WORKING paper

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Unconventional Monetary Policy and Bank Lending Relationships

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ABSTRACT

How do banks transmit long-term central bank liquidity injections to borrowers? We exploit unique variation in how the ECB's 2011-12 Long-Term Refinancing Operations (LTROs) affected lending to firms discontinuously across credit ratings (within banks) to make four contributions. (i) We show the LTROs induced increased bank lending to firms in France, including to SMEs, an elusive policy objective. (ii) We uncover important heterogeneity: banks pass through LTRO liquidity very differently to multi- bank firms than they do to firms with only one bank. (iii) Differences in liquidity transmission map to archetypal lending types: single-bank firms receive relationship lending, and these firms invest and grow in response, while multi-bank firms receive transactions-style lending and do not increase their investment. (iv) While the majority of the effect flows to firms whose loans are policy-eligible, we identify a spillover (onto multi-bank firms only) that appears to be driven by bank competition for borrowers.

Keywords: Unconventional Monetary Policy, Relationship Banking, SME Finance, Bank Lending, Collateral

JEL classification: E51 ; E52 ; E58 ; G01 ; G2

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NON-TECHNICAL SUMMARY

Supporting bank lending to firms in times of economy-wide financial stress has been a major policy goal of the Unconventional Monetary Policies (UMP) deployed since 2008, in response to extensive evidence that constrained banks sharply reduce the availability of credit to firms, especially smaller firms. However, the effectiveness of different types of UMP in stabilizing bank lending, the transmission mechanisms, and which firms benefit from such policies remain unclear.

In response to the deepening European Sovereign Debt Crisis, in December 2011 the ECB announced two rounds of Long-Term Refinancing Operations (LTROs henceforth), a €1 trillion program of extraordinary long-term lending to banks. The LTROs allowed banks to borrow amounts that were limited only by their ability to provide eligible collateral, at a time when market liquidity was scarce, and as expensive as it had been during the crisis of 2008-9. In an important change to standard ECB liquidity provision, which had maximum maturity of three months, LTRO lending had a three-year term, greatly reducing rollover risk for banks and providing funding at below market rates. Because bank loans to firms with strong credit ratings could themselves be pledged as LTRO collateral, the cost of lending to firms whose debt was eligible dropped suddenly and substantially, in contrast to the cost of lending to firms whose debt was ineligible. We exploit this exceptional policy change − the sudden appearance of a difference in the cost of lending to some bank borrowers (but not to others at the same bank) − to identify the causal effect of the LTRO program in France, the third largest recipient of LTRO funds after Spain and Italy.

Our main empirical design exploits the fact that in France the second LTRO was paired with a lowering (by one notch) of the minimum firm credit rating required for a firm's loans to be eligible as collateral for the ECB. This created a policy change with clear "treated" and "control" groups for a difference in differences research design: firms on either side of the new eligibility threshold. We report several novel findings.

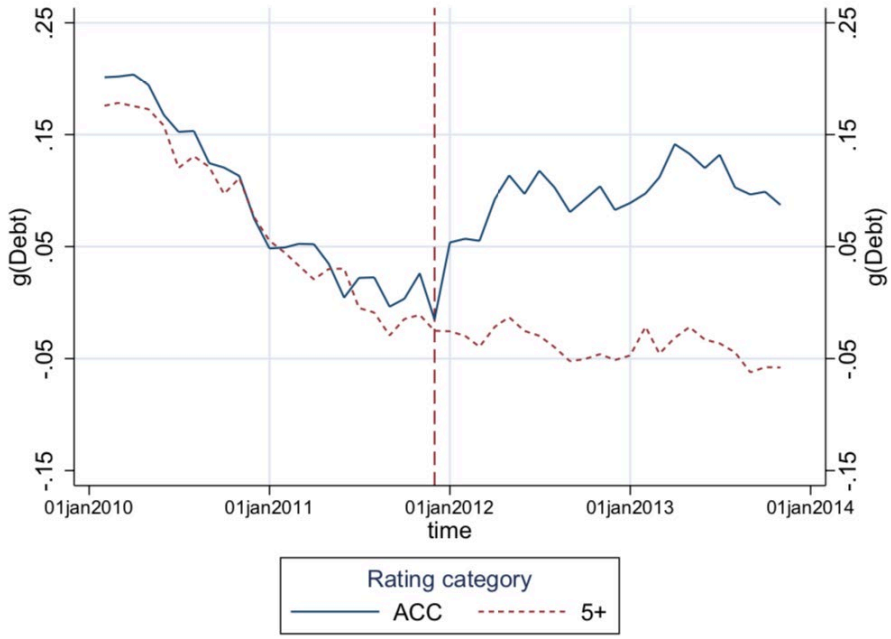
Firstly, we examine the effectiveness of the policy in supporting bank lending to firms, and to small and medium enterprises (SMEs) in particular, as they are well-known to bear the brunt of credit contractions. In particular, we find that the fall in the cost of funding loans to eligible firms is rapidly transmitted, leading to an increase in the amount of bank credit to SMEs of 8-9 percentage points for newly-eligible firms (relative to controls), and 12 percentage points for the LTRO program as a whole. That is, a policy reducing banks' cost of funding loans to a subset of firms in a crisis period is effective in causing an increase in credit supply to such firms (see figure).

We then examine how policy-driven liquidity is passed through to firms, uncovering a new and important dimension of heterogeneity: banks transmit LTRO liquidity differently to multi-bank firms than they do to firms with only one bank. Single-bank firms are especially well suited to relationship borrowing because their banks can access information on the full range of their bank interactions, and also face reduced concerns regarding borrower strategic default, or information externalities benefiting other banks. Accordingly, we find that differences in liquidity transmission map to archetypal lending types: single-bank firms receive credit with characteristics consistent with relationship lending, while multi-bank firms receive more transactions-style lending. In turn, this affects which firms receive new loans, whether the funds are used for investment, the existence of spillover lending to ineligible firms, and ex post loan performance.

In short, banks respond to the policy by offering a different lending product to single-bank firms than they do to multi-bank firms, a difference that is crucial to understanding the channels and effects of liquidity-based UMP on bank borrowers. More generally, our results

strongly suggest that the effects of bank shocks or Government policies on research samples of multi-bank firms should not be directly extrapolated to the large population of single-bank firms, which appear to be differentially affected. Using our estimates of the effect on lending to firms (and subject to numerous caveats), we calculate that, absent the LTRO policy, total credit to the firms in the affected credit rating categories would have decreased by at least 10%, whereas the actual decrease was only 4%.

Trends in Credit Growth for Treated and Control firms Single-bank firms



Source: Cahn, Duquerroy, and Mullins (2019)

Politique monétaire non conventionnelle et relation bancaire

RÉSUMÉ

Comment les banques transmettent-elles les injections de liquidités à long terme des banques centrales aux emprunteurs ? En exploitant une discontinuité dans la manière dont les opérations de refinancement à long terme (LTRO) de la BCE de 2011-2012 ont affecté les prêts aux entreprises, nous montrons (i) que les LTRO ont permis d'accroître le crédit aux entreprises en France, y compris aux PME ; (ii) que les banques transforment la liquidité LTRO de manière très différente selon que les entreprises n'ont qu'une seule banque ou plusieurs ; (iii) que cette distinction se reflète dans la nature du crédit : crédit relationnel pour les entreprises monobancaires, crédit transactionnel pour les autres ; (iv) qu'il existe un effet d'entraînement de la mesure (sur les entreprises multi-banques uniquement) qui semble être lié à la concurrence entre banques d'un même emprunteur.

Mots-clés : Politique monétaire non conventionnelle, Relation bancaire, Financement des PME, Offre de crédit, Collatéral.

Les Documents de travail reflètent les idées personnelles de leurs auteurs et n'expriment pas nécessairement la position de la Banque de France. Ils sont disponibles sur publications.banque-france.fr

Unconventional Monetary Policy and Bank Lending Relationships*

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Abstract

How do banks transmit long-term central bank liquidity injections to borrowers? We exploit unique variation in how the ECB's 2011-12 Long-Term Refinancing Operations (LTROs) affected lending to firms discontinuously across credit ratings (within banks) to make four contributions. (i) We show the LTROs induced increased bank lending to firms in France, including to SMEs, an elusive policy objective. (ii) We uncover important heterogeneity: banks pass through LTRO liquidity very differently to multi-bank firms than they do to firms with only one bank. (iii) Differences in liquidity transmission map to archetypal lending types: single-bank firms receive relationship lending, and these firms invest and grow in response, while multi-bank firms receive transactions-style lending and do not increase their investment. (iv) While the main effect flows to firms whose loans are policy-eligible, we identify a spillover (onto multi-bank firms only) that appears to be driven by bank competition for borrowers.

JEL Classification: G21, E52, E58, E51, G01
Keywords: Unconventional Monetary Policy, Relationship Banking, SME Finance, Bank Lending, Collateral

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1 Introduction

"We established very long-term refinancing operations with a maturity of three years. This duration is a novelty in ECB monetary policy operations...

[We] will allow banks to use loans as collateral with the Eurosystem, thereby unfreezing a large portion of bank assets. It should also provide banks with an incentive to abstain from curtailing credit to the economy...

The goal of these measures is to ensure that households and firms – and especially small and medium-sized enterprises – will receive credit as effectively as possible under the current circumstances."

Mario Draghi, President of the ECB, 15 December, 2011

Supporting bank lending to firms in times of economy-wide financial stress has been a major policy goal of the Unconventional Monetary Policies (UMP) deployed since 2008, in response to extensive evidence that constrained banks sharply reduce the availability of credit to firms, especially smaller firms. However, the effectiveness of different types of UMP in stabilizing bank lending, the transmission mechanisms, and which firms benefit from such policies remain unclear.

In response to the deepening European Sovereign Debt Crisis, in December 2011 the ECB announced two rounds of Long Term Refinancing Operations (LTROs henceforth), a €1 trillion program of extraordinary long-term lending to banks. This paper examines the effectiveness of the LTROs in driving lending to firms, and uncovers the role of lending relationships in policy transmission, highlighting an important source of heterogeneity across firms.

The LTROs allowed banks to borrow amounts that were limited only by their ability to provide eligible collateral, at a time when market liquidity was scarce, and as expensive as it had been during the crisis of 2008-9. In an important change to standard ECB liquidity provision, which had maximum maturity of three months, LTRO lending had a three year term, greatly reducing rollover risk for banks and providing funding at below market rates.[1] Because bank loans to firms with strong credit ratings could themselves be pledged as LTRO collateral, the cost of lending to firms whose debt was eligible dropped suddenly and substantially, in contrast to the cost of lending to firms whose debt was ineligible. We exploit

[1]Borrowing from the ECB in 2010-11 was available at short horizons (one week and three month maturities, although in October 2011 a special one year maturity program was implemented). Thus, the LTRO is best understood as a policy intervention aimed at banks' debt (value and structure), in contrast to policies aimed at recapitalizing banks by purchasing bank equity (or equity-like) securities, or bank assets (e.g., QE).

this exceptional policy change - the sudden appearance of a difference in the cost of lending to some bank borrowers (but not to others at the *same* bank) - to identify the causal effect of the LTRO program in France, the third largest recipient of LTRO funds after Spain and Italy.

Our main empirical design exploits the fact that in France the second LTRO was paired with a lowering (by one notch) of the minimum firm credit rating required for a firm's loans to be eligible as collateral for the ECB. This created a policy change with clear "treated" and "control" groups for a difference in differences research design: firms on either side of the new eligibility threshold. The groups are closely comparable and have very clear common trends in ex ante credit growth. We report several novel findings.

Firstly, we examine the effectiveness of the policy in supporting bank lending to firms, and to small and medium enterprises (SMEs) in particular, as they are well-known to bear the brunt of credit contractions (e.g., Galbraith, 1957; Gertler and Gilchrist, 1994; Khwaja and Mian, 2008; Chodorow-Reich, 2014). Around the world, such policies have been an area of major policy activism in recent years.[2] However, the existing evidence indicates that most schemes to support banks in times of financial stress are poorly transmitted to firms for a variety of reasons, such as liquidity hoarding (e.g., Allen et al., 2009; Caballero and Krishnamurthy, 2008; Heider et al., 2015), or because actual or potential fire sales of financial assets crowd out lending to firms (Shleifer and Vishny, 2010; Diamond and Rajan, 2011; Abbassi et al., 2016; Drechsler et al., 2016). Indeed, the evidence suggests that central-bank policies have been largely ineffective at inducing lending to firms (e.g., Iyer et al., 2014; Acharya et al., 2017; Chakraborty et al., 2019), or only of benefit to the largest firms (e.g., Andrade et al., 2018; Rodnyansky and Darmouni, 2017).[3]

In contrast to these results, we find that the fall in the cost of funding loans to eligible firms is rapidly transmitted, leading to an increase in the amount of bank credit to SMEs of 8-9 percentage points for newly-eligible firms (relative to controls), and 12 percentage points for the LTRO program as a whole (with a 9 percentage point increase for large firms). That is, a policy reducing banks' cost of funding loans to a subset of firms in a crisis period is effective in causing an increase in credit supply to such firms.

We then examine *how* policy-driven liquidity is passed through to firms, uncovering a

[2]Examples of policies operating via collateralized central bank lending include the Term Asset-Backed Securities Loan Facility (USA, in 2008), the Funding for Lending Scheme (UK, in 2012), and the Targeted Long-Term Refinancing Operations (Eurosystem, in 2014 and 2016). The Bank of Japan implemented a similar policy to the LTRO in 2009-10.

[3]The ECB itself viewed the LTRO policy as relatively ineffective: President Draghi noted that "several months have passed and we see that credit flows [...] remain weak." [ECB press conference, July 5th, 2012]

new and important dimension of heterogeneity: banks transmit LTRO liquidity differently to multi-bank firms than they do to firms with only one bank. Single-bank firms are especially well suited to relationship borrowing because their banks can access information on the full range of their bank interactions, and also face reduced concerns regarding borrower strategic default, or information externalities benefiting other banks. Accordingly, we find that differences in liquidity transmission map to archetypal lending types: single-bank firms receive credit with characteristics consistent with relationship lending, while multi-bank firms receive more transactions-style lending.[4] In turn, this affects which firms receive new loans, whether the funds are used for investment, the existence of spillover lending to ineligible firms, and ex post loan performance.

In short, banks respond to the policy by offering a different lending product to single-bank firms than they do to multi-bank firms, a difference that is crucial to understanding the channels and effects of liquidity-based UMP on bank borrowers. More generally, our results strongly suggest that the effects of bank shocks or Government policies on research samples of multi-bank firms - which make up the the vast majority of the extant literature - should not be directly extrapolated to the large population of single-bank firms, which appear to be differentially affected.[5] Using our estimates of the effect on lending to firms (and subject to numerous caveats), we calculate that, absent the LTRO policy, total credit to the firms in the affected credit rating categories would have decreased by at least 10%, whereas the actual decrease was only 4%. We now lay out our results in greater detail.

We first examine the policy effects on single-bank firms, as they are naturally most likely to be relationship borrowers, and have different pre-trends in lending to multi-bank firms. For single-bank firms, the LTRO-driven lending is largely composed of longer-term debt, an indicator of relationship lending (Mian, 2006; Sutherland, 2018) which is highly valued by firms because banks are only willing to provide such lending when they have good information regarding a firm's type: by lending at longer maturity banks delay their ability to exit the loan if the borrower becomes riskier.[6] Consistent with this observation, we find that LTRO-

[4]We refer to this lending as transactions-style lending throughout because it does not correlate with standard relationship measures, and in order to distinguish it from the archetypal relationship-based lending received by single-bank firms.

[5]Differential effects for single-bank firms, while largely unexplored by the empirical literature, are consistent with both the models of relationship lending (Sharpe, 1990; Rajan, 1992; von Thadden, 1995) and the evidence that such firms are unavoidably exposed to any shock affecting their lender, given the difficulty of quickly establishing a new bank relationship (e.g., Paravisini, 2008; Khwaja and Mian, 2008; Jiménez et al., 2012). An important contemporaneous paper, Degryse et al. (2018), also highlights the importance of including single-bank firms in estimating and evaluating the effects of bank shocks over the business cycle.

[6]Loan maturity falls during periods of economy-wide financial stress, making long-maturity loans espe-

driven lending is directed towards the strongest borrowers: firms with well-developed lending relationships - we find no policy impact for firms with weaker relationships - and to those with stronger observable characteristics, such as high asset tangibility. Eligible single-bank firms' leverage increases, but so do their interest coverage ratios. Furthermore, single-bank firms show strong ex post loan performance, and increase their investment in fixed assets (growing total assets), without increasing employment.

The effects on multi-bank firms provide a striking contrast, and are instead consistent with transaction lending. The policy-driven lending to these firms is short maturity, and banking relationships do not correlate with which firms receive additional lending. Further, firms with weaker observable characteristics also receive policy-induced lending, and interest coverage ratios decline, both suggesting that the lending standards being applied to multi-bank firms are less strict, likely because banks retain the option to cut their exposure due to the short horizon of the lending. Unlike single-bank firms, these firms do not increase their total assets or investment.

We also confirm the importance of policy design by showing that almost all the policy benefits are transmitted to eligible firms only. By tying the collateral eligibility of loans to specific loan characteristics, the policy directly increased lending to the selected groups. However, we do find strong evidence of a spillover onto a single group: ineligible firms in the credit rating category one notch below the eligibility threshold receive additional short-term credit, but only if they have multiple bank relationships. Deeper bank relationships do not appear to drive this spillover. Instead, our tests suggest that the competitive threat of losing a customer drives this lending: the spillover comes from the main banks of firms that have recently added a bank relationship, or have increased their borrowing from other banks. The absence of spillover lending for single-bank firms underlines the power of the "lock-in" effect of existing bank relationships in periods of economy-wide financial stress. We also provide suggestive evidence that, in our setting, the balance sheet strength of banks appears to have no material impact on liquidity transmission.

The LTRO policy change provides a unique window into which firms receive additional bank credit in times of aggregate financial stress. The banking literature typically makes use of bank-level shocks to provide identifying variation, together with a within-firm specification following Khwaja and Mian (2008) to control for demand and bank-firm matching. However, this influential empirical strategy has disadvantages, most especially in the setting of the

cially valuable: in the US between May 2007 and May 2010 the average weighted maturity of new C&I loans with a maturity of between 1 month and a year fell by over two thirds, while for loans with maturity over a year it fell by more than a quarter (US Survey of Terms of Business Lending).

LTRO: firstly, banks themselves choose how much LTRO lending they receive, making for an unusually acute endogeneity problem; secondly, the within-firm design necessarily drops single-bank firms from the sample. Instead, we employ an identification strategy at the firm-credit rating level (used within-bank, rather than across banks), allowing us to examine the effects of a change in the cost of bank funds on lending to all firms in the relevant credit rating categories, and within the same bank and month. This means we avoid the endogeneity of bank uptake of LTROs (and any other bank-specific shocks), and we are able to examine policy effects on multi-bank firms, and to contrast it with transmission to single-bank firms, which make up a large majority of firms in France, and employ 38% of the private sector workforce. Understanding credit access for single-bank firms (disproportionately small and young) when banks are under stress is crucial to our comprehension of changes in productivity and economic activity more broadly in these periods (e.g., Decker et al., 2014; Ates and Saffie, 2016).

2 Related Literature

This paper is directly related to the large literature on the role of banks in lending to firms, and to the banking relationship literature.[7] In particular, we provide a new perspective on the old idea that a strong bank-firm relationship leads to improved access to finance along multiple dimensions (see references in Boot, 2000), by showing that, in our setting, strong single-bank relationships give firms access to a different product: longer term relationship lending, as opposed to the short term transactions-type lending provided to multi-bank firms. In addition, we provide novel evidence on the role of competition on lending relationships, suggesting that banks' marginal credit recipients, even in a period of financial stress, are multi-bank firms that they risk losing to other banks, rather than the less mobile single-bank borrowers.

Several recent papers focusing on the role of relationship banks during recessions have found that they partially shield their borrowers.[8] However, these papers do not distinguish the different dynamics of lending to single-bank vs multi-bank firms during recessions, our main contribution. Our finding that single-bank borrowers only obtain additional funding if they have strong relationships and strong observable characteristics is consistent with the

[7] See Stiglitz and Weiss (1981), Fama (1985), Diamond (1991), and James (1987) for early work on the role of banks in lending to firms.

[8] For example, Sette and Gobbi (2015), Beck et al. (2018), and Liberti and Sturgess (2018) find protective results, although Jiménez et al. (2017) find no differential effect of lending relationships.

important model in Bolton et al. (2016): relationship banks provide continuation financing for their borrowers in crisis periods, but only if they are the high quality type. However, in our setting two features emerge as strong empirical markers for relationship lending: having only one banking relationship, and long-maturity lending. These features are not included in the Bolton et al. (2016) model, potentially because they focus on multi-bank firms.

More generally, this paper relates to the vast literature on the bank-lending channel (Bernanke, 1983; Stein, 1998) which tracks the transmission of financial conditions and constraints on banks to their borrowers. Extensive evidence supports the view that banks pass on monetary policy tightening (Kashyap et al., 1993; Kashyap et al., 1994; Kashyap and Stein, 2000; Jiménez et al., 2012) and unexpected liquidity shocks (Peek and Rosengren, 2000; Khwaja and Mian, 2008; Chava and Purnanandam, 2011; Schnabl, 2012) to their borrowers. Much less is known about adjustments to positive liquidity shocks and in particular, how effects differ across firms, the focus of this paper.

We also contribute to the literature on the effects of changes in the collateral value of assets over the business cycle (e.g., Fostel and Geanakoplos, 2008). The limited empirical evidence on the effects of increases in collateral values in crises is focused on changes to traded securities (Ashcraft et al., 2011; Garleanu and Pedersen, 2011); we examine the effects of increases in the collateral value of banks' non-traded assets, a major portfolio component.[9]

This paper also relates to the growing literature on the effects of other unconventional monetary policies - especially large scale asset purchase programs - and particularly those papers' focus on how the policy designs affected targeted borrowers, and spilled over onto non-targeted borrowers (Krishnamurthy and Vissing-Jorgensen, 2013; Di Maggio et al., 2016; Rodnyansky and Darmouni, 2017; Luck and Zimmermann, 2017; Chakraborty et al., 2019).

Three related papers examine the broad question of the impact of the LTROs on bank lending to firms: Garcia-Posada and Marchetti (2016) for Spain, Andrade et al. (2018) for France, and a contemporaneous paper by Carpinelli and Crosignani (2018) for Italy.[10] Their identification strategies differ importantly from ours in that they make use of the LTRO

[9]During crises, assets' collateral values (i.e., the extent to which the asset finances itself) fall, often sharply (Allen, 2009), putting pressure on bank capital and leading banks to reallocate away from sectors (such as lending to firms), which have low collateral values and higher regulatory capital costs. This is consistent with the shift away from lending to firms, especially small firms (e.g., Fort et al., 2013; Deyoung et al., 2015) during such times.

[10]A paper subsequent to ours, Mésonnier et al. (2017), examines the effects of the ACC component of the LTRO-ACC policy on loan interest rates in France using survey data, focusing on bank heterogeneity. They report a robust but small drop in new loan rates in response to the policy shock (of 7bp as compared to average lending rates in their sample of around 250bp), which is comparable to the 3bp drop we find using accounting data.

as a cross-bank shock to liquidity (and examine multi-bank firms only), as opposed to our within-bank focus that exploits the fact that not all firms' loans were eligible as collateral.[11] As a result, we reach novel conclusions in the areas where our research questions overlap. Most notably, we find the LTROs generated substantial additional lending to SMEs - an important policy target; we find that relationships are central to policy transmission; and we find that the eligibility-based design of the LTRO largely determines which firms receive additional lending. We emphasize, however, that these differences are to be expected, given that (i) we make use of different identifying variation (and so examine differing segments of the firm distribution); (ii) these studies do not all study the same countries; and (iii) each paper focuses on different aspects of the policy's impact.

Finally, we contribute to the debate over whether unconventional ECB policies have undesired effects on bank risk taking via the quality of lending. The LTROs have the potential to induce risky lending by simply reducing funding costs (Jiménez et al., 2014), or by leading banks to over-produce collateralizable assets (Van Bekkum et al., 2017; Nyborg, 2017). Similarly, Acharya et al. (2018) argues that the ECB's Outright Monetary Transactions (OMT) intervention in summer 2012 led to "zombie" lending by banks. We find no clear evidence of low quality lending by French banks induced by the LTROs.

3 The Unconventional Monetary Policy we examine: a change in Eurosystem collateral policy

3.1 The Eurosystem collateral framework

All borrowing by private banks from the Eurosystem requires banks to provide eligible collateral, which include traded securities as well as bank assets such as loans to high credit-quality firms, known as eligible "credit claims." Since October 2008 there has been no limit on how much a bank may borrow from the Eurosystem (known as "full allotment") if the borrower provides sufficient eligible collateral. Collateral pledged to the Eurosystem is placed in each bank's collateral pool (i.e., each collateral item is not tied to a specific loan from the Eurosystem) and its eligibility is assessed daily - for France the Banque de France has an automated

[11] Carpinelli and Crosignani (2018) takes a further identification step beyond the other papers by using an Italian Government scheme to generate additional eligible collateral in order to separate banks using the LTRO to restore borrowing, from those that were simply taking advantage of the subsidy. Their focus is on cross-bank heterogeneity of the effect and the use of LTRO funds by banks for securities purchases. See section 5.4 for more on how our empirical approach differs.

platform for easy pool management.[12]

If an asset becomes ineligible (e.g., if a firm defaults on a bank loan, or is downgraded) the borrowing bank must immediately remove it from the pool and replace it with eligible collateral. Thus, the only scenario in which the Eurosystem would bear default risk on this lending would be if the borrowing bank itself defaulted, and had insufficient assets to cover its borrowing after collateral was valued. This structure implies that, unless the private bank is close to default and does not expect to be rescued, the collateral system does not incentivize banks to make or pledge negative NPV loans, because they bear the full expected losses.

In 2011 banks throughout the Eurozone were very likely to have been collateral constrained (Barthélemy et al., 2017), because the constraint can bind for individual banks even when, on aggregate, banks appear to have ample free collateral. Moreover, apparent over-collateralization also occurs because this same collateral pool is also used for intraday payments, both intra- and internationally, and this additional use is generally not considered when banks' collateral constrainedness is measured.

3.2 The policy change: LTROs and Additional Credit Claims

In the second semester of 2011 French banks came under severe funding stress during the "Quiet Run of 2011" (Chernenko and Sunderam, 2014), due to their exposures to Eurozone periphery sovereign debt and subsequent withdrawal of over US$100 billion in funding from the ten largest US money market funds - the largest drop in both percentage and absolute terms across the Eurozone in this period (IMF, 2013). Figure 1 illustrates the dramatic drop in listed French banks' stock prices that occurred in the second semester of 2011: the stock prices of Crédit Agricole and Société Générale, two of the largest French bank groups, fell by over 50 percent in the second semester. This was reflected in their debt funding costs also: the top panel of Figure 2 displays the cost of market debt from Gilchrist and Mojon (2017). Towards the end of 2011 bank marginal funding costs were approximately as high as they were at the peak of the US financial crisis. In response, on December 8th the ECB announced a package of unconventional monetary policy measures, consisting most notably of two "long-term refinancing operations" (LTROs) with three year maturities, and the possible lowering of the rating requirement for some bank assets to be eligible for posting

[12]For more detail on the Eurosystem collateral framework see Bindseil et al., 2017.

as collateral at the ECB.[13]

The package provided substantially-below-market-rate funding to participating Eurozone banks, allowing them to borrow amounts limited only by their available collateral, at the (low) main refinancing rate, and, crucially, at much longer maturity - the LTROs' three year maturities were unprecedented for the ECB, which regularly lent at weekly and three month maturities only.[14] Thus, the main liquidity channel used by the ECB was temporarily switched from providing only short-term liquidity to providing three year liquidity, without any increase in cost to borrowers. Further, more of banks' existing loans to firms were made eligible collateral, via what was termed the Additional Credit Claim (ACC) framework, which was especially valuable because these loans - assets of the bank - were unusable as collateral in any other contexts.

The LTROs provided massive amounts of collateralized liquidity. The first took place on December 21st, 2011, and provided €489 billion to 523 banks across the Eurozone, while the second (on February 29th, 2012) provided €530 billion to 800 banks; in total, banks in France received at least €153 billion via this LTRO mechanism (Andrade et al., 2018).

This paper exploits both the LTROs' substantial easing of banks' general liquidity constraints and the lowering of credit standards for eligible collateral (the ACC) to provide plausibly exogenous variation in the cost of bank funding of loans to some firms. The ECB announcement was largely unexpected, most especially the ACC framework, and the ECB left decision of whether and how to participate in the ACC to each country's National Central Bank.

Until February 2012, the firms receiving the bank loans being posted as collateral had to be rated 4+ or higher in the Banque de France's credit rating scale to be eligible as collateral.[15] On February 9th 2012 (i.e., before the second LTRO), the ECB approved the implementation criteria proposed by seven national central banks, including the Banque de France. This was the first public acknowledgment that the Banque de France had chosen to implement a reduction in the minimum credit quality of collateral-eligible loans, and also provided the crucial details that it had lowered the minimum eligible credit rating by one

[13]The LTROs provided the option to repay after one year, and were fixed rate, with the rate paid at the end and fixed at the average main refinancing operations rate over the life of loan. The rating requirement for residential mortgage backed securities to be eligible collateral was also lowered, and banks' minimum reserve ratio was lowered from 2% to 1%. These measures reinforce the overall effect of reducing banks' liquidity constraints, but we do not exploit these features in our empirical design.

[14]In 2009 the ECB first implemented LTROs with a one year maturity. One year lending was not provided again until a one year LTRO was announced on 6 October 2011.

[15]The Banque de France assigns credit ratings to (private and public) French non-financial companies with a minimum turnover of €0.75 million; more details are provided in section 4.

notch, from 4+ to 4, and that it applied to loans of all sizes.[16] Figure 3 illustrates the change in eligible ratings.

Thus, the possibility of Eurozone members' participation in the ACC framework was announced together with the LTROs, and France's participation was announced two months later, between the first and the second LTRO. However, it is important to note that these are not separate policy shocks: the newly-eligible ACC credit claims were used as collateral for the LTROs. Thus, we refer to the policy as the LTRO-ACC.

3.3 Estimating the effect of the LTRO-ACC policy on bank funding costs

Credit claims made up 36% of the €413 billion of collateral pledged with the Banque de France by 54 banks at the end of 2011. In France, the ACC made available an *additional* pool of corporate credit claim collateral of about €90 billion after haircuts, which according to Bignon et al. (2016) corresponds to a collateral shock for French banks of 4.8% to 15.1% of their drawn loans.[17]

An estimate for the size of the fall in the marginal cost of funding for French banks at the time the LTRO-ACC program was announced is the spread between the cost of market debt for these banks, and the ECB main refinancing rate at which they could obtain financing using the newly-eligible collateral.[18] The bottom panel of Figure 2 displays the cost of market debt from Gilchrist and Mojon (2017) and the ECB's main refinancing operation (MRO) rate, which is the rate paid by banks borrowing in the LTROs. Bank marginal funding costs rose throughout 2011, but this rise greatly intensified in the second semester of 2011. The cost of market funding reached about 5.2% on average in the last quarter of 2011, whereas the main refinancing rate was 1% at the end of the year, making the spread over 400 basis points.[19]

[16] To our knowledge France was the only large Eurozone economy that implemented the ACC at this time without imposing a minimum size requirement on the loans which were newly eligible as collateral.

[17] In practice, the use of the ACC-rated loans in France as pledged Eurosystem collateral totaled €9 billion, while for firms rated above ACC it stood at €126 billion as of June 2012, with both totals being *after haircuts*. Haircuts on eligible credit claims range from 17% to 70% depending on loan maturity and rating, making the total value of the underlying loans correspondingly larger.

[18] Note that in times of stress, the price in the overnight market may be a poor proxy for banks' cost of funding as interbank markets become dysfunctional (Frutos et al., 2016).

[19] This is an approximation, as estimating the true marginal cost of market funding is challenging. Firstly, the maturity of borrowing from the Eurosystem will likely be lower than that of the Gilchrist and Mojon (2017) data, which is a weighted average of different bond maturities. Secondly, market rates reflect rates for partly unsecured lending, while the ECB refinancing rate is fully secured. Finally, the banks in the market

However, over the course of 2012 the spread between the cost of market debt and the MRO rate fell in response to the massive injections of liquidity by the ECB; by the end of 2012 it seems clear that the advantage of the LTRO-ACC in terms of below-market-cost funding had largely disappeared (although listed bank equity did not recover until mid to late 2013).

4 Data and Summary Statistics

4.1 Sample composition

The sample we use results from merging the French national credit register, the FIBEN financial statement database, and the credit rating database, all from the Banque de France (BdF). These administrative data contain close to the universe of firms in some cases, but merging the datasets, the nature of the policy, and the removal of potentially confounding sub-populations result in a much smaller sample, which includes - for the part of the credit rating distribution we examine - most non-business group Small and Medium Size Enterprises (SMEs) in France.

We focus our attention on SMEs so as to examine changes in credit availability for the most numerous, but also most financially constrained firms (Galbraith, 1957; Gertler and Gilchrist, 1994; Chodorow-Reich, 2014). In particular, Khwaja and Mian (2008) and subsequent papers have shown that large firms can find substitutes for bank lending when their banks experience negative shocks, but that smaller firms cannot. While loans to large firms were not excluded from the policy, large firms in our data are generally part of business groups rather than standalone corporations, and we lack consolidated balance sheet data. We consolidate the data ourselves and examine the effects of the policy on large firms in section 9, but limitations on the accuracy of consolidation (discussed below), together with the fact that large firms are almost all multi-bank firms, mean that large firms are not the focus of the paper.

4.1.1 Firms in the sample

We restrict the sample to independent SMEs in order to exclude confounding effects that occur if firms are part of business groups. Doing otherwise would likely generate bias, because

data are likely riskier than the average bank, as they were sufficiently constrained that they issued expensive market debt.

such firms can obtain credit as a result of their holding company's balance sheet (as opposed to their own), via implicit or explicit guarantees. Using BdF ownership data we identify independent SMEs as firms that are not majority owned by another firm.[20]

We also exclude micro-firms, as they are extremely heterogeneous and are often non-standard, reducing external validity.[21] We follow standard practice with this data and drop firms in the agriculture, financial, utility and public sectors. Finally, we keep firms with standard limited liability legal form (i.e., SA and SARL firms).

4.1.2 Firm accounting data

Accounting data mainly consists of firm balance sheets from the BdF's FIBEN database, which is compiled from tax returns. It includes all French firms with sales of €750,000 or greater.[22] We drop firms with negative debt, negative or zero total assets, or missing number of employees. All firm characteristics are winsorized at the 0.5 and 99.5 percentile.

4.1.3 Firm credit rating data

BdF assigns credit ratings to all French non-financial companies, public or private, that have FIBEN accounting data. The rating is an assessment of firms' ability to meet their financial commitments over a three-year horizon. The main use of the ratings is to determine the eligibility of bank loans to rated firms as collateral for Eurosystem funding.[23] Ratings are based on firms' accounting statements, information on failures to pay trade bills (described later in this section), bank loans reported by credit institutions, and legal information, as well as other sources including soft information from local BdF analysts. The latter review ratings whenever a significant new development occurs, as well as on receipt of firms' yearly financial statements.

[20] We do not have consolidated balance sheet data for most business groups. While we are able to combine the firms in a business group into a synthetic single firm by summing their accounting and debt totals, we cannot remove intra-group transfers and transactions, and we cannot infer when a firm that is not majority-owned by a group is in fact controlled by the group. This makes accurate consolidation impossible. Perhaps more importantly, credit ratings (which are central to our empirical design) exist for each firm in the group, and there is no clean way to aggregate ratings across firms.

[21] We use the definition employed by the European Commission and France for both SMEs and micro-firms. SMEs have fewer than 250 employees, and annual sales or total assets of less than €50 million or €43 million respectively. Micro-firms have fewer than 10 employees, and sales and total assets below €2 million.

[22] In 2004, FIBEN covered 80% of the firms with 20 to 500 employees, and 98% of those employing more than 500 employees.

[23] Banks were permitted to use alternative ratings systems to determine loans' collateral eligibility (subject to additional requirements), but internal BdF data indicate that 96% of banks were using the BdF ratings for Eurosystem collateral assessment as of December 2011.

The BdF receives no payments from rated companies. It always informs companies of their rating, and while the rating is not public, banks may access the ratings and make use of them. Thus, BdF has no obvious incentives to inflate firms' ratings, and firms have no leverage over the ratings process. Firms that do not provide financials cannot be rated, and thus lose eligibility as collateral for the Eurosystem, so we remove firms with inactive ratings data from the sample.

The rating scale contains twelve ordered notches from 3++ (safest), to P (in bankruptcy) - see Figure 3. A rating of 4+ is approximately equivalent to a long-term rating of BBB-/Baa3 from S&P/Moody's (i.e., just above the investment grade threshold). The main treatment group is firms rated 4, and the control group is one notch below (5+). A rating of 4 corresponds to a 1% probability of default at a 1-year horizon. Firms in the 4+, 4 (ACC), and 5+ rating categories represent about 50% of the total sample of SMEs with an active credit rating as of December 2011, with 22.1% having a rating of 4 (ACC), and 12.6% a rating of 5+.

4.1.4 Firm-bank credit registry data

Monthly firm-bank debt data comes from the French national Central Credit Register (CCR), which captures the total exposure (i.e. the sum of all credit of any kind) of a credit institution to a firm that exceeds €25,000.[24] Because we drop micro-firms and thus only have firms with sales exceeding €2 million we do not view this extreme left-tail truncation as a consequential data constraint.

We aggregate credit exposures at the level of banking groups (henceforth referred to as a "banks"), and identify the main lender of each firm as the bank with the largest average share of drawn credit in 2011. We then aggregate exposures across banks for each firm, since we are interested in the overall effect of the policy at the firm level, as opposed to the firm–bank level. Indeed, firms with multiple bank relationships may react by adjusting their sources of financing, such that firm–bank level effects are not representative of the aggregate effect (Jiménez et al., 2014).

We also require banking groups to be present in the sample each month, limiting attrition due to bankruptcy, restructuring or mergers.[25] Finally, an implicit requirement of a valid

[24]The reporting threshold on credit data applied at the branch level until April 2012, and at the bank level afterwards. Following Andrade et al. (2018), we drop all branch-firm links with a total exposure below €25,000 and then collapse this database to the bank-firm level.

[25]Dexia bank entered a resolution process in October 2011, after reporting large losses. To avoid potential effects on our estimates resulting from the disappearance of a bank, we drop all firms that had credit from

difference in differences strategy is that firms must be present before and after the LTRO policy. Thus, our main sample spans a two year period centered on the announcement date (i.e., between March 2011 and February 2013) and we require firms to maintain a bank relationship throughout.

4.1.5 Data on defaults on suppliers

The fourth dataset collects defaults on commercial paper intermediated by French banks (i.e., intermediated debts to suppliers), including all firms with a credit rating. Thus, for each incident, the database contains the involved parties, the date of default, the amount, and the default motive.

4.2 Examining the sample

Table 1 reports sample summary statistics for our main sample, with single-bank firms in the upper, and multi-bank in the lower panel. Each panel compares the treatment group, 4-rated/ACC firms, to the main control group, 5+ (one notch down from ACC) for single-bank and 5 (two notches down) for multi-bank. We use 5 rated firms as the main control group for multi-bank firms because of the evidence presented in later tables that 5+ multi-bank firms appear to receive treatment despite being formally ineligible (i.e., they receive spillover lending), making them poor controls (although we use them as controls in Table 2 for transparency).

We classify firms as single-bank if they borrow from only one bank throughout 2011, and multi-bank if they borrow from more than one bank for at least one month in 2011. A total of 23 banking groups or standalone banks appear in our sample in 2011 for single-bank firms, with 34 banks in the multi-bank subsample. Bank market share of corporate credit is very concentrated: 8 banks provide 96% of credit in 2011 in our sample.

Due to data coverage and the constraints imposed by the policy we examine, the SMEs in our sample are slightly larger (average assets of around €2 million) and older than typical SME samples. As is usual for SMEs, bank loans remain their main external financial source: fewer than 1% of the firms in our sample have access to the bond market. In 2008, 83% of the population of French firms had a single-bank relationship, and this is highly correlated with firm size: 86% of micro firms, 39% of SMEs and 21% of large firms had only one bank relationship (Aleksanyan et al., 2010). A large proportion credit registry samples are

Dexia or from banking entities in its group at any point in the period January 2010-December 2014.

dominated by single-bank firms (e.g., in Pakistan, 90% of firms are single-bank in Khwaja and Mian, 2008), and the prevalence of such firms is a feature of research datasets from the US, Germany, Chile, and Portugal, among others. However, in our sample we have broadly equal numbers of single and multi-bank firms because we require firms to have credit ratings near investment grade and we drop micro-firms, making for a relatively high minimum firm size.

4.2.1 Single-bank sample

The upper panel of Table 1 compares single-bank ACC and 5+ rated firms. The two groups show similarities in terms of assets (total and fixed), profitability and apparent cost of debt (total financing costs divided by total debt). Moderately-sized differences also emerge, such as for headcount (15% higher for 5+ firms), and proportion of short term debt used (5+ firms use 19% less), while there are larger differences on age (5+ firms are 25% younger) and investment (where 5+ firms invest double the amount into fixed assets). The remaining differences follow differences in the firms' credit ratings: 5+ firms have much higher debt and leverage, and much lower interest coverage ratios. These differences highlight the need for firm and industry fixed effects in our empirical specification, as well as a careful examination of pre-trends in credit growth, as shown in Figures 4 and 5, and discussed later.

4.2.2 Multi-bank sample

Comparing multi-bank ACC and 5 rated firms in the lower panel of Table 1 we see that they are broadly similar in terms of size (assets, employees), age, length of main bank relationship, apparent cost of debt and investment. However, Table 1 shows some clear differences in variables related to credit ratings. In particular, debt and leverage (around 20% larger for 5 rated firms); dependence on short-term debt (38% more for 5 rated firms); fixed assets as a proportion of total assets (15% more for 5 rated firms); and flow items such as asset growth, profitability, and interest coverage ratio.

4.2.3 Comparing single and multi-bank firms

Firms tend to begin life with a single-bank lender and acquire more lenders as they grow (Petersen and Rajan, 1994; Farinha and Santos, 2002), which is consistent with the evidence in Table 1 that single-bank firms are on average smaller in terms of assets and employees.[26]

[26]Extant explanations for why firms develop multiple bank relationships include: bank specialization (Paravisini et al., 2017); reducing bank information rents (Rajan, 1992; Sharpe, 1990); reduced exposure to

This is corroborated by unreported cross-sectional regressions comparing the two groups of firms in June 2011, and including a wide variety of firm characteristics as controls. These regressions also show that multi-bank firms are more profitable on average, consistent with the idea that faster growing firms are more likely to become multi-bank earlier, to minimize hold up and liquidity risks (Ongena and Smith, 2001).

The most striking difference that emerges from these comparisons is that single-bank firms have substantially more medium/long term bank debt than multi-bank firms, as is also clear in Table 1. No differences across single and multi-bank firms in other variables (such as leverage, interest coverage, or asset tangibility) are consistently statistically significant in these cross sectional regressions.

5 Empirical strategy

5.1 Empirical design

As illustrated by Figure 3, our empirical strategy exploits the fact that, together with a major relaxation of banks' overall liquidity constraint, the introduction of the ACC framework reduced the costs to banks of lending to firms rated 4 (also referred to as ACC-rated firms) — by making loans to these firms eligible as ECB collateral— but not to other firms at the same bank that are closely comparable (firms rated 5+, one notch below). Thus, the firms rated 5+, whose loans were just-ineligible as collateral, are our main control group in a difference in differences research design for the impact of the program on firm-level outcomes.

We do not use the firms rated above the ACC group (eligible both before and after) as controls because they were simultaneously subject to a large positive shock to their value as collateral; i.e., they were themselves "treated", and at a higher treatment intensity than the ACC loans. This is because the LTROs in December 2011 and February 2012 induced a large increase in bank borrowing from the Eurosystem, which generated a corresponding increase in banks' need for ECB-eligible collateral. Loans to firms rated above ACC were even more attractive as collateral for LTRO borrowing than loans to newly-eligible firms, because the higher-rated loans had lower haircuts, and as a result we see loans to higher rated firms increase by more (see Figure 9), as described in section 9.

We assign firms to treatment and control groups based on the rating of each firm as of December 2011, the month in which the LTROs and the possibility of the ACC change was

bank-specific shocks (Detragiache et al., 2000); and improved financing terms resulting from higher ex post debt renegotiation costs (Bolton and Scharfstein, 1996).

announced, but when its specifics and ECB approval were unknown: recall participation in the ACC, and which rating categories were newly-eligible, were announced and implemented in February 2012. We drop observations in January and February 2012.[27] We estimate an Intention-To-Treat (ITT) effect because firms' ratings make their loans eligible or ineligible for treatment, but we do not observe which firms are actually treated (i.e., whose loans are pledged as collateral). Self-selection by firms into eligibility can be ruled out, because ratings are assigned by the Banque de France, as described in section 4.

5.2 Specification

Our main dependent variable (g_{ft}) measures the change in firms' total debt; we follow Amiti and Weinstein (2017) and use a percentage change measure relative to a base period, which they argue has superior properties to a natural log transformation in this setting. Hence, g_{ft} is the percentage change in the firm's total bank debt, relative to the firm's 2011 average, and it is summed across all banks when the firm has multiple banks:

$$g_{ft} = (\sum_{b} Debt_{fbt} \ / \ \overline{Debt}_{f2011}) \quad - 1 \tag{1}$$

Where $Debt_{fbt}$ is the outstanding amount of outstanding bank debt (short-term plus long-term bank loans) in month t, for firm f, borrowed from bank b, and $\overline{Debt}_{f2011}$ is the 2011 average for firm f of its total outstanding bank debt, summed across all its banks.[28] Appendix Table 3 provides results for alternative base periods; results are unaffected. To mitigate the effect of outliers and especially to reduce the weight given to firms with low levels of debt in 2011 we top-winsorize g_{ft} at 2% (our estimates are robust, and larger, if we do not do this).

We estimate a difference in differences model of the form:

[27] Results are unaffected if we define treatment and control groups based on September, October, or November 2011 firm credit ratings, as shown in Appendix Table 14. They are also not sensitive to including January and February 2012 observations. Firm rating varies over time as firms are downgraded or upgraded, generating mixing in our treatment and control groups both in the pre and post-treatment periods. The pattern of mixing suggests a downward bias to our estimates: control firms are more likely to be upgraded (i.e. into the treatment group) than treated firms, while the latter are more likely to be downgraded (into the control group).

[28] To ensure that the results are not driven by firms with low levels of debt, and to explore the intensive margin of the effect we also examine leverage as a dependent variable. Following Amiti and Weinstein (2017), which reports that only firms with substantial bank leverage (over 14 percent of assets in their Japanese sample) are sensitive to lender supply shocks, we restrict this separate sample to firms with at least 5% leverage.

$$g_{ft} = \alpha_f + \beta(ACC_f \times Post_t) + \Lambda_{bt} + \Upsilon_{It} + \Gamma' X_{f,y-1} + \epsilon_{ft} \quad (2)$$

where f indexes firm, I indexes industry, b indexes banks, t denotes months, and y fiscal year.[29] ACC_f is an indicator that takes a value of one for any firm with a rating of 4 as of December 2011 and zero otherwise, thus identifying the newly-eligible firms that make up the treated group. *Post* is a post-treatment indicator equal to 1 in each month after February 2012, when loans to ACC-rated firms were eligible for as LTRO collateral. The parameter of interest is β, the ITT effect on newly-eligible borrowers of reduced bank funding costs induced by the LTRO-ACC. We cluster standard errors at the firm level to address serial correlation; results are fully robust to double-clustering by firm and bank-month (see Table 3).[30]

We include an extensive set of fixed effects. Firm fixed effects (α_f) remove average differences in debt growth across firms, and so absorb unobserved, time-invariant firm characteristics that affect credit. Though risk or investment opportunities may vary over time, our estimation window is limited to two years, centered around the policy introduction, mitigating the impact of time varying firm-level factors, and we also include controls for standard time-varying firm-level determinants of credit ($X_{f,y-1}$): ln(total assets), tangible assets over total assets, and profitability (EBITDA/total assets), 0.5% winsorized at both tails. Industry–quarter (Υ_{It}) fixed effects absorb any industry-level shocks.

Because the policy we exploit for identification varies at the firm credit-rating level (as opposed to the bank-level variation provided by shocks to banks), we are able to include a full set of bank–month fixed effects (Λ_{bt}). These serve to absorb both observed and unobserved time-varying bank heterogeneity. Crucially, bank-month fixed effects absorb any differences *between* banks in terms of LTRO uptake or usage across clients, an endogenous choice by banks likely based on variables that are not observable to the econometrician, and thus which cannot be fully controlled for using existing data from bank balance sheets or supervisory data. Further, bank–month fixed effects also remove other bank credit supply shocks, such as differences in bank responses to the ECB announcement of outright monetary transactions (OMTs) in August 2012.

In short, our baseline specification compares debt growth for firms borrowing *from the*

[29] For clarity, and because the regression is at the firm-month level, we omit the b (bank), I (industry) and y (year) subscripts for the dependent and error variables; bank and industry are used only in fixed effects; y is used for covariates only available at a yearly frequency. We use the t subscript for the industry by quarter fixed effect Υ_{It} for notational simplicity.

[30] A two-period difference in difference specification generates very similar estimates and standard errors.

same bank, with credit ratings *one notch apart*, *in the same month*, dramatically reducing the scope for confounding variation to affect our estimates. Of course, because of the difference in differences design, any estimate of LTRO effects we obtain necessarily excludes the (likely material) effects of the policy in relaxing the bank-level liquidity constraint, to the extent that it affects treatment and control firms equally.[31]

To examine the dynamics of the effect we use a specification that interacts the ACC indicator with indicators for each quarter (q) instead of with *Post* (omitting the first two quarters of 2010), providing noisier but finer-grained estimates of the ACC effect over time:

$$g_{fq} = \alpha_f + \sum_{q>2010\text{Q2}} \beta_q \left(ACC_f \times Qtr_q\right) + \Lambda_{bq} + \Upsilon_{Iq} + \Gamma' X_{f,y-1} + \epsilon_{fq} \tag{3}$$

5.3 Main identification assumption: no differential pre-trends

We focus on the difference in firm-level debt growth between newly eligible and ineligible firms from the nearby credit rating category. Our main identification assumption is that credit trends of the treatment and control groups would have been identical in the absence of the LTRO policy. Figure 4 shows the monthly average growth rates in debt for treated and untreated firms (with no controls), and Figure 6 displays quarterly β estimates from equation 3. Control firms look very similar to treatment firms in terms of their debt growth rate prior to the policy. We further test the parallel pre-trend assumption in unreported regressions covering the two years prior to December 2011: we interact the treatment group indicator with a time trend and find no evidence that pre-trends in debt growth differ between groups.

[31] One might be concerned that we do not fully control for firm credit demand using firm-time fixed effects as in Khwaja and Mian (2008). However, to believe that our estimates might be biased due to differential demand shocks, one would have to believe that (i) these demand shocks are systematically stronger for either the treatment or control credit rating group, (ii) that these shocks occur at the same time as the LTRO policy; and (iii) that these differential shocks' ability to bias our estimates survives the inclusion of bank-month and industry-quarter fixed effects, firm fixed effects over a short time window, and time-varying firm controls. Further, our treatment and control groups are adjacent or nearby credit rating categories that display very parallel trends at a monthly frequency. Finally, the negative shock to banks that drove the policy response worked through French bank holdings of non-core Eurozone sovereign debt, that is, a foreign financial shock, rather than a real domestic shock that might naturally reduce credit demand. Thus, in our view, it is hard to develop a story that would generate a bias in our estimates due to differential demand across treatment and control groups. However, we provide additional demand controls based on interacted fixed effects in Appendix Table 3, and describe them in section 6.1.3.

5.4 Comparing shocks to banks with the credit-rating-level LTRO policy shock

Our main empirical challenge is to isolate the credit supply effect of the LTRO-ACC program from other potential supply effects, as well as credit demand and macroeconomic effects. The empirical banking literature typically uses bank-level shocks for identification of causal effects, which has resulted in important advances in our understanding of bank and borrower behavior.[32]

Normally, concerns that more-exposed banks are different to less-exposed banks in important ways (e.g., an omitted bank characteristic determining both bank exposure and response to the shock) are addressed using bank-level controls.[33] In our setting this strategy is less effective, because unlike most bank shocks (which affect banks to different degrees that are not chosen contemporaneously by the banks themselves) in this case banks each choose how much subsidized LTRO lending they receive, subject to available collateral.

A second challenge with using bank-level shocks is that banks more affected by the shock may match to firms that are systematically different to those at less affected banks (i.e., some response-relevant but omitted firm characteristic is correlated with bank exposure via matching). To address this concern, the banking literature routinely uses the firm-time fixed effects specification (Gan, 2007; Khwaja and Mian, 2008; Paravisini, 2008).[34] Unfortunately, this method means that only multi-bank firms remain in the sample.[35] However, as we show, single-bank firms are both important in their own right, and are subject to different policy responses from their banks.

In contrast to the main body of the literature, our paper exploits a policy-induced positive supply shock generated by the deteriorating value of foreign sovereign debt, and which varies at the firm-credit-rating level instead of at the bank level. This empirical strategy has several

[32] Notable exceptions to empirical strategies based around specific bank shocks are Amiti and Weinstein (2017) and Degryse et al. (2018).

[33] While bank-level controls are valuable, they may not be a full solution if bank data becomes more opaque - and thus observables omit more information - in periods of stress (see Flannery et al., 2013), leading the empirical frontier to adopt an Altonji et al. (2005)-style strategy (e.g., Irani et al., 2018; Jiménez et al., 2017).

[34] The standard firm-time fixed effects empirical strategy assumes that firm credit demand is not bank-specific; however, Paravisini et al. (2017) argues that bank specialization often generates bank-specific demand. Recently, careful empirical design, often together with more granular fixed effects, have been used to sidestep this problem in some settings (e.g., Ippolito et al., 2016; Paravisini et al., 2014).

[35] Important but rare exceptions are papers using data on credit applications (e.g., Jiménez et al., 2012), which require only that firms *apply* to more than one bank, and papers that can exploit specific policy features to identify local average treatment effects using a regression discontinuity design (e.g., Jiménez et al., 2018).

virtues. First, the economic interpretation is direct, because banks' response to the (positive) shock likely reflects their normal adjustment process to a (large) change in the cost of funds in recession periods, in contrast to the disordered reaction of banks to emergency conditions generated by large negative and unexpected liquidity shocks. Second, our within-bank-and-month estimation strategy deliberately absorbs cross-bank heterogeneity, eliminating concerns about biases stemming from the endogenous bank decision of how much LTRO funding to receive (and also from any bank-specific shocks). Finally, our strategy allows us to examine policy effects on the large population of single-bank firms, as well as on multi-bank firms.

6 Results

6.1 Effects of the LTRO-ACC collateral policy

6.1.1 Effects on leading to firms: graphical evidence

Figure 4 shows the average growth rate in debt around the LTRO-ACC policy for newly-eligible firms (ACC firms, rated 4) and for firms one rating notch below (ineligible firms rated 5+), with separate plots for single and multi-bank firms. Firstly, note that the treatment and control groups follow parallel trends prior to policy implementation, a requirement for valid difference in differences inference. The plots provide unusually strong evidence for parallel (or near-identical) trends, because these lines are drawn prior to including controls, and show the data at high (monthly) frequency for the two years before the policy introduction. The difference between the dashed line (ineligible control firms) and the solid line (newly-eligible treated firms) widens markedly after the LTRO-ACC policy was implemented.

We plot the same figure separately for single-bank firms and multi-bank firms to illustrate that these two groups have different responses to the policy change. The ACC group for both single and multi-bank firms sees its debt rise concurrent with the timing of the policy implementation, but the effect appears much stronger for single-bank ACC firms relative to the counterfactual suggested by the line for the closest ineligible firms.

The small differential effect between eligible and ineligible firms in the multi-bank plot occurs because the ineligible group also sees its credit increase markedly in 2012, strongly suggesting that for multi-bank firms there is a positive policy spillover onto this group of ineligible firms. We present regression evidence for this in subsequent sections, but Figure 5 adds a plot of the next credit rating notch below 5+ (firms rated 5) to the multi-bank graph,

to show that for 5 rated firms there is no positive effect of the policy - the line is essentially flat. This suggests that the relevant counterfactual for multi-bank firms is captured by the 5 group, 2 notches below the ACC firms, and that the 5+ firms are also being "treated" by the policy to some extent. We will use both the 5+ and the 5 rated firms as controls for the multi-bank firms in our empirical specifications.

By contrast, the flat or declining trend for ineligible, 5+ rated single-bank firms in the top panel suggests there is no positive spillover effect for single-bank firms, a result reminiscent of the "flypaper" effect reported by Di Maggio et al. (2016) for the asset purchases of the Federal Reserve's QE1, where only the securities purchased (and their underlying mortgages) reflected any policy effects. However, unlike the latter paper we observe both spillover and flypaper effects at the same time, in response to the same policy (and within the same bank). What drives which firms obtain a spillover effect in our setting is the number of banks the borrower has: single-bank borrowers do not receive spillover effects from the LTRO-ACC policy, unlike multi-bank firms. We explore these ideas further in our empirical tests, especially in section 8.

6.1.2 The impact of the LTRO-ACC policy on lending to firms

Table 2 presents estimates of the impact of the LTRO-ACC policy on lending to firms, comparing newly-eligible firms to ineligible firms one notch or two notches below (5+ or 5 respectively). Newly-eligible firms saw their total borrowing rise on average relative to ineligible firms.

We examine single-bank firms in columns (1)-(3), defining single-bank as having only one bank in every month of 2011. Column (3) contains the results of estimating our most demanding, baseline empirical specification, while column (1) omits bank-month and industry-quarter fixed effects, and column (2) omits only the industry-quarter fixed effects. In our baseline specification, treated firms have 7.8% higher debt levels relative to the control group on average in the year after the introduction of the LTRO-ACC.[36]

In columns (4) and (5) we run our baseline specification for multi-bank firms. Column (4) runs the same specification as (3), but with multi-bank firms, and as is clear from the graphical evidence discussed above, shows only weakly statistically significant evidence of a smaller (3.2 percent) difference between newly-eligible ACC firms and ineligible 5+ rated firms one notch below. Column (5) instead compares ACC firms to those two notches below

[36] Adding interactions of the covariates with the post indicator provides essentially identical results: estimates are 0.1-0.2 percentage points smaller, with the same standard errors, for both single and multi-bank firms.

(rated 5) on the credit rating scale, and reports a much larger and more statistically significant difference of 8.9 percent, of similar magnitude to the effect for single-bank firms.[37] This difference between the estimates in columns (4) and (5) supports the graphical evidence presented for the existence of a spillover of the policy onto ineligible multi-bank firms. The existence of such a spillover exclusively for multi-bank firms, shows that banks treat these two groups of firms differently, confirming the importance of analyzing single and multi-bank firms separately.

For multi-bank firms it is not obvious whether additional, policy-induced lending will come mainly from firms' main banks, or from banks with a smaller share, aiming perhaps to increase it. Column (6), estimated with a control group of 5-rated firms, shows that virtually all the extra lending comes from firms' main bank.

We then look to the time dynamics of the effect around the introduction of the policy, estimating equation (3) and presenting the quarterly coefficient estimates in figure 6. The figure provides additional evidence for parallel trends for both single and multi-bank groups, and shows that the policy effects are sustained into 2013, although the September 2012 announcement of the ECB's Outright Monetary Transactions (OMT) policy by the ECB confounds the measurement of the pure LTRO policy effect.

Finally, to examine only the intensive margin of the effect (i.e., the effect on firms with non-trivial levels of leverage) we examine firms with bank debt of at least five percent of total assets (Amiti and Weinstein, 2017), and find similar results: an increase in leverage of 1.4 percentage points for single-bank (∼8% of the mean for ACC), and 2 percentage points for multi-bank firms (∼14% of the mean), as reported in the Appendix (Table 13).

6.1.3 Robustness of the main results

Table 3 examines a variety of potential concerns with our main results. Columns (1) to (3) illustrate that our results are not dependent on the scaling of our debt growth variable: instead of using the firm's average debt in 2011 to scale the firm's current total debt (i.e., $\overline{Debt}_{f2011}$ in our dependent variable $g_{ft} = (\sum_{b=1} Debt_{fbt})/\overline{Debt}_{f2011} - 1$), we instead

[37] We use drawn credit as our main measure of firm debt rather than drawn plus undrawn credit because it reflects the policy measure of interest, and because the use of undrawn credit in this period is likely to have been highly dependent on bank discretion: banks can cancel credit lines whenever firms violate covenants (such as profitability levels), and they are especially likely to do so in periods of aggregate or bank-specific stress (e.g., see Thakor, 2005; Acharya et al., 2014; Chaderina and Tengulov, 2017). Nonetheless, if we use drawn plus undrawn credit as our dependent variable we obtain estimates that are slightly smaller (i.e., approximately 6% and 7% debt growth for single and multi-bank firms respectively), but still statistically significant at the 1% level.

use, respectively, average debt in 2010, in the first semester of 2011 and in the second semester. Results are very similar and statistically indistinguishable. Moreover, it is worth recalling that results are also very similar if instead of scaling by debt we scale by total assets (i.e., a standard leverage measure), as reported in Table 13 in the Appendix. Column (4) clusters the standard errors by firm (as in the main specification), but also by bank-month, with no change in our estimate, which suggests, as per Petersen (2009), that the relevant heterogeneity has been well absorbed by the included bank-month fixed effects.

Column (5) adds bank cross firm fixed effects to focus on the variation in the pre-existing set of bank relationships (ensuring that results are not driven by firms that switch banks). Because very few single-bank firms switch banks or become multi-bank in this period (consistent with the "sticky" bank relationships), there is no change in the estimate for single-bank firms. For multi-bank firms the estimate falls by about one percentage point to approximately 8 percent, closely matching the estimate obtained when the dependent variable was constrained to be debt at the firm's main bank only. Thus, neither firms obtaining new bank relationships nor switching between existing relationships seems likely to be driving the estimated effects.

A different concern arises if eligible firms realize that they are effectively cheaper to lend to as a result of the policy, inducing an increase in credit demand that would not be fully captured by a firm fixed effect (which is not time varying). We believe this to be very unlikely, because, anecdotally, firms were largely unaware of the LTRO-ACC policy. This is plausible because it was a very complex policy that was directed towards banks, and also because it was the banks' decision (not the firms') whether to use loans to eligible firms as collateral, which is the only way to access the subsidy. Nonetheless, one way to address this concern is to replace the firm fixed effect with alternate, time varying controls that might capture this hypothetical demand effect. Columns (6) and (7) replace the firm fixed effect with an interacted fixed effect that is time varying, following the narrow fixed effects approach in Degryse et al. (2018), which advocates size-industry-location-month fixed effects (as in column (7)) as effective controls for credit demand.[38] Column (6) addresses the "induced demand" concern even more directly, as the fixed effect is rating (the determinant of eligibility that could potentially induce demand here) interacted with industry-location-month. Both specifications provide estimates similar to our baseline, although the estimated effects in column (6) of both tables is double that of our main specification, suggesting that our estimate is downwards biased if the narrow fixed effects strategy controls for demand

[38]Location is a Département, approximately equivalent to a county.

more effectively. Taken together, columns (6) and (7) strongly suggest that there is no induced demand that may be biasing our estimates upwards.

A potential interpretation of our results is that it is shifting credit across rating categories rather than increasing overall credit to firms. This is impossible to completely rule out, but we can examine whether ACC firms are receiving credit by crowding out the most closely comparable but ineligible firms (i.e. the 5+ firms). Column (8) of the single-bank robustness table compares the effect of the policy on these firms (the control group, rated 5+) with a similarly ineligible rating group one notch below (rated 5). The effect is small and statistically insignificant, suggesting that the estimated effect is not driven by crowding out of existing credit to the closest substitute borrowers: adjacently rated ineligible firms. We do not present this test for multi-bank firms because it is run in the spillover section.

Finally, to corroborate that the effects we estimate are driven by the policy and not some alternate mechanism, in unreported regressions we replace the dependent variable with the sum of only those components of bank debt that are not eligible to be pledged under the collateral eligibility rule (e.g., leasing, credit lines); there is no effect for these types of bank debt.

6.2 How bank lending relationships mediate the policy effects

An extensive literature focuses on the importance of lending relationships for bank-firm lending. This section uses proxies for the quality of bank lending relationships to examine whether the effects of the LTRO-ACC policy differ across firms with weak versus strong relationships. We do not have exogenous variation in treatment based on relationship length (or on anything other than credit rating), and so we cannot claim that the results in this section are causal, but they do provide strongly suggestive evidence on the mechanism behind the causal effects (on total bank lending) described in the preceding section.

SMEs are especially dependent on bank lending because they rarely have access to alternate funding sources (such as syndicated lending, bond financing, or equity issuance), and are more informationally opaque than larger firms. Bank lending relationships allow lenders to develop non-verifiable soft information (Stein, 2002; Liberti and Petersen, 2018) about the quality of the borrower over time, the result of repeated interactions with the firm (increasing in the length of relationship) and across a range of different products (relationship scope). Further, the effectiveness of this information gathering will be markedly higher if the firm has only one bank, because that bank observes virtually all of the firm's interactions with the financial system, and has stronger incentives to monitor the firm. The

acquisition of soft information should mitigate the information asymmetry that particularly affects SMEs (Cahn et al., 2019), and if they are revealed as good borrowers, improve their access to credit.

6.2.1 Are policy effects stronger for firms with deeper bank relationships?

We use two proxy variables for the quality of bank lending relationships, corresponding to the length and scope of lending relationships. Most firms in our sample have relatively long bank relationships, so relationship length will likely only be a weak indicator that the bank has better information about the quality of the borrowing firm. By contrast, we have substantial variation in the scope of firm relationships across banks. We leverage the micro-data in the credit registry to generate a measure of relationship scope that captures how many bank products the firm makes use of (and to what extent it does so).

For each proxy variable we create an indicator $D = 1$ that captures relatively deeper bank relationships (above median), and estimate a triple-difference specification:

$$g_{ft} = \alpha_f + \beta_0(D_f \times Post_t) + \beta_1(ACC_f \times Post_t) + \beta_2(ACC_f \times Post_t \times D_f) + \Lambda_{bt} + \Upsilon_{It} + \Gamma' X_{f,y-1} + \epsilon_{ft} \quad (4)$$

We construct the D indicators in Table 4 as follows. LR is the length of the lending relationship between the firm and its bank, and the sample median is six years.[39] Thus LR above median indicates a relatively long firm-bank relationship. For our scope variable we decompose each firm's bank financing into five categories: short-term credit, medium and long-term credit, accounts receivables financing, leasing, and undrawn credit lines. Using the share of each lending type we compute the firm's Herfindahl index (HHI) to measure how concentrated across bank products is the firm's borrowing. An HHI measure below median (i.e., less concentrated across product types) thus indicates a lending relationship with a larger scope, because the lender and the borrower interact across a greater range of financing products, generating more soft information for the bank. For multi-bank firms the LR and HHI measures are calculated for the main bank, as Table 2 shows that it is the main bank that drives the LTRO-ACC effect.

The results in Table 4 show that - for single-bank firms - the additional lending attributable to the LTRO-ACC policy is driven by lending to firms with deep lending relation-

[39] The average length of lending relationship may be longer, as our data is right-censored at 14 years: we cannot measure the length of the relationship before 1998.

ships: banks chose to focus additional lending on single-bank firms about which they had especially precise (and likely positive) quality signals. The first row of the table reports the estimated triple-interaction coefficient β_2, and shows that treated single-bank firms with a longer relationships benefit more from the policy. However, the largest effects are for firms with wide scope relationships (column 2), because the high average relationship length in our sample makes it a weak relationship variable. In fact, the estimate of β_0 in column (2) shows that high scope relationships receive more lending after the policy reduced overall bank funding constraints, even if the firms are not policy-eligible, making the combined effect for firms with wide scope banking relationships even larger. Moreover, single-bank firms are, by their nature, more likely to be relationship borrowers, because their banks have access to the full range of their bank interactions (i.e., have better information than the banks of multi-bank firms), and also do not need to be concerned about strategic default behavior by borrowing firms, or information externalities benefiting other banks.

Table 4 also reveals that bank-firm relationships do not appear to mediate the LTRO-ACC effect for multi-bank firms, in stark contrast to the results for single-bank firms. Columns (3) and (4) show no appreciable effect for the interactions with the proxies for deeper lending relationships.

6.2.2 Bank relationships and debt maturity

Lending at longer maturities is riskier for banks, because they relinquish the real option to eliminate their exposure to a borrower in the intervening period. Further, banks' seniority as creditors risks being undercut if the firm issues new shorter term debt (even if it is junior), because if the new debt matures before the senior debt then the new debt is effectively senior, whatever its formal status. Firms tend to strongly prefer long term debt precisely for the reasons that banks dislike it, and also because it pushes rollover risk further into the future, which is especially valuable in periods of aggregate financial stress.

The credit registry data we use has total bank debt separated into short-term debt, defined as having initial maturity of less than a year, and its complement: medium/long-term debt. If banks have more precise information about borrowers' types, as provided by banking relationships, they are more likely to be willing to lend at longer maturities. In line with this logic, Table 5 shows that for single-bank firms the LTRO-ACC drives an increase in longer maturity debt (column 2), and also increases short term debt (column 1) although the latter effect is noisy and only weakly statistically significant. By contrast, for multi-bank firms the LTRO-ACC generates an increase in short-term debt only (columns 5 and 6).

Columns (3), (4), (7) and (8) in Table 5 interact an indicator for banking relationships with a wide scope - the proxy for relationship quality that has the most power in our sample, given the long average relationship lengths.[40] They provide evidence that it is the single-bank firms with deeper lending relationships that receive long-term loans as a result of the policy, while for multi-bank firms lending relationships do not appear to affect which firms receive additional lending, and this lending is short term only (despite the three year maturity of the LTRO-ACC). Bank willingness to lend at longer maturities to single-bank firms may be due to borrowers' reduced ability to strategically default (in order to repay other banks), and also to scope economies in the provision of both credit and payment services: the banks of single-bank borrowers can observe virtually all their cash flows in real time, generating high quality information about the firm.

Thus, the evidence in Table 5 reinforces the results from the previous subsection: the LTRO-ACC effect is very strongly mediated by bank relationships for single, but not for multi-bank firms. It also suggests that multi-bank and single-bank firms receive different products from banks, at this margin: those single-bank firms with deeper relationships are granted the more-preferred longer-term debt, while multibank firms receive short-term lending only (irrespective of the depth of the lending relationship).

6.3 How observable characteristics of firms mediate policy effects

Not all firms are equally affected by the reduced cost of funding loans induced by the policy. Banks adjust their lending portfolio as existing loans mature or as firms request new loans - our policy provides a window into this portfolio adjustment process. In this section we examine how the policy impact varies with observable, or "hard" information about the firm, a standard proxy measure of lending standards. However, as was true for the preceding section, it is worth noting that we do not have exogenous variation in treatment based on firm characteristics, making this section's results suggestive rather than causal.

6.3.1 Collateral policy effects and observable firm information

We begin by examining if firms with weaker hard information are more likely to benefit from the LTRO-ACC policy. We use observable variables as of 2011 - leverage, tangibility of assets, age and size (number of employees) - and for each we create an indicator D that captures relatively higher risk borrowers based on these variables. Specifically, we successively look

[40] We obtain similar results using relationship length as a proxy for quality, but for brevity we use lending scope as our main proxy for relationship quality or depth.

at high leverage firms ($D = 1$ for firms whose average leverage ratio was above the sample median); firms with with low asset tangibility ($D = 1$ for firms whose ratio of tangible to total assets was in the bottom quintile); young firms ($D = 1$ if firm age was below 6 years), and small firms ($D = 1$ for firms with fewer than 10 employees). Table 6 presents the triple difference estimates (as in equation (4)) of the effect of the policy on lending to firms, conditional on our indicators for firms with weak observables.

For single-bank firms (odd numbered columns) the negative coefficients on $ACC \times Post \times D$ almost exactly offset the coefficients on $ACC \times Post$, indicating that the additional credit attributable to the LTRO-ACC only flows, on average, to firms with strong observables: firms with lower leverage, with collateral, older firms, and (to a lesser extent) larger firms. This appears inconsistent with the evidence for other European countries (e.g., Iyer et al., 2014; Acharya et al., 2018), which report lending patterns consistent with evergreening or zombie lending. Instead, this suggests relatively high lending standards at the margin by French banks. Unreported results for single-bank firms with which the bank has large magnitude exposures also show no differential policy effects. However the firms in this sample are only one and two notches below investment grade, which limits the extent to which these firms could be considered zombies. Nonetheless, within these credit buckets we see the marginal portfolio allocation moving towards the firms with the strongest observables.

However, the preceding result may be mechanical: if firms with weaker observables in 2011 are more likely to be downgraded, and thus become ineligible, then we should expect similar results. Appendix Table 15 requires firms to retain their rating (assigning them to either treatment or control) for 6 or 12 months in order to test whether the reported effects are driven by changes in rating correlated with hard information.[41] The estimated coefficient for leverage for the 12 month rating sample of only -2.6%, in comparison to the -8.9% coefficient in the table without the rating requirement, suggests that the reason that high leverage firms did not receive additional credit was mechanical: they were more likely to be downgraded. This is unsurprising, because leverage is a first order determinant of credit ratings. However, all the other coefficients are very similar across the two tables, and sometimes more negative, suggesting that (excepting leverage) this is not a mechanical effect.

The even columns of Table 6 repeat the analysis for multi-bank firms. Unlike for single-

[41]Note that requiring firms to maintain their rating biases the sample in potentially either direction. For example, if firms are likely to be downgraded on average, then requiring rating stability provides a selected sample of only the strongest firms, because they were not downgraded despite an average trend in that direction. Thus we perform this check as a robustness test only.

bank firms, for multi-bank firms there is no effect of weak observables on lending. That is, multi-bank borrowers with weak observables were just as likely to receive policy-driven lending as borrowers with strong observables. These apparently lower lending standards for multi-bank borrowers are in line with the shorter term (lower risk) lending product they are receiving in comparison to single-bank borrowers, as reported in the preceding section.

6.3.2 The policy effect on high-growth firms

An important category of firms that we examine are young and high-growth firms, often called "gazelles." These firms play a critical role in job creation (Haltiwanger et al., 2013), which makes it particularly important for policy to know whether a reduction in banks' cost of funding is transmitted to them in periods of financial stress.

Appendix Table 16 presents the triple difference estimates of the effect of the policy on such firms. While imprecisely estimated because of their scarcity in the sample, high growth firms see larger lending increases than our baseline estimates, relative to ineligible high-growth firms, and the effect is present for both single and multi-bank borrowers. Because high growth firms generally have high credit demand, this differential effect suggests these firms were credit constrained ex ante.

6.3.3 The policy effect on financially constrained firms

Financially constrained firms will often have the highest shadow value of financing, and so the highest demand for additional credit. However, these may also be the least credit-worthy firms. Appendix Table 17 uses two proxies to attempt to identify financially constrained firms: firms lacking unused credit lines, and firms that are net users of trade credit (i.e. firms whose accounts payable exceed accounts receivable). While the latter is not an unambiguous measure of financial constraints, it captures the fact that trade credit is an extremely expensive form of financing.

For single-banks the negative triple difference coefficient on $ACC \times Post \times D$ strongly offsets the positive coefficient of $ACC \times Post$ in columns 1 and 2, meaning that financially constrained firms receive little additional credit as a result of the introduction of the LTRO-ACC framework. In unreported results we find similar effects for financially constrained firms with deeper lending relationships, suggesting a hard limit to the value of relationships. By contrast, for multi-banks the results are less clear: such firms appear to receive less credit if they have no undrawn credit lines, but the effect is not present for firms that are net trade credit users.

In short, single-bank firms that appear financially constrained receive little extra lending as a result of the policy, while multi-bank firms that are financially constrained may receive at least some additional lending.

6.4 Policy effects on rating downgrades and on defaults

A central concern with policy-induced lending is whether the additional lending is "bad" lending - lending that is negative NPV. This is often linked to lending aiming to avoid recognizing bad loans by "ever-greening" them, or lending to "zombie" firms that will eventually default but can be kept alive for the present by extending additional credit, as mentioned earlier. While this is difficult to clearly rule out, clear correlates of bad lending, such as a higher probability of ex post defaults or credit rating downgrades are testable, and we do so in this section.

6.4.1 Credit rating downgrades

We first estimate the probability that an eligible firm is downgraded such that it falls two or more notches below its December 2011 rating.[42] To this end we estimate equation 2 as before, but begin the analysis in January 2012 and define our post-treatment period to begin in June 2012 (because our research design forces all our firms to have specific ratings as of December 2011, limiting our pre-period).

Table 7 estimates a linear probability model for the probability of severe downgrade and shows that for single-bank firms, the probability of such a downgrade is lower for treated firms than for control firms by about a third of a percentage point (column 1). Column (2) shows a quarterly decomposition of the effect, with the coefficient on $ACC \times 2012q2$ serving as a pre-period test relative to the omitted period of the first quarter of 2012, and the coefficients for the three subsequent quarters reflecting the post-period. Column (3) increases the power of the estimate by combining both pre-periods (the two first quarters of 2012) into a single pre-period, resulting in a more precise estimate of the ex post effects, which are concentrated in the fourth quarter of 2012. In contrast to single-bank firms, the probability of a two notch downgrade for multi-bank firms does not fall, and in fact may have risen in the fourth quarter of 2012.

[42] We require the rating to be two notches below the initial rating, instead of a single notch, because we aim to provide evidence on zombie lending, and a single notch downgrade in the year following the policy implementation does not constitute plausible evidence of this - a single notch downgrade could be caused, for example, by levering up in response to the policy (because leverage is a major ratings determinant), coupled with a negative firm-specific shock.

6.4.2 Defaults on debt to suppliers

We now analyze the effects of the policy on default by firms on (bank-intermediated) debts to their suppliers. A payment default is defined as a failure to pay a debt to a supplier ("trade bill") in full and/or on time, and we scale the amount defaulted on by the firm's (yearly) payables, making the monthly numerator into a yearly figure by multiplying it by twelve. We focus on non-payment incidents labeled as being due to insolvency (liquidation of the firm), to debtor liquidity shortages, and when bills are "contested" because the label is ambiguous and may often reflect non-payment for liquidity reasons.[43] Our sample is composed of relatively high credit quality firms, so payment defaults are rare.

As illustrated in Figure 7 for single-bank firms there is little difference in both treatment and control groups until mid-2012, when the newly-eligible group separates from the ineligible control group. To test this empirically, we estimate equation (2) for this default variable.

Columns (1) and (2) of Table 8 show that the policy reduced defaults on debt to suppliers for single-bank firms, as compared to untreated firms by between 1.2 and 1.5 percent of payables. Column (3) provides a quarterly breakdown, and shows that the reduced defaults begin at around six months following the shock (and the coefficient on the second row provides a pre-trend test). The analogous results for multi-bank firms are presented in columns (4) to (6), and while the point estimates are of similar magnitude, the standard errors are too large to suggest that there is a real reduction in defaults on suppliers.

These results, like those for the credit rating downgrades, suggest that the lending extended to single-bank firms as a result of the change in collateral policy was not obviously bad lending. Instead, the lending had a positive externality by reducing defaults on these firms' suppliers. In turn, this reduces the contagion chain of defaults caused by bank belt tightening that may propagate through supplier networks in crisis periods, in line with the findings of Boissay and Gropp (2013). In sum, our results for downgrades and defaults for single-bank firms point to relatively good lending based on measures of ex post default and creditworthiness. For multi-banks the results are less clear, and at minimum suggest a weaker effect for such firms.

7 Real and financial effects of the policy

In this section we make use of annual accounting data to explore both the financial and the real effects of the LTRO policy. We take the data from financial years 2010 and 2011 and

[43] Omitting contested incidents does not change the results.

combine them into a single pre-period (we do the same with 2012 and 2013 for the post-period), and run a two-period difference in differences design using fixed effects for firm, bank-time and industry-time, as well as the same controls (lagged size, profitability, and tangible assets) used in all the preceding regressions. Results are reported in Table 9.

Single-bank firms see their leverage rise by around 2 percentage points over this period, but despite this their interest coverage ratio (ICR) rises by 2 (relative to a pre-period mean of 18). That is, firms in the treatment group have both higher leverage and can more easily cover interest payments in the post period than firms in the control group, which likely reflects the strong positive selection (by banks) of which firms receive lending, within the group of single-bank firms. Note that the change in ICR is not driven by a large change in interest rates paid by firms: their apparent, all-in cost of debt (CoD, calculated as total interest expenses over financial debt) remains unchanged in economic terms, falling by three basis points (column 3). The latter indicates that banks mainly adjusted lending on the quantity (and maturity) margin rather than on price. Turning to how firms employed the additional credit, total assets grow by slightly over 1 percentage point for single-bank firms (column 4), driven by a 2.6 percentage point relative increase in fixed asset investment (column 5), without any increase in employment (column 6).[44]

Multi-bank firms also see their leverage increase, but they see the opposite result for the interest coverage ratio: it declines by 2.5 (relative to a pre-period mean of around 20). This does not seem to be a price effect, as interest rates (proxied for by CoD) also remain essentially unchanged. Instead it likely reflects a different firm population receiving credit, reinforcing the central finding that multi- and single-bank firms receive differing treatment from banks. In turn, multi-bank firms use the additional resources differently to single-bank firms: perhaps because they receive short term lending only, total assets and investment do not increase. Instead, employment increases by slightly over half an employee, or 3% of the pre-period mean, a small economic effect.

In unreported results we explore whether there are detectable differences between two-bank and three or four bank firms in terms of the outcomes examined in Table 9. We find no robust differences across these sub-populations of multi-bank firms. We also check for evidence of zombie lending, and find no evidence that firms in the bottom quartiles of 2011 (or pre-period) profitability or ICR receive more policy-driven lending.

The results in this section illustrate that the fact that banks provide single and multi-bank

[44]This may be related to the results in Aghion et al. (2018), which reports an inverted-U relationship between credit access and productivity growth in France.

firms with different lending products in response to the policy has material consequences for firms' ex post financial structures and real investment decisions.

8 Spillover effects of the policy

Figure 5 provides strong graphical evidence of a spillover of additional lending onto ineligible firms for multi-bank, but not for single-bank firms. As a result we have used firms rated 5 - i.e. firms two notches down from the treated group - as a control group for multi-bank firms throughout. In this section we quantify the spillover, and examine potential drivers.

This spillover is surprising, because while it is causally attributable to the LTRO-ACC policy, the lending itself was explicitly not targeted by the policy: it is lending to ineligible firms. A natural explanation for the un-subsidized spillover lending to some firms but not others is that it is a manifestation of relationship lending, whereby banks use some of the relaxation of the *bank-level* liquidity constraint produced by the LTROs to lend to firms with which they have valuable relationships that will provide future rents. Consistent with this idea we find that the spillover lending is only provided by firms' main bank.

However, we find that there is no spillover for single-bank firms, even for those with long lending relationships. Instead, what may drive the spillover is competition between banks: when firms have more than one bank there is a credible threat to the main bank of losing future lending rents. This threat could induce main banks to invest in the relationship by providing additional lending (despite the lack of subsidy eligibility). In contrast, for single-bank firms this threat of moving their business to other banks is much lower, especially in our sample period during which banks were under considerable liquidity pressure. In fact, we see significant firm-bank stickiness in our data: 86% of our single-bank firms remain single-bank with their original bank (and none that remain single-bank switch banks) in the 2011-2013 period. This is likely because the significant information opacity of firms makes relationships important sources of credit-relevant information, and because switching firms may be adversely selected.

We present evidence indicating that spillover lending is provided by main banks in order to retain clients in the face of competitive threats from other banks. This risk is largely absent for single-bank firms, and so the equivalent single-bank firms do not receive spillover lending.

8.1 Spillover: graphical evidence

Figure 8 provides clearer evidence of the spillover. It shows debt growth for the two highest-rated ineligible categories (5 and 5+) for both single and multi-bank firms. Only 5+ rated multi-bank firms see their debt rise markedly and consistently after the LTRO program was introduced.[45]

This graph illustrates that we could potentially use three different control groups to estimate the size of the spillover (the three other lines in the graph), and that this will not materially affect the results. However, we highlight that we show only 1 year as a pre-period, instead of the two years we have used in previous graphs. This is because here (unlike in our other graphs and tests) we are comparing single to multi-bank firms, and their pre-trends diverge as we move further from the focal period where assignment to each group is determined (December 2011), and membership of each group becomes noisier. Nonetheless, our regressions have always been run with a one year pre-period and a symmetric post period, so this does not affect our regression results.

8.2 Spillover: regression evidence

Table 10 estimates the size of spillover lending by comparing multi-bank firms rated 5+ (highest rated ineligible firms) with firms rated 5 (the next notch down). We focus on multi-bank firms only, because there are no spillover effects for single-bank firms.

In column (1) we estimate the size of the spillover at 7.5 percentage points of additional lending, and column (2) shows that spillover lending is essentially entirely coming from the main lender. Columns (3) and (4) restrict the dependent variable to short-term debt, and medium/long-term debt respectively, and show that the spillover lending is short-term lending only. Thus, in magnitude and type of lending, the spillover looks very similar to the policy-driven lending provided to eligible multi-banks.

In unreported regressions we examine whether the spillover is associated with a variety of observable firm characteristics: it is not. Nor is it associated with relationship length, or with deeper (wider scope) relationships. Moreover, it is not driven by banks identifying firms that are subsequently upgraded into eligible credit ratings.[46] Instead, the spillover is

[45] Unreported regressions provide estimates that closely match the graphical evidence: using our standard difference in differences specification, 5+ rated multi-bank firms receive approximately 10% more credit than 5+ rated single-bank firms; the estimate has a t-statistic of 3.

[46] Columns 9 (and 10) are estimated on samples of firms with unchanged ratings for at least 6 months (1 year). Slightly larger coefficients rule out the idea that the spillover is driven by 5+ firms being more likely to be upgraded than 5 firms.

provided by main banks to clients that the banks seem to be at risk of losing. In columns (5)-(8) we show that the spillover lending goes to firms that either increased their number of banks (column 5), or to firms that decreased the concentration of lending across their banks in the year to September 2011 i.e., in the pre-period (column 7). This pattern suggests that the competitive threat of other banks is what drives spillover lending.

9 Policy effects on lending to firms across the size distribution and across-bank heterogeneity

So far we have focused on comparing the effect of the LTRO policy on newly-eligible firms (rated 4/ACC) with the two never-eligible credit categories (5 and 5+) below it. We now take a broader perspective, and evaluate the overall effect of the LTRO policy on lending to SMEs, by comparing the firms rated above the ACC firms (i.e. the firms that were always eligible for standard Eurosystem collateralization of their loans) with the never-eligible categories we have already been using as controls (ratings of 5 and 5+).

We do this because the LTROs were a major positive liquidity shock to banks, allowing for unlimited borrowing at the MRO rate against eligible collateral, and at much longer maturity (3 years) than had previously been possible. This subsidized, long maturity debt had the advantage of minimizing rollover risk, and matching firm loan maturities much more closely than the existing Eurosystem lending facilities. Thus, we should expect the LTRO shock to affect always-eligible (i.e. higher rated) firms, as much as it affected ACC firms, because it also alleviated severe funding pressures at the bank level, and because the haircuts applied to bank loans for the purposes of Eurosystem collateral were significantly lower for higher rated firms, making the magnitude of the subsidy greater.

9.1 Graphical evidence for the general LTRO effect

Figure 9 graphs average debt growth relative to each firm's 2011 mean, by rating category. The figure excludes the 4/ACC category that we have been using thus far as a treatment group, to focus attention on the higher-rated always-eligible firms that are likely to make up the bulk of the overall LTRO effect. Note that for this section we define groups using their ratings as of September 2011, because firms with ratings above 4/ACC were eligible to be used as collateral for the October 2011 one-year LTRO. The figure shows strong debt growth for always eligible single and multi-bank firms after the policy was announced. It

also provides strong evidence for parallel (or near-identical) trends at monthly frequencies for the two years preceding the LTRO policy shock for all the credit rating categories in the graph.

9.2 Regression evidence for the general LTRO effect

Table 11 reports estimates of equation 2 for the average effect of the LTRO on always-eligible categories of firms, using 5+ as a control group for single-bank firms and 5 as a control group for multi-bank firms (because the spillover effect for 5+ multi-bank firms makes them a poor control group). Columns (1) and (3) split the treated group of always-eligible firms into two: the highest rated firms (rated 3++ and 3+), and the next two credit ratings (3 and 4+). Each group has the same haircut applied to its value by the Eurosystem. There are no statistically significant differences across the two groups, so the estimates in columns (2) and (4) provide a representative estimate of the average causal effect of the LTRO: approximately 12 percentage points higher debt for both single and multi-bank firms.

9.3 The general LTRO effect on mid-sized and large firms

We also examine the LTRO policy's effect on larger firms. These firms almost always have subsidiaries, and we cannot confidently consolidate their information because of the aggregation issues explained in section 4, making all evidence for these firms suggestive rather than causal. We use data on ownership of all firms in the economy to identify subsidiaries and to aggregate them into their owner firms, assigning the rating of the holding company to be the rating for the whole firm structure.

Using our main research design we find that the LTRO caused an increase of 9 percentage points in total bank debt for large and intermediate sized always-eligible firms (significant at the 5% level in unreported results), which fits with the finding in Andrade et al. (2018) that LTRO liquidity was transmitted to large firms. However, we find that the effect is driven by the very highest-rated firms, and by mid-sized firms (approximately, firms with 250 to 5,000 employees and sales (or assets) below €1.5bn (€2bn)), rather than by the relatively few large firms, which are likely to be the least constrained. Consistent with the effect being driven by the highest-rated firms, we find no debt increase for ACC-rated firms, or any spillover effects.

9.4 Cross-Bank heterogeneity of policy effects

We now examine whether there are differences across banks in the impact of the LTRO policy, specifically with regards to the health of their balance sheets. However, because regulatory and financial conditions bind at the bank-group level, and groups have active internal capital markets, all measures of balance sheet strength (e.g., regulatory capital, funding costs, market value of equity) must be measured using the consolidated balance sheet of bank groups, and not at the level of sub-entities within a banking group (i.e., the level identified by the "code interbancaire" − CIB − in France). Unlike the US and Italy, which have an unusually high number of distinct banks, making a research design focused on cross-bank differences feasible, France has a very concentrated banking market (6 banking groups grant more than 90% of total credit as of December 2011) so the results in this section should be viewed as suggestive only, and are based on interactions of bank groups within our firm-level research design.

The columns in Table 12 display the results of adding an interacted indicator for several measures of bank group balance sheet strength (for the 6 largest banking groups, capturing over 97% of our sample) to our research design, as in equation 4. Data sources are listed in the Appendix. Columns (1)-(3) for single-bank and (6)-(8) for multi-bank show that the main policy effect is not driven by cross-sectional differences in measures of bank-group-level balance sheet strength: funding cost, level of regulatory capital, or change in the market value of equity. This suggests that, broadly, bank groups were equally constrained ex ante with respect to lending to firms.

The one area of difference that emerges is for multi-bank firms only, and is that the policy effect is stronger for borrowers of bank groups with more unpledged collateral (columns 5 and 10), and with more LTRO-eligible borrowers in their lending portfolios before the policy was implemented (columns 4 and 9); that is, the banks best placed to take maximum advantage of the policy. Similar to the result for spillover lending, this again suggests that multi-bank lending is the marginal use of bank funds, and that single-bank lending is infra-marginal, consistent with the existence of rents to banks from relationships, and with banks competing for multibank borrowers.

9.5 Approximate estimate of total additional lending due to the LTROs

Although we are able to generate an approximate estimate of the total additional lending to firms generated by the LTRO policy, our study is focused on identifying causal effects using differences across groups of firms, and so is not well-suited to such a calculation. Thus, any estimate we make is subject to many caveats, ignores the effects of the overall relaxation of liquidity constraints at the bank level, and makes the strong assumption that our sample is representative of all eligible French firms.

Using our estimates in tables 2 and 11 for each combination of eligible credit rating and single/multi-bank status, and multiplying them by the respective amounts of total drawn credit of French nonfinancial corporations (excluding real estate and micro firms) as of December 2011, we calculate that the LTRO generated an additional €37 bn of lending (€40bn including spillover lending). That is, absent the LTRO policy, total credit to these firms would have decreased by around 10%, whereas the actual decrease was only 4%.

10 Concluding remarks

Large scale unconventional monetary policy interventions have become a feature of the central bank policy landscape since 2008, and understanding their effectiveness and channels is crucially important for the design of future policies. This paper studies an intervention that injected long-term liquidity into banks via subsidized central bank lending against eligible collateral: the 3-year LTROs of 2011-2012. We focus on lending to firms, particularly SMEs, as this was an explicit goal of this policy and remains a major objective of policymakers around the world, as evidenced by the ECB's subsequent launch of two "targeted" variants, known as TLTROs (in 2014 and 2016, the third is scheduled for September 2019) that explicitly tie ECB liquidity provision to the amount lent by each bank to firms and households (excluding mortgages).

We find that the policy was effective in raising lending to eligible firms in a period of severe financial stress for banks. We also provide novel evidence regarding which firms are selected by banks to receive additional lending, what kind of lending firms receive, and how they make use of these additional resources. Our main contribution is to uncover the major differences in how banks treat firms with only one bank relationship relative to firms with multiple relationships, underlining the importance of not excluding the numerous, and macroeconomically important single-bank firms from policy analysis.

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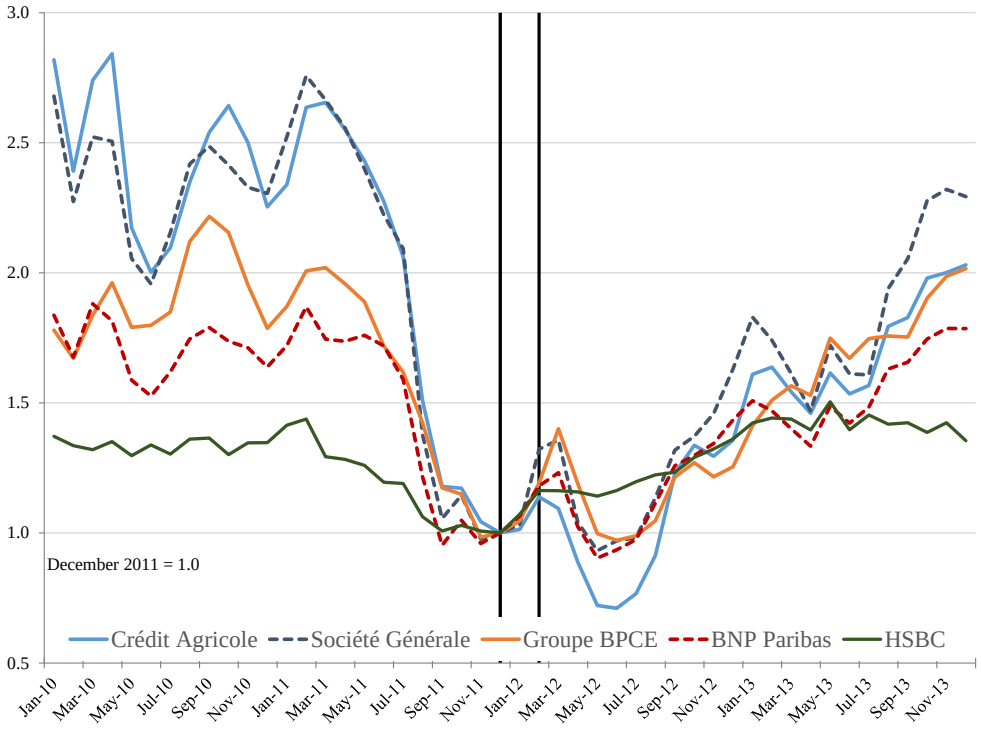
A Figures and Tables

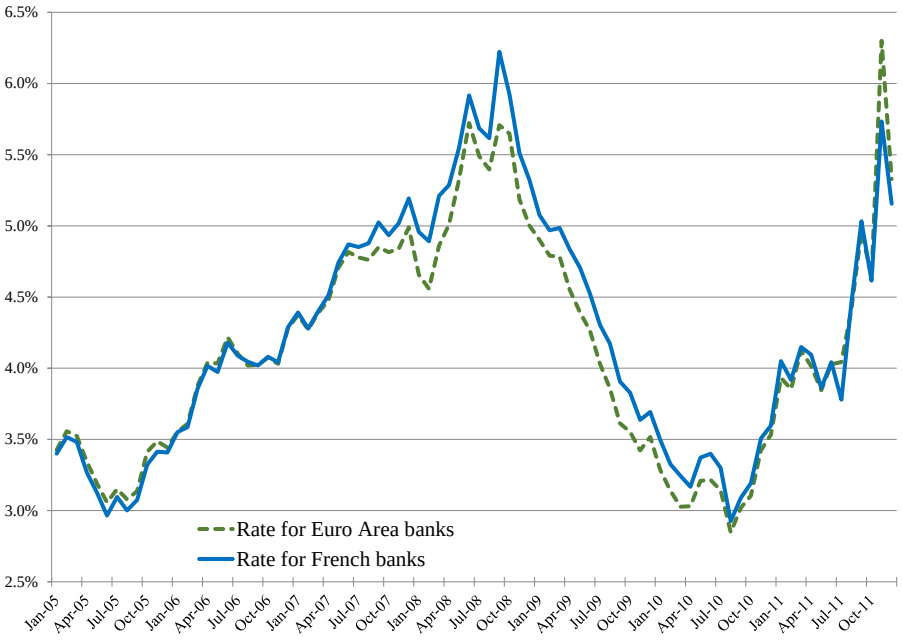
Figure 1
Equity values for listed French bank groups



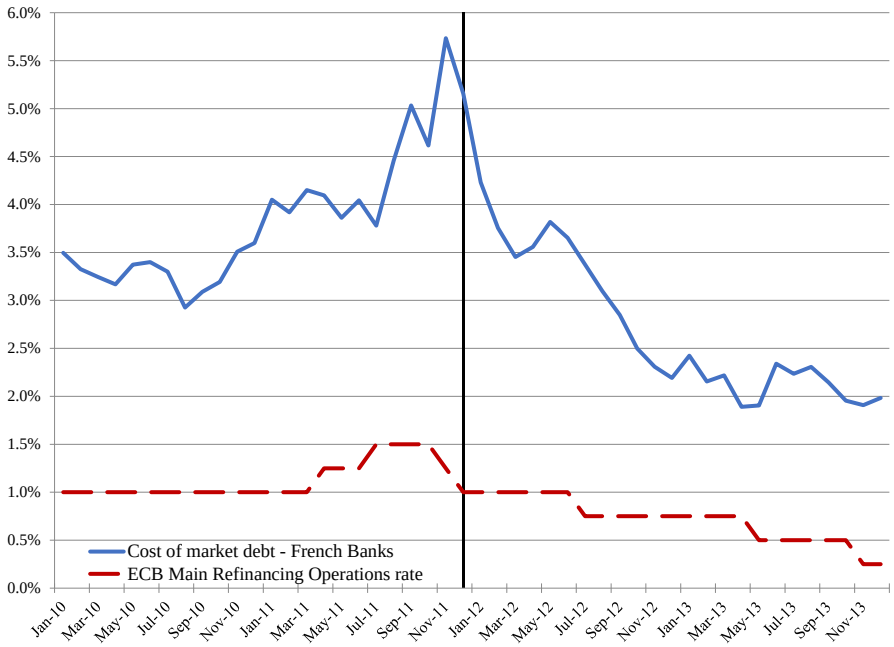
Note: This figure plots the monthly average stock price for the 5 largest publicly-listed French banking groups, as a fraction of each bank's December 2011 equity market value. The vertical lines denote the months of the two LTROs.

Figure 2

Market funding cost for French and Eurozone Banks, 2005-2011

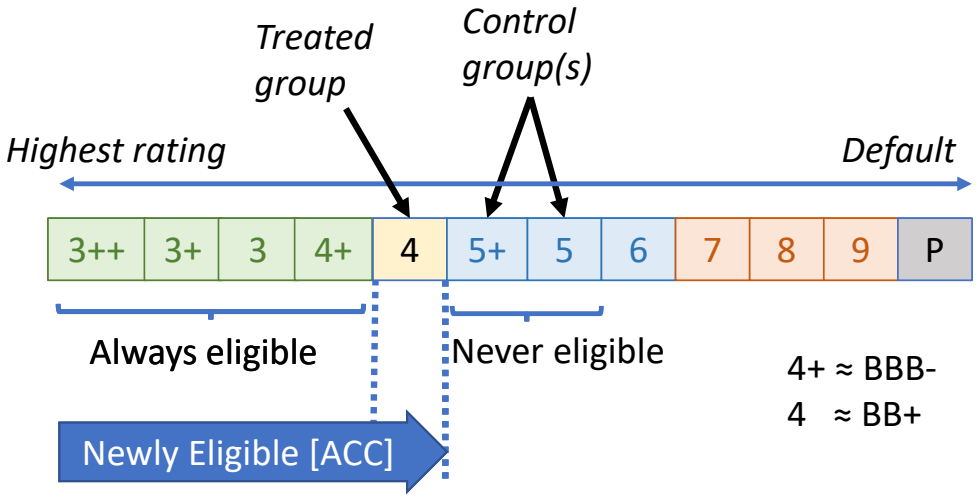


Market versus ECB funding cost for French banks, 2010-2013



Note: The top figure plots market funding costs for both French and Euro area banks extracted from bond issues from Gilchrist and Mojon (2017) - a proxy for banks' marginal longer-term funding cost. In the pre-period for our difference in differences estimate (2011, the final year in the graph, especially the second semester) bank marginal funding costs were as high as they were at the peak of the US financial crisis. The bottom figure focuses on French banks in the estimation period, and adds the ECB's main refinancing operation rate (the rate at which LTRO borrowing occurs). The vertical line is in December 2011, when the LTROs were announced (8th) and the first was implemented (21st).

Figure 3
Banque de France's credit rating scale for firms

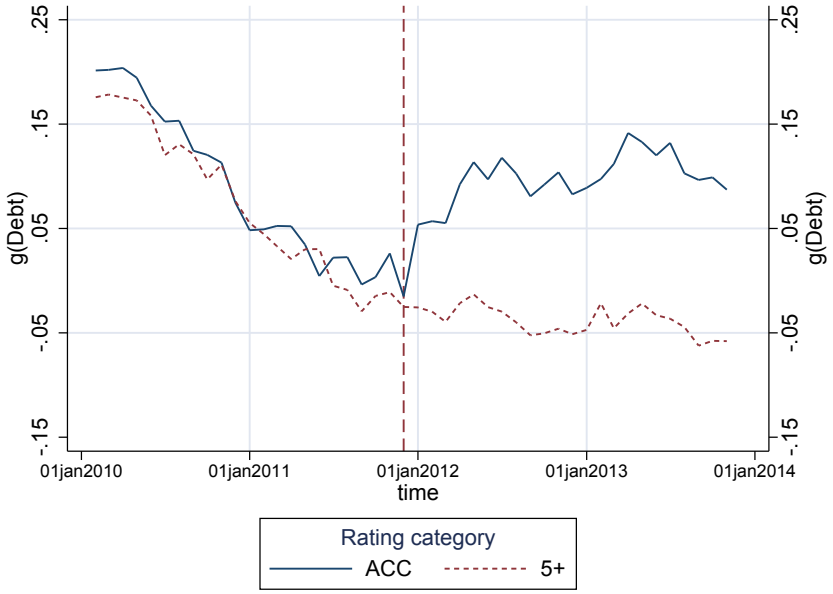


Note: This figure illustrates the Banque de France credit rating scale for firms, as well as whether loans to such firms are eligible as collateral for bank borrowing from the Eurosystem. It also shows the baseline empirical setting for the difference in differences research design (intention-to-treat). Assignment to treatment and control group is based on firms' credit rating in December 2011.

Figure 4

Average debt growth for treated and control firms

Single-bank firms

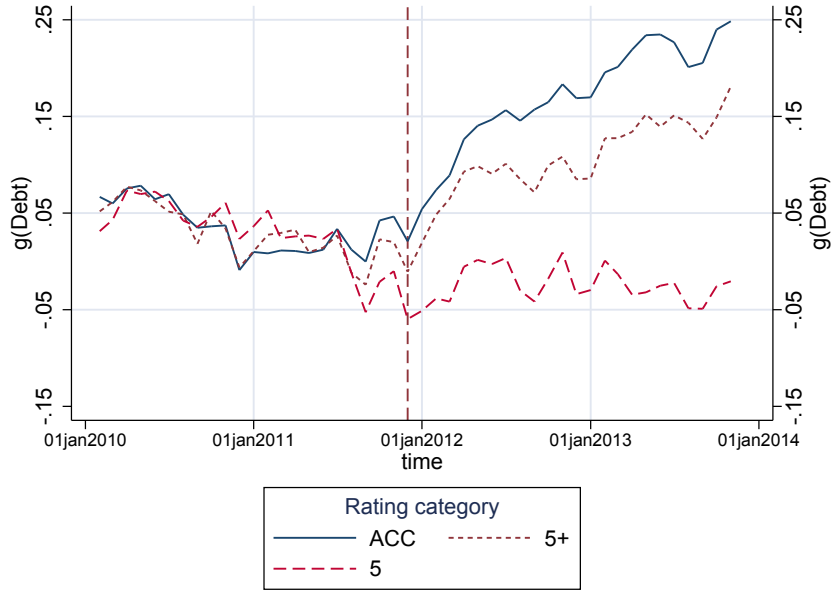


Multi-bank firms

Note: The figures show the percentage change in firms' total bank debt (relative to their 2011 average) around the LTRO-ACC policy (announcement date: December 2011 - vertical line) for the treatment group and the control group. The top panel is for single-bank borrowers and the bottom panel for multi-bank borrowers. Assignment to treatment and control groups is based on firms' credit rating in December 2011. The treated group is composed of 4-rated firms (newly-eligible borrowers or "ACC firms"). The control group is composed of 5+ rated firms (closest ineligible borrowers on the Credit Rating scale of the Banque de France). For each month we plot the unconditional average across firms, of the growth rate of debt relative to the firm's 2011 average: i.e. $g_{ft} = (\sum_{b=1} Debt_{fbt})/\overline{Debt}_{f2011} - 1$ averaged across firms, where b indexes banks.

Figure 5

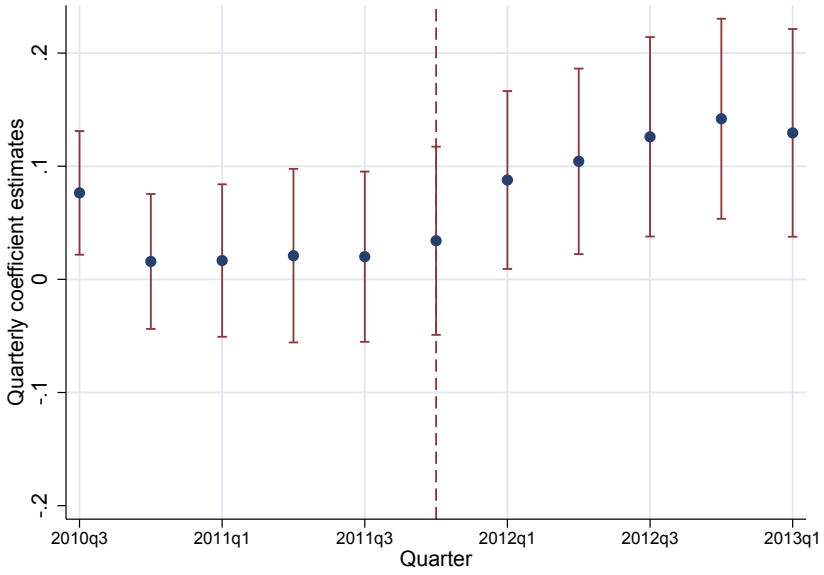
Average debt growth for multi-bank firms: Spillover



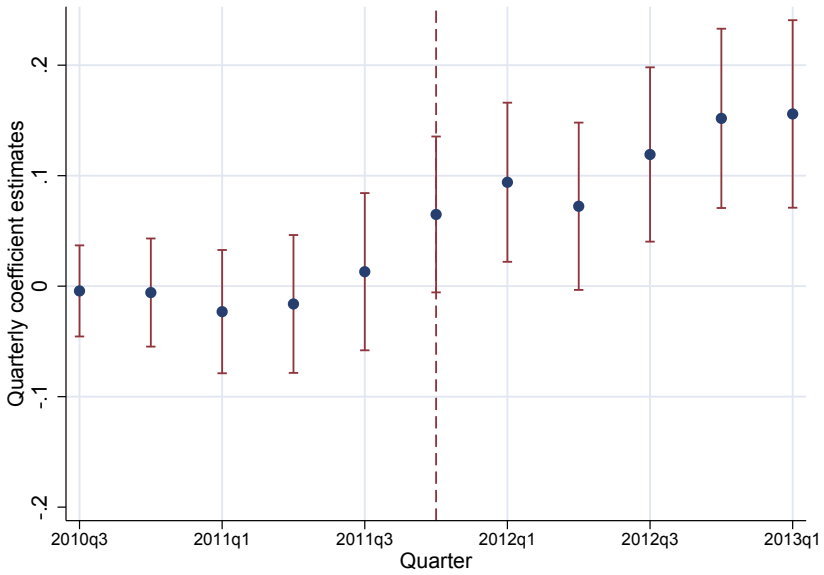
Note: The figure shows the percentage change in firms' total bank debt (relative to their 2011 average) around the LTRO-ACC policy (general announcement date: December 2011 - vertical line) for the treatment group (ACC or 4 rated firms) and for firms in the two credit rating notches below (5+ and 5, both ineligible). Assignment to credit rating groups is based on firms' ratings in December 2011. For each month we plot the unconditional average across firms, of the growth rate of debt relative to the firm's 2011 average: i.e. $g_{ft} = (\sum_{b=1} Debt_{fbt})/\overline{Debt}_{f2011} - 1$ averaged across firms, where b indexes banks.

Figure 6
Quarterly dynamics of the LTRO policy effect on firm debt

Single-bank firms



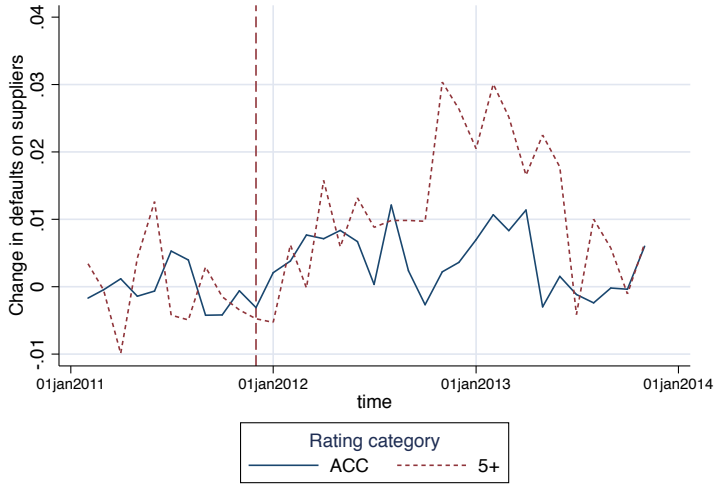
Multi-bank firms



Note: The top (bottom) figure displays the evolution in the bank debt of eligible (ACC) single-bank (multi-bank) firms around the LTRO policy announcement date, relative to the closest rating group of ineligible firms (5+ rated firms, for single-bank firms, 5 for multi-bank firms). Using specification 3, we estimate the treatment coefficient for each quarter, rather than for a single "Post" period. The omitted periods are quarters 1 and 2 of 2010 and we require that firms' rating be maintained throughout 2011. The coefficient corresponds to the percentage change in firms' total bank debt (relative to their 2011 average), after controls. The dashed lines plot 95% confidence intervals; standard errors are clustered by firm. The vertical line identifies the announcement (and implementation) quarter of the first LTRO; the second LTRO (including the ACC) was implemented in the following quarter.

Figure 7

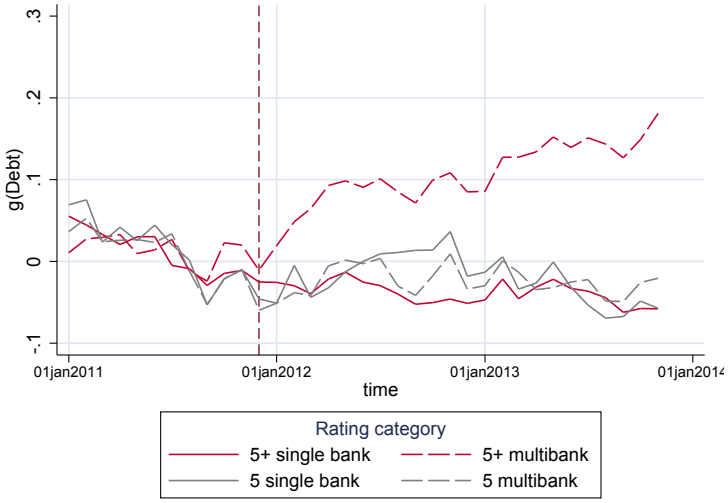
Defaults on debt to suppliers - single-bank firms



Note: This figure depicts the time-series average of defaults on debt to suppliers for single-bank firms around the LTRO-ACC policy announcement. Default is normalized (divided by) each firm's total payables, and is expressed as deviations (in percentage points) from each firm's 2011 average.

Figure 8

Spillover of lending onto ineligible firms



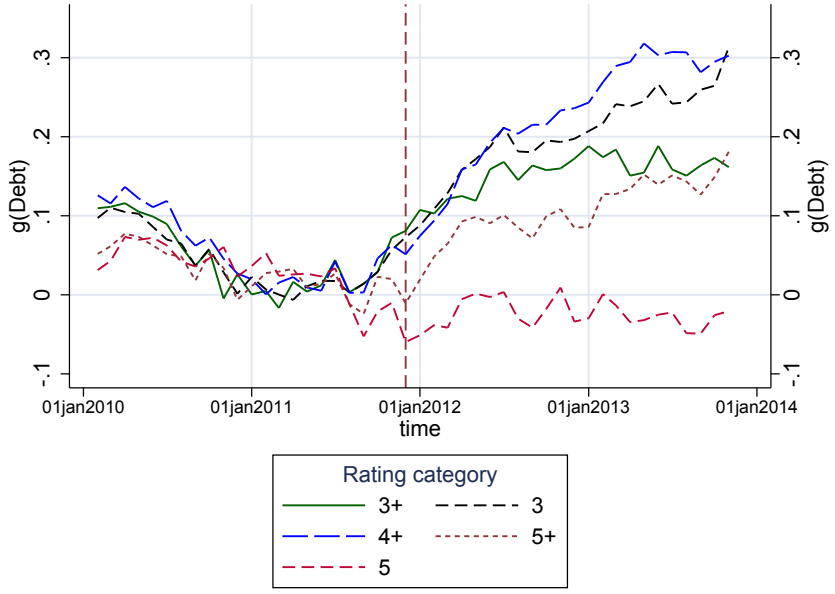
Note: The figure shows the average growth rate in debt around the LTRO-ACC policy (general announcement date: December 2011 - vertical line) for ineligible firms rated 5 and 5+. Assignment to credit rating groups is based on firms' ratings in December 2011. For each month we plot the unconditional average across firms, of the growth rate of debt relative to the firm's 2011 average: i.e. $g_{ft} = (\sum_{b=1} Debt_{fbt})/\overline{Debt}_{f2011} - 1$ averaged across firms, where b indexes banks.

Figure 9

Overall LTRO effect: average debt growth for always eligible vs. always ineligible firms

Single-bank firms

Multi-bank firms



Note: The figures show the average growth rate of debt around the LTRO policy (general announcement date: December 2011 - vertical line) for always eligible firms (4+, 3 and 3+ rated firms which are, respectively, one, two and three notches higher than newly-eligible 4/ACC firms) and ineligible firms (5 and 5+ rated - one and two notches lower). 4/ACC rated firms are omitted for clarity. Firms are assigned to credit rating categories based on their credit rating in September 2011, because always-eligible firms were eligible for use as collateral for the the October 2011 one-year LTRO. For each month we plot the average across firms of the growth rate of debt relative to the firm's 2011 average: i.e., $g_{ft} = (\sum_{b=1} Debt_{fbt})/\overline{Debt}_{f2011} - 1$ averaged across firms, where b indexes banks.

Table 1
Summary Statistics

	Single-bank ACC (4)				**Single-bank 5+**			
	Mean	Median	Sd	N firms	Mean	Median	Sd	N firms
Total Assets (thousands of euros)	1,820	1,031	8,126	1913	1,977	1,422	3,568	1139
N.of Employ.	16.83	13.00	13.72	1877	14.62	12.00	12.03	1112
Age (years)	19.59	17.00	14.65	1913	14.01	9.00	13.12	1139
Bank debt (thousands of Euros)	290	122	512	1913	730	304	986	1139
Leverage	0.18	0.13	0.18	1866	0.34	0.30	0.26	1069
Short-term debt / Total debt	0.21	0.00	0.35	1862	0.17	0.00	0.32	1118
Length of main bank relationship (years)	7.56	6.58	4.30	1913	6.25	5.42	4.21	1139
Fixed Assets over Total Assets	0.47	0.46	0.28	1866	0.45	0.40	0.34	1069
Profitability	0.11	0.10	0.12	1913	0.10	0.10	0.10	1139
Interest Coverage Ratio	16.33	9.30	23.31	1830	8.51	5.42	14.89	1121
Apparent Cost of Debt	0.06	0.04	0.05	1900	0.05	0.04	0.04	1137
Investment	0.18	0.03	1.47	1689	0.38	0.02	2.53	871
Default as % of Payables	0.012	0.00	0.16	1913	0.017	0.00	0.22	1139
	Multi-bank ACC (4)				**Multi-bank 5**			
	Mean	Median	Sd	N firms	Mean	Median	Sd	N firms
Total Assets (thousands of euros)	2,488	1,446	4,411	3287	2,148	1,255	3,158	830
N.of Employ.	23.81	18.00	20.95	3262	24.44	18.00	20.10	813
Age (years)	22.33	19.42	15.39	3287	21.48	18.00	15.82	830
Bank debt (thousands of Euros)	452	219	914	3287	534	267	874	830
Leverage	0.14	0.11	0.15	3193	0.17	0.11	0.19	788
Short-term debt / Total debt	0.40	0.31	0.38	3236	0.55	0.62	0.38	823
Length of main bank relationship (years)	7.79	7.08	4.44	3287	7.64	7.08	4.45	830
Fixed Assets over Total Assets	0.45	0.42	0.26	3193	0.52	0.48	0.31	788
Profitability	0.12	0.10	0.11	3287	0.08	0.08	0.14	830
Interest Coverage Ratio	18.53	9.13	29.06	3220	9.13	4.44	21.04	804
Apparent Cost of Debt	0.06	0.05	0.05	3281	0.06	0.05	0.05	825
Investment	0.13	0.03	0.60	2959	0.13	0.02	0.90	705
Default as % of Payables	0.012	0.000	0.346	3287	0.035	0.000	0.273	830

Note: The top panel reports sample summary statistics across 2011 for single-bank firms , and the bottom panel for multi-bank firms. Each panel compares the treatment group, ACC firms (credit rating of 4), to the main control group, which is 5+ (one notch below ACC) for single-bank and 5 (two notches below) for multi-bank. Single-bank (multi-bank) refers to firms with only one (>1) bank relationship throughout 2011.

Table 2
Effect of the LTRO-ACC policy on firm debt

	Single-bank (ACC vs. 5+)			**Multi-bank**		
	(1) Firm, Month FE	(2) Bank x Month	(3) Ind x Quarter	(4) ACC vs. 5+	(5) ACC vs. 5	(6) Mainbank only
ACC×post	0.098***	0.082***	0.078***	0.032*	0.089***	0.077***
	(0.019)	(0.018)	(0.018)	(0.016)	(0.019)	(0.018)
Covariates	yes	yes	yes	yes	yes	yes
Bank-Month FE		yes	yes	yes	yes	yes
Industry-Qtr FE			yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Month FE	yes					
N of clusters (firms)	2679	2675	2675	4576	3612	3612
Observations	56,153	55,568	55,568	89,828	70,721	70,333
R^2	0.42	0.44	0.45	0.48	0.48	0.44

Note: This table presents difference in differences estimates of the effect of the LTRO-ACC policy on the total bank debt of firms. We estimate equation 2:

$$g_{ft} = \alpha_f + \beta(ACC_f \times Post_t) + \Lambda_{bt} + \Upsilon_{It} + \Gamma' X_{f,y-1} + \epsilon_{ft}$$

where f indexes firm, I indexes industry, b indexes banks, t denotes time in months, and y fiscal year. The dependent variable g_{ft} is debt growth, defined as the percentage change in a firm's total bank debt, relative to the firm's 2011 average. g_{ft} is the sum of the firm's debt across all banks, divided by the the firm's average debt in 2011 ($\overline{Debt}_{f2011}$), minus one, i.e. $g_{ft} = (\sum_b Debt_{fbt} \ / \ \overline{Debt}_{f2011}) \ - 1$.

Because the regression is at the firm-month level we omit the b (bank) and I (industry) subscripts for the dependent and error variables. The *ACC* variable takes a value of one for any firm with a rating of 4 as of December 2011 and zero otherwise, thus identifying the newly-eligible firms that make up the treated group. *Post* is a post-treatment indicator equal to 1 in each month after February 2012. α_f is a vector of firm fixed effects; Λ_{bt} are bank–month fixed effects; Υ_{It} are industry-quarter fixed effects. $X_{f,y-1}$ are firm characteristics obtained from the previous year's accounting data: ln(total assets); tangible assets / total assets; EBITDA / total assets. Column 6 shows the effect when we restrict the dependent variable to be debt held at the firm's main bank only (i.e. the bank with highest proportion of total lending). Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 3
Robustness tests

	Alternate debt scalings(instead of 2011)			Specification changes				Crowd-out
	(1) Using 2010	(2) Using 2011s1	(3) Using 2011s2	(4) Double cluster	(5) Bank-Firm FE	(6) Rating-plus FE	(7) Size-plus FE	(8) 5+ vs. 5
Single-bank firms (ACC vs 5+)								
ACC×post	0.081***	0.092***	0.072***	0.078***	0.078***	0.168***	0.060**	
	(0.022)	(0.019)	(0.019)	(0.018)	(0.017)	(0.036)	(0.026)	
ACC						0.002	0.009*	
						(0.009)	(0.005)	
Rating 5+×post								-0.024
								(0.025)
N of clusters (firms)	2510	2632	2647	2675	2675	2419	2183	1327
N of clusters (Bank-Month)				372				
Observations	52,177	55,117	55,359	55,568	55,513	42,843	44,669	27,651
R^2	0.76	0.56	0.44	0.45	0.48	0.33	0.33	0.45
Multi-bank firms (ACC vs 5)								
ACC×post	0.096***	0.097***	0.084***	0.089***	0.079***	0.195***	0.126***	
	(0.022)	(0.019)	(0.018)	(0.019)	(0.019)	(0.057)	(0.036)	
ACC						-0.011	0.031***	
						(0.014)	(0.008)	
N of clusters (firms)	3420	3581	3579	3612	3607	3319	2436	
N of clusters (Bank-Month)				470				
Observations	67,045	70,354	70,482	70,721	69,887	55,039	43,732	
R^2	0.77	0.61	0.45	0.48	0.58	0.34	0.47	
Covariates	yes	yes	yes	yes	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Bank-Firm FE					yes			
Industry-Qtr FE	yes	yes	yes	yes	yes			yes
Firm FE	yes	yes	yes	yes				yes
Rating-Ind-Loc-Time FE						yes		
Size-Ind-Loc-Time FE							yes	

Note: This table presents results from estimating equation 2. Columns 1-3 change the main dependent variable: instead of using the firm's average debt in 2011 to scale the firm's current total debt (i.e. $\overline{Debt}_{f2011}$ in the formula $g_{ft} = (\sum_{b=1} Debt_{fbt})/\overline{Debt}_{f2011} - 1$), we use average debt in 2010, the first semester of 2011 and the second semester, respectively. Column 4 clusters the standard errors by both firm and by bank-month. Column 5 adds a bank-firm fixed effect. Columns 6 and 7 replace the firm fixed effect with alternative fixed effects: {Rating notches or Size terciles} x Industry x Location x Month, where location is Département (∼ US county). Column 8, for single-bank firms only, compares the effect of the policy on the control group (rated 5+) with a similarly ineligible rating group one notch below (rated 5), to examine whether ACC firms are receiving credit by crowding out the most closely comparable but ineligible firms (i.e. the 5+ firms). *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 4
Effect of the LTRO-ACC policy by depth of bank-firm relationship

	Single-bank (ACC vs. 5+)		Multi-bank (ACC vs. 5)	
	(1) D=1 if LR>median	(2) D=1 if HHI<median	(3) D=1 if LR>median	(4) D=1 if HHI<median
ACC×post×D	0.069*	0.117***	-0.008	0.027
	(0.036)	(0.036)	(0.036)	(0.038)
ACC×post	0.036	0.014	0.093***	0.076***
	(0.024)	(0.018)	(0.027)	(0.025)
post×D	-0.001	0.055**	-0.016	0.031
	(0.024)	(0.027)	(0.030)	(0.032)
Covariates	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
N of clusters (firms)	2675	2675	3612	3612
Observations	55,568	55,568	70,721	70,721
R^2	0.45	0.45	0.48	0.48

Note: This is a triple difference estimation as in equation (4), with percentage change in a firm's total bank debt as the dependent variable. D indicators are proxies for bank-firm relationship quality or depth. LR is the length of the lending relationship between the firm and its bank - above median indicates a relatively long firm-bank relationship. We also decompose each firm's bank financing into five categories: short-term credit, medium and long-term credit, accounts receivables financing, leasing, and undrawn credit lines. Using the share of each lending type we compute the firm's Herfindahl index (HHI) to measure the degree of concentration across products. An HHI measure below median (i.e. less concentrated across product types) is thus an indicator of a lending relationship with a wider scope, because the lender and the borrower interact across a greater range of financing products, generating more soft information for the bank. For multi-bank firms the LR and HHI measures are calculated for the main bank. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 5

Effect of the LTRO-ACC policy on long and short term debt

	Single-bank(ACC vs. 5+)		**D=1 if HHI<median**		**Multi-bank(ACC vs. 5)**		**D=1 if HHI<median**	
	(1) g(ST)	(2) g(MLT)	(3) g(ST)	(4) g(MLT)	(5) g(ST)	(6) g(MLT)	(7) g(ST)	(8) g(MLT)
ACC×post×D			0.164	0.139***			-0.084	0.096
			(0.388)	(0.043)			(0.205)	(0.078)
ACC×post	0.225*	0.063***	0.150	-0.006	0.280***	-0.010	0.365**	-0.057
	(0.131)	(0.021)	(0.367)	(0.019)	(0.100)	(0.039)	(0.177)	(0.057)
post×D			-0.639**	0.019			-0.301*	-0.064
			(0.254)	(0.033)			(0.175)	(0.074)
Covariates	yes	yes	yes	yes	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
N of clusters (firms)	1381	2417	1381	2417	2818	3117	2818	3117
Observations	19,317	50,299	19,317	50,299	44,536	59,410	44,536	59,410
R^2	0.51	0.60	0.51	0.60	0.54	0.63	0.54	0.63

Note: This is a triple difference estimation as in equation (4), but in this table we split our main dependent variable (percentage change in a firm's total bank debt) separately into percentage change in short term debt (initial maturity below one year, denoted by *g(ST)*) and medium/long term debt (denoted by $g(MLT)$) respectively. The D variable is an indicator for a relatively deep (wide scope) banking relationship. We decompose each firm's bank financing into five categories: short-term credit, medium and long-term credit, accounts receivables financing, leasing, and undrawn credit lines. Using the share of each lending type we compute the firm's Herfindahl index (HHI) to measure the degree of concentration across products. An HHI measure below median (i.e. less concentrated across product types) is thus an indicator of a lending relationship with a wider scope, because the lender and the borrower interact across a greater range of financing products, generating more soft information for the bank. For multi-bank firms the HHI measure is calculated for the main bank. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 6

Effect of the LTRO-ACC policy conditional on observables

	High Leverage		**Low Tangibles**		**Young**		**Small**	
	(1) Single	(2) Multi	(3) Single	(4) Multi	(5) Single	(6) Multi	(7) Single	(8) Multi
ACC×post×D	-0.089**	-0.027	-0.096***	0.010	-0.089**	0.041	-0.055*	0.001
	(0.039)	(0.042)	(0.030)	(0.046)	(0.038)	(0.069)	(0.033)	(0.059)
ACC×post	0.092**	0.081**	0.091***	0.090***	0.083***	0.086***	0.093***	0.089***
	(0.037)	(0.038)	(0.023)	(0.021)	(0.021)	(0.019)	(0.023)	(0.020)
post×D	-0.135***	-0.135***	-0.007	0.020	-0.027	-0.053	0.008	-0.007
	(0.032)	(0.037)	(0.026)	(0.035)	(0.022)	(0.052)	(0.024)	(0.053)
Covariates	yes	yes	yes	yes	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
N of clusters (firms)	2675	3612	2675	3611	2675	3612	2675	3612
Observations	55,568	70,721	55,568	70,699	55,568	70,721	55,568	70,721
R^2	0.45	0.48	0.45	0.48	0.45	0.48	0.45	0.48

Note: This is a triple difference estimation as in equation (4), with percentage change in a firm's total bank debt as the dependent variable. D indicators are proxies for weak observable firm characteristics. *High Leverage* is an indicator equal to one (D=1) for firm with average leverage in 2011 above the sample median. *Low Tangibles* is an indicator equal to one for firm with ratio of tangible assets to total assets in 2011 in the bottom quintile of the distribution. *Young* is an indicator equal to one if firm age is no greater than five years in 2011. *Small* is an indicator equal to one for firms with less than ten employees in 2011. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 7

Effect of the LTRO-ACC policy on Prob[credit rating falls two notches]

	Single-bank (ACC vs 5+)			**Multi-bank (ACC vs 5)**		
	(1)	(2)	(3)	(4)	(5)	(6)
ACC×postJune	-0.003**			0.000		
	(0.001)			(0.002)		
ACC×2012q2		0.002			-0.000	
		(0.002)			(0.003)	
ACC×2012q3		0.000	-0.001		-0.001	-0.001
		(0.002)	(0.002)		(0.004)	(0.004)
ACC×2012q4		-0.003	-0.004**		0.005*	0.005**
		(0.002)	(0.002)		(0.003)	(0.002)
ACC×2013q1		-0.001	-0.002		-0.004	-0.004
		(0.002)	(0.002)		(0.004)	(0.003)
Covariates	yes	yes	yes	yes	yes	yes
Bank-Time FE	yes	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
N of clusters (firms)	2743	2743	2743	3611	3611	3611
Observations	37,851	40,456	40,456	50,274	50,274	50,274
R^2	0.09	0.09	0.09	0.18	0.18	0.18

Note: This table presents difference in differences estimates as in Equation 2, where the dependent variable is an indicator equal to one in the month the firm's credit rating hits two notches below its December 2011 rating, if this occurs, and zero otherwise. The sample period begins in January 2012 because all firms are constrained to being rated either 4 (ACC) or 5/5+ in December 2011. The single-bank (multi-bank) control group is composed of firms rated 5+ (5), as per our baseline specification. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 8

Effect of the LTRO-ACC policy on defaults on debt to suppliers

	Single-bank (ACC vs 5+)			Multi-bank (ACC vs 5)		
	2011m3–2013m2	2011m3–2013m12		2011m3–2013m2	2011m3–2013m12	
	(1)	(2)	(3)	(4)	(5)	(6)
ACC×post	-0.012*	-0.015**		-0.022	-0.019	
	(0.007)	(0.006)		(0.034)	(0.027)	
ACC×pre			0.004			-0.012
			(0.006)			(0.015)
ACC×$1_{t>2012m2\,\&\,t\leq 2012m8}$			-0.002			-0.053
			(0.007)			(0.060)
ACC×$1_{t>2012m8\,\&\,t\leq 2013m2}$			-0.019			-0.014
			(0.012)			(0.016)
ACC×$1_{t>2013m2}$			-0.017**			-0.011
			(0.009)			(0.013)
Covariates	yes	yes	yes	yes	yes	yes
Bank FE	yes	yes	yes	yes	yes	yes
Industry-time FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Num. clustering firms	2,746	2,746	2,746	3,674	3,706	3,706
Observations	59,194	77,296	77,296	73,700	97,418	97,418
R^2	0.13	0.14	0.14	0.14	0.12	0.12

Note: This table presents difference in differences estimates as in Equation 2, where the dependent variable is total amount defaulted on as a proportion of accounts payable. A payment default is defined as a failure to pay a debt ("trade bill") to a supplier in full and/or on time. We scale the amount defaulted on by the firm's (yearly) payables, and make the monthly numerator into a yearly figure by multiplying it by twelve. Columns 1 and 5 show that there is no pre-trend as the effect is insignificant in the year prior to the reform. Columns 2 and 3 (for single-bank) and 6 and 7 (for multi-bank) provide the main estimates of the effect. Columns 4 and 8 provide a quarter by quarter breakdown for the effect. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 9

Real and Financial effects from annual data

	Financial effects				**Real effects**	
	(1) Leverage	(2) ICR	(3) Apparent CoD	(4) Asset growth	(5) Investment	(6) Employment
Single-bank firms						
ACC×post	0.023***	2.019**	-0.003*	0.011**	0.026***	-0.060
	(0.004)	(0.799)	(0.002)	(0.005)	(0.009)	(0.157)
Covariates	yes	yes	yes	yes	yes	yes
Bank-Post FE	yes	yes	yes	yes	yes	yes
Industry-Post FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
N of clusters (firms)	2590	2493	2577	2590	2590	2570
Observations	5,180	4,986	5,154	5,180	5,180	5,140
R^2	0.95	0.84	0.85	0.70	0.62	0.98
Multi-bank firms						
ACC×post	0.014***	-2.476**	-0.003*	0.006	0.009	0.602**
	(0.004)	(0.995)	(0.002)	(0.007)	(0.009)	(0.245)
Covariates	yes	yes	yes	yes	yes	yes
Bank-Post FE	yes	yes	yes	yes	yes	yes
Industry-Post FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
N of clusters (firms)	3686	3604	3680	3686	3683	3674
Observations	7,372	7,208	7,360	7,372	7,366	7,348
R^2	0.90	0.85	0.83	0.68	0.60	0.99

Note: This table presents difference in differences estimates using annual accounting data, where fiscal years 2010-11 are collapsed into a single pre-period, and 2012-13 into a single post-period: $LHS_{ft} = \alpha_f + ACC_f \times Post_t + Bank_b \times Post_t + Industry_f \times Post_t + \Gamma' X_{ft}$. f indexes firm, t indexes time (pre or post). Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Covariates also include lagged sales growth for the real effects regressions (investment and employment) as a proxy for investment opportunities as per standard investment regressions. Asset growth = ln(Tot Asset) - ln(Tot Asset_lag); Interest Coverage Ratio (ICR) = EBITDA / Interest Expenses; Apparent Cost of Debt (CoD) = Interest Expenses / Financial debt; Investment = ΔFixed Assets / Fixed Assets; Employment = N of employees. All variables are winsorized at 1% except ICR and CoD that are winsorized at the 2% level. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 10
Exploring the spillover

					D=1 if increase N bank		D=1 if decrease HHI			
	(1) Baseline	(2) Mainbank	(3) g(ST)	(4) g(MLT)	(5) g(ST)	(6) g(MLT)	(7) g(ST)	(8) g(MLT)	(9) 6m stable	(10) 12m stable
Rating 5+×post	0.075***	0.070***	0.168**	-0.062	0.088	-0.087*	0.081	-0.076	0.092***	0.115***
	(0.020)	(0.020)	(0.085)	(0.041)	(0.098)	(0.046)	(0.101)	(0.047)	(0.023)	(0.027)
Rating 5+×post×D					0.415**	0.133	0.364**	0.060		
					(0.182)	(0.089)	(0.168)	(0.086)		
post×D					-0.355***	-0.070	-0.290**	-0.035		
					(0.127)	(0.076)	(0.128)	(0.077)		
Bank-Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
N of clusters (firms)	2280	2279	1985	1917	1985	1917	1985	1917	1702	1251
Observations	43,637	43,438	38,446	35,410	38,446	35,410	38,446	35,410	32,168	23,170
R^2	0.50	0.46	0.36	0.67	0.36	0.67	0.36	0.67	0.51	0.52

Note: This table presents difference in differences estimates as in Equation 2, but where the "treated" group are now 5+ rated firms (just ineligible) and the control firms are now 5 rated (also ineligible, but one notch lower than 5+ firms). The dependent variable is percentage change in a firm's total bank debt. This table is estimated for multi-bank firms only (there are no effects for single-bank firms). Column 2 restricts the dependent variable to bank debt with the firm's main bank only. Column 3 restricts the dependent variable to short-term debt only, while column 4 restricts it to medium and long-term debt. Columns 5-8 are a triple difference estimation as in equation (4), with the D indicator =1 if the firm increased its number of banks (columns 5-6) or if the firm's HHI across lenders decreased (columns 7-8), both in the preceding year . Columns 9 (and 10) are estimated on samples that have 6 months (1 year) of no change in their rating, to rule out the effect being driven by rating changes. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 11
General effect of the LTRO on debt: effects on always-eligible firms

	Single-bank (Eligible vs. 5+)		**Multi-bank (Eligible vs. 5)**	
	(1)	(2)	(3)	(4)
Rating(3++ and 3+)×post	0.112***		0.106***	
	(0.026)		(0.029)	
Rating(3 and 4+)×post	0.127***		0.127***	
	(0.020)		(0.020)	
Eligible ×post		0.123***		0.123***
		(0.018)		(0.019)
Bank-Month FE	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Covariates	yes	yes	yes	yes
N of clusters (firms)	4057	4057	4371	4371
Observations	83,943	83,943	87,090	87,090
R^2	0.48	0.48	0.51	0.51

Note: This table presents difference in differences estimates as in Equation 2, with percentage change in a firm's total bank debt as the dependent variable. Note that for this table we define groups using their ratings as of September 2011, because firms with ratings above 4/ACC were eligible to be used as collateral for the October 2011 one-year LTRO. Columns 1 and 3 extend the specification to have two treated groups (3++ and 3+ rated firms; and separately, 3 and 4+ rated firms) but they retain a single control group (firms rated 5+ for single-bank firms and 5 for multi-bank firms). Each rating within a treatment group has the same haircut applied when loans from those firms are used as collateral in the Eurosystem. Columns 2 and 4 collapse both treated groups into a single group. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 12
Bank heterogeneity

	Single-Bank (All Eligible vs 5+)				
	(1) High funding cost	(2) Low tier 1	(3) Large stock price Δ^-	(4) High eligible share	(5) Low free collateral
(All Eligible)×post	0.096***	0.077***	0.119***	0.109***	0.088***
	(0.026)	(0.027)	(0.021)	(0.019)	(0.022)
(All Eligible)×post×Interaction	0.023	0.037	-0.022	-0.017	0.027
	(0.034)	(0.033)	(0.034)	(0.032)	(0.030)
Covariates	yes	yes	yes	yes	yes
Bank-Time FE	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
N clusters (firm,bank-month)	4,566	5,649	4,566	5,649	5,649
Observations	95,325	117,225	95,325	117,225	117,225
R^2	0.48	0.47	0.48	0.47	0.47
	Multibank (All Eligible vs 5)				
	(6) High funding cost	(7) Low tier 1	(8) Large stock price Δ^-	(9) High eligible share	(10) Low free collateral
(All Eligible)×post	0.075***	0.120***	0.089***	0.094***	0.127***
	(0.025)	(0.024)	(0.021)	(0.019)	(0.021)
(All Eligible)×post×Interaction	0.020	-0.016	-0.000	0.036*	-0.032*
	(0.023)	(0.021)	(0.024)	(0.019)	(0.019)
Covariates	yes	yes	yes	yes	yes
Bank-Time FE	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
N clusters (firm,bank-month)	5,566	6,859	5,566	6,859	6,859
Observations	111,837	138,025	111,837	138,025	138,025
R^2	0.50	0.49	0.50	0.49	0.49

Note: This is a triple difference estimation as in equation (4), with percentage change in a firm's total bank debt as the dependent variable. The interaction terms are named above each column. The first three are proxies for firms whose (main) bank has a relatively weaker balance sheet; specifically, above median funding costs (columns 1 and 6), below median Core Tier 1 capital (columns 2 and 7), or above median drop in stock prices (columns 3 and 8). The remaining two interactions identify firms whose (main) bank has differential ability to take advantage of the LTRO funding: above median share of eligible loans in their December 2011 lending portfolio (columns 4 and 9), and below median unused collateral (columns 5 and 10). The sample consists of all SMEs in eligible credit ratings (i.e., 4/ACC to 3++), and the control groups (5+ for single-bank, 5 for multi-bank). Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Data sources and details for this table are in the Appendix. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

B Appendix to Unconventional Monetary Policy and Bank Lending Relationships

By Christophe Cahn, Anne Duquerroy, and William Mullins

Table 13
Effect of the LTRO-ACC policy on Leverage

	Single-bank (ACC vs. 5+)			**Multi-bank**	
	(1) Firm, Month FE	(2) Bank x Month	(3) Ind x Quarter	(4) ACC vs. 5+	(5) ACC vs. 5
ACC×post	0.017***	0.015***	0.014***	0.007***	0.020***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Covariates	yes	yes	yes	yes	yes
Bank-Month FE		yes	yes	yes	yes
Industry-Qtr FE			yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
Month FE	yes				
N of clusters (firms)	2204	2201	2201	3962	3126
Observations	47,830	47,286	47,284	79,787	62,791
R^2	0.91	0.92	0.92	0.84	0.84

Note: This table presents difference in differences estimates of the effect of the LTRO-ACC policy on the leverage of SMEs. We estimate the following equation: $L_{ft} = \alpha_f + \beta\ (ACC_f \times Post_t) + \Lambda_{bt} + \Upsilon_{It} + \Gamma' X_{f,y-1} + \epsilon_{ft}$ where f indexes firm, I indexes industry, b indexes banks, t denotes time in months, and y fiscal year. The dependent variable is total leverage, obtained by summing the firm's debt across all banks, and scaling by total assets: $L_{ft} = (\sum_{b=1}\ Debt_{fbt})/TA_{f2011}$, where TA_{f2011} is the firm's total asset value in 2011. Note that because the regression is at the firm-month level there is no k subscript for the dependent variable. The ACC_f indicator takes a value of one for any firm with a rating of 4 as of December 2011 and zero otherwise, thus identifying the newly-eligible firms that make up the treated group. *Post* is a post-treatment indicator equal to 1 in each month after February 2012. α_f are firm fixed effects; Λ_{bt} are bank–month fixed effects; Υ_{It} are industry-quarter fixed effects. $X_{f,y-1}$ are firm characteristics obtained from the previous year's accounting data: ln(total assets); tangible assets / total assets; EBITDA / total assets. The sample consists of all 4-rated firms (newly-eligible borrowers or "ACC firms", i.e., treated firms) and 5+ rated firms (closest ineligible borrowers on the internal Credit Risk Rating scale of the Banque de France) as rated in December 2011 that meet the data requirements detailed in the text, and have at least 5% leverage in 2011. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 14
Robustness to differing sample definitions

	Single-bank (ACC vs. 5+)				**Multi-bank (ACC vs. 5)**			
	(1) Baseline	(2) Nov11	(3) Oct11	(4) Sep11	(5) Baseline	(6) Nov11	(7) Oct11	(8) Sep11
ACC×post	0.082***	0.081***	0.073***	0.078***	0.089***	0.099***	0.101***	0.103***
	(0.018)	(0.018)	(0.020)	(0.019)	(0.019)	(0.019)	(0.018)	(0.019)
Covariates	yes	yes	yes	yes	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
N of clusters (firms)	2675	2629	2605	2620	3612	3564	3550	3543
Observations	55,568	54,583	54,048	54,373	70,721	69,724	69,371	69,124
R^2	0.44	0.44	0.52	0.51	0.48	0.47	0.48	0.55

Note: This table presents results from estimating equation 2; the dependent variable is percentage change in a firm's total bank debt. Columns 1 and 5 provide our baseline results based on firms being assigned to rating groups (i.e. treatment and control categories) based on their ratings as of December 2011. The other columns estimate the same equation using samples defined by firms' ratings as of earlier months: September, October, and November 2011. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 15

Robustness of policy effects conditional on observables (for single-bank firms)

	6-month rating stability				**12-month rating stability**			
	(1) High lev	(2) Low Tan	(3) Young	(4) Small	(5) High lev	(6) Low Tan	(7) Young	(8) Small
ACC×post×D	-0.0767	-0.1066***	-0.0949**	-0.0566	-0.0262	-0.1173***	-0.1113**	-0.0678
	(0.0468)	(0.0352)	(0.0446)	(0.0391)	(0.0560)	(0.0357)	(0.0468)	(0.0447)
ACC×post	0.0859*	0.1004***	0.0887***	0.0986***	0.0461	0.0915***	0.0829***	0.0950***
	(0.0445)	(0.0269)	(0.0246)	(0.0277)	(0.0538)	(0.0320)	(0.0294)	(0.0335)
post×D	-0.1556***	0.0097	-0.0158	-0.0035	-0.1802***	-0.0130	-0.0151	0.0127
	(0.0389)	(0.0290)	(0.0266)	(0.0273)	(0.0474)	(0.0332)	(0.0316)	(0.0334)
Covariates	yes	yes	yes	yes	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
N of clusters (firms)	2175	2175	2175	2175	1633	1633	1633	1633
Observations	45,713	45,713	45,713	45,713	34,286	34,286	34,286	34,286
R^2	0.44	0.43	0.43	0.43	0.43	0.43	0.43	0.43

Note: The dependent variable is percentage change in a firm's total bank debt. For the purposes of robustness testing in this table we require 6 or 12 month rating stability of firms. $HighLeverage$ is an indicator equal to 1 for firm with average leverage in 2011 above the sample median. $LowTangibles$ is an indicator equal to 1 for firm with ratio of tangible assets to total assets in 2011 in the bottom quintile of the distribution. $TradeCreditUsers$ is an indicator equal to 1 for firms which are net credit users i.e. (Payables - receivables) > 0. $Young$ is an indicator equal to 1 if firm age is no greater than 5 years in 2011. $Small$ is an indicator equal to 1 for firms with less than 10 employees in 2011. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 16

Effect of the LTRO-ACC policy on high growth ("Gazelle") and young firms

	Single-bank firms (ACC vs. 5+)		**Multi-bank firms (ACC vs. 5)**	
	(1) G=1 if Gazelles	(2) G=1 if High Sales	(3) G=1 if Gazelles	(4) G=1 if High Sales
ACC×post×G	0.072	0.216**	0.225*	0.067
	(0.223)	(0.090)	(0.131)	(0.078)
ACC×post	0.080***	0.069***	0.076***	0.076***
	(0.020)	(0.020)	(0.020)	(0.021)
post×G	0.056	-0.108*	-0.129	-0.001
	(0.204)	(0.060)	(0.118)	(0.061)
Covariates	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
N of clusters (firms)	2297	2297	3277	3277
Observations	48,040	48,040	64,225	64,225
R^2	0.44	0.44	0.47	0.47

Note: This is a triple difference estimation as in equation (4), with percentage change in a firm's total bank debt as the dependent variable. D indicators are proxies for high growth firms. *Gazelle* is an indicator equal to one when firm sales growth is 10% or greater for each of 2009, 2010 and 2011. *HighSales* is an indicator equal to one if the sales to total assets ratio is in the top quintile in 2011. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 17

Effect of the LTRO-ACC policy on financially constrained firms

	Single-bank (ACC vs. 5+)		**Multi-bank (ACC vs. 5)**	
	(1) D=1 if Undrawn=0	(2) D=1 if NetTC user	(3) D=1 if Undrawn=0	(4) D=1 if NetTC user
ACC×post×D	-0.096**	-0.076*	-0.067*	0.056
	(0.038)	(0.040)	(0.039)	(0.037)
ACC×post	0.133***	0.120***	0.110***	0.066**
	(0.033)	(0.034)	(0.023)	(0.026)
post×D	-0.031	0.007	0.000	-0.035
	(0.026)	(0.034)	(0.032)	(0.032)
Covariates	yes	yes	yes	yes
Bank-Month FE	yes	yes	yes	yes
Industry-Qtr FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
N of clusters (firms)	2675	2675	3612	3612
Observations	55,568	55,568	70,721	70,721
R^2	0.45	0.45	0.48	0.48

Note: This is a triple difference estimation as in equation (4), with percentage change in a firm's total bank debt as the dependent variable. D indicators are proxies for financial constraints. In odd-numbered columns D is a dummy variable equal to one when firms have some undrawn credit lines. In even-numbered D is a dummy variable equal to one when firms are net users of trade credit i.e. when (Accounts Receivables - Account Payables) < 0. Covariates are firm characteristics (size, tangibility, and profitability) lagged by one year. Standard errors are clustered by firm, and are reported in brackets; *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Variables used in Table 12: Bank heterogeneity

The set of bank groups consists of 3 mutual banks (Credit Mutuel (CM), Credit Agricole (CASA), BPCE), and 3 commercial banks (Societe Generale (SG), BNP-Paribas (BNPP), HSBC). These bank groups capture over 97% of observations in our final sample, and over 90% of total drawn credit in the French Credit Registry as of December 2011.

- Free Collateral: This is computed as 1-RO/Collateral, where RO is each bank's ECB liquidity as of Dec. 2011 (computed as the sum of all the bank's outstanding refinancing operations with the ECB, i.e. MRO, LTRO1M, LTRO3M, LTRO6M, LTRO1Y, LTRO3Y) and Collateral is the total posted collateral, valued after haircut. Source: Banque de France Market Operation data. The low free collateral group consists of BNPP, BPCE, and HSBC.

- Funding Cost: Bank funding cost expressed in basis points as of December 2010. Source: Results of the 2011 European Bank Assessment EU-wide stress tests. Missing information for Credit Mutuel. The high funding cost group consists of BNPP and BPCE.

- Core Tier 1 capital: the ratio of a bank's core tier 1 capital (equity plus reserves) to total risk-weighted assets. Source: Bank groups' Financial Reports. The low Tier 1 capital group consists of BNPP, BPCE, and SG.

- Stock price. Source: Bloomberg. Missing information for Credit Mutuel (unlisted). The large drop in stock price group consists of CASA and SG.

- Eligible Share in Loan Portfolio: for each bank, this is the ratio of drawn credit to eligible firms (ratings 4/ACC and higher) over drawn credit to all non-financial corporations (SMEs, intermediate, and large firms, excluding real estate companies). The source is the French Credit Register, hence this variable does not take into account loans abroad. The high eligible share group consists of CM, HSBC, and SG.