and zero otherwise, while *Treatment* is a dummy variable that takes on the value of one if a firm is in the treatment group. Each firm has thus two observations in the regression. As we include firm φ_i and year fixed effects τ_t, the *Treatment* and *Post* dummies are absorbed and not shown in equation (1) and in the results tables. This regression specification is used for the firm-level outcomes *Number of lenders, Average loan amount,* and *Number of employees* (and the breakdown by ownership type of lender and lender size). The coefficient of interest is δ, which captures the difference in the respective outcome for treated borrowers versus control borrowers before and after the change of the minimum reporting amount (*DiD* estimator). In the regressions, we also differentiate between prime, good and risky borrowers by interacting both the *Post* and the *Post*Treatment* variables with *Prime* and with *Good*. Standard errors are clustered on the firm-level.

We run cross-sectional regressions using only post-period observations and robust standard errors when the dependent variables are defined for such period; for example, *New Lender* is defined as 1 (0) when the firm has (not) received at least one loan from a new lender in the post-period:

$$y_i = \delta Treatment_i + \varepsilon_{i,}, \qquad (2)$$

For the loan-level regressions, we use the following specification:

$$y_{i,j,t} = \delta Post_t * Treatment_i + \tau_t + \varphi_i + \vartheta_j + \varepsilon_{ijt}, \qquad (3)$$

where subscripts *i, j,* and *t* denote borrower, loan type and month, respectively. In addition to firm fixed effects (φ), we also include fixed effects for the month and year in which the loan was approved (τ), and for the loan type (ϑ) (working capital or guaranteed overdraft loan). Further to that, in some regressions we include interacted bank-firm fixed effects. As before, the dummy variables *Post* and *Treatment* are absorbed by the time dummies and by the firm fixed effects.

Similar to equation (2), we run cross-sectional regressions using only post-period observations when we investigate the terms of loans granted by new lenders:

$$y_{i,j,b,t} = \delta Treatment_i + \tau_t + \vartheta_j + \varepsilon_{ijt}, \qquad (4)$$

In all loan-level regressions, standard errors are clustered at the firm-level.

3. Information Sharing and Firms' Access to Finance

In this section, we will present the findings for access to finance, in particular the number of lenders, whether firms were able to start new lending relationships and with what types of lenders, and the intensive margin expressed as the average loan amount borrowed.

3.1. Results for the extensive margin

The results in Panel A of Table 2 show that treated risky borrowers have more lenders after the lowering of the reporting threshold, while treated prime borrowers have fewer. Here, we run a difference-in-differences regression with control and treatment firms before and after the policy change (column 1) using regression (1) and also add interaction terms with pre-prime and pre-good in column (2). While the *Post*Treatment* coefficient (*DiD*) enters insignificantly in column 1, suggesting that on average, inclusion in the credit registry has not increased the number of lenders for these firms, this coefficient and its interaction terms with pre-prime and pre-good enter significantly, though with opposite signs, in column (2).

These results indicate that risky firms that enter the credit registry because of the lowering of the minimum reporting amount have, on average, 11% more lenders after their inclusion in the credit registry than non-prime control firms, while treated prime borrowers have 4.2% fewer lenders than prime control firms (sum of *DiD* and *DiD x prime*). Good borrowers do not see any change in their number of lenders after inclusion in the credit registry (sum of *DiD* and *DiD x good*). It is notable that this change in the number of lenders compares to an overall increase in the number of lenders after lowering of the minimum loan amount for both prime and good borrowers and a reduction for risky borrowers. While the economic significance is relatively small (the average number of lenders in our sample is 1.25 and the 99th percentile is 3 lenders), it is interesting to note that the risky customers are the ones most benefitting from the inclusion in the credit registry in terms of the number of lending relationships, while prime and good customers do not.

We next dig deeper to understand what new lending relationships if any, treated firms started after their inclusion in the credit registry. Therefore, in Panel B of Table 2, we regress

the likelihood of starting a new lending relationship after the inclusion in the credit registry on the treatment dummy and explore differences across lenders of different ownership types. Specifically, we gauge whether treated firms are more likely to get a loan from a lender type it has not borrowed from before and the type of new lender. It is important to note that these regressions only include observations from the post-period and the number of observations in the first two columns is therefore half of that in Panel A of Table 2. The sample size in columns (2) to (7) of the table is further reduced to 69,365 as in those regressions we only include the firms that borrow from at least one new lender in the post period. Furthermore, these regressions do not include any fixed effects, but only the treatment dummy and its interaction with *Prime* and *Good*. Hence, identification in these regressions is less strong than in Panel A of Table 2.

The results in column (1) of Table 2, Panel B show that treated risky firms are 4.9% less likely to start at least one new relationship, on average, while prime borrowers are 14.9% less likely than control firms.[20] Good borrowers are statistically neither more, nor less likely to start a new relationship. Importantly, however, the risky firms that actually start a new lending relationship primarily do so with a private, a foreign and/or a Top-5 lender. Among firms with new lending relationships after inclusion in the credit registry, risky treated borrowers are 12.8% more likely to start a relationship with a private domestic lender (as opposed to other lender types) than control firms (column 2), but 13.1% less likely to start a new relationship with a government-owned lender (column 3). This suggests the credit information about treated firms, newly included in the credit registry, is used differently by privately-owned and by government-owned lenders, consistent with bank-level survey evidence documented in Beck et al. (2011).

The higher likelihood of starting at least one new relationship with a private lender also holds for good but not for prime borrowers, however, while the lower likelihood of starting a new relationship with a government-owned lender is of similar magnitude for good borrowers (13.9%) but of smaller economic significance for prime borrowers (5.1%). We also find that treated risky firms with new lending relationships are 6.3% more (5.8% less) likely to start a new lending relationship with a foreign lender (credit union) than control firms (columns 4 and 5), while there is no significant difference in starting a new relationship with either for prime borrowers. Good borrowers are 1.9% less likely to start a new relationship with a foreign lender, while there is again no significant difference for credit unions.

[20] While the results for treated risky borrowers in Panel A and B seem contradictory, the Panel B coefficient estimates focus exclusively on treated vs. control borrowers. On the other hand, Panel A coefficient estimates measure effects relative to pre-treatment trends.

When distinguishing between large and small lenders, we find that treated risky borrowers are 4.8% more likely to start a new lending relationship with one of the Top-5 Brazilian lenders than control firms, while they are not more or less likely to start a relationship with a smaller lender (columns 6 and 7). Both good and prime borrowers are less likely to start a new relationship with a Top-5 lender, while prime borrowers are more likely to start a new relationship with a small lender. We do not find the same effect for good borrowers.

Together the results in Table 2 suggest that relative to pre-treatment trends, riskier borrowers increase their number of lenders. The results suggest privately-owned, foreign-owned and Top-5 lenders use the newly available information to start relationships with risky, but not with good or prime borrowers. As regards the extensive margin, it is thus risky customers who were able to increase credit options relatively more from the inclusion in the credit registry than safe or good borrowers.

3.2. Results for the intensive margin

While the results so far show a limited impact of the policy change on the extensive margin of access to finance, the results in Table 3, obtained by estimating regression (1), show a significant and large effect on the intensive margin, as proxied by the average loan amount.[21] The results in columns (1) and (2) show an increase of the average loan amount by 74% for risky, 109% for good and 68% for prime borrowers, with no significant difference across the three borrower groups. This suggests that while there has not been an economically significant average increase in the number of lending relationships, borrowers whose credit information was newly included in the credit registry were able to increase their financing options by having access to larger loans after the change in the credit registry, controlling for comparable firms that did not experience an exogenous shock to available information.

The remaining results in Table 3 show some variation of the increase in average loan size across borrowers with different types of new relationships. The results in columns (3) and (4) show the increase in average loan amount is more than twice as high for the group of firms with at least one new lender than for firms with no new lenders, suggesting that borrowers with new lenders benefit from the inclusion in the credit registry along both extensive and intensive margins. This holds across all types of borrowers. When focusing on firms with at least one new relationship, the increase in average loan amount for risky borrowers is largest for those

[21] As mentioned before, given that we cannot observe the initial loan amount for the pre-period loans, in the Table 3 regressions we use the largest amount outstanding of each loan to compute the average loan amount for loans originated in November and December 2011 for the pre-period.

with a new relationship with a credit union, with no significant increase for those with a new relationship with a government-owned lender. For prime borrowers with new relationships, the increase in the average loan amount is largest for those with new relationships with foreign, private, government and a Top-5 lender, while for good borrowers the increases in average loan amount are similar across firms with new relationships with different types of lenders. In sum, inclusion in the credit registry resulted in a marked increase in average loan amounts across all treated borrowers.

Taking the results of intensive and extensive margins together, we find a pronounced difference between risky, good and prime borrowers along the extensive margin, with only risky borrowers increasing the number of relationships with private, foreign and Top-5 lenders. As regards the intensive margin, all borrower types were able to increase their financing options by having access to larger amounts of credit. Together, these results suggest an easing of financing constraints for firms newly included in the credit registry, though in different forms across borrowers of different risk profiles. Interpreting the results from the lenders' viewpoint, private, foreign and Top-5 lenders are those using the newly available credit information to start novel relationships, especially with risky borrowers. This interesting finding suggests different types of lenders use the same information in seemingly different ways, which can be interpreted as a sign of different credit technologies, in particular difference screening mechanisms, at use. These findings are not consistent with the screening but with the competition hypothesis.

4. Information Sharing, Loan Contract Terms and Loan Quality

We now turn from access to finance and lending relationships to loan contract terms and loan quality and from firm- to loan-level regressions. Specifically, we gauge the effect of the inclusion in the credit registry on interest rates, maturity, collateralization, and arrear occurrence. In addition to average effects for prime, good and risky borrowers, we are able to undertake several sample splits. Specifically, we distinguish between loans by incumbent and by new lenders and among new lending relationships, we differentiate between borrowers that have at least one new relationship and borrowers with no new relationship. Finally, among new lenders, we distinguish between lenders of different ownership and size. All results presented in this section were obtained by estimating different versions of regressions (3) and (4).

4.1. Interest Rates

The results in Panel A of Table 4 show that, on average, treated firms see a reduction in interest rates after the treatment, but this is driven by established and not new lending relationships.[22] Specifically, the results suggest interest rates for treated firms fall, on average, by 11.5 percentage points (column 1), compared to an average interest rate of 62.8%, a substantial economic magnitude of about 18%. This interest rate reduction is strongest for good borrowers (20.1 percentage points), followed by prime borrowers (7.6 percentage points) and lowest for risky borrowers (3.5 percentage points).

Splitting the effect into interest rates on loans from existing and new lenders, we find that this interest rate reduction is mostly driven by existing lending relationships. Specifically, loans with existing lenders show 16, 7.9 and 4.5 percentage points lower interest rates for good, prime and risky borrowers, respectively (column 3), while there is no significant difference in interest rates on loans from new lenders in the post-period compared to the control group for any treated borrower (column 4). This suggests the lower interest rates given to treated firms are primarily a reaction by incumbent lenders against new competitors (other lenders with newly gained access to credit information).

Interestingly, the relationship between interest rate reduction and borrowers' risk profile is U-shaped, with the largest reduction for good borrowers and the smallest effect for risky borrowers. While (as shown above) it is riskier borrowers who benefit most from tapping new lenders after inclusion in the credit registry, it is mostly good (and to a certain extent prime) borrowers who benefit from lower interest rates by incumbent lenders. We explain this finding with risky borrowers having the biggest financing constraints before the credit registry change and because of their risk profile, incumbent lenders may not expect them to increase those financing options much after more, specifically negative, information about them becomes available. On the other hand, the good or relatively safe borrowers might be able to increase their financing options relatively more after more information about them is made available. To prevent them from seeking new lending relationships, incumbent lenders give them substantially better financing terms. This interpretation would be consistent with the finding and could explain that good borrowers did not start significantly more lending relationships after the change occurred in the credit registry.

The results in Panel B confirm it is indeed competition that at least partly drives the reduction in interest rates for treated firms. Here we distinguish between (i) borrowers with no

[22] Note that the reports lenders get from the credit registry do not include information about interest rates, but only about borrower risk.

new lending relationship, (ii) borrowers with at least one new lending relationship after treatment, and (iii) borrowers that switched to new lenders. As before, the effects are largest for good borrowers: interest rates by incumbent lenders drop by 15.6 (16) percentage points if a firm adds at least (does not add) one other lender, while they drop by 32.7 percentage points if the firms switch to a new lender. For prime borrowers, the results in columns (2) and (1) show interest rates by incumbent lenders drop by 10.3 (7.1) percentage points if a firm adds at least (does not add) one other lender, while the column (3) results show they drop by 9.9 percentage points if the firms switch to a new lender. The effects are smallest for risky firms: in column (1) we see that interest rates drop by 4.2 percentage points if the firm does not add a new lender, while there is no significant reduction if a borrower adds at least one other lender. Switching to a new lender, on the other hand leads to an increase in interest rates by 8.4 percentage points for risky borrowers, albeit the result being only significant on the 10%-level. So, it is again on the good (or relatively safe) borrowers, that the effects of competition are strongest, especially if borrowers switch to a new lender, while no such effects are at work for risky borrowers. The findings for switching are to some extent in line with the results of Ioannidou and Ongena (2010). We add to their results by showing that being offered lower interest rates when switching depends crucially on the borrower type. While safe and relatively safe borrowers profit from switching, at least initially, risky borrowers do not.[23]

The results in Table Panel C show that in terms of interest rates charged by new lenders, prime borrowers do not experience any significantly (at least not at the 5% level) different interest rates than control borrowers, while risky and good borrowers are charged significantly lower interest rates by government-owned lenders and significantly higher interest rates by Top 5 lenders. Furthermore, good borrowers get charged higher interest rates in the case of new relationships with foreign-owned lenders.

In sum, incumbent lenders reduce interest rates on loans granted to treated borrowers (compared to control borrowers), especially for good and prime borrowers. On average, new lenders do not charge higher interest rates, although there are differences across different types of new lenders. There seem to be different effects in play across borrowers of different risk types. While for risky borrowers the higher competition from inclusion in the credit registry and their access to more funding sources does not come with lower interest rates from new lenders, good and prime borrowers benefit from higher competition in the form of lower interest rates, both from incumbent and new lenders.

[23] We did not explore how interest rates behave after some time passes as in Ioannidou and Ongena (2010) because this is not a central research question in our study.

Overall, our findings point to a heterogeneous effect of information sharing for reducing information asymmetries that depend crucially on the borrower type. This is additional evidence that information sharing has a different value not only for different types of lenders or credit technologies used by these different lenders, but also for different types of borrowers. All borrowers seem to profit regarding the interest rate, but prime borrowers profit differently from information sharing than the good and the risky borrowers. Furthermore, the findings for interest rates suggest incumbent lenders try to protect themselves from an increase in competition caused by the gain of information by other lenders about the incumbent lenders' borrowers by offering them more attractive prices. This effect is strongest where the risk of losing borrowers to new lenders is highest – in case of good borrowers where the additional information made available through the reporting threshold reduction should have the highest value. These findings suggest information sharing among lenders may reduce hold-up problems.

4.2. Loan Maturity

The results in Panel A of Table 5 show that treated firms receive shorter-maturity loans from incumbent lenders after treatment than control firms, while there is no significant difference for new lenders. Specifically, the coefficient of the difference-in-differences estimator in column (1) suggests treated firms receive 1.8 months shorter loans in the post-period than control firms. Given the average maturity of 20 months, this represents an economic magnitude of the effect of about 10%. The maturity effects of information sharing are stronger for prime and good borrowers (column 2). Focusing on loans from incumbent lenders only, the results in columns (3) suggest incumbent lenders reduce maturities most for prime borrowers (1.5 months), followed by good borrowers (1 month) and risky borrowers (0.6 months). The results in column (4), on the other hand, do not show any significant effects for new lending relationships.

The regression results in Panel B show shorter maturities on loans by incumbent lenders for prime borrowers with either no new lender or at least one new lender, while there is no different maturity if the firm switches to a new lender. For good borrowers, we find a shorter maturity of loans by incumbent lenders if the borrower does not have any additional lender and a large reduction (over 6 months) in maturity if the borrower switches to a new lender. We find no differential effect for risky borrowers. These findings are consistent with the disciplining hypothesis, especially for good and prime borrowers; unlike the ones for interest rates inconsistent with the competition hypothesis. This suggests the applied credit technology is

adjusted after more information becomes available about borrowers that were not included in the credit registry before.

Panel C show contrasting results for the effect of the inclusion in the credit registry on loan maturities by new lender type. We do not find any significant effect for prime borrowers, while for good borrowers we find longer maturities for new relationships with government-owned lenders, credit unions and small lenders and shorter maturities for new relationships with foreign-owned new lenders. For risky borrowers, we find that new lenders provide shorter-maturity loans if they are foreign-owned or Top-5 lenders and longer-maturity loans if they are small lenders.

In sum, lower interest rates following inclusion in the credit registry are also accompanied by shorter maturities, but mostly limited to incumbent lenders. These findings can be explained with a disciplining effect, i.e., as information sharing allows borrowers access to more lenders, incumbent lenders react by reducing loan maturity. Together with the interest rate results, these findings point toward lenders changing the contract structures when there is an exogenous increase of available information. Borrowers receive more attractive credit price terms, but the shorter maturities imply less attractive contracts as the borrowed amounts have to be paid back in less time.[24] Furthermore, the maturity findings across lender types suggest different types of lenders react differently to new borrower information made available through the credit registry expansion.

4.3. Collateralization

The results in Panel A of Table 6 show treated firms are less likely to have to put up collateral than control firms after inclusion in the credit registry. Specifically, we find that loans to treated firms are 12 percentage points less likely to post collateral compared to control loans after their inclusion in the credit registry (column 1). This is a strong economic effect, given that in the overall sample 72% of all loans are collateralized. Prime borrowers are 13.8 percentage points, risky borrowers 12.5 percentage points and good borrowers 8.4 percentage points less likely to have to pledge collateral.

The result is driven by incumbent lenders – a lower likelihood to pledge collateral of 10, 7.7 and 7 percentage points for risky, prime and good borrowers, respectively (column 3). For new lenders, we do not find any treatment effects. These results suggest incumbent lenders relax the demand of collateralization, in line with the results for interest rate, which could be

[24] Our setting does not allow us to comment on the overall effect of receiving lower credit prices while at the same time having to pay back the loans in a shorter period.

interpreted as a protection device from competition used by incumbent lenders. Access to information about borrowers through the credit registry thus seems to constitute somewhat of a substitute for collateral.

The results in Panel B confirm the differences across borrowers with different risk profiles. Good borrowers are 7 percentage points (4.8) less likely to have to post collateral on loans with existing lenders if they have no new (have at least one new) lender and are 10.8 percentage points more likely to have to post collateral if they switch lenders. In the case of risky (prime) borrowers, they are 10.7 (8.7) percentage points less likely to have to post collateral on loans with existing lenders if they do not add any new lender after the reduction of the reporting threshold, while there is no significant reduction if they add at least one new lender or switch lenders. On the one hand, these results can be explained with competition effects (in case of no new lender), but also show new lenders might not completely trust information sharing and are more likely to insist on collateral in the case of good borrowers.

The regressions in Panel C show risky borrowers benefit from lower collateralization in the case of new relationships with credit unions, foreign lenders and, especially, small lenders. Good borrowers benefit from lower collateralization in the case of new relationships with credit unions, foreign lenders and small lenders, but are more likely to have to post collateral in the case of new government lenders and new Top-5 lender. Prime borrowers, on the other hand, do not benefit from lower collateralization in new relationships, no matter what type of new lender.

In sum, prime borrowers benefit primarily from lower collateral requirements with incumbent lenders and risky borrowers from lower collateral requirements from incumbent and some types of new lenders. Good borrowers face lower collateral requirements from incumbent lenders but higher collateral requirements if they switch lenders. especially to new government or Top- 5 lenders. This suggests competition effects being at work, but also limitations in the use of credit information.

Taken together, the results for loan contract terms provide some interesting and important insights of the effects of information sharing for small and very small businesses. First, lenders differentiate according to the riskiness of the borrowers and this is most pronounced for the interest rate and maturity. Second, it seems the use of the new information about borrowers is different depending on the lender type, suggesting important differences with regard to their respective credit technology and also speaks to how credit technologies are adjusted across different lender types once information sharing is applied. All these results are, to the best of our knowledge, new in the literature.

4.4. Results for Loan Quality

Finally, we turn to investigating how the use of the newly made available information resulting from the credit registry expansion affects loan quality. We measure loan quality as a loan being in arrears for at least 30 days at least once during the lifetime of the loan. The 30-day threshold is common in the literature when investigating the default behavior of small and very small businesses.[25]

In column (1) of Table 7, Panel A we find that loans to newly included borrowers are 0.9 percentage points less likely to go into arrears than loans to control firms after the policy change (column 1), compared to an average default rate of 11%.[26] This higher loan quality is driven by good borrowers whose loans have a 1.6 percentage point lower default probability, while there is no significant change in loan quality for risky or prime borrowers (column 2).

The effect also seems to be driven by incumbent lenders (column 3), where we find a significantly lower default probability for risky, but not good or prime borrowers. In the case of loans given by new lenders (column 4), we find a 13 percentage point higher default probability for risky borrowers and a 3.4 percentage point lower default probability for good borrowers, with no significant difference for prime borrowers. While there is a relatively small decline in default probability on average, there are large differences between incumbent and new lenders and between borrowers of different risk types. This is most striking for risky borrowers who show a decline in default probability on loans with incumbent lenders and an increase on loans with new lenders. Not surprisingly, when investigating how information sharing affects default rates, the smallest (or in our case zero) effects are found for prime borrowers. As default risk is already very small for loans to such firms, information sharing does not add significant value for these borrowers.

The results in Panel B confirm these striking differences. Specifically, we find a reduction in default probability on loans of incumbent lenders to risky borrowers when they do not add any new relationship, no significant change if they add at least one new relationship and an increase in default probability of large economic magnitude if the risky borrowers switch to a new lender. While we find no significant difference between treatment and control group for prime borrowers, good borrowers show a lower default probability if they switch to a new lender after inclusion in the credit registry.

[25] Results do not change substantially if we apply 60- or 90-day arrears definitions.
[26] To make statements about overall benefits for borrowers and lenders and the entire credit sector, we would need detailed firm-specific and lender-specific information which we do not possess at this time.

The results in Panel C show differential effects of the inclusion in the credit registry across different lender types. Specifically, we find a higher probability of loan arrears for risky borrowers in the case of new relationships with privately-owned and small lenders. For prime borrowers, on the other hand, we find no significant difference in default probability on new lending relationship compared to the control group. The rationale is as discussed before. As regards the default probability, information sharing has the lowest value for borrowers that can easily be identified as very low risk borrowers – in our case the prime borrowers. For good borrowers, we find a reduction in default probability compared to the control group in the case of new relationships with government-owned and Top-5 lenders.

4.5. Discussion

In the previous sections, we documented a multitude of results for important firm- and loan-level variables, while distinguishing between borrowers of different risk classes and lender types. How do we reconcile these results so far? It is the riskiest firms that are best able to tap new lending sources after inclusion in the credit registry, but they are also the ones whose default probability increases most in new lending relationships. This finding holds especially for risky borrowers that switch to new lenders and holds especially for privately-owned lenders, which are most willing to start new relationships with risky borrowers. While new lenders do not necessarily charge lower interest rates or change maturities on risky borrowers, they reduce collateralization. Risky borrowers thus primarily gain from inclusion in the credit registry in terms of being able to access more lenders, obtaining larger loans and being less likely to have to post collateral. All this points to a reduction of financing constraints for risky borrowers once information about them becomes available by means of inclusion in a public credit registry. However, while certain lender types seem to focus their outreach efforts on these risky borrowers, there is a cost to it, in the form of a higher default probability.

Good borrowers benefit primarily from an expansion along the intensive rather than the extensive margin, but also show a consistent reduction in their default probability. Compared to other borrower types, they benefit from the largest interest rate reduction, experience a reduction in loan maturities from incumbent lenders and benefit from a reduction in collateral requirements from incumbent lenders. We cannot comment on the net effect of lower interest rates, shorter loan maturities and a lower likelihood of having to pledge collateral, but based on these results we can clearly state that loan contracts for these borrowers change substantially as a result of information sharing.

Prime borrowers, finally, see no increase in the number of lenders but experience an increase in average loan size, while seeing no change in default probability. Prime borrowers benefit from some reduction in interest rates (though less so than good borrowers) and a reduction in collateralization, but also suffer a reduction in loan maturity in existing relationships.

Information sharing thus benefits good and prime borrowers through lower loan interest rates, while lenders do not suffer in terms of higher default probability. Risky borrowers benefit from a funding extension and lower collateralization, but lenders bear the cost of a higher default probability. All these findings point towards very substantial changes in how contract structures are adjusted when new credit registry information becomes available. They also clearly show that how credit information sharing affects credit markets depends crucially on the type of borrower risk. Our results further point towards structural changes of credit markets caused by information sharing. Finally, different types of lenders seem to adjust very differently to an exogenous shock to the quantity of available credit registry information, which suggests credit technologies with information sharing change differently depending on the type of lender.

To strengthen the causal interpretation of our results, we undertake a placebo test (results available on request). Specifically, we drop treated firms from our sample and then randomly assign treatment status to the same share of firms in the control sample as in the overall (treatment plus control) sample. Rerunning the regressions in Tables 2 to 7 we find either insignificant coefficient estimates or significant coefficients of the opposite sign as in our main regressions. We interpret this finding as suggesting that it is the inclusion into the credit registry that drives our results rather than some other policy change or event that affects small enterprises.

5. Information Sharing and the Labor Market

In one final set of analyses, we turn to how the effects we have documented so far impact firms' labor market activity. For these analyses, we make again use of the firm-level sample for which the regressions only include firm fixed effects. For each sample firm, we know the number of employees at the end of December of both sample period years 2011 and 2012 from RAIS, the database managed by the Brazilian Ministry of Labor. The main purpose of this analysis is to explore how the different use of the newly made available information depending on the type of borrower (prime versus good versus risky) and the lender ownership type (e.g., privately- versus government-owned) translates into labor market effects. To the best of our knowledge, so far no theory and no empirical evidence exist about how credit information sharing affects

employment, though there is theory and evidence on the positive impact that access to finance has for employment.[27] Hence, while it is not possible to make any empirical predictions based on existing theory, we hypothesize that the benefits of credit information sharing should translate into firms' labor market activity and that we should find different results based on borrower risk and depending on the lender type.

The results in Panel A of Table 8 show a significant increase in the number of employees for treated firms compared to control firms following their inclusion in the credit registry, with the effect being strongest for risky firms (columns 1 and 2). Specifically, we find an increase in employment of 11.3% for risky, 8.6% for good and 4.2% for prime borrowers. For risky borrowers, this increase is primarily driven by firms with no new lenders, while for good and for prime borrowers, the effect is stronger for firms with at least one new lender (columns 3 and 4). This suggests that for risky borrowers, the effect derives primarily from easing financing constraints and receiving larger loans from incumbent lenders, while for good and prime borrowers the effect derives as much if not more from the expansion of their funding sources and, at least in the case of good borrowers, from substantially lower prices of credit.

The results in Panel B show the relative change in employment for firms with at least one new lending relationship and document significant variation across lender type. For risky borrowers, we find an increase in employment for firms with a new relationship with either a private lender or a credit union. For good borrowers, we find an increase in employment for firms with a new relationship with private and government lenders, credit unions as well as both large and small lenders. For prime borrowers, finally, we find an increase in employment for firms with a new relationship with private, government-owned, foreign and Top-5 lenders.

These findings clearly indicate that the easing of information asymmetries by including borrowers in the credit registry has positive implications for firm growth. Easing of financing constraints, especially for riskier firms, results in hiring of new staff and an expansion of firms.[28]

[27] Among others, Pagano and Pica (2012) show a positive and significant relationship between financial development and job creation in developing countries, Beck et al. (2010) and Benmelech et al. (2011) show that financial liberalization in the U.S. led to decreases in unemployment and increased labor market participation especially among low-skilled workers. Chodorow-Reich (2014) shows that firms with a pre-crisis relationship with less healthy lenders had higher reductions in employment following the Lehman bankruptcy compared to pre-crisis clients of healthier lenders. Cingano et al. (2016) show that bank exposure to the credit shock predicts lower levels of firm employment, while Popov and Rocholl (2018) find that firms which have credit relationships with German savings banks exposed to the mortgage crises experienced a significant decline in labor demand during the Global Financial Crisis.

[28] We undertake again a placebo test as discussed in section 4.5 and find no significant coefficient estimates. Our empirical setup does not allow to evaluate whether such firm expansion increases firm value, profitability or the wealth of workers.

6. Conclusions

In this paper we document effects of credit information sharing on access to finance, loan contract terms, loan quality, and the labor market for small and very small businesses in Brazil. We use a change in the reduction of the minimum loan amount reporting threshold in the Brazilian credit registry in January 2012 as an exogenous shock and show a number of interesting and important effects that differ by the type of borrower – safe versus relatively safe versus risky – and by lender ownership type. While access to finance does not improve for all borrowers, we find that it does so for risky borrowers. Furthermore, access to finance improves for firms that borrow from private domestic lenders. The strongest results are obtained for the intensive margin. Average loan amounts increase for all borrower types, which indicates that financing constraints are reduced as a result of credit information sharing, but the reduction stems from the intensive rather than the extensive margin.

The results for loan contract terms also show interesting heterogeneous effects depending on borrower risk. Overall, the relatively safe borrowers are most affected by the credit registry expansion, followed by the risky and the safe borrowers. We interpret this as direct effects of information asymmetry reduction that affects different borrowers differently. In the case of relatively safe borrowers (those that are at the margin between being safe and risky borrowers), where information asymmetries can be expected to be most pronounced, the value of information sharing has the highest value.

The results we obtain are also mostly consistent with higher competition following the availability of information about treated borrowers where incumbent lenders adjust contract terms such that borrowers are less likely to leave the incumbent lender or add new lenders. The results for maturity, on the other hand, can best be explained by trying to discipline the borrowers simultaneously while offering more attractive terms for interest rates and collateral requirements. Finally, contract structures are adjusted differently depending on the ownership type of lenders. This suggests different lenders use the same information in very different ways. This is also reflected in the results for loan quality. The risk-reducing effect of information sharing is smaller for the safe borrowers than for the riskier borrowers, likely because the informational gains are also smaller in that case. They are overall highest for the relatively safe borrowers, which is consistent with the findings from the loan contract terms.

Finally, this study shows that the documented findings for access to finance, loan contract terms, and loan quality do transfer into firms' labor market activity. All borrower types

increase employment, but the effect is in absolute and in relative terms strongest for risky borrowers. Taken together, finding that credit information sharing affects financing constraints, loan contracts, loan quality and the labor market together with the crucial finding that all these effects depend on the borrower and to some extent the lender type provide a novel perspective on the benefits of credit information sharing systems. Our results have important policy implications, for instance, regarding the design of credit information systems and the usage of the newly made available information by lenders. Furthermore, they inform us about possible consequences for labor markets. Taken together, our results may call for new theories as we are not aware of any theory about information sharing combining all aspects this study considers.

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Tables

Table 1: Summary statistics

This table presents the descriptive statistics of our main variables used in the analyses. *Number of lenders* is the number of financial institutions with which a firm has outstanding loans. *Number of employees* is the total number of employees per firm and refers to December 2011 and 2012. *Prime borrower* is a firm level dummy that takes on the value of one if the firm only has loans that were never past due longer than 14 days in the pre-period and zero otherwise. *Good borrower* is a firm level dummy that takes on the value of one if the firm only has loans that were past due between 15 and 90 days in the pre-period and zero otherwise. *Average loan amount* is the mean amount of loans granted in November and December of 2011 and throughout 2012. *Treatment* is a firm level dummy that is one if the firm had not appeared in the Brazilian credit registry in 2009, 2010, and 2011, but shows up in 2012 and has loans outstanding in 2012 that had been granted in the previous years. *Interest rate* is the annual interest rate in percentages, winsorized at the 5- and 95-percentiles. *Loan maturity* is the number of months between the loan approval date and the loan due date using pre-period (2011) loans granted in Nov-Dec. *Collateral* is a loan level dummy that takes on the value of one if the loan is secured by collateral. *Arrears > 30 days* indicates if at least one loan of the firm was in arrears for more than 30 days in the sample period or not. SD indicates the standard deviation, P1 and P99 are the one- and 99-percentile, respectively. N indicates the number of observations.

	Mean	Median	SD	P1	P99	N
Panel A: Firm-level variables						
All firms						
Number of lenders	1.25	1	0.53	1	3	351,074
Number of employees	6.95	4	7.66	1	38	351,074
Prime borrower	0.38	0	0.49	0	1	351,074
Good borrower	0.52	1	0.50	0	1	351,074
Average loan amount	14,856.95	11,666.98	12,207.00	111.67	46,451.84	16,774
Treatment	0.04	0	0.20	0	1	351,074
Firms that have at least one new lender						
Number of lenders	1.44	1	0.66	1	4	138,730
Number of employees	7.84	5	8.16	1	40	130,658
Prime borrower	0.36	0	0.48	0	1	138,730
Good borrower	0.55	1	0.50	0	1	138,730
Average loan amount	16,413.61	14,437.99	12,413.10	118.03	47,075.24	6,720
Treatment	0.04	0	0.19	0	1	138,730
Panel B: Loan-level variables						
All firms						
Interest rate (%)	62.81	41.00	53.64	11	207.00	1,299,753
Loan maturity (months)	16.70	12.23	13.59	0.57	42.07	966,302
Collateral	0.57	1	0.50	0	1	799,401
Arrears > 30 days	0.11	0	0.31	0	1	1,299,753
Number of loans	20.11	11	30.87	2	154	1,299,753
Loans of firms that have at least one new lender						
Interest rate (%)	52.73	34.00	51.86	11	207.00	560,401
Loan maturity (months)	20.46	24.17	14.25	0.83	47.67	441,228
Collateral	0.55	1	0.50	0	1	324,741
Arrears > 30 days	0.12	0	0.33	0	1	560,401

Table 2, Panel A: Firm-level results for the number of lenders

This table shows results for the number of lenders as dependent variable. *DiD* is the interaction of *treatment* and *post*. All variables are explained in Table 1. Fixed effects are included as indicated in the table. Standard errors clustered on the firm-level are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1)	(2)
DiD	-0.0023	0.1108***
	[0.0032]	[0.0098]
DiD x prime		-0.1526***
		[0.0108]
DiD x good		-0.1046***
		[0.0110]
Post	0.0876***	-0.0672***
	[0.0010]	[0.0036]
Post x prime		0.2007***
		[0.0039]
Post x good		0.1505***
		[0.0039]
DiD+DiD x prime		-0.0417***
		[0.0044]
DiD+DiD x good		0.0062
		[0.0050]
Firm fixed effects	Yes	Yes
Observations	351,074	351,074
Adjusted R^2	0.254	0.268

Table 2, Panel B: Firm-level results for starting a new lending relationship in the post-period

This table shows results for firms that received at least one loan from a new lender in the post-period as dependent variable. In columns (2) to (5), the dependent variable takes on the value of one if the firm's new lender is a private lender (2), a government lender (3), a foreign lender (4), or a credit union (5) and zero otherwise. In columns (6) and (7), we only include loans to firms where the new lender is a big or a small lender. All remaining variables are explained in Table 1. Robust standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) New lender	(2) New private lender	(3) New government lender	(4) New foreign lender	(5) New credit union lender	(6) New big lender	(7) New small lender
Treatment	-0.0488***	0.1281***	-0.1307***	0.0624**	-0.0584***	0.0477**	-0.0419
	[0.0159]	[0.0301]	[0.0277]	[0.0257]	[0.0167]	[0.0235]	[0.0261]
Treatment x prime	-0.1002***	-0.1418***	0.0802**	-0.0481*	0.0709***	-0.1084***	0.0923***
	[0.0175]	[0.0339]	[0.0326]	[0.0286]	[0.0191]	[0.0270]	[0.0298]
Treatment x good	0.0390**	-0.0226	-0.0083	-0.0816***	0.0519***	-0.0717***	0.0398
	[0.0181]	[0.0330]	[0.0309]	[0.0274]	[0.0182]	[0.0256]	[0.0283]
Prime	0.0220***	-0.0385***	0.1585***	-0.0322***	-0.0714***	0.1027***	-0.1148***
	[0.0042]	[0.0068]	[0.0072]	[0.0054]	[0.0048]	[0.0059]	[0.0064]
Good	0.0503***	-0.0478***	0.1577***	-0.0136**	-0.0588***	0.0846***	-0.0833***
	[0.0041]	[0.0066]	[0.0069]	[0.0053]	[0.0047]	[0.0058]	[0.0062]
Treatment + treatment x prime	-0.1489***	-0.0137	-0.0505***	0.0142	0.0125	-0.0606***	0.0504***
	[0.0073]	[0.0156]	[0.0173]	[0.0126]	[0.0094]	[0.0134]	[0.0143]
Treatment + treatment x good	-0.0098	0.1055***	-0.1390***	-0.0192**	-0.0065	-0.0240**	-0.0020
	[0.0086]	[0.0135]	[0.0137]	[0.0097]	[0.0072]	[0.0103]	[0.0111]
Observations	175,537	69,365	69,365	69,365	69,365	69,365	69,365
Adjusted R^2	0.004	0.002	0.010	0.001	0.005	0.006	0.006

Table 3: Firm-level results for intensive margin

This table shows the results for the natural log of the average loan amount. For the pre-period, we only include loans from November and December 2011. *DiD* is the interaction of *treatment* and *post*. In column (3), we only include firms that *DiD* not add any new lender in the post-period. In column (4), we include all loans to firms that added at least one new lender in the post-period. Columns (5) to (8) provide the breakdown by type of added new lender, and columns (9) and (10) the breakdown by lender size. Fixed effects are included as indicated in the table. All variables are explained in Table 1. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels,, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All firms	All firms	Firms without new lenders	Firms with at least one new lender	At least one new private lender	At least one new government lender	At least one new foreign lender	At least one new credit union lender	At least one new big lender	At least one new small lender
DiD	0.8564***	0.7426***	0.7966***	1.3820**	1.5946*	1.1180	1.7525***	2.6672***	1.4866**	1.7797**
	[0.0762]	[0.2333]	[0.2446]	[0.6250]	[0.9024]	[0.8743]	[0.3324]	[0.3409]	[0.7098]	[0.8413]
DiD x prime		-0.0612	-0.2896	-0.0309	-0.0573	0.4296	-0.1344	-2.5881**	0.0556	-0.5999
		[0.2561]	[0.2709]	[0.6620]	[0.9971]	[0.9133]	[0.5018]	[0.9987]	[0.7478]	[0.9563]
DiD x good		0.3502	-0.1473	0.2997	0.1364	0.5853	0.0000	-0.9296	0.1839	-0.2765
		[0.2643]	[0.2924]	[0.6482]	[0.9537]	[0.9124]	[0.0000]	[0.5662]	[0.7349]	[0.9054]
Post	-0.0630***	-0.3588***	-0.5867***	-0.0852	-0.1091	-0.0097	-0.4473	0.3741	-0.1159	0.1306
	[0.0176]	[0.0839]	[0.1032]	[0.1348]	[0.2731]	[0.1709]	[0.3658]	[0.3409]	[0.1532]	[0.2535]
Post x prime		0.4062***	0.5880***	0.2091	0.0969	0.2594	0.5326	-0.3063	0.2954*	-0.2839
		[0.0884]	[0.1078]	[0.1447]	[0.2937]	[0.1831]	[0.3925]	[0.3882]	[0.1630]	[0.2877]
Post x good		0.2468***	0.4108***	0.0591	0.1084	0.0127	0.3261	-0.1785	0.1300	-0.2890
		[0.0871]	[0.1070]	[0.1404]	[0.2853]	[0.1773]	[0.3777]	[0.3631]	[0.1588]	[0.2687]
DiD + DiD x prime		0.6814***	0.5070***	1.3512***	1.5373***	1.5475***	1.6182***	0.0791	1.5423***	1.1799*
		[0.1056]	[0.1164]	[0.2182]	[0.4240]	[0.2639]	[0.3759]	[0.9388]	[0.2355]	[0.4546]
DiD + DiD x good		1.0928***	0.6493***	1.6817***	1.7309***	1.7033***	1.7525***	1.7376***	1.6706***	1.5033***
		[0.1241]	[0.1602]	[0.1718]	[0.3083]	[0.2610]	[0.3324]	[0.4521]	[0.1906]	[0.3344]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,774	16,774	10,054	6,720	1,790	4,126	898	570	5,772	1,274
Adjusted R^2	0.385	0.388	0.509	0.179	0.135	0.177	0.282	0.120	0.174	0.187

Table 4, Panel A: Loan-level results for interest rate

This table presents results with the interest rate as dependent variable. *DiD* is the interaction of *treatment* and *post*. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. In column (3) we only use observations of loans given out by incumbent lenders and in column (4) only loans given out by new lenders in the post-period. Standard errors in columns (1) to (3) are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels,, respectively.

	(1)	(2)	(3)	(4)
	All loans	All loans	Only loans from incumbent lenders	Only loans from new lenders
DiD	-11.5375***	-3.5113***	-4.4680***	
	[0.4420]	[1.0611]	[0.8722]	
DiD x prime		-4.0594***	-3.4234***	
		[1.1628]	[0.9648]	
DiD x good		-16.5563***	-11.5703***	
		[1.3832]	[1.1845]	
Post x prime		-5.4001***	-0.7288**	
		[0.3101]	[0.2891]	
Post x good		-6.9188***	-2.0179***	
		[0.3048]	[0.2820]	
Treatment				1.3659
				[3.2896]
Treatment x prime				0.9778
				[3.9778]
Treatment x good				0.6559
				[3.5056]
Prime				-11.5627***
				[0.9755]
Good				-10.2619***
				[0.9623]
DiD + DiD x prime		-7.5707***	-7.8914***	
		[0.4767]	[0.4128]	
DiD + DiD x good		-20.0676***	-16.0384***	
		[0.8885]	[0.8024]	
Treatment + treatment x prime				2.3436
				[2.2363]
Treatment + treatment x good				2.0218*
				[1.2112]
Included fixed effects				
Loan type	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	No
Approval month	Yes	Yes	Yes	Yes
Approval year	Yes	Yes	Yes	No
Bank-firm	No	No	Yes	No
Observations	1,299,753	1,299,753	985,962	236,052
Adjusted R^2	0.761	0.761	0.813	0.591

Table 4, Panel B: Loan-level results for interest rate for loans by incumbent lenders and for lender switches

This table presents results with the interest rate as dependent variable. In column (1), we only show loans for firms that received loans from the same lender(s) in the pre- and post-period and *DiD* not add any new lenders in the post-period. In column (2) we show results for loans to firms that added at least one new lender in the post-period, while continuing to receive loans from incumbent lenders. In column (3) we present loans to firms that switched to one or more lenders. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels,, respectively.

	(1) Loans from incumbent lenders for borrowers that do not add new lenders	(2) Loans from incumbent lenders for borrowers that add at least one new lender	(3) Loans for firms that switched to new lenders
DiD	-4.2442***	-1.8858	8.3511*
	[0.9682]	[4.1565]	[4.8839]
DiD x prime	-2.8453***	-8.4137*	-18.2425***
	[1.0769]	[4.3969]	[5.7757]
DiD x good	-11.7180***	-13.7049***	-41.0740***
	[1.3483]	[4.7858]	[5.5115]
Post x prime	-0.9879***	1.3371**	-20.0721***
	[0.3484]	[0.6069]	[1.1274]
Post x good	-1.8027***	-0.9973*	-22.3245***
	[0.3389]	[0.5853]	[1.1206]
DiD + DiD x prime	-7.0895***	-10.2996***	-9.8914***
	[0.4704]	[1.4404]	[3.0998]
DiD + DiD x good	-15.9622***	-15.5907***	-32.7229***
	[0.9389]	[2.3755]	[2.5682]
Included fixed effects			
Loan type	Yes	Yes	Yes
Firm	Yes	Yes	Yes
Approval month	Yes	Yes	Yes
Approval year	Yes	Yes	Yes
Lender-firm	Yes	Yes	No
Observations	717,930	268,032	130,247
Adjusted R^2	0.794	0.798	0.697

Table 4, Panel C: Results for interest rate by type of new lender and lender size

This table presents results with the interest rate of the loan as dependent variable. Columns (1) to (4) provide the breakdown by type of added new lender, and columns (5) and (6) the breakdown by lender size. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) New private lender	(2) New government lender	(3) New foreign lender	(4) New credit union lender	(5) New big lender	(6) New small lender
Treatment	0.5924	-5.5923***	5.5069	10.5462	8.1492**	-14.5350*
	[3.2891]	[1.8902]	[7.1523]	[6.5618]	[3.2659]	[8.2679]
Treatment x prime	5.1002	4.2806**	-1.3393	-10.2790	-7.1404**	17.0344*
	[4.8372]	[2.0130]	[8.5174]	[6.8892]	[3.4937]	[10.3504]
Treatment x good	1.2983	1.8984	1.0273	-7.9181	-5.2560	11.0592
	[3.7124]	[1.9588]	[7.6147]	[6.8715]	[3.3985]	[9.1044]
Prime	-18.6644***	-1.5626***	14.2150***	-3.0382***	-9.0440***	-7.3395***
	[1.6526]	[0.4049]	[1.9363]	[0.8436]	[0.7261]	[2.0034]
Good	-15.6493***	-0.2039	10.4562***	-4.1459***	-8.1574***	-5.8875***
	[1.6249]	[0.3981]	[1.8872]	[0.8845]	[0.7177]	[1.9155]
Treatment + treatment x prime	5.6927	-1.3117*	4.1676	0.2672	1.0088	2.4995
	[3.5449]	[0.6926]	[4.6299]	[2.1045]	[1.2421]	[6.2268]
Treatment + treatment x good	1.8908	-3.6939***	6.5342**	2.6280	2.8932***	-3.4758
	[1.7209]	[0.5122]	[2.6185]	[2.0272]	[0.9424]	[3.7946]
Included fixed effects						
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Approval month	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,126	148,637	24,433	9,739	189,355	46,697
Adjusted R^2	0.447	0.603	0.728	0.434	0.562	0.601

Table 5, Panel A: Loan-level results for loan maturity

This table presents results with the loan maturity as dependent variable. *DiD* is the interaction of *treatment* and *post*. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. In column (3) we only use observations of loans given out by incumbent lenders and in column (4) only loans given out by new lenders in the post-period. Standard errors in columns (1) to (3) are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1)	(2)	(3)	(4)
	All loans	All loans	Only loans from incumbent lenders	Only loans from new lenders
DiD	-1.7820***	-1.2088***	-0.5579**	
	[0.1464]	[0.3267]	[0.2735]	
DiD x prime		-0.9981***	-0.9379***	
		[0.3700]	[0.3088]	
DiD x good		-0.8225*	-0.4233	
		[0.4442]	[0.3448]	
Post x prime		4.4312***	3.1679***	
		[0.1321]	[0.1224]	
Post x good		3.3279***	2.4859***	
		[0.1255]	[0.1154]	
Treatment				0.9884
				[1.4427]
Treatment x prime				-1.2904
				[1.6756]
Treatment x good				-0.9111
				[1.5372]
Prime				5.8427***
				[0.5009]
Good				4.7879***
				[0.4982]
DiD + DiD x prime		-2.2069***	-1.4959***	
		[0.1742]	[0.1438]	
DiD + DiD x good		-2.0313***	-0.9812***	
		[0.3018]	[0.2104]	
Treatment + treatment x prime				-0.3021
				[0.8525]
Treatment + treatment x good				0.0773
				[0.5289]
Included fixed effects				
Loan type	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	No
Approval month	Yes	Yes	Yes	Yes
Approval year	Yes	Yes	Yes	No
Lender-firm	No	No	Yes	No
Observations	966,302	966,302	715,646	236,052
Adjusted R^2	0.624	0.625	0.678	0.105

Table 5, Panel B: Maturity for loans by incumbent lenders and for lender switches

This table presents results with the loan maturity as dependent variable. In column (1), we only show loans for firms that received loans from the same lender(s) in the pre- and post-period and did not add any new lenders in the post-period. In column (2) we show results for loans to firms that added at least one new lender in the post-period, while continuing to receive loans from incumbent lenders. In column (3) we present loans to firms that switched to one or more new lenders. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) Loans from incumbent lenders for borrowers that do not add new lenders	(2) Loans from incumbent lenders for borrowers that add at least one new lender	(3) Loans for firms that switched to new lenders
DiD	-0.4776	-0.9049	-3.9578
	[0.3077]	[1.1271]	[2.7714]
DiD x prime	-0.9989***	-0.3788	4.5764
	[0.3534]	[1.1839]	[2.9728]
DiD x good	-0.4365	1.1566	-2.9070
	[0.3890]	[1.3277]	[3.5983]
Post *x prime*	3.2118***	3.1765***	7.9617***
	[0.1492]	[0.2787]	[0.6537]
Post *x good*	2.3050***	2.4287***	5.6633***
	[0.1383]	[0.2681]	[0.6589]
DiD + DiD x prime	-1.4765***	-1.2837***	0.6186
	[0.1741]	[0.3635]	[1.0851]
DiD + DiD x good	-0.9141***	0.2517	-6.8648***
	[0.2383]	[0.7030]	[2.3110]
Included fixed effects			
Loan type	Yes	Yes	Yes
Firm	Yes	Yes	Yes
Approval month	Yes	Yes	Yes
Approval year	Yes	Yes	Yes
Lender-firm	Yes	Yes	No
Observations	517,100	194,053	86,467
Adjusted R^2	0.652	0.632	0.703

Table 5, Panel C: Results for loan maturity by type of new lender and lender size

This table presents results with the loan maturity as dependent variable. Columns (1) to (4) provide the breakdown by type of added new lender, and columns (5) and (6) the breakdown by lender size. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) New private lender	(2) New government lender	(3) New foreign lender	(4) New credit union lender	(5) New big lender	(6) New small lender
Treatment	1.8783	1.1809	-3.3138***	-0.3809	-2.5876*	4.9027**
	[1.1601]	[1.3679]	[1.2107]	[1.9694]	[1.4413]	[2.0251]
Treatment x prime	-1.3714	-0.4388	2.7445*	1.3233	3.1034**	-4.9280*
	[1.4830]	[1.4481]	[1.4595]	[2.4415]	[1.5413]	[2.5809]
Treatment x good	-1.5278	1.3475	1.8595	3.5333	2.1481	-1.7862
	[1.2786]	[1.4342]	[1.4116]	[2.3016]	[1.5198]	[2.2722]
Prime	3.4366***	1.4120***	-1.3741**	-2.9643***	2.6081***	4.8825***
	[0.5492]	[0.2808]	[0.6231]	[0.7155]	[0.3106]	[0.6712]
Good	2.7721***	0.0442	-0.9265	-3.6663***	1.8912***	3.7433***
	[0.5166]	[0.2876]	[0.6180]	[0.9317]	[0.3115]	[0.6301]
Treatment + treatment x prime	0.5069	0.7421	-0.5693	0.9424	0.5158	-0.0253
	[0.9237]	[0.4751]	[0.8101]	[1.4407]	[0.5469]	[1.5979]
Treatment + treatment x good	0.3505	2.5284***	-1.4543**	3.1524***	-0.4395	3.1165***
	[0.5369]	[0.4299]	[0.7250]	[1.1639]	[0.4826]	[1.0223]
Included fixed effects						
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Approval month	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,126	148,637	24,433	9,739	189,355	46,697
Adjusted R^2	0.161	0.042	0.141	0.099	0.088	0.043

Table 6, Panel A: Loan-level results for collateralization

This table presents results with the existence of collateral as dependent variable. The sample size is smaller because reporting of collateralization is voluntary in the cases of loans without collateral. *DiD* is the interaction of *treatment* and *post*. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. In column (3) we only use observations of loans given out by incumbent lenders and in column (4) only loans given out by new lenders in the post-period. Standard errors in columns (1) to (3) are clustered on the firm-level and shown in brackets, in column (4) we show robust standard errors in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1)	(2)	(3)	(4)
	All loans	All loans	Only loans from incumbent lenders	Only loans from new lenders
DiD	-0.1192***	-0.1254***	-0.1001***	
	[0.0047]	[0.0100]	[0.0088]	
DiD x prime		-0.0122	0.0227**	
		[0.0118]	[0.0102]	
DiD x good		0.0417***	0.0298**	
		[0.0138]	[0.0119]	
Post x prime		-0.0288***	-0.0832***	
		[0.0043]	[0.0038]	
Post x good		-0.0251***	-0.0494***	
		[0.0040]	[0.0037]	
Treatment				-0.0314
				[0.0413]
Treatment x prime				0.0472
				[0.0444]
Treatment x good				0.0533
				[0.0437]
Prime				0.0408***
				[0.0086]
Good				0.0078
				[0.0087]
DiD + DiD x prime		-0.1376***	-0.0774***	
		[0.0062]	[0.0052]	
DiD + DiD x good		-0.0836***	-0.0703***	
		[0.0094]	[0.0080]	
Treatment + treatment x prime				0.0158
				[0.0163]
Treatment + treatment x good				0.0219
				[0.0142]
Included fixed effects				
Loan type	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	No
Approval month	Yes	Yes	Yes	Yes
Approval year	Yes	Yes	Yes	No
Lender-firm	No	No	Yes	No
Observations	799,401	799,401	609,853	139,412
Adjusted R^2	0.561	0.561	0.659	0.070

Table 6, Panel B: Loan-level results for collateralization for loans by incumbent lenders and for lender switches

This table presents results with the existence of collateral as dependent variable. The sample size is smaller because reporting of collateralization is voluntary in the cases of loans without collateral. In column (1), we only show loans for firms that received loans from the same lender(s) in the pre- and post-period and did not add any new lenders in the post-period. In column (2) we show results for loans to firms that added at least one new lender in the post-period, while continuing to receive loans from incumbent lenders. In column (3) we present loans to firms that switched to one or more new lenders. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) Loans from incumbent lenders for borrowers that do not add new lenders	(2) Loans from incumbent lenders for borrowers that add at least one new lender	(3) Loans for firms that switched to new lenders
DiD	-0.1075***	0.0459	-0.0558
	[0.0094]	[0.0636]	[0.0945]
DiD x prime	0.0204*	-0.0694	0.0687
	[0.0112]	[0.0667]	[0.1097]
DiD x good	0.0370***	-0.0935	0.1642
	[0.0132]	[0.0690]	[0.1078]
Post x prime	-0.0741***	-0.0896***	0.1974***
	[0.0047]	[0.0088]	[0.0193]
Post x good	-0.0472***	-0.0471***	0.1835***
	[0.0045]	[0.0085]	[0.0188]
DiD + DiD x prime	-0.0871***	-0.0235	0.0130
	[0.0061]	[0.0203]	[0.0557]
DiD + DiD x good	-0.0704***	-0.0477*	0.1084**
	[0.0093]	[0.0269]	[0.0518]
Included fixed effects			
Loan type	Yes	Yes	Yes
Firm	Yes	Yes	Yes
Approval month	Yes	Yes	Yes
Approval year	Yes	Yes	Yes
Lender-firm	Yes	Yes	No
Observations	457,615	149,630	77,223
Adjusted R^2	0.604	0.599	0.567

Table 6, Panel C: Results for collateralization by new lender type

This table presents results with the existence of collateralization as dependent variable. The sample size is smaller because reporting of collateralization is voluntary in the cases of loans without collateral. Columns (1) to (4) provide the breakdown by type of added new lender, and columns (5) and (6) the breakdown by lender size. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) New private lender	(2) New government lender	(3) New foreign lender	(4) New credit union lender	(5) New big lender	(6) New small lender
Treatment	-0.0661	0.0198	-0.0540***	-0.1298***	-0.0168	-0.1703***
	[0.0433]	[0.0363]	[0.0196]	[0.0192]	[0.0317]	[0.0342]
Treatment x prime	0.0777	-0.0093	0.0587	0.0710	0.0423	0.1662***
	[0.0601]	[0.0378]	[0.0402]	[0.0456]	[0.0344]	[0.0604]
Treatment x good	0.0379	0.0050	0.0195	0.0444	0.0477	0.1061**
	[0.0512]	[0.0379]	[0.0228]	[0.0370]	[0.0338]	[0.0426]
Prime	-0.1090***	0.0246***	-0.0064	0.0136	0.0071	-0.0158
	[0.0159]	[0.0083]	[0.0107]	[0.0218]	[0.0073]	[0.0191]
Good	-0.0927***	0.0039	-0.0003	0.0417*	-0.0124*	-0.0047
	[0.0153]	[0.0083]	[0.0102]	[0.0233]	[0.0072]	[0.0189]
Treatment + treatment x prime	0.0116	0.0105	0.0047	-0.0588	0.0254*	-0.0042
	[0.0417]	[0.0106]	[0.0350]	[0.0413]	[0.0134]	[0.0498]
Treatment + treatment x good	-0.0282	0.0248**	-0.0344***	-0.0854***	0.0309***	-0.0642**
	[0.0274]	[0.0107]	[0.0117]	[0.0323]	[0.0117]	[0.0255]
Included fixed effects						
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Approval month	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,503	90,551	15,005	8,079	120,835	18,577
Adjusted R^2	0.010	0.092	0.008	0.009	0.077	0.005

Table 7, Panel A: Results for arrears > 30 days

This table presents results with the probability of a loan being in arrears for more than 30 days as dependent variable. *DiD* is the interaction of treatment and post. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. In column (3) we only use observations of loans given out by incumbent lenders and in column (4) only loans given out by new lenders in the post-period. Standard errors in columns (1) to (3) are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1)	(2)	(3)	(4)
	All loans	All loans	Only loans from incumbent lenders	Only loans from new lenders
DiD	-0.0089***	-0.0053	-0.0548***	
	[0.0032]	[0.0130]	[0.0134]	
DiD x prime		0.0053	0.0587***	
		[0.0134]	[0.0139]	
DiD x good		-0.0102	0.0473***	
		[0.0139]	[0.0146]	
Post x prime		0.0451***	-0.0053	
		[0.0036]	[0.0038]	
Post x good		0.0508***	0.0010	
		[0.0036]	[0.0038]	
Treatment				0.1303***
				[0.0384]
Treatment x prime				-0.1271***
				[0.0404]
Treatment x good				-0.1645***
				[0.0395]
Prime				-0.1613***
				[0.0097]
Good				-0.1113***
				[0.0098]
DiD + DiD x prime		-0.0000	0.0040	
		[0.0036]	[0.0037]	
DiD + DiD x good		-0.0155***	-0.0075	
		[0.0052]	[0.0059]	
Treatment + treatment x prime				0.0032
				[0.0125]
Treatment + treatment x good				-0.0342***
				[0.0091]
Included fixed effects				
Loan type	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	No
Approval month	Yes	Yes	Yes	Yes
Approval year	Yes	Yes	Yes	No
Lender-firm	No	No	Yes	No
Observations	1,299,753	1,299,753	985,962	236,052
Adjusted R^2	0.438	0.438	0.491	0.022

Table 7, Panel B: Loan-level results for arrears > 30 days for loans by incumbent lenders and for lender switches

This table presents results with the probability of a loan being in arrears for more than 30 days as dependent variable. In column (1), we only show loans for firms that received loans from the same lender(s) in the pre- and post-period and did not add any new lenders in the post-period. In column (2) we show results for loans to firms that added at least one new lender in the post-period, while continuing to receive loans from incumbent lenders. In column (3) we present loans to firms that switched to one or more new lenders. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) Loans from incumbent lenders for borrowers that do not add new lenders	(2) Loans from incumbent lenders for borrowers that add at least one new lender	(3) Loans for firms that switched to new lenders
DiD	-0.0557***	-0.0537	0.1354***
	[0.0152]	[0.0545]	[0.0493]
DiD x prime	0.0629***	0.0453	-0.1419***
	[0.0158]	[0.0555]	[0.0512]
DiD x good	0.0498***	0.0534	-0.1605***
	[0.0167]	[0.0566]	[0.0505]
Post x prime	-0.0086*	-0.0093	0.2093***
	[0.0047]	[0.0081]	[0.0114]
Post x good	-0.0027	-0.0080	0.2016***
	[0.0047]	[0.0080]	[0.0116]
DiD + DiD x prime	0.0072	-0.0084	-0.0065
	[0.0044]	[0.0102]	[0.0140]
DiD + DiD x good	-0.0059	-0.0004	-0.0251**
	[0.0071]	[0.0153]	[0.0110]
Included fixed effects			
Loan type	Yes	Yes	Yes
Firm	Yes	Yes	Yes
Approval month	Yes	Yes	Yes
Approval year	Yes	Yes	Yes
Lender-firm	Yes	Yes	No
Observations	717,930	268,032	130,247
Adjusted R^2	0.402	0.447	0.425

Table 7, Panel C: Results for arrears > 30 days by new lender type

This table presents results with the probability of a loan being in arrears for more than 30 days as dependent variable. Columns (1) to (4) provide the breakdown by type of added new lender, and columns (5) and (6) the breakdown by lender size. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. Standard errors are clustered on the firm-level and shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) New private lender	(2) New government lender	(3) New foreign lender	(4) New credit union lender	(5) New big lender	(6) New small lender
Treatment	0.1238***	0.0576	0.0877	0.1417	0.0821*	0.2201***
	[0.0437]	[0.0789]	[0.0740]	[0.1110]	[0.0438]	[0.0709]
Treatment x prime	-0.1297***	-0.0521	-0.0654	-0.1134	-0.0705	-0.2368***
	[0.0481]	[0.0809]	[0.0776]	[0.1174]	[0.0462]	[0.0726]
Treatment x good	-0.1290***	-0.1125	-0.0945	-0.1693	-0.1169***	-0.2453***
	[0.0465]	[0.0801]	[0.0762]	[0.1151]	[0.0451]	[0.0724]
Prime	-0.1901***	-0.2089***	-0.2781***	-0.3080***	-0.2065***	-0.0914***
	[0.0146]	[0.0144]	[0.0166]	[0.0195]	[0.0107]	[0.0138]
Good	-0.1496***	-0.1501***	-0.2357***	-0.2592***	-0.1509***	-0.0625***
	[0.0145]	[0.0145]	[0.0166]	[0.0199]	[0.0108]	[0.0136]
Treatment + treatment x prime	-0.0059	0.0055	0.0223	0.0283	0.0116	-0.0167
	[0.0202]	[0.0178]	[0.0231]	[0.0381]	[0.0147]	[0.0153]
Treatment + treatment x good	-0.0052	-0.0549***	-0.0068	-0.0276	-0.0348***	-0.0252*
	[0.0158]	[0.0137]	[0.0176]	[0.0303]	[0.0107]	[0.0144]
Included fixed effects						
Loan type	Yes	Yes	Yes	Yes	Yes	Yes
Approval month	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,126	148,637	24,433	9,739	189,355	46,697
Adjusted R^2	0.034	0.022	0.071	0.088	0.024	0.025

Table 8, Panel A: Firm-level results for the number of employees

This table shows results for the number of employees as of December as dependent variable. *DiD* is the interaction of *treatment* and *post*. Fixed effects are included as indicated in the table. All variables are explained in Table 1. Standard errors clustered on the firm-level are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) All firms	(2) All firms	(3) Firms with only loans from incumbent lenders	(4) Firms with loans from at least one new lender
DiD	0.0678***	0.1132***	0.1387***	0.0691
	[0.0068]	[0.0229]	[0.0266]	[0.0442]
DiD x prime		-0.0715***	-0.1062***	0.0120
		[0.0248]	[0.0287]	[0.0488]
DiD x good		-0.0273	-0.0586*	0.0257
		[0.0252]	[0.0300]	[0.0471]
Post	0.0066***	-0.1109***	-0.1321***	-0.0763***
	[0.0013]	[0.0052]	[0.0067]	[0.0082]
Post x prime		0.1370***	0.1509***	0.1135***
		[0.0056]	[0.0072]	[0.0088]
Post x good		0.1230***	0.1309***	0.1067***
		[0.0055]	[0.0071]	0.1135***
DiD + DiD x prime		0.0417***	0.0325***	0.0811***
		[0.0096]	[0.0108]	[0.0205]
DiD + DiD x good		0.0859***	0.0801***	0.0948***
		[0.0106]	[0.0141]	[0.0161]
Firm fixed effects	Yes	Yes	Yes	Yes
Observations	323,416	323,416	192,758	130,658
Adjusted R^2	0.863	0.864	0.858	0.869

Table 8, Panel B: Firm-level results for the number of employees for firms that have at least one new lender in the post-period, by new lender type

This table presents results for the number of employees per December. Columns (1) to (4) provide a breakdown by lender type, while columns (5) and (6) provide a breakdown by lender size. All variables are explained in Table 1. Control variables and fixed effects are included as indicated in the table. Standard errors clustered on the firm-level are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

	(1) New private lender	(2) New government lender	(3) New foreign lender	(4) New credit union lender	(5) New big lender	(6) New small lender
DiD	0.1400**	0.0600	-0.0842	0.3495***	0.0597	0.1156
	[0.0611]	[0.0753]	[0.1184]	[0.1171]	[0.0460]	[0.1002]
DiD x prime	-0.0511	0.0099	0.2462*	-0.4147***	0.0479	-0.1362
	[0.0729]	[0.0802]	[0.1302]	[0.1379]	[0.0514]	[0.1077]
DiD x good	0.0026	0.0290	0.1388	-0.2229*	0.0359	-0.0187
	[0.0661]	[0.0790]	[0.1285]	[0.1336]	[0.0494]	[0.1064]
Post	-0.1326***	-0.0113	-0.0461**	-0.1443***	-0.0602***	-0.1185***
	[0.0155]	[0.0113]	[0.0204]	[0.0225]	[0.0092]	[0.0158]
Post x prime	0.1594***	0.0605***	0.0730***	0.1744***	0.0993***	0.1518***
	[0.0166]	[0.0120]	[0.0222]	[0.0256]	[0.0098]	[0.0176]
Post x good	0.1447***	0.0529***	0.0912***	0.1670***	0.0946***	0.1342***
	[0.0163]	[0.0118]	[0.0216]	[0.0244]	[0.0096]	[0.0169]
DiD + DiD x prime	0.0889**	0.0700**	0.1620***	-0.0652	0.1076***	-0.0206
	[0.0398]	[0.0276]	[0.0541]	[0.0727]	[0.0230]	[0.0393]
DiD + DiD x good	0.1426***	0.0891***	0.0546	0.1266**	0.0955***	0.0969***
	[0.0251]	[0.0238]	[0.0499]	[0.0643]	[0.0179]	[0.0356]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	38,810	74,700	20,190	10,342	111,814	25,856
Adjusted R^2	0.863	0.876	0.859	0.863	0.870	0.865