Banks and Firms: Evidence from a legal reform altering contract design *

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Abstract

Banks are unique in designing loan contracts. Contract design determines liquidation and continuation decisions of firms. Smaller firms are often the weaker party when interacting with their banks. We study a legal reform that aims to improve small firms' bargaining position by altering the contractual environment. The new law gives small firms the right to prepay loans against a contractually specified penalty and requires banks to offer firms' best-suited loan type. Using this quasi-natural experiment, we show that, while the legal reform increases overall credit availability, banks dampen the effect of the act by tilting their credit supply to loans that are unaffected by the legal change, i.e., credit lines. Using bank-firm-credit-type data, we show that banks reduce the supply of term loans by 0.7% while credit lines increase by 4%. This effect is more pronounced for borrowers with longer relationships. Our results show that reforms generate unintended consequences since banks strategically try to undo part of the regulation.

Keywords: Credit composition, SME financing, legal reform, bank credit, contract design

JEL classification: G21, G28

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1 Introduction

The design of bank-loan contracts determines a bank's ability to liquidate firms, or to continue them at initial or renegotiated loan terms (e.g., Rajan, 1992; Gorton and Kahn, 2000). The legal framework determines the feasible outcomes in the contracting space. In this paper, we study how a legal reform introducing prepayment options on loans to firms modifies banks and firms contract choices as well as firm outcomes.

The legal reform affects bank loans to small firms. Small firms are the engine of economic growth. In most countries, banks are central in the credit provision to small and medium sized firms (SMEs). Firms in general and SMEs in particular are often in a weak position relative to their main bank as they have limited access to alternative sources of finance stemming from asymmetric information and limited competition. In this paper, we study the impact of a legal reform that aimed to improve SMEs bargaining position relative to the bank, and investigate the effects on credit provision, the composition of credit (credit lines versus term loans), and other lending terms.

In particular, the legal reform aimed to resolve inefficiencies between creditors and borrowers by requesting banks to provide the best-suited loan type to SMEs, and to promote pre-contractual fairness for SMEs by giving to borrowers the right to 'buy back' the project by prepaying their loan against a contractually specified penalty of six of months interest rate. The aim was to stimulate SMEs access to credit by ameliorating their bargaining position, since it would be easier for firms to 'reposses' their project by switching across financial institutions or obtain better loan terms. However, by embedding in all contracts the possibility to prepay a loan, the act altered the contracting environment and removed the ability for SMEs to make credible long-term commitment to a bank-firm relationship. This implicitly increases competition across financial institutions, which may lead either to strengthening of bank-firm relationships or to have the opposite effect essentially shifting the banking model from relationship to transactional (see, e.g., Sharpe, 1990; Rajan, 1992; von Thadden, 1995; Boot and Thakor, 2000). The law change can thus be seen as a shock to competition between banks, which from the seminal works of Petersen and Rajan (1994, 1995) and Boot and Thakor (2000), can lead either to lower or higher credit availability.

Furthermore, banks may have incentives to use their bargaining power to shift credit to least affected products across borrowers who were marginally indifferent between affected and non affected credit types. In particular, before the law change, banks may have incentives to oversupply term loans as these were protected from prepayments and maximize their loan book. However, in

the presence of prepayment options, the equilibrium supply of loan types changes and firms could end up with another type of credit such as credit lines. With credit lines, it is the borrower who decides how much to draw from it at any point in time.

In this paper we use as laboratory experiment a legal change that altered the contracting environment for newly issued loans to SMEs in Belgium. We use this legal change to investigate the equilibrium implications on contract features, and study the reactions of banks which could be towards mitigating the regulatory impact and thus creating unintended consequences of the implemented legal change. To study the impact of the legal reform on firms and banks, we employ a proprietary dataset provided by the National Bank of Belgium (hereafter NBB). We employ more than 273,000 bank-firm-credit mode observations at monthly frequency, that cover almost all banks and non-financial firms in Belgium. We further augment our data set by annual firm balance sheet data to obtain firm controls. This information allows us to saturate our model with fixed effects in order to control for the demand side (see, e.g., Khwaja and Mian, 2008; Degryse et al., 2019).

Our analysis generates insights on how banks respond to the modified contractual environment by adjusting the composition of their credit supply. The reform however is a shock on both the supply and the demand side, affecting the overall volume, the composition of credit and interest rates. From a firm's perspective, term credit enjoys improved contractual features as it allows to 'buy back' the project at a contractually (i.e., 6 months loan rate) specified cost. This implies that firms should exhibit an increased demand for term loans, possibly stemming from substitution away from credit lines. However, our empirical results, indicate the opposite as the importance of term loans drops relative to credit lines compared to firms not subject to the legal change. The estimated causal impact of the reform to the supply of term loans indicates a drop in the range of 0.7%, when we account for the typical repayment scheme of term loans. At the same time this credit reduction, is combined by an increase in the growth of credit lines by more than 4%. Banks thus seem to "undo" the modified contractual environment by supplying credit lines rather than term loans. Economically speaking, credit lines are unaffected by the legal change as it is the borrower who decides on how much to draw upon the line at any point in time, and 'buying back' the project is always possible. From the supply side, the reform can be seen as a shock to competition, since banks cannot expect firms to remain in a long-term relationship, essentially shifting the banking model from relationship to transactional. This is due to the structural changes in the contract design, which gives to firms the possibility to early repay their loans but at the same time, because firms cannot credibly commit to long-term projects, reduces banks' incentive to "incur" the cost of

screening in order to learn the true quality of the borrower (see, e.g., Sharpe, 1990; von Thadden, 1995).

The expected effect of the act in the overall credit provision is thus theoretically ambiguous, since it depends on the credit demand and supply as well as the intensity of credit substitution. Our empirical findings indicate an increase of the credit growth by 2.2%, which even though it is in the direction that the act was aiming for, the credit compostion effects that led to this outcome were unanticipated. The strategic reaction of financial institutions to mitigate the regulatory impact, which creates unanticipated effects and can lower the effectiveness of regulation is in the core of our research investigation. Finally, we document a drop in the probability of default (PD), by 0.2 p.p. and an increase in the collateral pledged in the magnitude of 4%. Two possible explanations drive our findings. First, banks are obliged to gather additional information on their borrowers, which makes them more prudent and is reflected on the lower PD and higher collateral. Second, the option to repay and "buyback" the project incentivizes borrowers to reduce risk-taking which further decreases the estimates of the PD that we report (Gorton and Kahn, 2000).[1]

In our analysis we identify two channels which both act towards mitigating the regulatory impact on banks. First, banks increase interest rates on term loans to affected borrowers by 6 basispoints relative to non-affected firms. This reflects the new fundamentals of term loans and is consistent with stylized results from contract theory in which banks adjust rates to compensate for the possibility of strategic switching (Egli et al., 2006).[2] Second, banks shift credit supply towards credit lines, which are essentially unaffected by the reform.

The shift in credit composition is not equal across borrowers. First, exploiting their existing relationships and the bargaining power stemming from it, we document an increase in the volumes for credit lines in the range of 11% more for treated firms with longer bank-firm relationships.[3] This is consistent with Rajan (1992), who documents that banks obtain bargaining power over the customers with whom they have a longer relationship. After the reform, within the pool of projects where the borrower is indifferent between a credit line or a term loan, the bank uses previous relationships in order to sell more credit lines. Prior to the legal act, the bank could favor term loans in order to maximize its lending portfolio, since these were protected from prepayment. In

[1] Increased collateral serves also as a mechanism that locks customers to their particular credit type, reducing the likelihood of switching within and across banks.

[2] Egli et al. (2006), study the implications in the decision of an entrepreneur who faces relationship or arm's-length financing, under the friction of strategic defaults.

[3] Firms with high cash flows are more likely to use credit lines (see, e.g., Sufi, 2009). However, our results are not driven by an increase in the demand from high cash flow firms (for more details, see, Section 6.2).

addition borrowers that would be rationed from term loans due to higher interest rates, may be offered credit lines with similar maturity. Second, our results are more pronounced for smaller firms, which are financially dependent and rely more to the suggestions of their bank regarding the optimal product for their project, as opposed to larger SMEs who may have a greater in-house financial expertise.

The reform also has implications for the maturity of a loan as the financial penalty for loan prepayment is a fixed six months interest rate. This implies that the prepayment option has larger value to the firm for longer maturity loans.[4] In support to the claim that banks modify their supply in order to mitigate the new act, we document empirically that the drop in term loans is driven exclusively by *Long-term* term loans, whereas we see an increase in *Long-term* credit lines.

Finally, we examine the impact of the reform on firms' asset growth and investment to study whether the modified contractual environment creates real effects for the economy. Firms affected by the reform, document a lower asset growth, and investments suggesting that the pricing and credit supply changes generate some negative real effects.

Our paper contributes to several strands of literature. First, we build on the literature that links law and finance, which traditionally has focused on bankruptcy reforms and creditor-borrower legal rights. Changes in law that aim to ameliorate debtors' position create incentives to lenders to mitigate the regulation, by either pricing loans differently i.e., increase interest rates (Cruz et al., 2020), or leading to a redistribution of credit or debt (see, e.g., Cerqueiro and Penas, 2017; von Lilienfeld-Toal et al., 2012).

Second, we build on a large empirical literature studying strategic behavior of banks to mitigate effects of regulation which affect their optimal asset allocation. Boyson et al. (2016) focus on how banks may use specific securities in order to evade capital requirements, while Acharya et al. (2013) investigate how banks employ asset backed commercial paper conduits to reduce the asset size of their balance sheet, but not their risk exposure. Boyer and Kempf (2019) focus on regulatory arbitrage stemming from banks' strategic selection of the location in which they will offer their services, fact that can have implications to financial integration. Our main differentiation to the above literature is that we investigate how banks may strategically supply different products, as a tool to mitigate the impact on the contractual environment.

Third, we extend on a very small literature that focuses on prepayment indemnities. While

[4] A borrower will exercise this option if the current interest rate is lower than the rate upon origination, adjusting for the additional penalty and remaining maturity. Hence products with higher maturity are more susceptible to the effects of the legal act.

most of the previous work examines mainly penalties on mortgages and their implications either to riskiness, or to refinancing and switching (e.g., Dunn and Spatt, 1985; Mayer et al., 2013; Brunetti et al., 2017), we focus on the implications of prepayment penalties in a corporate setting after a legal reform. The paper closest to our work is by Cruz et al. (2020), where the authors investigate the implications of prohibiting prepayment indemnities, in Colombia. Their main interest is on the probability of repayment and the effect on interest rates. We focus mainly on changes in the composition of credit as well as to implications in the probability of default and collateral. To do that, we employ a proprietary data set provided by the NBB, the corporate credit registry, which contains information at monthly frequency, of all credit outstanding at the bank-firm-credit mode level.

Our paper also links to the literature on bank-loan contract design (see, e.g., Sharpe, 1990; Rajan, 1992; Berglöf and Von Thadden, 1994; von Thadden, 1995; Gorton and Kahn, 2000; Egli et al., 2006). This literature has extensively investigated pricing, maturity and collateralization of debt contracts (see, e.g., Benmelech and Bergman, 2008; Fan et al., 2012; Berg et al., 2016). Nini et al. (2012) document that firms that violate covenant credit agreements suffer from a decline in shareholder payouts and increased CEO turnover, while Chava and Roberts (2008) document a drop in capital investment. The legal reform investigated in our paper reduces the bargaining power of banks over firms by weakening the hold up problem which has implications to the ability of banks to extract rents from their borrowers and therefore actions that may happen ex-post (i.e., repayment) can have real implications ex-ante to the composition of credit.

Finally, this paper contributes on a large literature related to relationship lending and different conditions that may lead to a switching (e.g., Petersen and Rajan, 1994; Berger and Udell, 1995; Ioannidou and Ongena, 2010; Beck et al., 2018). However, while most of the previous work investigates how bank relationships can provide a helping hand or extract more rents in the form of higher interest rates, we document how banks can use their existing power within their borrowers, to shift credit allocation to specific type of products following limitations on the contracting space banks and firms can employ.

The reminder of the paper is organized as follows. In Section 2 we describe the legal reform and formulate our hypotheses. Section 3 describes the data, and the various measures that we use. Our identification, methodology, and econometric model are presented in Section 4, while Section 5 contains the results of our main analysis. In Section 6 we investigate the channels that determine our empirical findings, while in Section 7 we study the real effects of the reform to the economy

focusing on growth and investment. Finally Section 8 concludes this paper.

2 The New Act and Hypotheses Formulation

In this section, we discuss the details of the quasi-natural experiment, e.g., the legal reform. Next we build and formulate our main hypotheses.

2.1 The Legal Reform

We start by providing the details of the legal reform that took place in Belgium on January 10^{th}, 2014. This reform focused on bank loans to SMEs. The legal change makes prepayments of newly originated loans possible and less costly. It furthermore requires a bank to provide more information on why a specific loan type suits the firm, and why a bank rejects a loan application. On December 21^{st}, 2013, the act was voted by the Belgian Parliament and went into force in two waves; January 10^{th}, for the clause related to loan prepayment, and March 1^{st} for the ones related to information.[5] The general aim of the act is to improve SMEs' bargaining position relative to banks and to promote contractual fairness by improving the informational environment and by reducing the prepayment indemnities (see, e.g., Ferrando and Mulier, 2019).[6].

The law introduces three novel provisions regarding new loans issued to SMEs. First, banks need to provide borrowers information in writing regarding the types of credit suitable to their project, including information on the crucial contract features of the loan products that are relevant to the firm. In practice, in many loan applications banks did not propose alternative credit types. It was actually in the discretion of the bank not to recommend additional products, if believed that the credit type applied for was the best one. Hence banks retained flexibility regarding this clause. Second, in case of a loan rejection, banks must provide better information and motivation on the risk assessment and the reasons for rejection. Third, the right for prepayment of a term

[5] Apart from the short duration between the vote of the act and the implementation, also a search in the financial press did not reveal important discussion about the reform prior to the vote. Thus it is fair to assume that the law was unanticipated to both banks and firms.

[6] The intention of the law was also to stimulate credit provision to SMEs which after the sovereign crisis had been reduced.

loan was transferred from the bank to the firm.[7] The regulator changed the design of contracts, by embedding the prepayment option at a low cost to be exercised at will of the borrower and hence reduced the bargaining power of the bank which accrues over the lifespan of the bank-borrower relationship; financial institutions offer competitive rates at the initial period, and as soon as they obtain the "informational advantage", threaten to terminate funding unless the firm agrees to more favorable terms (Sharpe, 1990).[8] Prior to the legal act, firms were bound by their original contract, which further increased the leverage of the bank towards the firm. Prepayment penalties were regulated in Belgium, for credit types that qualify as "interest rate loans", i.e., term loans, by the Article 1907bis of the Civil Code. The penalties could not exceed six months of interest over the prepaid amount. The Article 1907bis was implemented to all term loans regardless the amount outstanding. However, banks held the right to refuse the loan prepayment, given the six months interest rate stipulation, essentially canceling the beneficial provision of the law.[9] As a result, banks could deny firms to prepay term loans, for example when firms experience a positive cash-flow shock or when interest rates were decreasing. Credit agreements that were not classified as a term loan, i.e., credit lines (this is demandable credit including overdrafts and credit lines, for short "credit lines") were not subject to Article 1907bis of the Civil Code and thus not regulated. Table 1 depicts schematically the legal reform for both types of loans.

*** **Please Insert Table 1 around here** ***

It is important to mention that the legal reform did not apply to outstanding loans, but only to new loan originations contracted after the reform. In Figure 1 we plot the evolution of (log) volume (normalized to one, for January 2014, i.e., the date of the event) for the overall credit provision as well as its decomposition in credit lines and term loans for the whole banking system excluding

[7] Under the new law, credit lines below EUR1 million are classified as credit agreements regulated by the new act, and therefore firms obtain the right to early repayment. However, the effect of the reform for these should be minimal, since credit lines in the majority of our sample have maturity less than a year. So the benefit to borrowers stemming from the third clause is expected to be limited. This is further strengthened, since credit lines already allow for flexibility in taking up and repaying loans. Thus we argue that credit lines, essentially are not "economically" affected by the new act. For loan agreements classified as credit lines, above EUR1 million, the prepayment indemnities are set bilaterally, but should be in the spirit and under the calculation modalities set by the Code of Conduct of the act. The Code of Conduct determines the minimum amount of information that the bank has to ask the firm in order to evaluate the firm's loan application.

[8] Sharpe (1990) discusses the inefficiencies that arise from the informational advantage of banks in lengthy relationships, which may lead to inefficient capital allocation and can be mitigated by "implicit contracts".

[9] The firm was essentially bounded by the original contract.

loans granted to firms with a turnover above EUR50 million.[10]

*** **Please Insert Figure 1 around here** ***

2.2 Hypotheses Development

The new act on SME financing, reformed the legal environment related to credit provision to small and medium enterprises. The specifics of the reform, indicate that the law affected credit supply as well as demand, impacting the equilibrium volume, composition of credit and interest rates. The SME refinancing law aims to improve credit availability for SMEs (e.g., information and prepayment related clauses), but the overall effects of the reform are directly related to banks' responses who were subject to a negative shock to their bargaining position which increased bank competition. The structural changes in the contract design for term loans, limits firms in making credible long-term commitments reducing the efficiency of contracts and potentially affect credit provision (see, e.g., Berglöf and Von Thadden, 1994). Petersen and Rajan (1995) argue that increasing competition, in this case stemming from the legal shock which limited the contracting space of firms, can reduce relationship specific investments and therefore credit availability. This implicitly changes the banking model from relationship to transactional, driving term loan interest rates up. Boot and Thakor (2000) in contrast argue that an increase in competition may stimulate banks to invest in relationship lending to shield their rents, and therefore produce greater credit availability, and possibly products that are better suited to the firm. See Elsas (2005), Degryse and Ongena (2007) or Presbitero and Zazzaro (2011) for empirical investigation of the impact of banking competition and relationship lending.

Overall, after the new act firms should increase the demand for term loans where they enjoy greater benefits following the new regulation, while banks should try to circumvent the implications of the reform, since it reduces their ability to invest on firm relationships which later on they can use to extract rents. Which of the two opposite forces (supply or demand) dominates is an empirical question that we intend to investigate further. We find evidence that overall credit provision increased, however the driving force of this increment may be completely different than what was intended. The reform may increase credit demand, but the specifics of the legal change created incentives that alter the composition of credit; banks aim to "undo" the impact of the act either by

[10]The reason for the exclusion is related to our identification strategy, see, Section 4. These correspond to less than 1% of our sample.

increasing term loan interest rates, or by switching from term loans to credit lines, where the effect of the reform is minimal and in addition borrowers decide themselves how much to draw on.[11] As a result the effect on the overall output is subject to multiple forces acting together. Furthermore, the credit composition effect should be more pronounced where banks have stronger bargaining power (i.e., firms with lengthier relationships).

Not only the composition of credit could be affected. Due to the second clause, banks need to underpin to a greater degree their response to credit requests implying that it may improve their estimates of the actual default risk of the incumbent borrowers. This could induce banks towards a more prudent behavior which in combination of offering better suited products and aiming to keep their customers (additional collateral), translates in a drop in the probability of default or an increase in the collateral pledged for the accepted loans.[12] In addition, banks and firms commit to each other through other mechanisms such as asking collateral, while firms reduce risk-taking as they are more likely to "repossess" their project leading to a lower probability of default (Gorton and Kahn, 2000). Based on the above discussion, we formulate the following hypotheses, each stemming from the different aspects of the reform:

Hypothesis 1 (H1): *The legal reform, due to the change in the right for early prepayment embed in the new contract design, makes it harder for a firm to commit to a project, implicitly increasing bank competition. Banks try to mitigate the effect of the law by reducing the supply of loans affected by the reform (i.e., term loans, in particular those with longer maturity), shift credit allocation to less affected products (i.e., credit lines) and adjust interest rates to reflect the new fundamentals.*

Hypothesis 2 (H2): *Following the legal reform, borrowers reduce inefficient risk to their projects resulting in a reduction in the probability of default. Furthermore, banks and firms increase collateral as a mechanism to commit to each other.*

[11] Credit lines have higher liquidity risk for banks, since they can be drawn at any point. However this risk is priced in the form of higher fees (see, e.g., Berg et al., 2016, for a discussion on fees), and in addition credit lines employ floating rate which reduces the risk imposed to banks from a dropping interest rate environment. From firms' perspective, literature has shown that these are a liquidity substitute only for firms with high cash flows and are preferred by firms with volatile earnings (Sufi, 2009). Furthermore increased uncertainty is positively correlated to credit line growth (Berrospide and Meisenzahl, 2015). However, during our sample period, Belgium "uncertainty" indexes are dropping (see, e.g., Algaba et al., 2020), while we do not observe any difference in the demand of credit lines after the reform, from firms with higher cash flows. Hence, any increase that we may observe is not attributed to these factors.

[12] To remove the concern that banks reduce PD in order to decrease the level of regulatory capital they need to hold, and therefore reduce the cost of the reform imposed on them, we study the changes in the level of common equity at the bank level, before and after the reform. We find no significant differences (for more details please see Section A0.3 in Appendix)

The effect of the act on the overall credit provision, depends on which force, demand or supply is more pronounced, and the substitution effect from term credit to credit lines. Our main empirical findings indicate an increase in the overall credit growth in the range of 2.2%, which we interpret as a combination of the substitution effect (within firms indifferent across the two products) which leads them to commit higher amount on their credit lines, and the informational clause that makes products more tailored to the average firm. We notice that (H1) implies that the shock on the supply side dominates and hence the reform created unintended consequences due the strategic reaction of banks. By decomposing credit growth to credit lines and term loans, we observe that this increase is mainly driven by credit lines by 4.44%, while term loans drop by 0.7%. Banks "undo" the act by increasing credit supply of the least affected instrument, which is consistent to (H1). Finally we observe both a drop in PD in the range of 0.2 p.p., and increase of collateral by 4% which we associate with the informational related clauses of the act which led banks to a more prudent behavior, consistent with (H2).

3 Data, Summary Statistics and Variable Definitions

We first start with the description of our sample. In the sequel we report the construction of the variables used in this paper.

3.1 Data and Sample Construction

We obtain data from two sources, made available by the NBB. The first dataset is the corporate credit registry, which contains granular information at the bank-firm-loan type-maturity level for all credit provided by financial institutions to firms operating in Belgium. The second dataset contains firm balance sheet and income statements. We restrict ourselves to credit granted in Euros and to credit institutions that are incorporated under the Belgian law. We exclude loans towards firms that did not report balance sheet information, in 2012, and therefore we cannot classify them as an SME or not.

Our sample spans the period of one year before to one year after the legal reform. We also track firms' future performance and hence we examine if the policy created real effects. To do that we use information for total assets and fixed assets, from firms' balance sheet and we construct proxies for asset growth and investment.

Banks mandatory report credit outstanding at a monthly frequency to the NBB for all loans when the total exposure exceeds EUR7500. Since April 2012, the credit registry also includes information on the borrower's probability of default (as reported internally by banks), the credit mode (i.e., term loan or credit line) and the collateral pledged. In order to reliably estimate any bank induced effect, we follow Degryse et al. (2019) and we drop banks that have less than 30 firms in their lending portfolio for a particular month. Our sample consists of 214,250 firms resulting in more than 273,000 bank-firm-credit mode loans. In our analysis we focus to credit modes that are classified as credit lines and term loans and thus excluding leasing and factoring loans.[13] Furthermore, to examine the implications of the reform to the maturity of loans granted, we classify loans in our sample as *Short-term* (*Long-term*) when having initial maturity less (more) than a year.

The monthly data from the credit registry are merged with the annual financial statements of Belgian firms, provided by the Central Balance Sheet Office (CBSO) of the NBB. We exclude financial and insurance companies. We then use the information provided by the CBSO to classify firms as SMEs or large firms excluding firms that have a turnover larger than 50 million. We do so to make sure that the firms in our sample (treated and control) are subject to similar capital regulation at the bank level.[14] The definition of an SME is included in Article 15 of the Belgian Companies Code and states that a firm is classified as such if it exceeds at most one of the following criteria the previous financial year.[15]

- average numbers of employees less than 50;
- annual turnover (not including VAT) less than EUR7.3 millions;
- total balance sheet less than EUR3.65 millions.

If a firm has an average number of employees more than 100, it is automatically classified as large. Since the legal reform was voted by the Belgian parliament on December 2013, which corresponds to the month that most firms report their balance sheet statement to the CBSO, to avoid any simultaneity bias we use information submitted one year prior to the event, to determine which firms are treated by the act.

[13]Credit lines constitute from *overdrafts* and *other cash credit*, while terms loans consist of *fixed term credits, mortgage loans* and *non-mortgage installment loans*. See, Table 2, for the detailed variable definitions.

[14]The choice for the 50 million turnover threshold is dictated by our identification strategy. See, Section 4 for details.

[15]See, e.g., http://www.ejustice.just.fgov.be/cgi_loi/loi_a1.pl?language=fr&caller=list&cn=1999050769 &la=f&fromtab=loi &sql=dt=%27loi%27&tri=dd+as+rank&rech=1&numero=1#LNK0013

3.2 Main Variable Definition

We use monthly information on the credit mode at the loan level to construct our two main variables; amounts of credit lines and term loans outstanding. Then we define overall credit as the sum of these two modes. In addition, from the credit registry we obtain information on the probability of default and on the collateral pledged, at the bank-firm level. To avoid serial correlation in our standard errors, we follow Bertrand et al. (2004) and we collapse our data to a pre- and post-event observation at the bank-firm-credit mode level (for credit lines and term loans). Our variables then are defined as the logarithmic difference of the post minus the pre-shock average, i.e., credit growth and denoted in regression equations as *Growth*.[16] To further examine if the law affected equally all types of borrowers, or incentivized banks to allocate more credit to specific class of borrowers we use a set of variables that account for a number of firm and bank-firm characteristics including the duration of a bank-firm relationship, firm leverage and financial pressure.[17] Table 2 defines the variables used in this study and Table 3 provides the summary statistics for the main variables of interest.

*** **Please Insert Table 2 around here** ***

*** **Please Insert Table 3 around here** ***

Our sample spans from January 2013 to December 2014. The first part of Table 3, provides the descriptive statistics for our main variables, from which we observe that overall the post event credit growth drops by more than 8%, which is mainly driven by drop in term loans. Note that this captures the incentive margin such that expiring loans that are not replaced within the same bank are behind this result.[18] This is not only statistically significant but also economically sizable since the total amount of credit prior to the shock was in the range of EUR80 billions, which corresponds to approximately a EUR6 billion drop. Credit growth rates of term loans at the intensive margin are also negative in other years since it incorporates monthly loan repayments and hence these descriptives results may overestimate the true effect of the legal reform. In our sample there are more than 270,000 bank-firm-credit mode observations, which drop to just less than 215,000, when

[16] Our results are robust when constructing credit growth while maintaining the time dimension.

[17] We do not directly account for size, since it would be endogenously selected, already contained in the definition of an SME.

[18] This result does not account for the repayment scheme of term loans. For more details, please see, Section 5.

we aggregate at the firm level.[19] This is not surprising in Belgium, where the banking sector is highly concentrated and most firms have a single bank relationship. The four largest banks account for more than 80% of the banking assets, while our sample contains in total 26 banks.

4 Loan level analysis

4.1 Identification strategy

In this section we discuss the identification strategy that we follow to study the impact of the legal reform for overall credit growth at bank-firm level as well as at credit-mode level (i.e., credit lines and term loans). We exploit a unique quasi natural experiment in Belgium to examine the implications of a shift in the bargaining position of banks and firms.

Our empirical identification poses several challenges that need to be addressed. One potential confounder is the introduction of the SME supporting factor that allows banks to hold less regulatory capital for SME loans. Note that the SME definition employed in capital regulation is less restrictive than the one employed for the Belgian legal reform, allowing us to have a treated group that is affected by the SME supporting factor and the legal act, and a control group that is affected by the supporting factor but not by the reform. The supporting factor was introduced around the same time as the legal reform. To identify the causal effect of the legal reform to lending provision, credit decomposition and firm behavior, we use a difference-in-difference framework within the set of firms that are subject to the supporting factor. Since the reform affected only loans granted to SMEs, we use SMEs to identify the impact of the reform relative to an unaffected group of firms i.e., larger firms (labeled as non-SMEs, but still affected by the supporting factor). We make the identifying assumption that any difference in the refinancing behavior between similar (SME and non-SME) firms, should be due to legal reform and thus any finding on the difference-in-difference estimator would accurately measure the true impact of the legal implementation.[20] The main identifying assumption is that credit demand is homogeneous across financial institutions and firms ex-post cannot influence their firm classification in order to exploit the possible beneficial clauses of the law. One concern could be that smaller firms are more likely to team up with banks that

[19] In our sample, approximately 99% of the firms are SMEs. This number is similar to other locations across Europe.

[20] The law did not apply to existing stock of credit and thus treating all loans after the event as subject to the new law, we tend to underestimate the true effects of the reform.

are benefiting more from the supporting factor, that is lowly capitalized banks. To deal with this concern, we do the analysis within bank, by including bank fixed effects.

4.2 Difference-in-Difference set up

In December 2013, the legal reform was voted by the Belgian parliament and on January 10, 2014 it was implemented nationally. In January 2014, also the SME supporting factor (SF) program started, which was proposed by the Basel committee (see, e.g., EBA, 2016) and aimed to increase credit towards small and medium size firms. The classification of a firm as an SME under the Belgian corporate law, does not coincide with the proposal that the Basel committee made. Firms with turnover below EUR50 million are eligible for the supporting factor program, regardless their state classification as an SME or not.[21] In order to isolate the full impact of the reform, we restrict ourselves to firms that have turnover below EUR50 million, i.e. the threshold for eligibility to the SF and thus we restrict our sample to SMEs and non-SMEs (according to the Belgian corporate law), which are also eligible for the SF program.

Since the SF was largely anticipated (see, e.g., Dietsch et al., 2016), we expect that the effects to the credit provision are minimal.[22] Thus by implementing a difference-in-difference set up, in which *Treated* are Belgian firms classified as SMEs, and the control are large Belgian firms, not affected by the legal act that we study, and at the same time, both groups are affected by the supporting factor, we are able to estimate the differential effect of the policy to credit growth and credit composition. We compute growth (overall, credit lines, term loans, PD and collateral), by collapsing our data to a pre- and post-event point average at the bank-firm (-credit mode) level before we differentiate the natural logarithm of the volume and we denote it by *Growth*. Formally, the following difference-in-difference model identifies this differential effect and is estimated through OLS:

$$Growth_{bf} = b_1 Treated_f + b_2 FirmControls + v_b + a_{fc} + \epsilon_{bf}, \tag{1}$$

where $Treated$ is a dummy that equals 1 when a firm is subject to the legal reform (i.e., SME firms in Belgium) and 0 when a firm belongs to the control group (i.e., non-SMEs with turnover below

[21] In our sample, the average firm has turnover in the range of EUR500,000.

[22] In Appendix A0.2, we examine the effects of the supporting factor to Belgian firms, and similar to the EBA (2016) report, we find insignificant results, both economically and statistically. For a detailed study of the SF, see, e.g., Mayordomo and Rodríguez-Moreno (2018).

EUR50 million). To further ensure the validity of our assumption, we allocate firms in the two groups based on their balance sheet information, one year prior to the reform. The coefficient of interest is b_1 and is associated with the causal effect of the reform to the variable of interest. We collapse our data to a single pre- and post-observation, essentially removing the time dimension to avoid serial correlation that may augment the statistical significance of the causal inference (Bertrand et al., 2004).[23] To further reduce the effects of potential serial correlation within and across treated units, in all of our regressions specifications, we follow a variant of Cerqueiro et al. (2017) and Cerqueiro and Penas (2017) and cluster standard errors based on Industy*Location bins.

The main concern when employing loan level data, is to disentangle the demand from the supply side. For that we saturate our model in (Eq. 1) with bank fixed effects that control for (observed and) unobserved bank specific, time invariant heterogeneity. To capture the firm-borrowing channel and to absorb any firm specific credit demand shock we follow Degryse et al. (2019), De Jonghe et al. (2019), and Popov and Van Horen (2015) and saturate our model with firm-cluster a_{fc} fixed effects, by including firm-clusters according to geographic location, using the two-digit postal code and by industry using the NACE identifier.[24] We do not include size in the firm-clusters, since size is one of the determinant variables that is used in the SME classification and that would raise endogeneity issues. Similar approaches to control for credit demand have been employed by Edgerton (2012), Gropp et al. (2019) and Morais et al. (2019). We further control for the demand side, by including the vector $FirmControls$, which comprises by the pre-shock *Working Capital over Total Assets, EBIT over Total Assets, Retained Earnings over Total Assets, Operating Revenue over Total Assets, Leverage, Fixed Assets over Total Assets* and *Financial Pressure*. For details please see, Table 2.

5 Main Results

In this section we report the main results for our empirical analysis. We start by establishing the baseline effect of the legal reform. Next, in order to mitigate concerns regarding the intrinsic differences between the two groups, we focus on a matched sample between treated and control firms. Since the law only applies to new loans, we further investigate the impact of the law over

[23] As a robustness we replicate our main models without collapsing in order to exploit time dimension and observe the dynamics of the treatment effect. Our findings are quantitatively similar.

[24] The location classification corresponds to district level.

time. In the third subsection, we study the intensive and extensive margin. Finally, we investigate the implications of the reform to the likelihood of loan prepayment.

5.1 Baseline Results

Table 4 reports the results from estimating our difference-in-difference equation (1) at bank-firm-credit mode level. The first three columns provide the estimates for overall credit growth, growth in credit lines, and term loans, respectively. Columns (4) and (5) report the impact on PD, and the growth in collateral pledged.

*** **Please Insert Table 4 around here** ***

After the reform we observe a change in the overall credit growth rate as well as in the composition of credit; credit lines and term loans towards treated firms. In particular, the credit growth rate is 2.2% higher for treated firms than control firms. A point estimate of 2.2% is about a quarter of the mean credit growth and 4% of the standard deviation at the bank-firm level. Columns (2) and (3) furthermore show that the composition of credit is affected: while credit lines for treated firms exhibit a 4.4% higher growth rate, the growth in term loans is 6% lower for treated firms versus control firms. Our results are not only statistically but also economically significant. The 4.4% and 6% are about the 15% and 10% of the standard deviation of the credit line and term loan growth, respectively. Our empirical results show that term loans drop whereas credit lines increase. This essentially implies that banks seem to try to mitigate the effect of the act, which is consistent to (H1) and points to some unintended consequences of the law. Nonetheless, a drop of 6% in term loans may overestimate the true effect since term loans have a regular repayment scheme and the repayment schedule could differ between treated and control firms (small firms may employ a monthly repayment scheme as opposed to long-term bullet term loans usually contracted by larger firms). To account for the magnitude of the term loan amortization process and to ensure that our results reflect the actual implications of the act, we replicate our analysis selecting January 2013, as the falsified event date. Table 5 contains the estimation results.

*** **Please Insert Table 5 around here** ***

We observe that all our key variables, with the exception of term credit, document insignificant difference between, before and after the falsified date. This further supports our findings, towards

the assumption that the reform was an unanticipated shock with economically meaningful effects. Furthermore, we observe that the drop in the difference-in-difference estimator in term loans, is of the range of 5.2% which implies an actual effect on term loans in the magnitude of 0.8%, accounting for differences in the amortization of term loans during our event study. To further verify this finding, we need to alter the way that we measure growth rate in term loans. In particular to absorb any effect due to the repayment scheme, we first calculate the monthly credit growth, before we collapse our data to a pre- and post-period point at the bank-firm-credit mode level and perform our analysis. We report the coefficient in Table 6 below, noticing that the 0.7% statistical significant drop that we observe is consistent with the results presented on Table 4 and Table 5.

*** **Please Insert Table 6 around here** ***

Our empirical analysis reveals a direct effect of the legal change consistent with (H2). We observe a drop in PD by 0.002 and an increase in the collateral pledged by 4% for treated firms versus the control group. That corresponds to more than one third and 11% of the mean, respectively. The collateralization rate of treated firms thus seems to go up relative to non-SMEs as the value of collateral pledged increases faster than the growth of credit. The reform thus forces banks to revise the actual default risk of incumbent borrowers which is consistent with the results observed. In the aforementioned specification we control for bank fixed effects as well as for the credit demand by saturating our model by industry*location fixed effects.

To further alleviate any concerns that our results are driven by the demand side, we restrict our sample to firms that have exposures for both types of credit, and we examine the ratio of credit lines over term loans as well as we compare *within* a firm, the difference in the growth rate between a credit line and a term loan before and after the reform. We do that by stacking the growth rates at the firm level, which allow us to saturate the model with firm fixed effects. Under the main identification assumption that the proportion between the two types of credit demanded by firms remains constant before and after the event, we obtain that both of our variables document an increase for credit lines, attributed by the supply side. For brevity, we report the results in Table A4 in the Appendix.

Our results are consistent with the hypotheses presented in Section 2. To further investigate H1, i.e., that the supply channel dominates, we study the implications of the act to loan *maturity*. We decompose credit lines and term loans into *Short-term* and *Long-term* maturity and we repli-

cate our main exercise. Results are reported in Table 7. We notice that our results further support H1. Banks do not cut term loan credit provision homogeneously; *Long-term* term loans are more affected, since these are more likely to benefit from the clause of the legal reform related to prepayment indemnities. This is in contrast to the demand side as borrowers with *Long-term* term loans have higher likelihood to exploit the right for early prepayment in order to benefit from an interest rate drop. Furthermore, clients that would potentially prefer a *Long-term* loan, now obtain a different and less affected product, i.e., *Long-term* credit lines. Notice that these are eonomically unaffected from the reform, since firms can prepay at any time due to the nature of the credit line.

*** **Please Insert Table 7 around here** ***

5.2 Matched sample and staggered impact of law

In the previous subsection we examined the overall effects of the reform to the lending behavior of banks and provided evidence that banks increase credit supply but mitigate the effect of the legal change by offering economically unaffected credit modes. A potential concern is that treated firms may have a higher growth rate than control firms, which can be reflected in higher needs for additional loans. In order ensure that our results are not driven by any pre-event trends and fundamental differences between the treated group and control group, we restrict our analysis to a matched sample using multiple pre-event characteristics in order to obtain a better similarity. We match the two groups on the following characteristics; *Credit Growth (overall, credit lines and term loans), Altman Z-score, Financial Leverage, (Trade Credit / Total Assets), (Fixed Assets / Total Assets), Financial Pressure, Employees* and *Total Assets*. Following Campello and Giambona (2013) and Gropp et al. (2019), we use the bias corrected Abadie and Imbens (2011) estimator, selecting a tight "bandwidth" in order to obtain a very similar match. Our regression output is presented in the following table.

*** **Please Insert Table 8 around here** ***

As expected we lose around 50% of the observations in our sample. However the coefficients obtained are similar in economic magnitude and statistical significance with the ones obtained using all firms. This suggests that there are no differences in pre-event characteristics that could create a bias in our findings.

One additional aspect of the legal reform is that it affects only new loans and does not apply to existing loans. Therefore, in our baseline model, we may underestimate the true magnitude of the reform as the reform's impact realizes in a staggered way. To address this, we collapse our data after the event period, per quarter, and we compute the credit growth for every quarter. In the sequel, we replicate our main exercise and we report the regression output in Table 9.

*** **Please Insert Table 9 around here** ***

The longer the time window following the introduction of the law, the larger the fraction of loans that is subject to the new regulation. We find that the economic impact of the legal act increases over time and starts to stabilize in magnitude after Q3. The reason is that as more loans expire and are rolled over, the new contracts are subject to the new reform and therefore banks are contractually obliged to follow the new legal requirements which as discussed in Section 2, create implications to credit provision, PD and collateral pledged.

5.3 Intensive and extensive margin

In our main analysis we document the overall effect of the reform without distinguishing between the intensive and extensive margin. However, if banks are using their bargaining power, stemming from lengthy relationships, in order to influence the borrowing choices of their clients, our results should also hold in the intensive margin since relationship strength increases by duration. We measure the intensive margin at the bank-firm level, and we keep only bank-firm relationships that were present for the whole duration of our sample, which constitute the majority of our starting sample. In Belgium the banking sector is highly concentrated, and most firms have single relationships and do not switch frequently.

The extensive margin shows whether banks started or stopped to lend more on a particular type of credit and hence, it can provide further indication on the channel that dominates; supply or demand. If the demand side dominates we should see more entries for term credit, while if the supply side prevails we should document more entries on credit lines and more exits on term loans. For the extensive margin, we follow Gropp et al. (2019), and we distinguish between *Entries* and *Exits* on the bank-firm-credit mode level. For the extensive margin, we estimate the following regression specification:

$$Y_{bfm} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bfm},$$

where Y_{bfm} is a dummy that represents either entries or exits. Dummy *Entries* takes 1 at the bank-firm-credit mode level, if a bank b, starts lending to a firm f, a particular credit type m after the reform, zero otherwise. The dummy *Exits*, gives 1 at the bank-firm-credit mode level if the bank b stopped lending firm f the product m, after the new act, zero otherwise. Table 10 reports our estimates on the intensive margin while in Panel A (B) of Table 11, we tabulate our results for *Entries* (*Exits*) respectively.

*** **Please Insert Table 10 around here** ***

*** **Please Insert Table 11 around here** ***

We notice that our results also apply at the intensive margin. This is an indication that firms do not use the reform in order to obtain better terms for their preferred credit type and switch banks, but rather shift to another product within the same bank. This is in line with our main argument that banks in order to mitigate the impact of the law, shift the supply of credit towards least affected products.

When we turn to the extensive margin, we view similar results. Entries in the banking system are only different between treated and control for credit lines, 0.6p.p. increase (Panel A, Table 11), while exits for term loans are 1.7p.p. higher for SMEs relative to the control group (Panel B, Table 11). We expect exits to be higher for SMEs in comparison to large firms, but if these were related to defaults, then we should not observe any difference between exits in credit lines and term loans. In contrast, we document four times more exits in term credit relative to credit lines, which is an indication of firms switching to another product, i.e. credit lines, *within* the same bank rather than exiting entirely the bank.

So far we investigated the implications of the legal reform to the intensive margin and to entries and exits. Our key finding is that differences before and after the reform, between SMEs and large firms are only observed in entries for credit lines and mainly in exits for term loans, which is an indication of a shifting credit towards credit lines. One concern related to the extensive margin is that it captures also exits from the banking system due to defaults. To address this possibility, we work with the intensive margin sample and we use *large* increases and *decreases* in credit as proxies for prepayment. *Large* increases in the intensive margin are associated with contracting new loans *within* credit type while large *decreases* with repayment *between* credit types, in the same bank. We measure increases (decreases), by a dummy that takes the value of 1, if within a credit type

we have an increase (decrease) of more than 25% in volume before and after the reform. Table 12 reports the regression coefficients.

*** **Please Insert Table 12 around here** ***

The estimation results are consistent to the ones obtained in Table 11. Treated firms have a 6.9 p.p. increase in the likelihood of a large decrease in term credit relative to large firms after the reform, while the likelihood of contracting new term loans drops by 4.4 p.p.; treated firms are less likely to increase their term loans with big amounts. All of our tests indicate that the legal act, shifted the composition of credit towards the opposite than the intended direction. The channels that led to this unintended consequence is the main topic of our next section.

6 Credit composition: Channels

So far we investigated the effect of the reform to credit provision, credit composition as well as the implications to PD and collateral. In this section, we aim to shed light on the different channels behind our findings. The act increases a bank's marginal cost of offering term loans as these now include a prepayment option. Therefore, banks may decide to pass on this cost to borrowers by increasing the interest rates on term loans (i.e., price the legal change), or shift credit towards products unaffected by the act (i.e., credit lines). In the next two sections we provide evidence in the existence of both channels.

6.1 Loan Rates

We now examine if banks price the reform and compensate for the prepayment option by increasing the interest rate on loans granted to treated firms relative to control firms to align with the new fundamentals. The Belgian credit registry does neither provide interest rate information at the bank-firm level nor at the bank-firm-credit mode level. We therefore proxy loan rates by the *implicit interest rate* from a firms' balance sheet information and income statement. Most firms in Belgium have a single bank relationship; hence we can accurately proxy from the interest rate payments, the true effect of the reform. We postulate that the differential effect would be positive, signifying that banks price the reform and pass on (part of) the additional cost of the included prepayment option to their clients. We start by estimating the following regression specification

across the sample of firms considered in our main analysis to obtain the overall effect on interest rates:

$$\Delta y_f = b_1 Treated_f + b_2 FirmControls_f + a_{fc} + \epsilon_{bf},$$

where Δy_f denotes our key variable (i.e., difference in the implicit rate before and after the reform, measured in percent). In Table 13, we report the regression coefficients.

*** **Please Insert Table 13 around here** ***

From Column (1) of Panel A, we notice that the interest rate for treated firms increases by 0.017p.p. relative to similar, non affected firms. In Columns (2) (respectively, (3)), we estimate the effect of the reform on the interest rate based on a sample of firms that has only credit lines (respectively, term loans) contracted before and after the act.[25] We notice that the increase that we documented in Column (1), is actually driven by increased interest rate for term loans (0.068 p.p.), which is in line with the premise that banks price the reform for the affected product. Finally, in Panel B, we further decompose our sample based on maturity (*Short-term* and *Long-term*), within firms that have only one credit type as in Panel A. In all specification we find insignificant results with the exception of *Long-term* term loans, for which we find an increase of 0.03p.p. relative to non affected firms having the same products.[26] However this should not come as a surprise since we know from Section 5.1, that reform does not affect all maturity levels homogeneously but rather is more pronounced within *Long-term* term loans. Having established that banks price the reform, our next goal is to study if banks use to a different degree their bargaining power across borrowers to further increase credit lines origination.

[25]Restricting our sample to firms that have only one type of loan before and after the reform, allows to identify the impact for each credit mode.

[26]The act embeds on new loans an (exotic) option, i.e., the possibility to early repay. Hence, we can view equivalently the 0.03 p.p. increase in *Long-term* term loans for treated firms in comparison to the control group, as the actuarial risk premium for the additional cost imposed to banks by this option. The actual exercise of this (exotic) option not only depends upon the behavior of interest rates in the future, but also is affected by behavioral characteristics of borrowers. To be empirically consistent, we explicitly compute the actuarial benefit for the borrower that stems from the legal act. Using simulations for obtaining future realizations of the interest rate, we estimate the actuarial benefit of the reform on *Long-term* term loans, to be 0.036 p.p. For more details, please see, Appendix A0.4.

6.2 Legal Reform: role of relationship and firm characteristics

If banks use their bargaining position relative to firms to shift credit towards least affected products, then this should be more pronounced among firms where a bank has a greater bargaining power. To test this premise, we augment (Eq.1) with two variables that proxy the bank-firm relationship strength based on the duration that a firm is on the bank portfolio. The first, *Relat_Dummy*, assigns the value 1 if a bank-firm relationship has been established before January 2013, i.e., the length of the relationship at the time of the event is more than one year, zero otherwise. The second, *Relat_Duration*, counts the number of months that a firm is on the lending portfolio of a bank.

From the demand side, firms that ex-ante are more risky and have a higher likelihood of getting a rating increase (e.g., have more volatile cash flows) stand to benefit more from the legal change than otherwise similar firms. We therefore interact SMEs (i.e., *Treated*) with proxies for firm risk. We proxy for firms' riskiness by the pre-treatment levels of leverage ratio (*Leverage*), financial pressure (*Financial_Pr*), and Altman Z-score. Finally, to ensure that our findings are not driven by firms that have high cash holdings, we interact with a dummy that takes 1 for firms with cash holding (*Cash_Hold*) above the median value, and zero otherwise. The different panels in Table 14 reports the regression coefficients on the interaction terms. Columns (1) to (2) report results related to the reallocation of credit for credit lines and term loans. In all regressions, we exploit within bank heterogeneity by saturating our model with bank fixed effects and we control for the demand side by introducing Industry*Location clusters.

*** **Please Insert Table 14 around here** ***

Columns (1) and (2) of Panels A and B, reveal that firms with longer relationships see a bigger increase in credit line growth than non-SMEs with similar relationship length, while no differential effect is found for term loan growth. Put differently banks seem to supply more credit lines on treated firms with whom they have stronger relationships.

Panels C, D and E, investigate whether firms with different degrees of riskiness, proxied by leverage, Altman Z-score and financial pressure, are affected differently. In general, we do not find significant effects. We only find a differential effect regarding the growth rate for credit lines on Panel C. However this finding is consistent with the results of Panels A and B, since leverage is a also an indicator of the degree that banks can exert power over firms. All in all, our results point against the idea that demand by riskier firms would be driving our findings and indicate that banks

exploit their lengthier relationships to increase credit line growth.

A second way to investigate whether the strength of bank-firm relationship matters for the credit composition effects we document, is to focus on firms with multiple lending relationships. If banks are able to exert power over firms with lengthier relation, then this power should be diluted for firms with multiple bank relationships. In contrast if it is the demand side that dominates, then all the results we document, would be even more pronounced within these firms, since any substitution effect would be easier to be accomplished. To examine this argument we replicate our previous analysis for our main relationship variables, by focusing specifically on the sub-sample of firms with multiple bank relationships (Panels A and B, Table 15) and we exploit the variation in the relationship length at the bank-firm level by saturating our model with firm fixed effects.

*** **Please Insert Table 15 around here** ***

We notice that our results point once more in the direction that it is the supply side that dominates. Firms that are not dependent upon one bank, i.e. firms with multiple sources of financing do not seem to establish the behavior of migrating towards credit lines or reduce term loan exposure. This finding further hints that it is banks that alter the composition of credit and they do that only within a particular set of customers, i.e., firms depending upon single relationships.

7 Real Effects

We have shown that the legal reform affects contract design, and generates unintended implications to the composition of credit as well as to lending standards, by reducing the probability of default and increasing collateral pledged. In this section, we investigate the real effects of the reform, and in particular the impacts on firm growth and investment. Since banks are pricing the reform and adjusting credit supply, we hypothesize that treated firms will experience negative real effects stemming from the negative supply effects previously documented in the paper. We employ two indicators to capture real effects being firm growth and net investments. Following Degryse et al. (2021), we proxy firm growth and net investments by growth in total assets and fixed assets, respectively, and obtain these data from firms' balance sheets.

We measure the real effects as the growth rate over the period prior to the legal change and one year after (end of 2014). We then regress the growth rate variables, to the treated variable saturating our model with firm-cluster fixed effects, firm controls and clustering standard errors

according to Industry*Location. Table 16 reports the descriptive statistics while Table 17 tabulates the corresponding results.

*** **Please Insert Table 16 around here** ***

*** **Please Insert Table 17 around here** ***

The first and second column of Table 17 report the results on firm growth and investment, respectively. We find that growth in total assets and fixed assets for treated firms are 1.1% and 2.9% lower than otherwise similar control firms. These findings suggest that the reform generated negative effects for treated firms, as the reform got priced in and may have limited the possibility towards relationship banking as in Petersen and Rajan (1995).

8 Conclusion

Small firms all over the world heavily rely on bank loans for their external financing. Small firms have limited outside options putting them in a weak bargaining position relative to their banks. The legal framework shaping the contracting space therefore determines how banks and firms are interacting with each other. In this paper, we study a quasi-natural experiment that aims to increase small firms' bargaining position relative to banks by imposing a limitation on the contracting space of loan contracts. The limitation on the contracting space we study is the legal imposition of a prepayment option in loan contracts given to firms that can be exercised at a small cost.

We study a proprietary dataset containing monthly credit data at the bank-firm-credit mode level as well as annual firm balance sheet data. We study how firms and banks react to the legal change, and find that banks shift credit supply away from term loans towards credit lines as the latter are economically unaffected by the legal change. The legal change thus causes a *credit composition* effect which can be see as an unintended consequence of the law.

We identify two channels through which banks induce this credit composition effect. The first is the pricing of the prepayment option leading to an increase in loan rates on term loans. Second, we also find that banks use their bargaining power as firms with longer lending relationships are more subject to the credit composition effect. We furthermore show that the legal change generated negative real effects for the treated firms as they have lower asset and investment growth compared to control firms.

Our results show that limiting the contracting space for loan contracts to small firms has unintended consequences and modifies the way how firms and banks interact. Our study thus signifies the importance of anticipating the strategic reaction of banks when imposing restrictions on the contractual agreements banks and firms can make.

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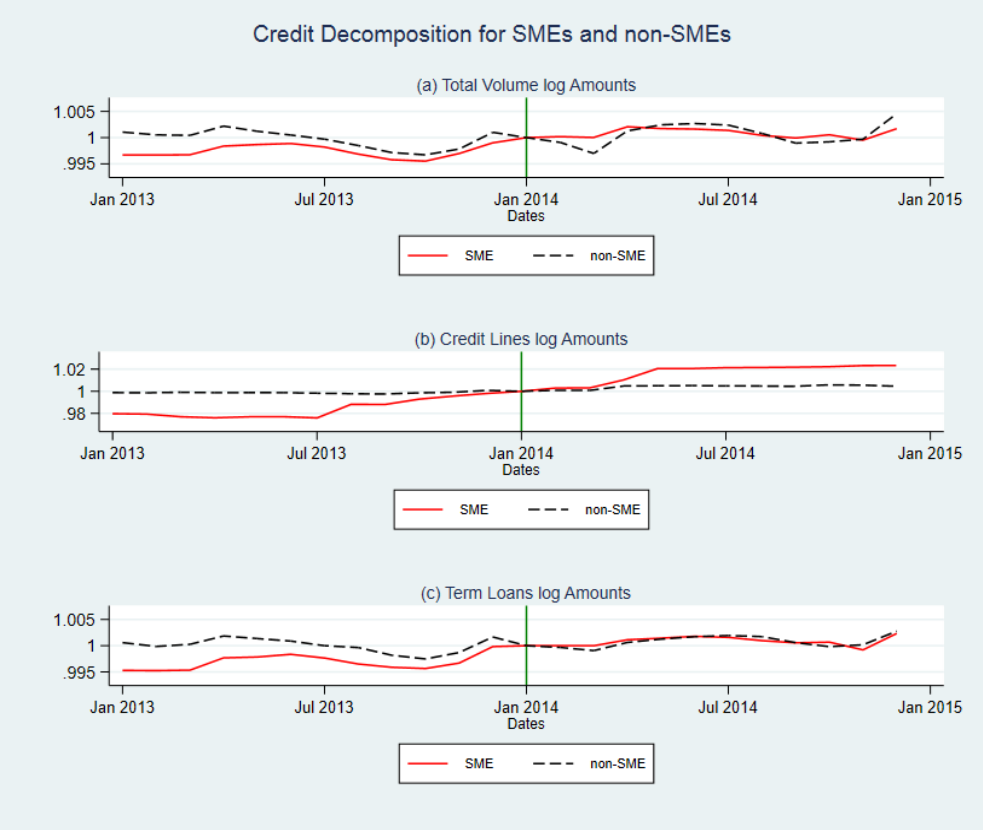

Figure 1: Visual Inspection of Credit Composition

This figure shows the evolution of credit, for the whole banking system from January 2013 to December 2014, standardized to 1 at the month of the event. We plot the median total (log) volume, as well as the volume for credit lines and term loans, granted to firms with turnover below EUR50 million. The solid (dashed) vertical line indicate in the implementation (vote) of the reform. Detailed definitions are provided in Table 2.

Table 1: Impact of legal change to Term Loans and Credit lines

This table schematically summarizes the clauses of the SME refinancing law related to prepayment indemnities to term loans and credit lines.

		Before	After
Term Loans	above 1 million	Article 1907bis 6-months prepayment penalty Banks had the right to refuse	Article 1907bis 6-months prepayment penalty Banks *do not* have the right to refuse
	below1 million	Article 1907bis 6-months prepayment penalty Banks had the right to refuse	Article 1907bis 6-months prepayment penalty Banks *do not* have the right to refuse
Credit Lines	above 1 million	Unregulated but according to the Code of Conduct	Arranged bilaterally
	below 1 million	Unregulated but according to the Code of Conduct	6-months prepayment penalty Banks *do not* have the right to refuse

Table 2: Variable Definition

This table provides the definition of the main variables employed in the paper.

MODES and MATURITIES	
Credit	Sum at bank firm month level of Credit Lines and Term Loans
Credit Lines	Sum at bank firm month level of credit modes classified as *Overdrafts* and *Other cash credits*
Term Loans	Sum at bank firm month level of credit modes classified as *Fixed term credits, Mortgage loans* and *Non-mortgage installment loans*
Pr. of Default	Probability of Default at bank firm month level reported by banks
Collateral	Sum at bank firm month level of Pledged Collateral
Short-term	Loans with initial maturity less than a year
Long-term	Loans with initial maturity more than a year
CREDIT VARIABLES	
Δ log Credit_{bf}	log of time averaged total credit granted post legal shock - log of time averaged total credit granted pre legal shock
Δ log Credit Lines_{bf}	log of time averaged credit lines granted post legal shock - log of time averaged credit lines granted pre legal shock
Δ log Term Loans_{bf}	log of time averaged term credit granted post legal shock - log of time averaged term credit granted pre legal shock
RISK VARIABLES	
Δ $\text{Pr. of Default}_{bf}$	time averaged Probability of Default post legal shock - time averaged Probability of Default pre legal shock
Δ log Collateral_{bf}	log of time averaged pledge collateral post legal shock - log of time averaged pledge collateral pre legal shock
FIRM VARIABLES	
Total assets_f	pre shock time averaged natural logarithm of total assets
Turnover_f	pre shock time averaged natural logarithm of turnover
FTE_f	pre shock time averaged number of Employees
Leverage_f	pre shock time averaged total debt / pre shock total assets
Altman Z-score_f	pre shock time averaged of Altman Z-score. Altman Z-score is calculated as: (0.717 * Working Capital + 0.847 * Retained Earnings /+ 3.107 * EBIT+ 0.988 * Operating Revenue) / Total assets + 0.42 * Equity / Total Debt.
EBIT_f	pre shock time averaged of Net Income + Interest + Taxes
Gross Margin_f	pre shock time averaged of (revenue - COGS) / pre shock revenue
$(\text{Working Capital/TA})_f$	pre shock floating assets / pre shock total assets
$(\text{Retained Earnings/TA})_f$	pre shock profits / pre shock total assets
$(\text{Operating Rev./TA})_f$	pre shock Gross Margin / pre shock total assets
$(\text{FA / TA})_f$	pre shock fixed assets / pre shock total assets
$\text{Financial Pressure}_f$	pre shock interest payments / pre shock EBIT
OTHER VARIABLES	
SME	Firms with less than 100 employees which violate at most one of: Employees less than 50; Turnover less than EUR7.3 millions; Balance Sheet less than EUR3.65 millions
non-SME	all firms that are not SME. We also refer non-SME as *large firms*

Table 3: Summary Statistics

This table provides the summary statistics of our main variables. The sample spans from January 2013 to December 2014. Descriptive statistics for the firm variables, are presented for the pre- event period, i.e., up to December 2013. Detailed definitions are provided in Table 2.

	Obs	Mean	St.Dev	P05	P25	p50	p75	P95
CREDIT VARIABLES								
bank-firm level								
Δ log Credit_{bf}	273894	-.0876	.4969	-.9459	-.2301	-.0396	0	.7221
Δ log Credit Lines_{bf}	147708	.0489	.2659	-.0634	0	0	0	.6335
Δ log Term Loans_{bf}	206780	-.1718	.5393	-1.1698	-.3501	-.1100	0	.6539
RISK VARIABLES								
bank-firm level								
Δ Pr. of Default_{bf}	262616	.0052	.0529	-.0348	-.0028	0	.0024	.0564
Δ log Collateral_{bf}	208194	.3555	1.083	-.9197	-.1814	-.0117	.5566	2.3370
FIRM VARIABLES								
bank-firm level *(pre-event)*								
log Total Assets_{bf}	273894	12.8369	1.6567	10.1818	11.6860	12.7787	13.9226	15.7133
log Turnover_{bf}	56953	13.1364	1.9262	10.3729	11.7658	12.7908	14.3909	16.7058
FTE_{bf}	273894	3.8674	9.1352	0	0	.1959	3.1403	20.6249
Leverage_{bf}	273894	.7313	.4925	.1446	.4614	.6978	.8884	1.3613
Altman Z-score_{bf}	273654	1.1472	2.2739	-1.2063	.2940	.8600	1.7281	4.2754
$(\text{EBIT/Total Assets})_{bf}$	273894	.0756	.1867	-.1721	.0146	.0597	.1369	.3875
$(\text{Working Cap./Total Assets})_{bf}$	273894	.0763	.4772	-.6287	-.1018	.1051	.3491	.7224
$(\text{Retained Earn./Total Assets})_{bf}$	273894	-.0953	.6695	-.9206	-.0640	0	.0957	.4945
$(\text{Operating Rev./Total Assets})_{bf}$	273894	.3340	.3828	-.0057	.0975	.2322	.4522	1.0678
$(\text{Fixed Assets/Total Assets})_{bf}$	273894	.3852	.3030	.0044	.1117	.3266	.6292	.9282
Financial Pressure_{bf}	273894	.2434	1.203	-.9031	.0141	.1556	.4903	1.4694
firm level *(pre-event)*								
log Total Assets_{f}	214250	12.6628	1.6340	10.0517	11.5289	12.6018	13.7323	15.4961
log Turnover_{f}	43346	12.8476	1.8493	10.2082	11.6137	12.5263	13.8746	16.4822
FTE_{f}	214250	2.9793	7.3567	0	0	0	2.3178	15.7613
Leverage_{f}	214250	.7383	.5606	.1276	.4392	.6876	.8913	1.4358
Altman Z-score_{f}	214017	1.2083	2.6438	-1.4670	.2723	.8840	1.8304	4.6457
$(\text{EBIT/Total Assets})_{f}$	214250	.0765	.2041	-.1981	.0119	.0610	.1441	.4118
$(\text{Working Cap./Total Assets})_{f}$	214250	.0687	.5328	-.6901	-.1084	.1093	.3674	.7441
$(\text{Retained Earn./Total Assets})_{f}$	214250	-.1256	.7884	-1.0644	-.0780	0	.0982	.5140
$(\text{Operating Rev./Total Assets})_{f}$	214250	.3324	.3953	-.0164	.0950	.2281	.4483	1.0801
$(\text{Fixed Assets/Total Assets})_{f}$	214250	.3813	.3070	.0026	.1022	.3182	.6301	.9320
Financial Pressure_{f}	214250	.2183	1.1733	-.8949	.0081	.1314	.4488	1.419

Table 4: The Effect of the Law Reform to Credit Provision

This table provides the estimates from panel regressions of the differential effect of the law reform to loan provision. The main regression equation is given by

$$Growth_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the bank-firm level before we differentiate the natural logarithm of the data points. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Credit* is the sum of *Credit Lines* and *Term Loans* volume. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(3) analyze the effect on credit growth and Columns (4)-(5) the overall effect on the probability of default (calculated internally by banks) and on collateral pledged. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Data are provided by the NBB.

	(1) Δ log Credit	(2) Δ log Credit Lines	(3) Δ log Term Loans	(4) Δ Pr. of Default	(5) Δ log Collateral
Treated	0.022***	0.044***	-0.060***	-0.002***	0.040***
	(0.0077)	(0.0060)	(0.0086)	(0.0857)	(0.0117)
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Obs	273894	147708	206780	262616	208194
R^2	0.041	0.098	0.046	0.036	0.657
Adjusted R^2	0.025	0.074	0.026	0.020	0.650

Table 5: Falsified Event Date

This table provides a robustness check to our main exercise. The main regression equation is given by

$$Growth_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

Our sample extends from April, 2012 to December 2013 and excludes credit granted to firms with turnover larger than EUR50 millions. We select as falsified event date January 2013. We collapse our data to a pre- and post-event point at the bank-firm level before we differentiate the natural logarithm of the volume. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Credit* is the sum of *Credit Lines* and *Term Loans* volume. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(3) analyze the effect on credit growth and Columns (4)-(5) the overall effect on the probability of default (calculated internally by banks) and on collateral pledged. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1) Δ log Credit	(2) Δ log Credit Lines	(3) Δ log Term Loans	(4) Δ Pr. of Default	(5) Δ log Collateral
Treated	0.008	-0.007	-0.052***	0.005	0.019*
	(0.0077)	(0.0069)	(0.0078)	(0.0896)	(0.0099)
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Obs	273008	148764	206147	264822	208838
R^2	0.040	0.044	0.051	0.056	0.029
Adjusted R^2	0.024	0.018	0.031	0.040	0.009

Table 6: Alternative Calculation of Term Loans credit growth

This table provides the estimate from a panel regression of the differential effect of the law reform to term loans provision. The main regression equation is given by

$$Gr.Repaid_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. *Gr.Repaid* is computed by first calculating the monthly credit growth for term loans before we collapse out data to a pre- and post-event point and differentiate after. *Treated* corresponds to SMEs affected by the law reform. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1)
	Gr. Repaid
Treated	-0.007***
	(0.0016)
Bank FE	Yes
Firm Cluster FE	Yes
Firm Controls	Yes
Obs	203760
R^2	0.036
Adjusted R^2	0.016

Table 7: Effect of the Law Reform to Loan Maturity

This table provides the estimates from panel regressions of the differential effect of the law reform to bank lending. The main regression equation is given by

$$y_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We time-average our data to a pre- and post-event point before we differentiate the natural logarithm. *Treated* corresponds to SMEs affected by the law reform. *Firm Controls* include *Leverage, Altman Z-score, (EBIT/Total Assets)* and *(Gross Margin)/(Total Assets)* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(2) (resp. (3)-(4)) provide the estimates of the causal effect of the reform to Short-term and Long-term credit provision for credit lines (term loans) respectively. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1) Δ log *Short-term* Credit Lines$_{bf}$	(2) Δ log *Long-term* Credit Lines$_{bf}$	(3) Δ log *Short-term* Term Loans$_{bf}$	(4) Δ log *Long-term* Term Loans$_{bf}$
Treated	-0.009	0.135***	-0.009	-0.029**
	(0.0086)	(0.0087)	(0.0142)	(0.0104)
Bank FE	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Obs	109765	38024	60078	186859
R^2	0.049	0.114	0.061	0.057
Adjusted R^2	0.017	0.053	0.011	0.036

Table 8: Main effect of the reform: Matched Sample

This table provides the estimates from panel regressions of the differential effect of the law reform to loan provision. The main regression equation is given by

$$Growth_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

We match treated with control group based on pre-event characteristics which include *Credit Growth (overall, credit lines and term loans), Altman Z- score, Financial Leverage, (Trade Credit / Total Assets), (Fixed Assets / Total Assets), Financial Pressure, Employees* and *Total Assets*. Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the bank-firm level before we differentiate the natural logarithm of the volume. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Credit* is the sum of *Credit Lines* and *Term Loans* volume. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(3) analyze the effect on credit growth and Columns (4)-(5) the overall effect on the probability of default (calculated internally by banks) and on collateral pledged. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1) Δ log Credit	(2) Δ log Credit Lines	(3) Δ log Term Loans	(4) Δ Pr. of Default	(5) Δ log Collateral
Treated	0.018**	0.044***	-0.049***	-0.001	0.043***
	(0.0087)	(0.0071)	(0.0099)	(0.0010)	(0.0131)
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Obs	117050	63762	85066	112797	85954
R^2	0.052	0.115	0.060	0.054	0.657
Adjusted R^2	0.022	0.070	0.022	0.022	0.643

Table 9: Staggered Impact of Law - Quarter regressions

This table provides the estimates from panel regressions of the staggered impact of the law reform to loan provision. The main regression equation is given by

$$Growth_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre-event and to *four* post-event points according to post-event quarters before we differentiate the natural logarithm of the data points per quarter. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Credit* is the sum of *Credit Lines* and *Term Loans* volume. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(3) analyze the effect on credit growth and Columns (4)-(5) the overall effect on the probability of default (calculated internally by banks) and on collateral pledged. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1) Δ log Credit	(2) Δ log Credit Lines	(3) Δ log Term Loans	(4) Δ Pr. of Default	(5) Δ log Collateral
Panel A: Quarter 1					
Treated	0.036***	0.024***	-0.017**	-0.002***	0.024***
	(0.0059)	(0.0047)	(0.0068)	(0.0685)	(0.0081)
Obs	272085	146959	204600	260603	202387
R^2	0.035	0.067	0.049	0.029	0.055
Adjusted R^2	0.019	0.042	0.029	0.012	0.035
Panel B: Quarter 2					
Treated	0.019**	0.047***	-0.052***	-0.002***	0.039***
	(0.0079)	(0.0048)	(0.0089)	(0.0746)	(0.0106)
Obs	263871	141831	197686	252722	195526
R^2	0.034	0.113	0.041	0.030	0.861
Adjusted R^2	0.017	0.088	0.020	0.013	0.858
Panel C: Quarter 3					
Treated	0.022**	0.055***	-0.073***	-0.003***	0.042***
	(0.0090)	(0.0057)	(0.0101)	(0.0891)	(0.0118)
Obs	256950	138493	190772	246314	189891
R^2	0.038	0.119	0.045	0.032	0.055
Adjusted R^2	0.021	0.094	0.024	0.015	0.034
Panel D: Quarter 4					
Treated	0.038***	0.063***	-0.059***	-0.0031***	0.037***
	(0.0102)	(0.0068)	(0.0115)	(0.1009)	(0.0135)
Obs	250257	134413	185474	239950	179694
R^2	0.041	0.111	0.045	0.031	0.043
Adjusted R^2	0.024	0.085	0.024	0.013	0.021
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes

Table 10: The effect of the reform on the Intensive Margin

This table provides the estimates from panel regressions of the differential effect of the law reform to loan provision for the *intensive margin*. Our sample consists of bank-firm-credit mode relationships, that have been present throughout the duration of our investigation. The main regression equation is given by

$$Growth_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the bank-firm level before we differentiate the natural logarithm of the volume. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Credit* is the sum of *Credit Lines* and *Term Loans* volume. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(3) analyze the effect on credit growth and Columns (4)-(5) the overall effect on the probability of default (calculated internally by banks) and on collateral pledged. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1) Δ log Credit	(2) Δ log Credit Lines	(3) Δ log Term Loans	(4) Δ Pr. of Default	(5) Δ log Collateral
Treated	0.033***	0.054***	-0.047***	-0.004***	0.035***
	(0.0066)	(0.0051)	(0.0070)	(0.0007)	(0.0109)
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Obs	215265	119172	152972	209656	169597
R^2	0.043	0.134	0.043	0.028	0.706
Adjusted R^2	0.024	0.106	0.018	0.008	0.699

Table 11: Extensive Margin

This table provides the estimates from panel regressions of the differential effect of the law reform on the *extensive margin*, i.e. on whether banks started or stopped to contract loans after the legal implementation. The main regression equation is given by

$$Y_{bfm} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bfm},$$

where Y_{bfm}, is either the dummy variable $Entries_{bfm}$, (Panel A), which takes 1 if a bank b contracts a new loan with the firm f, for a credit mode m, after the reform, and 0 otherwise; or the dummy variable $Exits_{bfm}$ (Panel B), which takes 1 if a bank b stops lending to the firm f, the credit mode m, after the reform, and 0 otherwise. Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Column (1) in both panels, measures entries and exits at the bank-firm level, while Column 2 (3), measures entries (exits) at the bank-firm-credit lines (term loans), level respectively. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Entries

	(1) Entries	(2) Entries Credit Lines	(3) Entries Term Loans
Treated	-0.003	0.006***	0.002
	(0.0020)	(0.0014)	(0.0023)
Obs	273894	273894	273894
R^2	0.022	0.019	0.023
Adjusted R^2	0.006	0.003	0.007

Panel B: Exits

	(1) Exits	(2) Exits Credit Lines	(3) Exits Term Loans
Treated	0.007***	0.004***	0.017***
	(0.0026)	(0.0015)	(0.0017)
Obs	273894	273894	273894
R^2	0.024	0.020	0.025
Adjusted R^2	0.008	0.004	0.009
Bank FE	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes

Table 12: Likelihood of prepayment: Large increases and decreases in the Intensive Margin

This table reports the effect of the legal reform to the prepayment of credit lines and term loans proxied by large increases and decreases at the bank-firm-credit mode level, in the *intensive margin*. We create a dummy variable that assigns 1, if we have a large increase (decrease), above 25% at the bank-firm-credit mode level, before and after the reform. The main regression equation is given by

$$Y_{bfm} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bfm},$$

where Y_{bfm}, corresponds either to large increases or decreases. Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1) and (2) analyze the effect on large increases for credit lines and term loans, while Columns (3) and (4) report the estimates of the differential impact on large decreases. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
	Incr. Credit Lines	Incr. Term Loans	Decr. Credit Lines	Decr. Term Loans
Treated	0.050***	-0.044***	-0.004	0.069***
	(0.0092)	(0.0083)	(0.0043)	(0.0075)
Bank FE	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Obs	119159	152957	119159	152957
R^2	0.084	0.044	0.036	0.098
Adjusted R^2	0.054	0.019	0.005	0.074

Table 13: Legal reform and Loan Rates

This table reports the impact of the legal reform to loan rates among all firms, as well as decomposed to firms with only credit lines and term loans (Panel A) and further on maturities (short-term and long-term) in Panel B. The main regression equation is given by

$$\Delta Y_f = b_1 Treated_f + b_2 FirmControls_f + a_{fc} + \epsilon_f,$$

where Y_f corresponds to the loan rate in percentages (computed from a firm's balance sheet statements) of firm f. Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the firm level before we differentiate our data points. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(3) (Panel A) analyze the effect on implicit rates, for all firms and for firms that employ only credit lines and term loans, for both periods respectively. In Panel B, we compute the change in the implicit rates for firms that employ only credit lines or term loans, further decomposed based on maturity (short-term and long-term). We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A

	(1) All	(2) Only Credit Lines	(3) Only Term Loans
Treated	0.017***	0.044	0.068***
	(0.0023)	(0.1422)	(0.0140)
Firm Cluster FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Obs	144531	10519	58612
R^2	0.036	0.141	0.056
Adjusted R^2	0.010	0.010	0.006

Panel B

	(1) Short-term Credit Lines	(2) Long-term Credit Lines	(3) Short-term Term Loans	(4) Long-term Term Loans
Treated	0.009	0.085	0.039	0.030**
	(0.0646)	(0.0626)	(0.0279)	(0.0143)
Firm Cluster FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Obs	5066	1603	922	32478
R^2	0.182	0.284	0.295	0.097
Adjusted R^2	0.011	0.005	-0.037	0.029

Table 14: Heterogeneity in Credit composition effects: Firm Characteristics

This table provides the estimates of the reform to credit supply, conditional on various pre-treatment firm characteristics; relationship strength (overall and exact duration), riskiness (leverage, financial pressure and Altman Z-score) and liquidity management (cash holdings). We include these variables as well as their interaction with the *Treated* dummy. The main regression equation is given by

$$Growth_{bf} = b_1 Treated_f * Characteristic_{(b)f} + b_2 Characteristic_{(b)f} + \\ + b_3 Treated + b_4 FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the bank-firm level before we differentiate the natural logarithm of the data points. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Column (1) (resp. (2)), analyzes the effect on credit lines (resp. term loans). Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1) Δ log Credit Lines	(2) Δ log Term Loans
Panel A: Relat. Strength		
Treated * Relat_Dummy	0.113***	-0.040
	(0.0409)	(0.0291)
Obs	147708	206780
R^2	0.102	0.049
Adjusted R^2	0.077	0.030
Panel B: Relat. Duration		
Treated * Relat_Duration	0.006**	-0.002
	(0.0029)	(0.0019)
Obs	147708	206780
R^2	0.103	0.054
Adjusted R^2	0.079	0.034
Panel C: Borrower's Leverage		
Treated * Leverage	0.062**	-0.048
	(0.0266)	(0.0443)
Obs	147708	206780
R^2	0.099	0.046
Adjusted R^2	0.074	0.026

Table Continue: Heterogeneity in Credit composition effects: Firm Characteristics

	(1) Δ log Credit Lines	(2) Δ log Term Loans
Panel D: Financial Pressure		
Treated * Financial Pr.	-0.007	0.006
	(0.0062)	(0.0075)
Obs	147708	206780
R^2	0.098	0.046
Adjusted R^2	0.074	0.026
Panel E: Altman Z-score		
Treated * Altman Z-score	-0.005	-0.003
	(0.0047)	(0.0103)
Obs	147511	206719
R^2	0.099	0.047
Adjusted R^2	0.075	0.027
Panel F: Cash Holdings		
Treated * Cash Hold.	-0.021	-0.013
	(0.0350)	(0.0841)
Obs	147708	206780
R^2	0.099	0.046
Adjusted R^2	0.075	0.026
Bank FE	Yes	Yes
Firm Cluster FE	Yes	Yes
Firm Controls	Yes	Yes

Table 15: Legal reform and firms with multiple bank relationships

This table provides the estimates of the reform to credit supply, conditional on the relationship strength (overall and exact duration) for the firms with *multiple* bank relationships. The main regression equation is given by

$$Growth_{bf} = b_1 Treated_f * Characteristic_{(b)f} + b_2 Characteristic_{(b)f} + v_b + a_f + \epsilon_{bf}.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the bank-firm level before we differentiate the natural logarithm of the data points. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. Column (1) (resp. (2)), analyzes the effect on credit lines (resp. term loans). Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1) Δ log Credit Lines	(2) Δ log Term Loans
Panel A: Relat. Strength		
Treated * Relat_Dummy	0.041	-0.014*
	(0.1147)	(0.0729)
Obs	32848	67614
R^2	0.509	0.461
Adjusted R^2	0.056	-0.004
Panel B: Relat. Duration		
Treated * Relat_Duration	-0.000	-0.005
	(0.0083)	(0.0046)
Obs	32848	67614
R^2	0.511	0.466
Adjusted R^2	0.059	0.004
Bank FE	Yes	Yes
Firm FE	Yes	Yes

Table 16: Real Effects: Descriptive Statistics

This table provides the descriptive statistics for the real effects variables. Data are provided by the NBB.

	Obs	Mean	St.Dev	P05	P25	p50	p75	P95
firm level								
Δ log Total Assets - Growth$_f$	202184	0.005	0.307	-0.445	-0.103	-0.008	0.111	0.506
Δ log Fixed Assets - Investment$_f$	190621	-0.067	0.630	-0.852	-0.287	-0.080	0.026	1.010

Table 17: Real Effects

This table reports the impact of the legal reform to firm growth and investment proxied by *Total* and *Fixed Assets*. The main regression equation is given by

$$\Delta Y_f = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_f.$$

Our sample extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the firm level before we differentiate the natural logarithm of the data points. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), Leverage* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Column (1) analyze the effect on growth, while on Column (2) we report the estimate of the differential impact on investment. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)
	Δ log Total Assets	Δ log Fixed Assets
Treated	-0.011***	-0.029***
	(0.0039)	(0.0075)
Firm Cluster FE	Yes	Yes
Firm Controls	Yes	Yes
Obs	202184	190621
R^2	0.024	0.024
Adjusted R^2	0.003	0.003

Appendix

A0.1 Parallel Trends Assumption

The validity of a difference-in-difference estimator requires the existence of a group, which is similar to the treated, but not affected by the regulation. Thus in the absence of treatment, the two groups would follow similar trends.[27] Since our focus is on credit growth, we need to verify that pre-event the two groups demonstrated similar credit evolution. Figure 1, offers a visual inspection of the credit evolution, and we postulate that the control and treated groups, are not governed by any pre-treatment trends. Since plotting, offers only an indication and does not constitute a formal test for the validity of the diff-in-diff estimator (see, e.g., Carletti et al., 2020; Degryse et al., 2021), we formally test our premise using regression analysis, employing a falsified specification. We follow Derrien and Kecskés (2013), and we focus in the pre-event period to examine the existence of pre-treatment trends, by regressing the pre-event difference in credit growth between the two groups over time. Our results point to the validity of the parallel trends assumption. Table A1 reports the coefficients for our main variables.

*** **Please Insert Table A1 around here** ***

The statistical significance of the slope term, determines the acceptance, or the fail to reject of the null hypothesis, which corresponds to the validity of the parallel trends. With the exception of the PD, in all of our falsified models, we are unable to reject the H_0 and therefore the main assumption for the statistical validity of the diff-in-diff econometric model holds.

A0.2 Robustness on the Supporting Factor

In January 2014, the SME supporting factor, suggested by the Basel committee was implemented. The implementation date was identical to the date of the legal reform that we examine in our paper. Even though our proposed identification presented in Section 4, is robust to the SME supporting factor, we further investigate the implications of the SF to Belgian firms, by including all firms in our sample and estimating the following model.

[27]There is a large heterogeneity between firms to expect that SMEs and non-SMEs would have exact pre-treatment behavior.

$$Growth_{bf} = b1Treated_{LegalReform} + b_2Treated_{SupportingFactor} + b_3FirmControls_f + v_b + a_{fc} + \epsilon_{bf},$$

where growth represents the main variables, presented in Table 4. $Treated_{LegalReform}$ is a dummy the assigns 1 to firms that are classified as SMEs according to the Belgian law and have turnover below EUR50 million. $Treated_{SupportingFactor}$ is a dummy variable that assigns 1 if a firm is eligible for the supporting factor and 0 if not (i.e. turnover larger than 50 million). We saturate our model by firm controls, bank fixed effects and firm-cluster fixed effects. Table A2, reports the results of the regression.

*** **Please Insert Table A2 around here** ***

We notice that the results obtained by the coefficient of variable $Treated_{LegalReform}$, are quantitatively similar to the one reported in Section 4, while the the coefficient of the $Treated_{SupportingFactor}$ variable, with the exception of PD are insignificant. We expected to find a drop in PD due to the SF, since banks need to asses riskiness of the firms, in order to reflect the actual risk weighted equity ratio after the suggestions of the Basel committee.

A0.3 Bank level analysis: Average effects

In this section we investigate the implications of the reform to some key banking variables. In particular we focus to *(Com. Equity / Total Assets), (NPL / Loans)*, and *(Loans / Deposits)*. The legal reform increased the cost per unit of loan granted by banks. Therefore one concern that we need to investigate is whether banks reduced PD in order to hold less equity and compensate for this cost. Under the identification assumption that treated firms are uniformly distributed among banks, a difference estimator would accurately measure the implication of the reform to the banking sector. We use the following regression model:

$$Y_b = b_1Post_t + b_2BankControls_b + v_b + \epsilon_b,$$

where Y_b, represents the logarithm of the main key banking variables and b_1 identifies the coefficient of interest. Table A3, reports the regression estimates.

*** **Please Insert Table A3 around here** ***

In both panels, in Column (1) we observe insignificant coefficients that leads us to the conclusion that banks maintained the same level of equity after the reform and hence did not use PD as an instrument to compensate for the additional cost imposed by the new act.

A0.4 Actuarial value of the reform

The legal act gives the option to the borrower to early repay the loan. This can be particularly important in periods were interest rates are dropping. In Section 6.1, using a difference-in-difference empirical methodology, we identified the effect of the reform to interest rates, which was in the magnitude of around 3 b.p. That is the increase in the value of the product due to the inclusion of the option. The rational lies in the remark that after the reform, the price of the product has to adjust in order to reflect the new fundamentals. In this section, we estimate the actuarial benefit of the act by simulating future interest rate realizations, and calculating the interest rate savings that the borrower has. In order to obtain interest rates which are not adjusted to the new fundamentals, we focus to loans contracted one year prior to the act, and we assume that the borrower can exercise the prepayment option. In particular, we use the implicit rate for firms that have *Long-term* term loans, originated in 2013, and for each of these loans, we "follow" them in time, at a monthly frequency, and we compute if the borrower, based on the simulated rate would exercise the option for a duration of 5 years after the reform and for a total of 50,000 interest rate trajectories. In the sequel, we take the grand average of the *new* interest rate that the borrowers pay, and we compare it to actual payments upon origination. This difference which corresponds to the value of the option, and was computed to 3 b.p. in Section 6.1, is derived by the simulations to be 3.6 b.p.

For our simulations, we assume that the term structure of the interest rate is follows the Vasicek model, i.e., the instantaneous interest rate is given by the following stochastic differential equation:

$$dr_t = a(b - r_t)dt + \sigma dW_t,$$

where a is the speed of reversion, b the long-term mean level, σ the volatility, and W_t a Wiener process (for more details see, e.g., Brigo and Mercurio, 2007). We use the 10 year Belgium Government Bond, in order to estimate the parameters of the Vasicek model, and in the sequel we estimate 50,000 interest rate trajectories, at a monthly frequency.

For each loan originated in 2013, and hence its price was not adjusted to reflect the new

fundamentals, and for each month after the origination we compute the following quantity:

$$L_{l,t,i} = r_l(T-t) - \big(r^s_{t,i}(T-t) + 6r_l\big),$$

where r_l, corresponds to the original interest rate of loan l, $r^s_{t,i}$ is the simulated interest rate of month t for the i simulation, and $(T-t)$ counts the residual maturity. The last term is the penalty that the borrower has to pay in order to repay the loan. We notice that if the quantity $L_{l,t,i}$ is positive, then the borrower has a benefit to repay the loan, and potentially contract a new loan with maturity $(T-t)$ and interest $r^s_{t,i}$. Here, since we aim in calculating the benefit, we make the assumption that the borrower will not switch to another product. If $L_{l,t,i}$ is negative for all i and t, then the borrower of loan l, will not exercise the option and continue paying the interest rate contracted upon origination. Let $R_{l,i}$ be the overall interest rate that the borrower of loan l, has to pay based on simulation i and we notice that this can be equal to the original interest rate. We also let R_l denote the grand average of $R_{l,i}$ across all i, in order to account for simulation noise. R_l is the *new* interest rate that the borrower of loan l would pay, had he had the possibility to exercise the option in 2013.[28] The quantity

$$B_l = R_l - r_l,$$

identifies the benefit of the act on loan l. Taking the average of B_l, across all loans, we compute the benefit B and is equal to 3.6 b.p.

[28]Notice that for a particular l, R_l can be equal to the interest rate upon origination, i.e., the borrower did not exercise the option.

A1 Additional Tables

Table A1: Pretreatment Trends

This table provides a formal statistical test for the validity of the difference-in-difference parallel trends assumption, employed in (Eq. 1). For our main variables, we report the coefficients and t-statistic of a falsified model, which focuses in the pre-treatment period, and examines the existence of pre-event trends between the two groups measured by the difference of the corresponding variables between Treated and Control. Insignificant coefficients point in the validity of the parallel trend assumption. Columns (1)-(3) focus on credit growth while Columns (4)-(5) on the probability of default (calculated internally by banks) and on collateral pledged. *Credit* is the sum of *Credit Lines* and *Term Loans* volume. Detailed definitions are provided in Table 2. t-statistics are based on Newey and West (1987) standard errors, with number of lags selection following Newey and West (1994), robust to serial correlation and heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Data are provided by the NBB.

	(1)	(2)	(3)	(4)	(5)
	Δ log Credit	Δ log Credit Lines	Δ log Term Loans	Δ Pr. of Default	Δ log Collateral
coefficient	-0.023	0.006	-0.017	-0.002**	-0.151
t-statistic	(-0.8362)	(0.4945)	(-0.5619)	(-2.5894)	(-0.6639)

Table A2: Robustness on the Supporting Factor

This table provides the estimates from panel regressions of the differential effect of the law reform to loan provision while controlling for the SME Supporting Factor (SF). $Treated_{SupportingFactor}$, is a dummy that assigns 1, to firms that are eligible for the SF risk weighted adjustment, and $Treated_{LegalReform}$ is a dummy that takes 1 for firms that are subject to the Belgian law reform. Large firms eligible for the SF but not for the law change are the reference group. The main regression equation is given by

$$Growth_{bf} = b1Treated_{LegalReform} + b_2Treated_{SupportingFactor} + b_3FirmControls_f + v_b + a_{fc} + \epsilon_{bf}.$$

We collapse our data to a pre- and post-event point before we differentiate the natural logarithm. *Credit* is the sum of *Credit Lines* and *Term Loan* volume. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Columns (1)-(3) analyze the effect on credit growth and Columns (4)-(5) the overall effect on the probability of default (calculated internally by banks) and on collateral pledged. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Data are provided by the NBB.

	(1) Δ log Credit	(2) Δ log Credit Lines	(3) Δ log Term Loans	(4) Δ Pr. of Default	(5) Δ log Collateral
Treated$_{LegalReform}$	0.020***	0.042***	-0.061***	-0.002***	0.038***
	(0.0079)	(0.0058)	(0.0088)	(0.0854)	(0.0113)
Treated$_{SupportingFactor}$	0.023	0.011	0.030	-0.006***	0.029
	(0.0181)	(0.0236)	(0.0189)	(0.1463)	(0.0281)
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm Cluster FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Obs	275332	148014	208006	264034	209061
R^2	0.041	0.098	0.046	0.036	0.657
Adjusted R^2	0.025	0.074	0.026	0.019	0.650

Table A3: Bank level analysis: Average effect

This table provides the estimates from panel regressions of the average effect of the law reform to the main bank characteristics. The main regression equation is given by

$$Y_b = b_1 Post_t + b_2 BankControls_b + v_b + \epsilon_b,$$

where Y_b, represents the logarithm of the main key banking variables. Our sample extends from January, 2013 to December 2014, and in Column (1) we report the average effect on *Common Equity/Assets* while Columns (2) and (3) document the effect on *NPL/Loans* and on *Loans/Deposits*. We average our data to a single pre- and post-observation point at the bank level, and *Post* is a dummy that takes one, for the period after the reform. In Panel A we include bank controls (*ROA, ROE, Dep/Assets* and *Cash/Assets*), while in Panel B we use bank fixed effects. We report t-statistics (given in parenthesis) robust on autocorrelation. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Data are provided by the NBB.

Panel A			
	(1) log (Com. Equity/Assets)	(2) log (NPL/Loans)	(3) log (Loans/Deposits)
Post	-0.122	-0.080	0.015
	(0.1262)	(0.3603)	(0.0954)
Bank FE	No	No	No
Bank Controls	Yes	Yes	Yes
Obs	51	41	47
R^2	0.544	0.029	0.312
Adjusted R^2	0.494	-0.110	0.229
Panel B			
Post	0.003	-0.097	0.040
	(0.0183)	(0.0685)	(0.0269)
Bank FE	Yes	Yes	Yes
Bank Controls	No	No	No
Obs	50	40	46
R^2	0.995	0.980	0.972
Adjusted R^2	0.991	0.958	0.943

Table A4: Increments in Credit Lines and Term Loans

This table provides the estimates from panel regressions of the differential effect of the legal reform in the growth of credit lines and term loans combined. Our main variables are the ratio $CreditL./TermL.$ and the *stacked* growth rates for credit lines and term loans. The regression equations that correspond to our two variables respectively are given by

$$y_{bf} = b_1 Treated_f + b_2 FirmControls_f + v_b + a_{fc} + \epsilon_{bf},$$

and

$$Y_{bfm} = b_1 Treated_f * CreditType_{bfm} + b_2 CreditType_{bfm} + v_b + a_f + \epsilon_{bfm}.$$

Our sample contains firms that have exposures for both types of credit and extends from January, 2013 to December 2014 and excludes credit granted to firms with turnover larger than EUR50 millions. We collapse our data to a pre- and post-event point at the bank-firm level before we obtain the ratio for our first variable or we differentiate the natural logarithm of the volume to obtain the growth rates for credit lines and term loans and *stack* them at the firm level to construct our second variable. *Treated* corresponds to SMEs affected by the law reform, while the reference group is large firms. *CreditType* is a dummy that assigns 1 for credit line growth. *Firm Controls* include *(Working Cap./Total Assets), (EBIT/Total Assets), (Retained Earn./Total Assets), (Operating Rev./Total Assets), Leverage, (Fixed Assets/Total Assets)* and *Financial Pressure* for the pre-event period. a_{fc}denotes firm-cluster fixed effects according to Industry*Location, using the two-digit postal code and the two-digit NACE identifier. Column (1) analyze the effect on the ratio of credit lines and term loans, while on Column (2) we report the estimate on the change of the growth rate between credit lines and term loans combined, saturating our model with firm fixed effects a_f. Detailed definitions are provided in Table 2. We report t-statistics based on standard errors (given in parenthesis) clustered at the (Industry*Location) pairs. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Data are provided by the NBB.

	(1) Credit L. / Term L.	(2) Stacked Growth R.
Treated	0.147***	
	(0.0255)	
Treated * Credit Type		0.142***
		(0.0276)
Bank FE	Yes	Yes
Firm Cluster FE	Yes	No
Firm FE	No	Yes
Firm Controls	Yes	No
Obs	80741	162810
R^2	0.060	0.513
Adjusted R^2	0.020	0.083