The Bank Lending Channel of Monetary Policy Has Real Effects

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Abstract

Using a unique identification methodology, we provide evidence that easing collateral requirements has economy-wide causal effects on firms' real outcomes, through increased credit. These effects extend beyond firms with newly eligible collateral because the credit expansion benefits all firms. We categorize banks based on their pre-reform loan portfolios, allowing us to compare banks with varying exposures to the change in collateral constraints but otherwise similar loan portfolios. We introduce a bank-level metric for firms' real outcomes, calculated as a loan-weighted average across borrowers, which enables us to use the same identification for both credit and real effects. The effects on credit and on firms' investment, productivity, and dividends are large.

JEL Codes: E44, E58, G21, G32.

Keywords: Bank lending channel, Collateral constraints, Credit supply, Real effects of monetary policy.

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1 Introduction

The bank lending channel was formalized by Bernanke and Gertler (1995) to describe the increase in bank credit following expansionary monetary policy. The channel has predictions that are largely supported by the behavior of credit aggregates, and validated by the cross-sectional response of banks lending decisions to monetary policy shocks.[1] However, quantifying the putative real effects of the bank lending channel has proven elusive, even though we do know that an expansion in credit supply has a wide range of economic consequences.[2] The difficulty here is to characterize the causal chain of events going from a monetary shock to end effects on firms' real decisions via an expansion in bank lending.

This paper aims to achieve that purpose. We consider an unconventional shock to monetary policy: A relaxation in the eligibility criteria of securities that can be posted as collateral with the monetary authority. Monetary policy is conducted by exchanging central bank reserves against a selected range of eligible private securities, e.g., loans to firms with sufficiently high credit ratings. With years spent at the zero lower bound, changing collateral requirements has become a permanent fixture of central banks' arsenal everywhere.[3]

The shock we consider is well-known: It pertains to a surprise change in the risk threshold of loans that are acceptable as collateral with the European Central Bank. Our primary contribution is to provide causal evidence that the change in collateral requirements initiated a credit expansion and firm-level real responses. These effects were not confined to firms with newly eligible collateral but extended across the entire economy. To do this we employ a novel identification scheme, designed to create groups of banks with different exposures to the change in collateral constraint but otherwise similar loan portfolios. We then characterize the typical firms that borrow from credit-expanding banks and show they exhibit sizable increases in investment, productivity, and dividend distribution. The approach delivers two important results: First, the expansion in credit occurs on the supply side, as it is not driven by an increased demand for credit from the firms whose loans become eligible: It is therefore caused by the policy change. Second, the real effects happen throughout the economy, they are not focused on the subset of treated firms: They provide a measure of the real effects of the bank lending channel.

[1]See among many others Kashyap et al. (1993), or Kashyap and Stein (1995), or Kashyap and Stein (2000).
[2]See among many others Chodorow-Reich (2014) or Alfaro et al. (2021).
[3]In the US, see the Term Auction Facility, the Primary Dealer Credit Facility, and the Term Securities Lending Facility (Del Negro et al. (2017)).

In February 2012, the Banque de France announced that loans to firms with credit rating of 4 would become eligible as collateral, whereas previous eligibility stopped at 4+ (a rating of 4 on the Banque de France's scale is approximately equivalent to a Fitch rating of BB-, with 4+ being less risky). The cut-off implies variation in exposure across banks depending on the share of these newly eligible loans that were held on their loan portfolios prior to the announcement. This variation is largely exogenous to developments in the French economy: The European Central Bank announced at the end of 2011 that national central banks were allowed to implement the change at their leisure, which the Banque de France elicited to do in February 2012. The ECB announcement came as a surprise, as it was issued by the then President Mario Draghi barely one month after he took office. There were no observable changes in the holdings of newly eligible loans in the interim period between the ECB's announcement and the Banque de France's implementation.

We explore whether banks with a larger share of newly eligible loans react differently than others, a conventional difference-in-differences approach. There is however a serious complication: The "treated" banks choose to hold a large fraction of loans that were issued to risky firms (rated 4), which must be the outcome of a meaningful and systematic loan portfolio allocation strategy. In that sense, treated banks are likely to be fundamentally different from untreated ones, in ways that are not necessarily observable. Therefore, the conventional difference-in-differences approach applied to the fraction of eligible loans held by the universe of banks is inherently flawed, in the sense that treated and untreated banks are dis-similar in fundamental and potentially unobserved ways. This issue plagues most of the literature.

An intuitive resolution of this problem is to focus the analysis on a homogeneous subset of banks (or firms), but doing so eschews the generality of the results, without guarantee that the problem is addressed decisively. Our first contribution is to adapt a methodology recently proposed by Carbonnier et al. (2022) in a different setting. The idea is to partition all banks into categories that are determined by the overall composition of their loans portfolios: We compare banks that have a similar distribution of loans, except immediately around the newly eligible loans. The estimation is then performed within these categories, i.e., holding constant the overall features of bank portfolios. The categorization of the data can then be validated by verifying whether the (within-category) estimates are unaffected by the inclusion of bank-specific fixed effects: If so, the categorization absorbs all the relevant time-invariant heterogeneity across banks and the treatment effect is well identified.[4] Since it is performed on the universe of bank

[4]In practice the criterion used for categorization involves the characteristics of each bank's portfolio

lending data, this identification also makes it possible to eliminate "treated" firms (rated 4) from the set of borrowers, which rules out an explanation of the expansion in credit based on increased demand on the part of these firms that benefit from the policy change. Any remaining significant effects must come from an expansion of credit supply.

Our second contribution is a consequence of the first one. We exploit the same identification scheme to estimate the effects of the shock to credit supply on firms' real outcomes. We construct artificial bank-specific firms, whose characteristics (investment, employment, etc.) are given by a loan-weighted average across the firms borrowing from each bank. This generates an association between each bank and the average characteristics of the firms borrowing from it, which we then exploit to identify the real effects of the change in collateral requirements. The estimation is performed in a panel of banks, which makes it possible to identify any real effects within the bank categorization designed in the first step. In other words, we evaluate whether the average firm borrowing from a treated bank displays significantly different real responses than the average firm borrowing from an untreated bank. As before, the estimation is performed within homogeneous categories of banks, which ensures identification of the treatment effect. It is also possible to eliminate treated firms (rated 4) from the sample, which ensures that we document the macroeconomic consequences of an economy-wide increase in credit supply, not merely focused on the subset of treated firms. This extends the Khwaja and Mian (2008) approach to estimating firm real effects while preserving the *ceteris paribus* nature of the identification. The approach can also accommodate the type of reallocation of borrowers across banks documented in Jiménez et al. (2020).

We document large and significant real effects. In response to the reform, the typical firms borrowing from a treated bank increases productivity, investment, and dividend distribution. There is no significant response of employment so that the benefits of the policy seem to accrue to capital holders. The effects of a one standard deviation relaxation in collateral requirements are economically large: Between a quarter and a third of the standard deviation in the corresponding measure of real activity, depending on the specification. Interestingly, these effects are *not* confined to (4-rated) treated firms: There is no statistically significant difference between untreated and treated firms. This documents the powerful widespread effects of collateral policies via an economy-wide expansion of credit.

Related literature. Several papers exploit the quasi-random nature of the 2011/2012 change in collateral eligibility instigated by the ECB. In the Dutch context, Van Bekkum

"around" the treatment level, i.e. according to their holdings of loans above and below 4 rating.

et al. (2018) find that the change in collateral requirement affected bank lending positively in the specific segment that became eligible (Residential Mortgage-Backed Securities). Garcia-Posada and Marchetti (2016) find the policy change in Spain had heterogeneous effects on credit across banks. In Italy, Carpinelli and Crosignani (2021) show a significant positive response of the supply of credit and increased purchases of government bonds by liquid banks. Alves et al. (2021) show an effect on credit supply in the context of the 2008 crisis in Portugal.

A few papers evaluate the impact of the new collateral framework in the French context. Mésonnier et al. (2022) identify an effect on the terms offered to newly eligible borrowing firms vs. (closely-comparable but) not newly eligible firms: They find a reduction in loan rates by 7 basis points. Cahn et al. (2024) discuss the heterogeneous effect on credit for single-bank vs. multi-bank borrowers. Andrade et al. (2019) exploit firms that borrow from multiple banks to isolate the effect of the policy change on credit supply.

We differ from these papers in two ways: First, our estimations are run between banks, not between firms, and within selected categories of banks. This enables us both to identify causal effects and to document the economy-wide response of credit supply. Second, thanks to the introduction of a bank-level metric for firms' real outcomes, we are able to identify the real effects caused by the shock on *all* firms, not only treated ones (i.e., not only firms whose loans became eligible as collateral).

There is an extensive literature interested in the real consequences of credit expansions, though not necessarily triggered by monetary shocks. For example, Jiménez et al. (2020) show that the Spanish housing boom resulted in a significant expansion of credit with no observable real effect because firms tended to replace old loans with newer, cheaper ones. In an emerging market context, Khwaja and Mian (2008) show that small firms can suffer financial distress when their bank reduces credit in response to an exogenous liquidity shock, for lack of alternative lenders. Mian and Sufi (2021) and Favara and Imbs (2015) document that exogenous increases in credit supply triggered by regulation changes affect house prices.

There is limited evidence that (unconventional) monetary policy has real effects via a bank lending channel. Acharya et al. (2019) show that the Outright Monetary Transactions introduced by the European Central Bank in 2012 had no real effects because the increase in credit supply it created was not allocated efficiently. Ferrando et al. (2019) conclude otherwise and document a positive response of small firms' investment and profits. Rüden et al. (2023) provide suggestive evidence that the Long Term Refinancing Operations launched by the European Central Bank after the global financial crisis did not result in observable increase in real activity, but mostly in cash hoarding by borrowers and lenders alike. Darmouni (2017) show that Quantitative Easing had consequences on the supply of credit, but stop short of investigating any real effects.

To our knowledge, our paper is the first to consider the collateral channel of monetary policy, document its consequences on credit supply across all banks, and establish economy-wide real effects in firm-level real outcomes. Significant real effects have not been identified very often in this literature, which begs the question why we find them. One explanation is our estimator, which is designed to guarantee a *ceteris paribus* environment. Another is that we are able to identify economy-wide effects, i.e., not focused on the treated sample of firms or banks. Our findings indicate that the responses of treated and untreated firms are quite similar. As a result, a difference-in-difference analysis between these two groups is unlikely to reveal significant differences. Yet another is that we consider a specific type of unconventional monetary shock, that intervened at a time of significant financial stress with pre-existing unmet demand for credit. It is interesting that such well-timed policy moves should have unambiguously positive consequences on the real economy, since it is typically their very purpose.

2 Data and Methodology

A key methodological contribution of our paper is the discretization of banks into homogeneous categories, adapted from Carbonnier et al. (2022). The purpose of the partition is to construct categories that contain banks with some degree of homogeneity in their overall portfolio composition, while preserving dispersion in their exposure to the treatment, i.e., to 4-rated loans. We perform this discretization over a range of loan ratings that surrounds the newly eligible threshold of 4-rated loans. We discretize banks according to ratings of 4+, 4, 5+, and 5, out of a scale that ranges from 3++ (safest) to 9 and P (bankruptcy).[5]

The categorization is two-dimensional and based on the proportion of bank loans that are below a certain category. The first dimension categorizes banks according to the percentage of loans below (and excluding) the 4+ rating (i.e., loans rated 3++, 3+, and 3), the other dimension categorizes banks according to the percentage of loans below (and including) the 5 rating (i.e., loans rated 3++, 3+, 3, 4+, 4, 5+, and 5). Crossing these two criteria gives rise to "buckets" corresponding to different percentage

[5]Credit ratings are administered by the Banque de France on a twelve point scale: 3++, 3+, 3, 4+, 4, 5+, 5, 6, 7, 8, 9, P. We experimented with alternative categorization ranges with no significant change in our findings.

ranges for the holdings of loans with ratings of 4+, 4 and 5+. Figure 1 illustrates the discretization when there are six discrete categories along each dimension. Each cell in the figure contains the percentage ranges of holdings of loans rated within $[4+, 5]$. For example, the upper left bucket in Figure 1 is populated by banks whose portfolios contain between 80 and 100 percent of loans within $[4+, 5]$. Banks that lend to firms riskier than 4-rated are located in the lower left area of the figure, where holdings of loans below (and excluding) 4+ and below (and including) 5 are low, and therefore loans above 5 are prevalent. Similarly, banks with portfolios more conservative than 4-rated loans are located in the upper right area of the figure, with a majority of loans rated 3 or safer.

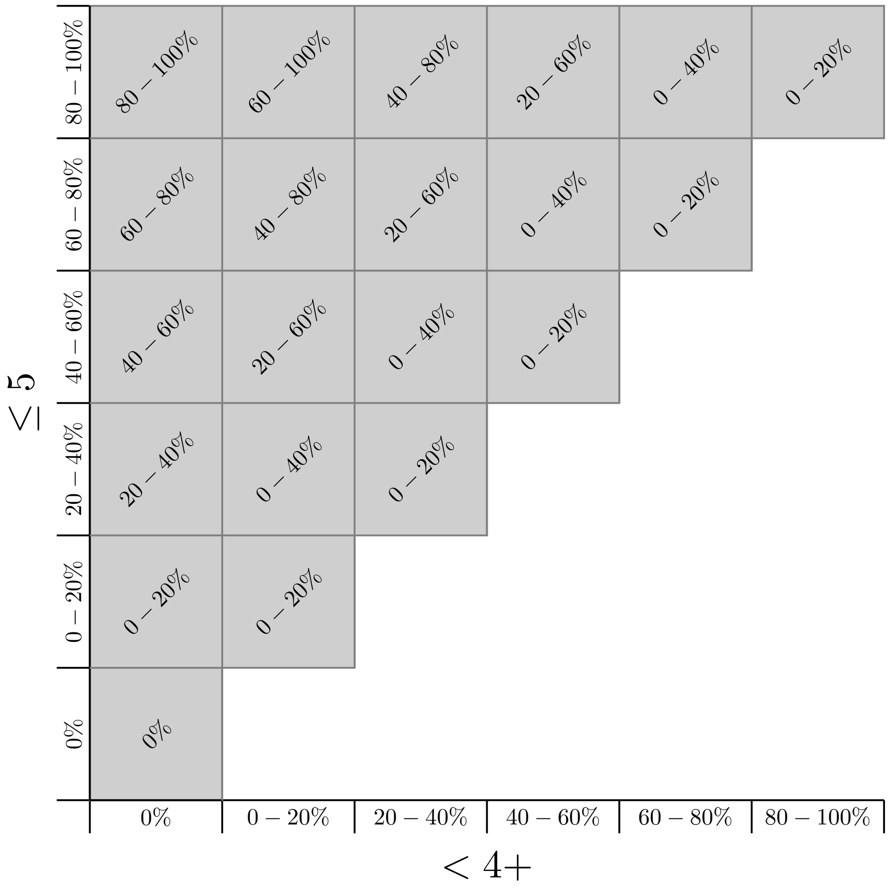


Figure 1: Loan shares by bucket

The two-dimensional discretization of banks sharpens our identification: Our estimations are all run within a "bucket", i.e., within a group of banks whose loan portfolios are relatively homogeneous except immediately around the newly eligible threshold. This ensures the treatment effect is identified *ceteris paribus*, in comparison to banks with overall similar lending strategies. The resulting within-bucket variation in the exposure of banks to 4-rated loans is more likely to be exogenous, as it is influenced by the arbitrary policy cutoff rather than by the bank's underlying lending strategy.

It is still possible that observable (and non-observable) bank heterogeneity survives within-bucket, e.g., according to bank balance sheet or deposits. That can be addressed with further controls that effectively split a bucket into "cells". In what follows we consider banks assets as a criterion to split buckets into cells: Within-cell identification is meant to control for heterogeneity in bank portfolios *and* in bank size. We allow throughout for cell-specific time effects, which absorbs any time-varying characteristic common to all banks in a cell. Ultimately, the question that needs answering in the data is whether heterogeneity between banks within a cell can still be detected empirically. A natural check is whether the inclusion of bank-specific intercepts changes the results of an estimation performed within-cell.

Our approach presents an additional desirable feature when it comes to identifying shocks to the supply of credit with consequences on real activity. In terms of real effects, the literature has focused on conventional treatment effect estimations, where the credit conditions offered to treated firms (i.e., those rated 4) are compared with the conditions offered to other, untreated, firms. The approach potentially conflates supply and demand effects, since treated firms can simply respond to the policy change by demanding more credit, which complicates identification. The discretization performed here achieves identification within categories of banks and real effects are established across all firms, not only those whose loans have a 4-rating. It is difficult to think of reasons why firms that do not have a 4-rating should increase their credit demand in the face of a policy change that does not concern them. It is also difficult to think of reasons why firms that are not directly affected by the policy change should choose to change unilaterally investment or dividend decisions if their access to credit were not modified. This facilitates the identification of a credit supply shock and of its effects on the real economy.

2.1 Data

We merge the French central credit register, the credit rating database, and the FIBEN financial statement database, all from the Banque de France.

The credit register contains data on corporate borrowers with total exposure (debt and guarantees) above 25,000 EUR toward financial intermediaries operating in France. For each bank-firm pair, we recover the end-of-month total outstanding credit (whether it is drawn or not) for each month from January 2011 till December 2014. The register reports a monthly average of 2.5 million bank-firm observations, with information on all existing lines of credit of any type, the geographical location of borrowers, the type of sector they belong to, and whether borrowers are private or public entities. Each bank and firm in the data is uniquely identified by anonymous identifiers (CIB for banks and SIREN for firms). These identifiers allow us to match firms in the credit register with balance sheet data reported in the FIBEN database.

Credit rating information comes from the internal credit rating database at Banque de France, available from FIBEN. The Banque de France attributes credit ratings to around 270,000 companies on an annual basis. Information on a firms' riskiness is updated annually using firm accounting information, provided it is made available.[6] Banque de France ratings indicate a company's ability to meet its financial commitments over a one- to three-year horizon. The criteria for ratings rest on firms' earning power (net income, gross operating surplus, etc.), financial autonomy (self financing capacity, debt stability, etc.), liquidity, and solvency.

We compute the exposure to the policy at the bank level as the share of 4-rated loans in their total pre-reform loan portfolio. In defining a bank's total pre-reform portfolio, we consider all credit lines (short, medium, and long term loans and off-balance sheet credit) extended to every firm, irrespective whether the firm is listed on the FIBEN database. We exclude from our sample any firms whose financial information has not been updated by the Banque de France in the preceding 23 months or more. These firms receive a rating of "X0" on the FIBEN database and constitute a major fraction of aggregate lending.[7] We also exclude inter-bank lending.[8] Lastly, we exclude loans to investment trusts and funds that often benefit from preferential tax treatment. Dropping inactive firms and inter-bank lending reduces the monthly bank-firm observations to an average of around 460,000 out of 2.5 million observations. Most of these choices are standard in empirical work based on the French credit register.

The accounting data on firm balance sheets comes from FIBEN, a database compiled from tax returns by Banque de France. The database is annual and includes all firms

[6]Ratings are also updated throughout the year should new relevant information emerge.

[7]Around 70 percent of all observations are "X0" rated. These are typically small firms lacking administrative data that have small credit lines. Together, they comprise around 60 percent of overall credit.

[8]Inter-bank lending refers to lending to other financial or insurance companies, especially between banks from the same banking group. These comprise a large share of credit volumes (about a third of short-term credit).

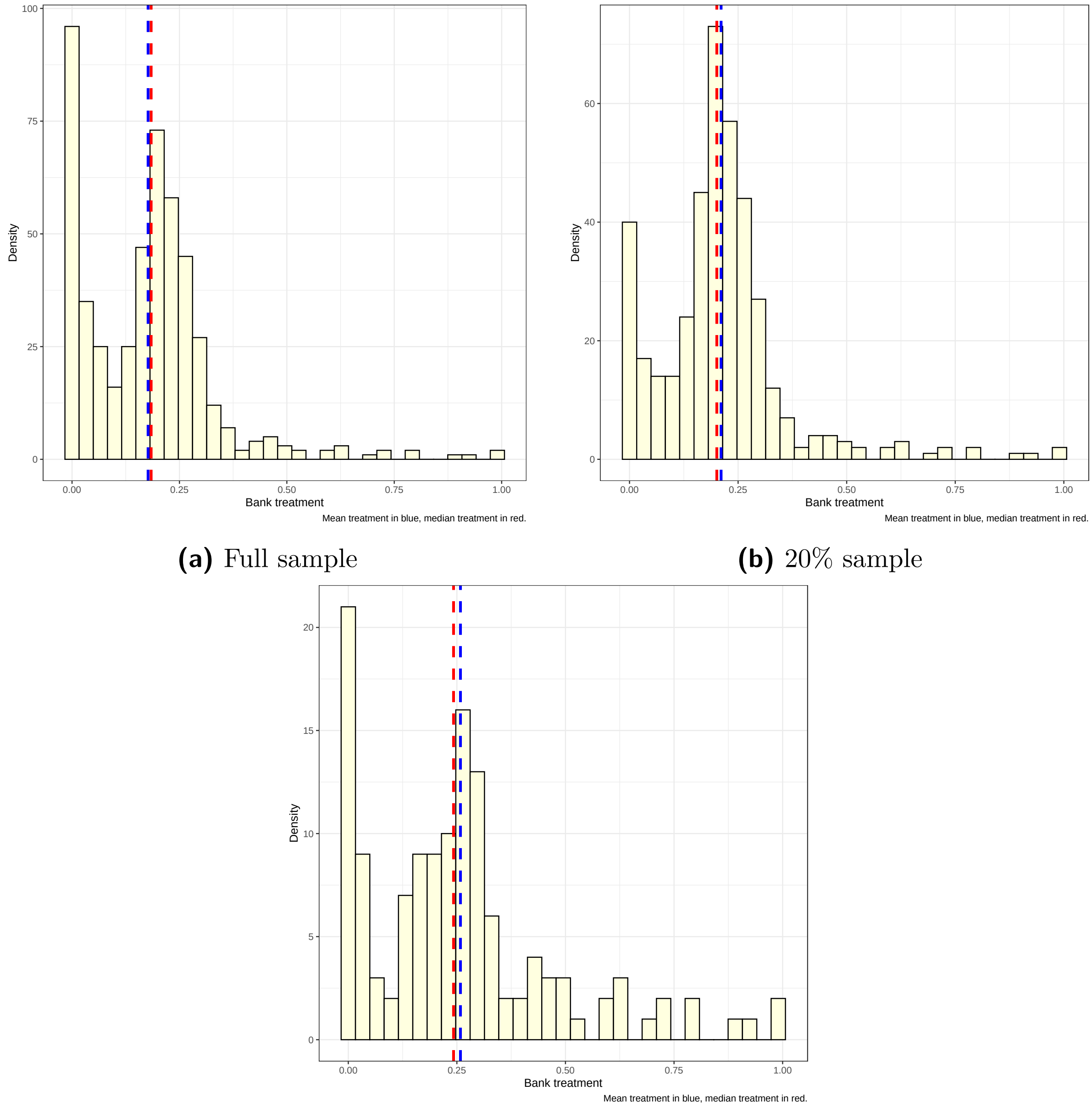
whose turnover in a fiscal year is at least equal to 750,000 EUR. The cut-off of 750,000 EUR is inclusive of all but the very smallest firms.[9] We drop firms with zero total assets. All firm characteristics are winsorized at the 0.5 and 99.5 percentile. Our sample goes from 2009 to 2016.

In addition to the full sample covering the universe of French banks, we consider two sub-samples centered on specific loan ratings. The first one is focused on banks that have at least 20 percent of their portfolio in loans rated between 4+ and 5; The second one raises the fraction to 60 percent. Both sub-samples narrow the analysis onto banks that are substantially affected by the policy change, with the consequence that the discretization is focused on increasingly homogeneous categories of banks. This constitutes a robustness check in the sense that it establishes the extent to which full sample results are due to residual unobserved heterogeneity in banks.

Table A1 in the Appendix highlights some key features of the data. The full sample includes larger banks compared to the two reduced samples. Portfolios in the full sample are generally riskier and consequently have fewer treated loans. All three samples are skewed, which reflects the dominance of a few very large banks in the French banking sector. Borrowing firms are relatively homogeneous across the three samples, perhaps slightly smaller in the 60 percent sample. Firms characteristics are highly dispersed within each of the three samples.

Figure 2 reports the distributions of the share of 4-rated loans in bank portfolios averaged over the 12-month period prior to the reform, from February 2011 to January 2012. The three panels correspond to the three samples. The median and average holdings of 4-rated loans are similar across the three panels, 21 percent in the full sample, closer to 25 percent in the narrowest sample in panel (c): By construction, panels (b) and (c) plot the distributions of banks that hold more 4-rated loans on average. There is no significant difference between median and average holdings in any of the three samples, suggesting relatively low skewness. The majority of banks hold less than 50 percent of 4-rated loans across all three panels.

[9]As per French Law, small and medium-sized enterprises (SMEs) are firms with fewer than 250 employees, with turnover of less than 50 million EUR or total assets less than 43 million EUR.



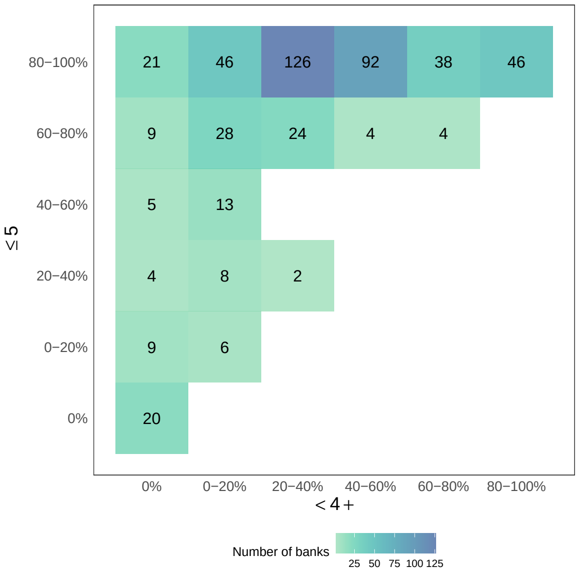
(a) Full sample

(b) 20% sample

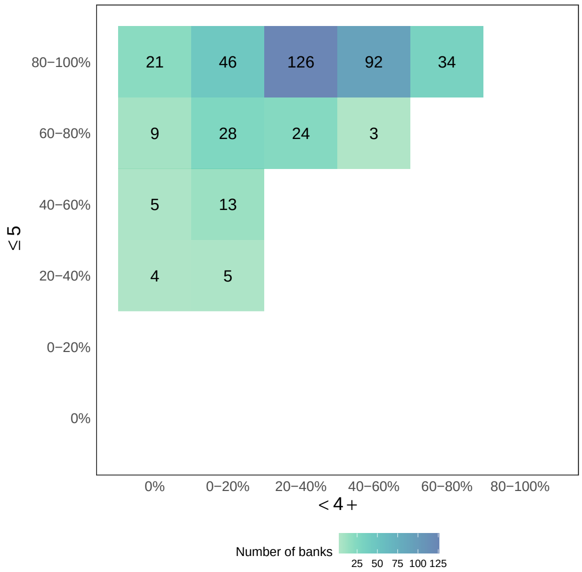
(c) 60% sample

Figure 2: Distribution of treatment intensity in 2011

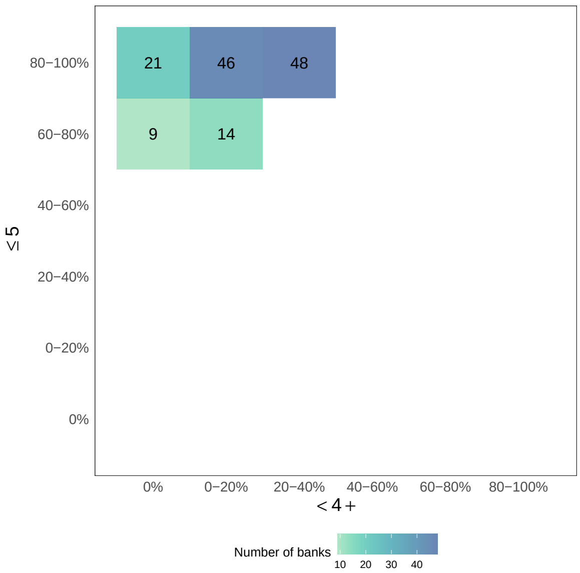
We perform a discretization of banks' portfolios according to the six categories in Figure 1. The categories are chosen to minimize bank heterogeneity within-bucket while preserving enough observations for identification. The three panels in Figure 3 report the number of banks per bucket in the three samples considered, on the basis of their average loan portfolios in the twelve months prior to the policy change. The figure suggests that a majority of banks in all three samples are rather conservative in their lending strategies: They tend to hold relatively large proportions of loans with ratings between 4+ and 5 and are located on the upper region of the figure. Figures 3b and 3c illustrate the assignment of banks to buckets in the two reduced samples we consider. The first sub-sample is focused on banks that hold a minimum of 20 percent of their portfolios in loans rated between 4+ and 5, which means the outer diagonal of Figure 3a is dropped and some banks are omitted from the new diagonal. The resulting matrix is not a complete upper triangular one because some bins are empty.



(a) Full sample



(b) 20% sample



(c) 60% sample

Figure 3: Bank Discretization

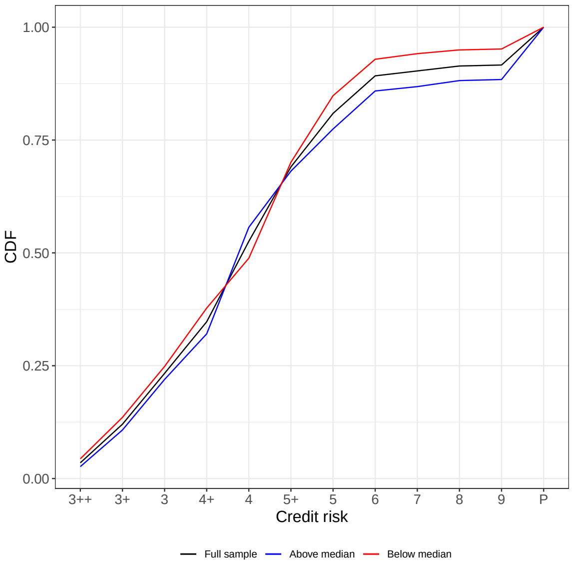
The second sub-sample focuses on banks that maintain at least 60 percent of their portfolio in loans rated between 4+ and 5. Consequently, most of the buckets in the full sample must be purged of banks that do not meet this criterion.

2.2 Identification

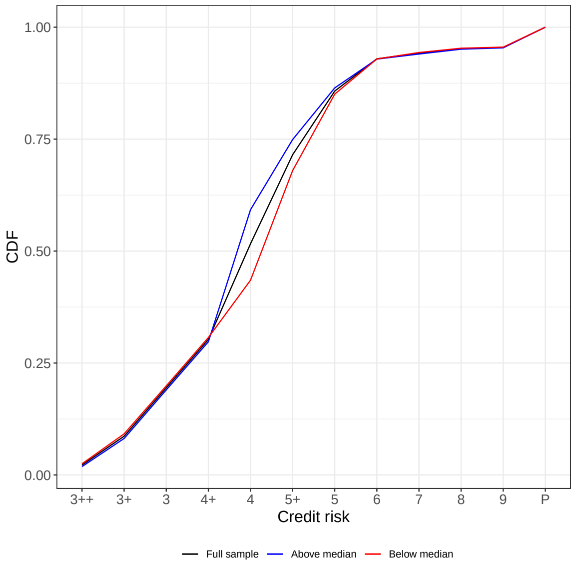
How good is our identification strategy at controlling for the distribution of loan portfolios across banks, while preserving their treatment exposure? We begin to answer this by documenting the cumulative distribution (CDF) of loan shares within-bucket. In each bucket, we compute the cumulative distribution of portfolio shares for all banks and identify the bank with median holdings of 4-rated loans. We then compute the average cumulative distributions for the sub-samples constituted by banks whose holdings of 4-rated loans are above and below that median, still within-bucket. Ideally, we would like loan portfolios to be similarly distributed across banks in all risk ratings except for the $[4+, 5]$ range, where we should see divergence across banks. Figure 4 plots the three distributions corresponding to the three samples we consider. Several facts stand out.

Firstly, the discretization is performed so that the portfolio shares of loans weakly below 4+ and above 5 are similar within-bucket. This happens because by construction all banks in a given bucket hold the same proportion of loans below 4+ and below 5. It follows that the three CDFs in Figure 4 must be very close together from rating 3++ up to (and including) rating 4+, and also from (and including) rating 5 up to bankruptcy P. Secondly, by construction, within a bucket most of the dispersion between banks must happen for ratings 4 and 5+. These two facts are salient in the three samples considered in Figure 4: They are most evident in the sub-samples presented in panels (b) and (c), since these are focused on banks with large holdings of loans between 4+ and 5. Thirdly, in these two panels, the dispersion is largest for holdings of loans rated 4, because there is simply more loans at that rating level in our data.

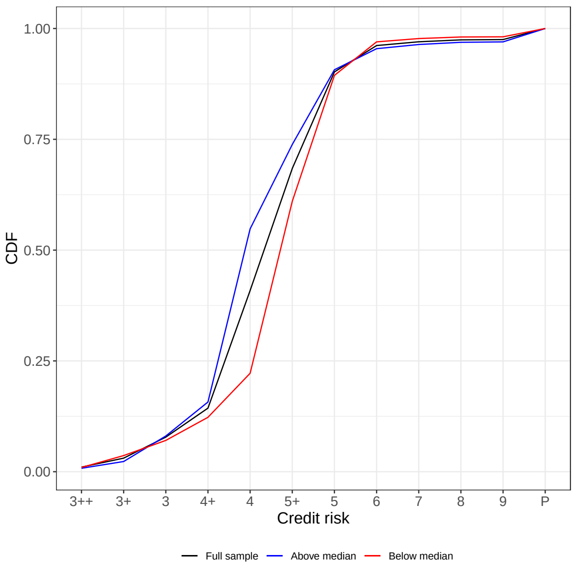
Figure 4 illustrates how the discretization of banks sharpens the treatment effect estimation. The assignment of banks into buckets creates sub-sets of banks in which by construction loan portfolios are very similar *but for* the share of the loans rated 4. Since the estimation is performed within-bucket, the approach holds constant bank portfolios outside of the rating segment that is affected by the change in collateral requirement. As such it provides a precious *ceteris paribus* environment to estimate the treatment effect of interest.



(a) Full sample



(b) 20% sample



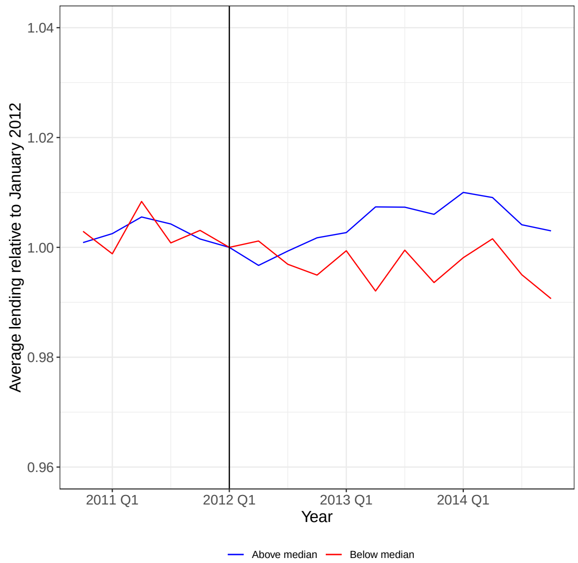
(c) 60% sample

Figure 4: Dispersion within-bucket

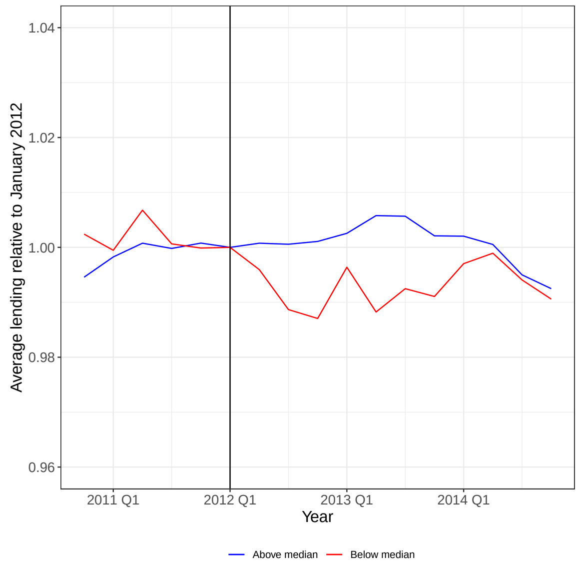
Notes: This figure shows the cumulative distributions (CDF) of loans within-bucket. The black line represents the within-bucket median CDF and the blue (red) line represents the average CDF of loans in banks whose holdings of 4-rated loans are above (below) the within-bucket median.

The categorization of banks ensures some degree of homogeneity across banks' portfolios within each bucket. However, homogeneity serves little purpose for identification if systematic differences in lending policies existed prior to the change in collateral requirement within-bucket. Figure 5 shows average bank lending, distinguishing between those above and below the median within each bucket. The average is computed on the value of newly originated loans across all banks in each group, and normalized to 1 in

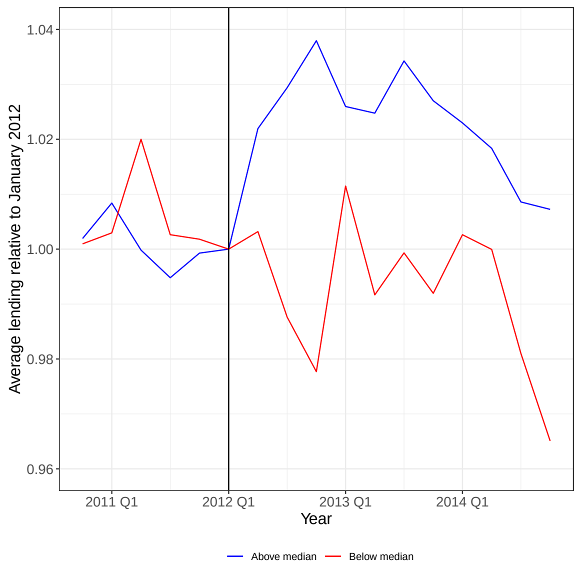
January 2012. The time series is smoothed to quarterly frequency. The three panels in Figure 5 suggest there are no pre-existing differences within-bucket in lending patterns between treated and untreated banks prior to the date of the policy change.



(a) Full sample



(b) 20% sample



(c) 60% sample

Figure 5: Pre-existing trends within-bucket

Notes: Average lending by banks with holdings of 4-rated loans above and below the median value within-bucket.

After February 2012, however, lending grew substantially faster in banks above median exposure to 4-rated loans than for banks below the median. That is true in all three considered samples, most saliently in the one restricted to banks that hold at least 60 percent of their portfolio in loans rated between 4+ and 5. That is to be expected since this is the sample in which banks' portfolios are presumably most affected by the policy change.

We establish formally the absence of any significant pre-existing difference by estimating the well-known specification introduced by Autor (2003):

$$L_{b,t} = \alpha_{c,t} + \alpha_b + \sum_{\substack{k=\{-12,\ldots,20\} \\ k\neq -1}} \beta_1^k \cdot (T_b \times D_{2012m2+k}) + \beta_2 \cdot T_b + \varepsilon_{b,t}, \tag{1}$$

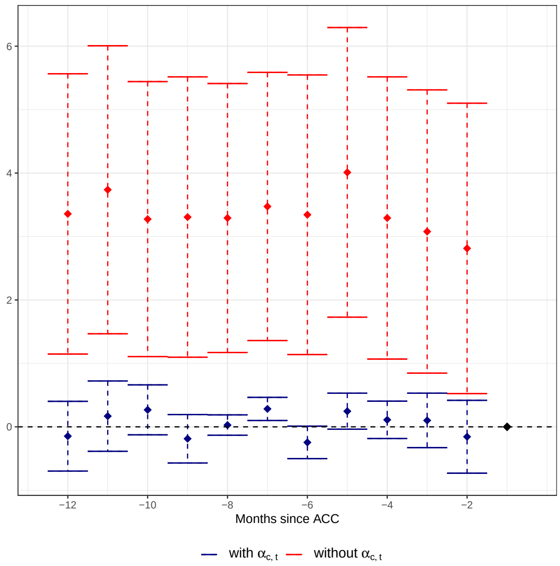
where $L_{b,t}$ denotes the (log) value of new loans originated by bank b at time t, T_b denotes the fraction of 4-rated loans in bank b's portfolio averaged over the 12 months preceding the reform, and $D_{2012m2+k}$ is a binary variably taking value 1 in the month $2012m2 + k$.

The term $\alpha_{c,t}$ refers to cell $\times$ time fixed effects. We define cells as a partition of bucket categories into septiles of size categories, as implied by bank assets as of January 2012.[10] The inclusion of cell fixed effects implies that we are comparing ex-ante similar banks in terms of their overall loan portfolios outside of the $[4+, 5]$ range, since cells exist within buckets, but also in terms of their size. The fact that cell fixed effects are time varying allows for the characteristics of banks within a cell to change over time.

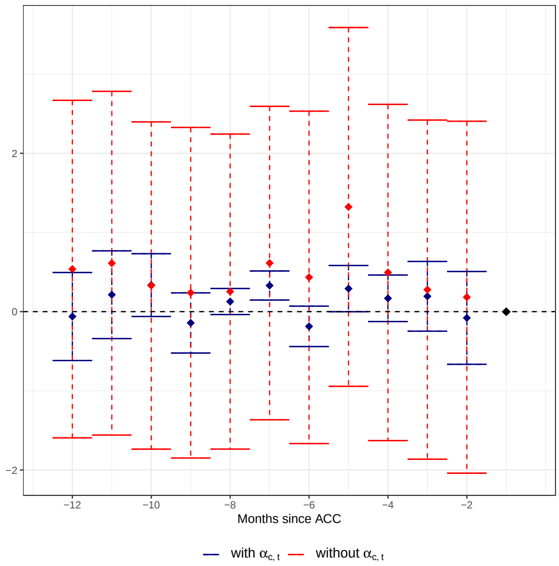
Figure 6 reports the estimates of β_1^k in equation 1 for $k < 0$, with or without $\alpha_{c,t}$.[11] Estimates of β_1^k are never significantly different from zero for $k < 0$ when $\alpha_{c,t}$ is included, which confirms the relevance of within-cell estimations for identification. But in the full sample, excluding cell $\times$ time fixed effects clearly creates an identification issue in the sense that the dynamics of loans origination are significantly heterogeneous *between cells* prior to the policy change. While the heterogeneity ceases to be significant in panels (b) and (c) focused on narrower samples, estimates of β_1^k are considerably more precise when cell $\times$ time effects are included. This confirms the importance of the key element in this paper's identification strategy, viz. the discretization of the panel of banks into cells that contain similar banks but for their holdings of the treated loans.

[10] We replicate the analysis with deciles and find our results are robust. We retain septiles throughout the paper as these imply more populated cells.

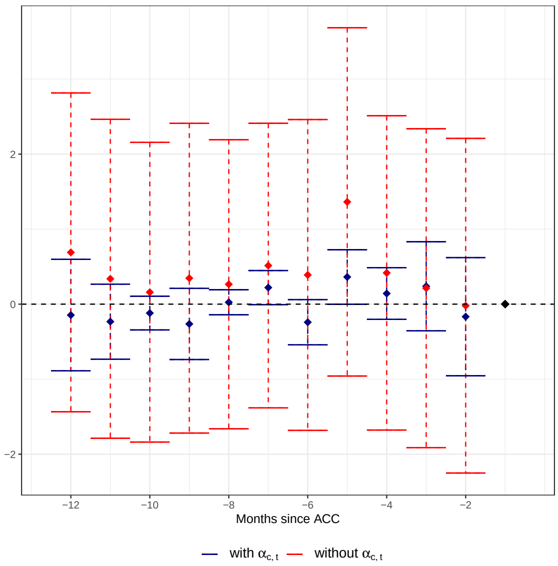
[11] In order to focus the comparison on the inclusion of cell effects, time effects are included when $\alpha_{c,t}$ is omitted.



(a) Full sample



(b) 20% sample

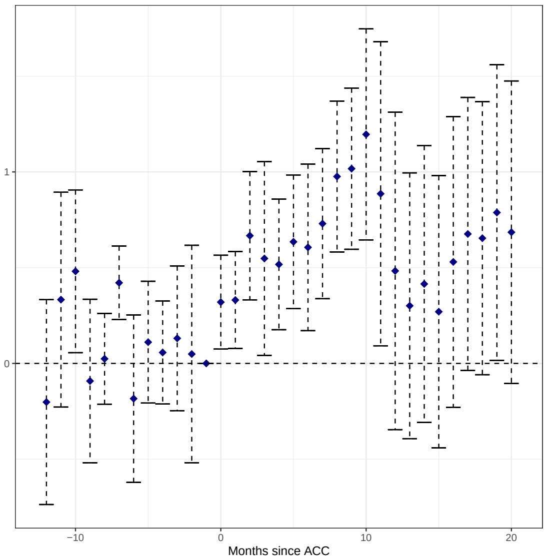


(c) 60% sample

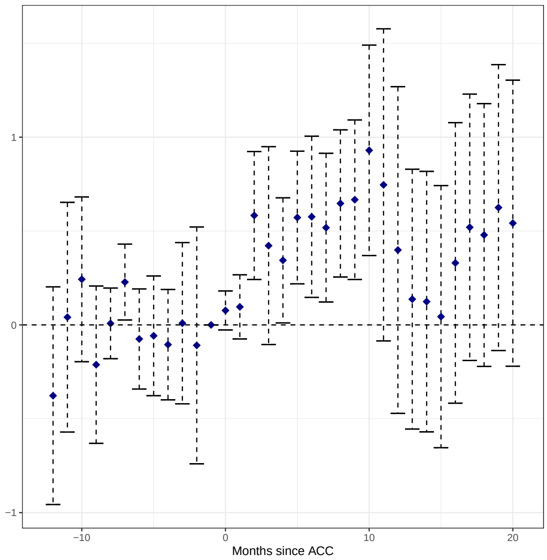
Figure 6: Pre-existing trends with and without cell $\times$ time effects

Notes: This figure plots the value of β_1^k for $k < 0$ in equation (1) with and without cell $\times$ time fixed effects along with 95% confidence intervals. Time effects are included when $\alpha_{c,t}$ is omitted.

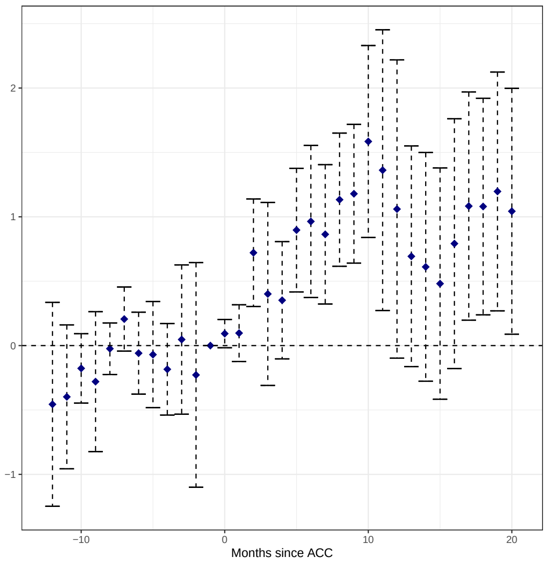
Figure 7 plots β_1^k from equation 1 for all values of $k = (-12, ..., 20)$ and including cell $\times$ time fixed effects. The figure confirms the lack of pre-existing trends within-cell across all three samples. It also reveals the emergence of a significant heterogeneity between banks within cell according to the intensity of their treatment for $k > 0$.



(a) Full sample



(b) 20% sample

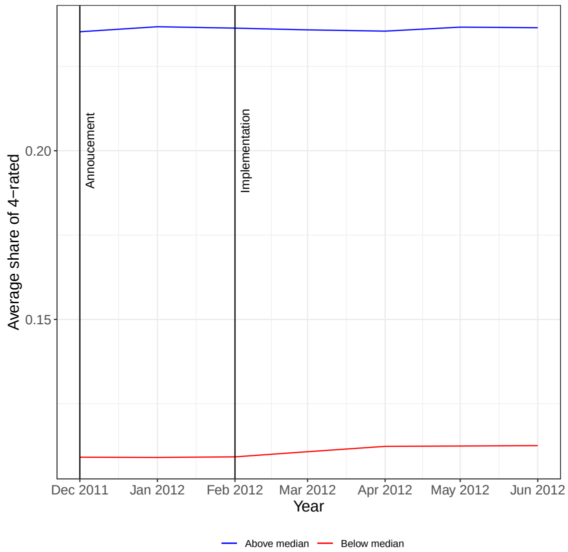


(c) 60% sample

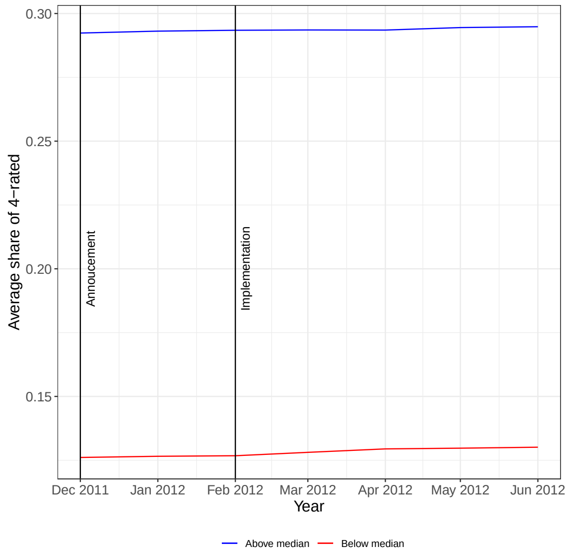
Figure 7: Pre-existing trends within-cell

Notes: This figure plots the value of β_1^k in equation (1) along with 95% confidence intervals.

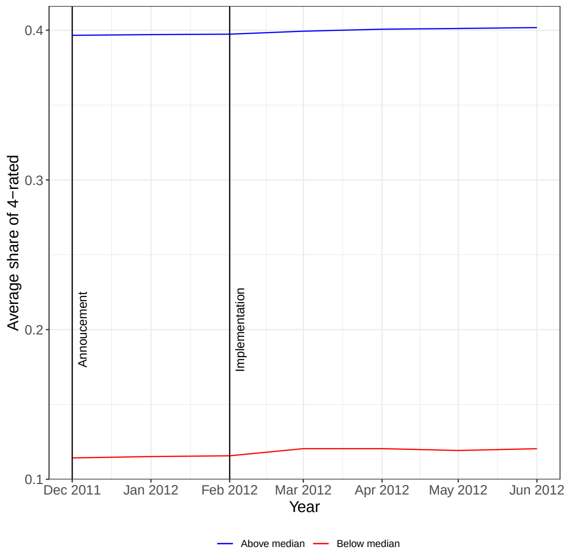
Was the policy shock anticipated by banks? A few months elapsed between the ECB announcement of a change in the collateral requirements and its actual implementation by the Banque de France. The exogeneity of the shock would become questionable if French banks actually took advantage of this interim period to alter their loan portfolios in preparation of the actual change. Figure 8 shows the average holdings of 4-rated loans over time (computed across buckets) for banks with holdings above or below the median within each bucket. Prior to the implementation there is no observable trend in the holdings of 4-rated loans in any of the three samples. A slight upward trends materializes after the implementation for below-median banks that presumably react to the shock by increasing their holdings of now eligibile loans. But there is no endogenous portfolio adjustment prior to the actual shock.



(a) Full sample



(b) 20% sample



(c) 60% sample

Figure 8: Holdings of 4-rated loans between announcement and implementation

Notes: This figure plots the average share of 4-rated loans (computed across buckets) in the overall loan portfolio of banks with treatment above and below the median value within-bucket.

For identification, it is essential that there exist sufficient within-cell variation in exposure to the policy. Table 1 presents a variance decomposition of the share of 4-rated loans in each sample, measured before implementation. In the full sample, 57.4 percent of the variation in the policy exposure happens between banks that belong to the same cell, suggesting identification within cell is easily within reach. Logically, the more concentrated the samples, the lower the between-cell variance.

Table 1: Variance decomposition of treatment

Sample	# Banks	Std. Dev.	Between Cell	Within Cell
Full sample	505	0.156	42.6%	57.4%
20% sample	410	0.151	28.2%	71.8%
60% sample	138	0.223	25.5%	74.5%

Finally, Table 2 checks whether the categorization into cells leaves any residual heterogeneity by measuring the correlation between T_b and three different bank size characteristics. The unconditional correlations are often large, as are, sometimes, the correlations within-bucket. Both facts suggest portfolio allocation is not random as it depends on bank size, not a very surprising conclusion. However, the correlation within-cell is considerably smaller and statistically insignificant in all cases, which indicates that identification within-cell is indeed *ceteris paribus*.

Table 2: Correlations between treatment and bank characteristics

Statistic	Sample	# Banks	Unconditional	Bucket FE	Cell FE
$\rho(\mathrm{T}_b, \mathrm{Assets}_b)$	Full sample	505	0.046	0.061	-0.006
$\rho(\mathrm{T}_b, \mathrm{Capital}_b)$	Full sample	505	0.130***	0.134***	0.013
$\rho(\mathrm{T}_b, \mathrm{Deposits}_b)$	Full sample	505	0.110**	0.132***	-0.018
$\rho(\mathrm{T}_b, \mathrm{Assets}_b)$	20% sample	410	0.040	0.069	-0.008
$\rho(\mathrm{T}_b, \mathrm{Capital}_b)$	20% sample	410	0.130***	0.149***	0.009
$\rho(\mathrm{T}_b, \mathrm{Deposits}_b)$	20% sample	410	0.135**	0.163***	-0.036
$\rho(\mathrm{T}_b, \mathrm{Assets}_b)$	60% sample	138	0.299***	0.300***	0.080
$\rho(\mathrm{T}_b, \mathrm{Capital}_b)$	60% sample	138	0.318***	0.323***	0.072
$\rho(\mathrm{T}_b, \mathrm{Deposits}_b)$	60% sample	138	0.298***	0.313***	0.115

Notes: This table estimates the correlation between bank characteritics and bank treatment. *, ** and *** denote significance at the 10%, 5% and 1% level.

3 Estimations and Results

3.1 Credit effects

Our first objective is to estimate the consequences of the relaxation in collateral constraints on credit supply. We consider the following specification:

$$L_{b,t} = \alpha_{c,t} + \alpha_b + \beta_1 \cdot (T_b \times D_{2012m2}) + \beta_2 \cdot T_b + \varepsilon_{b,t}, \tag{2}$$

where once again $L_{b,t}$ denotes the (log) value of new loans originated by bank b at time t, T_b denotes the fraction of 4-rated loans in bank b's portfolio averaged over the 12 months preceding the reform, and D_{2012m2} is a binary variable taking value 1 after the relaxation in collateral requirements in February 2012. Identification is performed within-cell thanks to the inclusion of $\alpha_{c,t}$. Equation (2) is estimated with and without a bank-specific intercept α_b to gauge the extent to which any time-invariant unobserved heterogeneity survives between banks within a cell, which will happen if the estimates of β_1 (and β_2) are affected by the inclusion of α_b.[12] Equation 2 is estimated at bank-month frequency, with standard errors clustered by bank and year to control for bank-level autocorrelated residuals.[13]

Table 3 reports the estimates of β_1 and β_2 in equation (2), with and without bank fixed effects and for the three samples. Columns (1) and (2) reports the estimates in the full sample: β_1 is positive and significant at the 10 percent confidence level, with a magnitude that is unchanged whether α_b is included or not.[14] β_1 is estimated imprecisely in the full sample, possibly because it contains many banks that are not much affected by the change in collateral requirements.

Columns (3) and (4) in Table 3 report the estimates of equation (2) in the reduced sample formed by banks that have a minimum of 20 percent of their portfolio holdings in loans rated between 4+ and 5. Estimates of β_1 without and with bank fixed effect are positive and significant, estimated with more precision than in the full sample, and not significantly different from each other. They are not significantly different from the

[12] Another reason to include bank fixed effects is the fact that the banking sector in France is dominated by a few large networks of branches belonging to the same mother institution. The credit effect we document could be driven by a central decision-making process at the level of network headquarters. The irrelevance of bank fixed effects for coefficient estimates tells us that the credit effect occurs within bank network, since α_b subsumes bank networks. Appendix Table A2 provides a more direct check by estimating the baseline estimation without bank-specific intercepts but allowing for bank holding group fixed effects. We find no significant change in the estimated credit effect.

[13] Results are robust to clustering by banks alone.

[14] β_2 is subsumed in the bank fixed effect.

Table 3: The response of credit

	Dependent variable:					
	Log(Loans)					
	Full sample		20% sample		60% sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$T_b \times D_{2012m2}$	0.746*	0.694*	0.852**	0.656*	1.297**	1.213**
	(0.272)	(0.294)	(0.280)	(0.304)	(0.297)	(0.386)
T_b	−0.032		−0.372		−0.654	
	(0.564)		(0.568)		(0.634)	
Bank FE	N	Y	N	Y	N	Y
Cell x Time FE	Y	Y	Y	Y	Y	Y
Observations	18,960	18,960	15,752	15,752	4,837	4,837
Adjusted R^2	0.619	0.971	0.601	0.975	0.439	0.957

Notes: This table estimates equation (2). *, ** and *** denote significance at the 10%, 5% and 1% level. Heteroskedasticity-robust standard errors clustered at the bank-month level are in parentheses.

full sample estimates. Finally columns (5) and (6) consider the narrowest sample: The estimates of β_1 are still positive and significant, point estimates are now 50 percent larger, and still of a similar magnitude to each other.

The fact that bank fixed effects make no significant difference suggests the treated and control banks within-cell are similar except for their holding of 4-rated loans, as they should. We conclude that the relaxation of collateral constraints has significant consequences on the supply of credit, especially by banks that hold a substantial proportion of their portfolios in 4-rated loans. The point estimates of β_1 in columns (3) and (5) suggest that a one standard deviation relaxation of collateral constraints results in a 12.9 and 28.9 percent increase in new loans respectively, which corresponds to increases in credit equal to 0.12σ and 0.19σ, where σ denotes the empirical standard deviation in credit.

Relaxing collateral constraints is a tool of monetary policy that purports to have economy-wide consequences: Credit should increase across the board, and not only towards those firms whose loans become eligible as collateral. Since we identify effects within specific categories of banks, it is easy to verify whether it is an increase in credit to newly eligible firms (rated 4) that accounts for our results. Table 4 presents the results of estimating equation (2) omitting all firms whose loans are rated 4, which captures the credit effect

Table 4: The response of credit omitting 4-rated firms

	Dependent variable:					
	Log(Loans)					
	(1)	(2)	(3)	(4)	(5)	(6)
$T_b \times D_{2012m2}$	0.747^{*}	0.693^{*}	0.841^{**}	0.674^{*}	1.215^{**}	1.220^{**}
	(0.270)	(0.295)	(0.278)	(0.315)	(0.266)	(0.423)
T_b	-1.565^{**}		-1.932^{**}		-2.296^{***}	
	(0.517)		(0.512)		(0.484)	
Bank FE	N	Y	N	Y	N	Y
Cell x Time FE	Y	Y	Y	Y	Y	Y
Observations	18,846	18,846	15,656	15,656	4,751	4,751
Adjusted R^2	0.613	0.970	0.600	0.974	0.442	0.953

Notes: This table estimates equation (2) excluding 4-rated firms. *, ** and *** denote significance at the 10%, 5% and 1% level. Heteroskedasticity-robust standard errors clustered at the bank-month level are in parentheses.

for firms that are not directly affected by the policy change. The coefficient estimates are not significantly different from their respective counterparts in Table 3.[15] We conclude most of the credit expansion caused by the relaxation of collateral constraints happened to firms that were in fact unaffected by the policy change, a likely reason why a conventional difference-in-differences approach underestimates its magnitude.

To explore the economic magnitude of the credit response, we run the following aggregation exercise. Following Chodorow-Reich (2014), we assume no substitution in lending between banks such that the aggregate effect is a sum over direct effects. We estimate a version of equation (2) allowing for heterogeneous credit responses across the quartiles of T_b. In particular we estimate:

$$L_{b,t} = \alpha_{c,t} + \alpha_b + \sum_{s \in \{2,3,4\}} \beta_{1,s} \cdot (T_b \times D_{2012m2} \times \mathbb{1}_{b,s}) + \beta_2 \cdot T_b + \varepsilon_{b,t}, \tag{3}$$

where $\mathbb{1}_{b,s}$ is an indicator function taking value one when bank b belongs to quartile s of the distribution of treatment T_b. We compute the aggregate consequence of the policy change as $\sum_{s=2,3,4} \sum_b \; [\omega_{b,s} \times \widehat{\beta}_{1,s} \times (T_{b,s} - T_{b,1})]$, where $\omega_{b,s}$ denotes the 2011

[15]Except for estimates of β_2, which becomes significantly negative. This is to be expected since 4-rated firms are treated banks' bread and butter, and omitting them from the sample mechanically lowers credit supplied by these banks.

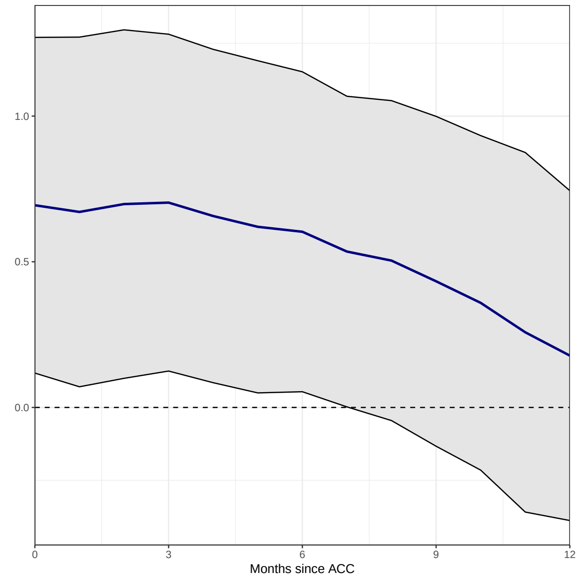
share of the total lending volume originated by bank b in quartile s and $T_{b,s}$ denotes the 2011 share of 4-rated loans in the portfolio of bank b in quartile s. We asssume that banks in the the first quartile do not expand credit. This procedure implies a 14.7 percent increase in aggregate lending because of the policy. Data on total lending to non-financial corporations in France suggest a cumulated credit growth of 11.6 percent in the three years that followed the policy change in 2012Q1.[16] The aggregate effects of the policy shock on credit are substantial.

Finally, we assess the evolution over time of the effects in Table 3 with a local projection estimation following Jorda (2005). The specification becomes:

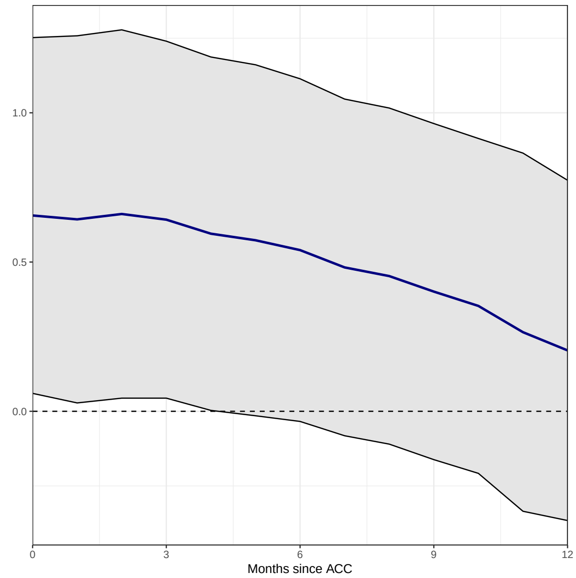
$$L_{b,t+h} = \alpha_{c,t} + \alpha_b + \beta_h \cdot (T_b \times_{2012m2}) + \beta_2 \cdot T_b + \varepsilon_{b,t+h}, \tag{4}$$

for h in $\{0, 1, ..., 12\}$. The estimates of β_h are presented in Figure 9, in three panels corresponding to the three bank samples. The effect of the policy change on credit lasts between 6 and 10 months before becoming insignificant in the three samples. The analysis confirms that the relaxation of collateral requirements has a temporary positive effect on credit, which increases significantly over the few months that follow the policy announcement.

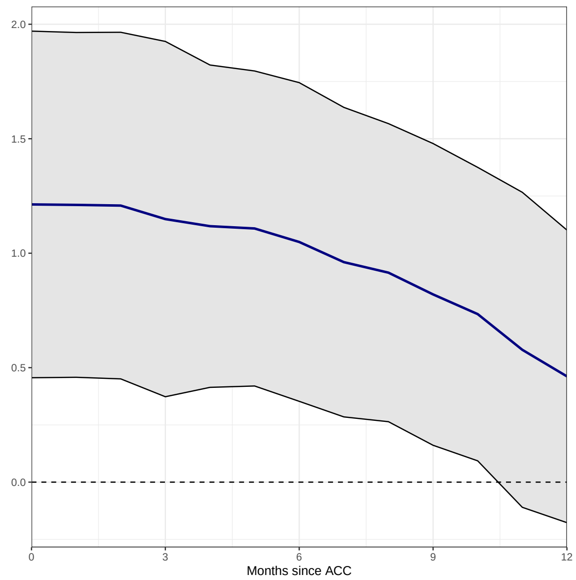
[16]The data come from FRED at the Federal Reserve of St Louis.



(a) Full sample



(b) 20% sample



(c) 60% sample

Figure 9: Credit response - dynamics

Notes: This figure plots the value of β_h in equation (4) with 95 percent confidence intervals.

3.2 Real effects

We now turn to an analysis of the firm-level real effects engendered by the expansion in credit supply. The estimator continues to exploit the partition of banks introduced in Section 2 thanks to a mapping between banks and their borrowers that we introduce to extend the identification to firms' real outcomes. Thus we preserve the *ceteris paribus* nature of identification and we are able to examine whether real effects prevail across *all* firms, not only those whose credit is rated 4.

3.2.1 Approach

We estimate

$$Y_{b,t} = \alpha_{c,t} + \alpha_b + \beta_1 \cdot (T_b \times D_{2012m2}) + \beta_2 \cdot T_b + \varepsilon_{b,t}, \tag{5}$$

where $Y_{b,t}$ denotes the average real outcome (employment, investment, etc.) in the representative synthetic firm that borrows from bank b. We know the characteristics of all borrowing firms and we can pair them with their lenders to compute an average of the characteristics of each bank's borrowers, weighted by the share of each firm in the bank loan portfolio. Formally, for bank b,

$$Y_{b,t} = \sum_f \frac{L_{b,f}^{2011}}{\sum_f L_{b,f}^{2011}} Y_{f,t},$$

where $L_{b,f}^{2011}$ denotes the average value of loans borrowed from bank b by firm f in 2011 and $Y_{f,t}$ denotes a characteristic of firm f at time t, e.g., employment, investment, etc. The rest of the specification is unchanged relative to the previous section: Identification is still obtained within-cell, and the consequences of including a bank fixed effect still help us gauge the extent of residual heterogeneity within-cell. The mapping between banks and firms extends to real firm outcomes the loan-level estimator first introduced by Khwaja and Mian (2008). Owing to the annual frequency of firm financial statements, we estimate equation (5) at bank-year frequency. We cluster standard errors by banks.

An alternative approach to equation (5) is to perform the estimation at firm-level. For instance Cingano et al. (2016), Jiménez et al. (2012), and Jiménez et al. (2020) propose firm-level estimators in matched bank-firm datasets akin to ours, in Italy or in Spain. Firm-level identification is not natural in our context, since the partition of the data is designed to be *ceteris paribus* only within cell. One intuitive alternative is to construct a synthetic cell for each firm, instead of a synthetic firm for each bank, computing a weighted average of the cells a given firm belongs to on the basis of the banks it borrows from. We explored this option and estimated a version of equation (5) modified accordingly. A salient empirical difficulty is that firm-level treatment then becomes a weighted average of bank-level treatments, which actually tends to average out all differences between banks in a cell: Following this approach, we find the empirical dispersion in firm treatment is a small fraction of the dispersion in bank treatment. Unsurprisingly, as a result the coefficients are estimated imprecisely.

An immediate issue with the specification in equation (5) comes from the fact that firms do not typically conduct business with a single bank. Andrade et al. (2019) exploit precisely the existence of multi-bank firms in their identification. Jiménez et al. (2020) document firms respond to a change in lending conditions by reallocating borrowing across banks. This is an issue for us, since a firm could borrow from treated and untreated banks, which would pollute identification. We address the issue in two ways that create two different subsets of firms. In the first subset, we confine the analysis to firms that borrow from banks that are all categorized above (or below) the within-cell median treatment: A firm may borrow from more than one bank, each potentially belonging to different cells, but all the banks that a firm borrows from have to be either above or below the median of the cell they are located in. In other words, the treatment of all the banks lending to a given firm must be homogeneous. This excludes 32,677 firms from the analysis, out of a total of 229,878. In the second subset, we limit the sample of firms to those that borrow at least 75 percent of their total borrowing from a single bank: We then assign this firm to that bank. This results in omitting 86,429 firms.[17] To get a sense of the importance of these omissions we also present the estimation results for equation (5) on the full sample of firms.

Jiménez et al. (2020) show that firms respond to a change in credit conditions: They tend to borrow more from those banks that propose better loan terms, i.e., treated banks, and less from others. Therefore the weighting in equation (5) can respond to the treatment in a potentially systematic manner. Given the definition of that weighting, however, this response will only matter if a negative correlation exists between firms' outcomes and the magnitude of their reallocation in response to the shock: If, for example, firm f with bad real outcomes reallocates more than others towards treated banks, then the weight $\frac{L_{b,f}^{2011}}{\sum_f L_{b,f}^{2011}}$ will overestimate the real outcome of treated bank b's typical firm. It is not clear, however, why firm heterogeneity should prevail in terms of their ability or willingness to take advantage of better credit conditions. If firms reallocate homogeneously to changes in the credit conditions they are offered, there is no bias in equation (5).

The existence of a relationship between a firm and a bank is potentially time-varying, particularly in response to the change in collateral requirements. The characteristics of synthetic firms in equation (5) must therefore be computed on the basis of the bank-firm relationships observed prior to the policy change, lest the real effects we document be caused by new relationships that arise in response to the change in collateral requirements. In practice, we use the lending decisions made by banks in the 12 months that

[17] We experimented with other percentages: a conservative 51 percent and an aggressive 90 percent. Our results were qualitatively unchanged.

precede the reform, from February 2011 to January 2012. But by doing this, we may be missing a substantial part of the expansion of credit that happened after the relaxation of collateral requirements. We must verify that the response of credit did in fact occur mostly at the intensive margin, with no significant increase in the number of firms banks lent to. We estimate:

$$\text{NFirms}_{b,t} = \alpha_{c,t} + \alpha_b + \beta_1 \cdot (T_b \times D_{2012m2}) + \beta_2 \cdot T_b + \varepsilon_{b,t}, \tag{6}$$

where $\text{NFirms}_{b,t}$ denotes the number of new firms borrowing from bank b at time t. The rest of the specification is identical to equation (2) in the previous section.

Table 5 presents the estimates of β_1 when the dependent variable is the number of new bank-firm relationships: They are insignificant across the three samples and whether bank fixed effects are included or not. The credit expansion documented in the previous section happens at the intensive margin, as banks choose to lend more to their existing customers. This result is to be expected given the well-known persistence in bank-firm relationships, which we expect to hold in our data as well.[18]

Table 5: The extensive margin

	Dependent variable:					
	Log(Number of firms)					
	Full sample		20% sample		60% sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$T_b \times D_{2012m2}$	0.288	0.192	0.254	0.120	0.266	0.048
	(0.293)	(0.095)	(0.302)	(0.090)	(0.315)	(0.085)
T_b	0.622		0.411		0.518	
	(0.578)		(0.595)		(0.623)	
Bank FE	N	Y	N	Y	N	Y
Cell x Time FE	Y	Y	Y	Y	Y	Y
Observations	22,117	22,117	18,331	18,331	5,849	5,849
Adjusted R^2	0.636	0.993	0.574	0.995	0.421	0.993

Notes: This table estimates equation (6). *, ** and *** denote significance at the 10%, 5% and 1% level. Heteroskedasticity-robust standard errors clustered at the bank-month level are in parentheses.

We compute the synthetic values for $Y_{b,t}$ using firm-specific data on tangible investment, employment, dividends, and productivity. Tangible investment I_t is computed as a share

[18] See for instance Petersen and Rajan (2002) and Chodorow-Reich (2014).

of (lagged) total assets, employment dN_t is in growth rates, dividends D_t are computed as a share of total (lagged) liabilities, and productivity Z_t is measured by gross operating surplus to total (lagged) sales. The normalizations are introduced to address issues of non-stationarity.

3.2.2 Pre-existing trends

The estimation of equation (5) is performed within-cell, and therefore within samples of homogeneous banks prior to the modification of collateral requirements. However, there is no guarantee that the typical average firms within a cell are similarly homogeneous: These are synthetic firms computed as weighted averages of many potentially very different firms. We need to know whether, before the change in policy, above and below median banks within a cell lend to firms with equal characteristics and equal trends on average, since this is crucial to the identification of real effects. We establish formally the absence of any significant pre-existing difference in the characteristics of synthetic firms by running Welch two-sample (unequal variances) t-tests. The null hypothesis posits that, prior to February 2012, the within-cell averages of I_t, dN_t, D_t, and Z_t have equal means above and below the median. Tables 6 and 7 present the Welch tests for the levels and growth rates of the four outcome variables.

Columns (1)-(3) of Table 6 presents the results of the Welch t-tests on the levels of characteristics for the full sample of banks, columns (4)-(6) for the 20 percent sample, and columns (7)-(9) for the 60 percent sample. The null hypothesis is not rejected in 32 of the 36 cases considered in the table, and in the four cases where the null is rejected, it is at relatively low levels of confidence, i.e., always above 5 percent.

Similarly, Table 7 presents the results of the Welch tests on growth rates. The null hypothesis is never rejected. The data suggest that ex-ante differences in the real performance of firms borrowing from treated versus control banks are rarely significant within cells, which supports the *ceteris paribus* nature of the estimation of real effects.[19]

3.2.3 Results

Table 8 presents the result of estimating equation (5) on the three samples of banks (i.e., the full sample, the 20, and the 60 percent samples), and on the three samples of firms (the full sample, firms with only treated or untreated lenders, and firms with a prime

[19] We have not been able to estimate the specification introduced by Autor (2003) because of the annual frequency of the real variables: There are 8 years in the estimation of real effects, as compared with 48 months in the estimation of the response of credit.

Table 6: Welch two-sample T-test in levels

	Full sample			20% sample			60% sample		
	T-stat	p-value	DoF	T-stat	p-value	DoF	T-stat	p-value	DoF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample of firms									
I_t	1.232	0.218	1,180	0.065	0.949	968	−0.401	0.688	210
dN_t	0.222	0.824	1,141	0.344	0.731	901	0.138	0.890	204
D_t	−1.387	0.166	1,197	−1.404	0.161	998	−1.613	0.108	276
Z_t	−0.518	0.604	1,128	−1.028	0.304	915	−1.307	0.193	191
Panel B: Firms borrowing from only treated or only control									
I_t	1.522	0.128	1,165	1.101	0.271	982	−0.403	0.688	203
dN_t	1.178	0.239	1,109	0.679	0.498	934	−0.021	0.983	183
D_t	−1.905	0.057	1,149	−1.744	0.081	973	−1.600	0.111	266
Z_t	−0.658	0.511	1,050	−0.721	0.471	893	−1.530	0.128	182
Panel C: Firms with a prime lender (75%)									
I_t	0.764	0.445	976	0.307	0.759	824	−0.294	0.769	206
dN_t	1.395	0.163	908	1.344	0.179	802	−0.028	0.977	187
D_t	−1.666	0.096	892	−0.979	0.328	797	−1.566	0.119	274
Z_t	−0.483	0.630	979	−1.011	0.312	850	−1.898	0.059	189

Notes: Welch's t-test compares the means of two independent groups with unequal variances and sample sizes. The two samples are measured using banks within a cell that are either above or below the median treatment bank within that cell prior to the shock. The null hypothesis posits that there are no significant differences in the means of the outcome variables. The t-statistic is calculated as $(\bar{\mu}_1 - \bar{\mu}_2)/\sqrt{\frac{\sigma_1^2}{n_1} + \frac{\sigma_2^2}{n_2}}$, where μ, σ^2 and n are the sample mean, variance, and size respectively.

Table 7: Welch two-sample T-test in growth rates

	Full sample			20% sample			60% sample		
	T-stat	p-value	DoF	T-stat	p-value	DoF	T-stat	p-value	DoF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample of firms									
I_t	0.056	0.955	740	0.539	0.590	471	−0.070	0.944	148
dN_t	−1.198	0.232	453	−0.519	0.604	303	0.519	0.606	71
D_t	0.776	0.438	388	0.656	0.512	345	0.968	0.337	63
Z_t	−0.263	0.793	697	−0.253	0.801	303	0.271	0.787	124
Panel B: Firms borrowing from only treated or only control									
I_t	−1.256	0.210	666	−1.421	0.156	409	−1.016	0.311	160
dN_t	−1.440	0.150	547	−0.998	0.319	320	0.800	0.427	66
D_t	0.007	0.994	313	0.915	0.361	353	0.937	0.352	62
Z_t	−1.032	0.303	536	−1.068	0.286	581	−0.217	0.828	176
Panel C: Firms with a prime lender (75%)									
I_t	1.085	0.278	493	1.443	0.150	414	−1.253	0.212	196
dN_t	−1.099	0.272	327	−1.040	0.299	287	0.483	0.631	71
D_t	−0.548	0.548	410	−0.880	0.379	464	0.897	0.374	59
Z_t	−1.005	0.316	360	−0.289	0.773	287	−1.559	0.121	147

Notes: Welch's t-test compares the means of two independent groups with unequal variances and sample sizes. The two samples are measured using banks within a cell that are either above or below the median treatment bank within that cell prior to the shock. The null hypothesis posits that there are no significant differences in the means of the outcome variables. The t-statistic is calculated as $(\bar{\mu}_1 - \bar{\mu}_2)/\sqrt{\frac{\sigma_1^2}{n_1} + \frac{\sigma_2^2}{n_2}}$, where μ, σ^2 and n are the sample mean, variance, and size respectively.

lender). Bank fixed effects are systematically included, as are cell-time fixed effects. The results are unambiguous: In all but one specification investment, dividends, and productivity increase significantly on impact at conventional confidence levels. Employment growth, on the other hand, does not respond on impact.

Table 9 verifies whether the real effects are confined to the firms that are treated, by omitting altogether firms whose loans are rated 4. The results are not significantly different from the estimates in Table 8. All but two of the coefficients on I_t, D_t, and Z_t are positive and significant. The response of employment growth continues to be insignificant. This confirms that the expansion of credit caused by a modification of collateral constraints benefits *all* firms. The positive responses of tangible investment, productivity, and dividends suggest an effect on capital investment, with no response of employment.

How economically relevant are these responses? Tables 8 and 9 report the sample standard deviations of outcome variables $\sigma(Y)$ across all specifications. From these values, the coefficient estimates reported in Table 8 imply that one standard deviation relaxation in collateral requirement increases tangible investment by an average of 0.3σ (ranging from 0.24σ to 0.39σ depending on the specification), dividends by an average of 0.26σ (ranging from 0.22σ to 0.29σ), and productivity by an average of 0.31σ (ranging from 0.23σ to 0.41σ).

Figure 10 plots the responses of the four outcome variables over time as implied by linear projections up to 3 years after the policy change. All responses are relatively short-lived and stop being significant between six months and a year from the shock. Interestingly, there is a lagged significant negative response of employment growth, which confirms the benefits of the policy change accrue to capital owners at the possible expense of labor.

In results available upon request we estimate equation (5) on a time period that excludes the policy change. We extend the dataset back in time until 2004, include eight years of data (as in the main estimation) until 2011, and posit a placebo treatment date in 2007. This sample stops right before the change in collateral requirement. We find that there are no significant differences in real outcomes between firms that borrowed from treated firms and firms that did not.

3.2.4 Financially constrained firms

Finally, we study whether the real effects of the policy change are magnified for firms that are constrained financially as approximated by Rajan and Zingales (1998) measure

Table 8: Real effects

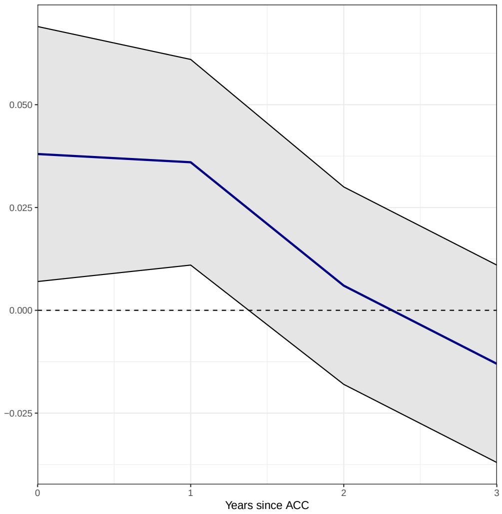
	Dependent variable:											
	Full sample				20% sample				60% sample			
	I_t	dN_t	D_t	Z_t	I_t	dN_t	D_t	Z_t	I_t	dN_t	D_t	Z_t
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Full sample of firms												
$T_b \times D_{2012m2}$	0.027^{**}	0.011	0.015^{***}	0.086^{***}	0.031^{**}	-0.001	0.015^{***}	0.084^{***}	0.038^{**}	0.071	0.019^{**}	0.090^{***}
	(0.012)	(0.045)	(0.005)	(0.022)	(0.012)	(0.047)	(0.006)	(0.022)	(0.016)	(0.053)	(0.018)	(0.029)
Observations	3179	3076	3153	3179	2642	2564	2628	2642	799	744	788	799
Adjusted R^2	0.527	0.299	0.056	0.238	0.512	0.246	0.038	0.251	0.510	0.293	-0.026	0.214
$\sigma(Y)$	0.020	0.066	0.012	0.046	0.019	0.063	0.011	0.041	0.027	0.076	0.014	0.056
Panel B: Firms borrowing from only treated or only control												
$T_b \times D_{2012m2}$	0.026^{**}	0.003	0.015^{***}	0.082^{***}	0.028^{**}	0.001	0.012	0.086^{***}	0.037^{**}	0.064	0.018^{**}	0.091^{***}
	(0.013)	(0.053)	(0.006)	(0.023)	(0.013)	(0.054)	(0.008)	(0.029)	(0.027)	(0.053)	(0.008)	(0.028)
Observations	3076	2967	3051	3079	2602	2516	2585	2602	796	741	785	796
Adjusted R^2	0.503	0.294	0.057	0.212	0.484	0.242	-0.026	0.277	0.496	0.336	-0.030	0.028
$\sigma(Y)$	0.021	0.075	0.013	0.048	0.020	0.070	0.013	0.043	0.027	0.075	0.014	0.056
Panel C: Firms with a prime lender (75%)												
$T_b \times D_{2012m2}$	0.044^{***}	-0.108	0.020^{**}	0.102^{**}	0.045^{***}	-0.092	0.018^{**}	0.103^{**}	0.036^{**}	0.059	0.016^{*}	0.104^{***}
	(0.016)	(0.077)	(0.008)	(0.045)	(0.016)	(0.076)	(0.008)	(0.045)	(0.017)	(0.062)	(0.009)	(0.035)
Observations	2688	2532	2651	2688	2366	2236	2325	2366	788	731	776	788
Adjusted R^2	0.385	0.239	0.100	0.207	0.340	0.184	0.014	0.182	0.497	0.344	-0.013	0.257
$\sigma(Y)$	0.022	0.082	0.014	0.052	0.022	0.079	0.013	0.051	0.027	0.080	0.014	0.056
Bank FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cell × Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table estimates equation (5). *, ** and *** denote significance at the 10%, 5% and 1% level. Heteroskedasticity-robust standard errors clustered at the bank level are in parentheses.

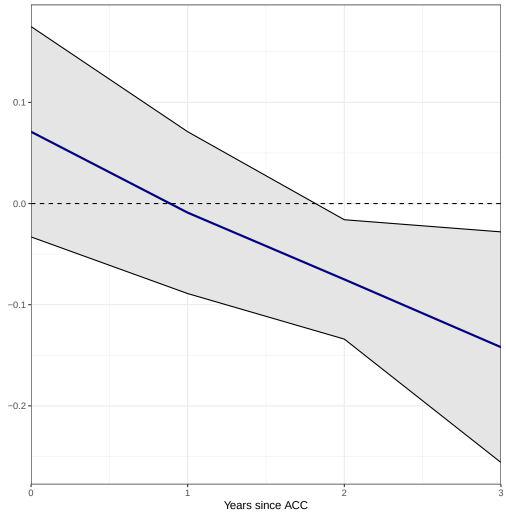
Table 9: Real effects excluding 4-rated firms

	Dependent variable:											
	Full sample				20% sample				60% sample			
	I_t	dN_t	D_t	Z_t	I_t	dN_t	D_t	Z_t	I_t	dN_t	D_t	Z_t
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Full sample of firms												
$T_b \times D_{2012m2}$	0.023*	-0.066	0.013**	0.079***	0.027**	-0.065	0.011**	0.078***	0.036**	0.002	0.015**	0.070**
	(0.013)	(0.051)	(0.005)	(0.023)	(0.013)	(0.053)	(0.005)	(0.023)	(0.018)	(0.038)	(0.007)	(0.030)
Observations	3097	2972	3055	3097	2582	2484	2554	2582	758	695	744	758
Adjusted R^2	0.530	0.260	0.095	0.233	0.482	0.266	0.061	0.225	0.462	0.432	-0.026	0.157
σ(Y)	0.020	0.071	0.012	0.047	0.020	0.067	0.011	0.042	0.027	0.068	0.011	0.056
Panel B: Firms borrowing from only treated or only control												
$T_b \times D_{2012m2}$	0.024*	-0.059	0.012**	0.080***	0.028**	-0.065	0.013**	0.072***	0.036**	0.000	0.015**	0.067**
	(0.013)	(0.056)	(0.005)	(0.023)	(0.014)	(0.058)	(0.005)	(0.027)	(0.018)	(0.038)	(0.007)	(0.029)
Observations	2976	2842	2935	2976	2523	2416	2493	2523	754	691	740	754
Adjusted R^2	0.480	0.287	0.159	0.215	0.425	0.267	0.054	0.237	0.445	0.469	-0.031	0.173
σ(Y)	0.022	0.078	0.012	0.048	0.021	0.072	0.012	0.044	0.027	0.068	0.011	0.056
Panel C: Firms with a prime lender (75%)												
$T_b \times D_{2012m2}$	0.039**	-0.124	0.016**	0.054	0.040**	-0.116	0.013*	0.053	0.034*	0.005	0.016**	0.060**
	(0.018)	(0.082)	(0.008)	(0.036)	(0.018)	(0.081)	(0.007)	(0.036)	(0.018)	(0.039)	(0.007)	(0.028)
Observations	2580	2419	2549	2580	2280	2152	2254	2280	747	684	733	747
Adjusted R^2	0.395	0.240	0.186	0.204	0.357	0.191	0.102	0.170	0.469	0.419	-0.021	0.203
σ(Y)	0.022	0.082	0.013	0.051	0.023	0.078	0.012	0.049	0.027	0.073	0.012	0.055
Bank FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cell × Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

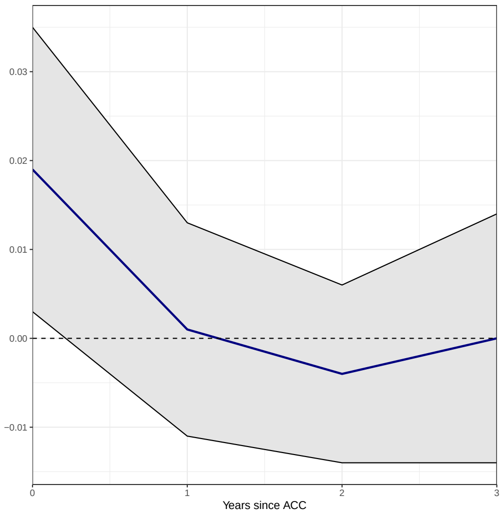
Notes: This table estimates equation (5) excluding 4-rated firms. *, ** and *** denote significance at the 10%, 5% and 1% level. Heteroskedasticity-robust standard errors clustered at the bank level are in parentheses.



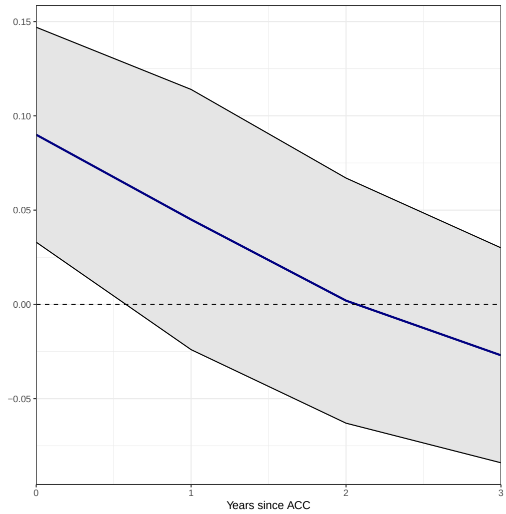
(a) I



(b) dN



(c) D



(d) Z

Figure 10: Local projections for real effects (Full sample of firms)

Notes: This figure plots the estimates of β_1^k in equation (5) for $k = 1, 2, 3$ as implied by linear projections, along with 95% confidence intervals.

of dependence on external finance, EFD. As a reminder, EFD is defined as,

$$\text{EFD} = \frac{\text{Capital expenditure} - \text{Cash flows from operations}}{\text{Capital expenditure}}.$$

Using balance sheet data, we compute firm-level measures of external dependence $EFD_{f,t}$ before the policy shock for t between 2009 and 2011, which we then average over time to allay the concern that access to external financial changes after the reform. We aggregate them using contemporaneous loan weights $\frac{L_{b,f,t}}{\sum_f L_{b,f,t}}$ to obtain a bank-level metric for the external finance dependence of the typical firm borrowing from bank b.[20] We then estimate:

$$\begin{aligned} \mathrm{Y}_{b,t} = \alpha_{c,t} + \alpha_b + \beta_1 \cdot (T_b \times EFD_b \times D_{2012m2}) + \beta_2 \cdot (T_b \times D_{2012m2}) \\ + \beta_3 \cdot (EFD_b \times D_{2012m2}) + \varepsilon_{b,t}, \end{aligned} \tag{7}$$

for investment, dividends, and productivity. Estimates of β_2 measure the real effect of the policy change on unconstrained firms. The real effect on constrained firms is given by $\beta_1 + \beta_2$. Estimates of β_3 measure the real effect on constrained firms borrowing from untreated banks.

Table 10 reports the results. Estimates of β_2 are (almost) always significant and positive, which confirms earlier conclusions. Interestingly, estimates of β_1 are positive and significant in more than a few cases, especially investment that is significant in eight out of nine cases. It is well-known that investment decisions are highly dependent on access to finance, making this result plausible. The coefficient estimate on productivity is also significant in a few cases (4/9). By contrast, estimates of β_3 are all negative and significant for investment, which suggests constrained firms that are historically borrowing from untreated banks tend to curtail investment, perhaps because of the European recession happening in the background. These results suggest a salient asymmetry in the consequences of the policy change, which boosted investment significantly more in financially constrained firms than in the rest of the population.

4 Conclusion

We document large, significant, and widespread effects of an unconventional expansionary monetary policy shock, in the form of an unexpected easing of collateral eligibility. The shock led to an economy-wide credit expansion with substantial positive impacts on

[20] We also normalize EFD_b by its standard deviation across banks.

Table 10: Real effects for constrained firms

	Dependent variable:								
	Full sample			20% sample			60% sample		
	I_t	D_t	Z_t	I_t	D_t	Z_t	I_t	D_t	Z_t
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample of firms									
$T_b \times EFD_b \times D_{2012m2}$	0.011**	0.003	0.007	0.013**	0.005	0.019*	0.019*	0.010*	0.015
	(0.006)	(0.003)	(0.015)	(0.006)	(0.003)	(0.011)	(0.010)	(0.006)	(0.020)
$T_b \times D_{2012m2}$	0.038**	0.019**	0.088***	0.045**	0.021**	0.101***	0.049**	0.027**	0.090***
	(0.016)	(0.008)	(0.029)	(0.018)	(0.009)	(0.027)	(0.021)	(0.011)	(0.032)
$EFD_b \times D_{2012m2}$	-0.004***	0.000	-0.001	-0.005**	-0.001	-0.006	-0.008*	-0.003*	-0.005
	(0.001)	(0.001)	(0.006)	(0.002)	(0.001)	(0.004)	(0.004)	(0.002)	(0.009)
Observations	3159	3138	3159	2627	2618	2627	786	778	786
Adjusted R^2	0.528	0.053	0.241	0.512	0.038	0.257	0.509	-0.027	0.220
Panel B: Firms borrowing from only treated or only control									
$T_b \times EFD_b \times D_{2012m2}$	0.010	0.002	0.022*	0.013**	0.002	0.021	0.018*	0.010*	0.013
	(0.006)	(0.004)	(0.012)	(0.007)	(0.005)	(0.015)	(0.010)	(0.005)	(0.019)
$T_b \times D_{2012m2}$	0.035*	0.018**	0.093***	0.040**	0.014	0.098**	0.047**	0.027**	0.091***
	(0.018)	(0.009)	(0.027)	(0.018)	(0.013)	(0.040)	(0.021)	(0.011)	(0.032)
$EFD_b \times D_{2012m2}$	-0.004**	0.000	-0.011**	-0.006***	0.000	-0.009*	-0.008*	-0.003	-0.005
	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)	(0.005)	(0.004)	(0.002)	(0.009)
Observations	3051	3028	3051	2583	2571	2583	783	775	783
Adjusted R^2	0.503	0.035	0.224	0.485	-0.027	0.294	0.494	-0.032	0.244
Panel C: Firms with a prime lender (75%)									
$T_b \times EFD_b \times D_{2012m2}$	0.013**	0.005	0.037**	0.017**	0.001	0.033**	0.018*	0.008	0.023
	(0.006)	(0.004)	(0.016)	(0.007)	(0.004)	(0.016)	(0.010)	(0.006)	(0.022)
$T_b \times D_{2012m2}$	0.056***	0.025**	0.137**	0.061***	0.020*	0.140**	0.047**	0.023*	0.112**
	(0.021)	(0.010)	(0.056)	(0.022)	(0.010)	(0.059)	(0.022)	(0.014)	(0.044)
$EFD_b \times D_{2012m2}$	-0.005***	-0.001	-0.012**	-0.007***	0.000	-0.009*	-0.008*	-0.002	-0.008
	(0.002)	(0.001)	(0.005)	(0.003)	(0.001)	(0.005)	(0.004)	(0.002)	(0.010)
Observations	2658	2625	2658	2345	2308	2345	773	764	773
Adjusted R^2	0.376	0.112	0.218	0.330	0.013	0.190	0.496	-0.015	0.264
Bank FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cell x Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table estimates equation (7). *, ** and *** denote significance at the 10%, 5% and 1% level. Heteroskedasticity-robust standard errors clustered at the bank level are in parentheses.

firms' productivity, investment, and dividend distributions. These effects are identified causally and the responses in credit and real outcomes are not confined to the firms directly benefiting from the policy change, which may explain the difficulty in documenting them previously. These findings suggest that the bank lending channel can be a powerful tool of monetary policy when the economy is sluggish and, potentially, near the zero lower bound.

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Appendix

Table A1: Summary statistics

	Full sample			20% sample			60% sample		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Panel A: Bank variables									
Loans	552,308	97,712	1,113,680	580,440	158,999	1,063,691	180,488	33,014	363,863
Assets	2,264,614	317,089	7,371,713	2,225,988	364,145	7,057,518	1,126,094	158,666	6,341,112
Deposits	658,934	70,260	1,782,099	698,604	83,010	1,731,231	132,563	42,243	283,462
Capital	176,434	25,713	491,827	175,953	30,521	455,097	99,673	14,213	614,499
T_b	0.18	0.18	0.16	0.21	0.20	0.15	0.26	0.24	0.22
Panel B: Synthetic firm variables									
Investment to assets	0.004	0.001	0.020	0.004	0.001	0.019	0.001	-0.001	0.027
Employment growth	0.002	0.003	0.066	0.003	0.003	0.063	-0.002	-0.001	0.076
Dividends to liabilities	0.001	0.000	0.012	0.001	0.000	0.011	0.000	0.000	0.014
EBE to sales	0.010	0.010	0.046	0.010	0.010	0.041	0.010	0.003	0.056

Notes: Bank variables are in thousands, except T_b.

Table A2: The response of credit controlling for bank holding companies

	Dependent variable:					
	Log(Loans)					
	(1)	(2)	(3)	(4)	(5)	(6)
$T_b \times D_{2012m2}$	0.875^{**}	0.694^{*}	0.899^{**}	0.656^{*}	1.532^{***}	1.213^{**}
	(0.227)	(0.294)	(0.236)	(0.304)	(0.207)	(0.386)
T_b	−0.926		−0.917		−0.652	
	(0.505)		(0.520)		(0.720)	
Bank FE	N	Y	N	Y	N	Y
Bank holding group FE	Y	Y	Y	Y	Y	Y
Cell x Time FE	Y	Y	Y	Y	Y	Y
Observations	18,909	18,909	15,749	15,749	4,837	4,837
Adjusted R^2	0.749	0.971	0.744	0.975	0.731	0.957

Notes: This table estimates equation (2) controlling for bank holding companies. *, ** and *** denote significance at the 10%, 5% and 1% level. Heteroskedasticity-robust standard errors clustered at the bank and year level are in parentheses.