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 by reducing risk-weighted assets. Most 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 and that this effect is driven by small firms.

JEL-codes: E51, G21, G28

Keywords: Banking, capital requirements, macroprudential regulation.

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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 next year, 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. 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.

In this paper we use the Norwegian implementation of the Basel III requirements to decompose the increase in capital ratios into increases in equity, reductions in total assets and reductions in average risk weights. Further, we use administrative loan level data on the universe of Norwegian firms to investigate how different firm types are affected, and to trace out the effects on the real economy.

Capital requirements for Norwegian banks increased substantially in 2013. Low-capitalized banks had to increase their capitalization levels in order to fulfill the new requirements, whereas high-capitalized banks did not. The main result of this paper - illustrated in Figure 1 - is that low-capitalized banks increased their capitalization mainly by reducing *corporate* credit growth. While low-capitalized and high-capitalized banks look virtually identical prior to the reform, there is a large gap opening up in lending growth to the corporate sector as capital requirements are increased in early 2013. The unaffected 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.

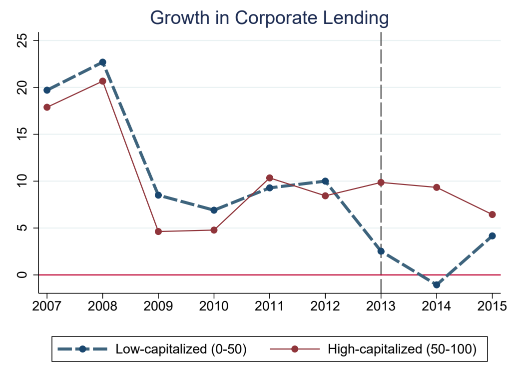

Figure 1: Corporate lending growth for low-capitalized and high-capitalized banks. We divide banks into groups based on their 2012 capital ratio.

The key identification challenge is to disentangle supply from demand. Our empirical strategy 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. Informally, our main identification relies on the fact that low- 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. We exploit this in a flexible difference in difference framework, explicitly testing for parallel trends prior to the reform.

Using loan level data, we also include industry×size×year fixed effects in an attempt to control for credit demand. Further, for firms borrowing from multiple banks we include firm×year fixed effects, thereby relying only on within firm-time variation for identification. Finally, given the nature of the data we can also test whether low-capitalized banks increase interest rates in response to the reform, suggesting that the reduction in corporate credit is supply-driven.

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. The tax data has the benefit of also including small firms, often missed in loan level analysis due to data availability. 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 13 percent of the reform-induced increase in capital ratios. However, this channel is not statistically significant. Capital ratios are mainly increased by reducing the growth in risk-weighted assets. 36 percent of the reform-induced increase in capital ratios is due to lower growth in total assets, and 51 percent is due to a reduction in average risk weights. Hence, substituting low risk assets for high risk assets is the quantitatively most important channel in explaining the increase in capital ratios following the reform. We refer to this channel as "portfolio rebalancing".

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 1-1.4 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 confirm that firms which borrow from low-capitalized banks prior to the reform have lower credit growth. Finally, we show that a negative credit supply shock leads to lower employment growth on the firm level. Firms borrowing from low-capitalized banks have approximately 0.l standard deviations lower employment growth after the reform. 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.

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 the Spanish dynamic provisioning scheme 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), which compare banks experiencing an increase in capital requirements to other banks across Europe. They show that banks respond to capital requirements by reducing risk-weighted assets rather than increasing equity.

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 instance, we show that the portfolio rebalancing effect is relatively short-lived compared to the effect on total asset growth. Second, after having established that a reduction in average risk weights is an important margin of adjustment, we use the richness of our data to dig deeper into *how* banks reduce average risk weights. We show that the shift from corporate lending to household lending can explain roughly 80 percent of the observed decline in average risk weights.

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. 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. Gropp, Mosk, Ongena, and Wix (2017) do not find significant employment effects, potentially due to this sample selection issue. Using data on *all* limited liability firms, we find that the negative employment effect is exclusively driven by smaller firms. Hence, the real effects of increased capital requirements would be substantially understated if smaller firms are excluded from the analysis.

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. These requirements are usually risk-weighted, in order to account for differences in risk 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.[1] Risk-weighting implies that assets with higher risk weights require banks to hold more equity relative to assets with lower risk weights. 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

[1] 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.

excluding IRB-banks from the sample. Hence, we think of the risk weights as being outside of the banks' control.

A simplified version of a bank's risk-weighted capital ratio is given by equation (1). The bank's capital is equal to the bank's equity, denoted by E. Bank assets are denoted by A and risk weights by α.

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

Following the financial crisis of 2007/2008, the Basel III accord was put forward by the Basel Committee on Banking Supervision (BCBS 2010). One of the prominent features of the Basel III accord was to increase the lower bound on banks' capital ratios. 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 the Basel III accord was accompanied by new liquidity requirements. In Norway however, 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). We therefore believe that an advantage of investigating Norwegian banks' response to Basel III 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 in late March 2013, passed in late June and adopted on the 1st of July the same year. The new requirements were phased in over a two-year period. As in the EU-legislation, 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 - set to vary between 0 and 2.5 percent. As a result, Norwegian banks faced a maximum requirement of 12.5 percent. In addition, there was an additional requirement for two systemically important banks. Only one of these banks is in our sample, and all results are robust to dropping this bank from the analysis. The requirements, along with the aggregate capital ratio, are illustrated in Figure 2.[2]

[2] 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.

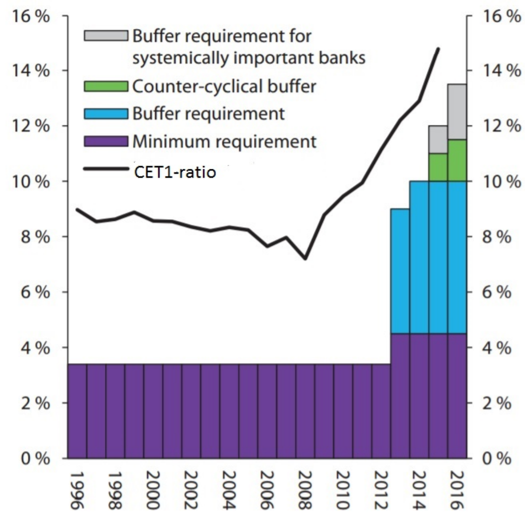

Figure 2: Risk-weighted capital requirements for Norwegian banks. Source: Ministry of Finance.

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). 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.

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 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 of 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.[3] 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 data source.

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

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. As an alternative, we also consider 1-quarter growth.[4] We use 2013q2 as our reform quarter, but it is possible that banks started reacting already in 2013q1. 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. In our main analysis we use type and quarter interactions, which allows us to be agnostic about when the reform came into effect.

The average (median) capital ratio prior to the reform was 16.2 (15.9) percent. The distribution is depicted in Figure 6 in Appendix A. Roughly 1/4 of the banks in our sample had capital ratios below the new (maximum) requirement of 12.5 percent. As expected, banks responded to the reform by increasing their capitalization levels. 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. We also note that the right tail of the distribution remained relatively unchanged, reflecting that the high-capitalized banks did not change their capital ratios in response to the reform.

Summary statistics for 2012q4 are reported in Table 1. The average bank has assets worth roughly USD 3,000 million, while the largest bank has more than USD 200,000 million 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 as 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. However, the distinction between commercial and savings banks in Norway is not very clear. 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.

[4]Thanks to an anonymous referee for suggesting this.

Variable	Mean	Median	Std.dev.	Min.	Max.	Obs.
Capital Ratio (%)	16.2	15.9	4.2	9.7	31.3	119
Assets (million 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 2012q4. NOK/USD = 8.61 (5/8/2017)

Most of our analysis will rely on dividing banks into two groups based on their pre-reform capital ratios. In Table 7 in Appendix B we report differences in key observables between low- and high-capitalized banks. On average, high-capitalized banks are smaller, have higher loan-to-asset ratios, and rely more heavily on deposit financing. They are also more likely to be savings banks and less likely to be IRB-banks. 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. Using this sample, the only statistically significant difference between low- and high-capitalized banks is that the latter relies more heavily on deposit financing. In the appendix we confirm that our results are robust to controlling for all the variables listed in Table 1.

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-year fixed effects to hold demand factors fixed. Firm-year 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. As an alternative approach, we also include industry×size×year fixed effects for the full sample.

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. 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. Taking logs and first differences of equation (1) yields

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

where $\overline{\alpha} \equiv \dfrac{\sum \alpha_i A_i}{\sum A_i}$ is the average risk weight on the bank's assets. 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 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. We address this critical issue in three ways.

First, we use a flexible difference in difference methodology to explicitly test whether low- and high-capitalized banks have similar outcomes prior to the reform. Later, in Section 5, we use loan level data and saturate our regression with industry×size×year fixed effects in an attempt to control for credit demand. Further, we follow Khwaja and Mian (2008) in including firm×year fixed effects. In this case, the effect of bank capitalization on credit supply is identified while holding firm×year 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. Because a negative supply and demand shock have different implications for prices, 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 $D_i = 1$ if bank i is low-capitalized, and $D_i = 0$ if bank i is high-capitalized, to capture exposure to the reform. The coefficients of interest are the γ_t'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-quarter, relative to the average difference between the two bank types.

We can directly test the parallel trends assumption by testing whether $\gamma_t = 0 \ \forall \ t < 0$, using $t = 0$ to capture the time of the reform. Given that the parallel trends assumption holds, the treatment effects will be captured by the γ_t's for $t \geq 0$. A comparison of the γ_t's for $t \geq 0$ will allow us to map out the dynamic treatment effects.

$$\Delta log(Y_{it}) = \alpha + \sum_{\tau} \delta_\tau \mathbf{1}_{t=\tau} + \gamma D_i + \sum_{\tau} \gamma_\tau D_i \times \mathbf{1}_{t=\tau} + \epsilon_{it} \tag{3}$$

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 bank type with time dummies, we now interact bank type with a dummy for the full post-reform period. That is, $I_t^{post} = 1$ if $t \geq 0$, and $I_t^{post} = 0$ 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.

$$\Delta log(Y_{it}) = \alpha + \sum_{\tau} \delta_\tau \mathbf{1}_{t=\tau} + \gamma D_i + \beta D_i \times I_t^{post} + \epsilon_{it} \tag{4}$$

Standard errors are clustered at the bank level. The baseline estimates are based on regressions without control variables. In Appendix B, we also report results controlling for numerous variables such as size, average risk weights, asset composition, deposit financing, return on equity and organizational structure. Our results are largely unchanged.

3.2 Results

The upper left panel of Figure 3 plots $\Delta \log(\text{Capital ratio}_{it})$ for low-capitalized and high-capitalized banks.[5] In the time prior to the reform, low-capitalized and high-capitalized banks have similar changes in capital ratios. 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 ratio growth rates is coming to an end. The upper right panel depicts the coefficient estimates from equation (3) and shows that low-capitalized banks have significantly higher growth in capital ratios in all periods following the reform.

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 pattern, we perform a falsification 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 2012q2. As illustrated in Figure 8 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.[6]

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 3. The left panel depicts growth rates in equity for low- and high-capitalized banks, while the right panel depicts the coefficient estimates when $Y_{it} = \text{Equity}_{it}$. 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 2014q1. Both bank types increase the growth in equity, but the magnitude is larger for

[5] All results from this section using 1-quarter growth rates are shown in Appendix A.

[6] Another potential concern is that the observed divergence between low- and high-capitalized banks is affected by a policy rate cut by the Norwegian central bank in 2014q4. To the extent that monetary policy affects low- and high-capitalized banks differentially, this could potentially confound our estimates. In an unreported falsification test, we have compared the evolution of low- and high-capitalized banks during a prior policy rate cut. There are no significant differences between the two bank types, suggesting that monetary policy has roughly similar impacts on low- and high-capitalized banks.

low-capitalized banks and borderline insignificant. 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 2013, contributing to higher equity growth through retained earnings.[7]

We next move on to consider the impact on assets in the third row of Figure 3. The growth in assets for low- and high-capitalized banks are plotted in the left panel. 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 banks on the other hand, increase their growth rates. This difference is statistically significant and also relatively persistent.

Finally, we study the effect on average risk weights, and plot the results in the bottom row of Figure 3. 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. There is a slight reduction in the relative growth of average risk weights for low-capitalized banks at the onset of the reform, followed by a larger and statistically significant reduction in 2014. The effect is quantitatively larger than for the other outcome variables, although less persistent than the reduction in asset growth.

[7]The increase in equity for low-capitalized banks occurs in 2014q1, as illustrated in Figure 7 in Appendix A. There was no raising of external equity for Norwegian banks in 2014, suggesting that any increase in equity was primarily due to retained earnings.

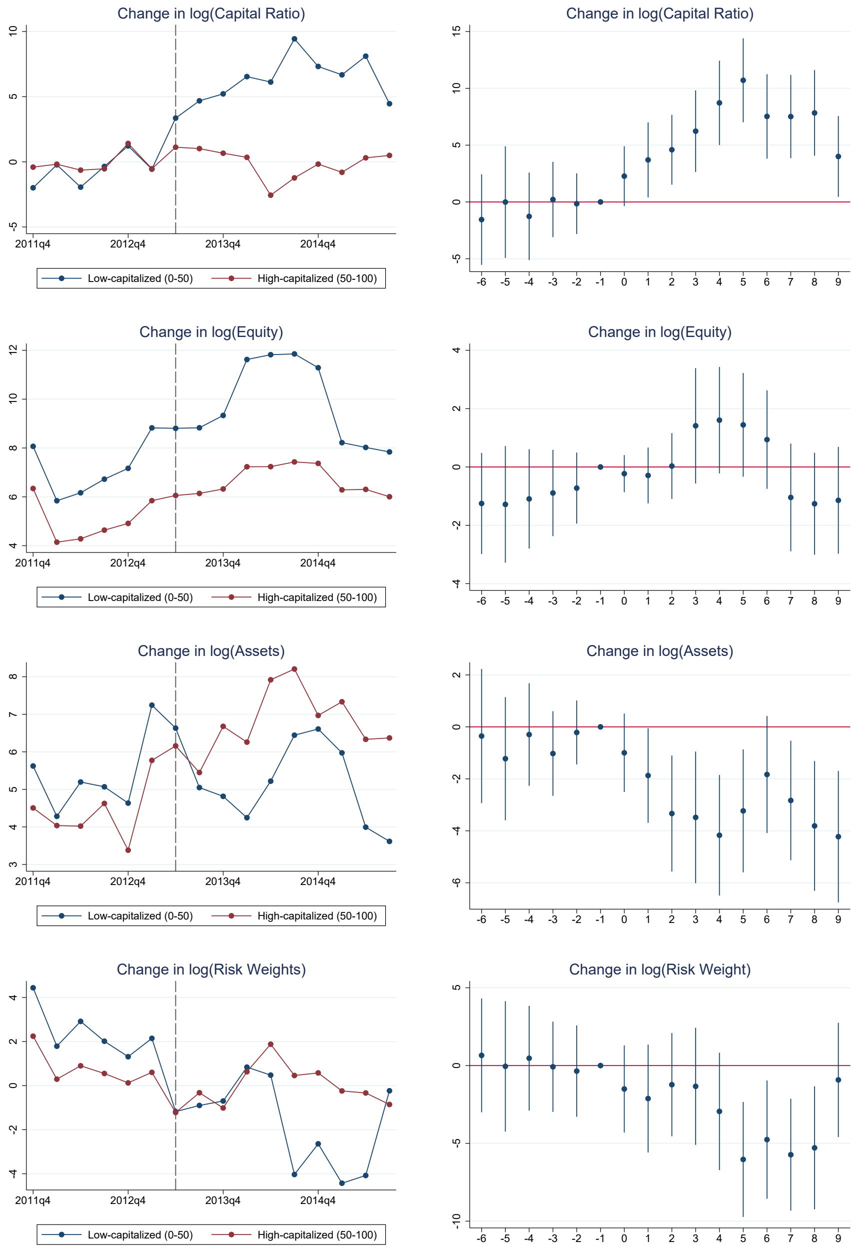

Figure 3: Capital ratios, equity, assets and average risk weights. Banks are divided into groups based on their 2012 capital ratio. Left panels: Growth rates for low-capitalized (below median) and high-capitalized (above median) banks. The growth rate for $year_t$-$quarter_i$ denotes the (approximate) percentage change from $year_{t-1}$-$quarter_i$ to $year_t$-$quarter_i$. The solid red line marks the growth rate from 2012q2 to 2013q2 (the reform date). Right panels: Regression results from estimating equation (3). Interaction coefficients γ_t are plotted relative to time $t = -1$. Standard errors are clustered at the bank level. Time zero marks the growth rate from 2012q2 to 2013q3 (the reform date).

The flexible difference in difference regressions confirms that the parallel trends assumption holds for all our outcome variables. Hence, we are comfortable estimating the more restrictive difference in difference regression in equation (4). In Table 2 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 6.3 percentage points higher growth in capital ratios than high-capitalized banks. The difference is significant at the one percent level.

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 0.8 percentage points higher growth in equity than high-capitalized banks. This difference is however not statistically significant.

Column 3 reports results using the growth in assets as the dependent variable. In the post-reform period, low-capitalized banks had on average 2.3 percentage points lower growth in assets than high-capitalized banks. The difference is significant at the five percent level. Finally, column 4 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.

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.[8] A one percentage point higher reform-induced growth rate in capital ratios leads to an increase in equity growth of 0.13 percentage points, a decrease in asset growth of 0.36 percentage points, and a decrease in the growth rate of average risk weights of 0.51 percentage points.

Results using 1-quarter growth rates instead of 4-quarter growth rates are reported in the lower panel of Table 2. Reassuringly, the main conclusions remain unchanged. Our results are also robust to adding the control variables listed as summary statistics in section 2.2. The results when control variables are included in the regression are reported in Table 8 in Appendix B.

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

	(1)	(2)	(3)	(4)
	$\Delta log(Cap.Ratio_{it})$	$\Delta log(Equity_{it})$	$\Delta log(Assets_{it})$	$\Delta log(RiskWeight_{it})$
$D_i \times I_t^{post}$	6.33***	0.83	-2.25**	-3.25***
	(5.23)	(1.23)	(-2.34)	(-2.72)
Share of response	**100%**	**13%**	**36%**	**51%**
Growth rate	4q	4q	4q	4q
Time FE	yes	yes	yes	yes
Type FE	yes	yes	yes	yes
Clusters	120	120	120	120
Observations	1,788	1,788	1,788	1,788
$D_i \times I_t^{post}$	1.42***	0.10	-0.49*	-0.84**
	(3.85)	(0.53)	(-1.87)	(-2.21)
Share of response	**100%**	**7%**	**35%**	**59%**
Growht rate	1q	1q	1q	1q
Time FE	yes	yes	yes	yes
Type FE	yes	yes	yes	yes
Clusters	120	120	120	120
Observations	1,793	1,793	1,793	1,793

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

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

Table 2: Restrictive difference in difference estimation with 4q growth rates (upper panel) and 1q growth rates (lower panel).

Based on the preceding analysis, we 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. In the next section, we further explore why this is the case.

4 Portfolio Rebalancing

4.1 Why do banks rebalance their portfolio?

In this section we analyze under which conditions portfolio rebalancing is an optimal response to higher capital requirements. 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 $\boldsymbol{A} = \{A_1, \dots A_n\}$ in n lending markets. For instance, we can think of A_1 as being single-family mortgages, A_2 as being corporate loans to $BB+$ rated public corporations etc. The remainder of the bank's 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 risk weights corresponding to the respective loan categories.[9]

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-owner's 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

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

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,

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

the banks overall holdings of risky assets fall. How is the provision of credit to various sectors affected? This depends on the 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 further 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 $\frac{\alpha_1}{\alpha_2} = \frac{\rho_1}{\rho_2}$. 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.

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 firm lending. We now proceed to investigate whether this is indeed the case.

Our quarterly balance sheet data for corporate lending starts in 2012. In order to obtain a longer time series for corporate lending we aggregate the annual loan level tax data into a time series for corporate bank lending. Results using the balance sheet data are depicted in Appendix A. Here we focus on the tax data in order to obtain a longer sample. However, both data sources provide similar conclusions.

The results using annual tax data are depicted in Figure 4. First, note that low- and high-capitalized banks look very similar prior to the reform. After the reform, their lending behavior diverges however. While high-capitalized banks continue to have fairly stable growth rates in firm lending, the growth rate in firm lending for low-capitalized banks plummets. Low-capitalized banks even experience negative corporate credit growth in the year following the reform.

We report interaction coefficients from estimating equation (3), using the annual change in firm lending as our dependent variable. These interaction coefficients are depicted in the right panel of Figure 4. Prior to the reform, the interaction coefficients are small and insignificant. Post-reform, the interaction coefficients are negative and significantly different from zero. Hence, there is a significant reduction in corporate lending growth for low-capitalized banks following the reform.

Figure 4: Firm lending - tax data. Banks are divided into groups based on their 2012 capital ratio. Left panel: Growth rates 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). Interaction coefficients γ_t 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 3. Using the quarterly balance sheet data, we find that following the reform, low-capitalized banks had on average 6.3 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.[10] These effects are substantially larger than the reduction in total 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

[10]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.

a one percentage point higher increase in capital ratios leads to a 1 to 1.4 percentage points lower growth in corporate credit supply.

	(1)	(2)
	Δlog(Firm-Lending$_{it}$)	Δlog(Firm-Lending$_{it}$)
$D_i \times I_t^{post}$	-6.26**	-8.930***
	(-2.30)	(-2.97)
Time FE	yes	yes
Type FE	yes	yes
Data Source	balance sheet data	tax data
Clusters	114	110
Observations	1,251	1,094

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

Table 3: Restrictive difference in difference estimation - firm lending. Regression results from estimating equation (4).

What about household lending? While corporate lending growth for low- and high-capitalized banks suddenly diverges, no such pattern is observed for household lending - as documented in Figure 10 in Appendix A. In fact, lending growth to the household sector remains relatively stable for both bank types throughout our sample period. 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.

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, banks can also reduce their average risk weights through other channels. In order to evaluate the quantitative importance of shifting from firm lending to household lending, we perform a back-of-the-envelope calculation 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 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). 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).[11] 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 the risk weights $\left\{\alpha^{HH}, \alpha^{firms}, \alpha^{other}\right\}$ 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 4 lists the observed average risk weight for low-capitalized banks from 2013 to 2015. Over the period, average risk weights fell by 2.5 percent. Simultaneously, household lending relative to firm lending increased by 17 percent. Keeping risk weights and the share of other assets fixed, we calculate the implied average risk weights in the last column of Table 4. 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 can potentially account for nearly all of the reduction in average risk weights.

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

	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 4: 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 our administrative loan level data. This has two advantages. First, it allows us to include firm×time fixed effects to strengthen our identification along the lines of 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

The baseline regression is given by equation (10)

$$\tilde{\Delta} L_{ijt} = \alpha + \sum_{\tau} \delta_\tau \mathbf{1}_{t=\tau} + \gamma D_i + \beta^l \, D_i \times I_t^{post} + \epsilon_{ijt} \tag{10}$$

In an attempt to control for credit demand, we augment equation (10) by including industry×size×year fixed effects.[12] Firm size is a dummy for whether the firm had less than 25 employees in 2012, which corresponds to the sample average. We also follow Khwaja and Mian (2008) by including firm×time fixed effects. Note that this can only be done on the subsample of firms borrowing from more than one bank. In order to allow for entry and exit, the dependent variable is the symmetric change in lending between a firm j and a bank i in year t.[13]

[12] We thank an anonymous referee for suggesting this approach. For recent studies employing similar strategies to control for credit demand, see Edgerton (2012) and Morais, Peydró, and Ruiz Ortega (2015).

[13] 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.

5.1.1 Results

The results from estimating equation (10) are reported in the first column of Table 5. 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 4.1 percentage points lower growth in lending in the post-reform period relative to the pre-reform period. In the second column we include industry×size×year fixed effects. The coefficient remains largely unchanged.

	(1)	(2)	(3)	(4)
	$\tilde{\Delta} L_{ijt}$	$\tilde{\Delta} L_{ijt}$	$\tilde{\Delta} L_{ijt}$	$\tilde{\Delta} L_{ijt}$
$D_i \times I_t^{post}$	-4.06***	-4.10**	-8.74***	-11.14***
	(-2.64)	(-2.57)	(-3.16)	(-3.23)
Time FE	yes	yes	yes	yes
Type FE	yes	yes	yes	yes
Industry × Size × Year FE	no	yes	no	no
Firm × Year FE	no	no	no	yes
Firms	all	all	multiple banks	multiple banks
Clusters	114	114	113	111
Observations	208,351	206,327	39,289	15,807

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

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

Table 5: Restrictive difference in difference estimation - loan level firm lending. Regression results from estimating equation (10)

In the third column we restrict the sample to only include firms with more than one bank connection. The coefficient increases in size and is still significant at the one percent level.[14] Finally, we add firm×year fixed effects in the last column. The identification is now coming from within firm-year variation. The coefficient remains 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. Note that the coefficient increases further in size, implying that if anything, low-capitalized banks are matched to firms with higher

[14]Firms which borrow from multiple banks tend to be larger in terms of number of employees. However, we have rerun the regression in equation (10) for small and large firms (not shown), and do not find support 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 significant. 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.

credit demand. Hence, any bias from not controlling for demand factors is likely to work against us.

5.2 Employment

We have documented a significant reduction in corporate lending growth from low-capitalized banks following the reform - both at 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. In Appendix C 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 a firm-year, we cannot include firm×year fixed effects. However, the results from the previous section imply that any bias from not controlling for firm specific factors would most likely work against us.

Using the full sample of banks, we find that firms borrowing from low-capitalized banks have lower employment growth following the reform. However, the effect in the flexible difference in difference analysis is not statistically significant, as illustrated in the top row of Figure 11 in Appendix A, and the pre-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 aligned. The results from this analysis are illustrated in the bottom row of Figure 11 in Appendix A, in which we compare quartile 2 banks to quartile 3 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 from low-capitalized banks have several percentage points lower growth in employment in the year following the reform.[15]

[15] 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 9 in the appendix. Also note that a reduction in credit supply for 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

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 reform. The results are reported in Table 6. 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 a 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 significantly negative impact on small firms, as seen in the third column.

Because the parallel trends assumption is satisfied when excluding the very high- and low-capitalized banks, we also show results using this restricted sample. The results are reported in the last three columns of Table 6. Firms borrowing from low-capitalized banks have significantly lower employment growth in the year following the reform - also when not conditioning on firm size. Again, the coefficient increases in magnitude and statistical significance when only considering smaller firms.

To get a sense of the economic magnitudes, we append Table 6 with summary statistics of the dependent variable in 2012 for the various subsamples. The average symmetric growth in employment ranges from 4.3 to 8.9 percent - recall that this variable is bounded between -200 and 200 percent at the firm level. Considering small firms and including all banks (column 3), we find that firms borrowing from low-capitalized banks had on average 3.1 percentage points lower growth in employment after the reform. This compares to a mean of 4.8 percent. An alternative way to interpret the magnitude, is to note that the estimated employment reduction corresponds to 0.06 standard deviations. If we exclude the very low- and high-capitalized banks from the sample (column 6), we find that small firms borrowing from low-capitalized banks had on average 4.6 percentage points lower employment growth after the reform. The average employment growth in this sample before the reform was 4.3 percent. In terms of standard deviations, the estimated employment reduction corresponds to just below 0.1 standard deviations.

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 Figure 11.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\tilde{\Delta} Empl_{jt}$	$\tilde{\Delta} Empl_{jt}$	$\tilde{\Delta} Empl_{jt}$	$\tilde{\Delta} Empl_{jt}$	$\tilde{\Delta} Empl_{jt}$	$\tilde{\Delta} Empl_{jt}$
$D_i \times I_t^{2014}$	-1.89	-0.53	-3.12***	-3.21**	-0.207	-4.58***
	(-1.54)	(-0.20)	(-3.05)	(-2.11)	(-0.06)	(-2.99)
Mean ($\tilde{\Delta} Empl_{j2012}$)	5.24	8.88	4.78	4.72	7.88	4.32
Median ($\tilde{\Delta} Empl_{j2012}$)	0	3.04	0	0	2.89	0
Std ($\tilde{\Delta} Empl_{j2012}$)	48.58	31.12	50.34	48.09	34.02	49.56
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	118	118	117	57	54	54
Observations	137,781	44,538	93,223	39,224	11,947	27,277

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

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

Table 6: Restrictive difference in difference estimation - employment.

Although not reported, we have split the sample further by looking at subsets of firms with less than 25 employees. The negative employment effect for small firms seems to be present both for those with a strictly positive number of employees, and also for those with zero employees. We interpret this to mean that lower credit supply reduces the likelihood of zero-employee firms hiring the first employee (extensive margin), as well as the probability that somewhat larger firms hire an additional employee (intensive margin).

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 reduction 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 interest rate.

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.[16]

We aggregate the loan level interest rate data to bank level averages, and plot the resulting time series in Figure 5. The left panel compares interest rates for low-capitalized banks to that of high-capitalized banks. 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 5 we exclude the 25 percent most and least capitalized banks from our sample. Hence, we compare quartile 2 banks to quartile 3 banks. 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.

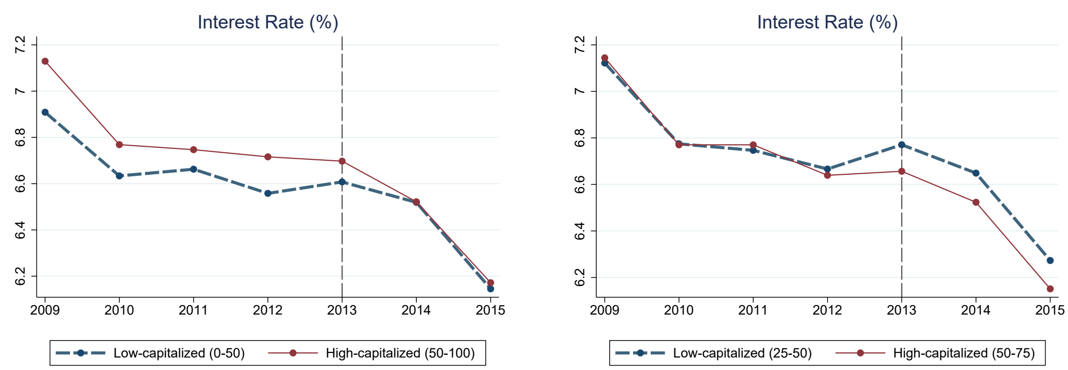

Figure 5: Interest rates. Banks are divided into groups based on their 2012 capital ratios. Left panel: Interest rates for low-capitalized banks (below median) and high-capitalized banks (above median). Right panel: Interest rates for low-capitalized banks (25th to 50th percentile) and high-capitalized banks (50th to 75th percentile) .

[16]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.

While the results in Figure 5 are visually quite striking, the difference in interest rates between high- and low-capitalized banks is 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 9 in Appendix B, and confirm that low capitalized banks significantly increased interest rates relative to high capitalized banks following the reform.

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 find this 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 14 in Appendix D, we can explicitly calculate the number of firms which switch from low-capitalized banks 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 D we make use of some additional assumptions to back out plausible bounds for the impact on aggregate credit supply from our cross-sectional results. These calculations suggest that the increase in capital requirements reduced the aggregate corporate credit growth with approximately five percentage points.

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, low-capitalized banks increased their interest rates, which supports the interpretation of the reduction in lending being supply driven. 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. 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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A Additional Figures

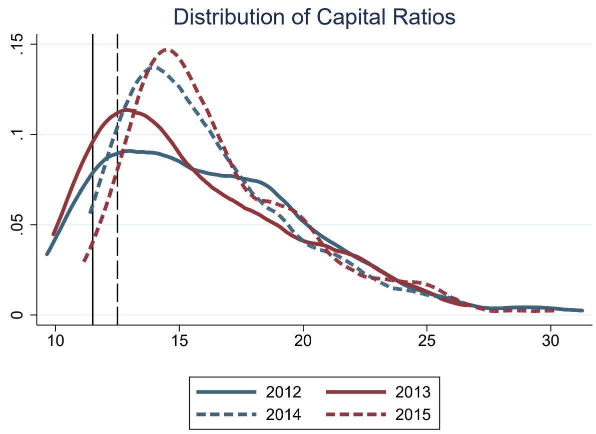

Figure 6: Distribution of capital ratios (%) prior to the reform (2012q4), 1 year later (2013q3), 2 years later (2014q3) and 3 years later (2015q3). The solid line marks the baseline requirement of 11.5 %, while the dashed line marks the new maximum requirement of 12.5 %.

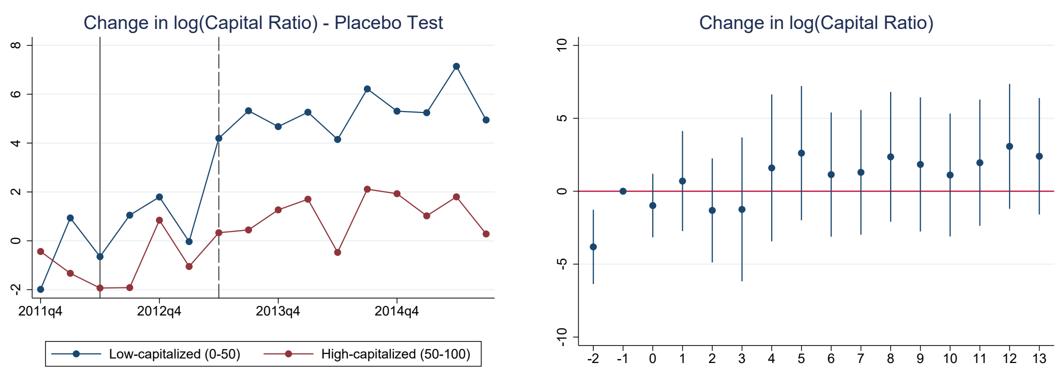

Figure 8: Falsification test - change in log(Capital Ratio) for low and high capitalized banks.

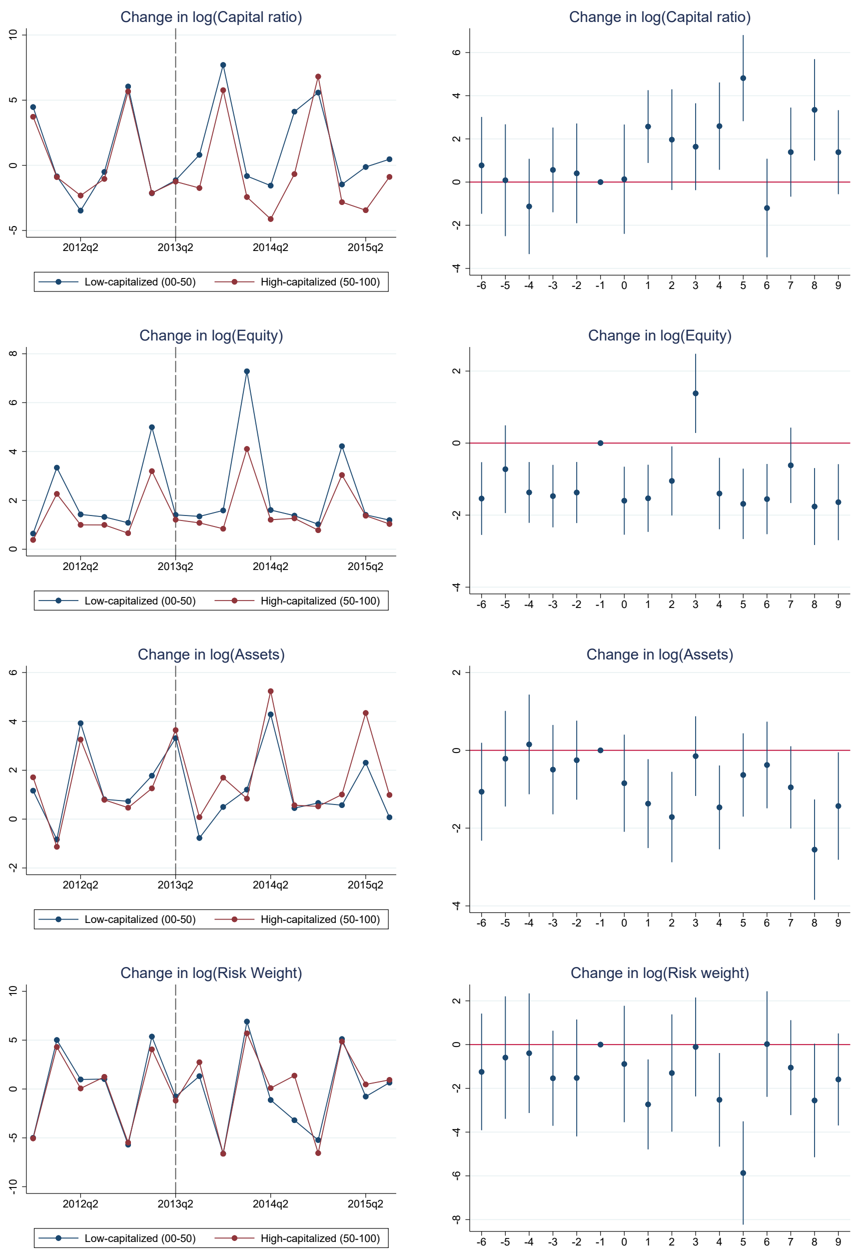

Figure 7: Change in outcome variables - 1-quarter growth. Banks are divided into groups based on their 2012 capital ratio. Left panels: Growth rates for low-capitalized (below median) and high-capitalized (above median) banks. The solid red line marks the growth rate from 2013q1 - 2013q2 (the reform date). Right panels: Regression results from estimating equation (4). Interaction coefficients γ_t are plotted relative to time $t = -1$. Standard errors are clustered at the bank level. Time zero marks the growth rate from 2013q1 to 2013q2 (the reform date).

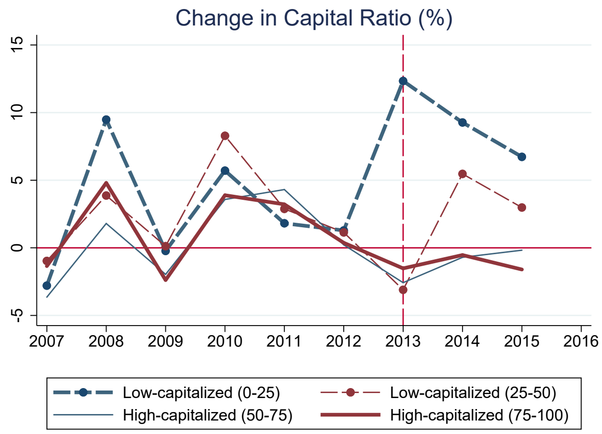

Figure 9: Capital ratios. Banks are divided into groups based on their 2012 capital ratios. Change in capital ratios for banks in quartile 1 (0th to 25th percentile), quartile 2 (25th to 50th percentile), quartile 3 (50th to 75th percentile) and quartile 4 (75th to 100th percentile).

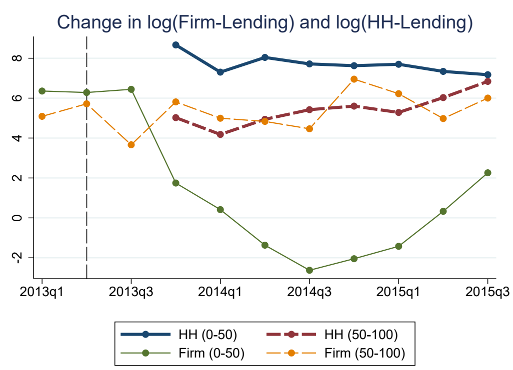

Figure 10: Firm lending and household lending. Growth rates in firm and household lending for low-capitalized (below median) and high-capitalized (above median) banks. Banks are divided into groups based on their 2012 capital ratio. The growth rate for $year_t$-$quarter_i$ denotes the (approximate) percentage change from $year_{t-1}$-$quarter_i$ to $year_t$-$quarter_i$. The solid red line marks the growth rate from 2012q2 to 2013q2 (the reform date).

Figure 11: Employment. Banks are divided into groups based on their 2012 capital ratio. Top row: full sample. Bottom row: 2nd and 3rd quartile banks. Left panel: Growth rates in employment for low-capitalized (0th to 50th percentile) and high-capitalized (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 panel: Regression results from estimating equation (3). Interaction coefficients γ_t are plotted relative to year 2012. Standard errors are clustered at the bank level.

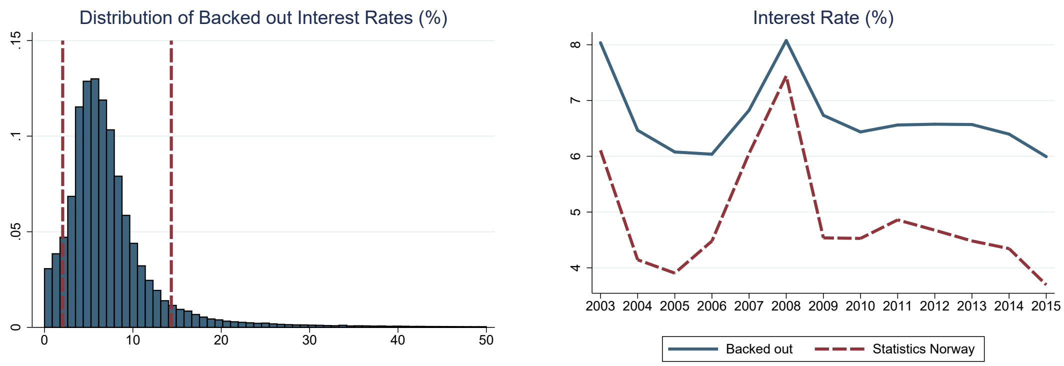

Figure 12: Interest rates. Left panel: Distribution of backed out interest rates. Right panel: Aggregate backed out interest rates and aggregate interest rates on corporate lending from Statistics Norway

B Additional Tables

	$mean^{50-100} - mean^{0-50}$	$mean^{50-75} - mean^{25-50}$
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 7: Comparison of banks by capitalization level (2012q4). Banks are labeled low-capitalized or high-capitalized based on their pre-reform (2012q4) 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).

	(1) $\Delta log(Cap.Ratio_{it})$	(2) $\Delta log(Equity_{it})$	(3) $\Delta log(Assets_{it})$	(4) $\Delta log(RiskWeight_{it})$
$D_i \times I_t^{post}$	6.06***	0.97	-2.30**	-2.79**
	(4.84)	(1.37)	(-2.41)	(-2.32)
Share of response	**100%**	**14%**	**33%**	**53%**
Time FE	yes	yes	yes	yes
Type FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Clusters	114	114	114	114
Observations	1,710	1,710	1,710	1,710

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

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

Table 8: Restrictive difference in difference estimation. Regression results from estimating equation (4) with control variables from Table (1).

	(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 9: Restrictive difference in difference estimation - interest rates. Regression results from estimating equation (4).

C 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 13 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[17]. That is, we run the regression specified in equation (11). 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 13 we expect a positive and large $\hat{\beta}$.

$$I_{ij} = \alpha + \beta \text{Bank}_{ij,-1} + \epsilon_{ij} \tag{11}$$

The regression results are reported in Table 10. The second column controls for bank market size by year, and provides an estimate of β equal to 0.88. Hence, the estimated

[17] 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.

probability of switching banks conditional on taking up a new loan is just above ten percent[18]. 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)	(2)
	L_{ij}	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 10: Relationship lending. Regression results from estimating equation (11).

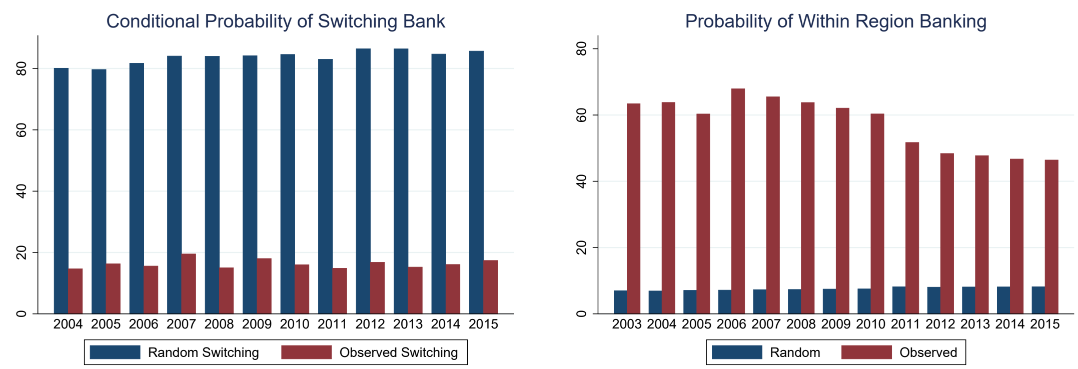

Figure 13: Observed and random switching probabilities

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

[18]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 13 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

D 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.

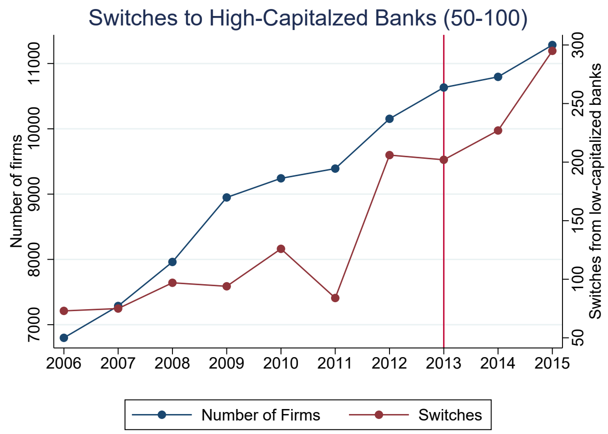

Figure 14: Spillovers. Number of firms switching from low-capitalized banks to high-capitalized banks

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 14. 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 blue line[19]. 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

[19] 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. 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 15 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 11 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 15. 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 15 and reported in Table 11. 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 15. 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.

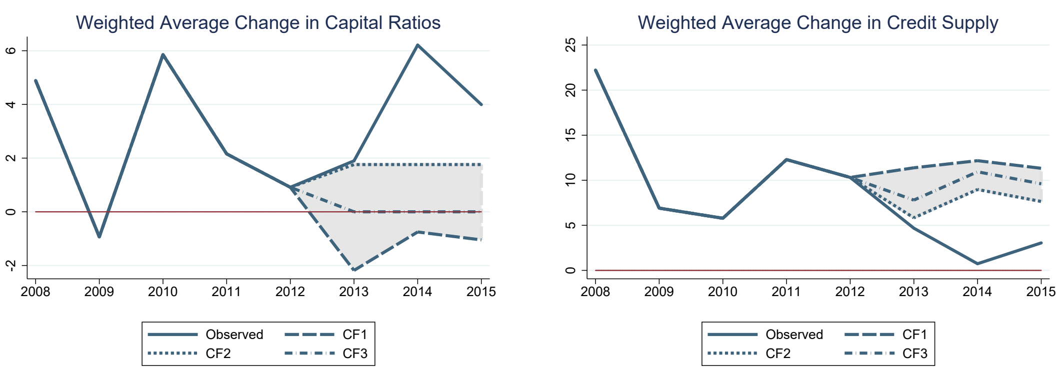

Figure 15: Observed and counterfactual changes in capital ratios and credit supply

	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 11: Cumulative weighted average observed and counterfactual changes in capital ratios (%-change) and credit supply (symmetric %-change) from 2012 to 2015