Risk-Weighted Capital Requirements and Portfolio Rebalancing*

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Abstract

We use a 2013 Norwegian policy reform to study how banks react to higher capital requirements and how these adjustments transmit to the real economy. Using bank balance sheet data, we document that banks raise capital ratios mainly by reducing risk-weighted assets. The majority of the reduction in risk-weighted assets is accounted for by a reduction in average risk weights. Consistent with this reduction in risk, we document a substantial decline in credit supply to the corporate sector relative to the household sector. We also show that banks react to higher requirements by increasing interest rates, consistent with the reduction in corporate credit growth being supply driven. Using administrative loan level tax data, we document a reduction in lending on the firm level. This is robust to controlling for firm fixed effects, thereby accounting for potential firm-bank matching. Finally, we find that the reduction in bank lending has a negative impact on firm employment growth.

JEL-codes: E51, G21, G28

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1 Introduction

Bank regulation has been high on the policy agenda since the financial crisis. An important component of the post-crisis policy reforms has been higher capital requirements for banks. The EU is scheduled to fully implement the Basel III regulation on capital requirements by 2019, and several member countries have already started increasing required capitalization levels. Similar policies have been adopted in the US, and further amendments are being discussed on both sides of the Atlantic[1]. In order to understand how capital requirements affect not only the bank sector, but also the broader economy, it is crucial to identify through which channels banks react to stricter regulation. Banks can respond not only by increasing equity, but also by reducing risk-weighted assets. While the former has been referred to as good deleveraging (Gropp, Mosk, Ongena, and Wix 2017), the latter is likely to adversely affect at least some sectors of the economy.

Capital requirements for Norwegian banks increased substantially in 2013[2]. Low-capitalized banks had to increase their capitalization levels in order to fulfill the new requirements. High-capitalized banks on the other hand, were not directly affected. As a result, ample cross-sectional variation in capital ratio growth rates emerged. At the same time, many Norwegian banks suddenly reduced their corporate credit growth. A closer look at the data reveals that this reduction in credit supply was entirely accounted for by the low-capitalized banks - which had to increase their capital ratios following the reform. To illustrate the divergence that took place in 2013, we plot corporate credit growth for low-capitalized and high-capitalized banks in Figure 1. The picture is striking. While low-capitalized and high-capitalized banks look virtually identical prior to the reform, there is a large gap opening up as capital requirements are increased in early 2013. The less affected high-capitalized banks keep their credit growth roughly constant, while credit growth for low-capitalized banks plummets to negative levels. This divergence suggests that banks which increased their capital ratios due to the reform did so, at least partly, by reducing credit growth to the *firm* sector. Interestingly, credit growth to the *household* sector reveals no similar pattern. This differential effect across sectors is the main finding of the paper.

[1]See for example The Minneapolis Plan to End Too Big to Fail (November 2016), The Financial CHOICE Act (2016) and discussions surrounding a new Basel accord.

[2]The 2013 reform was the Norwegian implementation of Basel III - which constitutes changes to both capital and liquidity regulation. An advantage of studying the Norwegian reform in 2013 is that it only incorporated changes to the capital requirements. We discuss this in more detail in Section 2.1.

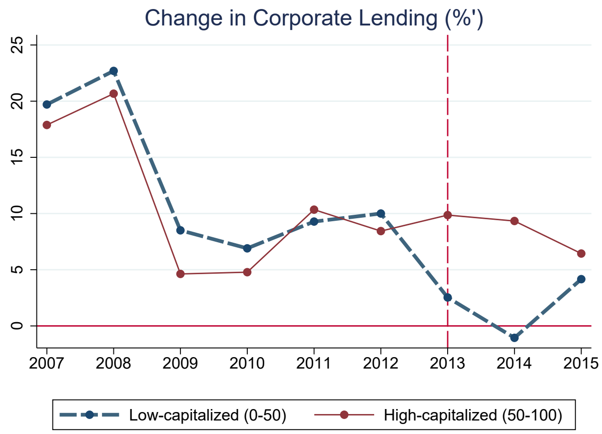

Figure 1: Corporate lending growth for low-capitalized and high-capitalized banks. We divide banks into groups based on their 2012 capital ratio.

We use the 2013 Norwegian policy reform to decompose the increase in capital ratios into increases in equity, reductions in total assets and reductions in average risk weights. Further, we evaluate whether changes in average risk weights can be explained by a shift from corporate lending growth to household lending growth. Finally, we look for spillover effects to the real economy in terms of employment growth.

The challenge in figuring out how banks respond to increased capital requirements is to establish an appropriate counterfactual. That is, one has to tease out how banks would have changed their equity, assets and average risk weights in absence of new regulation. The methodology we use is motivated by the striking pattern seen in Figure 1. Although the Norwegian requirements implemented in 2013 were levied on all banks, they affected banks differentially due to their pre-reform capital ratios. That is, low-capitalized banks had to increase their capitalization levels, whereas high-capitalized banks did not. Informally, our main identification relies on the fact that low-capitalized and high-capitalized banks look very similar prior to the reform. This is captured by the two lines in Figure 1 following each other closely up until 2013. In other words, high-capitalized banks seem to be an appropriate control group for low-capitalized banks. More formally, we use a flexible difference in difference methodology to identify the effect of the reform. This has two main advantages. First and most importantly, it allows us to explicitly test the standard assumption of parallel trends. For all our outcome variables, we confirm that low-capitalized and high-capitalized banks are similar prior to the reform, ensuring that they are not on different growth trajectories. We conclude that although low-capitalized and high-capitalized banks may differ along observable dimensions, they do not differ in terms of credit growth *prior* to the reform. Secondly, the flexible difference in difference framework allows us to map out dynamic treatment effects and thereby be agnostic about the exact timing of the reform. The dynamic treatment effects are important because they allow us to also identify short-lived responses. For example, we find that banks react to higher requirements by increasing the growth in equity, but that this effect is small in size and limited to a one-year period. If we instead estimate average treatment effects over the entire post-reform period, we are not able to pick up a statistically significant effect on equity. Being agnostic about when the reform starts is also useful, as we do not have to impose a reform date explicitly, based on our priors. While the reform was implemented in the third quarter of 2013, it was announced in the first quarter of 2013. Hence, it is ex ante unclear in which quarter banks started reacting to the reform. Further, some variables are slow-moving in the sense that they are generally decided upon at the annual general assembly and apply to one calendar year at the time. These variables may start reacting only with the beginning of a new calendar year (i.e. the first quarter of 2014). By estimating dynamic treatment effects, we can let the data tell us when the reform effects occurred for different variables.

While the flexible difference in difference analysis can be done using bank level data, the loan level data enables us to achieve even tighter identification. Following Khwaja and Mian (2008) we make use of firms borrowing from more than one bank and include firm×year fixed effects in our regressions. This means that we can estimate the impact of higher capital requirements on lending to the corporate sector, while holding firm×year characteristics fixed. If one accepts that, within a given year, credit demand is determined at the firm level (and not at the loan level), this explicitly controls for any demand effects. Note that including firm×year fixed effects in our dynamic setup results in even tighter identification than the standard cross-sectional Khwaja and Mian (2008) estimator. Finally, our results are robust to including a wide range of control variables.

Our data comes from three main sources. First, we have quarterly bank level balance sheet data from The Norwegian Banks Guarantee Fund. Second, we have matched firm-bank data from the Norwegian Tax Authorities. Here we observe debt, deposits and interest paid/received for the universe of Norwegian limited liability firms and all their (domestic) bank connections. Finally, we use firm level data from a national public register to obtain employment data on the firm level.

We find that growth in equity accounts for 14 percent of the reform-induced increase in capital ratios. However, this channel is not statistically significant, as the increase in equity appears to be relatively short lived. Concentrating on the first calendar year after the reform, the impact on equity is modest in size, but statistically significant. Capital ratios are mainly increased by reducing the growth in risk-weighted assets. 38 percent of the reform-induced increase in capital ratios is due to lower growth in total assets, and 48 percent is due to a reduction in average risk weights. Hence, shifting from assets with high risk weights to assets with low risk weights is the quantitatively most important channel in explaining the increase in capital ratios following the reform.

The average risk weight on mortgage lending is 0.35 (Andersen 2013), compared to an average risk weight of roughly 1.0 for corporate lending (Andersen and Winje 2017). Hence, shifting credit supply from firms to households is an efficient way to reduce average risk weights. Consistent with this, we find an economically and statistically significant impact on lending growth to the corporate sector. A one percentage point higher growth rate in capital ratios is found to reduce corporate credit growth by 1.0-1.5 percentage points. We find no evidence of a reduction in household lending over the same time period, implying that low-capitalized banks are increasing their relative share of household lending. Back-of-the-envelope calculations suggest that the shift from corporate credit supply to household credit supply can account for roughly 80 percent of the reduction in average risk weights. On the loan level, we find that firms which borrow from low-capitalized banks prior to the reform have lower credit growth and lower employment growth in the post-reform period. The negative employment effect is driven by smaller firms. Consistent with the reduction in corporate credit being supply driven, we also document an increase in corporate interest rates for the low-capitalized banks.

Before discussing the existing literature, it is worthwhile to highlight some reasons why sectoral credit allocation is important. First, our results have implications for the effectiveness of the countercyclical capital buffer, which has been introduced in many countries following the financial crisis. The main goal of the capital buffer is to make banks increase their capitalization levels in good times. But it has also been suggested that the countercyclical capital buffer could potentially have a second benefit, in terms of dampening the boom in household credit (Ministry of Finance 2016). Our results suggest that banks reduce corporate lending rather than household lending when faced with increased requirements, suggesting that any dampening effect on household credit would be limited. Note however, that our results are conditional on the current risk weights. Reducing the difference in risk weights between mortgage lending and corporate lending would probably imply that more of the reduction in credit supply would be directed towards the household sector. Second, the importance of the sectoral allocation of credit for the macro economy is not limited to credit booms. The reduction in credit to the corporate sector is found to reduce employment growth. More generally, redirecting credit from the firm sector to the household sector may have detrimental effects on the growth potential of the economy (Beck, Büyükkarabacak, Rioja, and Valev 2012). Hence, our results highlight the importance of not only considering the impact on total credit, but also the impact on the *allocation* of credit, when discussing the design of risk-weighted capital requirements.

Literature Since the financial crisis, several countries have changed their capital requirements, resulting in a handful of recent papers on the topic. Brun, Fraisse, and Thesmar (2013) use variation in internal risk models among French banks, and document significant effects on corporate lending from increasing risk-weighted capital requirements. Jimenez, Ongena, Peydro, and Saurina (2016) evaluate the effect of time-varying capital requirements based on individual credit portfolios in Spain, and reach similar conclusions. Studies based on bank specific capital requirements in the UK also document significant credit supply effects (Bridges, Gregory, Nielsen, Pezzini, Radia, and Spaltro 2014, Aiyar, Calomiris, and Wieladek 2016). De Jonghe, Dewachter, and Ongena (2016) uses idiosyncratic variation in capital requirements and find significant credit supply effects for loans with relatively high capital charges. The paper most similar to ours is perhaps Gropp, Mosk, Ongena, and Wix (2017), who use variation in capital requirements based on country-specific size cutoffs in a group of European countries. They show that banks adjust to capital requirements by reducing risk weighted assets rather than increasing equity. They find that banks in response to increased capital requirements shrink their assets, especially on corporate loans.

We contribute to this recent literature in three important ways. First, using a flexible difference-in-difference approach we can uncover novel evidence on the dynamics of banks' adjustments to increased capital requirements. For example, we are able to identify a short-lived effect on equity which is not evident when considering the full post-reform period. Second, having established that a reduction in risk-weighted assets is an important margin of adjustment for banks, we dig deeper into *how* banks reduce risk-weighted assets. We focus on how banks decrease corporate lending *relative* to household lending, as a way of reducing the average risk weight on their portfolio. We show that the shift from corporate lending to household lending can explain roughly 80 percent of the observed decline in average risk weights[3].

[3]Note that because risk weights vary across several dimensions, it is possible that credit is redirected from the corporate sector to the household sector, even if there is no effect on average risk weights. Di-

Third, and most importantly, we document that the increase in capital ratios has negative spillover effects to employment using data on a much wider set of firms than what is typically used in the literature[4]. Most of the existing literature, such as Gropp, Mosk, Ongena, and Wix (2017), uses data on syndicated loans, a debt market typically skewed towards bigger and less bank-dependent firms. Evaluating the real effects of capital requirements is therefore challenging, as outcomes are only evaluated for a subset of the firms in the economy. Using data on all limited liability firms, we find that the aggregate real effects of capital requirements are substantially understated if smaller firms are not considered. In fact, our employment results relate to a larger literature on credit shocks in general. Early seminal papers on this topic include Bernanke, Lown, and Friedman (1991) and Peek and Rosengren (1996). Recent contributions are for instance Chodorow-Reich (2014) and Greenstone, Mas, and Nguyen (2014), who document that bank credit was important for employment growth during the Great Recession. Greenstone, Mas, and Nguyen (2014) also look for employment effects during normal economic times, without finding any. Our results suggest that the importance of bank credit is *not* limited to episodes of economic turmoil.

Our empirical results show that banks rebalance their portfolio when capital requirements increase. We tie our findings to the optimal design of capital regulation, specifically the design of risk weights and countercyclical capital requirements. The theoretical literature on bank regulation is nicely summarized in Santos (2001) and Van Hoose (2007). In the spirit of Kim and Santomero (1988), we construct a simple model showing that capital requirements have no effect on the portfolio allocation of banks if risk weights are proportional to excess returns, which again are proportional to systematic risk. Our finding that banks reduce their average risk weights when capital requirements increase, indicates that assets with relatively low risk weights (such as mortgages) are in fact being assigned risk weights which are *too low* relative to their systematic risk[5]. An adaptive risk-weighting scheme, as suggested by Glasserman and Kang (2014), would hence imply adjusting the assigned risk weights to reduce the difference in risk weights between mortgages and corporate loans.

rectly studying the impact on firm and household credit is therefore crucial in uncovering potential portfolio rebalancing.

[4]A notable exception is Jimenez, Ongena, Peydro, and Saurina (2016). However, they (1) do not focus on the portfolio rebalancing aspect of capital requirements and (2) find that, in normal times, there is no adverse effect on firm employment of reductions in credit supply.

[5]It is possible that a social planner would in fact want to distort banks' portfolio allocations, although this has not been part of the policy discussion surrounding the design of risk-weighted capital requirements.

2 Reform and data

2.1 Reform

Regulators across the globe use minimum requirements on banks' capital ratios to ensure some level of loss-absorption capacity. Such requirements are usually risk-weighted, in order to account for differences in risk-taking across banks. Capital requirements mean that banks need to hold some amount of equity for every asset they own, or for every loan they grant[6]. Risk-weighting implies that assets with higher assigned risk weights require banks to hold more equity relative to assets with lower risk weights. In our sample, the assigned risk weights are mostly exogenous to the bank. Policy makers determine risk weights for different asset classes, and banks take these as given. The exception is so called internal ratings based (IRB) banks, which have some freedom in calculating their own risk weights. The vast majority of banks in our sample are non-IRB banks however, and our results are robust to excluding IRB-banks from the sample. Hence, we think of the assigned risk weights as being outside of the banks control and constant over time in the relevant period.

Risk weights vary both across and within asset classes. The most important dimension of variation for this paper is the difference in risk weights for lending to households relative to firms. The average risk weight on mortgages for non-IRB banks is 0.35, compared to an average risk weight on corporate loans of around 1.0 (Andersen 2013, Andersen and Winje 2017). Hence, extending a corporate loan requires the bank to hold roughly three times as much additional equity as when extending a mortgage loan.

A bank's risk-weighted capital ratio is given by equation (1). Equity is denoted by E, assets by A and risk weights by α. We use the terms *capital ratio* and *CET1-ratio* interchangeably, although they rely on slightly different definitions of capital. While capital ratios are based on equity plus hybrid capital, CET1-ratios are based on so called Tier 1 Core Equity (CET1)[7].

$$\text{Capital Ratio} = \frac{E}{\sum_i \alpha_i A_i} \tag{1}$$

Following the financial crisis of 2007/2008, the Basel III accord was put forward by the Basel Committee on Banking Supervision (BCBS 2010). The accord outlined a set of standards on capital and liquidity regulation. One of the prominent features of the Basel III

[6]We use the popular term "holding equity", although this expression is somewhat misleading. Equity is not an asset, and as such not something that banks hold, but rather a source of financing.

[7]CET1 capital consists of equity less regulatory deductions (Norges Bank 2014).

accord was to increase the lower bound on banks' capital ratios. Legislative changes based on the Basel III accord were adopted by the EU in June 2013, in the form of the Capital Requirements Directive IV (CRD IV) and the Capital Requirements Regulation (CRR).

As a member of the European Economic Area, Norway implemented the directive into its own legislation. However, because Norway is not a member of the EU, Norwegian policy makers did not participate in designing the reform. Hence, the new requirements were not tailored to the specifics of the Norwegian bank sector in any way. A challenge with isolating the effects of increased capital requirements is that CRD IV/CRR was accompanied by new liquidity requirements. For Norwegian banks however, satisfying the proposed liquidity coverage ratio (LCR) and the net stable funding ratio (NSFR) was challenging due to a lack of assets satisfying the requirements for high-quality liquid assets (HQLA). For this reason, the implementation of the new liquidity regulation was postponed, and Norwegian authorities "accorded priority to early phase-in of the new capital requirements (Ministry of Finance, 2014)[8]. We therefore believe that an advantage of investigating Norwegian banks' adaption to higher capital requirements is that we to a larger extent can isolate the effects of increased capital requirements.

The increase in capital requirements for Norwegian banks was proposed on March 22nd 2013 and adopted on the 1st of July 2013. The new requirements were phased in over a two-year period. As in the EU-legislation, CET1-capital was required to account for ten percent of risk-weighted assets. This included a minimum requirement of roughly five percent, as well as a constant buffer requirement levied on all banks. In addition, a countercyclical capital buffer was adopted. The buffer requirement can vary between 0 and 2.5 percent. All requirements came into effect on the 1st of July 2013 and were phased in incrementally. As a result, Norwegian banks faced a maximum requirement of 12.5 percent. In addition, there was an additional requirement for three systemically important banks[9]. All the requirements,

[8] A revised version of HQLA was introduced later, and the LCR is expected to be fully phased-in by 2018. With respect to the net stable funding ratio (NSFR), the Ministry plans to introduce it in 2018 (Ministry of Finance, 2016). Uncertainty surrounding the definition of HQLA lead the Norwegian authorities to postpone details on how the NSFR would be implemented in Norway. Hence, there is virtually no overlap between our sample period - which ends in 2015 - and the implementation of the new liquidity/funding requirements.

[9] On top of the general requirements, three banks (or credit institutions) were declared systemically important and subjected to an additional requirement of two percent. We only have one systemically important bank in our sample, and all our results are robust to dropping this bank. The two other systemically important institutions are not in our sample because they are not part of The Norwegian Banks Guarantee Fund. One is a Norwegian subsidiary of a Swedish bank (Nordea). The other is a public credit institution which extends loans to municipalities (Kommunalbanken).

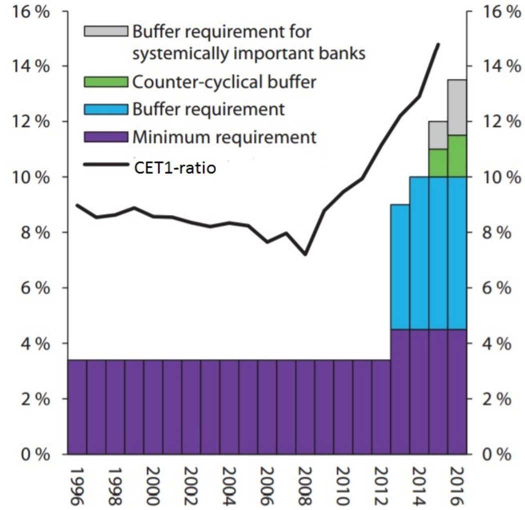

Figure 2: Risk Weighted Capital Requirements for Norwegian Banks. Source: Ministry of Finance.

along with the aggregate capital ratio, are illustrated in Figure 2[10][11].

Figure 2 documents a steady increase in capital ratios starting shortly after the financial crisis. Such increases are seen also in other European countries (Gropp, Mosk, Ongena, and Wix 2017). This increase is probably due, at least in part, to an expectation of stricter regulation. However, as documented in the next section, high-capitalized and low-capitalized banks had similar growth rates in capital ratios prior to the reform. Only after the reform do low-capitalized banks significantly increase their capitalization levels *relative* to that of high-capitalized banks[12].

2.2 Data

In our analysis on how banks respond to increased capital requirements, we use quarterly bank balance sheet data. The data is provided by The Norwegian Banks' Guarantee Fund, and contains information on nearly all Norwegian banks and subsidiaries. Foreign banks

[10]The reform of 2013 contained two types of requirements - minimum requirements and buffer requirements. Minimum requirements have to be strictly satisfied at all times. Buffer requirements can in theory be temporarily violated. If a bank's capital ratio falls below a buffer requirement, it is required to take immediate steps to get above the buffer requirement. For example, its dividend policies will be subject to strict regulation. In practice however, banks do not seem to distinguish between buffer and minimum requirements.

[11]The countercyclical capital buffer was set to 1 percent in 2015 and 1.5 percent in 2016.

[12]Bankers and policy makers have indicated that although Norwegian banks were anticipating increased requirements, the actual increase adopted in 2013 was larger than expected.

operating in Norway are not included in the dataset. These banks were also not affected by the Norwegian regulation. Foreign financial institutions account for 15 percent of total assets for banks operating in Norway. The second largest bank in Norway is the Norwegian subsidiary of the Swedish bank Nordea, which is not in our sample[13]. Nordea accounts for roughly 13 percent of the remaining bank assets. Hence, our data covers 74 percent of total bank assets in Norway, and includes 110-120 different banks - depending on the data source. We remove cases with missing observations, and a limited number of observations in which the capital ratio is reported to be zero although risk-weighted assets and equity are both positive and finite.

Our unit of observation in the bank-level analysis is the change in a given variable from quarter i in year $t-1$, to quarter i in year t. The reform was implemented in July 2013, but was proposed in March. We use Q3-2013 as our reform quarter, but it is possible that banks started reacting already in Q1-2013. Additionally, some bank responses are likely to appear at the start of the following year. The reason is that some decisions, such as dividend policies, are generally taken once a year at the general assembly, and apply to one calendar year at the time. In our main analysis we use type and quarter interactions, which allows us to be agnostic about when the reform came into effect. In terms of notation, we denote the approximate percentage change in variable X from quarter i in year $t-1$ to quarter i in year t as $\Delta log(X_{it})$.

The average (median) capital ratio prior to the reform was 16.2 (15.9) percent. The distribution is depicted by the solid blue line in Figure 3. Roughly 1/4 of the banks in our sample had capital ratios below the new (maximum) requirement of 12.5 percent. The figure shows that banks responded to the reform as expected. A year later the average (median) capital ratio had increased to 16.6 (15.5) percent, and then to 17.1 (16.2) percent after two years. At the same time, the minimum observed capital ratio in our sample increased from 9.7 percent, to 10.7 percent, and finally to the new minimum required level of 11.5 percent. Also note that the right tail of the distribution remains relatively unchanged, reflecting that the high-capitalized banks are not changing their capital ratios in response to the reform.

[13]After the reform, Nordea Norway changed its status from a subsidiary to a branch, thereby avoiding the new Norwegian capital requirements.

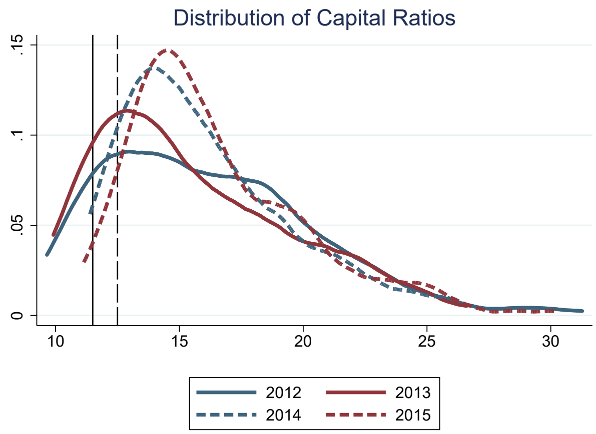

Figure 3: Distribution of capital ratios (%) prior to the reform (2012-Q4), 1 year later (2013-Q3), 2 years later (2014-Q3) and 3 years later (2015-Q3). The solid line marks the baseline requirement of 11.5 %, while the dashed line marks the new maximum requirement of 12.5 %.

Summary statistics for 2012-Q4 are reported in Table 1. The average bank has assets worth roughly 3,000 million US dollars, while the largest bank has more than 200,000 million US dollars in assets. As reported in the third row, loans make up on average 80 percent of total bank assets. There is substantial variation in bank financing, as captured by deposits as a share of total assets. On average, deposits account for 68 percent of assets. Average risk weights range from 0.45 to 0.99, with a mean of 0.59. These differences reflect, at least in part, differences in lending shares to households and firms. The average bank lends almost five times as much to households relative to firms, but the standard deviation is large. Several banks lend more to firms than to households. As seen from the two last rows of Table 1, most banks in our sample are non-IRB, savings banks. At the end of 2013, the total assets of Norwegian savings banks constituted approximately 91 percent of total assets. The distinction between commercial and savings banks in Norway is not very clear, however. For instance, DNB ASA, the largest bank in Norway and one of the larger banks in Northern Europe, is legally defined as a savings bank but is – in terms of operations – very similar to traditional commercial banks. Larger Norwegian savings banks issue equity, covered bonds and other forms of financing, pay dividends and compete nationally alongside commercial banks.

Variable	Mean	Median	Std.dev.	Min.	Max.	Obs.
Capital Ratio (%)	16.2	15.9	4.2	9.7	31.3	119
Assets (mill USD)	2,913	375	18,422	57	200,345	119
$\frac{\text{Loans}}{\text{Assets}}$	0.80	0.84	0.10	0.20	0.91	119
$\frac{\text{Deposits}}{\text{Assets}}$	0.68	0.67	0.12	0.005	0.89	119
Avg. Risk Weight	0.59	0.58	0.082	0.45	0.99	119
$\frac{\text{Profits}}{\text{Assets}}$ (%)	0.45	0.44	0.21	-0.25	1.64	119
$\frac{\text{Profits}}{\text{Equity}}$ (%)	5.0	4.7	2.8	-3.8	22	119
$\frac{\text{HH-Lending}}{\text{Firm-Lending}}$	4.9	2.5	17.5	0.12	179	114
Savings Bank (binary)	0.87	1	0.33	0	1	119
Non-IRB Bank (binary)	0.94	1	0.24	0	1	119

Table 1: Summary Statistics for 2012-Q4. NOK/USD = 8.61 (5/8/2017)

Most of our analysis will rely on dividing banks into two groups based on their pre-reform capitalization levels. It will therefore be useful to study the differences between low-capitalized and high-capitalized banks. In the first column of Table 2 we report the difference in means between low capitalized and high capitalized banks. Low (high) capitalized banks are defined as banks with pre-reform (Q4-2012) capital ratios below (above) the median. As seen from the first row, on average high-capitalized banks have almost seven percentage points higher capital ratios than low-capitalized banks. They are also smaller, have higher loan-to-asset ratios, and rely more heavily on deposit financing. Further, high-capitalized banks have lower average risk weights and lower profit-to-equity ratios. In addition, high-capitalized banks are more likely to be savings banks and less likely to be IRB-banks. Hence, we conclude that low-capitalized and high-capitalized banks differ along several observable dimensions.

In some of our analysis we exclude the 25 percent most and least capitalized banks. This leaves us with a more homogeneous group of banks, in which we compare quartile 2 banks to quartile 3 banks. The second column of Table 2 compares high-capitalized and low-capitalized banks for this restricted sample. The difference in capital ratios is roughly half of the difference for the full sample, but remains statistically significant at the one percent level. All of the other differences between bank types fall substantially, and only the level of deposit financing remains significantly different. Hence, the only statistically significant difference when using the restricted sample is that quartile 3 banks rely more heavily on deposit financing than quartile 2 banks. We document that our results are robust to controlling for all of the variables listed in Table 1.

	$mean^{50-100} - mean^{0-50}$	$mean^{50-75} - mean^{25-50}$
Capital Ratio (%)	6.78***	3.19***
Assets (mill USD)	- 4,870*	- 429
$\frac{\text{Loans}}{\text{Assets}}$	0.046**	0.023
$\frac{\text{Deposits}}{\text{Assets}}$	0.10***	0.052**
Avg. Risk Weight	- 0.034**	0.013
$\frac{\text{Profits}}{\text{Assets}}$ (%)	0.011	0.032
$\frac{\text{Profits}}{\text{Equity}}$ (%)	- 1.7***	- 0.81
$\frac{\text{HH-Lending}}{\text{Firm-Lending}}^{+}$	- 1.21	2.08
Savings Bank (binary)	0.15**	0.071
Non-IRB Bank (binary)	0.083*	0.001
Observations	119 ($^{+}$114)	59 ($^{+}$54)

Table 2: Comparison of Banks by Capitalization Level (2012-Q4). Banks are labeled low-capitalized or high-capitalized based on their pre-reform (2012-Q4) ratio. The first column compares banks in quartiles 3 and 4 (high-capitalized) to banks in quartiles 1 and 2 (low-capitalized). The second column compares banks in quartile 3 (high-capitalized) to banks in quartile 2 (low-capitalized).

After documenting how banks adjust their balance sheets in response to increased capital requirements, we proceed by using a loan level dataset provided by The Norwegian Tax Authorities. This dataset contains annual, matched firm-bank data for the universe of Norwegian firms. The tax data has three advantages. First, it lets us observe the entire portfolio of domestic corporate credit for all Norwegian banks, enabling us to do a more granular analysis of how banks respond. Second, it strengthens identification by allowing us to include firm fixed effects to hold demand factors fixed. Firm fixed effects are only feasible for multiple-bank firms however. In our test for correlated supply and demand shocks we therefore rely on the roughly 10 percent of firms which borrow from multiple banks. Using the tax data, we can observe the interest paid on loans. This enables us to also study the price effects of the reform. Finally, the loan level data lets us trace out the effect of bank credit contractions on the real economy by linking firms and banks. For the latter exercise we also rely on a final dataset containing firm level employment data. This data comes from the firms' annual reporting, compiled in a national public register (*The Bronnoysund Register*).

3 Bank Level Analysis

We start by investigating how banks respond to increased capital requirements. Recall that a bank's risk-weighted capital ratio in period t is given by

$$\text{Capital Ratio}_t = \frac{E_t}{\sum_i \alpha_{ti} A_{ti}} = \frac{E_t}{\overline{\alpha}_t \, A_t}$$

where the average risk weight is given by $\overline{\alpha} \equiv \frac{\sum_i \alpha_i A_i}{\sum_i A_i}$ and total assets by $A \equiv \sum_i A_i$. Taking logs and first differences yields

$$\Delta \log \left(Capital\ Ratio_t\right) = \Delta \log \left(E_t\right) - \Delta \log \left(A_t\right) - \Delta \log \left(\overline{\alpha}_t\right) \tag{2}$$

As seen from equation (2), banks can increase their capital ratio growth rate in three ways. First, they can increase the growth in equity, for example through retained earnings. Second, they can reduce the growth in assets, which is likely to imply a reduction in credit supply. Finally, they can reduce the growth in the average risk weight $\bar{\alpha}$. This implies shifting their asset composition towards assets with lower assigned risk weights. In this section we decompose the reform-induced change in capital ratio growth rates, and quantify the relative importance of equity, assets and average risk weights.

3.1 Methodology

Our analysis relies on the cross-sectional differences in capital ratios prior to the reform. Whereas high-capitalized banks were not directly affected by the reform, low-capitalized banks had to increase their capitalization levels. The main identification challenge is to separate supply factors from demand factors. If high-capitalized and low-capitalized banks lend to systematically different clients, we risk falsely attributing demand-driven changes in bank outcomes to higher capital requirements. For example, low-capitalized banks may be lending to firms with low credit demand, which could reduce equilibrium lending regardless of the reform. We address this important issue in three ways. First, we use a flexible difference in difference methodology to directly test the standard assumption of parallel trends for the high-capitalized and low-capitalized banks. Hence, we can explicitly test whether low-capitalized and high-capitalized banks have similar outcomes prior to the reform, suggesting that they are not lending to systematically different clients. Later, in Section 5 we use loan level data and follow Khwaja and Mian (2008) in including firm fixed effects. In this case, the effect of bank capitalization on credit supply is identified while holding firm characteristics fixed. Finally, in Section 6, we back out bank specific interest rates using loan level tax data. This allows us to evaluate not only how lending *volumes* are affected by higher requirements, but also how lending *prices* are affected. While a negative supply shock has the same qualitative implications for lending volumes as a negative demand shock, the two shocks have different implications for prices. Hence, an increase in interest rates supports the interpretation of the fall in credit being supply-driven.

The flexible difference in difference regression is specified in equation (3). Our main dependent variables are the growth rates in capital ratios, equity, assets and average risk weights for bank i. Hence, we estimate equation (3), with $Y_{it} = \{\text{Capital Ratio}_{it}, \text{Equity}_{it}, \text{Assets}_{it}, \text{Risk Weight}_{it}\}$. The time fixed effects δ_t account for common cyclical patterns in these variables. We use a type dummy $D_i = 0$ if bank i is high-capitalized ("low treatment intensity"), and $D_i = 1$ if bank i is low-capitalized ("high treatment intensity"). The coefficients of interest are the γ_{1t}^{D}'s on the type×time interaction terms. These coefficient estimates identify the difference in $\Delta log(Y_{it})$ for high and low-capitalized banks in a given year, relative to the average difference between the two bank types. We can directly test the parallel trends assumption by testing whether $\gamma_{1t}^{D} = 0 \; \forall \; t < 0$, using $t = 0$ to capture the time of the reform. If there are anticipation effects of the reform, this will show up in our estimates as a violation of the parallel trends assumption. Given that the parallel trends assumption holds, the treatment effects will be captured by the γ_{1t}^{D}'s for $t \geq 0$. Note that we will also be able to test the hypothesis of constant treatment effects, i.e. $\gamma_{1t}^{D} = \bar{\gamma}^{D} \; \forall t \geq 0$. This will turn out not to hold, as the effect of the reform appears to be transitory. Once banks have adjusted to the new capital requirements, low-capitalized and high-capitalized banks return to their previous parallel trends. Hence, a comparison of the γ_{1t}^{D}'s for different post-reform time periods will allow us to map out the dynamic treatment effects.

$$\Delta log(Y_{it}) = \alpha + \delta_t + \gamma_i^D D_i + \gamma_{it}^D D_i \times \delta_t + \epsilon_{it} \tag{3}$$

The flexible difference in difference specification is attractive because it can explicitly test the parallel trends assumption, and because it allows for dynamic treatment effects. However, it is quite data demanding, and will sometimes fail to produce significant results in cases where more restrictive difference in difference estimations *will* produce significant results (Reggio and Mora Villarrubia 2012). Therefore, after having verified the validity of the parallel trends assumptions, we proceed by estimating a less data demanding regression, as specified in equation (4). Instead of interacting type with time dummies, we now interact type with a dummy for the full post-reform period. That is, $I_t^{post} = 0$ if $t < 0$, and $I_t^{post} = 1$ otherwise. This specification imposes a parallel trends assumption explicitly, which we are comfortable doing based on the results from the flexible difference in difference regression. The specification also imposes a constant treatment effect. Hence, the coefficient estimate $\hat{\beta}$

should be interpreted as the average treatment effect in the post-reform period.

$$\Delta log(Y_{it}) = \alpha + \delta_t + \gamma_i^D D_i + \beta D_i \times I_t^{post} + \epsilon_{it} \tag{4}$$

The baseline estimates are based on regressions without control variables. This is a valid approach if our identifying assumption holds. That is, pre-reform capitalization levels are only related to post-reform outcome variables through the effect on changes in capital ratios. We also report results controlling for numerous variables such as size, average risk weight, asset composition, deposit financing, return on equity and organizational structure. Our results are largely unchanged. Finally, standard errors are clustered at the bank level[14].

3.2 Results

The upper, left panel of Figure 4 plots the growth rate in capital ratios for low-capitalized and high-capitalized banks. In the time prior to the reform, low-capitalized and high-capitalized banks have similar changes in capital ratios. These pre-reform growth rates fluctuate around zero, indicating that capital ratios are roughly constant. At the time of the reform, a new pattern emerges. While high-capitalized banks continue to have growth rates close to zero, there is a spike in growth rates for low-capitalized banks. This divergence seems to start when the reform is announced, and grows in magnitude over time. By the end of the sample the difference decreases, suggesting that the transitory adjustment in capital ratios is coming to an end.

Using $\Delta log(Cap.Ratio_{it})$ as our dependent variable, the flexible difference in difference regression in equation (3) results in the interaction coefficients plotted in the upper, right panel of Figure 4[15]. The interaction terms prior to the reform are stable and not significantly different from zero, implying that the parallel trends assumption holds. Then, at the time of the reform, the interaction terms start increasing in magnitude and statistical significance. Low-capitalized banks have higher annual growth in capital ratios than high-capitalized banks for six consecutive quarters. The difference is no longer statistically significant for the last observation in our sample, suggesting that the adjustment in capital ratios is limited to a slightly less than two-year period.

[14]Clustering at other levels provide less conservative standard errors.

[15]Capital ratios are defined as banks' CET1-ratios. Because CET1-ratios have only been reported as of 2012, we use regular capital ratios as a proxy for CET1-ratios prior to 2012. Note that this leaves us with at least two pre-reform CET1 observations, meaning that the break in CET-ratios at the time of the reform cannot be explained by measurement issues.

A potential concern is that the divergence in capital ratio growth rates is partly driven by mean reversion. If banks target similar capital ratios, low capitalized banks may have high growth rates in capital ratios for reasons unrelated to the reform. To test whether mean reversion can explain the observed patterns we perform a placebo test in which we repeat our analysis one year prior to the reform. That is, we define banks as low or high capitalized based on their capital ratios in 2011, and test whether the two groups have different growth rates in capital ratios after Q3-2012. As illustrated in Figure 11 in the appendix, there is no divergence at this artificial reform date. Instead, there is a (noisy) divergence at the time of the reform, suggesting that the reform itself is the driving force behind our results.

How much of the increase in capital ratios is due to an increase in equity? We plot the equity results in the second row of Figure 4. The left panel depicts growth rates in equity for low-capitalized and high-capitalized banks. Low-capitalized banks have consistently higher growth rates in equity prior to the reform, but the difference between the two bank types is stable. There is no apparent trend break at the time of the reform. However, an interesting pattern emerges starting as of 2014-Q1. Both bank types increase the growth in equity, but the increase is larger for low-capitalized banks. We believe this delayed response to the reform is due to banks decision making processes. Important decisions such as dividend policies are taken at the general assembly, and apply to one calendar year at the time. The data is consistent with low-capitalized banks deciding to lower their dividend payouts for the calendar year 2014, contributing to higher equity growth through retained earnings.

In the right panel we plot the interaction term coefficients from equation (3) with $Y_{it} = \text{Equity}_{it}$. Prior to the reform, the interaction coefficients are stable and not significantly different from zero. Consistent with the raw data, there is no statistically significant effect at the time of the reform. However, low-capitalized banks do have significantly higher growth in equity from 2013 to 2014, suggesting that part of the increase in capital ratios is due to an increase in equity. This effect is however limited to a one calendar-year period, and relatively modest in size.

We next move on to consider the impact on assets in the third row of Figure 4. The growth in assets for low-capitalized and high-capitalized banks are plotted in the left panel. Although the data is somewhat noisy, the two bank types have similar growth rates in assets prior to the reform. At the time of the reform however, there is a decline in asset growth for low-capitalized banks. High-capitalized bank on the other hand, increase their growth rates.

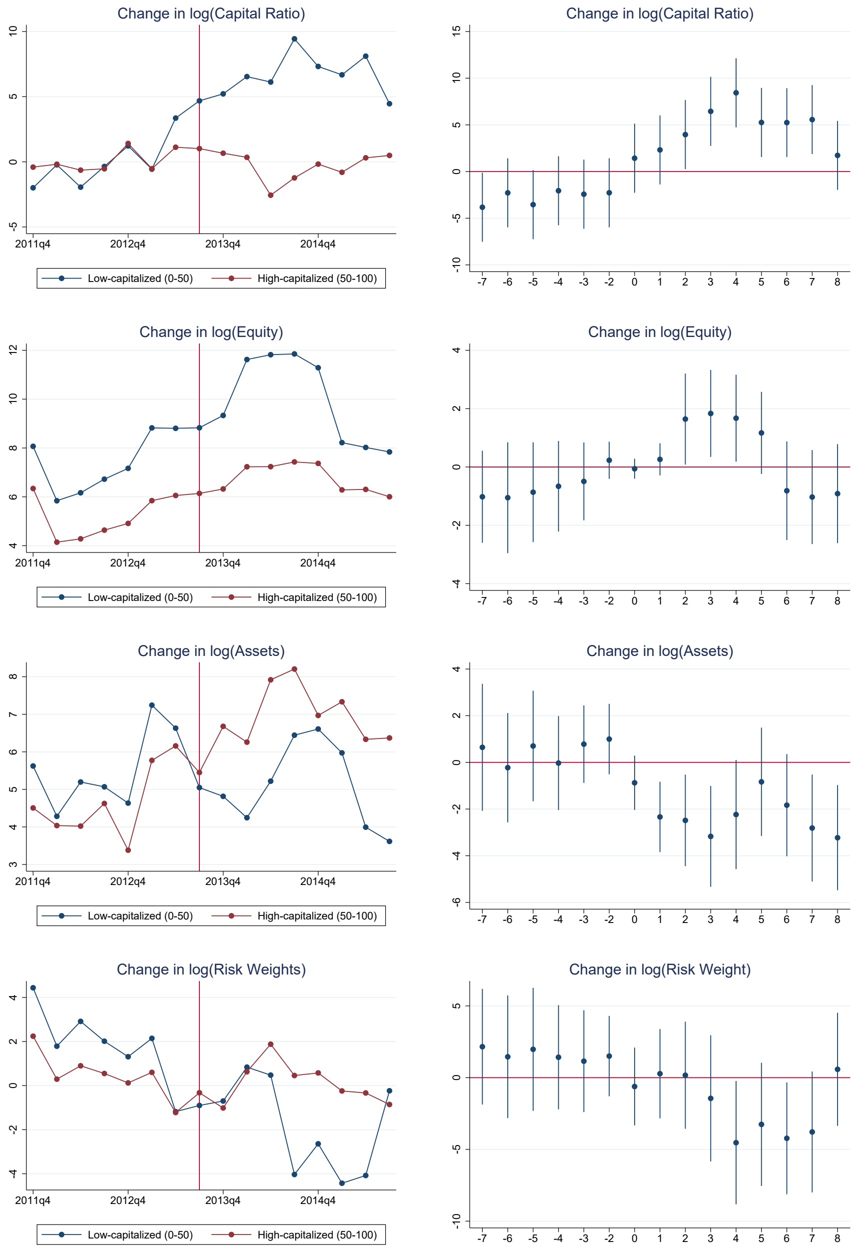

Figure 4: Capital Ratio, Equity, Assets and Average Risk Weights. Banks are divided into groups based on their 2012 capital ratio. Left panels: Growth rates in capital ratios, equity, assets and average risk weights for low-capitalized (below median) and high-capitalized (above median) banks. The growth rate for $year_t$-$quarter_i$ denotes the (approximate) percentage change from $year_{t-1}$-$quarter_i$ to $year_t$-$quarter_i$. The solid red line marks the growth rate from 2012-Q3 to 2013-Q3 (the reform date). Right panels: Regression results from estimating equation (3) with dependent variable Y_{it} = $\{\text{Capital Ratio}_{it}, \text{Equity}_{it}, \text{Assets}_{it}, \text{Risk Weight}_{it}\}$. Interaction coefficients γ_{1t}^{D} are plotted relative to time $t = -1$. Standard errors are clustered at the bank level. Time zero marks the growth rate from 2012-Q3 to 2013-Q3 (the reform date).

The right panel depicts interaction coefficients from the flexible difference in difference regression in equation (3), with growth in assets as the dependent variable. Prior to the reform, the interaction coefficients are small and stable. The parallel trend assumption thus appears reasonable. Starting at the reform date, low-capitalized banks experience lower growth in assets than high-capitalized banks. This effect seems to persist for the full post-reform period, although there is one date for which the difference is not statistically different from zero. Hence, it appears that a reduction in asset growth contributed substantially to the increase in capitalization levels caused by the reform.

Finally, we study the effect on average risk weights, and plot the results in the bottom row of Figure 4. High-capitalized banks have slightly lower growth in average risk weights *prior* to reform, but higher growth in average risk weights *after* the reform, as illustrated in the left panel. The data is consistent with a reduction in average risk weights for low-capitalized banks following the reform. As was the case with equity, the difference between low-capitalized and high-capitalized banks increases with the beginning of a new calendar year.

As before, we plot interaction coefficients from regressing equation (3) with $Y_{it} = \text{Risk Weight}_{it}$ as the dependent variable in the right panel. The interaction coefficients are stable and not statistically different from zero prior to the reform. There is a small trend break at the time of the reform, but this is not statistically significant. However, as seen from the raw data, there appears to be a delayed effect starting at the beginning of the new calendar year in 2014-Q1. Low-capitalized banks significantly reduce their average risk weights relative to high-capitalized banks. The effect is large in magnitude, but not as precisely measured as the reduction in asset growth. Still, low-capitalized banks seem to have relied substantially on a reduction in the average riskiness of their portfolios to achieve the observed increase in risk-weighted capital ratios.

The flexible difference in difference regressions have confirmed that the parallel trends assumption seems to hold for all our outcome variables. Hence, we are comfortable estimating the more restrictive difference in difference regression in equation (4). Note that this specification imposes a constant treatment effect, so the estimates should be interpreted as average treatment effects over the time period studied.

In Table 3 we report regression results for the four outcome variables studied above. The first column shows results using $\Delta log(\text{Capital Ratio}_{it})$ as our dependent variable. In the post-reform period, low-capitalized banks had on average 6.8 percentage points higher growth in capital ratios than high-capitalized banks. The difference is significant at the one percent level of significance.

Results using the growth in equity as the dependent variable are reported in the second column. In the post-reform period, low-capitalized banks had on average 1.0 percentage points higher growth in equity than high-capitalized banks. This difference is however not statistically significant. The reason is that the equity response seems limited to a one-calendar-year period, resulting in a low average treatment effect. Hence, we conclude that although there does appear to have been some response through equity, changes in equity growth are not quantitatively as important as changes in risk-weighted assets in explaining the increase in capital ratios.

Column three reports results using the growth in assets as the dependent variable. In the post-reform period, low-capitalized banks had on average 2.6 percentage points lower growth in assets than high-capitalized banks. The difference is significant at the five percent level of significance. Finally, column four reports results using the growth in average risk weights as the dependent variable. We estimate that low-capitalized banks had on average 3.3 percentage points lower growth in average risk weights than high-capitalized banks, in the post-reform period. This difference is significant at the one percent level of significance.

The results are robust to adding control variables, as reported in the lower panel of Table 3[16]. While none of the point estimates are significantly different from the estimation without control variables, the impact on equity is slightly larger and significant at the ten percent level.

[16] Control variables include pre-reform values for assets, loan-to-assets, deposit-to-assets, average risk weights, profit-to-equity, household lending relative to firm lending, saving bank status and IRB bank status.

	(1) $\Delta log(Cap.Ratio_{it})$	(2) $\Delta log(Equity_{it})$	(3) $\Delta log(Assets_{it})$	(4) $\Delta log(RiskWeight_{it})$
$D_i \times I_t^{post}$	6.830***	0.966	-2.613**	-3.252***
	(5.44)	(1.43)	(-2.63)	(-2.77)
Time FE	yes	yes	yes	yes
Type FE	yes	yes	yes	yes
Controls	no	no	no	no
Clusters	120	120	120	120
Observations	1,907	1,907	1,907	1,907
$D_i \times I_t^{post}$	6.387***	1.223*	-2.511**	-2.653**
	(4.89)	(1.79)	(-2.46)	(-2.32)
Time FE	yes	yes	yes	yes
Type FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Clusters	114	114	114	114
Observations	1,824	1,824	1,824	1,824

t statistics in parentheses, Std. err. clustered at bank level

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3: Restrictive Difference in Difference Estimation. Regression results from estimating equation (4).

In order to decompose the growth rate in capital ratios we simply divide the coefficients in columns 2, 3 and 4 with the coefficient in column 1[17]. A one percentage point higher reform-induced growth rate in capital ratios leads to an increase in equity growth of 0.14 percentage points, a decrease in asset growth of 0.38 percentage points, and a decrease in the growth rate of average risk weights of 0.48 percentage points. We thus conclude that more than 85 percent of the increase in capital ratios is achieved by adjusting risk-weighted assets. Of these 85 percent, the majority is explained by a portfolio rebalancing effect, in which banks substitute high-risk assets with low-risk assets[18]. In the next section, we further explore why this is the case.

[17]Identical results can be obtained by a two staged least square regression, with the predicted change in capital ratio as the independent variable.

[18]In a previous version of the paper, we employed a continuous treatment intensity variable rather than grouping banks according to high and low treatment intensity. While this approach uses more of the cross-sectional variation available in the data, it does not fully exploit the time variation. We prefer the flexible difference in difference analysis, as it explicitly allows us to test the parallel trends assumption. However, the qualitative results are reassuringly very robust to using other approaches.

4 Portfolio Rebalancing

4.1 Why do banks rebalance their portfolio?

The reduction in average risk weights is due to a shift in portfolio composition. In this section we analyze this in more detail. To fix ideas, we start by setting up a simple model based on Freixas and Rochet (2008).

The model is static. A bank allocates funds to different competitive lending markets. For simplicity, we assume that equity E is fixed. Although this is a strict assumption, our empirical results from the previous section suggest that the impact on equity is limited. We assume that A_0 is a risk-free asset, i.e. government bonds or central bank reserves, and that assets $1, ..., n$ are loans to different markets. The bank chooses a vector of asset allocations $\boldsymbol{A} = \{A_1, ... A_n\}$ in n lending markets. For instance, we can think about A_1 as being single-family mortgages, A_2 as being corporate loans to $BB+$ rated public corporations etc. The remainder of the banks funds is used to purchase the riskless asset.

The vector of expected excess returns in the respective lending markets is joint-normal with mean $\rho = \{\rho_1, ...\rho_n\}$, and with invertible variance-covariance matrix Σ. The bank is subject to a capital requirement $\bar{k}$. By law, the bank is required to ensure that

$$\frac{E}{\alpha \cdot \boldsymbol{A}} \geq \bar{k} \tag{5}$$

where $\cdot$ denotes the dot-product and $\alpha = \{\alpha_1, ..., \alpha_n\}$ denotes a vector of pre-assigned risk-weights corresponding to the respective loan categories[19]

We assume that the bank (or bank owner) has CARA preferences. This, in combination with the normality of the asset-returns, allows us to write the certainty equivalent of the bank-owners pay-off as

$$U(\mathbf{A}) = \rho^T \mathbf{A} - \frac{1}{2} \gamma \mathbf{A}^T \Sigma \mathbf{A} \tag{6}$$

where γ is the bank owner's coefficient of risk-aversion. Thus, the portfolio allocation problem is to maximize utility given by equation (6), subject to the capital requirement (5) and the balance sheet constraint $\sum A = D + E$.

Letting λ denote the shadow-value of the capital requirement constraint, the set of first-order conditions for portfolio allocations can be written compactly as

[19] Since the zeroth asset is risk-free, it is assigned a risk-weight of zero percent.

$$\rho - \gamma \mathbf{A}\mathbf{\Sigma} - \lambda \bar{k} \alpha = 0 \tag{7}$$

or in terms of portfolio allocations (in dollars invested in each asset)

$$\mathbf{A} = \Sigma^{-1} \frac{\rho - \lambda \bar{k} \alpha}{\gamma} \tag{8}$$

In the absence of a binding capital requirement ($\lambda = 0$), this is the mean-variance efficient portfolio in the sense of the traditional capital asset pricing model (CAPM).

Equation (8) sheds light on how risk-weighted capital requirements affect banks lending decisions. Because the effective excess return is reduced by a binding capital requirement, the banks overall holdings of risky assets fall. How is the provision of credit to various sectors affected? This depends on the pre-assigned risk weights, and how they relate to systematic risk. The traditional CAPM would require that in a competitive market, the return-vector ρ is colinear to the systematic risk of the various assets. From equation (8) it is clear that the introduction of a binding risk-weighted capital requirement ($\lambda > 0$) could lead to an inefficient allocation across risky assets, relative to the mean-variance efficient benchmark. This occurs when the risk-weights α are not proportional to ρ, and therefore not proportional to systematic risk.

We illustrate this point with a simple example of two lending markets, i.e. $n = 2$. Maximizing (6) with respect to (5) and the balance sheet condition, results in the optimal allocations

$$A_1^* = \frac{\rho_1 - \lambda \bar{k} \alpha_1}{\gamma(\sigma_{11}^2 + \sigma_{21}^2)}, \quad A_2^* = \frac{\rho_2 - \lambda \bar{k} \alpha_2}{\gamma(\sigma_{12}^2 + \sigma_{22}^2)}$$

It is easy to show that $\frac{A_1^*}{A_2^*}|_{\lambda>0} = \frac{A_1^*}{A_2^*}|_{\lambda=0}$ if and only if $\frac{\alpha_1}{\alpha_2} = \frac{\rho_1}{\rho_2}$[20]. In words, the relative asset allocation is independent of the capital requirement only if the relative risk weights are proportional to expected returns, and thereby to systematic risk. Suppose however that this was not the case, and that $\frac{\alpha_1}{\alpha_2} < \frac{\rho_1}{\rho_2}$. This implies that the relative risk weight of the first asset, A_1, is too low, causing A_1 to be inefficiently high relative to the efficient portfolio. In other words, the introduction of capital requirements would in this case lead to a shift in lending towards the first market, away from the second market.

[20]The argument also applies when going from a binding constraint to a stricter constraint. To see this consider increasing capital requirements from $\bar{k}$ to $\bar{k}'$, with respective shadow values λ and λ'. The relative asset allocations $\frac{A_1}{A_2}$ and $\frac{A_1'}{A_2'}$ will once again equal each other if $\frac{\alpha_1}{\alpha_2} = \frac{\rho_1}{\rho_2}$. Specifically, $\frac{A_1}{A_2} = \frac{A_1'}{A_2'} \iff (\alpha_1\rho_1 - \alpha_2\rho_2)\left(\lambda\bar{k} - \lambda'\bar{k}'\right) = 0$. Hence, observing $\frac{A_1}{A_2} \neq \frac{A_1'}{A_2'}$ implies $\frac{\alpha_1}{\alpha_2} \neq \frac{\rho_1}{\rho_2}$.

4.2 How do banks rebalance their portfolio?

Due to the large difference in average risk weights between corporate lending and household lending, the reduction in average risk weights can imply a relative reduction in corporate lending. We now proceed to investigate whether this is indeed the case.

We have quarterly balance sheet data for corporate lending starting in 2012. Growth rates for corporate lending based on the balance sheet data are depicted in the left panel of Figure 5. Although we only have two pre-reform observations for growth in firm lending, the picture is quite striking. Low-capitalized and high-capitalized banks have very similar growth rates in corporate lending prior to the reform, if anything low-capitalized banks have slightly higher growth rates. This changes abruptly at the time of the reform. While high-capitalized banks continue to see growth rates in corporate lending volumes of 4-6 percent, the growth rate in corporate lending for low-capitalized banks plummets. In fact, low-capitalized banks have negative growth in firm lending for four consecutive observations. Mirroring the evolution in capital ratios and average risk weights, the difference in lending growth is mostly over by the end of our sample. The right panel of Figure 5 plots the interaction coefficients from estimating equation (3) using the growth in firm lending as the dependent variable. The interaction coefficients are small and insignificant prior to the reform, and then become negative and significantly different from zero after the reform. Hence, we conclude that low-capitalized banks significantly and substantially reduced corporate lending growth in response to the reform.

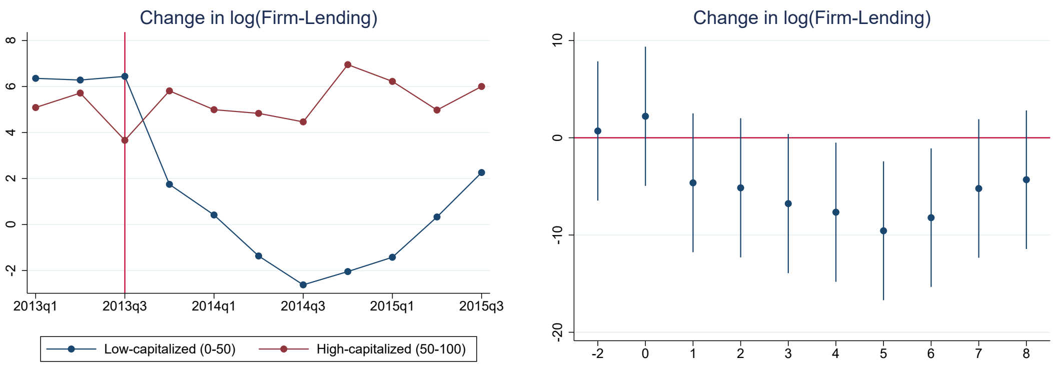

Figure 5: Firm Lending. Banks are divided into groups based on their 2012 capital ratio. Left panel: Growth rates in capital ratios for low-capitalized (below median) and high-capitalized (above median) banks. The growth rate for $year_t$-$quarter_i$ denotes the (approximate) percentage change from $year_{t-1}$-$quarter_i$ to $year_t$-$quarter_i$. The solid red line marks the growth rate from 2012-Q3 to 2013-Q3 (the reform date). Right panel: Regression results from estimating equation (3) with dependent variable $Y_{it} = Firm\, Lending_{it}$. Interaction coefficients γ_{1t}^{D} are plotted relative to time $t = -1$. Standard errors are clustered at the bank level. Time zero marks the growth rate from 2012-Q3 to 2013-Q3 (the reform date).

In order to obtain a longer time series for corporate lending, and to confirm our results using an alternative data source, we aggregate the loan level tax data into a time series for corporate bank lending. We then redo the above analysis, now using annual data. The results are depicted in Figure 6. First, note that the assumption of parallel trends is not only valid in the short time period prior to the reform shown in Figure 5, it seems to hold for at least a six-year period prior to the reform. Lending growth is high for both bank types prior to the financial crisis, before falling substantially in 2009. Credit growth is then fairly stable between five and ten percent for both bank types in the period leading up to the reform. While high-capitalized banks continue to have fairly stable growth rates in firm lending post-reform, the growth rate in firm lending for low-capitalized banks once again plummets. As was the case for the quarterly data, low-capitalized banks even experience negative credit growth in the year following the reform. As before, we report interaction coefficients from regressing equation (3), using the annual change in firm lending as our dependent variable. These interaction coefficients are reported in the right panel of Figure 6. Prior to the reform, the interaction coefficients are small and insignificant. Post-reform, the interaction coefficients are negative and significantly different from zero. Hence, the tax data confirms a significant reduction in corporate lending growth for low-capitalized banks following the reform.

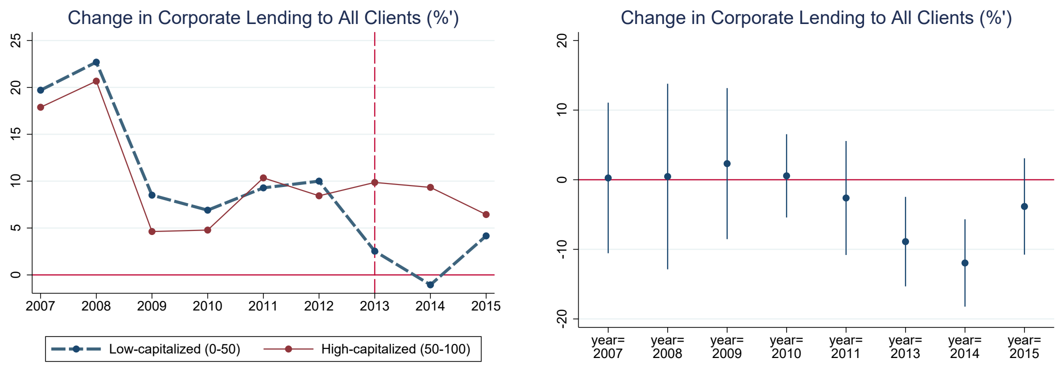

Figure 6: Firm Lending - Tax Data. Banks are divided into groups based on their 2012 capital ratio. Left panel: Growth rates in corporate lending for low-capitalized (below median) and high-capitalized (above median) banks. The growth rate for $year_t$ denotes the symmetric percentage change from $year_{t-1}$ to $year_t$. The dashed red line marks the growth rate from 2012 to 2013 (the reform year). Right panel: Regression results from estimating equation (3) with dependent variable $Y_{it} = Firm - Lending_{it}$. Interaction coefficients γ_{1t}^{D} are plotted relative to year 2012. Standard errors are clustered at the bank level.

After having confirmed that the parallel trends assumption is appropriate, we now move on to estimating the more restrictive difference in difference regression specified in equation (4). The results are reported in Table 4. Using the quarterly balance sheet data, we find that following the reform, low-capitalized banks had on average 5.8 percentage points lower growth in corporate lending than high-capitalized banks. Using the aggregated tax data increases this number to 8.9, as reported in the second column[21]. These effects are substantially larger than the total reduction in assets, suggesting that low-capitalized banks are especially willing to reduce *firm* lending. Scaling the results with the increase in capital ratios, we find that a one percentage point higher increase in capital ratios leads to a 1.0 to 1.5 percentage points lower growth in corporate credit supply[22].

[21]Note that the quarterly data on corporate lending from The Norwegian Banks Guarantee Fund is not exactly the same as the annual data on corporate lending from The Norwegian Tax Authorities, as the latter only consists of Norwegian limited liability firms and not foreign firms and sole proprietorships.

[22]This is equivalent to regressing the change in corporate lending on the predicted change in capital ratios.

	(1)	(2)
	Δlog(Firm-Lending$_{it}$)	Δlog(Firm-Lending$_{it}$)
$D_i \times I_t^{post}$	-5.834**	-8.930***
	(-2.55)	(-2.97)
Time FE	yes	yes
Type FE	yes	yes
Data Source	balance sheet	tax data
Clusters	116	110
Observations	1,251	1,094

t statistics in parentheses, Std. err. clustered at bank level
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 4: Restrictive Difference in Difference Estimation - Corporate Credit Supply. Regression results from estimating equation (4).

What about household lending? In Figure 7 we plot the growth rates in household and firm lending, for low-capitalized and high-capitalized banks[23]. Focusing first on the dashed lines, we see that high-capitalized banks have stable and similar growth rates in firm and household lending. Lending growth to both groups fluctuates between four and seven percent, with no apparent trend breaks. Contrast this to the low-capitalized banks as captured by the solid lines, which have a striking divergence in the growth rates of household lending and firm lending post-reform. While lending growth to the household sector remains stable, lending growth to the corporate sector falls to negative values. Hence, we conclude that low-capitalized banks reduce lending growth to the firm sector relative to the household sector, whereas high-capitalized banks do not.

[23]We only have data on household lending for a shorter time-period, which means that we cannot estimate the same regressions as for firm lending. However, the visual evidence from Figure 7 makes us confident that there is no or limited response in mortgage lending.

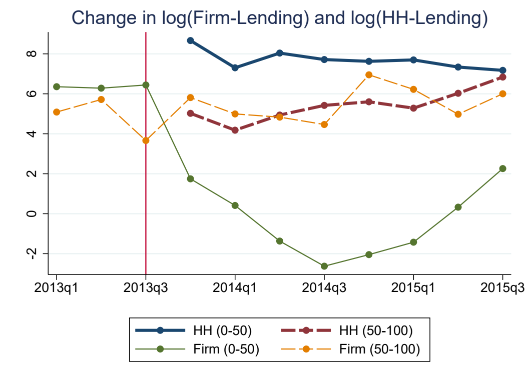

Figure 7: Firm Lending and Household Lending. Growth rates in firm and household lending for low-capitalized (below median) and high-capitalized (above median) banks. Banks are divided into groups based on their 2012 capital ratio. The growth rate for $year_t$-$quarter_i$ denotes the (approximate) percentage change from $year_{t-1}$-$quarter_i$ to $year_t$-$quarter_i$. The solid red line marks the growth rate from 2012-Q3 to 2013-Q3 (the reform date).

Due to the large differences in risk weights between mortgage lending and corporate lending, cutting back on corporate lending is an efficient way to reduce average risk weights. In Figure 8 we plot the change in corporate lending relative to household lending as a function of the change in average risk weights. There is a strong positive relationship between the two variables, suggesting that the reduction in average risk weights reflects a reduction in firm lending relative to household lending. We next explore this correlation with some simple calculations based on the average bank's balance sheet.

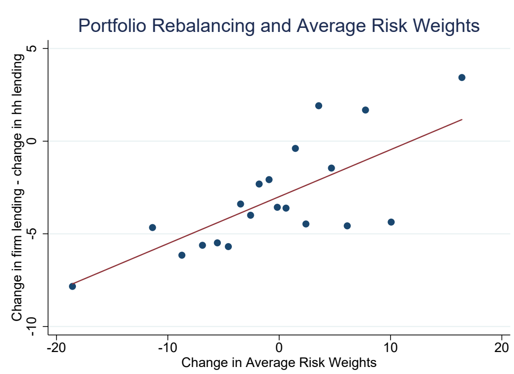

Figure 8: Portfolio Rebalancing. Binscatter of $\Delta log(Firm{-}Lending) - \Delta log(HH{-}lending)$ and Δlog(Risk weight) for all banks. Binscatter groups the x-axis variable into equal-sized bins, computes the mean of the x-axis and y-axis variables within each bin, then creates a scatterplot of these data points.

Can the shift from firm lending to household lending quantitatively explain the reduction in average risk weights? Shifting from corporate lending to household lending will generally reduce the average risk weight on a bank's portfolio. However, the magnitude will depend on the characteristics of the specific firm and household loans. Banks can also reduce their average risk weights through other channels, for example by changing the share of other assets it holds (for example government bonds). Additionally, shifts within asset classes could also reduce average risk weights. For example, by extending low-risk corporate loans the bank could decrease its average risk weight, while holding the total corporate lending share constant.

In order to evaluate the quantitative importance of shifting from firm lending to household lending we perform some back-of-the-envelope calculations using the balance sheet of an average low-capitalized bank. We calculate the *implied* change in risk weights if the only moving part of the balance sheet is the share of household versus firm lending. Comparing this estimate to the *observed* change in risk weights gives us at least a rough idea of whether the relative reduction in corporate lending is quantitatively important.

We observe total assets, household lending and firm lending. We thus define other assets to be the component of assets which is neither household nor firm lending $A^{other} = A^{tot} - L^{HH} - L^{firm}$. The average risk weight ARW is then given by equation (9). While we observe the average risk weight, we do not observe the actual risk weights for each asset class. Hence, we assume that $\alpha^{HH} = 0.35$, which is the average risk weight on mortgages for non-IRB banks (Andersen 2013)[24]. For corporate lending we assume $\alpha^{firm} = 1.0$, in line with the average risk weight on firm loans for non-IRB banks as outlined in Andersen and Winje (2017)[25]. The risk weight on other assets is then backed out to match the observed average risk weight, resulting in $\alpha^{other} = 0.52$.

$$ARW = \frac{L^{HH}}{A^{tot}}\alpha^{HH} + \frac{L^{firm}}{A^{tot}}\alpha^{firm} + \frac{A^{other}}{A^{tot}}\alpha^{other} \tag{9}$$

The change in average risk weights depends on the change in the asset composition between household lending, firm lending and other assets, as well as changes in the respective risk weights. We are interested in isolating the impact of shifts from corporate lending to household lending. To do so we perform a counterfactual exercise in which we assume that

[24] The risk weight of 0.35 is for mortgages which have loan-to-value ratios of maximum 80 percent. As this is the recommended maximum loan-to-value ratio, the majority of mortgages fall within this category.

[25] Loans to corporations with high credit ratings have assigned risk weights below 1.0. However, for non-IRB banks, Andersen and Winje (Norges Bank 2017) conclude that the average corporate risk-weight is close to 1.0.

the risk weights (α^{HH} , α^{firms}, α^{other}) and the share of other assets $\frac{A^{other}}{A^{tot}}$ is constant over time. The latter assumption necessarily implies that the total share of household and firm lending $\frac{L^{HH}+L^{firm}}{A^{tot}}$ is also constant over time. However, the quantity of household lending relative to firm lending is set to match the data. The first column of Table 5 lists the observed average risk weights for low-capitalized banks from 2013 to 2015[26]. Over the period, average risk weights fell by 2.5 percent. Simultaneously, household lending relative to firm lending increased by 17 percent. Note that in the same period high-capitalized banks experienced an *increase* in average risk weights and a *decrease* in household lending relative to firm lending. Keeping risk weights and the share of other assets fixed, we calculate the implied average risk weights in the last column of Table 5. By construction, the average risk weight in the first year is the same as in the data. Shutting down the effect of changes in risk weights for the different asset classes and changes in the share of other assets, we calculate a fall in implied risk weights of 2.0 percent. Hence, the increase in household lending relative to firm lending can explain 80 percent of the observed reduction in average risk weights for low-capitalized banks. We thus conclude that considering average balance sheet data, the fall in relative corporate lending is quantitatively consistent with the reduction in average risk weights. Moreover, although these back-of-the-envelope calculations are quite rough, they suggest that shifts from firm lending to household lending can potentially account for nearly all of the reduction in average risk weights.

	Avg. Risk Weight	L^{HH} / L^{firms}	Implied Avg. Risk Weight
2013	0.630	0.692	0.630
2014	0.621	0.773	0.621
2015	0.614	0.810	0.617
Change 2013 to 2015 (%)	**-2.5**	17	**-2.0**

Table 5: Observed and implied change in average risk weights for low-capitalized banks. When calculating implied average risk weights we assume $\alpha^{hh} = 0.35$, $\alpha^{firm} = 1.0$, and $\alpha^{other} = 0.52$, as well as $\frac{A^{other}}{A} = 0.495$.

5 Firm Level Analysis: Lending and Employment

So far we have been using bank level data, or loan level data aggregated to the bank level. In this section we use loan level data from tax reports. This has two advantages. First, it allows us to include firm fixed effects to strengthen our identification along the lines of

[26]We start in 2013 because we do not have complete data on household lending for 2012.

Khwaja and Mian (2008). Second, it means that every firm is matched to its relationship bank(s), allowing us to evaluate whether there are adverse employment effects at the firm level.

5.1 Lending

To see why loan level data is useful for identification purposes, consider a bank i which lends to a firm j. While the lending amount is a result of both bank specific and firm specific factors, we only observe total equilibrium lending. Because firm specific factors are unobserved to us, we risk mis-measuring the effect of stricter requirements if supply and demand factors are correlated. With loan level data we can control for firm specific factors, thereby isolating the supply effects on total credit.

5.1.1 Stylized Model of Firm-Bank Lending

To inform our empirical specification, we construct a stylized model in the spirit of Khwaja and Mian (2008). The main difference is that we add labor to the firm problem and assume that the wage bill has to be financed with credit due to a cash-in-advance constraint. We also consider a slightly different shock from their shock to deposit financing. Adding labor to the firm problem enables us to derive a structural expression for employment growth as a function of credit supply factors.

Consider a bank i that lends to one firm j. We can think about the bank and firm being linked either through relationship-banking, or through their spatial locations (see Appendix B for empirical evidence on these frictions). Bank profits are given by $\Pi_{it}^{b} = r_{it} L_{ijt}^{1-\beta} - (\bar{c}_t c_{it})^{\gamma} L_{ijt}$. The bank earns an interest rate r_{it} from lending $L_{ijt}^{1-\beta}$, where β captures the degree of decreasing marginal returns to lending. The costs associated with lending are given by $(\bar{c}_t c_{it})^{\gamma}$, where $\bar{c}_t$ is a time-varying common cost component for all banks and c_{it} is a time-varying bank-specific cost component. Maximizing profits with respect to the lending volume, and taking logs, we get the following supply side expression $log(r_{it}) = \gamma log(\bar{c}_t) + \gamma log(c_{it}) - log(1-\beta) + \beta log(L_{ijt})$.

Next, consider a firm j that borrows L_{ijt} from one bank i. Firms require labor to produce output according to the production function $N_{jt}^{1-\alpha}$ where α captures the degree of decreasing returns to scale in production. The price of the good produced is $p_{jt} = \overline{\eta}_t \eta_{jt}$ where $\overline{\eta}_t$ is a common factor across firms and η_{jt} is firm specific. Firms have no internal funds, and use bank loans to finance their wage bill. Due to unmodeled frictions in the credit market, L_{ijt}

dollars borrowed only finance L_{ijt}^{ψ} of the wage bill, where $0 < \psi < 1$[27]. Credit thus serves the role of enabling the firm to finance more labor. Thus, $L_{ijt} = (W_t N_{jt})^{\frac{1}{\psi}}$. We refer to this as the cash-in-advance constraint. Firm profits are given by $\Pi_{jt}^{f} = \overline{\eta}\eta_{jt}N_{jt}^{1-\alpha} - r_{jt}L_{ijt}$. Solving the cash-in-advance constraint for N_{jt}, inserting it into the profit function, maximizing with respect to the borrowing amount L_{ijt}, and taking logs, we get the following demand side expression $log(r_{jt}) = log(\psi\,(1-\alpha)) + log(\bar{\eta}_t) + log(\eta_{jt}) - (1-\psi\,(1-\alpha))\,log(L_{ijt}) - (1-\alpha)log(W_t)$.

In equilibrium, the interest rate paid by the firm equals the interest rate received by the bank, so that $r_{it} = r_{jt}$. Combining the first order conditions for the bank and the firm, and solving for equilibrium lending we have that $log(L_{ijt}) = \frac{log(\psi(1-\alpha))+log(1-\beta)}{\beta+1-\psi(1-\alpha)} + \frac{log(\bar{\eta}_t)-\gamma log(\bar{c}_t)}{\beta+1-\psi(1-\alpha)} + \frac{1}{\beta+1-\psi(1-\alpha)}log(\eta_{jt}) - \frac{\gamma}{\beta+1-\psi(1-\alpha)}log(c_{it}) - \frac{1-\alpha}{\beta+1-\psi(1-\alpha)}log\,(W_t)$. Taking the first difference of this expression, letting $\kappa \equiv \beta + 1 - \psi\,(1-\alpha)$ and denoting $\Delta log(X) = \frac{\Delta X}{X} = \tilde{X}$, with Δ being the change from $t-1$ to t, we get the expression in equation (10). The equation says that the (approximate) percentage change in lending $\tilde{L}_{ijt}$ is given by a common, economy-wide term, a firm-specific term, and a bank-specific term.

$$\tilde{L}_{ijt} = \underbrace{\frac{\tilde{\bar{\eta}}_t - \gamma\tilde{\bar{c}}_t - (1-\alpha)\,\tilde{W}_t}{\kappa}}_{\text{Common factors}} + \frac{1}{\kappa}\underbrace{\tilde{\eta}_{jt}}_{\text{Firm-specific}} - \frac{\gamma}{\kappa}\underbrace{\tilde{c}_{it}}_{\text{Bank-specific}} \tag{10}$$

$$\tilde{N}_{jt} = \psi\tilde{L}_{ijt} - \tilde{W}_t = \frac{\psi\tilde{\bar{\eta}}_t - \psi\gamma\tilde{\bar{c}}_t - [\psi\,(1-\alpha)+\kappa]\tilde{W}_t}{\kappa} + \frac{\psi}{\kappa}\tilde{\eta}_{jt} - \frac{\gamma\psi}{\kappa}\tilde{c}_{it} \tag{11}$$

Our goal is to obtain an unbiased estimate of $\frac{\partial\tilde{L}_{ijt}}{\partial\tilde{c}_{it}} = -\frac{\gamma}{\kappa}$. Note that if $\gamma = 0$ we should not expect to find any effect on lending. To understand why, consider the banks marginal cost of lending, given by $(\bar{c}_t c_{it})^{\gamma}$. If $\gamma = 0$, the banks marginal funding cost is constant. This would be the case if the Modigliani-Miller theorem holds (Modigliani and Miller 1958). An increase in capital requirements would have no impact on loan supply, as the bank can frictionlessly increase its share of equity financing. Hence, we can think of the case in which $\gamma = 0$ as a scenario in which a bank can react to a specific funding shock by shifting into other funding sources at no cost.

Equilibrium employment is given by the expression in equation (11). Here our goal is to obtain an unbiased estimate of $\frac{\partial\tilde{N}_{jt}}{\partial\tilde{c}_{it}} = -\frac{\gamma\psi}{\kappa}$. The credit supply shock should have no effect

[27] One way to microfound this is the presence of moral hazard, where the firm manager gets some private utility from diverting funds away from production. ψ would then capture the strength of the agency problem, with $\psi = 1$ corresponding to the frictionless case and $\psi \to 0$ being a limiting case where the manager diverts all funds, and credit demand for a given wage bill goes to infinity.

on employment if the banks marginal funding cost is constant ($\gamma = 0$), or if there is no link between borrowing and employment ($\psi \rightarrow 0$).

5.1.2 Estimation

As seen from equation (10), the equilibrium lending outcome we observe each period is composed of firm-specific factors, bank-specific factors, and an economy-wide common term. The identification exercise is to isolate the supply effect from firm-specific factors. Note that if supply shocks $\tilde{c}_{it}$ are uncorrelated with demand shocks $\tilde{\eta}_{jt}$, it would *not* be necessary to account for firm-specific factors in order to obtain an unbiased estimate of the impact of the cost shock. However, one might worry that supply and demand shocks are in fact correlated due to firm-bank matching. For example, high-capitalized banks may be lending to firms with positive demand shocks. In this case, not accounting for firm-specific factors would lead us to overestimate the effect of the reform.

Our data covers a period of thirteen years, enabling us to use the flexible difference in difference approach to test whether firms borrowing from low-capitalized and high-capitalized banks have different outcomes prior to the reform. As documented in the previous section, whether a bank is low-capitalized or high-capitalized does not appear to have any impact on our outcome variables prior to the reform. However, one might still worry that firms borrowing from different bank types are different *conditional* on capital requirements being raised, for reasons unrelated to credit supply. This would be a natural concern if the shock we were studying was a large economic downturn like the financial crisis. In this case one might think that firms which looked identical prior to the crisis, could have underlying differences in risk profiles that only mattered conditional on a crisis occurring. We believe this to be of less concern in our case, as we are studying the implementation of a bank reform in normal economic times, designed by EU policy makers, which revealed no new information about bank quality. Regardless, we can address this potential issue by using loan level data and firm fixed effects. By looking at firms which borrow from multiple banks, we can compare lending outcomes for different levels of our cost shock, while holding firm characteristics fixed. The identifying assumption then becomes that credit demand is constant within a firm. That is, credit demand is firm-specific (η_{jt}) rather than loan specific (η_{jit}).

The traditional Khwaja and Mian (2008) estimation uses only cross-sectional variation, and so it is sufficient to include firm fixed effects. In our dynamic difference in difference framework, the analogous regression should include year×firm fixed effects. The identification is then based on firms which borrow from multiple banks, and whether they are more likely to borrow from their high-capitalized relationship bank(s) during the reform period. Note that our dynamic Khwaja and Mian (2008) estimation strengthens the identification relative to the traditional Khwaja and Mian (2008) estimation. By using pre-reform data, we can evaluate whether multiple-bank firms are more likely to borrow from their high-capitalized relationship bank(s) after the reform - *relative* to the pre-reform period. The regression we estimate is given by equation (12), where η_{jt} denotes year×firm fixed effects. Also note that the dependent variable is now the symmetric change in lending between a firm j and a bank i in year t[28]. We follow the literature in using the symmetric change to allow for entry and exit. Our proxy for the cost shock in equation (10), $\tilde{c}_{it}$, will be bank-capitalization levels *prior* to the reform. Any common factors across firms and banks - the first term in equation (10) - will be captured by the constant term and the time fixed effects. Hence, in our model framework, $\hat{\beta}^l$ should provide us with an unbiased estimate of $\frac{\partial \tilde{L}_{ij}}{\partial \tilde{c}_i} = -\frac{\gamma}{\kappa}$.

$$\tilde{\Delta} L_{ijt} = \alpha + \delta_t + \eta_{jt} + \gamma_i^D D_i + \beta^l\, D_i \times I_t^{post} + \epsilon_{ijt} \qquad (12)$$

5.1.3 Results

Including firm×year fixed effects is computational demanding, as we have a very high number of firms in our sample[29]. We therefore focus on a shorter time period around the reform to make the estimation manageable. We start by estimating equation (12) without firm fixed effects. The results are reported in the first column of Table 6. In line with the bank level results, we find that firms which borrow from low-capitalized banks have lower growth in lending in the post-reform period. The effect is significant at the one percent level, and says that firms which borrow from low-capitalized banks have on average 4.0 percentage points lower growth in lending in the post-reform period relative to the pre-reform period.

In the second column we restrict the sample to only include firms with more than one bank connection in 2014[30]. These are the firms which can be used for identification when including firm×year fixed effects in the final column. While only including multiple bank firms reduces the sample substantially, precision falls only slightly. The coefficient increases in size and is still significant at the one percent level of significance[31]. Finally, we add firm×year fixed

[28]The symmetric change is defined as $\tilde{\Delta} X_t = \frac{X_t - X_{t-1}}{0.5\, X_t + 0.5\, X_{t-1}}$ and is bounded by -2 and 2.

[29]There are more than 137,000 unique firms in our cleaned sample, although the majority only borrows from a single relationship bank - and therefore cannot be used in the firm×year fixed effects regression.

[30]88 percent of the firms in our sample borrow from a single banks, 11 percent borrow from two banks, and 1 percent borrow from more than two banks.

[31]Firms which borrow from multiple banks tend to be larger in terms of number of employees. However, we have rerun the regression in equation (12) for small and large firms (not shown), and do not find support

effects in the third column. The identification is now coming from within firm-year variation. The coefficient is as before significant at the one percent level, implying that firms which borrow from multiple banks have lower credit growth at their low-capitalized banks in the post-reform period.

	(1)	(2)	(3)
	$\tilde{\Delta} L_{ijt}$	$\tilde{\Delta} L_{ijt}$	$\tilde{\Delta} L_{ijt}$
$D_i \times I_t^{post}$	-3.950***	-9.887***	-15.13***
	(-4.52)	(-3.20)	(-2.82)
Time FE	yes	yes	yes
Type FE	yes	yes	yes
Firm FE	no	no	yes
Firms	all	multiple banks	multiple banks
Clusters	110	110	110
Observations	135,399	14,617	14,617

t statistics in parentheses, Std. err. clustered at bank level
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 6: Restrictive Difference in Difference Estimation - Lending. Regression results from estimating equation (12)

Two features of the regression results with and without firm×year fixed effects are worth highlighting. First, the increase in capital ratios is estimated to have a larger negative impact on corporate credit supply when we include firm fixed effects. This suggests that the implied correlation between supply and demand shocks is negative, causing us to underestimate the effect of higher capital requirements when not accounting for firm specific factors. In other words, low-capitalized banks lend to firms with higher credit demand, meaning that there is negative firm-bank matching. Second, we note that the difference in coefficient estimates is not statistically different. Hence, we cannot rule out that there is *no* firm-bank matching in our sample. We thus conclude that not accounting for firm specific factors would, *if anything*, bias our results towards zero.

for large firms in general facing larger negative credit effects. If anything smaller firms have larger negative point estimates, although the difference is not statistically signficant. The (not significantly) stronger effect for multiple-bank firms may therefore seem puzzling. One potential explanation is that firms with multiple bank connections are more price sensitive, as they have multiple insider banks they can borrow from.

5.2 Employment

We have documented a significant reduction in corporate lending growth from low-capitalized banks following the reform - both on the bank and firm level. Ultimately, the reason why we care about reductions in credit supply is that it might have adverse impacts on the real economy. We now investigate whether firms borrowing from low-capitalized banks have lower employment growth than other firms in the year following the reform. Note that we expect to find negative effects on employment growth only if there are quantitatively important frictions in firm-bank lending. That is, bank specific shocks will affect firms differentially if it is costly for firms to switch banks. This could be caused by relationship banking or geographical matching[32]. In Appendix B we document that there are indeed substantial frictions in firm bank lending in our sample.

We again rely on the difference in difference framework to compare the employment outcomes of firms borrowing from high and low-capitalized banks. Because there is no variation in employment growth within firms, we cannot include firm fixed effects. However, the results from the previous section means that any bias from not controlling for firm specific factors would work against us.

In the upper row of Figure 9 we compare the growth in employment for firms borrowing from low-capitalized banks to that of firms borrowing from high-capitalized banks[33]. We see that while firms borrowing from low-capitalized banks tend to have higher employment growth - consistent with the negative matching found in the previous section - this is reversed in the year following the reform. Although this is suggestive of a negative employment effect, the difference is not statistically significant and the pre-reform trends do not look parallel. If we restrict the sample to exclude the very high and low-capitalized banks however, the pre-reform trends appear much more comparable. This is illustrated in the lower row of Figure 9, in which we compare quartile 2 banks (25th to 50th percentile) to quartile 3 banks (50th to 75th percentile) - and hence obtain a sample of more similar banks. Prior to the reform, firms borrowing from low-capitalized banks have slightly higher employment growth, with the difference between bank types being small and stable. After the reform however, employment growth for firms borrowing from low-capitalized banks falls, while employment growth for firms borrowing from high-capitalized banks increases. Hence, firms borrowing

[32]See for example Sharpe (1990), Rajan (1992) or Holmstrom and Tirole (1997) for theory on relationship lending. See for example Brevoort, Wolken, and Holmes (2010) for empirical evidence on geographical lending patterns.

[33]Growth rates in employment are calculated as symmetric percentage changes, as several firms have zero employees in some years, which makes the standard percentage change undefined.

from low-capitalized banks have several percentage points lower growth in employment in the year following the reform[34].

Figure 9: Employment. Banks are divided into groups based on their 2012 capital ratio. Left panels: Growth rates in employment for low-capitalized (25th to 50th percentile or 0th to 50th percentile) and high-capitalized (50th to 75th percentile or 50th to 100th percentile) banks. The growth rate for $year_t$ denotes the symmetric percentage change from $year_{t-1}$ to $year_t$. The solid red line marks the growth rate from 2012 to 2013 (the reform year). Right panels: Regression results from estimating equation (3) with dependent variable $Y_{jt} = Employment_{jt}$. Interaction coefficients γ^D_{1t} are plotted relative to year 2012. Standard errors are clustered at the bank level.

We estimate a version of the restrictive difference in difference equation, interacting a dummy for borrowing from a low-capitalized bank with a dummy for the year following the

[34]Note that we would not expect to see any effect in 2013 in this case. The reason is that quartile 2 banks did not start increasing the growth in capitalization levels until 2014 (while quartile 1 banks started in 2013), as illustrated in Figure 13 in the appendix. Also note that a reduction in credit supply to firms borrowing from low-capitalized banks could have positive spillover effects for firms borrowing from high-capitalized banks. If these firms are competing in the same markets, the firms with easy access to credit could benefit from less competition from the credit-constrained firms. Hence, the reduction in credit supply could have a negative impact on firms borrowing from low-capitalized banks and a positive impact on firms borrowing from high-capitalized banks. This seems consistent with the picture in the lower row of Figure 9.

reform. The results are reported in Table 7. The first three columns use the full set of banks, comparing the employment growth of firms borrowing from banks with above and below median pre-reform capital ratios. While firms borrowing from low-capitalized banks are found to have lower employment growth in the year following the reform, the difference is not statistically significant. As previous literature has found smaller firms to be more vulnerable to bank specific shock, we split the sample into firms with above and below 25 employees (the sample average). As seen from the second column, there is no statistically significant effect for the large firms. However, there is a signficantly negative impact on small firms, as seen in the third column. Because the pre-trends looked more similar when excluding the very high and low capitalized banks, we also show results using this restricted sample. The results are reported in the three last columns of Table 7. Firms borrowing from low-capitalized banks are now found to have significantly lower employment growth in the year following the reform. Again, the coefficient increases in magnitude and statistical significance when only considering smaller firms. Note that in terms of our model - if the negative employment effects are larger for smaller firms - this would imply that the agency problems are worse for smaller firms (ψ is smaller).

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta Empl_{jt}$	$\Delta Empl_{jt}$	$\Delta Empl_{jt}$	$\Delta Empl_{jt}$	$\Delta Empl_{jt}$	$\Delta Empl_{jt}$
$D_i \times I_t^{2014}$	-1.931	-0.486	-3.064***	-3.262**	-0.207	-4.717***
	(-1.55)	(-0.18)	(-2.97)	(-2.11)	(-0.06)	(-3.10)
Time FE	yes	yes	yes	yes	yes	yes
Type FE	yes	yes	yes	yes	yes	yes
Banks	all	all	all	25th-75th	25th-75th	25th-75th
Employment	all	25+	<25	all	25+	<25
Clusters	110	110	110	54	54	54
Observations	133,323	42,538	90,785	38,934	11,834	27,100

t statistics in parentheses, Std. err. clustered at bank level
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 7: Restrictive Difference in Difference Estimation - Employment.

6 Further Evidence: Interest Rates and Aggregate Effects

We have documented a substantial reduction in asset growth for low-capitalized banks following the reform, and an especially large reduction in corporate credit supply. While we believe the flexible difference in difference results make a convincing case for the reduction in credit supply being supply-driven, we now provide additional support for this interpretation. While a negative shock to demand and supply have similar implications for lending volumes, they have opposite implications for the price of lending.

Although we do not directly observe interest rates, we observe the amount of outstanding debt and the amount of interest paid. In theory, it is therefore straightforward to back out the implied interest rate. In practice, because the data is annual, this procedure is likely to entail non-trivial measurement error. We address this by cutting the ten percent highest and lowest interest rates from our sample. The resulting interest rate estimates are illustrated in Figure 12 in the appendix. Our interest rate estimates seem consistent with aggregate interest rate data from Statistics Norway[35].

We aggregate the loan level interest rate data to bank level averages, and plot the resulting time series in Figure 10. The left panel compares interest rates for low-capitalized banks (below median) to that of high-capitalized banks (above median). High-capitalized banks have slightly higher interest rates prior to the reform, but this gap closes after the reform. Hence, low-capitalized banks see a relative increase in interest rates post-reform, consistent with the reduction in credit being supply driven. In the right panel of Figure 10 we exclude the 25 percent most and least capitalized banks from our sample. Hence, we compare quartile 2 banks (capital ratios in the 25th to 50th percentile) to quartile 3 banks (capital ratios in the 50th to 75th percentile). Using this more homogeneous group of banks, the results are even more striking. While quartile 2 and quartile 3 banks have almost identical interest rates prior to the reform, quartile 2 banks have consistently higher interest rates than quartile 3 banks in the post-reform period.

[35]The backed-out estimates follow the official numbers closely, although at a slightly higher level. This could be due to a consistent upward bias in our calculations, but could also be due to differences in the banks and firms included in the two data series. For example, the data from Statistics Norway include life insurance companies in their group of lenders, while these do not enter into our sample. Also, the Statistics Norway data include sole proprietorships in their group of borrowers, while our data does not. Including sole proprietorships is likely to reduce the average interest rate, as these are less risky borrowers due to unlimited liability. Regardless, the exact interest rate level is not of first order importance to us, as we are interested in differences (in differences) between the interest rates of high-capitalized and low-capitalized banks. As long as any bias in our interest rate estimates does not systematically vary across bank types - and differentially so pre- and post-reform - it would not alter our main conclusions.

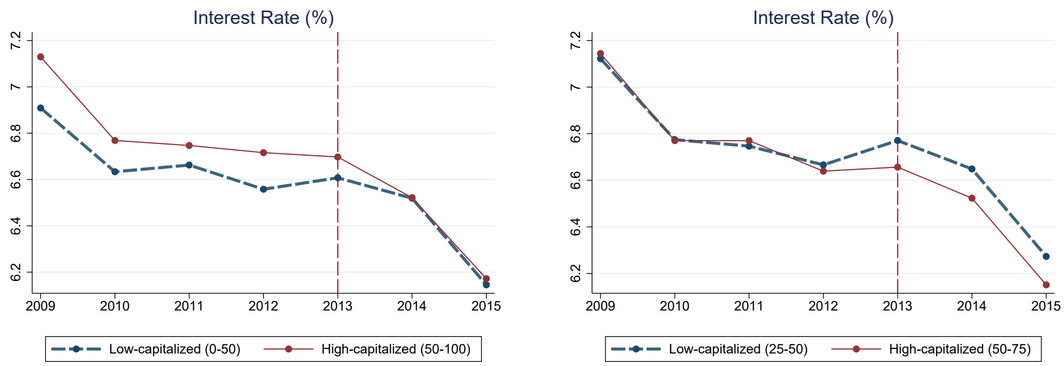

Figure 10: Interest Rates. Banks are divided into groups based on their 2012 capital ratios. Left panel: Interest rates for low-capitalized banks (below median) and high-capitalized banks (above median). Right panel: Interest rates for low-capitalized banks (25th to 50th percentile) and high-capitalized banks (50th to 75th percentile) .

While the results in Figure 10 are visually quite striking, the difference in interest rates between high-capitalized and low-capitalized banks are not statistically different from zero when using the flexible difference in difference approach specified in equation (3) (not shown). However, given the parallel trends observed, we are comfortable estimating the standard difference in difference equation specified in equation (4). The results are reported in Table 8. We find that low-capitalized banks have significantly higher interest rates than high-capitalized banks in the post-reform period.

	(1)
	$InterestRate_{it}$
$D_i \times I_t^{post}$	0.174**
	(2.03)
Time FE	yes
Type FE	yes
Clusters	110
Observations	329,024

t statistics in parentheses, Std. err. clustered at bank level
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 8: Restrictive Difference in Difference Estimation - Interest Rates. Regression results from estimating equation (4).

6.1 Aggregate Effects

Our cross-sectional results can only identify a reduction in credit growth from low-capitalized banks *relative* to that of high-capitalized banks. In principle, it is therefore possible that high-capitalized banks were able to "pick up the slack" resulting from reduced credit supply from low-capitalized banks - leaving aggregate credit supply unaffected. We think this is unlikely due to three features of the data. First, because all the largest banks are low-capitalized, the combined market share of low-capitalized banks vastly exceeds that of high-capitalized banks. Hence, it seems practically difficult for high-capitalized banks to absorb all the excess demand. Second, as shown in Figure 15 in Appendix C, we can explicitly calculate the number of firms which switch from low-capitalized bank to high-capitalized banks each year. There is no trend break in this series at the time of the reform, suggesting that the reform does not cause firms to switch banks. Finally and perhaps most importantly, the negative effect on employment provides indirect evidence that high-capitalized banks are not (fully) picking up the slack. If firms which were denied credit simply shifted to another bank, then there should be no differential effects on firm employment growth. Hence, we find it overwhelmingly likely that there was a reduction in aggregate credit supply. In Appendix C we make use of some additional assumptions to back out plausible bounds for the impact on aggregate credit supply from our cross-sectional results.

7 Concluding Remarks

We have documented that low-capitalized banks increased their capital ratios mainly by reducing the growth in risk weighted assets. This was done primarily by reducing average risk weights. Consistent with the reduction in average risk weights, we found that low-capitalized banks reduced corporate lending relative to household lending. Back-of-the envelope calculations suggested that the shift from corporate lending to household lending could account for roughly 80 percent of the fall in average risk weights. Reassuringly, we also found that low-capitalized banks increased their interest rates, which supports the interpretation of the reduction in lending being supply driven. Further, our results were robust to controlling for firm specific factors, suggesting that firm-bank matching is not an issue in our sample. The reduction in corporate credit supply was found to reduce employment growth for affected firms. Firms which borrowed from low-capitalized banks prior to the reform had lower employment growth following the increase in capital requirements.

We believe our results have implications for understanding the effectiveness of the countercyclical capital buffer, introduced in many countries as part of the Basel III regulation. While the main goal of this time-varying requirement is to make banks increase their capital ratios when times are good, it has also been suggested that the buffer can be used to smooth the credit cycle (Ministry of Finance, 2016). Norwegian authorities have a handful of indicators they look at when deciding whether the countercyclical capital buffer should be increased, one of which is rapid growth in household debt. If banks respond to higher capital requirements by reducing credit supply to the household sector, the countercyclical capital buffer could have a dampening effect on the credit boom. However, our results suggest that lending to the household sector is mostly unaffected by capital requirements. Hence, a dampening effect on the growth in household debt seems unlikely. It is important to highlight however, that this result is conditional on the current risk weights. Reducing the difference in risk weights between mortgages and corporate lending would likely lead to more of the reduction in credit supply being directed towards the household sector. More generally, the allocation of credit across sectors matters for the macro economy, and hence should be part of the discussion surrounding the design of capital requirements. Our finding that the reduction in credit supply is directed towards firms rather than households could be undesirable for several reasons. First, the Norwegian housing market was booming in 2013 and policy makers were concerned about unsustainable price growth (IMF, 2013). Hence, a reduction in household lending would probably have been preferred to the observed decline in corporate lending. Second and more generally, we found that the reduction in firm lending lead to lower employment growth. Relatedly, and as noted in Beck, Büyükkarabacak, Rioja, and Valev (2012), directing credit away from the corporate sector towards the household sector could have detrimental impacts on the long-term growth potential of the economy.

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Appendix A: Additional Figures

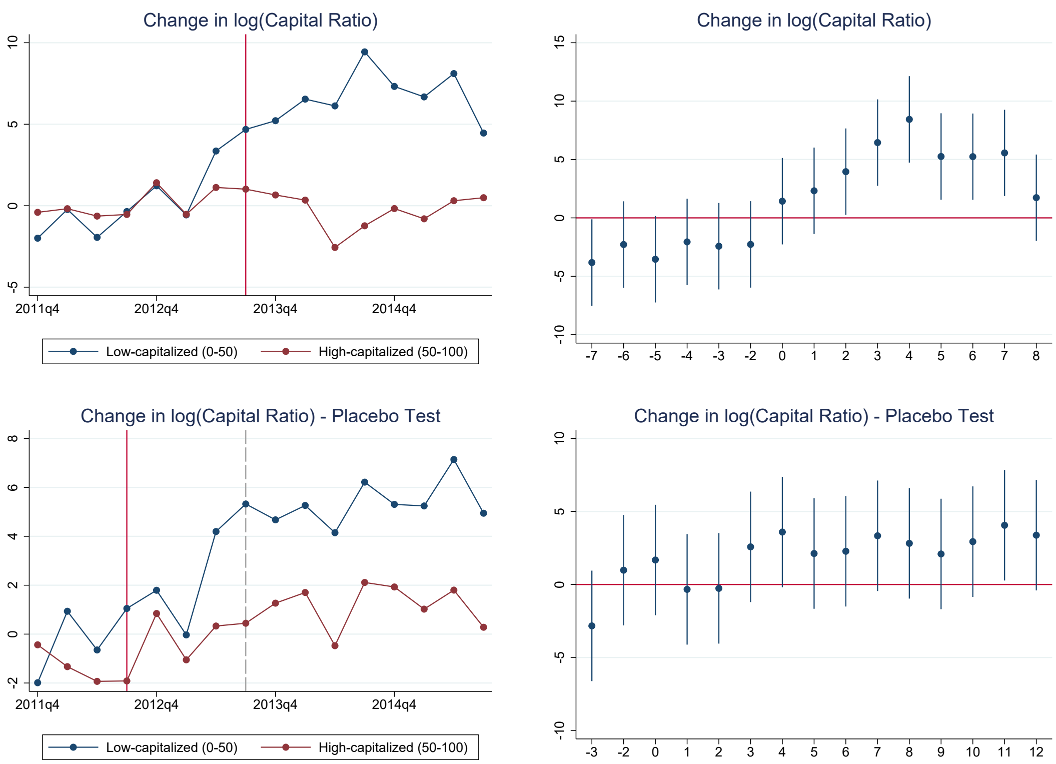

Figure 11: Placebo Test - Change in log(Capital Ratio) for low and high capitalized banks.

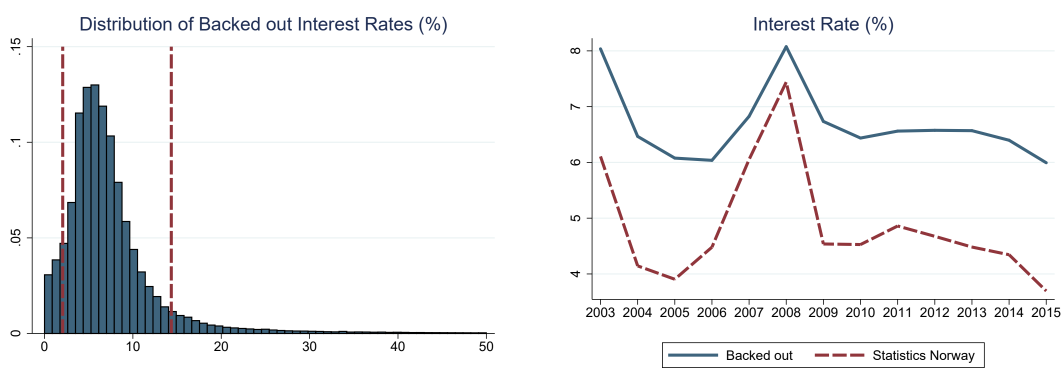

Figure 12: Interest Rates. Left panel: Distribution of backed out interest rates. Right panel: Aggregate backed out interest rates and aggregate interest rates on corporate lending from Statistics Norway

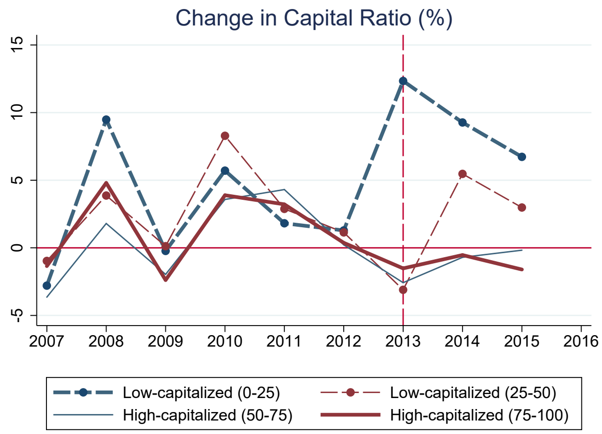

Figure 13: Capital Ratios. Banks are divided into groups based on their 2012 capital ratios. Change in capital ratios for banks in quartile 1 (0th to 25th percentile), quartile 2 (25th to 50th percentile), quartile 3 (50th to 75th percentile) and quartile 4 (75th to 100th percentile).

Appendix B: Frictions in Firm Bank Lending

In this appendix, we document two quantitatively important frictions in firm bank lending. First, there is inertia in bank lending, meaning that firms tend to borrow from their insider bank. Second, there is geographical matching, meaning that firms are more likely to borrow from a local bank.

If past bank relationships have no predictive power on future bank relationships, the probability of borrowing from any bank is constant across firms. We define *random switching* as the case in which current lending relationships between bank i and firm j, L_{ij}, are independent of past lending relationships $L_{ij,-1}$. Given random switching, the probability that firm j switches to a new bank is given by $1 - \Pr(L_{ij}|L_{ij,-1}) = 1 - \Pr(L_{ij}) = 1 - M_i$, where M_i denotes the market share of bank i. To obtain the *observed* switching probability we calculate the number of firms obtaining new loans from a *new* bank relative to the number of firms obtaining new loans from *any* bank. That is, $\Pr(\text{Switch})^{\text{observed}} = \frac{\# L_{ij}|\neg L_{ij,-1}}{\# L_{ij}}$, where L_{ij} denotes a new loan from bank i to firm j in the current period.

The left panel of Figure 14 shows the calculated random switching probabilities and the observed switching probabilities. The random switching probabilities vastly exceed the observed switching probabilities, suggesting that previous bank relationships do have power in predicting future bank lending. The random switching probability exceeds 80 percent, compared to an observed switching probability of somewhere between 10 and 20 percent.

To formally test for firm-bank stickiness, we restrict the sample to firms which are acquiring a new loan in a given period. We then estimate how the likelihood of obtaining a loan from bank i depends on already having outstanding debt with bank i[36]. That is, we run the regression specified in equation (13). The dependent variable I_{ij} is equal to one if firm j obtains a new loan from bank i, and zero otherwise. The independent variable $\text{Bank}_{ij,-1}$ is equal to one if firm j had outstanding debt at bank i in the previous period, i.e. if bank i is an insider bank to the firm. Based on the low degree of observed switching documented in Figure 14 we expect a positive and large $\hat{\beta}$.

$$I_{ij} = \alpha + \beta \text{Bank}_{ij,-1} + \epsilon_{ij} \tag{13}$$

The regression results are reported in Table 9. The second column controls for bank market size by year, and gives an estimate of β equal to 0.88. Hence, the estimated probability

[36]We define previous banking relationships based on debt, which seems to be the common practice in the literature. However, one could imagine that also deposits could be included in the definition.

of switching banks conditional on taking up a new loan is just above ten percent[37]. This is somewhat lower than in Chodorow-Reich (2014), which is not surprising as our sample consists of smaller firms which have been documented to have stronger attachments to their insider banks (Ongena and Smith, 2001).

	(1) L_{ij}	(2) L_{ij}
$Bank_{ij,-1}$	0.892***	0.880***
	(123)	(104)
Bank FE	-	yes
Observations	6,470,744	6,470,744

t statistics in parentheses. Std. err. clustered at bank × year level
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 9: Relationship Lending. Regression results from estimating equation (13).

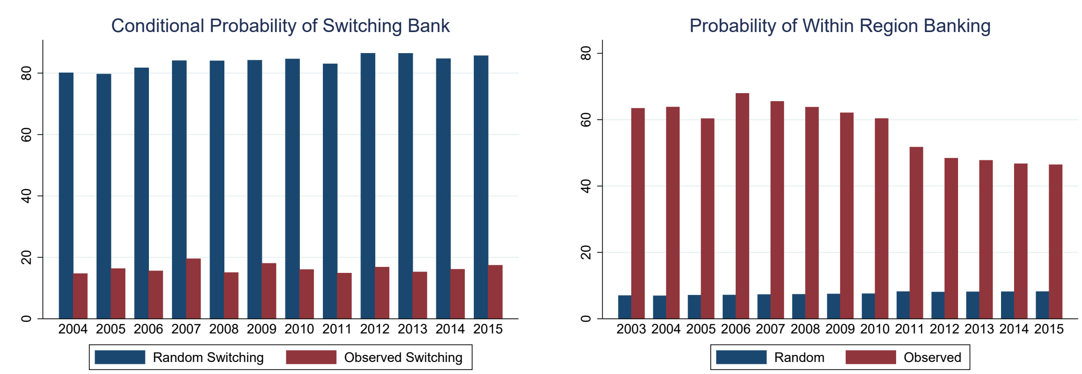

Figure 14: Observed and Random Switching Probabilities

To evaluate whether spatial frictions are important in our data, we calculate the observed probability that a firm j borrows from a bank i in its own region. Let $C_{ij} = 1$ if the region of firm j coincides with the region of its insider bank i, and zero otherwise. We define the observed matching as the average matching occurrence across firms $\Pr(\text{match})^{\text{observed}} = \bar{C}_{ij}$. We compare this to a *random matching* probability, calculated under the assumption that spatial locations have no predictive power on firm-bank lending. Given random matching, the probability that a firm i borrows from a bank in its region is simply given by the sum of the market shares M_i of all banks located in its region, i.e. $\Pr(\text{match}_j)^{\text{random}} = \sum_{i \in \text{match}_i} M_i$. The

[37] We follow the literature in using the term *switching* to capture both firms that actually switch banks, as well as firms that add a new bank connection without terminating the previous one.

aggregate matching probability under random matching is simply the average across all firms in our sample. Hence, the random matching probability depends on the spatial distribution of bank market shares and the spatial distribution of firms. The right panel of Figure 14 illustrates the degree of county matching in our sample, as well as the counterfactual matching under the assumption of random matching. On average, around 55 percent of lending relationships are within-county. This compares to a predicted matching of less than 10 percent if geography was irrelevant

Appendix C: Aggregate Corporate Credit Supply

In order to back out the reform-induced increase in capital ratios and credit supply we need to make use of some additional assumptions. First, we will abstract from any general equilibrium effects. Our cross-sectional results would not capture such effects. Second, we need an extra assumption in order to go from relative changes to aggregate changes. That is, we know that low-capitalized banks increased their capital ratios and reduced their credit growth *relative* to high-capitalized banks, but we do not know how these changes were distributed. For example, one could make the case that high-capitalized banks were unaffected by the reform, while low-capitalized banks reacted to the reform by substantially reducing credit growth. However, one could also imagine high-capitalized banks picking up some of the slack resulting from the reduction in credit supply from low-capitalized banks. In this case, credit supply for both bank types will be affected. Hence, the correct way to translate our cross-sectional results into aggregate results depends on the degree of spillovers.

Formally assessing the degree of spillovers is challenging. It is likely to depend on, among other things, the spatial distribution of low-capitalized and high-capitalized banks, the importance of spatial barriers and the degree of relationship banking. While we find it plausible that there is some degree of spillovers, we argue that it appears to be of limited magnitude. This is based on three features of the data. First, while there is the same number of low-capitalized and high-capitalized banks by construction, the two bank types account for very different market shares. Because the largest banks in our sample are all low-capitalized, the group of low-capitalized banks account for roughly 90 percent of corporate lending volumes. This makes it practically difficult for the high-capitalized banks to absorb a quantitatively important share of the credit demand usually directed at low-capitalized banks.

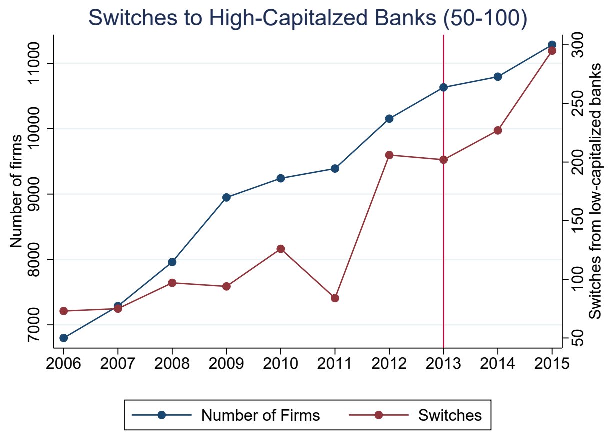

Figure 15: Spillovers. Number of Firms Switching from Low-Capitalized Banks to High-Capitalized Banks

Second, our loan level panel data allows us to explicitly calculate the number of firms that switch from low-capitalized banks to high-capitalized banks per year. The result of such a calculation is captured by the red line in Figure 15. Only between 50 and 250 firms switch from low-capitalized banks to high-capitalized banks each year, reflecting the low combined market share of high-capitalized banks. The number is growing over time, following the growth in the total number of firms as captured by the green, dashed line[38]. While we do not observe the counterfactual evolution of switches in absence of the reform taking place, we see no break in the time trend post-reform.

Finally, any evidence of real effects would suggest that low-capitalized banks are not (fully) picking up the slack caused by reduced credit supply from low-capitalized banks. We documented in Section 5 that there is a negative impact on employment, suggesting that aggregate credit supply falls as a result of the reform. Although we believe the magnitude of spillovers to be limited, we calculate the aggregate credit supply effect under a range of different assumptions, some of which allow for substantial spillovers.

In order to back out the aggregate effect on credit supply we rely on one of the alternative assumptions listed below. We chose these assumptions to provide plausible upper and lower bounds for the aggregate impact on credit supply.

1. *Unaffected at the top*: In absence of the reform all banks would have changed their

[38]We do not know for certain what is causing the drop in switches from low-capitalized banks to high-capitalized banks in 2011. There was an unusually high number of mergers and acquisitions in 2010 and 2011, involving relatively large banks. Although we account for the direct effect of mergers, there might still be indirect effects that are showing up in our calculations.

capital ratios by the same amount as high-capitalized banks

2. *Unchanged trends*: In absence of the reform low-capitalized and high-capitalized banks would have changed their capital ratios by their respective pre-reform trend levels

3. *Zero change*: In absence of the reform there would be no changes in capital ratios

First we assume that high-capitalized banks were unaffected by the reform. In this case we use the average change in capital ratios for high-capitalized banks as the counterfactual. That is, we assume that all banks would have changed their capital ratios by the same amount as high-capitalized banks in absence of the reform. Here we define high-capitalized banks to mean the 50 percent most highly capitalized banks, but we obtain similar results if we instead consider only the 25 or the 10 percent highest capitalized banks to be unaffected. Note that this counterfactual implies no spillovers, and so will provide us with an upper bound for the aggregate effect of the reform. Because we have data for several years prior to the reform, we can also calculate counterfactual changes in capital ratios based on pre-reform time trends. The challenge with this approach is that capital ratios have been increasing steadily since the financial crisis, partly due to expectations of higher capital requirements. Hence, assuming that banks would have continued on their pre-reform trends is likely to underestimate the effect of the reform. The aggregate impacts backed out under this assumption therefore provides a plausible lower bound for the effect of the reform. Finally, we also make use of an intermediate assumption, in which we assume that banks would have kept their capital ratios unchanged in absence of the reform. Both of the latter assumptions allow for substantial spillovers between bank types.

The left panel of Figure 16 depicts the observed and counterfactual changes in capital ratios. We calculate a weighted average change based on the market shares of high-capitalized and low-capitalized banks, as this is the most economically interesting outcome. Because all the market leaders are low-capitalized, this implies that low-capitalized banks receive a larger weight in the aggregated time series. Table 10 reports the cumulative changes in capital ratios and credit supply from 2012 to 2015. Capital ratios increased by 12.5 percent from 2012 to 2015. This is captured by the solid blue line in the left panel of Figure 16. Our counterfactual assumptions imply an increase in capital ratios over the same period of -4.0 to 5.5 percent. The reform-induced increase in capital ratios is largest when we assume that high-capitalized banks were unaffected, and smallest when we assume unchanged time trends. Hence, even our most conservative assumption implies that the reform caused capital ratios to increase by an additional seven percentage points, or more than twice as much as in absence of the reform.

The credit supply effects are illustrated in the right panel of Figure 16 and reported in Table 10. From 2012 to 2015, observed credit supply increased by 8.6 percent. This is captured by the solid blue line in the right panel of Figure 16. If capital ratios had behaved according to our counterfactual scenarios, the increase in credit supply would have been substantially larger, especially in the year immediately following the reform. Given the assumptions of high-capitalized banks being unaffected by the reform, credit supply would have increased by 39 percent over this three-year period. Given the assumption of zero change, credit supply would have increased by 31 percent. Finally, if we instead assume that low-capitalized and high-capitalized banks would have changed their capital ratios according to their respective pre-reform trends, credit supply would have increased by 24 percent from 2012 to 2015. Hence, even our most conservative assumption implies that the weighted average increase in credit supply in the three years following the reform would have been almost three times higher in absence of the reform.

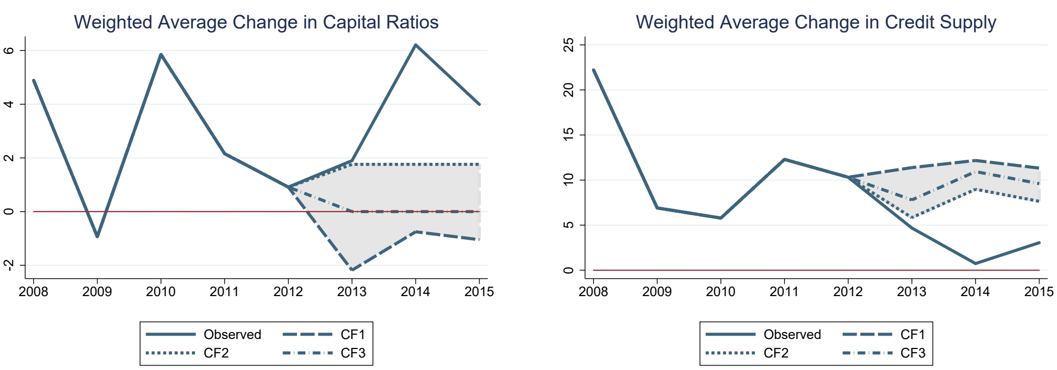

Figure 16: Observed and Counterfactual Changes in Capital Ratios and Credit Supply

	Cumulative Change in Capital Ratios	Cumulative Change in Credit Supply
Unaffected at the top	- 4.0 %	39 %'
Zero change	0 %	31 %'
Unchanged trends	5.5 %	24 %'
Observed	12.5 %	8.6 %'

Table 10: Cumulative Weighted Average Observed and Counterfactual Changes in Capital Ratios (%-change) and Credit Supply (symmetric %-change) from 2012 to 2015