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# The real effects of bank capital requirements


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**Abstract:** We measure the impact of bank capital requirements on corporate borrowing and investment using loanE level data. The Basel II regulatory framework makes capital requirements vary across both banks and across firms, which allows us to control for firmE level credit demand shocks and bankE level credit supply shocks. We find that a 1 percentage point increase in capital requirements reduces lending by 10%. Firms can attenuate this reduction by substituting borrowing across banks, but only partially. The resulting reduction in borrowing capacity impacts investment, but not working capital: Fixed assets are reduced by 2.6%, but lending to customers is unaffected.

**Keywords:** Bank capital ratios, Bank regulation, Credit supply

**JEL Classification:** E51, G21, G28

# 1. Introduction

Since the financial crisis, regulators have increased bank capital requirements in order to improve the resilience of banks to adverse shocks. The debate on the real consequences of such actions has been intense. According to the "Modigliani-Miller" (MM hereafter) view capital requirements have little impact on bank lending and economic activity (Admati, DeMarzo, Hellwig and Pfleiderer, 2010). Capital requirements are key provisions through which bank supervisors ask banks to hold a given share of their assets as equity. In order to maintain lending activity with higher requirements, banks need to issue more equity. The MM view holds that equity issuance costs are modest, so that raising capital requirements has a marginal impact on the cost of funding for banks (Hanson, Kashyap and Stein, 2009). The intuition is that better capitalized banks can issue less risky, and hence cheaper, equity. Thus, according to the MM view, banks can easily recapitalize while maintaining the same portfolio of loans. The alternative non-MM view relies on an equally long tradition in corporate finance, which states that raising equity is expensive because of frictions: asymmetric information on the primary market for shares (Myers and Majluf, 1984), agency costs of bank management (Diamond and Rajan, 2000), etc. When equity is scarce, NPV maximizing banks may have to give up positive NPV projects because they consume too much regulatory capital. According to the non-MM view, an increase in capital requirements has the potential to decrease lending (Kashyap and Stein, 2004) and investment.

In this paper, we estimate the effect of capital requirements on bank lending and corporate outcomes. The literature has historically faced an empirical challenge in identifying these effects since regulatory capital requirements were set at 8% for all banks from the 1990s. To deal with this homogeneity, economists have used regulatory actions where supervisors force banks to change their leverage ratios (Peek and Rosengren, 1995; Aiyar, Calomiris and Wiedalek, 2012) but such evidence may be hard to interpret since targeted banks are not randomly chosen. In this paper, we add to this literature by exploiting the intrinsic heterogeneity of the Basel II regulatory framework, which has been in place since the mid-2000s, and make capital requirements vary across both banks and firms. We exploit a large loan-level dataset which provides us with information on loan and borrower characteristics and in particular, capital requirements. As is the rule under Basel II, capital requirements differ both across borrowers of the same bank, and across banks within a given firm. This comes from the fact that risk differs across firms and that banks use different internal models to assess it. We use this rich source of variation to identify the impact of capital requirements on lending, and then on firm behavior.

Our research design enables us to control for two major (observed and unobserved) confounding variables. The first confounding variable is risk: riskier firms face higher capital requirements by design and, at the same time, banks may choose to lend them less. We can control for both observable and unobservable risk by using a classical technique in the banking literature, which consists in comparing lending by different banks to the same firm (Kwhaja and Mian, 2008). We can do this because we have loan-level data and the Basel II framework allows different banks to use different models of default risk for the same firm. We can thus control for unobserved firm-level characteristics that change over time. In particular, we can control for unobserved firm-level risk under the assumption that this risk is the same for all loans extended to the same firm.

A second major confounding effect is funding shocks on banks. Since Peek and Rosengren (1997, 2000), the banking literature has largely documented the impact of funding shocks on lending, in a similar way that the corporate finance literature has looked at the impact of funding shocks on financing constraints. This can be a problem for our estimation if the firms borrowing from banks that experience negative shocks are themselves non-random: for instance, if such adversely hit banks also tend to lend to riskier firms. We deal with this problem by comparing different borrowings from the same bank. We can do this because the same bank does not set the same capital requirement for different firms, even if these firms belong to the same observable risk category. Our implicit assumption is that banks respond to funding shocks by scaling their lending proportionally across all firms belonging to the same risk category. Overall, our methodology allows to control for unobservable bank-level funding shocks, an important aspect given that the period under review overlaps the banking crisis.

We proceed in two steps. First, we analyze the effect of capital requirements on lending, and find it to be large. We explore both the *intensive* and the *extensive margin* of lending. At the intensive margin (i.e. conditionally on lending), we find that a one percentage point increase in capital requirement (for example, from 7% to 8%) leads to a one percent reduction in lending. The effect is much larger when we also consider the extensive margin of lending (i.e. the decision to initiate or stop a lending relationship). In this case, we find that a one percentage point increase in capital requirements leads to an 8 percent reduction in lending. This is a potentially large effect, given that, for instance, the transition from Basel II to Basel III is supposed to raise minimum capital requirements from 4.5% to 6% over the course of 6 years.[5] Like in the rest of the banking literature, these estimates are subject to several assumptions. In particular, accounting for the extensive margin of lending requires that we make assumptions about which bank is a potential lender to a given firm. We posit that the set of potential lenders is restricted to banks that have been observed to lend at least once to the firm over the period under review. Another assumption is that, conditional on firm and bank-level shocks, capital requirements are at least partly exogenous for banks.

We then aggregate our findings at firm level. Because we have accounting data, we can investigate the impact of capital requirements on firm-level outcomes such as borrowing, total assets, fixed assets and working capital. We find that a change in the average capital requirement faced by a firm translates into lower investment: in our preferred specification, a one percentage point increase in capital requirements lead to a reduction in fixed assets of 2.5 percent. The overall impact of capital requirements on investment is non negligible, but

[5] See for instance http://www.bis.org/bcbs/basel3/basel3_phase_in_arrangements.pdf.

smaller than the impact on bank lending. The reason for this is that firms modify their balance sheets in response to the reduction in bank lending. They borrow less from high capital requirement (CR thereafter) banks, and more from low CR ones. They also reduce lending to their clients: given the large share of trade receivables on balance sheets, this offsets a large share of the impact of CRs on borrowing. Overall, capital requirements then have a real impact on corporate investment, although this impact is somewhat dampened by firms' decisions. Our firm-level analysis hinges on several assumptions. At the firm level, it is not possible anymore to control for firm-specific shocks as in the loan-level analysis. We take comfort in the fact that loan-level results are not very significantly affected by firm-specific shocks. We can, however, control for funding shocks affecting banks that are potential lenders to a firm. These effects are identified because banks lend to different firms, which allows intra-bank comparisons.

Finally, we address the issue that risk models may be manipulated by banks in order to underestimate risk and therefore minimize capital requirements (Acharya and Steffen, 2014; Behn, Haselman and Vig, 2014; Freixas, Laeven and Peydro, 2015). We provide three distinct robustness checks designed to address this issue in our sample of French banks. First, evidence from the *Asset Quality Review* suggests very small biases in risk assessment by banks in our sample. Second, we do not find any evidence that banks have lower capital requirement for firms to which they were highly exposed pre adoption of Basel II (a relation that would happen if banks were to strategically underestimate capital requirements). Third, we show that our effects are present in for loans whose capital requirements are calculated under the Standard Approach of Basel II, an approach that gives no discretion to banks over risk models.

A recent strand of literature assesses the impact of higher capital requirements on credit distribution using sharp identification strategies. Aiyar, Calomiris and Wiedalek (2012) compare the credit distribution of the resident foreign branches with the credit distribution of the UK-owned banks. These latter are subject to time-varying, bank-specific minimum capital requirements set by the national regulator. Jimenez, Ongena, Peydro and Saurina (2014) exploits successive dynamic provisioning requirements imposed on Spanish banks by the Bank of Spain in the 2000s and that are a function of the provisioning rate accordingly to a close formula. Behn, Haselman and Wachter (2015) compare corporate lending by banks that migrated to Basel II under two different regulatory regimes. This paper is the closest to ours. We extend their results in two directions. First, our data allows us to directly link capital requirements to loan size. Second, we have access to firm-level outcomes from accounting data, which allows us to investigate the effect of capital requirements on trade credit and investment. Other related papers are Acharya and Steffen (2014), Behn, Haselman and Vig (2014) and the book by Freixas, Laeven and Peydro, (2015), which contend that banks manipulate capital requirements in order to maximize leverage. We propose various tests of this hypothesis in our data, and do not find much support for manipulation via internal models in the context of plain vanilla corporate loans. Leverage ratio manipulation remains, however, possible, through holding sovereign bonds or off-balance sheet commitments that are beyond the scope of our paper.

More broadly, this paper refers to the literature on the effects of bank funding shocks on corporate lending. Since Peek and Rosengren (1997, 2000) and Kashyap and Stein (2000), this literature has provided ample evidence that exogenous bank funding shocks affect corporate lending. After Kwhaja and Mian (2008), more recent papers have sharpened identification using loan-level data, and confirmed the initial findings that funding shocks matter.[6] These results, very much in the vein of the literature on investment-cash-flow sensitivity in corporate finance, suggest that external financing is costly, so that the MM theorem does not hold for these banks, at least in the short-run. In this sense, our paper is in line with these findings. Another similarity is that we can control for firm-level shocks, and like in most of these papers, we find that this control does not change our estimates

[6] See for instance: Puri, Rocholl and Steffen (2011), Jimenez, Ongena, Peydro and Saurina (2012); Iyer, Peydro and Schoar (2014).

very much. What differs is that our identification strategy does control for bank-funding shocks through the inclusion of bank-time fixed effects in our regressions. We can do this owing to the very specific nature of our problem: capital requirements are not defined at the bank level, but at the bank-firm level.

The roadmap of this paper is simple. Section 2 outlines a simple framework that justifies our regression analysis. Section 3 describes the data, and Section 4 the identification strategy. Section 5 describes the results and robustness checks. Section 6 concludes.

## 2. Conceptual Framework

A bank has access to a set of N possible projects. $R = (r_1, \dots, r_N)$ is the vector of project returns, with expected returns $\mu$ and covariance matrix $\Sigma$. $x = (x_1, \dots, x_N)$ is the vector of dollar holdings. The bank is assumed to behave as a mean-variance investor with risk aversion $\gamma$.

Let $E$ be the dollar amount of equity held by the bank, and $w = (w_1, \dots, w_N)$ the vector of regulatory risk weights. The regulator forces the sum of weighted exposures to be less than the amount of bank equity. Thus, under Basel II, the bank is constrained to satisfy $x' \cdot w \leq E$.

The bank thus solves the following portfolio problem:

$$\max_x \left\{ x'\mu - \frac{\gamma}{2} x'\Sigma x \right\} \quad (1)$$

$$\text{s.t. } x' \cdot w \leq E$$

Let $\lambda$ be the Lagrange multiplier of the regulatory constraint. Let us call $x_M = (\gamma\Sigma)^{-1}\mu$ be the Markowitz portfolio. The solution of the bank's problem then takes the simple form:

$$x = x_M - \lambda(\gamma\Sigma)^{-1}w \quad (2)$$

where the Lagrange multiplier is given by $\lambda = \frac{x_M{}' \ w - E}{w' \ (\gamma\Sigma)^{-1} w}$. [7]

The first lesson drawn from this formula is that the bank's portfolio is unaffected by risk weights when it has enough equity. This happens when $x_M' \cdot w < E$. In this case, the bank has enough equity to cover the regulatory capital needed to hold the Markowitz portfolio. The Lagrange multiplier is zero, and $x = x_M$. This is the intuition that risk weights do not matter when the equity is costless to raise.

The second lesson of this formula is that, as soon as equity is scarce, the regulatory constraint binds: $\lambda > 0$. The portfolio is then directly affected by risk weights. The bank will invest less in projects with high risk weights. This effect is stronger in banks for which equity is scarcer. The Lagrange multiplier is also increasing in the distance between risk-adujsted expected returns $(\gamma\Sigma)^{-1}\mu$ and risk-adjusted risk weights $(\gamma\Sigma)^{-1}w$. When projects with the highest expected returns tend to be the most penalized by regulation, the bank is more constrained, and lending is more sensitive to risk weights.

## 3. Data

In this study, we use three different datasets. The period under review is 2008-2011. The first sample is a panel of "active" bank-firm linkages, corresponding to banks lending a positive amount to firms. It is drawn from a large loan-level sample gathered by the French bank supervisor (the Autorité de Contrôle Prudentiel et de Résolution). This sample allows us to study the effect of capital requirements on the "intensive margin" of lending (i.e. the decision of how much to lend conditional on lending at all). The second sample is a panel of

[7] To fix ideas, one can assume that $\Sigma = \sigma^2.I$ to simplify the formula. In this case:

$$x = x_M - w.\frac{x_M{}'w - E}{w'w}$$

This formula shows that it is the collinearity between $x_M$ and $w$ that makes the constraint more binding.

active and inactive linkages, i.e. linkages corresponding to firms with potential lenders, with banks lending positive amounts and banks not lending to each firm. This sample allows us to study the "extensive margin" of lending, i.e. the decision to extend a loan or not irrespective its amount. Our third and last sample is a firm-level version of the second one: it contains information on the actual capital requirements faced by firms, as well as other balance sheet items available from separate accounting data.

## 3.a. Intensive margin sample

### 3.a.i. The loan-level survey

Our starting point is a large sample of loans extended by French banks to French firms over the 2008-2011 period. This dataset is built using the results of a confidential survey, conducted in 2012 by the French prudential authority. This survey required the six largest banking groups in France to provide detailed information on all their corporate loan exposures along with firm and bank identifiers that make it possible to match bank loans and firm-level data. This survey collects information on capital requirements of each loan. Its coverage is also very wide, as the six banking groups surveyed account for about 80% of all corporate loans in the country.

The sampling design of this survey is the following: using an ancillary dataset (*Centrale des Risques*), the French prudential authority drew up the list of all firms to which at least one bank subsidiary has an exposure of at least EUR 25,000 over the 2008-2011 period. This list was then sent to all six banking groups, which were asked to report all of their exposures to each of these firms over the entire 2008-2011 period. This leads to slightly more than three millions exposures of bank subsidiaries to firms. In the rest of this paper, we will refer to bank subsidiaries as "banks".

We keep four variables from this loan survey. The first one is loan size. We construct it using the exposure at default (EAD), which is a regulatory quantity equal to the amount outstanding for most non-defaulted loans. Credit lines are treated differently: their EAD is equal to the size of the credit line, multiplied by a regulatory ratio (known as the credit conversion factor). EAD for undrawn lines is thus smaller than the commitment. There are 2,854,608 loans reported in the database among which 27% have an on-balance sheet and an off-balance sheet EAD, 72% have just an on-balance sheet EAD and 18% have just an off-balance sheet EAD. The second variable is the internal capital requirement (CR), which is computed by the bank under the Basel II regulation, using either its internal model or the so called "standardized approach" (Behn, Haselman and Vig, 2013). Under the standardized approach of the Basel II regime, bank supervisors impose a simple mapping between a public credit rating of the firm on the one hand, and capital requirements on the other hand. If no external rating is available a 100% weight applies. The third variable is a dummy variable equal to 1 if the bank is allowed to use internal models. The fourth variable is a dummy equal to 1 if the loan is classified as "corporate" or "retail" for regulatory purposes. "Retail loans" are typically extended to smaller firms and are subject to lower capital requirements all other things being equal.

### 3.a.ii. Firm-bank linkage sample

We then collapse loan-level data into firm-bank level data. Some firms may have multiple loans from the same bank (for example, a credit line, a secured loan and a short-term loan). To simplify the structure of the data, we collapse these exposures by adding up all the different EADs, and by computing the EAD-weighted average of capital requirements. The "internal model" dummy is the same for all corporate loans granted by the same bank, so aggregation is straightforward for this variable.

We then remove two categories of firm-bank-year observations from this sample. Firstly, we restrict ourselves to loans that banks refer to as "corporate" exposures. The main reason for this is that, given the sampling technique of our survey (only firms borrowing at least EUR 25,000 from at least one bank), the coverage of corporate exposures is almost exhaustive, while for retail exposures it is likely to be sparser. But, in aggregate, our restriction to corporate loans is not too restrictive. According to COREP data (the French equivalent of the US Call Reports), the corporate portfolio accounts for about 70% of the total credit exposure of the French banks to the non-financial corporate sector. In our data (which are skewed towards larger, hence more corporate, loans), the corporate portfolio represents only 45% of the loans, but as much as 89% of the aggregate volume of exposures. Secondly, we also exclude from the sample a few banks (subsidiaries) that do not consistently report information over time. We end up with 83 different "banks", each one of them being a subsidiary of one of the six large groups on which our loan-level data is constructed.

Finally, we merge this dataset with firm-level accounting and credit rating information available from the Banque de France (*Centrale des Bilans*). Such information is updated annually. Accounting information comes from the tax forms that firms have to fill in and provides us with detailed data on firms' balance sheet and income statement. Credit ratings are awarded by a specific department of the Banque de France.

After these manipulations, we end up with 414,895 observations. This panel is unbalanced as each bank may not be actively lending to all of its clients every year over the period under review. Table 1, Panel A, provides summary statistics for the EAD and CR. The mean EAD is EUR 1.95 million and the mean capital requirement is 7%. This sample only contains bank-firm-year linkages that are "active" in the sense that the firm borrows a positive amount from the bank. Hence, this sample only allows us to investigate the impact on the "intensive margin" of lending, i.e. the size of loans conditional on the decision to lend at all.

## 3.b. Extensive margin sample

### 3.b.i. Sample construction

To study the extensive margin of lending, we need to observe the creation or termination of "active" bank-firm linkages. To do this, we artificially create "inactive" bank-firm linkages that correspond to banks that could potentially lend to the firm, but do not. For each firm, we thus need to define the set of potential lenders. We make the following assumption: we assume that, at each date $t$, the set of potential lenders to firm $f$ consists of all banks $b$ that do lend, at any point in time, to $f$.

To implement this, we repeat the same procedure as for the intensive margin sample. We start off with the loan-level data described in Section 3.a.i. and collapse them into firm-bank-year data as in Section 3.a.ii. At this stage, we create new bank-firm-year observations so that the panel becomes fully balanced: each bank-firm linkage has now 4 observations, one for each year. Newly created bank-firm-year linkages correspond to "potential" bank-firm relationships where firms are not actually borrowing from banks, so we set the loan amount to zero. More importantly, we do not observe the capital requirements when the bank does not actually lend to the firm, so we need to impute them. We present our imputation approach in the following section. Overall, the implicit assumption that we make in constructing this "extensive margin" sample is that a firm can only borrow from banks that have been observed to lend at least once over the 2008-2011 period.

Finally, we remove the same firm-bank-year observations as in the intensive margin sample: all "retail" loans and loans extended by a few banks that were not consistently reporting loans over time. We then merge the data with accounting and rating data from the Banque de France. Overall, the creation of "inactive" bank-firm linkages significantly increases sample size. The balanced panel contains 555,615 observations against 414,895 in the unbalanced panel: hence, "zero lending" observations account for about one fourth of the sample. There are 118,168 distinct firms for 180,605 firm-bank pairs, so that the average firm is linked to 1.5 banks over the period.

### 3.b.ii. Imputing capital requirements for "inactive" bank-firm linkages

In the "extensive margin" data, we have created observations where the bank *does not* lend to the firm. In this case, we obviously do not observe the CR that the bank would be facing if it had decided to lend. We describe here how we impute these CRs.

To do this, we start off with the unbalanced panel. For each bank, we compute the average CR of all loans extended to firms with the same rating. Let $b$ be the bank index, $f$ the firm index, $r$ the rating and $t$ the time index. We compute:

$$\overline{K}_{b,r,t} = \frac{1}{N_{b,r,t}} \sum_{f \in r} K_{b,f,t} \quad (3)$$

where $N_{b,r,t}$ is the number of firms of rating $r$ to which bank $b$ lends at $t$.

For all bank-firm pairs in the balanced panel, we then set the CR to be equal to $\overline{K}_{b,r,t}$. We thus assume that banks apply the same capital requirement to all firms that have the same Banque de France rating. To make this imputation, we need to observe the firm rating which is not available for smaller firms. Table 1, Panel B, shows the summary statistics for the balanced panel. Compared to the unbalanced data, the average CR is unchanged, but its standard deviation is divided by two. Indeed, capital requirements are now uniform within each of the 1,936 bank-rating-year buckets.

In Table 2, we describe the extent to which our imputation procedure captures variation present in the data. To simplify the exposition, we focus on a single year (2008). First, for each rating category, we regress observed capital requirements on the entire set of bank-dummies. We show that for the eight safest rating groups (from 1 to 8, i.e. 95% of the observations), a F-test strongly rejects the null hypothesis that all bank dummies are equal. For each rating category, this test equivalently rejects the hypothesis that imputed CRs are equal for all banks. For the three safest categories (about 30% of the observations), the $R^2$ of the regression is high (above 40%), suggesting that the difference between bank models drive a large fraction of the variation. The second part of Table 2 describes the heterogeneity in imputed CRs in the data. As expected, the distribution of expected CRs shifts to the right as risk increases, but the interquartile range hovers between 2 and 6 percentage points.

### 3.c. Firm-level sample

Last, we collapse the "extensive margin" dataset previously constructed into a firm-level dataset. The objective is to measure the impact of heightened capital requirements on firm-level decisions and outcomes such as capital structure and investment. We start from the exposure-level dataset described in Section 2.a. For all observations corresponding to the same firm, we then take the average of all variables, with the exception of the exposure at default that we sum across all banks. Firm-level accounting variables are not affected by this procedure, as they are by definition the same across all firm-bank linkages corresponding to the same firm-year. Note that we keep the information related to the lender before collapsing. We create a dummy that takes the value of one when a firm borrows from a given bank (there are therefore 83 such dummies). Hence, even when working at the firm level, we are able to control for bank and bank-year fixed-effects.

More importantly, when averaging the imputed capital requirements, we do not use a weighted average but an equally weighted one. We do this to keep information from imputed capital requirements, as we believe that bank decisions at the extensive margin of lending (i.e. the choice between lending and not lending) should affect the amount of debt that firms may build up, and ultimately their ability to invest. Take for example the case of a firm with two banking relationships. One of the two banks has a constant CR. The other one suddenly faces a higher CR, and stops lending: the firm borrows less and invests less. If we were to take the weighted average CR across the two banks, we would see no change in the CR, and a reduction in lending. While the two are actually connected, our model would fail to make the connection. Hence, using unweighted CRs allows us to take into account the capital requirements faced by a firm's potential lenders, and not only the actual lenders.

The summary statistics at the firm-level are presented in Panels C and D of the Table 1. The average firm has a total EAD across all its lender of EUR 2.6 million. The average capital requirement faced by actual and potential lenders is 7.07%. The median firm has liabilities of EUR 3.4 million, a turnover of EUR 4.32 million and fixed assets of EUR 1.79 million. Our focus on corporate loans excludes very small firms from the sample.

# 4. Empirical Strategy and Identification.

## 4.a. Loan-level Equation

Our first objective is to test whether capital requirements affect bank lending decisions (see also Behn, Haselman and Wachtel, 2013, for a related study focusing on German data). Our baseline equation is the following:

$$log\left(1 + EAD_{b,f,t}\right) = \beta \cdot K_{b,f,t} + \alpha_{f,t} + \delta_{b,t} + \varepsilon_{b,f,t} \tag{4}$$

where banks are denoted by $b$, firms by $f$, and time by $t$. $EAD_{b,f,t}$ is the exposure at default of bank $b$ to firm $f$ at time $t$ and $K_{b,f,t}$ is the regulatory capital requirement charged by bank $b$ to firm $f$ at time $t$. $\varepsilon_{b,f,t}$ are error terms, which we cluster at the bank-year level. It means that we allow standard errors to be correlated within bank-year clusters but not across them, which seems a reasonable assumption.

The null hypothesis is that $\beta = 0$. In this case, bank lending is not constrained by regulation, for two possible reasons, which can be seen in equation $(2)$. First, the bank is well capitalized. In this case, CRs do not matter (the Lagrange multiplier of the regulatory constraint is zero). Second, the bank has little equity, but can issue as much as it needs to finance all the $NPV > 0$ projects that it wishes to invest in (which means that $E$ can be arbitrarily fixed in our model sketch).

Equation (4) is saturated with bank-year and firm-year fixed effects. In the banking literature, firm-year fixed effects $\alpha_{f,t}$ are a standard way of controlling for firm-level demand shocks (see for instance Kwhaja and Mian, 2008; Iyer, Peydro and Schoar, 2012; Jimenez, Ongena, Peydro and Saurina, 2012). These controls are important if firm-level demand shocks are correlated with changes in capital requirements. For instance, a given firm may become riskier, which would lead to a reduction in lending (as the bank perceives the firm to be a less profitable borrower in risk-adjusted terms) and a mechanical increase in capital requirement (riskier firms command higher CRs). A limitation of this approach is that it may fail to control effectively for all demand effects if borrowing from a given firm is not randomly "assigned" to banks (Paravisini et al, 2014). In our case, we believe that this theoretical concern is less of a problem. The banking groups in our cleaned data set are universal banks that are in position to offer a wide range of financial services. The big advantage of the approach is that identification is easily obtained as soon as there is a significant number of firms that borrow from several banks. In our context, the estimate of $\beta$ will tend to be more negative if, for a given firm, banks with relatively high CRs are the ones that tend to lend relatively less. Such intra-firm comparison requires that some firms borrow from multiple banks. In our data, we check that these firms do not behave differently from mono-bank firms by estimating our model (without firm-year FE) on the two samples. We find identical coefficients (see below).

$\delta_{b,t}$ is a bank-specific fixed effect desgined to control for bank specific shocks. A large banking literature (starting with Peek and Rosengren, 2000, and many papers cited in this article), which documents the negative impact of bank equity shocks on lending. Our bank-year fixed effects are designed to control for these shocks. This is particularly crucial in the context of our study, which overlaps with the 2008 banking crisis. Hence, as long as bank-specific shocks affect all corporate lending in equal proportion within banks, our estimates control for such shocks. If, however, the banking crisis did generate a rebalancing of lending activity by the hardest-hit banks away from high CR firms, it would create bias in our estimates. Note however that our study is already an improvement compared to the existing literature. Indeed, most banking studies cannot include these bank-year fixed effects $\delta_{b,t}$ since they generally focus on the effect of such bank-level shocks on banks' lending policy (a currency crisis, a money market run, etc). Conversely, our analysis can accommodate bank-year level fixed effects since regulation induces intra-bank heterogeneity in lending costs: the same bank does not face the same regulatory constraint for two different firms, which allows us to filter out bank-specific shocks.

In our intensive margin study, we can, however, go one step further and control for the fact that different banks may treat different levels of firm risk in a systematically different way. We add to our regressions a bank-year-rating category fixed effects. Such a model is then identified from the variation in capital requirements within bank/year/rating category. It is thus identified for our intensive margin analysis (where we directly observe CRs for all firm-bank linkages) but not for our extensive margin analysis (where CRs are imputed at the bank-rating category level). Encouragingly, we find that this control does not affect our intensive margin estimates at all.

Given this discussion, our identifying assumption is relatively mild. We assume that, conditional on bank-year and firm-year fixed effects, capital requirements are not endogenous to loan size (i.e., uncorrelated with $\varepsilon_{b,f,t}$). In recent work, Behn, Haselman and Vig (2014) have shown that banks that are allowed to compute their own capital requirements (under the "Internal Ratings-Based" approach of Basel II, a sizable fraction of our sample) tend to have lower and less conservative capital requirements. This suggests that banks have some leeway in choosing their CRs. As long as this CR reduction is fixed at the bank level, or at the bank-rating category level, our estimate of β remains unbiased. However, to ensure that our estimation do not suffer from this reverse causality issue, we run a robustness check. We extract from the *Centrale des Bilans* the total exposure (which is not exactly the EAD) of firm $f$ to bank $b$ in the last quarter of 2006, namely before the implementation of Basel II. We then run the following regression:

$$K_{b,f,t} = \beta \cdot log\left(1 + Expo_{b,f,2006-Q4}\right) + \alpha_{f,t} + \delta_{b,t} + \varepsilon_{b,f,t} \qquad (5)$$

The intuition is the following: if banks try to exploit the discretion offered by Internal Ratings-Based models to "manipulate" their capital requirements, they have strong incentives to intensively manipulate capital requirements associated with their largest exposures. Hence, under the "manipulation" hypothesis, we should capture a statistically significant relationship between the size of the pre-Basel II exposure and the post-Basel II capital requirement. We first check that the pre-Basel II exposures are connected to post-Basel II EAD.

Finally, note that we estimate equation $(4)$ on two different samples: the unbalanced data where $EAD_{b,f,t} > 0$ all the time, and the balanced panel where $EAD_{b,f,t} = 0$ approximately a third of the times. The first sample allows us to focus on the *intensive margin* of lending, i.e. the sensitivity of lending to CR conditional on positive lending. The second sample includes both *intensive and extensive margins* of lending, i.e. the propensity to lend at all and the amount lent conditional on lending. The other difference between the two datasets is that the balanced panel has only imputed CRs.

The use of imputed capital requirements in the balanced panel still leaves us with a lot of identifying power. As we noted in Section 3.b., imputed CRs have much less variability than true ones. But the model remains identified, even in the presence of firm-year fixed effects. Identification comes from the fact that different banks typically attribute different CRs to firms in the same rating category, partly because their internal models are sufficiently different. We provide evidence on this residual variability below.

### 4.b. Firm-level Equation

After the bank-firm level analysis, we turn to corporate outcomes. The objective is to understand to what extent the effect of capital requirements on lending can affect the firms' decisions, especially the investment decisions. For this purpose, we use the firm level dataset described in the previous section. We then regress:

$$Y_{f,t} = \beta \cdot K_{f,t} + \delta_{b,t} + \gamma . X_{f,t} + \varepsilon_{b,t} \qquad (6)$$

where $Y_{f,t}$ can be any corporate outcome (total liabilities, turnover, fixed assets) and $K_{f,t}$ is the average capital requirement across all the lenders of a given firm. $X_{f,t}$ are firm-level controls such as rating and size dummies. Rating dummies are critical because capital requirements are imputed using actual and potential lenders' identities and firm ratings. Their inclusion ensures that $\beta$ is identified because different firms face different sets of actual and potential lenders who face different capital requirements.

$\delta_{b,t}$ corresponds to bank fixed effects, and is equal to $1$ whenever the firm borrows from bank $b$ at date $t$. These fixed effects are designed to capture the fact that some banks --for instance, those facing difficulties to obtain financing through the interbank market-- may simultaneously experience an increase in their risk weights, and a reduction in lending, without any causal link between the two. As for our loan-level regression $(4)$, inclusion of such effects is critical as the period under review corresponds to the banking crisis.

## 5. Results

### 5.a. Intensive margin

We start by examining the effect of the capital requirement at the intensive margin. Table 3 presents the results of the various versions of the specification $(4)$. The dependent variable is $\log(1 + EAD_{i,j,t})$. The sample contains each bank-firm-year linkage $(i, j, t)$ for which the banks have reported a positive exposure at default and the associated capital requirement. The advantage of this approach is that we are using the actual capital requirements coming from the data. The drawback is that we cannot explore the effect of CRs on the propensity to lend (the extensive margin).

We gradually saturate the regression with different sets of fixed-effects. All the regressions systematically include (i) a set of year fixed-effects that should capture aggregate fluctuations and changes in the regulation at the national level among other things and (ii) controls for firm size and ratings. We then progressively include bank and firm fixed effects, as well as bank-year and firm-year fixed effects, designed to control for bank-level and firm-level specific shocks. In columns 1-4, we add, step-by-step, bank FEs, and firm FEs. In column 5, we further add bank-year FEs to control for bank-specific funding shocks, which have been the focus of existing banking research. These are identified through the fact that banks lend to different firms. Finally, we include firm-year FEs, which are identified through the fact that some firms borrow from several banks. In column 6, we first restrict ourselves to such multi-bank firms in order to verify that it does not significantly change our baseline estimate. In column 7, we finally add the firm-year fixed effect.

Estimates from columns 1-7 are still vulnerable to the critique that some banks may cut back lending to risky firms more than that to safe ones, and that these banks have unobservable characteristics correlated with higher risk weights. To deal with this concern, we add in column 8 an interaction term $\delta_{b,t} \cdot rating_{f,t}$ . These interaction terms capture the fact that some banks may choose to lend less to firms with different rating. These terms are identified because each year, each bank lends to several different firms within each rating category. Within each bank-year-rating category, $\beta$ will come from the correlation between loan size and risk weights. Our point estimate is not significantly affected by the inclusion of this demanding control.

All in all, our estimates of $\beta$ are very similar across samples and specification, and are strongly significant even for the fully saturated specification. The point estimate hovers around -0.01, which means that a 1 percentage point increase in the capital requirement (say, from 7 to 8%), leads to a 1% reduction in the amount lent by the bank. This effect is statistically significant but it is not huge, because it misses the extensive margin of lending.

### 5.b. Extensive margin.

We now turn to the extensive margin of lending, for which we expect larger effects. The banking literature that studies the impact of funding shocks typically finds large effects on the propensity to lend. For instance, Puri, Rocholl and Steffen (2011), compare German banks according to their exposures to the American subprime market and find that affected banks rejected substantially more retail loan applications than unaffected banks following the outbreak of the financial crisis. Hence, any estimation that misses the impact of capital requirements on the decision to start lending, or to stop lending, will underestimate the impact of capital requirements.

Table 4 shows that the adverse effect of capital requirement on bank lending is magnified once the extensive margin is taken into consideration. The specification is again that of equation (4). The sample is larger than in Table 3 as it includes bank-firm-year observations for which the amount lent is zero. Capital requirements are now imputed for all bank-firm-year observations using the methodology described in Section 3.b. The main advantage of this approach compared to the intensive margin is that we can account for the impact of CRs on the decision to initiate or end a lending relationship. The drawback is that we need to rely on imputed CRs. We use imputed CRs for all bank-firm relations even if a true CR is observed in the data. This choice is dictated by consistency but does not affect our results. Table 4 has 7 columns that correspond to the progressive inclusion of our array of fixed effects. Given that estimates with firm-year fixed effects are identified on the subsample of multi-bank firms, column 6 first restricts the sample to these firms; we then add the firm-year fixed effects in column 7.

The estimated $\beta$ is again consistent and strongly significant across specifications; it is also larger economically than the pure intensive margin effect. On average across specifications, a 1 percentage point change in capital requirement leads banks to decrease the exposure at default by 10 % once bank's or firm's fixed-effects are taken into account. The magnitude of the effect is within the range of values of the effects reported in related studies. For instance, Aiyar et al. (2012) use bank-level data, and measure how much bank-lending reacts to changes in bank-level CRs imposed by the supervisor. These changes are likely to be endogenous since in principle they target weak banks, but they give us an order of magnitude of what is reasonable in these data. Aiyar et al. (2012) find that an increase in the capital requirement ratio of 1 percentage point leads to a cumulative fall in the lending growth to the whole non-financial sector of between 5.7 and 8 ppts. Thus, we find a large but plausible effect of capital requirements on lending, even in our most saturated specifications.

This estimate is also consistent with a "credit multiplier" view of the effects of capital requirements, where banks (1) allocate their equity across different categories of assets, and then (2) maximise lending subject to regulatory constraints for each of these assets. For a given loan category $i$, let us assume a bank allocates equity $E_i$. Loans in this category have a capital requirement of $w_i$. Let us then assume that the bank lends as much as it can under regulatory constraint: $L_i = E_i/w_i$. Such a behavioral model is less sophisticated than the portfolio optimization we described in Section 2, but it is very close to what regulators believe about banks. Assume now that $E_i$ is fixed when the CR varies. Lending then grows as: $\frac{\Delta L_i}{L_i} = -\left(\frac{1}{w_i}\right) \cdot \Delta w_i$ . Our estimates imply that $(1/w_i) = 0.1$, which is consistent with an average capital requirement in the vicinity of 10%, not very far from a sample mean of about 7%. The fact that it is a bit higher suggests that banks would have to apply larger capital requirements to behave consistently with the "credit multiplier" model and the data, but our estimates are too imprecise and the model probably too simple, to firmly draw this conclusion.

Given the large effect of capital requirements on lending, we can expect to find large effect on firm borrowing capacity and therefore investment. We look at these real effects in the next section.

### 5.c. Corporate Outcomes

In this section, we look at the effect of capital requirements on bank lending. We estimate equation (6), which by design cannot control for firm-level shocks like the loan-level regressions. This is unlikely to be a big limitation, however, since in Tables 3 and 4, we have seen that inclusion of the firm-year fixed effect does not significantly change our estimate of the impact of capital requirements. This observation suggests that the sorting of firms to capital requirements is not very endogenous, and therefore does not create much bias in our estimates. This justifies our aggregation at firm-level. As explained in Section 3.c., we aggregate the data used in Table 4 (i.e. including imputed CRs), and retrieve total EAD by firm-year and average (imputed) capital requirement. We do this to be consistent with the analysis of Table 4. Taking average observed (instead of imputed) CRs at the firm level does not generate results that are very different. Second, when aggregating CRs at firm level, we use equal weightings, not loan size weighting. The logic here is that we want to investigate the reaction of the firm to the CRs of the banks that it faces as potential, and not only actual, lenders. Loan weightings would mechanically exclude banks that are not actively lending from the analysis. Finally, the resulting firm-level data is matched with firm-level accounting data described in Section 3.c.

We show the results of our investigation in Table 5, which looks at the effect of capital requirements on various company-level variables. Panel A focuses on measures of firm size:

total EAD, total liabilities (asset minus equity) and sales. Panel B looks at the components of assets: working capital and investment. For each of these LHS variables, we present two regressions: one that controls for the identity of lending banks, and one that does not. Bank-year fixed effects allow to control for bank-wide funding shocks that may have occurred for some banks in our sample, are known to affect lending and may be correlated with firm-level capital requirements.

Looking at Table 5, Panel A, we find that an increase in capital requirements reduces firm borrowing. We start by estimating the impact of capital requirements on aggregate EAD. It should be recalled that the estimated equation is not the exact aggregation of the equations estimated in Table 4, as the average capital requirement here is non-weighted. The resulting point estimate is smaller than the estimates of Table 4, but is of a comparable order of magnitude and is strongly significant: a 1 percentage point increase in capital requirements leads to a 5% decrease in firm-level borrowing. In columns 3-4, we use, instead of aggregate EAD, the total liabilities available from firm accounts. Total liabilities are equal to total assets minus book equity, and thus include financial debt, tax arrears and trade payables.

Regressions indicate a certain degree of substitution between debt instruments, but not total substitution. A one percentage point increase in CRs leads to a statistically significant 1.6% decrease in total corporate liabilities. This is three times smaller than the effect on EAD, suggesting that firms may use alternative sources of funding when bank funding becomes scarcer (and conversely when it becomes more abundant). We investigate this directly in Panel B, columns 1-2, by looking at the effect of CRs on supplier debt. We find that a 1 percentage point increase in bank capital requirements leads to an increase of about 1% in supplier debt (trade payables). Overall, the effects on supplier and bank debt aggregate well into the result we find for total liabilities. In our data, the ratio of supplier debt to aggregate EAD is approximately equal to 2.[8] Assume a 1 percentage point increase in CRs, which leads to a 1% increase in supplier debt, and a 5% reduction in EAD. Assuming liabilities are mostly made up of trade and bank credit, the two effects aggregate into $\left(\frac{2}{3}\right) \cdot 1\% - \left(\frac{1}{3}\right) \cdot 5\% = -1\%$, which is close to the number we find for all liabilities. Overall, firms thus manage to offset the effect of capital requirements by adjusting trade payables, but only partially.

In Panel B, columns 3-6, we move on to explore the effect of capital requirement on the investment policy of the firm, and see that the reduction in debt capacity mostly results in lower investment. We consider two large components of assets coming from firm accounts: trade receivables and fixed assets. Taken together, these two categories of assets account for 89**%** of total assets in our data: this is a large share but one consistent with Barrot (2015), who finds in similar data that fixed assets account for about 25% of total assets, and trade receivables (debt to clients) account for some 50%. Looking at Panel B, columns 3-4, we find no reliable effect of bank capital requirements on the propensity to lend to client (trade receivables). The specification without bank funding shocks has a negative sign and is significant, but this effect is statistically non-discernable from funding shocks impacting banks over the period under review. While firms do not seem to reduce lending to clients, they adjust by decreasing investment. We find in Panel B, columns 5-6, that 1 percentage point increase in capital requirements leads to a 2.6% reduction in investment. This is a small fraction of the cross-sectional standard deviation of the log of fixed assets, but at the macro-economic level the impact is potentially very large, as the standard deviation of the French aggregate investment time-series since 1970 is 3.7%. Hence, assuming we trust our micro-estimates enough to aggregate them at the macro-economic level, a banking reform involving a 1 percentage point increase in the average capital requirement for corporate

[8] This number is consistent with the finding of by Barrot (2015), which documents a similar ratio of trade receivables to fixed assets.

lending would lead to a reduction in corporate investment that would be visible at the macro level.

### 5.d. Dynamics of the effects over time

A last issue that we would like to explore relates is the dynamics of the effect of capital requirements. While our results so far suggest a very large deviation from the MM framework, where bank equity is scarce enough to constrain lending, we expect equity to become more and more available as time passes (Hanson et al, 2011): thus, MM should hold in the long-run. Banks were severely hit by the financial crisis precisely at a time when the new Basel II regulatory framework was being implemented. Shortly after, they have faced the implementation of Basel III rules that again significantly increases capital requirements. Hence, banks are very likely to have incurred a shortage of capital during the period under review. However, this should have forced them to rebuild their capital buffer. In this case, the effect of capital requirements could diminish as banks begin to accumulate an equity cushion. This is what we test in this section. Results are presented in Table 6. We focus on two corporate outcomes: aggregate EAD and investment.

We do not observe any fading out of the effect over time, although our data only covers 4 years (2008-2011). While we fail to capture any statistically significant relation between capital requirement and bank lending in 2008, the pattern of the effect seems to be more on an upward path than on a downward one. It is quite likely that the period that we are looking at is too short, and that French banks were still constrained in 2011.

### 5.e. Robustness Checks

In this section, we address the potential criticism that banks may be able to manipulate risk weights in order to minimize their capital requirements (Behn, Haselman and Vig, 2014; Freixas, Laeven and Peydro, 2015). In this case, risk weights may be more indicative of banks' manipulation efforts than constraints on their ability to lend.

### 5.e.i. Evidence of Manipulation from the Asset Quality Review

A quantitative assessment of the effective model bias is given by the outcome of the 2014 *Asset Quality Review* ("AQR" hereafter). The AQR consisted in a comprehensive audit of banks' balance sheets supervised by the ECB (which is the case for all six banking groups in our sample) and conducted independently of national supervisors on behalf of the ECB by an external consultant. For the review of domestic corporate loan portfolios, a random sample of loans was collected for each bank portfolio and an independent provisioning assessment was conducted for each loan. This assessment led to corrections in the probability of default (PD) and the loss given default (LGD), the most two important drivers of the CR computed under the internal approach. The findings were then projected onto the entire portfolio.

Publicly available data support the idea that internal models were broadly correct for all six banking groups in our data (see ECB, 2014, p. 150). At banking group level, this audit led to a downward revision of equity ratios between 10 and 30 basis points, to be compared to equity ratios in the vicinity of 10%. The downward bias in CRs was thus present, but of an order of magnitude of 1 to 3%.

However, these numbers are aggregated at banking group level and may thus conceal a large degree of heterogeneity in asset composition and across portfolios. We have access to the PD and the LGD at portfolio level for the six banking groups in our dataset, both as stated by banks and as estimated by the AQR. We plug these two parameters into the Basel II formula to obtain a CR for each loan portfolio from the two viewpoints, that of the bank and that of the independent auditor. For all the portfolios in our sample, the difference in

CR estimates ranges between 0 and 88 basis points, with a weighted average of 22 basis points, a small bias compared to the size of capital requirements, even at portfolio level.[9]

### 5.e.ii. Testing for targeted manipulations

Even if the average bias is small, our approach may face a reverse causality problem if banks allocate their manipulation effort in priority to firms to which they lend the most. Let us assume for instance that the regulator requests that the average estimate of default risk be unbiased (as it broadly turns out to be in the AQR data), but cannot assess the bias conditional on variables that the bank observes. The bank then optimally tilts its models towards overestimating the risk of firms to which its exposure is low and underestimating the risk of firms to which its exposure is high. In this case, there would be a negative correlation between lending and risk weights, like the one we document, but for a different reason (although even this model would require a substantial deviation from Modigliani-Miller, since the bank would have to care about regulatory capital).

In order to test for this directly, we regress post 2008 capital requirements as provided by our data on 2006 bank exposures. Our large-scale survey does not contain the level of 2006 bank exposures to firms (it starts in 2008), but it contains firm and bank identifiers. Using these identifiers, as well as the national credit register managed by the French supervisors, we recover for a subset of our bank-firm relationships the exposure at default in December 2006. The null hypothesis is that if banks have some leeway in choosing CRs, they should have greater incentives to report a lower CR for their large exposures e.g. the most costly in term of capital, and report a higher CR for small exposures, so as to have a small average bias. Given that the exposure in 2006 comes from a different data source (national credit register) than post 2008 exposures (large-scale survey *Contreparties Communes*), we first check that post 2008 EAD are correlated with 2006 exposures at bank-firm level. In Table

[9] Our estimation is at the portfolio level. Note that these levels are overestimated due to the strong concavity of the RWA function.

A.1, we show, using various specifications that they strongly are. We then implement our manipulation test in Table 7. In this table, using our bank-firm "intensive margin" sample between 2008 and 2011, we simply regress capital requirements on banks' exposures to firms in 2006. The lower number of observations (250,000 against 410,000) comes from the fact that some bank-firm relationships in 2008-2011 do not exist in the national register in 2006, either because they do not exist at all, or because they are too small to be included in it (i.e. below the threshold of EUR 25,000). We find that these capital requirements are not materially correlated with the size of the 2006 exposure. These results lend evidence against the null hypothesis that banks choose models with a particular downward bias for firms to which they had large exposures.

### 5.e.iii. Evidence from "standardized" exposures.

Our last robustness check rests on the idea that some banks in our sample were not allowed to use internal models (the so-called "standardized" approach of Basel II) in all their portfolios (see Behn, Haselman and Vig, 2014, for a description). For these portfolios, the capital requirement of each corporate loan is a function of the borrower's external credit rating that is given by the banking supervisor. It does not depend on internal models and therefore cannot be subject to manipulation. If, however, capital requirements constrain bank lending as we argue, CRs should be negatively correlated with lending even for portfolios that follow the standardized approach.

We thus restrict the "intensive margin" analysis of Table 3 to firm-bank relationships that follow the standardized approach. We first check that there is enough variability in the data to identify the effect that we are interested in, by reproducing the analysis of Table 2 in Table A.2. In Table A.2, we show that, even in the standard approach, banks assign significantly different capital requirements to firms that are in the same Banque de France rating category. Clearly, the heterogeneity across banks is much smaller since the mapping from Banque de France ratings to capital requirements is exogenously imposed by the supervisor. However, as illustrated by F-tests ran on bank dummies, differences in CRs remain (see Table A.2) even *within* Banque de France rating categories. For all banks following the standard approach, the mapping from borrower ratings to capital requirements is the same, but borrower ratings may be different. The reason is that each bank selects a set of three rating agencies, which cover the portfolio well enough (this is checked by the banking supervisor). Then, borrowers rated by at least one agency receive the average of available ratings and the corresponding CR. Borrowers not rated at all get a CR of 8%. This heterogeneous choice of agencies across banks leads to common borrowers having different ratings across banks, even under the standard approach. F-statistics in Table A.2 confirm that such heterogeneity is large enough to identify bank fixed effects in capital requirements, even within a rating category.

In Table 8, we re-run the intensive margin regressions of Table 3 for bank-firm relations that are subject to the standardized approach. Results in this Table are still strongly significant, in line with the idea that even capital requirements that may not be manipulated have a large effect on lending.

# 6. Conclusion

This paper documents the effect of capital requirements. We find that capital requirements imposed on banks have a large impact on their lending capacity. Extrapolating our results at the macroeconomic level, we find that the reaction of banks to a given change in capital requirement is consistent with a "credit multiplier" model where lending is a constant multiple of available equity. Furthermore, we find that changes in capital requirement have a real impact on corporate investment policy. Faced with a reduction in their ability to borrow, firms shrink their assets, and partly but not fully compensate for this by reducing their lending to suppliers. They ultimately reduce their productive capital. A one percentage point increase in capital requirements reduces bank lending by some 8%, firm borrowing by some 4%, total assets by 1.5%, trade credit to clients by 1% and fixed assets by 2.5%.

This result is consistent with the existing literature on bank reactions to funding shocks. Banks are reluctant to raise capital in the short run, so an increase in equity requirements force them to delever, which has real consequences on the productive sector.

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# Figures and Tables

*Table 1: Summary Statistics*

| Label | N | Mean | Std Dev. | p25 | Median | p75 |
|---|---|---|---|---|---|---|
| **Panel A : Intensive margin sample (active bank-firm linkages only)** | | | | | | |
| Exposure at default (K€) | 414 895 | 1 957.82 | 15 726.73 | 84.54 | 266.99 | 837.21 |
| Log of exposure at default | 414 895 | 5.59 | 1.83 | 4.45 | 5.59 | 6.73 |
| Capital requirement (%), observed | 414 895 | 6.99 | 3.94 | 4.63 | 8.00 | 8.00 |
| **Panel B : Extensive margin sample (active and inactive bank-firm linkages only)** | | | | | | |
| Exposure at default (K€) | 555 615 | 1 461.96 | 13 616.67 | 0.00 | 126.18 | 546.23 |
| Log of exposure at default | 555 615 | 4.17 | 2.90 | 0.00 | 4.85 | 6.30 |
| Capital requirement (%), imputed | 555 615 | 7.02 | 1.88 | 6.00 | 7.77 | 8.09 |
| Internal Ratings-Based Dummy | 555 615 | 0.63 | 0.48 | 0.00 | 1.00 | 1.00 |
| **Panel C : Firm level Sample /Including zero exposures** | | | | | | |
| Log of turnover | 246 028 | 8.57 | 1.66 | 7.66 | 8.51 | 9.48 |
| Log of fixed assets | 246 028 | 7.48 | 1.97 | 6.36 | 7.41 | 8.53 |
| Log of total liabilities | 246 028 | 8.33 | 1.51 | 7.31 | 8.10 | 9.09 |
| Total exposure at default (K€) | 246 028 | 2 603.75 | 30 758.74 | 29.82 | 207.19 | 847.24 |
| Log of total exposure at default | 246 028 | 4.75 | 2.83 | 3.43 | 5.34 | 6.74 |
| Avg Capital requirement (%), imputed | 246 028 | 7.07 | 1.66 | 6.07 | 7.46 | 8.08 |

Note: This table provides descriptive statistics concerning the three samples used in this paper. Our data cover the 2008-2011 period. Panel A is the basic loan-level sample aggregated at the firm-bank-year level. Exposure at Default (EAD) is the sum of EADs of loans made by a bank to a firm in each given year. By construction, all bank-firm-year linkages have positive EADs. Panel B starts with the data described in Panel A, and artificially creates observations for each firm-bank-year for which there exists at least one year where the firm actively borrows from the bank. Capital requirements are imputed using the average CR of loans made by each bank to a given rating category in a given year. EAD is set to zero when the bank does not actively lend to the firm. Panel C describes the data obtained by collapsing the data observations shown in Panel B into firm-year level data. The firm-level CR is the unweighted average of all CRs imposed by related banks. Turnover, fixed assets and liabilities are retrieved by matching this sample with firm-level accounting data provided by the Banque de France.

*Table 2: Testing the imputation procedure*

| | Capital Requirement (2008) | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Rating Category** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** | **10** | **11** | **12** |
| Observations | 2,792 | 7,207 | 11,395 | 16,084 | 12,948 | 11,865 | 7,345 | 2,686 | 259 | 222 | 94 | 2,715 |
| R-squared | 0.658 | 0.542 | 0.408 | 0.167 | 0.055 | 0.025 | 0.048 | 0.066 | 0.058 | 0.141 | 0.168 | 0.201 |
| F-test | 166.14 | 223.14 | 206.25 | 80.54 | 18.04 | 7.71 | 8.84 | 4.7 | .55 | 1.41 | .84 | 19.87 |
| Prob > F | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | .97 | .11 | .65 | 0 |
| p25 | 1.86 | 2.74 | 3.7 | 5.31 | 6.05 | 6.79 | 6.21 | 7.52 | 8 | 6.17 | 4.23 | 0 |
| p50 | 4.1 | 5.11 | 6.08 | 7.74 | 8 | 8 | 8 | 8 | 8 | 8 | 8.01 | 0 |
| p75 | 8 | 8 | 8 | 8 | 8.08 | 8.96 | 10.72 | 12 | 12 | 12 | 12 | 8 |

Note: In this table, we start off with the basic loan-level data of active bank-firm relationships described in Table 1, Panel A. We focus on one single year: 2008. We then regress the observed capital requirement on bank dummies. We run one regression per Banque de France rating category (numbered from 1 to 12, category #1 being the safer category and category #12 the riskiest one). Each column of the table reports the regression result in the corresponding rating class. In the first line, we report the number of observations used, and in the second line the resulting R-squared. In the third line, we report the F-statistic of joint significance of bank dummies, as well as the associated p-value in the fourth line. Since there are many bank dummies, we do not report all regression coefficients, but the quartile breakpoints of their distribution. The lines p25, p50 and p75 indicates the breakpoints associated with the 25$^{th}$, 50$^{th}$ and 75$^{th}$ percentile of the distribution of the average capital requirement conditional on the rating category. Reading: For all firms of rating category 4, there are 16,084 bank-firm-year active linkages in our basic sample. Regressing capital requirements on bank dummies leads to a R-squared of 16.7%, and an F statistic of joint significance of 80.54, so the data strongly reject a model where all CRs are equal across banks in this rating category. Looking at the distribution of CRs, the median is 7.74.

## *Table 3: Intensive Margin Regressions*

| | Log of (1+Exposure at Default) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| Capital requirement | -0.009** | -0.010* | -0.008*** | -0.008*** | -0.008*** | -0.009*** | -0.012*** | -0.009*** |
| | (0.004) | (0.005) | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) | (0.003) |
| Observations | 414,895 | 414,895 | 414,895 | 414,895 | 414,895 | 245,205 | 245,205 | 245,205 |
| Adjusted R-squared | 0.168 | 0.197 | 0.599 | 0.617 | 0.619 | 0.555 | 0.485 | 0.497 |
| Year FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Firm Size FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Firm Rating FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Bank FE | NO | YES | NO | YES | YES | YES | YES | YES |
| Firm FE | NO | NO | YES | YES | YES | YES | YES | YES |
| Bank*Year FE | NO | NO | NO | NO | YES | YES | YES | YES |
| Firm*Year FE | NO | NO | NO | NO | NO | NO | YES | YES |
| Bank*Year*Rating FE | NO | NO | NO | NO | NO | NO | NO | YES |
| Cluster | Bank*Year | Bank *Year | Bank *Year | Bank *Year | Bank *Year | Bank *Year | Bank *Year | Bank *Year |
| Sample | Full | Full | Full | Full | Full | Multi-Banks | Multi-Banks | Multi-Banks |

Note: This table shows the results of regression (4) for the intensive margin of lending. The sample is described in Table 1, Panel A. It is made up of firm-bank-year linkages. By construction, all firm-bank-year linkages in the data are such that the bank lends a strictly positive amount to the firm in the relevant year. Capital requirements are directly observed in the data. Columns 1 to 5 progressively include a large array of fixed effects. Columns 6-8 focus on observations corresponding to firms borrowing from at least two different banks, i.e. observations for which the model with firm-year fixed effects is identified. Column 7 does include a firm-year fixed effect. Column 8 further adds a bank x rating category fixed effect designed to capture the differential effect of bank funding shocks across observable risk categories. Error terms are clustered at the bank-year level. Stars indicate statistical significance (* for 10%, ** for 5% and *** for 1%). Standard errors are in brackets.

## *Table 4: Extensive Margin Regressions*

| | Log of (1+Exposure at Default) | | | | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| Capital requirement (imputed) | -0.044* | -0.094*** | -0.075* | -0.102** | -0.116** | -0.090** |
| | (0.025) | (0.032) | (0.040) | (0.050) | (0.051) | (0.039) |
| Observations | 555,615 | 555,615 | 555,615 | 555,615 | 555,615 | 555,615 |
| Adjusted R-squared | 0.052 | 0.076 | 0.266 | 0.288 | 0.298 | 0.280 |
| Year FE | YES | YES | YES | YES | YES | YES |
| Firm Size FE | YES | YES | YES | YES | YES | YES |
| Firm Rating FE | YES | YES | YES | YES | YES | YES |
| Bank FE | NO | YES | NO | YES | YES | YES |
| Firm FE | NO | NO | YES | YES | YES | YES |
| Bank*Year FE | NO | NO | NO | NO | YES | YES |
| Firm*Year FE | NO | NO | NO | NO | NO | YES |
| Cluster | Bank*Year | Bank *Year | Bank *Year | Bank *Year | Bank *Year | Bank *Year |

Note: This table shows the estimates of regressions (4) on the sample of both active and inactive bank-firm-year lending relationships described in Table 1, Panel B. For active relationships, observations come directly from our main sample. For inactive relationships, we set the EAD to zero. For all active and inactive relationships, we use imputed capital requirements defined by the average CR within each bank-rating category-year category. Various fixed effects are progressively added between columns 1 and 6. Unlike for Table 3, all firms have multiple bank relationships, so firm-year fixed effects are identified on the entire sample. Error terms are clustered at the bank-year level. Stars indicate statistical significance (* for 10%, ** for 5% and *** for 1%). Standard errors are in brackets.

*Table 5: Firm Level Outcomes*

| Panel A | Lof of (1+Total EAD) | | Log of (1+Total Liabilities) | | Log of (1+Turnover) | |
|---|---|---|---|---|---|---|
| Avg Capital requirement (imputed) | -0.009* | -0.048*** | -0.032*** | -0.014*** | -0.006** | 0.004 |
| | (0.005) | (0.005) | (0.003) | (0.003) | (0.003) | (0.004) |
| Observations | 316,805 | 316,805 | 246,028 | 246,028 | 246,028 | 246,028 |
| Adjusted R-squared | 0.098 | 0.187 | 0.665 | 0.684 | 0.589 | 0.594 |
| Year FE | YES | YES | YES | YES | YES | YES |
| Firm Size FE | YES | YES | YES | YES | YES | YES |
| Firm Rating FE | YES | YES | YES | YES | YES | YES |
| Bank*Year FE | NO | YES | NO | NO | NO | YES |
| Cluster | Firm | Firm | Firm | Firm | Firm | Firm |
| **Panel B** | **Log of (1+Trade Payables)** | | **Log of (1+Trade Receivables)** | | **Log of (1+Fixed Assets)** | |
| Avg Capital requirement (imputed) | -0.015*** | 0.009** | -0.050*** | 0.004 | -0.011** | -0.025*** |
| | (0.003) | (0.004) | (0.005) | (0.006) | (0.005) | (0.005) |
| Observations | 246,028 | 246,028 | 246,028 | 246,028 | 246,028 | 246,028 |
| Adjusted R-squared | 0.547 | 0.556 | 0.418 | 0.443 | 0.383 | 0.412 |
| Year FE | YES | YES | YES | YES | YES | YES |
| Firm Size FE | YES | YES | YES | YES | YES | YES |
| Firm Rating FE | YES | YES | YES | YES | YES | YES |
| Bank*Year FE | NO | YES | NO | YES | NO | YES |
| Cluster | Firm | Firm | Firm | Firm | Firm | Firm |

Note: In this table, we show the regression results of equation (6) for different firm-level outcome variables. Bank-year fixed effects are dummy variables for each bank with which the firm has established a relationship. Our sample is made up of firm-year observations and is described in Table 1, Panel C. Firm-level capital requirements are computed using the unweighted average of imputed capital requirements across all bank-firm relationships. In columns 1,3 and 5 we show the estimates without bank-year fixed effects. In columns 2,4 and 6, we add bank-year fixed effects. Panel A, columns 1-2 uses the log of firm-level aggregate EAD as a LHS variable. It is obtained by aggregating our loan-level data and is thus available for a larger sample. The other regressions use variables obtained from firm-level accounting data, and are thus available for a restricted subsample. Total liabilities are equal to total assets minus book equity. Turnover is total sales. Error terms are clustered at the firm level. Stars indicate statistical significance (* for 10%, ** for 5% and *** for 1%). Standard errors are in brackets.

## *Table 6: Effect of Capital Requirements Over Time*

| | Panel A : Log of (1+Total EAD) | | | |
|---|---|---|---|---|
| Avg Capital requirement (imputed) | -0.013 | -0.027*** | -0.113*** | -0.044*** |
| | (0.010) | (0.008) | (0.007) | (0.007) |
| Observations | 60,637 | 82,143 | 85,399 | 88,626 |
| Adjusted R-squared | 0.196 | 0.191 | 0.192 | 0.189 |
| Firm Size FE | YES | YES | YES | YES |
| Firm Rating FE | YES | YES | YES | YES |
| Bank FE | YES | YES | YES | YES |
| Year | 2008 | 2009 | 2010 | 2011 |
| | **Panel B : Log of (1+Fixed Assets)** | | | |
| Avg Capital requirement (imputed) | -0.009 | -0.020*** | -0.030*** | -0.047*** |
| | (0.007) | (0.006) | (0.006) | (0.007) |
| Observations | 54,518 | 72,867 | 75,451 | 73,560 |
| Adjusted R-squared | 0.423 | 0.393 | 0.383 | 0.381 |
| Firm Size FE | YES | YES | YES | YES |
| Firm Rating FE | YES | YES | YES | YES |
| Bank FE | YES | YES | YES | YES |
| Year | 2008 | 2009 | 2010 | 2011 |

Note: This table estimates the specification used in Table 5, Panel A, column 2, and Panel B, column 6, separately for each year in our data. The sample is the same as in Table 5, split into 4 cross sections for each year in our data (2008, 2009, 2010, 2011). Bank-year and firm-level fixed effects are not identified anymore, but bank FE control for bank-level lending shocks. Panel A uses log firm-level borrowing (firm-level aggregate EAD) as the LHS variable. This variable comes directly from our loan-level data. Panel B uses total fixed assets from firm-level accounts, and is thus estimated on a more restricted number of observations. Error terms are clustered at the firm level. Stars indicate statistical significance (* for 10%, ** for 5% and *** for 1%). Standard errors are in brackets.

## *Table 7: Reverse Causality and Capital Requirements*

| | Capital requirement | | | | | | |
|---|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** |
| Log of (1+2006-Q4 Exposure) | -0.091*** | -0.020 | 0.008 | 0.007 | -0.028* | -0.006 | -0.053 |
| | (0.027) | (0.028) | (0.023) | (0.023) | (0.016) | (0.021) | (0.035) |
| Observations | 255,570 | 255,570 | 255,570 | 255,570 | 255,570 | 89,992 | 165,578 |
| Adjusted R-squared | 0.002 | 0.062 | 0.067 | 0.070 | 0.374 | 0.517 | 0.348 |
| Sample | All | All | All | All | All | Standardized | IRB |
| Year FE | YES | YES | YES | YES | YES | YES | YES |
| Bank FE | NO | YES | YES | YES | YES | YES | YES |
| Size FE | NO | NO | YES | YES | YES | YES | YES |
| Bank*Year FE | NO | NO | NO | YES | YES | YES | YES |
| Firm FE | NO | NO | NO | NO | YES | YES | YES |
| Firm*Year FE | NO | NO | NO | NO | YES | YES | YES |
| Cluster | Bank*Year | Bank*Year | Bank*Year | Bank*Year | Bank*Year | Bank*Year | Bank*Year |

Note: This table shows the results of regression (5) testing for the potential reverse causality. The capital requirements corresponding to a given firm-bank relationship over the post-Basel II period (2008-2011) are regressed on the exposure observed in December 2006 for the same bank relationship. We start from the sample described in Table 1, Panel A. We then restrict our data to the firm-bank-year linkages with a strictly positive amount in the national credit register in December 2006 as well at least one positive amount over the period 2008-2001 for which the actual capital requirement are reported by the banks. Columns 1 to 5 progressively include a large array of fixed effects. Columns 6-8 focus on observations corresponding to firms borrowing from at least two different banks, i.e. observations for which the model with firm-year fixed effects is identified. Column 7 does include the firm-year fixed effect. Column 8 further adds a bank x rating category fixed effect designed to capture the differential effect of bank funding shocks across observable risk categories. Error terms are clustered at the bank-year level. Stars indicate statistical significance (* for 10%, ** for 5% and *** for 1%). Standard errors are in brackets.

*Table 8: Intensive Margin Regressions: Standardized Approach only*

| | Log of (1+Exposure at Default) | | | | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| Capital requirement | -0.087** | -0.066* | -0.023 | 0.021 | -0.069*** | -0.063** |
| | (0.037) | (0.034) | (0.050) | (0.046) | (0.014) | (0.029) |
| Observations | 207,834 | 207,834 | 207,834 | 207,834 | 207,834 | 207,834 |
| Adjusted R-squared | 0.056 | 0.085 | 0.288 | 0.309 | 0.324 | 0.292 |
| Year FE | YES | YES | YES | YES | YES | YES |
| Firm Size FE | YES | YES | YES | YES | YES | YES |
| Firm Rating FE | YES | YES | YES | YES | YES | YES |
| Bank FE | NO | YES | NO | YES | YES | YES |
| Firm FE | NO | NO | YES | YES | YES | YES |
| Bank*Year FE | NO | NO | NO | NO | YES | YES |
| Firm*Year FE | NO | NO | NO | NO | NO | YES |
| Cluster | CIB*Year | CIB*Year | CIB*Year | CIB*Year | CIB*Year | CIB*Year |

Note: This table shows the results of regression (4) for the intensive margin of lending. The sample is described in Table 1, Panel A. It is made up of firm-bank-year linkages. By construction, all firm-bank-year linkages in the data are such that the bank lends a strictly positive amount to the firm in the current year. Capital requirements are directly observed in the data. Columns 1 to 5 progressively include a large array of fixed effects. Columns 6-8 focus on observations corresponding to firms borrowing from at least two different banks, i.e. observations for which the model with firm-year fixed effects is identified. Column 7 does include the firm-year fixed effect. Column 8 further adds a bank x rating category fixed effect designed to capture the differential effect of bank funding shocks across observable risk categories. Error terms are clustered at the bank-year level. Stars indicate statistical significance (* for 10%, ** for 5% and *** for 1%). Standard errors are in bracket

## *Table A.1: Persistence of exposure across years and datasets*

| | Log of (1+Exposure at Default) | | | | | | |
|---|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** |
| Log of (1+2006-Q4 Exposure) | 0.741*** | 0.787*** | 0.745*** | 0.746*** | 0.641*** | 0.789*** | 0.579*** |
| | (0.016) | (0.011) | (0.013) | (0.013) | (0.034) | (0.047) | (0.059) |
| Observations | 335,187 | 335,187 | 335,187 | 335,187 | 335,187 | 119,275 | 215,912 |
| Adjusted R-squared | 0.168 | 0.194 | 0.213 | 0.227 | 0.835 | 0.892 | 0.899 |
| Sample | All | All | All | All | All | Standardized | IRB |
| Year FE | YES | YES | YES | YES | YES | YES | YES |
| Bank FE | NO | YES | YES | YES | YES | YES | YES |
| Size FE | NO | NO | YES | YES | YES | YES | YES |
| Bank*Year FE | NO | NO | NO | YES | YES | YES | YES |
| Firm FE | NO | NO | NO | NO | YES | YES | YES |
| Firm*Year FE | NO | NO | NO | NO | YES | YES | YES |
| Cluster | Bank*Year | Bank*Year | Bank*Year | Bank*Year | Bank*Year | Bank*Year | Bank*Year |

Note: This table reports the results of regression (5) testing for the potential reverse causality. The exposure corresponding to a given firm-bank relationship over the post-Basel II period (2008-2011) are regressed on the exposure observed in December 2006 for the same bank relationship. We start from the sample described in Table 1, Panel A. We then restrict our data to the firm-bank-year linkages with a strictly positive amount in the national credit register in December 2006 as well at least one positive amount over the period 2008-2001 for which the actual capital requirement are reported by the banks. Columns 1 to 5 progressively include a large array of fixed effects. Columns 6-8 focus on observations corresponding for firms borrowing from at least two different banks, i.e. observations for which the model with firm-year fixed effects is identified. Column 7 does include the firm-year fixed effect. Column 8 further adds a bank x rating class fixed effect designed to capture differential effect of bank funding shocks across observable risk classes. Error terms are clustered at the bank-year level. Stars indicate statistical significance (* for 10%, ** for 5% and *** for 1%). Standard errors are between parentheses.

*Table A.2: Capital requirement heterogeneity under the Standardized Approach*

| | Capital Requirement (2008) | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Rating Class** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** | **10** | **11** | **12** |
| Observations | 1,293 | 3,389 | 5,550 | 8,156 | 6,409 | 6,197 | 3,570 | 1,332 | 177 | 122 | 42 | 861 |
| R-squared | 0.573 | 0.579 | 0.463 | 0.336 | 0.219 | 0.132 | 0.134 | 0.173 | 0.083 | 0.425 | 0.657 | 0.855 |
| F-test | 58.52 | 131.54 | 135.82 | 114.08 | 47.06 | 25.95 | 14.41 | 7.33 | .6 | 3.73 | 3.32 | 163.45 |
| Prob > F | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | .92 | 0 | 0 | 0 |
| p25 | 4.61 | 5.84 | 6.4 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 0 |
| p50 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8 |
| p75 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8 | 8.45 | 8.27 | 9.45 | 9.88 |

Note: In this table, we start from the basic loan-level data of active bank-firm relationships described in Table 1, Panel A. We focus on one single year: 2008. We only consider the exposures for which the standardized approach applies. We then regress the observed capital requirement on bank dummies. We run one regression per Bank of France rating class (numbered from 1 to 12, class #1 being the safer class and class #12 the riskiest). Each column of the Table reports the regression result in the corresponding rating class. In the first line, we report the number of observations used, and in the second line the resulting R squared. In the third line, we report the F-statistic of joint significance of bank dummies, as well as the associated p-value in the fourth line. Since there are many bank dummies, we do not report all regression coefficients, but the quartile breakpoints of their distribution. The lines p25, p50 and p75 indicates the breakpoints associated with the $25^{th}$, $50^{th}$ and $75^{th}$ percentile of the distribution of average capital requirement conditional on the rating class. Reading: For all firms of rating class 2, there are 3,389 bank-firm-year active linkages in our basic sample. Regressing capital requirements on bank dummies leads an R squared of 57.9%, and an F statistic of joint significance of 131.08, so the data strongly reject a model where all CRs are equal across banks in this rating class. Looking at the distribution of CRs, the median is 8.

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