The Real Effects of the Bank Lending Channel of Monetary Policy

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Abstract

We present evidence that a loosening in collateral requirements instigated by the European Central Bank in 2012 had economy-wide real effects on firms' investment, productivity, and dividends, via an aggregate expansion of bank lending. This is a novel result, obtained thanks to our identification methodology. We partition banks into categories according to the pre-reform distributions of their overall loan portfolios, so that the comparison is performed between banks with different exposures to the change in collateral constraint, but otherwise similar loan portfolios. The policy has economy-wide real effects that are economically significant: Relaxing collateral constraints by σ results in an increase of 0.3σ in investment and productivity, and of 0.26σ in dividends.

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 in Vector Auto Regression analyses of macroeconomic aggregates, and validated by the cross-sectional response of banks lending decisions to monetary policy shocks.[1]. However, quantifying the actual magnitude of the bank lending channel has proved 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 full chain of events going from a monetary shock to end effects on economic activity via an expansion in bank lending, which is necessary to gauge the quantitative role of the bank lending channel of monetary policy.

This is the purpose of this paper. 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. With years spent at the zero lower bound, changing collateral requirements has become a permanent fixture of central banks' arsenal everywhere.[3] Monetary policy is conducted by exchanging central bank reserves against a chosen range of eligible private paper, e.g., loans to firms with sufficiently high credit ratings.

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. With data on the universe of loans matched with information on the borrowing firms' characteristics, we document the response of bank credit to the shock and the real consequences on firm level activity. Our main contribution is to implement 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 additional results: First, the expansion in credit happens on the supply side since it is not driven by the firms whose loans become eligible. Second, the estimated real effects are economy-wide, not focused on the subset of treated firms, and measure therefore the aggregate real effects of the bank lending channel.

[1]See among many others Kashyap and Stein (1995), Kashyap et al. (1993), 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 weights of newly eligible loans in the interim period between the ECB's announcement and the Banque de France's implementation.

Like Mésonnier et al. (2022) and Van Bekkum et al. (2018), we explore whether banks with a larger share of newly eligible loans react differently than others, a conventional difference-in-differences approach. The usual quasi-natural experiment does however present 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 potentially dis-similar in fundamental ways.

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 lending

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

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 credit demand on the part of these firms that benefit from the policy change.

Our second contribution is a consequence of the first one. Our estimation is performed on the universe of banks so we can identify the response of credit at the bank level. We then construct artificial firms, whose characteristics (investment, employment, etc) are given by a loan-weighted average of 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. The estimation is performed within homogeneous categories of banks, which ensures identification of the treatment effect. Here, too, it is possible to eliminate treated firms 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.

We document large and significant real effects: Treated firms have higher productivity, they increase investment as well as 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 treated firms (i.e., those with the credit ratings that became eligible): They are statistically indistinguishable from the significant changes we identify among untreated firms. This documents the powerful aggregate effects of collateral policies via an economy-wide expansion of credit.

Related literature

Several papers have exploited the quasi-randomness of the 2011/2012 change in collateral eligibility instigated by the ECB. In the Dutch context, Van Bekkum et al. (2018) find that the change in collateral requirement affected bank lending positively in the specific segment that became eligible (RMBS). Garcia-Posada and Marchetti (2016) find the policy change in Spain had heterogeneous effects on credit across banks. In

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

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. (2017) discuss the heterogeneous effect on credit for single-bank vs. multiple-banks firms. 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 (and within selected categories of banks). This enables us to document the response of credit supply in the economy at large. Second, because of our identification approach we are able to identify precisely the real effects that the monetary shock - and the afferent expansion in credit - had on all firms, not only treated ones (i.e., not only firms whose loans became eligible for collateral).

There is of course an extensive literature interested in the real consequences of credit expansions, though not necessarily triggered by monetary shocks. For example, Jiménez et al. (2020a) exploit Spanish data to show credit supply increases with improved access to wholesale financing via securitization. But the expansion has little real effects because firms tend 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 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 an economy-wide expansion in credit supply, and establish economy-wide real effects in firm-level investment, productivity, and dividend distribution.

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 "buckets" that contain banks with some degree of homogeneity in their overall portfolio composition, while preserving dispersion in the exposure to the treatment, i.e., to 4-rated loans. We perform this discretization over a range of loan ratings that surrounds the threshold eligibility. 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 banks 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+, 3), the other dimension categorizes banks according to the percentage of loans below (and including) the 5 rating. Crossing these two criteria gives rise to 6×6 "buckets" corresponding to different percentage ranges for the holdings of loans between 4+ and 5 ratings. Figure 1 illustrates the discretization when there are six categories along each dimension. Each cell in the figure contains the percentage ranges of holdings of loans rated in the ratings range $[4+, 5]$. For example the upper left bucket in Figure 1 is populated by banks whose portfolios contain between 80 and 100 percent of loans with ratings in $[4+, 5]$. Banks that lend to risky firms 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 conservative portfolios will be located in the upper right area of the figure.

[5]Credit ratings are administrated 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.

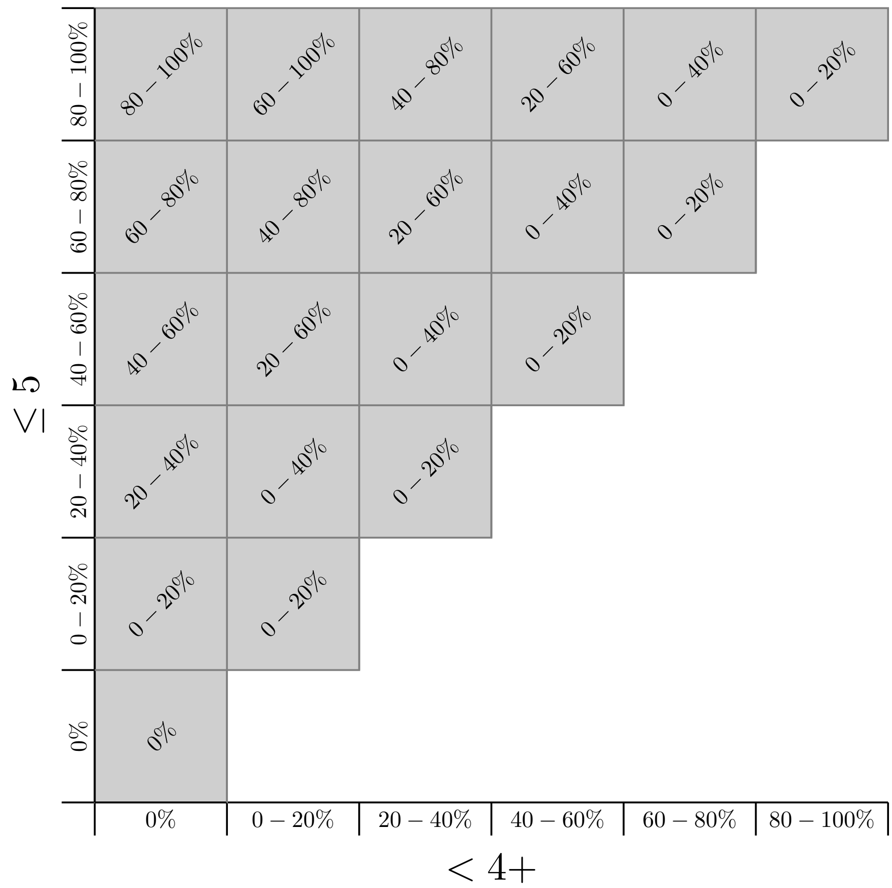


Figure 1: Loan shares in the bucket method

The two-dimensional discretization of banks sharpens identification: Our estimations are all run within bucket, i.e., within a group of banks whose loan portfolios are relatively homogeneous. This ensures the treatment effect is identified *ceteris paribus*, in comparison to banks with similar lending strategies. The resulting variation 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 size, measured by the size of the balance sheet or by deposits. That can be addressed with further controls that effectively split a bucket further into "cells". In what follows we consider banks assets as a criterion to further split buckets into cells. Within-cell identification is meant to capture heterogeneity across banks' portfolio distributions *and* across bank size. 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. We find that our within-cell specification results are robust to the additional inclusion of bank-specific fixed effects. In contrast, relying on only bank fixed effects and omitting cell-specific fixed effects fails to satisfy parallel trends assumption.

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. This facilitates the identification of a credit supply shock and of its effects on the real economy.

2.1 Data

We merge the French national credit register (CCR), the credit rating database and the FIBEN financial statement database, all from the Banque de France (BdF).

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 granted (whether drawn or undrawn) for each month from January 2011 till December 2014. The register reports a monthly average of 2.5 million bank-firm observations. The credit database provides information on all existing lines of credit of any type. The database also contains information on the geographical location of borrowers, the type of sector they belong to and the nature of ownership (private or public entities). Each bank and firm in the data is uniquely identified throughout the data based on anonymous identifiers (CIB for banks and SIREN for firms). These identifiers allow us to match firms in the credit registry one-to-one to firm balance sheet data reported in the FIBEN individual company database.

Credit rating information comes from the FIBEN internal credit rating database at Banque de France. The national central bank 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 pre-reform loan 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 firms whose financial information has not been updated by the Banque de France over the past 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 further 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 includes all firms 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 smallest firms.[9] We drop firms with zero total assets. All firm characteristics are winsorized at the 0.5 and 99.5 percentile.

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 groups 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.

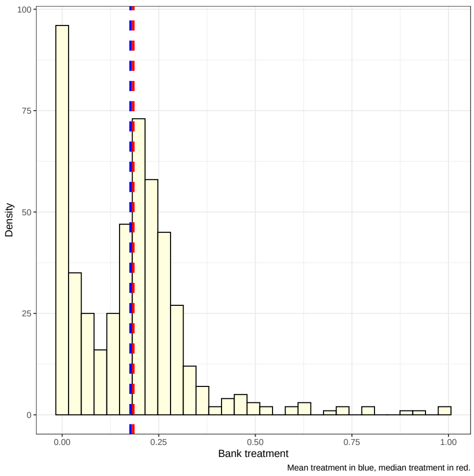
[6]Ratings are also updated throughout the year should relevant information be revealed.

[7]Around 70% of all observations are "X0" rated, and comprise around 60% of overall credit.

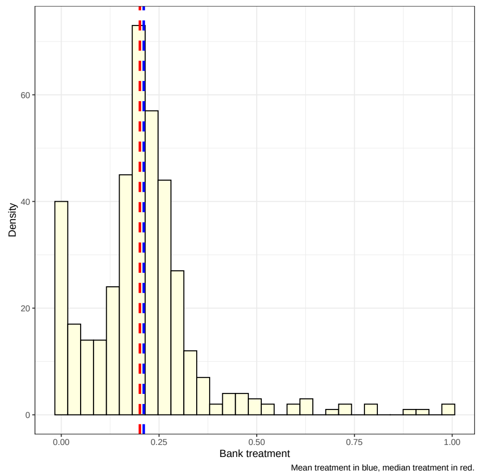
[8]Inter-bank lending refers to lending to other financial or insurance companies, especially between banks from the same banking group. These comprise a large share of credit volumes (about a third of short-term credit).

[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.

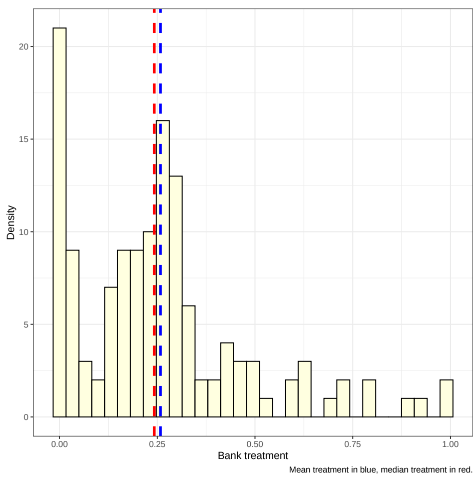
Figure 2 reports the distributions the 12-month average of the share of 4-rated loans in bank portfolios. The average is computed prior to the policy change, between January 2011 and January 2012. The three panels correspond to the three samples. The median and average holdings of 4-rated loans are similar 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 overall hold more 4-rated loans. 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.



(a) Full sample



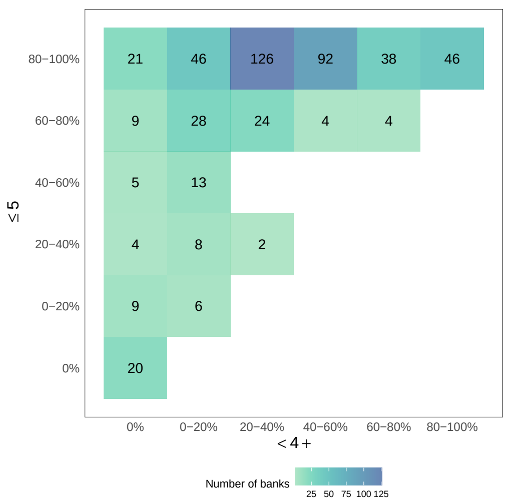
(b) 20% sample



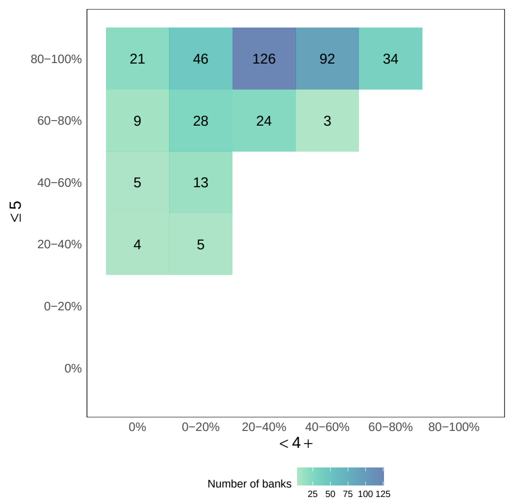
(c) 60% sample

Figure 2: Distribution of treatment intensity size in 2011

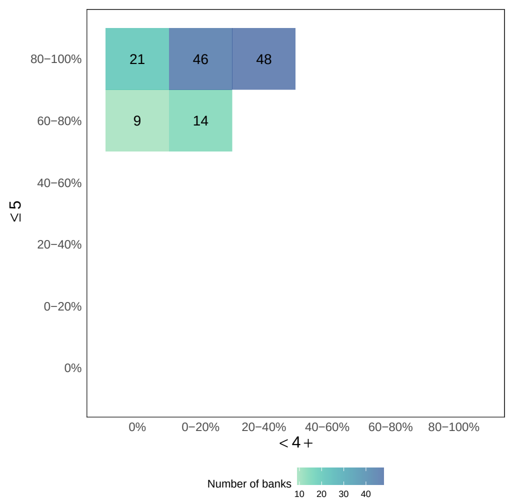
We perform a discretization of banks' portfolios according to the six categories in Figure 1. The categories are chosen to minimize bank heterogeneity within bucket while preserving enough observations for identification. The three panels in Figure 3 report the number of banks per bucket in the three 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

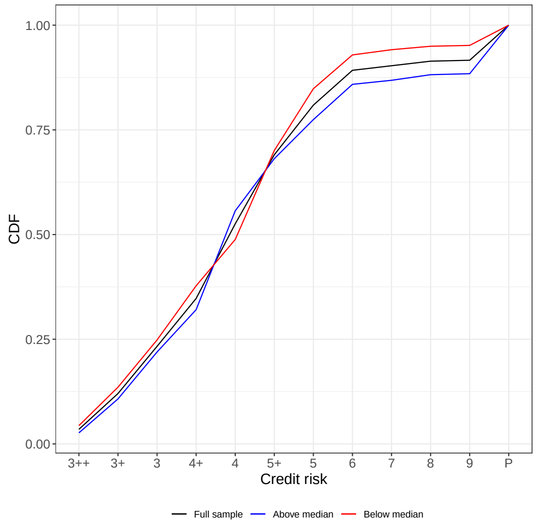
The second sub-sample is focused on banks that hold a minimum of 60 percent of their portfolio in loans rated between 4+ and 5, which means most of the buckets in the full sample have to be purged from banks with not enough such loans.

2.2 Identification

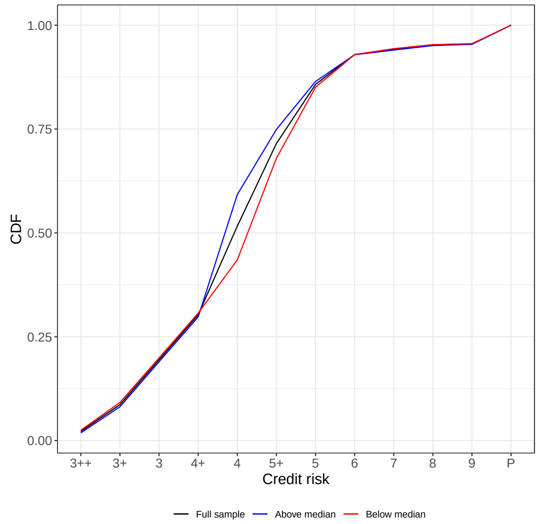
We document the average cumulative distribution (CDF) of loan shares within bucket. In each bucket we compute the cumulative distribution of portfolio shares for all banks to identify the bank with median holding of 4-rated loans. We then compute the cumulative distributions for the sub-samples constituted by banks whose holdings of 4-rated loans are above and below that median, still within bucket. Figure 4 plots the average of these three cumulative distribution functions across all buckets. Several facts stand out.

Firstly, the discretization is performed so that the portfolio shares of loans weakly below 4+ and above 5 are very 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 by definition 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.

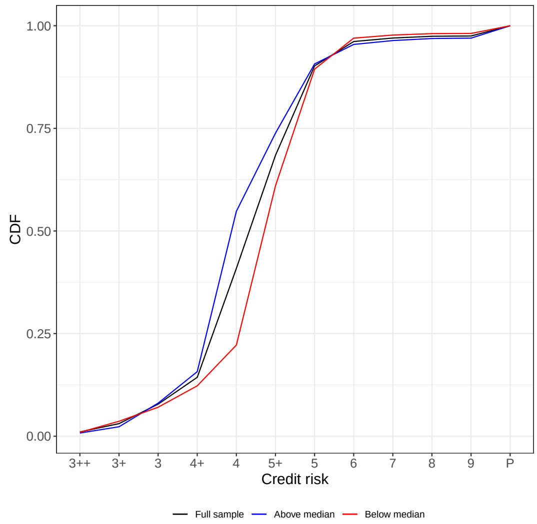
Figure 4 illustrates how the discretization of banks sharpens the treatment effect estimation. The assignment of banks into buckets creates sub-sets of banks in which by construction loan portfolios are very similar *but for* the share of the loans rated 4. Since the estimation is performed within bucket, 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



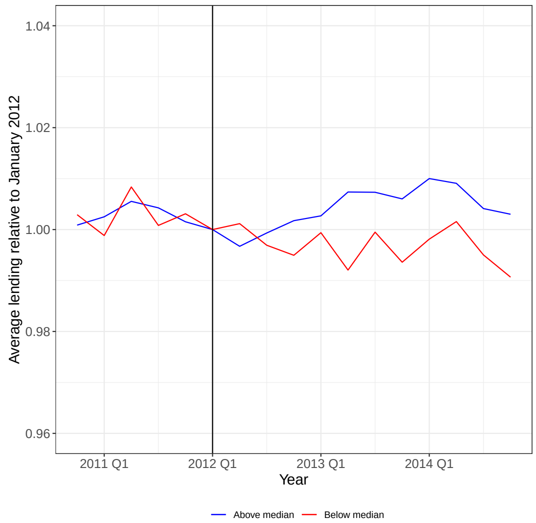
(b) 20% sample



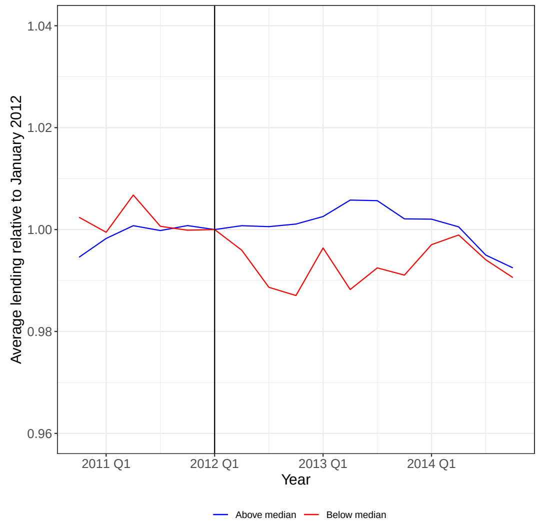
(c) 60% sample

Figure 4: Average dispersion within bucket
This figure shows the average cumulative distribution of loans across all buckets. The black line represents the median cumulative distribution of loans, while the blue and red lines represent the cumulative distribution of banks whose holdings of 4-rated loans are above and below median respectively

The categorization of banks ensures some degree of homogeneity across banks' portfolios within each bucket. However, homogeneity serves little purpose for identification if differences in lending policies existed prior to the change in collateral requirement within bucket. Figure 5 documents average lending by banks with holdings of 4-rated loans above and below the median value within bucket. Lending is computed relative to January 2012, just prior to the policy change, when lending by both categories of banks is normalized to one. 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. 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.



(a) Full sample



(b) 20% sample

(c) 60% sample

Figure 5: Pre-existing trends within bucket
This figure plots the average lending by banks with holdings of 4-rated loans above and below the median value within bucket

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

$$L_{b,t} = \alpha_{c,t} + \alpha_b + \sum_{\substack{k=\{-12,\dots,36\} \\ k\neq -1}} \beta_1^k \cdot (T_b \times D_{2012m2+k}) + \beta_2 \cdot T_b + \varepsilon_{b,t}, \tag{1}$$

where $L_{b,t}$ denotes the 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, and $D_{2012m2+k}$ is a binary variably taking value 1 for the month equal to $2012m2 + k$.

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 $\times$ time fixed effects implies that we are comparing ex-ante similar banks in terms of their overall loan portfolios outside of the $[4+, 5]$ range, and in terms of their size. The common trend assumption needs only hold within-cell.

Before reporting the estimation of equation 1 for the three sub-samples, we test whether the exclusion of the term $\alpha_{c,t}$ invalidates parallel trends à la Autor (2003).

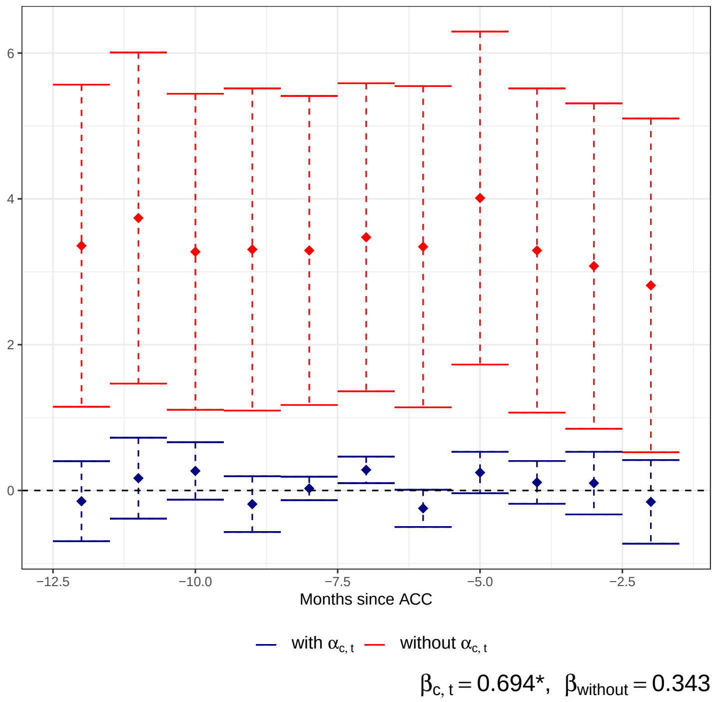
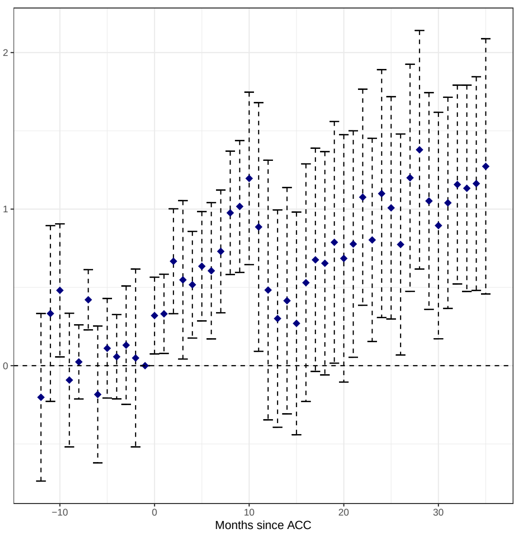


Figure 6: With and without cell FE

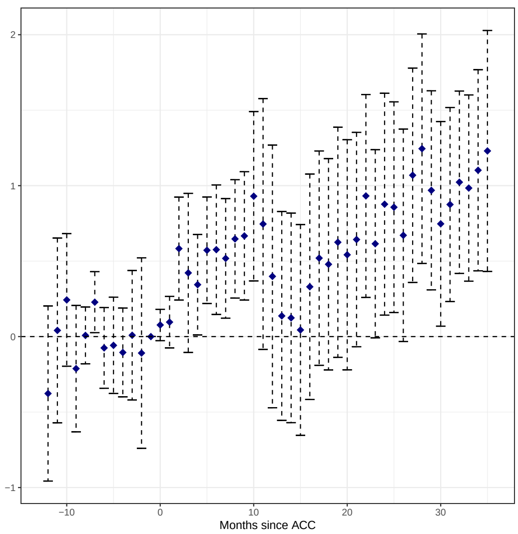
[10]We replicate the analysis with deciles and find our results are robust. We retain septiles throughout the paper as these imply more populated cells.

Figure 6 reports the pre-period coefficients of interest with and without $\alpha_{c,t}$. We immediately see why it is important to control for time-varying trends across banks' portfolios.

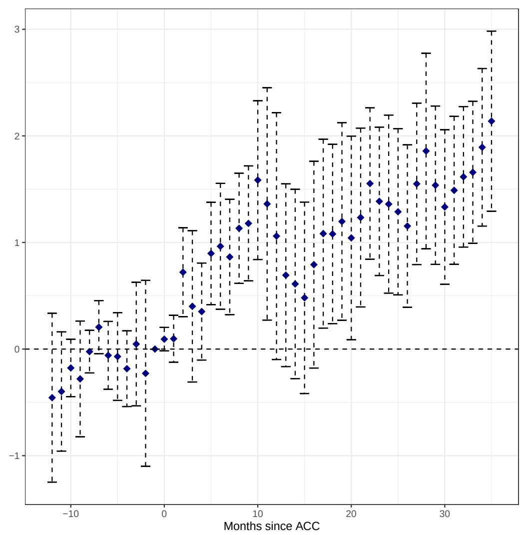
Figure 7 plots β_1^k for $k = (-12, ..., 36)$. The Figure confirms the lack of pre-existing trends with values of β_1^k not significantly different from zero for $k < 0$.



(a) Full sample



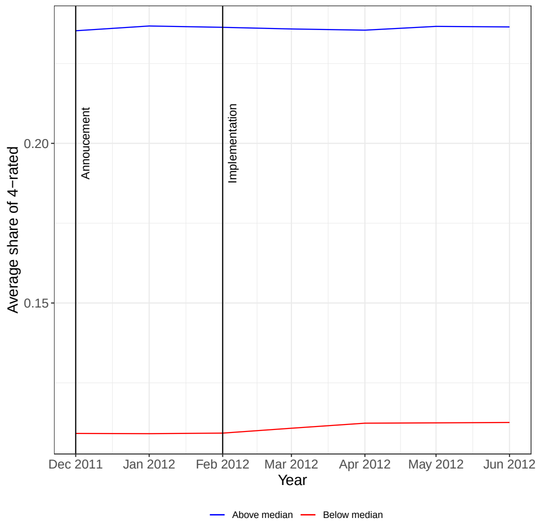
(b) 20% sample



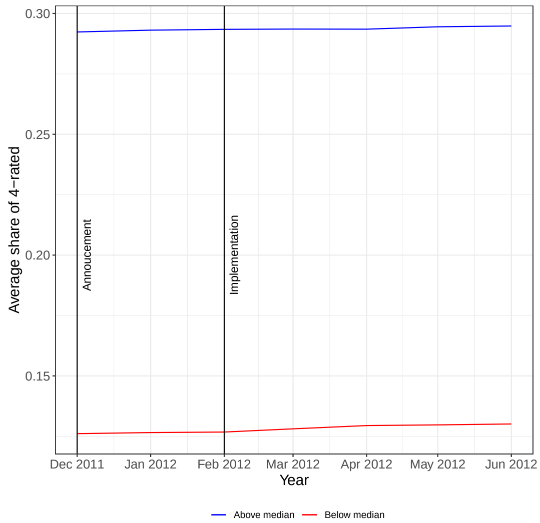
(c) 60% sample

Figure 7: Pre-existing trends à la Autor (2003)
This figure plots the value of β_1 in equation 1 to check whether treated and non treated banks are similar before the policy is implemented.

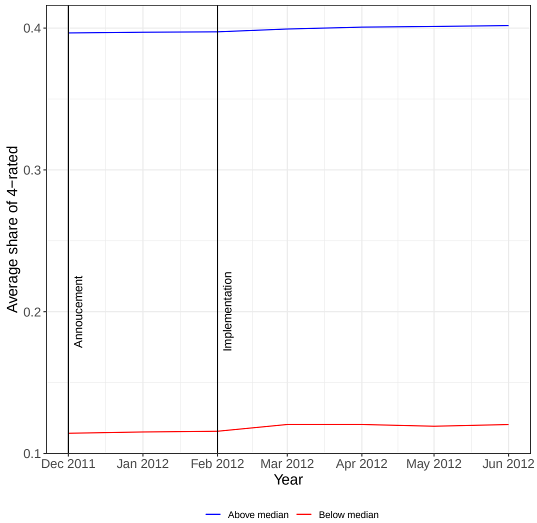
A few months elapsed between the ECB announcement of a change in the collateral requirements and its actual implementation by the Banque de France. The exogeneity of the shock would become questionable if French banks actually took advantage of this interim period to alter their loan portfolios in preparation of the actual change. Figure 8 displays the averages of 4-rated loans holdings for above and below median within bucket over the full time period. Prior to the implementation there is no observable average 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: The evolution of loan holdings between announcement and implementation

Table 1 presents a variance decomposition of the holdings of 4-rated loans before implementation in the three considered samples. The table shows that in the full sample 57.4% of the variation in the policy exposure is between banks that belong to the same cell. Logically the more concentrated the samples, the larger the within-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, the fraction of 4-rated loans in bank b's portfolio, and three different bank size characteristics. The unconditional correlations are often large, as are the correlations within bucket. Both facts suggest portfolio allocation is not random, which is not a very surprising conclusion. However, the correlation within cell is considerably smaller in all cases, which indicates that identification within cell is indeed *ceteris paribus*.

Table 2: Correlations between treatment intensity 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

Note: *p<0.1; **p<0.05; ***p<0.01

3 Estimations and Results

3.1 Credit effects

We first estimate the consequences of the relaxation in collateral constraints on bank's credit supply. Identification is achieved within-cell, although we report some results without cell effects for comparison purposes. 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 $L_{b,t}$ 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, and D_{2012m2} is a binary variable taking value 1 after the relaxation in collateral requirements. Identification is performed within-cell with the intercept $\alpha_{c,t}$, which also allows for time-varying cell-specific developments. We estimate equation (2) with and without a bank-specific intercept α_b to gauge whether any time-invariant unobserved heterogeneity survives between banks within a cell, i.e., whether the estimates for β_1 (and β_2) are affected by the inclusion of α_b.[11]

Table 3 reports the estimates of β_1 and β_2 in equation (2), without and with 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.[12] 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. For example one in every five banks holds less than 20% 4-rated loans in their portfolio in the full sample.

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

[11] 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, which would have a different interpretation. The irrelevance of bank fixed effects tells us that the credit effect occurs within bank network, since α_b subsumes bank networks. We check for the possibility in Appendix Table A2 and estimate our baseline estimation without bank-specific intercepts, but allowing for bank holding group fixed effects. We find no significant change in our baseline credit effect.

[12] β_2 is subsumed in the bank fixed effect when it is included.

Table 3: The credit effects of collateral constraints

	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)	(0.000)	(0.568)	(0.000)	(0.634)	(0.000)
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
Note:				*p<0.1; **p<0.05; ***p<0.01		

full sample estimates. Finally columns (5) and (6) consider the narrowest sample where $T_b > 0.6$: The estimates of β_1 are still positive and significant, point estimates are now 50 percent larger, and still not different from each other.

The fact that bank fixed effects make no significant difference suggests the treated and control groups within cell are similar except for their holding of 4-rated loans, as they should. We conclude that the relaxation of collateral constraints had significant consequences on the supply of credit, especially by banks that held 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, which corresponds to 0.12σ and 0.19σ increase in new loans.

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 within specific categories of banks, it is entirely possible to verify whether credit to newly eligible firms (rated 4) is actually driving the results. Table 4 presents the results of estimating equation (2) omitting all firms whose loans are rated 4. A quick comparison with Table 3 shows that the coefficient estimates are not significantly different. We conclude that the relaxation of collateral constraints does have aggregate consequences on credit.

Table 4: The credit effect of collateral constraints – omitting 4-rated firms

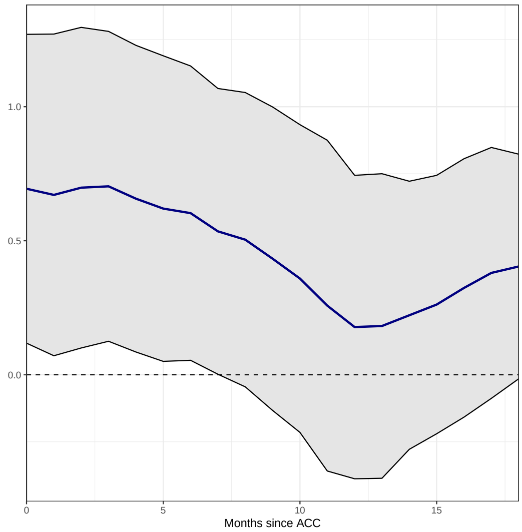
	Dependent variable:					
	Log(Loans)					
	(1)	(2)	(3)	(4)	(5)	(6)
$T_b \times D_{2012m2}$	0.747*	0.693*	0.841**	0.674*	1.215**	1.220**
	(0.270)	(0.295)	(0.278)	(0.315)	(0.266)	(0.423)
T_b	−1.565**		−1.932**		−2.296***	
	(0.517)	(0.000)	(0.512)	(0.000)	(0.484)	(0.000)
Bank FE	N	Y	N	Y	N	Y
Cell x Time 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
Note:				*p<0.1; **p<0.05; ***p<0.01		

Tables 3 and 4 focus on impact effects. We assess the evolution of these effects over time with a local projection estimation following Jorda, 2005. The specification becomes:

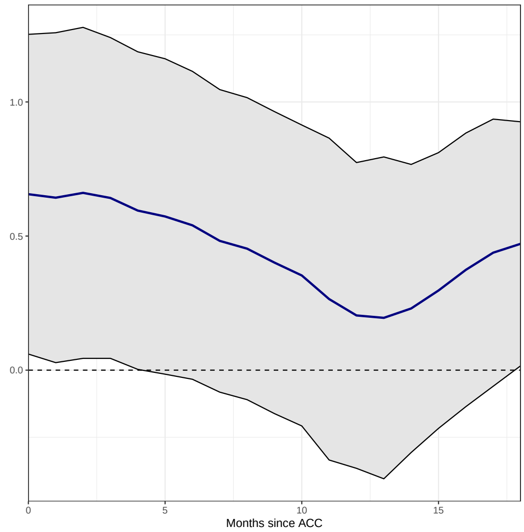
$$L_{b,t+h} = \alpha_{c,t} + \alpha_b + \beta_h \cdot (T_b \times_{2012m2}) + \beta_2 \cdot T_b + \varepsilon_{b,t+h}, \tag{3}$$

for all h in $\{0, 1, ..., 36\}$. The estimates of β_h are presented in Figure 9, in three panels corresponding to the three samples. The effect of the policy change on credit last 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. As a result, the level of credit supply increases permanently to reach a higher level after the policy change.[13]

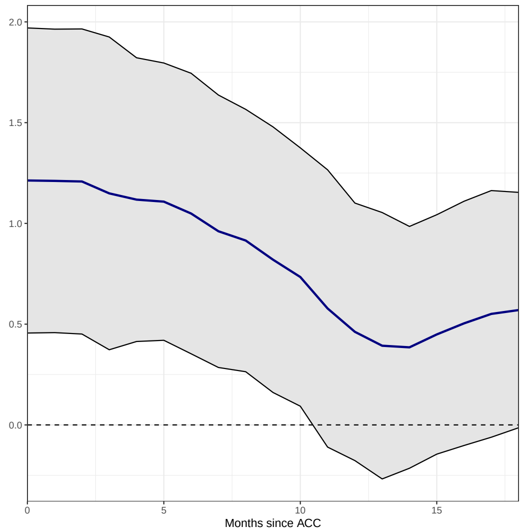
[13]In results available upon request we also show that interest rates fall with the shock, as banks pass the lower financing cost on to their borrowers.



(a) Full sample



(b) 20% sample



(c) 60% sample

Figure 9: Local projections for credit effects

3.2 Effect on loan portfolio riskiness

Table 5 presents the results. We report no significant effect of treatment on the riskiness of loan portfolios at banks upon relaxing collateral constraints.

Table 5: The effect of collateral constraints on portfolio riskiness

	Dependent variable:					
	Portfolio riskiness					
	(1)	(2)	(3)	(4)	(5)	(6)
$T_b \times D_{2012m2}$	2.292	2.935	0.619	0.055	-1.245	-1.373
	(1.629)	(2.780)	(1.608)	(2.022)	(1.445)	(2.784)
T_b	7.789**		8.607**		7.274**	
	(2.636)	(0.000)	(2.785)	(0.000)	(2.035)	(0.000)
Bank FE	N	Y	N	Y	N	Y
Cell *imes* Time FE	Y	Y	Y	Y	Y	Y
Observations	18,879	18,879	15,751	15,751	4,836	4,836
Adjusted R^2	0.581	0.925	0.336	0.895	0.185	0.852
Note:				*p<0.1; **p<0.05; ***p<0.01		

3.3 Real effects

We now turn to our analysis of the real effects engendered by the expansion of credit just documented. Thanks to our approach, we are able to examine whether real effects prevail across *all* firms, not only those that have the 4-rating affected by the policy. In particular, we estimate

$$Y_{b,t} = \alpha_{c,t} + \alpha_b + \beta_1 \cdot (T_b \times D_{2012m2}) + \beta_2 \cdot T_b + \varepsilon_{b,t}, \tag{4}$$

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

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

where $L_{b,f}^{2011}$ denotes the value of loans borrowed from bank b by firm f in each month of 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 bank heterogeneity within cell.

An alternative approach to equation 4 is to perform the estimation at firm-level. For instance both Cingano, Manaresi and Sette (2016) and Jimenez, Ongena, Peydro, and Saurina (2012) identify at firm-level in matched bank-firm datasets akin to ours, in Italy and Spain, respectively. Firm-level identification is not natural in our context, since we must identify within cell. One intuitive option 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 4 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 the differences between banks: 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 (4) comes from the fact that firms do not typically conduct business with a single bank. In fact, Andrade et al. (2019) exploit precisely the existence of multi-bank firms in their identification. 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 for our analysis. 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. Here 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. To get a sense of the importance of these omissions we also present the estimation results for equation (4) on the full sample of firms.

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 (4) 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 predate the policy change, from January to December 2011. 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 now 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

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

where $\text{NFirms}_{b,t}$ denotes the number of new firms borrowing from bank b. The rest of the specification is identical to equation (2) in the previous section.[14]

Table 6 presents the estimates of β_1 when the dependent variable is the number of new bank-firm relationships (to capture the extensive margin of lending) in response to the policy change. β_1 is insignificant across the three samples and whether bank fixed effects are included or not. We conclude that the credit expansion documented in the previous

[14] We ran logit specification with fixed effects and results were similar.

section happened at the intensive margin, as banks chose to lend more to their existing customers.

Table 6: The extensive margin effect

	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)	(0.000)	(0.595)	(0.000)	(0.623)	(0.000)
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
Note:				*p<0.1; **p<0.05; ***p<0.01		

In fact, this is to be expected given the well-known persistence in bank-firm relationships, which we expect to hold in our data as well.[15] We perform a simple auto-regressive specification on a variable capturing the existence of a bank-firm relationship at time t, estimating

$$\text{Active}_{b,f,t} = \alpha_{b,f} + \alpha_t + \rho \cdot \text{Active}_{b,f,t-1} + \varepsilon_{b,f,t}, \tag{6}$$

where $\text{Active}_{b,f,t}$ takes value 1 if a relationship exists between bank b and firm f at time t, and zero otherwise. Table 7 reports estimates of ρ in the three samples, with or without time and bank-firm specific intercepts. All estimates are large, close to 0.9 without fixed effects, down to about 0.75 if $\alpha_{b,f}$ and α_t are included. Unsurprisingly, bank-firm relationships are highly persistent in French data as well.

We compute 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 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 EBE_t is measured by

[15] See for instance Petersen and Rajan (2002), Chodorow-Reich (2014)

Table 7: The persistence of bank-firm relationships

	Dependent variable:					
	Active					
	Full sample		20% sample		60% sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Active}_{b,f,t-1}$	0.894***	0.751***	0.894***	0.752***	0.862***	0.732***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0005)
Bank x Firm FE	N	Y	N	Y	N	Y
Time FE	Y	Y	Y	Y	Y	Y
Observations	35,700,760	35,700,760	35,353,167	35,353,167	2,086,020	2,086,020
Adjusted R^2	0.800	0.811	0.801	0.812	0.746	0.759

Note: *p<0.1; **p<0.05; ***p<0.01

gross operating surplus to total (lagged) sales. The normalizations are introduced to bypass issues of non-stationarity.

The estimation of equation 4 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, since these are synthetic firms computed as weighted averages of many potentially very different firms. We need to know whether, before ACC, above and below median banks within a cell lend to firms with equal characteristics on average, since this is crucial to the identification of real effects. To establish this, we perform Welch two-sample t-tests of the null hypothesis that investment, employment, dividends, and productivity have equal means when comparing above versus below median banks within cell before February 2012. Columns (1)-(3) of table 8 presents the results for the full sample, 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. We conclude that the within cell ex-ante differences between the firms borrowing from treated vs. untreated banks lend are insignificant.

Table 8: Welch two-sample t-test of equal means for treated and control

	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									
$\frac{I(T)_t}{A_{t-1}}$	1.232	0.218	1,180	0.065	0.949	968	−0.401	0.688	210
$Gr(Emp)_t$	0.222	0.824	1,141	0.344	0.731	901	0.138	0.890	204
$\frac{Div_t}{L_{t-1}}$	−1.387	0.166	1,197	−1.404	0.161	998	−1.613	0.108	276
$\frac{EBE_t}{Sales_{t-1}}$	−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									
$\frac{I(T)_t}{A_{t-1}}$	1.522	0.128	1,165	1.101	0.271	982	−0.403	0.688	203
$Gr(Emp)_t$	1.178	0.239	1,109	0.679	0.498	934	−0.021	0.983	183
$\frac{Div_t}{L_{t-1}}$	−1.905	0.057	1,149	−1.744	0.081	973	−1.600	0.111	266
$\frac{EBE_t}{Sales_{t-1}}$	−0.658	0.511	1,050	−0.721	0.471	893	−1.530	0.128	182
Panel C: Firms with a prime lender (75%)									
$\frac{I(T)_t}{A_{t-1}}$	0.764	0.445	976	0.307	0.759	824	−0.294	0.769	206
$Gr(Emp)_t$	1.395	0.163	908	1.344	0.179	802	−0.028	0.977	187
$\frac{Div_t}{L_{t-1}}$	−1.666	0.096	892	−0.979	0.328	797	−1.566	0.119	274
$\frac{EBE_t}{Sales_{t-1}}$	−0.483	0.630	979	−1.011	0.312	850	−1.898	0.059	189

Table 9 presents the result of estimating equation 4 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 lender). Bank fixed effects are systematically included, as are cell-time fixed effects. The results are unambiguous: In all but one specifications investment, dividends, and productivity increase significantly at conventional confidence levels.[16]

A key point of our study is to demonstrate that the real effects we identify are not confined to the firms that are treated. This is done in Table 10, which reproduces Table 9 omitting altogether firms whose loans are rated 4. The results are not significantly different from the full sample, and all but two coefficients in Table 10 are positive and significant. We conclude that firms with a rating of 4 are not the ones driving the response of investment, dividends, and productivity: The expansion of credit caused by a modification of collateral constraints benefits *all* firms.

How economically relevant are these responses? Tables 9 and 10 report the standard deviations of the regressors across all specifications, which helps quantifying the economic significance of the effects in both tables. Applying a standard deviation of the treatment T_b equal to its average across samples (0.22), the coefficient estimates reported in Table 10 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).

Table 9 reports impact effects as predicted by equation 4. Figure 10 plots the corresponding linear projection estimates, up to 36 months after the policy change. All significant responses are relatively short-lived and stop being significant about one year from the shock: 15 months for tangible investment, 6 months for dividends, and 12 months for productivity. Interestingly, there is a lagged and temporary negative response of employment growth. The implication is the credit expansion increased productivity and dividends via higher capital per worker, as the stock of capital increased while employment contracted.

Figure 11 plots the evolution of coefficients to validate the lack of pre-existing trends.

[16]The one exception concerns dividends in the 20 percent sample when firms are constrained to borrow from either treated or untreated banks.

Table 9: Real effects of relaxing collateral requirements

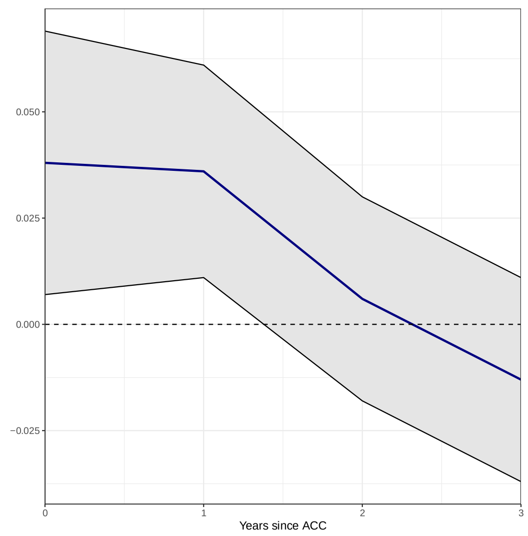
	Dependent variable:											
	Full sample				20% sample				60% sample			
	$\frac{I(T)_t}{A_{t-1}}$	$Gr(Emp)_t$	$\frac{Div_t}{L_{t-1}}$	$\frac{EBE_t}{Sales_{t-1}}$	$\frac{I(T)_t}{A_{t-1}}$	$Gr(Emp)_t$	$\frac{Div_t}{L_{t-1}}$	$\frac{EBE_t}{Sales_{t-1}}$	$\frac{I(T)_t}{A_{t-1}}$	$Gr(Emp)_t$	$\frac{Div_t}{L_{t-1}}$	$\frac{EBE_t}{Sales_{t-1}}$
	(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
σ(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
σ(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
σ(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

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

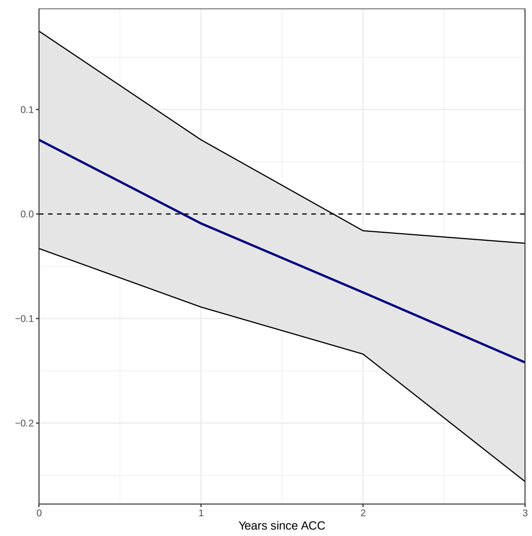
Table 10: Real effects of relaxing collateral requirements excluding 4-rated firms

	Dependent variable:											
	Full sample				20% sample				60% sample			
	$\frac{I(T)_t}{A_{t-1}}$	$Gr(Emp)_t$	$\frac{Div_t}{L_{t-1}}$	$\frac{EBE_t}{Sales_{t-1}}$	$\frac{I(T)_t}{A_{t-1}}$	$Gr(Emp)_t$	$\frac{Div_t}{L_{t-1}}$	$\frac{EBE_t}{Sales_{t-1}}$	$\frac{I(T)_t}{A_{t-1}}$	$Gr(Emp)_t$	$\frac{Div_t}{L_{t-1}}$	$\frac{EBE_t}{Sales_{t-1}}$
	(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
$\sigma(Y)$	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
$\sigma(Y)$	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
$\sigma(Y)$	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

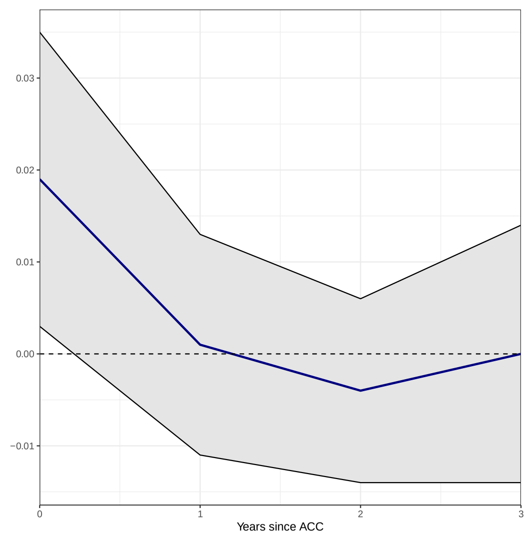
Note: *p<0.1; **p<0.05; ***p<0.01



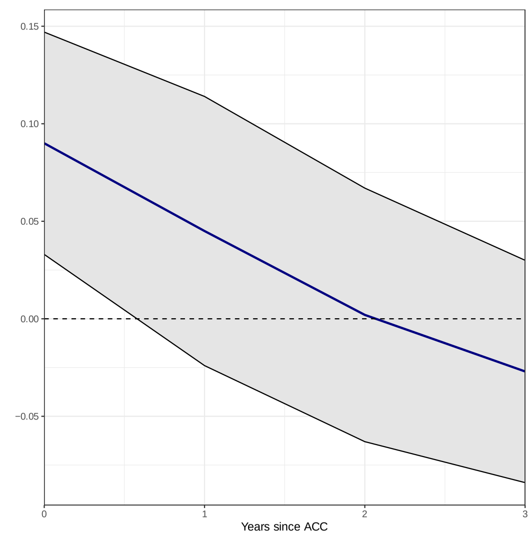
(a) Tangible investment to assets



(b) Growth in employment

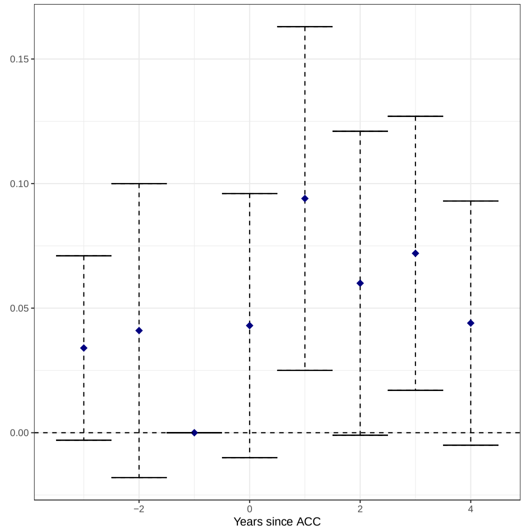


(c) Dividends to liabilities

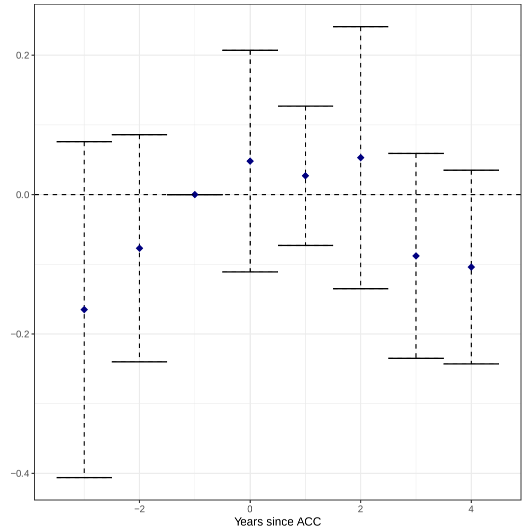


(d) EBE to sales

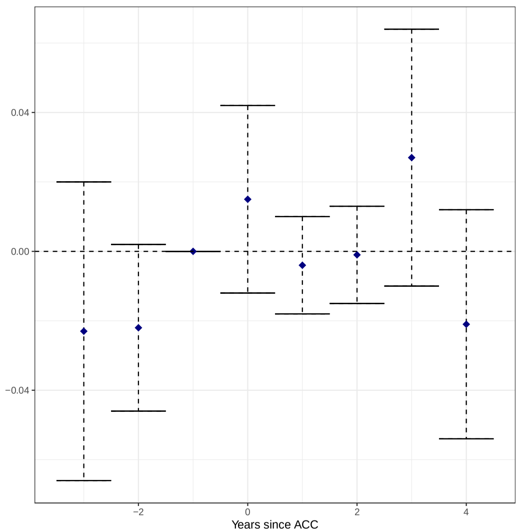
Figure 10: Local projections for real effects: Full sample of firms with fixed effects



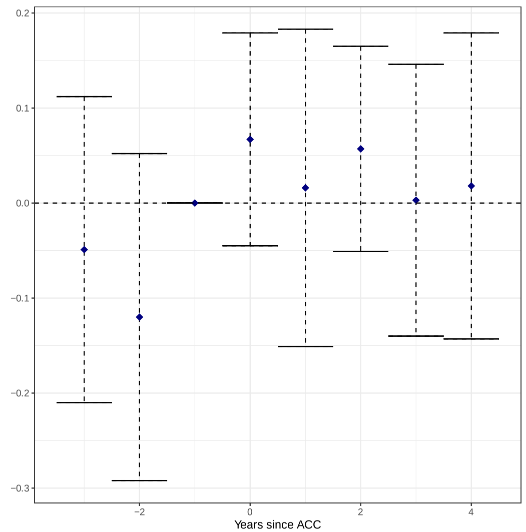
(a) Tangible investment to assets



(b) Growth in employment



(c) Dividends to liabilities



(d) EBE to sales

Figure 11: Pre-existing trends à la Autor (2003)

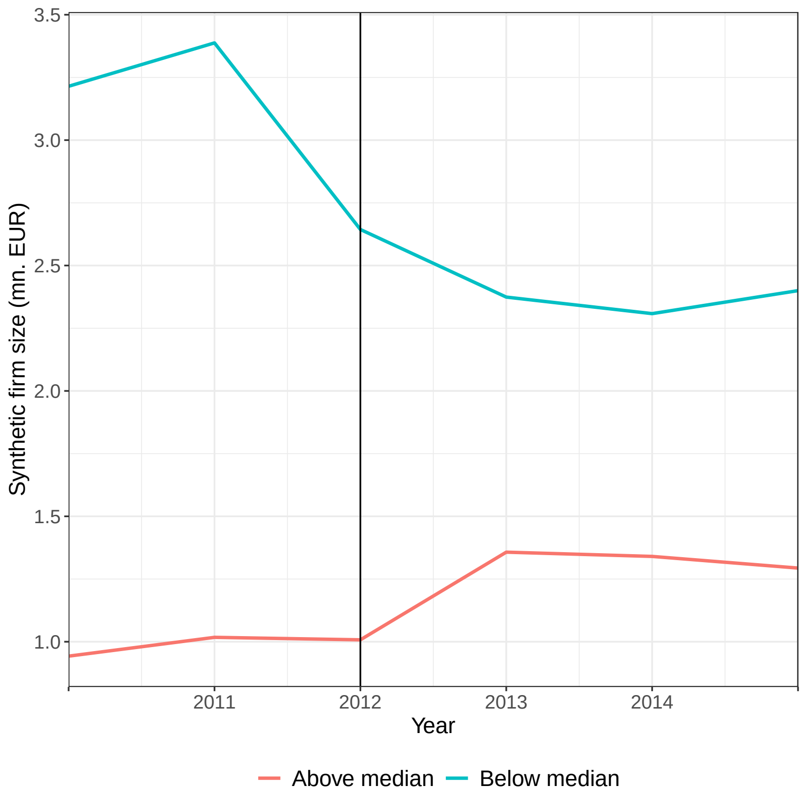


Figure 12: Size of synthetic firms within bucket

4 Conclusion

We document large and economy-wide consequences of an unconventional expansionary shock to monetary policy, in the form of an unexpected relaxation of collateral eligibility instigated by the ECB in 2011. In the French economy, the shock resulted in an economy-wide expansion of credit with positive and large consequences on all firms' productivity, investment, and dividend distributions. These findings suggest that the bank lending channel is a powerful tool of monetary policy when the credit expansion is caused by changes in collateral requirements. An interesting question, which we leave for further research, is whether the magnitude of this effect depends on the macroeconomic context, and in particular the proximity to the zero lower bound.

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Appendix

A1 Summary Statistics

Table A1: Summary statistics

	Full sample					20% sample					60% sample				
	Mean	Median	SD	Min	Max	Mean	Median	SD	Min	Max	Mean	Median	SD	Min	Max
Panel A: Bank variables															
Loans	552,308	97,712	1,113,680	1	9,458,132	580,440	158,999	1,063,691	1	7,392,862	180,488	33,014	363,863	20	2,187,102
Assets	2,264,614	317,089	7,371,713	2,578	90,129,858	2,225,988	364,145	7,057,518	2,780	84,211,026	1,126,094	158,666	6,341,112	2,598	81,407,631
Deposits	658,934	70,260	1,782,099	1.25	21,255,854	698,604	83,010	1,731,231	5.49	20,709,769	132,563	42,243	283,462	1.25	2,774,883
Capital	176,434	25,713	491,827	6.25	8,328,004	175,953	30,521	455,097	7.28	7,058,608	99,673	14,213	614,499	6.25	8,328,004
T_b	0.18	0.18	0.16	0.00	1.00	0.21	0.20	0.15	0.00	1.00	0.26	0.24	0.22	0.00	1.00
# of bins (average # banks per bin)			19 (65)					13 (73)					5 (37)		
Panel B: Synthetic firm variables															
Tangible investment to assets	0.004	0.001	0.020	-0.080	0.220	0.004	0.001	0.019	-0.080	0.220	0.001	-0.001	0.027	-0.080	0.220
Growth in employment	0.002	0.003	0.066	-0.590	0.580	0.003	0.003	0.063	-0.590	0.580	-0.002	-0.001	0.076	-0.580	0.580
Dividends to liabilities	0.001	0.000	0.012	-0.140	0.140	0.001	0.000	0.011	-0.140	0.130	0.000	0.000	0.014	-0.140	0.100
EBE to sales	0.010	0.010	0.046	-0.220	0.320	0.010	0.010	0.041	-0.220	0.320	0.010	0.003	0.056	-0.220	0.320

Level variables are reported in a scale of 1000s.

A2 Controlling for Bank Holding Companies

Table A2

	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)	(0.000)	(0.520)	(0.000)	(0.720)	(0.000)
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

Note: *p<0.1; **p<0.05; ***p<0.01