Capital Requirement Reductions, Heterogeneity, and Real Economic Outcomes*

Elif C. Arbatli-Saxegaard[a] and Ragnar E. Juelsrud[b]
[a]International Monetary Fund
[b]Norges Bank

We use bank-, loan-, and firm-level data to estimate the impact of capital requirement reductions on bank lending and real economic outcomes. We find that capital requirement reductions increase lending to both households and firms at the bank and loan level, and that the increased lending to firms translates into higher capital investment at the firm level. Furthermore, the transmission of lower capital requirements to the real economy depends on both bank and firm characteristics. It is stronger for banks with a lower capitalization. On the other hand, it is weaker for firms with a high leverage or a high default risk.

JEL Codes: E51, G21, G28.

1. Introduction

Financial crises that are preceded by strong credit growth are often associated with severe recessions (Schularick and Taylor 2012 and Jordà, Schularick, and Taylor 2013). An important component of the new international regulatory framework aimed at reducing the

*This working paper should not be reported as representing the views of Norges Bank. The views expressed are those of the authors and do not necessarily reflect those of Norges Bank and the International Monetary Fund. We are grateful to the editor, three anonymous referees, as well as Henrik Andersen, Hege Anderson, Per Atle Aronsen, Henrik Borchgrevink, Ronnaug Melle Johansen, Tord Krogh, Nina L. Midthjell, Carola Mueller, Kasper Roszbach, Bent Vale, Ella Getz Wold, and participants at seminars at the International Monetary Fund and Norges Bank for helpful comments and suggestions. Corresponding author (Juelsrud): ragnar.juelsrud@norges-bank.no.

probability and severity of financial crises (Basel III) is the countercyclical capital requirement (Basel Committee on Banking Supervision 2010a). The countercyclical capital buffer is built up during financial booms and is reduced in downturns when banks face losses. The key objective of the buffer is to reduce the risk that the supply of credit is constrained during an economic downturn, ultimately undermining the performance of the real economy. The relevance of countercyclical capital requirements as a potential tool for macroeconomic stabilization has become evident recently as several countries have lowered capital requirements in an attempt to stimulate lending following the COVID-19 outbreak.

While there is a relatively large and growing literature on the banking sector and the real effects of *increases* in capital requirements (see, for instance, Gropp et al. 2019 or Juelsrud and Wold 2019), the literature on the effectiveness of lower capital requirements as a policy tool is less developed.[1] Understanding the effectiveness of reducing capital requirements is critical for evaluating the potency of time-varying capital requirements as a macroeconomic stabilization tool. Estimating the effect of capital requirement reductions on lending and real economic outcomes is complicated, however, for multiple reasons. First, historically there are only a few instances when capital requirements were reduced. For instance, the countercyclical capital buffer was introduced in several countries but was lowered only recently after the onset of the COVID-19 epidemic. Second, capital requirement reductions are typically endogenous to factors which ultimately affect bank outcomes and the real economy through other channels, such as a decline in demand.[2]

In this paper, we use administrative and supervisory bank-, loan-, and firm-level data to investigate the impact of capital requirement reductions on credit and real economic outcomes. Our empirical analysis focuses on the lending response of a subset of Norwegian banks during their transition from Basel I to Basel II in 2008. Under Basel II, the risk weights for banks not employing internal-ratings-based risk models ("non-IRB banks") were reduced

[1]See Section 2 for a survey of the existing literature.

[2]For instance, capital requirement reductions are often implemented as a macroeconomic support measure and hence correlated with negative demand shocks ultimately affecting firms' demand for credit and performance.

for certain exposures, leading to a lower capital requirement. The capital requirement reduction was large, corresponding to a reduction in the required capital adequacy ratio of about 1 percentage point on average. Moreover, the decrease was a function of banks' initial portfolios which ultimately resulted in considerable variation in capital requirement reductions across banks. We refer to this capital requirement reduction as a "capital release." The transition to Basel II, by coincidence, coincided with the onset of the financial crisis, a negative macroeconomic shock which originated outside of Norway and affected the Norwegian economy through international spillovers. Norwegian banks' transition to Basel II therefore provides a useful laboratory to analyze how banks may respond to capital requirement reductions during economic downturns.

We use data from three different sources. First, bank-level data are obtained from a supervisory database covering all Norwegian banks and a large set of balance sheet items at a quarterly frequency. Since the capital requirement reduction was a function of detailed bank balance sheet characteristics, this data is necessary to properly measure the treatment intensity of the Basel II transition at the bank level. For each bank, we compute the reduction in the capital requirement due to the Basel II transition. We complement this database with an administrative loan-level data set, covering the universe of Norwegian firms. This data set allows us to provide further evidence that the capital release led to a credit supply expansion by using information on both quantities and prices, as well as by controlling for firm-specific factors that can proxy for demand effects. Finally, we merge the loan-level data with a third database, where we observe all major balance sheet and income statement data at the firm level, allowing us to trace the effects of capital requirement reductions on firm investment. Our combined database spans the period between 2005 and 2009, covers the universe of non-IRB Norwegian banks and all of their corporate loans, and contains information on all firms that have a banking relationship with them.

Our baseline identification strategy is to compare the evolution of bank-, loan-, and firm-level outcomes according to banks' capital release in a difference-in-differences setting. The identifying assumption we make is that the outcomes we consider would have been similar for banks with different capital releases or for firms borrowing from banks with different capital releases absent the Basel II

transition. We perform several robustness tests outlined in Section 4 to ensure that other confounding factors do not drive our findings.

We present three sets of results. First, we find that the capital requirement reduction led to an expansion of bank balance sheets and a broad-based increase in lending at the bank level, financed by increased debt. The semi-elasticity of lending is sizable, suggesting that a 1 percentage point reduction in the capital requirement expands overall lending growth by 7.7 percentage points, household lending growth by 4.4 percentage points, and non-financial corporate lending growth by 13 percentage points. At the loan level, the fact that the latter arises from a credit supply expansion rather than a contemporaneous increase in credit demand is corroborated by lower interest rates in addition to higher quantities. The increase in corporate lending is driven by an expansion of credit both at the intensive and at the extensive margins. Moreover, banks not only respond to the decrease in capital requirements by increasing lending; they also invest more in financial assets such as short-term bonds and central bank deposits. As a result of the balance sheet expansion, bank profits go up.

Second, the increase in lending leads to more favorable real economic outcomes at the firm level. Firms borrowing from banks experiencing a larger reduction in the capital requirement face a relaxed external financing premium, which ultimately results in higher growth in dividend payouts and capital investment. A 1 percentage point decrease in the capital requirement at the bank level translates into a 7.5 percentage point higher growth in capital investment at the firm level.

Third, the strength of the transmission of lower capital requirements to lending and real economic outcomes depends on both bank and firm characteristics. Banks with lower initial equity ratios increase lending more, conditional on having the same capital release. This is consistent with capital requirements being a non-negligible constraint on lending prior to the Basel II transition. We also find that the effect of capital requirement reductions on lending is higher among banks with lower losses or higher reliance on interbank funding.

The effect of capital requirement reductions also depends on firm characteristics. We find that the increase in credit is driven by firms with low initial leverage and default risk. The same firms

transmit the expansion in credit to higher dividend payouts and higher capital investment. Our results therefore suggest that the real effects of lower capital requirements depend on the health of firms and that lower capital requirements are most effective when the creditworthiness of firms is relatively high.

2. Related Literature

Our paper relates to a growing literature that studies the effects of capital requirements. Fraisse, Lè, and Thesmar (2020) use variation in internal risk models among French banks, and document significant effects on corporate lending from increasing risk-weighted capital requirements. Aiyar, Calomiris, and Wieladek (2014) compare credit provision by resident foreign branches and domestic U.K. banks, tracing the effects of time-varying bank-specific capital requirements that applied to the latter group. Gropp et al. (2019) study the impact of higher capital requirements on banks' balance sheets using the 2011 European Banking Authority capital exercise as a quasi-natural experiment.[3] Auer and Ongena (2019) document lower lending by banks more exposed to the introduction of the Swiss countercyclical buffer. Juelsrud and Wold (2019) use bank- and loan-level data for Norway, focusing on the 2013 Norwegian policy reform to study how banks react to higher capital requirements and how these adjustments affect the real economy.

A subset of the literature (see, e.g., Behn, Haselmann, and Wachtel 2016, Dietsch et al. 2019, Fraisse, Lè, and Thesmar 2020, Imbierowicz, Kragh, and Rangvid 2018, Jiménez et al. 2017, Mayordomo and Rodríguez-Moreno 2018) focuses explicitly or implicitly on capital requirement *reductions*. Focusing on the impact of capital requirement reductions in isolation is useful, as the effects of capital requirement changes are not necessarily symmetric. One potential source of asymmetry is related to how banks adjust their equity position. A large empirical literature has highlighted how banks are unwilling to raise new equity when capital requirements *increase*; see, for instance, Gropp et al. (2019). One potential explanation for this is asymmetric information—equity issuances can potentially be

[3]See also Basel Committee on Banking Supervision (2010b) on the effects of higher capital requirements.

seen as a bad signal about bank fundamentals, which could in turn affect stock market valuations and banks' ability to finance themselves in the short-term funding market. As a result, banks would prefer not to do it and rather adjust to higher capital requirements by changing their mix of assets. In contrast, reducing the equity position—via increased payouts—can be seen as a *positive* signal about bank fundamentals and hence the adverse effects of doing so on bank equity valuations and funding opportunities is likely to be relatively small or even negative. It is therefore less clear whether a reduction in capital requirements will lead to an adjustment on the asset side or via a reduction in equity via lower retained earnings.

Our paper builds especially on three studies in the existing literature. Jiménez et al. (2017) use Spain's experience with dynamic provisioning to study the effects of countercyclical adjustments in loss provisioning. The authors exploit the bank-specific nature of the requirements and bank-firm-level data to study the effects of both increases and decreases in capital requirements. They find that in good times, a higher need for provisioning leads to lower credit supply but that the effects are short-lived. In downturns, they find that a 1 percentage point increase in capital buffers boosts lending to firms by 9 percent and firm employment by 6 percent, and leads to a 1 percentage point increase in firm survival. Another paper that is closely related to ours is Imbierowicz, Kragh, and Rangvid (2018), which analyzes the Basel II transition in Denmark and focus on the bank-specific time-varying hard capital requirements that were imposed by the regulator. They find that banks adjust to capital requirement increases by adjusting risk weights, while capital requirement decreases lead to higher lending. Finally, Brun, Fraisse, and Thesmar (2013) also use the transition to Basel II and exploit the switch from capital charges that was common across all firms under Basel I to firm- and bank-specific capital charges for IRB banks under Basel II. Their results confirm large effects of capital requirements on bank lending—a 1 percentage point decrease in capital requirement leads in their study to an increase in loan size by about 5 percent.[4]

[4]In addition to these papers, Dietsch et al. (2019) and Mayordomo and Rodríguez-Moreno (2018) investigate the impact of lower capital requirements for small and medium enterprises (SMEs) and document an increase in credit

We contribute to the existing literature by providing complementary evidence on the bank-, loan-, and firm-level impact of lower capital requirements in the context of Norway. Our paper focuses on the effects of a reduction in the bank-level *capital requirement*, which can be different from the effects of having higher *capital buffers* or loan-specific capital requirements. Our bank-, loan-, and firm-level analysis allows us also to jointly study the effects of capital requirement reductions on firm-level outcomes and how banks adjust their balance sheets when capital requirements are reduced. Jiménez et al. (2017) focus on the former question, while Imbierowicz, Kragh, and Rangvid (2018) focus on the latter.

3. Introduction of Basel II in Norway

The Basel II Accord was published in 2004 and aimed to amend international capital requirements to make banks' capital allocation more risk sensitive. The transition changed how banks compute risk-weighted assets and entailed lower capital requirements for all banks but to a varying extent. The previous regulation, Basel I, had a coarse set of risk weights attributed to different types of loans. The effect of Basel II transition on risk weights differed depending on the method for calculating risks weights. First, banks were allowed to apply for using the so-called internal ratings based (IRB) approach, which entailed client-specific risk weights based on banks' own models of credit risk. Banks that did not apply to use the IRB approach—non-IRB banks—faced a similar split of assets into different risk bins as before, but the risk weights for some asset classes were reduced. Most Norwegian banks continued to use the standardized approach and transitioned to Basel II at the beginning of 2008.[5]

supply. Our paper differs from these studies in the sense that we are not analyzing a capital requirement reduction targeted at increasing credit to a specific subset of firms.

[5]The Basel II framework for capital regulation became effective in Norway from January 1, 2007. Most IRB banks transitioned to Basel II during 2007, while almost all non-IRB banks transitioned at the beginning of 2008, reflecting, among other things, the fact that non-IRB banks were smaller and that adapting to the new rules entailed non-negligible fixed costs.

Table 1. Changes in Risk Weights under Basel II for Standard-Method Banks

Loan Type	Basel II Risk Weight	Basel I Risk Weight
Mortgage (LTV < 0.8)	35%	50%
Other Exposures in Retail Portfolio[1]	75%	100%

[1]The retail portfolio consists of mortgage loans, other loans to households, and some of the loans to small enterprises.

The changes in risk weights for the non-IRB banks due to the Basel II transition are summarized in Table 1.[6] Mortgages with a loan-to-value less than 80 percent and certain other retail loans (typically corporate retail loans) were assigned a lower risk weight under Basel II. For all other assets, the risk weights remained unchanged. As a result, the Basel II transition led to cross-sectional variation in the change in capital requirements among non-IRB banks, reflecting differences in banks' pre-Basel II composition of assets.

Our focus in this paper is on evaluating the impact of lower capital requirements due to the change in risk weights for non-IRB banks. We focus on non-IRB banks for two reasons. First, the sample of non-IRB banks in Norway is significantly larger than the sample of IRB banks. This offers a greater degree of variation in the change in capital requirement due to lower risk weights across banks, which is critical for our empirical strategy.[7]

Second, there were important differences in how Basel II was phased in for IRB and non-IRB banks. For the IRB banks, the decline in risk weights was restricted by the imposition of a transitional floor, which dampened the initial reduction in IRB banks'

[6]Basel II also implemented a capital requirement for operational risk. The capital requirement due to operational risk typically constituted around 5 percent of the total change in the capital requirements, and we abstract from that here.

[7]There were only 7 banks that were approved to use the IRB approach as of 2008, while the rest of the banks operating in Norway (more than 130 of them) continued to use the standardized approach. These banks represented approximately 43 percent of all corporate lending relationships in 2006.

capital requirements.[8] For the non-IRB banks, on the other hand, the reduction in capital requirements was immediately realized in 2008.

The downside of using only the non-IRB banks is that they constitute a smaller share of total lending in Norway. Non-IRB banks constituted roughly 22 percent of lending to corporate sector and 40 percent of lending to households in 2006. However, non-IRB banks are disproportionately important for small, bank-dependent firms. Almost half (43 percent) of all credit relationships in 2006 were with non-IRB banks. While the non-IRB banks are different from IRB banks in many respects, the non-IRB banks were broadly representative of the total domestic banking sector during our period of analysis in terms of outcomes such as lending growth.

4. Methodology and Data

In this section, we outline the details of our empirical approach. We start by describing the data before explaining how we use the Basel II transition as a source of cross-sectional variation in capital release. We then discuss our econometric implementation as well as potential threats to identification and how we address them.

4.1 Data

We use data from three different sources. We start by using quarterly supervisory bank balance sheet data on all non-IRB banks and their associated credit companies in Norway. This data set covers up to 132 banks and includes information on loans, assets, equity, and profits. After documenting how banks adjust their balance sheets in response to decreased capital requirements, we proceed by using a loan-level data set provided by the Norwegian Tax Authorities. This data set contains annual, matched firm-bank data for the universe of Norwegian firms. The loan-level data has several advantages. First, it allows us to observe the entire portfolio of domestic corporate credit for all Norwegian banks. This enables us to do a more granular analysis of how banks respond. For example, using this

[8] See Andersen, Juelsrud, and Kostol (2020) for an analysis of the impact of the Basel II transition on the IRB banks.

loan-level database, it is possible to observe credit extension both at the intensive and at the extensive margins. Second, it strengthens identification by allowing us to control for various firm factors. Using the loan-level data, we can also observe the interest paid on loans. This enables us to study the price effects of the reform by constructing an interest rate proxy for each loan. Specifically, we compute the interest rate proxy by dividing interest paid by the outstanding debt balance at the loan level. Third, it allows us to study firm-level effects of lower capital requirements, including profits and investment which we obtain from our third data source, a firm-level data set covering all corporations in Norway.

Summary statistics for the three data sets we use are reported in Table 2.

4.2 Capital Release Due to Basel II

Banks in our data do not report the capital requirement for the same portfolio under Basel II and Basel I directly. In order to quantify the bank-level capital release as a result of the Basel II transition, we therefore compute the average risk weight $\overline{\alpha}^{i}_{b,2006q4}$ under Basel II rules and Basel I rules ($i \in \{\text{Basel I}, \text{Basel II}\}$) for each bank b. In order to calculate the average risk weight, we use data on banks' balance sheet composition as of 2006:Q4.[9] In our bank-level data, we observe balance sheet data that is sufficiently granular to compute the exact capital release for each bank, with two exceptions. First, we do not have bank-level data on the LTV breakdown of the mortgage portfolio. This is important, since the risk weight was only lowered for low-LTV mortgages. However, we do have the aggregate LTV distribution of mortgages in our sample. In expectation, this should therefore also represent the LTV distribution at the bank level. We therefore assume a similar breakdown between high-LTV and low-LTV mortgages across all banks. Second, risk weights were also reduced for *retail* corporate loans. We do not observe those loans in our data at the bank level, but we have an estimate for non-IRB banks as a whole. We therefore follow a similar approach as we did for the high- versus low-LTV mortgages, and assume that

[9]Our results are robust to computing the capital release using balance sheet composition in 2004 when Basel II was first announced.

Table 2. Summary Statistics

Variable	N	Mean	Std. Dev.	Min.	Max.
	Banks				
Capital Release	132	0.0093	0.016	0	0.0160
Δ Log(All Loans)	132	0.03	0.03	−0.03	0.19
Δ Log(Mortgages)	132	0.03	0.04	−0.05	0.21
Δ Log(NFC Loans)	132	0.05	0.08	−0.23	0.37
Δ Log(Total Assets)	132	0.03	0.04	−0.05	0.21
Δ Log(Fin. Assets)	132	−0.01	0.21	−1.77	0.34
Δ Log(Equity)	132	0.02	0.03	−0.07	0.20
Δ Log(Profit)	132	0.26	0.20	−0.25	1.41
Δ RoA Ratio	132	0.001	0.005	−0.05	0.01
	Loans				
$\tilde{\Delta}$ $L_{f,b,t}$	242,806	−0.04	0.64	−2	2
Interest Rate	302,924	0.06	0.05	0	1
New Relationship	302,924	0.20	0.39	0	1
	Firms				
Δ Log(Fin. Assets)	5,040	0.11	1.02	−8.90	9.35
Δ Log(Dividends)	11,814	0.63	2.30	−14.32	11.66
Δ Log(Sales)	8,544	0.11	0.64	−8.87	7.25
Δ Log(Capital)	10,676	0.01	0.60	−7.30	7.56
Δ Log(Wages)	8,419	0.13	0.63	−7.12	6.29

Note: This table shows summary statistics for our bank-level (top), loan-level (middle), and firm-level data. All summary statistics are computed at 2006:Q4 (bank level) or 2006 (loan level and firm level). Δ represents quarterly changes in the bank-level data and annual changes in the firm-level data. $\tilde{\Delta}$ represents annual symmetric change. The symmetric change is bounded between –2 and 2, and allows us to account for entry/exit. It also limits the influence of outliers. "New Relationship" is a dummy equal to 1 if the relationship is present in year t but not in year $t-1$. Note the units of the capital release: the mean corresponds to a 0.93 percentage point reduction.

the share of retail corporate loans to total corporate loans is similar across banks. To ensure the validity of this assumption, we use our loan-level data together with firm-level data to impute which loans are retail based on firm sales. Specifically, firms with annual sales of less than NOK 2 million are classified as retail.[10] We then check

[10] Actual retail classification was also based on the loan's contribution to diversification in the credit portfolio.

the relationship between our capital release measure, as computed below, and the share of retail loans according to this classification at the bank level. If there is a systematic relationship between the imputed retail share and our capital release measure, our estimates could be confounded. For instance, if the actual retail share is higher in banks for which we compute a higher capital release, our estimated impact of the capital release on bank lending would be biased. In Figure A.8 in the appendix, we show that the correlation between the capital release and the imputed retail share is essentially zero, suggesting that such biases are most likely to be small. Overall, these two assumptions leave us with a noisy proxy of the risk-weight change at the bank level as a consequence of the Basel II transition.

With our balance sheet data and the two assumptions outlined above, we can then compute the capital release for each bank using the following formula:

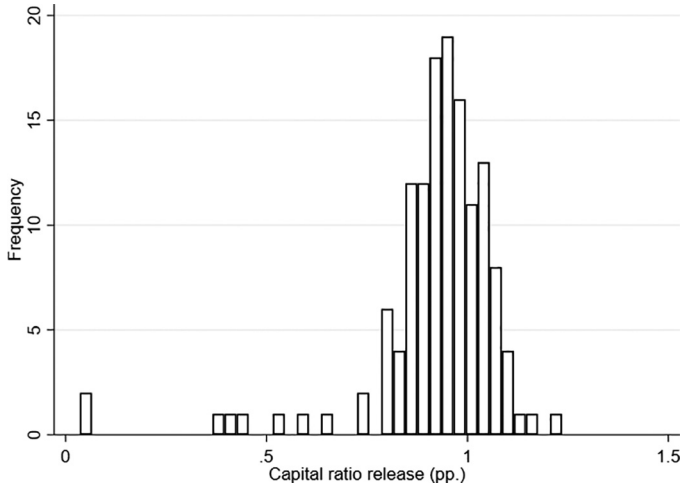
$$\text{Capital release}_{b,2006q4} \equiv \kappa \times \left(\overline{\alpha}^{\text{Basel I}}_{b,2006q4} - \overline{\alpha}^{\text{Basel II}}_{b,2006q4}\right). \tag{1}$$

The capital release equals the difference in average risk weights under Basel I and Basel II multiplied by the minimum capital ratio requirement (κ) ("headline capital requirement").[11] The capital release variable is interpreted as the reduction (in percentage points) of bank b's capital requirement as a consequence of the Basel II transition. The distribution of bank-level capital releases is shown in Figure 1.

Two observations are worth making. First, the average capital release is relatively large, corresponding to approximately 1 percentage point (Table 2). This corresponds to a reduction in headline capital requirement of about 2 percentage points, assuming an average risk weight of 50 percent for Norwegian banks. Second, its variation across banks is quite large. While the capital release is always positive, some banks experience a very low capital release while others experience capital releases well above 1 percentage point. The variation in capital release across banks is primarily driven by variation

[11]The headline capital requirement used in calculating the capital release is 8 percent.

Figure 1. Predicted Capital Release



Note: This figure shows the distribution of capital releases due to the Basel II transition, computed from Equation (1).

in the mortgage share, although there is also sizable variation in the corporate share across banks.[12]

There are two important caveats with using the Basel II transition as a laboratory for understanding the efficacy of lower capital requirements as a countercyclical macroprudential tool. First, our estimates will reflect the effects of a permanent capital release. If banks expect capital requirements to be increased later, their incentives to expand lending may be reduced. This suggests that estimates using the Basel II transition as an empirical strategy may be an upper bound for the effects of capital requirement reductions that are more transitory in nature. Also, since some risk weights were reduced especially for mortgages, the Basel II transition likely induced a relative shift to mortgage credit. However, as we show in the empirical analysis, it also led to an increase in corporate credit.

[12]The standard deviation of the mortgage share is 13 percentage points, while the standard deviation of the corporate share is 5 percentage points.

Second, our results will reflect a reduction in "hard" capital requirements, whereas the capital buffers built up after the financial crisis are to a large extent constituted of "soft" requirements.[13] While one would expect the effects of reducing soft capital requirements to be lower than the effects of reducing hard capital requirements, there is no indication that banks treat the Basel III capital buffers as easily usable. Market pressure is one reason why banks may individually find it very costly to use their regulatory capital buffers.

4.3 Empirical Strategy and Identification

Our empirical strategy involves estimating a relationship between the bank-specific capital release and different economic variables of interest. In doing this, we use both dynamic and static difference-in-differences models. The dynamic difference-in-differences model we use is given by Equation (2):

$$\Delta log(Y_{i,t}) = \boldsymbol{\alpha} + \sum_{\tau} \delta_{\tau} \mathbf{1}_{t=\tau} + \sum_{\tau} \gamma_{\tau} \times \left(\text{Capital release}_{b,2006q4} \times \mathbf{1}_{t=\tau}\right) + \epsilon_{i,t}, \qquad (2)$$

where $Y_{i,t}$ is a bank-, loan-, or firm-level outcome of interest at time t; α is bank-firm-year fixed effects (depending on specification); δ_{τ} are coefficients for time dummies; and $\epsilon_{i,t}$ are standard errors clustered at the bank or firm level.[14] γ_{τ} is an estimate of the relationship between the Basel II capital release and outcome Y at time τ, conditional on the fixed effects.

In the static version of our model, we assume a constant relationship between the capital release and the outcome of interest within the pre- and post-Basel II periods, instead of estimating a period-by-period slope with respect to the capital release. The coefficient of

[13]Soft requirements can be temporarily violated under certain conditions.

[14]The capital release is always at the bank level, irrespective of whether we focus on the bank, loan, or firm level.

interest β in Equation (3) is then interpreted as an average treatment effect over the Basel II transition period.

$$\Delta log(Y_{i,t}) = \alpha + \sum_{\tau} \delta_{\tau} \mathbf{1}_{t=\tau} + \beta \left(\text{Capital release}_{b,2006q4} \times I_t^{post} \right) + \epsilon_{i,t} \quad (3)$$

We estimate Equations (2) and (3) using different variables of interest at the bank, loan, and firm level. For firm-level variables, the capital release is computed as the unweighted average across all of a firm's banking connections.[15]

4.3.1 Threats to Identification

Our identifying assumption is that bank-, loan-, and firm-level outcomes would have been similar for banks with different capital releases or firms borrowing from banks with different capital releases absent the transition to Basel II. There are at least four threats to the validity of this identifying assumption.

Systematic Differences. The first threat to identification is that banks with different capital releases are structurally different in terms of outcomes. For instance, if banks with a high predicted capital release have higher and increasing lending growth compared with other banks throughout our sample period, we would estimate a positive and significant effect of the capital release on lending growth. Furthermore, variation in the bank-level capital release is driven by variation in the mortgage and corporate loan shares. Such variation may reflect differences in bank business models, hence making it likely that there are systematic differences between banks with different capital releases.

An advantage with the empirical specification in Equation (2) is that it allows us to directly test for systematic differences between banks according to their capital release, by estimating period-specific "treatment" effects also prior to the transition. Specifically, we can

[15]We restrict attention to only consider firms with no relationships to IRB banks. The rationale for this is that—due to transitional rules—we do not have an accurate measure of the capital release of IRB banks. Our capital release measure would therefore be noisy and attenuate our estimates. A small share (about 8 percent) of firms in 2006 are lost due to this sample criterion.

explore if there were parallel trends among banks with different capital releases prior to the transition by testing if $\gamma_\tau = 0 \; \forall \; \tau < 0$, using $\tau = 0$ to capture the start of the Basel II transition.

Confounding Demand Shocks. The second threat to identification is that even if banks with different capital releases are similar prior to the transition, they may have experienced different demand shocks during the Basel II transition. This is a concern, as the Basel II transition coincided with the financial crisis where firms may have been affected by different shocks. Shocks to the corporate clients of banks could affect our results if firms and banks are systematically linked. In that case differences in credit growth between banks of different capital releases could be a result of different demand shocks rather than the decline in capital requirements.

In order to alleviate this concern, we exploit the structure of our loan-level data to control for different firm characteristics to ensure that we compare outcomes for relatively similar firms. Using our loan-level data, we also investigate the impact of capital release on loan-level interest rates. While negative demand and supply shocks have similar effects on quantities, they have different effects on prices. We therefore investigate differences in loan pricing across banks with different capital releases to strengthen our inference further.[16]

Confounding Supply Shocks. A third threat to identification could arise if there are other factors that affect banks' supply of credit that are correlated with the capital release. For instance, it could be that banks with different levels of capital release have a different degree of direct exposure to the global financial crisis—for example, because they have different exposures to the U.S mortgage market. Most of the non-IRB banks in our sample are not internationally active, and we therefore believe this is less of a concern.

[16]A standard approach in the literature for identifying supply-side effects is to use firms with multiple bank relationships to control for firm-specific demand effects (Khwaja and Mian 2008). In our loan-level database, approximately 5 percent of firms borrow from at least two different non-IRB banks, and therefore this approach does not give us a sufficient source of variation. As an alternative approach, we add firm and (two-digit) industry × year fixed effects to control for time-invariant firm factors and factors that are constant within industry × year.

The largest non-lending asset category on bank balance sheets is financial instruments (marked to market). This asset class corresponds to roughly 5 percent of total bank assets on average. One potential concern is that there is a relatively flat but significant negative correlation between capital release and the share of these assets in 2006.[17] To the extent that these assets incur losses during the crisis, banks with relatively more financial instruments could be induced to cut back on lending and hence explain the relative difference in lending across banks with different capital releases. Due to this concern, we perform an ex post analysis where we investigate how the capital release correlates with write-downs during the crisis.

An important factor which could have affected credit supply, however, is the tightening of liquidity conditions during the crisis and the liquidity support measures that were provided by the central bank and the government. If banks that experienced a relatively high capital release happen to be less affected by the tightening in liquidity, we could have misinterpreted our results as capturing the effects of lower capital requirements. Conversely, if banks that benefited more from the liquidity support measures also happened to have received a higher capital release, we could have overestimated the effects of capital release.

Another potential factor affecting relative credit supply could be the anticipation of higher future capital requirements or at least a reversal of the capital release following the Basel II transition.

We conduct two robustness exercises to tackle these concerns. First, we include a proxy for banks' liquidity shock exposure as a control and an interaction in our bank-level analysis. Second, we reestimate all of our empirical models focusing on the period before Lehman Brothers collapsed and before the support programs were put in place as well as any international discussion of future regulatory changes.[18]

Finally, there could be other indirect effects of the financial crisis on banks' supply of credit. There is for instance some evidence

[17] A 1 percentage point increase in the capital release decreases the share of financial instruments marked to market by about 8 percentage points.

[18] A concern with this approach is low power. Most standard-method banks transitioned in 2008, resulting in only three-quarters of post-treatment data. As we show in Section A.2 of the appendix, however, our results are qualitatively unchanged albeit somewhat more imprecise.

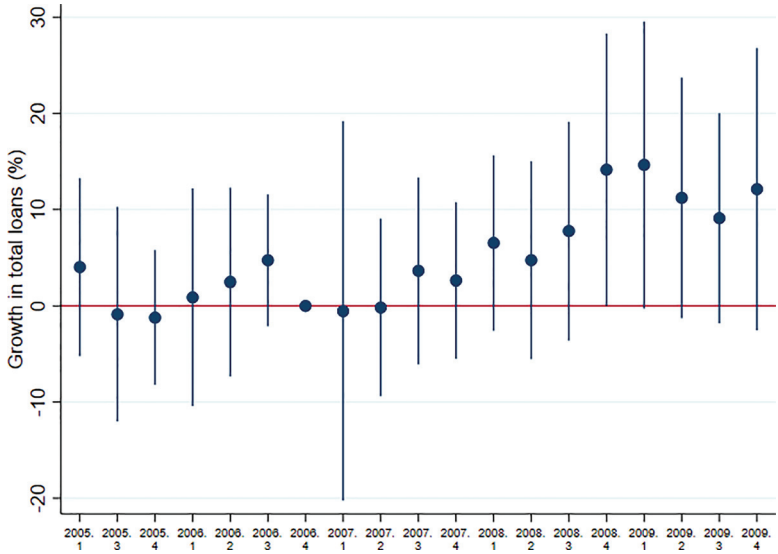
that the internationally active IRB banks which were more directly affected by the crisis retrenched credit in certain regions. This provided an opportunity for more regionally focused non-IRB banks to expand their market share. The fact that the Basel II transition potentially was relatively stricter for IRB banks due to the IRB-floor discussed in Section 3 could also contribute to a competitive advantage for non-IRB banks. In order to tackle this concern, we use the granularity of our data to investigate—at the regional level—the pre-crisis presence of IRB banks in the regional markets of non-IRB banks, and how it correlates with the capital release. A strong positive relationship between pre-crisis IRB presence and the capital release at the bank level would be a cause for concern, whereas no relationship or a negative relationship would suggest that these alternative hypotheses are less of a concern.

Anticipation Effects. A final concern is that treated banks according to our measures adjusted prior to the implementation of the Basel II transition. This is a valid concern, as the Basel II transition was communicated well in advance of 2007/08. Note that such anticipation effects are likely to lead us to underestimate the effects of the capital release. Moreover, the flexible difference in differences allow us to explicitly map out *when* treated banks adjust relative to the actual capital release and hence we can be somewhat agnostic about the exact timing of the reform.

5. Banks' Adjustment to Lower Capital Requirements

In this section, we present our main empirical results related to banks' response to the capital release. We start by investigating how the capital release following the Basel II transition affected credit supply, considering both bank- and loan-level outcomes. A key takeaway is that lower capital requirements lead to more lending both at the bank and the loan level. We then explore the underlying mechanisms behind our findings, and show that our results are driven by banks with low initial capital ratios. We then explore how banks adjust other components of their balance sheets. We find that lower capital requirements lead to an overall expansion of banks' balance sheets, rather than a substitution from other assets to lending.

Figure 2. Capital Release and Overall Lending Growth



Note: This figure shows the dynamic treatment effects (γ_τ) after estimating Equation (2). Circles indicate point estimates, and vertical lines represent the 95 percent confidence interval. All coefficients are plotted relative to 2006:Q4.

5.1 Effect of Capital Release on Lending

We first consider the dynamic treatment effects of the capital release on total lending as captured by γ_τ in Equation (2). The estimated coefficients are shown in Figure 2. After the Basel II transition in 2008, the dynamic treatment effects are positive, suggesting that banks with a higher capital release increased lending more relative to other banks. Figure 2 shows that this effect is larger throughout 2008 and the beginning of 2009, and statistically significant in 2008:Q4 and 2009:Q1.[19] Moreover, the dynamic treatment effects plotted in Figure 2 show that the capital release due to Basel II is not associated with credit growth prior to the Basel II transition. We interpret this as validation of our "parallel trends" assumption.

We next consider the average treatment effects of the capital release in 2008 and 2009, captured by the coefficient β in

[19]The difference in 2009:Q1 is significant at the 90 percent level.

Table 3. Bank-Level Results

	Δ Log (Total Loans) (1)	**Δ Log (Mortgages) (2)**	**Δ Log (NFC Loans) (3)**
$\text{Post}_t \times$ Capital $\text{Release}_{b,2006q4}$	7.74^{***} (0.0225)	4.37^{***} (0.0156)	13.01^{***} (0.0477)
N	3,821	3,821	3,778
No. of Clusters	132	132	132
Mean of Dependent Variable	0.0282	0.0264	0.0284
SD of Dependent Variable	0.0348	0.0400	0.131
Mean of Capital Release	0.0093	0.0093	0.0093
SD of Capital Release	0.00155	0.00155	0.00155
Bank FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes

Equation (3). Consistent with the dynamic treatment effects in Figure 3, banks with a higher capital release increase lending more after the Basel II transition (column 1 in Table 3). On average, a 1 percentage point higher capital release (constituting a 2 percentage point reduction in headline risk-weighted capital requirement based on an average risk weight of 50 percent) increases lending growth by 7.7 percentage points. This is economically sizable: a one-standard-deviation increase in the capital release leads to a 0.37-standard-deviation higher growth in credit.

Next, we focus on two subcomponents of lending: mortgages and lending to non-financial corporations. Columns 2 and 3 in Table 3 present results where growth in mortgage lending and lending to non-financial corporations are used as the dependent variables, respectively. The capital release has a positive and significant effect on both components of lending, suggesting that the increase in lending documented in column 1 and Figure 2 is driven by a broad-based increase in lending not particular to either the household or the corporate sector. A one-standard-deviation increase in the capital release is associated with a 0.16-standard-deviation increase in mortgage lending growth and a 0.2-standard-deviation increase in lending growth to non-financial corporations.

Quantitatively, our estimated response of capital requirement reductions on bank lending is in line with or somewhat larger than estimates in the existing literature. For instance, the slope of loan growth on capital requirement changes is somewhat larger than what is found in Imbierowicz, Kragh, and Rangvid (2018), but very similar to (the absolute value) of the estimates of Gropp et al. (2019). Specifically, Imbierowicz, Kragh, and Rangvid (2018) documents that an average reduction in capital ratio of 0.765 percentage point translated into a 0.885 percentage point higher loan growth, yielding a slope of 1.15, whereas our estimates is 4.4 for retail mortgages (7.7 percentage points for overall asset growth). Gropp et al. (2019) documents that a 1.86 percentage points increase in the capital ratio leads to a 24 percentage points drop in loan growth to corporate exposures, which corresponds to a slope of approximately 13, which is almost identical to our estimates. A potential explanation for the fact that our estimates are more in line with Gropp et al. (2019) is that the capital release we consider was more permanent in nature than the capital requirement changes considered in Imbierowicz, Kragh, and Rangvid (2018).

We next consider the impact of capital release on lending using loan-level data, where we focus exclusively on lending to nonfinancial corporations.[20] We report our estimates of the average treatment effects based on regression (3) in Table 4. The estimate in column 1 indicates that the positive association between bank-level capital release and corporate lending also extends to loan-level data. According to the estimated coefficient, a 1 percentage point higher capital release at the bank level leads to approximately 8.7 percentage points higher credit growth at the loan level.

In column 2, we consider the relationship between bank-level capital release and loan-level interest rates. Our point estimate is negative and statistically significant, consistent with the capital release leading to an expansion of credit supply.

[20]Note that the sample of banks in the loan-level data is somewhat smaller than in the bank-level data (98 versus 132). When using the loan-level data, we restrict attention to banks for which there were no mergers or acquisitions over the sample period to get a clean measure of both the intensive and the extensive margin effects of credit. In Section A.3 of the appendix we report the bank-level regression results on the same sample of banks as in the loan-level data. The results are similar to the results for the larger sample.

Table 4. Loan-Level Results

	Credit Growth (1)	Interest Rate (2)	New Relationship = 1 (3)
$\text{Post}_t \times$ Capital $\text{Release}_{b,2006q4}$	8.726^{***} (2.946)	-0.488^{***} (0.170)	29.69^{***} (5.550)
N	67,349	86,120	86,120
No. of Clusters	98	98	98
Mean of Dependent Variable	−0.0302	0.0618	0.161
SD of Dependent Variable	0.602	0.0464	0.368
Mean of Capital Release	0.00709	0.00709	0.00709
SD of Capital Release	0.00245	0.00245	0.00245
Bank FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Industry × Year FE	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Post = 1 for 2008 and 2009, and 0 otherwise. Standard errors are clustered at the bank level.

A relevant question is whether the capital release following the Basel II transition also led to a credit supply expansion along the extensive margin. In column 3, we reestimate Equation (3) with a dummy equal to 1 if the observed loan is to a new client, and 0 otherwise. A higher capital release at the bank level increases the incidence of new relationships significantly. A 1 percentage point increase in capital release increases the likelihood of a loan being to a new client by approximately 29 basis points, suggesting that the capital release also has a statistically significant impact on credit provisioning at the extensive margin.

5.2 Where Does the Credit Go?

Next, we explore whether the corporate credit supply expansion documented in the previous section is broad-based—i.e., banks expand credit to all firms—or whether more affected banks target specific types of customers. Whether a reduction in capital

requirements affects all or only some firms is theoretically unclear ex ante. If banks' credit allocation can be represented as a portfolio choice problem—i.e., as in Kim and Santomero (1988) or Juelsrud and Wold (2019)—a theoretical prediction is that a relaxation of capital requirements leads to a broad-based increase in lending, provided that the risk weight on individual assets is proportional to the systematic risk of a loan.

More generally, however, coarse risk weights and costly equity implies that banks can have incentives to shift credit once the capital requirement is relaxed. If risk weights are too high compared with the expected return on loans to safe borrowers, banks have incentives to underinvest in these assets when capital requirements are binding. A relaxation of capital requirements can therefore lead to a relative reallocation *towards* safe borrowers. Such an incentive to reallocate credit would be stronger for banks closer to the capital requirement. On the other hand, a large literature emphasizes how bank risk increases in response to relaxed financial conditions (Buch, Eickmeier, and Prieto 2014; Jiménez et al. 2014).[21] One explanation for this empirical finding is that at least certain banks can have incentives to target riskier borrowers (i.e., a "search for yield"), as documented in, for instance, Jiménez et al. (2017).

We therefore proceed by investigating whether the increase in lending growth documented in Table 4 arises across all firms, or whether it is driven by certain groups of firms. We partition firms based on proxies for solvency and leverage. Since most firms in our data are non-listed, we compute a simple z-score as a measure of default risk.[22] In addition to our measure of default risk, we also partition firms based on a simple leverage ratio.

Our main conclusion from this analysis, shown in Table 5, is that the loan-level increase in lending is stronger for the group of firms

[21] This finding is not universal, however. Andrade et al. (2019) documents how the expansion of long-term liquidity in the longer-term refinancing operations (LTRO) program of the European Central Bank did not lead to an expansion in risk.

[22] The z-score is defined as the average return on assets over the last four years plus the equity ratio of the firm, divided by the standard deviation of return on assets over the last four years. For interpretability, we invert the z-score so that an increase is interpreted as an increase in default risk. In Section A.4 of the appendix, we also report results using a window of eight years for calculating the z-score.

Table 5. Loan-Level Heterogeneity

	Credit Growth (1)	**Credit Growth (2)**
$\text{Post}_t \times$ Capital Release	1.910	4.245
	(3.869)	(2.941)
$\text{Post}_t \times$ Low z-score$_{f,2006} \times$ Capital Release$_{b,2006}$	12.87**	
	(5.126)	
$\text{Post}_t \times$ Low Leverage$_{f,2006} \times$ Capital Release$_{b,2006}$		10.42**
		(4.797)
N	58,893	58,974
No. of Clusters	98	98
Mean of Dependent Variable	−0.0328	−0.0324
SD of Dependent Variable	0.590	0.590
Mean of Capital Release	0.00709	0.00709
SD of Capital Release	0.00245	0.00245
Bank FE	Yes	Yes
Firm FE	Yes	Yes
Firm Industry × Year FE	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Post = 1 for 2008 and 2009, and 0 otherwise. Standard errors are clustered at the bank level. "Low z-score" is a dummy equal to 1 if the borrower had a 2006 (inverse) z-score below the median, and 0 otherwise. "Low Leverage" is a dummy equal to 1 if the borrower had a 2006 leverage ratio below the median.

with low default risk and low leverage. This is consistent with banks' allocating credit towards low-risk borrowers. At least based on the proxies we consider, there are no clear indications that the expansion in credit is consistent with a search for yield.

Can this finding be due to relatively high risk weights on safe borrowers, as discussed above? In Table 6, we test whether the reallocation towards safer borrowers is higher for low-capitalized banks. Consistent with this hypothesis, we find that banks with an equity ratio below the median—for which the capital requirement is more likely to be a binding constraint—have substantially stronger substitution towards safer borrowers compared with other banks. We thus conclude that the observed reallocation of credit to safer borrowers once capital requirements go down is consistent with risk weights being relatively high for these borrowers.

Table 6. Explaining Loan-Level Heterogeneity

	Credit Growth (1)	**Credit Growth (2)**
$\text{Post}_t \times \text{Capital Release}_{b,2006}$	2.088	3.926
	(3.975)	(3.145)
$\text{Post}_t \times \text{Low z-score}_{f,2006} \times \text{Capital Release}_{b,2006}$	9.030***	
	(2.258)	
$\text{Post}_t \times \text{Low z-score}_{f,2006} \times \text{Low Equity}_{b,2007} \times$ Capital $\text{Release}_{b,2006}$	54.31***	
	(4.100)	
$\text{Post}_t \times \text{Low Leverage}_{f,2006} \times \text{Capital Release}_{b,2006}$		7.015***
		(2.422)
$\text{Post}_t \times \text{Low Leverage}_{f,2006} \times \text{Low Equity}_{b,2007} \times$ Capital $\text{Release}_{b,2006}$		24.63***
		(8.376)
N	58,893	58,974
No. of Clusters	98	98
Mean of Dependent Variable	−0.0328	−0.0324
SD of Dependent Variable	0.590	0.590
Mean of Capital Release	0.00709	0.00709
SD of Capital Release	0.00245	0.00245
Bank FE	Yes	Yes
Firm FE	Yes	Yes
Firm Industry × Year FE	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Post = 1 for 2008 and 2009, and 0 otherwise. Standard errors are clustered at the bank level. "Low z-score" is a dummy equal to 1 if the borrower had a 2006 (inverse) z-score below the median, and 0 otherwise. "Low Leverage" is a dummy equal to 1 if the borrower had a 2006 leverage ratio below the median.

5.3 Which Banks Respond to the Capital Release?

In the previous section, we documented how a decrease in capital requirements induces an increase in lending to high-quality borrowers. In this section, we explore whether the effect of capital release on lending depends on bank characteristics.

We consider how the impact of a capital release depends on bank characteristics by estimating average treatment effects using bank-level data as in Equation (3) and adding an interaction term for different bank characteristics.[23] The interaction terms depend on the

[23]Bank characteristics are based on end-2007 values.

equity ratio, bank size (log of total assets), return on total assets, write-offs relative to total assets, and interbank borrowing relative to assets.[24]

We conclude that there are three main factors that are important for understanding how lower capital requirements affect lending (see Table 7). First, the effect of lower capital requirements is muted for banks with higher equity ratios. Put differently, low-capitalized banks respond more to a reduction in capital requirements compared with high-capitalized banks.[25] This suggests that the capital requirement prior to Basel II transition was a non-negligible constraint on bank behavior and that the capital release indeed captures a positive credit supply shock. Second, banks subject to more liquidity risk, proxied by a large interbank exposure, also respond more to a reduction in the capital requirement. This suggests that exposure to liquidity constraints and capital requirements have reinforcing effects. Third, banks with larger write-offs respond less to a reduction in capital requirements. This is consistent with models of debt overhang, where banks with larger latent losses abstain from profitable investments, as it represents a transfer to debt holders.[26] We emphasize, however, that the effect is imprecisely estimated.

5.4 Effect of Capital Release on Other Bank-Level Outcomes

We next explore whether banks responded to the capital release by adjusting other margins in addition to their lending behavior. The results from estimating Equation (3), but with a wide range of other bank outcomes such as total assets and profits, are reported in Table 8.

Consistent with the capital release leading to an overall expansion of the banks' balance sheets, rather than a substitution from other assets to lending, total assets grow more for banks with a higher capital release. The economic magnitude of the asset response

[24]Write-offs are weakly negative and bounded above by 0.

[25]A perhaps more accurate measure of whether a bank is low or high capitalized would be to compare equity to risk-weighted assets. Since risk-weighted assets were not consistently reported until after the Basel II transition, we restrict attention to focusing on the unweighted capital ratio here.

[26]See, for instance, Gropp et al. (2019) for empirical evidence of the relevance of debt overhang in the context of banking.

Table 7. Bank-Level Heterogeneity

	Δ Log (All Loans) (1)	**Δ Log (All Loans) (2)**	**Δ Log (All Loans) (3)**	**Δ Log (All Loans) (4)**	**Δ Log (All Loans) (5)**
$\text{Post}_t \times \text{Capital Release}_{b,2006q4}$	16.30^{***}	-6.105	8.194	10.15^{**}	8.967^{***}
	(2.208)	(19.54)	(5.533)	(3.886)	(3.113)
$\text{Post}_t \times \text{Capital Release}_{b,2006q4} \times$ Equity $\text{Ratio}_{b,2007q4}$	-104.9^{***}				
	(20.95)				
$\text{Post}_t \times \text{Capital Release}_{b,2006q4} \times$ $\text{Log(Total Assets)}_{b,2007q4}$		0.864			
		(1.298)			
$\text{Post}_t \times \text{Capital Release}_{b,2006q4} \times$ $\text{RoA}_{b,2007q4}$			-21.61		
			(540.8)		
$\text{Post}_t \times \text{Capital Release}_{b,2006q4} \times$ $\text{Writedowns}_{b,2007q4}$				831.5^{*}	
				(441.4)	
$\text{Post}_t \times \text{Capital Release}_{b,2006q4} \times$ Interbank Borrowing/Total $\text{Assets}_{b,2007q4}$					12.15^{**}
					(4.838)
N	3,786	3,786	3,786	3,557	3,204
Clusters	130	130	130	121	110
Bank FE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes

Table 8. Bank-Level Results

	Δ Log (Total Assets) (1)	**Δ Log (Fin. Assets) (2)**	**Δ Log (Equity) (3)**	**Δ Log (Liabilities) (4)**	**Δ Log (Profit) (5)**	**Δ RoA (6)**
$\text{Post}_t \times \text{Capital Release}_{b,2006}$	4.227***	21.58**	−9.929	5.088**	32.05***	0.138*
	(1.548)	(9.262)	(10.08)	(1.959)	(10.76)	(0.0779)
N	3,821	3,248	3,435	3,442	3,308	3,442
No. of Clusters	132	130	132	132	132	132
Mean of Dependent Variable	0.0264	0.0371	0.0223	0.0281	0.0838	0.000209
SD of Dependent Variable	0.0400	0.186	0.0970	0.0444	0.970	0.00443
Mean of Capital Release	0.00930	0.00939	0.00931	0.00930	0.00934	0.00930
SD of Capital Release	0.00155	0.00135	0.00150	0.00155	0.00146	0.00155
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

is somewhat smaller compared with that of the lending response, but still sizable: A one-standard-deviation increase in capital release leads to a 0.15-standard-deviation increase in growth in total assets. This effect is roughly comparable in absolute value to the estimated effects of a capital requirement increase on a relatively similar sample (Juelsrud and Wold 2019). The capital release also leads to an expansion in financial assets (column 2). This is consistent with the additional lending being matched by additional holdings of liquid assets. Furthermore, we find no clear indication that banks respond to the relaxation of capital requirements by reducing their equity.[27] Hence, our results suggests that lower capital requirements is met by an expansion of banks' balance sheet, rather than a reduction in equity or a shift in the composition of assets, which is confirmed by higher growth in liabilities (column 4).

Finally, we consider the overall impact on profits and return on assets (columns 5 and 6). Consistent with a larger balance sheet, bank profits increase. There are some indications that this is also the case when measured relative to total assets, i.e., that the marginal loans are more profitable, but this effect is imprecisely measured.

6. Lower Capital Requirements and Real Economic Outcomes

In the previous sections, we documented a sizable lending response of banks to the capital release. At the loan level, the growth in corporate credit was driven by an increase in lending for firms with low leverage and low default risk.

A natural question is whether this expansion of credit leads to more favorable real economic outcomes. In this section, we therefore investigate whether the reduction in capital requirements affects real economic outcomes. We do so by focusing on the performance and behavior of the corporate sector. For identification, we investigate

[27] A sizable fraction of our banks are savings banks which do not pay dividends, hence there is likely limited scope for banks to reduce their equity in response to the capital release. However, we do find similar results when restricting attention to banks that pay dividends, suggesting that banks' adjustment documented above is not driven by the fact that banks do not pay dividends by design. See Table A.5 in Section A.3 of the appendix.

whether firms borrowing from a bank with a relatively larger capital release experience different outcomes than other firms.

Specifically, we aggregate our data to the firm level and estimate Equation (3) using a broad range of firm outcomes as dependent variable. We focus on the growth in several balance sheet items (holdings of financial assets and capital), employment demand (as proxied with the wage bill), output (as proxied with sales), and dividend payouts. The results are reported in Table 9.

Our estimates indicate that the credit expansion due to the capital release translates into more favorable real economic outcomes at the firm level. Specifically, firms borrowing from banks with a larger capital release have larger capital investments (column 4). Moreover, they increase dividend payouts, consistent with a relaxation in the external finance premium.[28] We find no significant effect on firms' financial assets, sales, or wage costs. The economic magnitudes are muted but not negligible—a one-standard-deviation increase in the capital release at the bank level translates into a 0.03-standard-deviation increase in firm-level growth in capital expenditures.

To provide further evidence that the increase in capital investment documented in Table 9 is indeed driven by the increase in credit, we explore if our results are only present for low-risk/low-leverage firms, i.e., the firms experiencing an increase in credit supply. We therefore reestimate our empirical model using the same subsamples as in Table 5.

The results shown in Table 10 suggest that the effect of capital release on capital expenditures (column 1) is indeed driven by firms with low default risk (column 2) and low leverage (column 4). This is consistent with the capital response being indeed driven by higher bank credit. As a result of the lower capital requirements, both credit and capital investment are allocated towards firms with lower default risk and leverage.

[28] Although the slope of $\Delta \log$ (Dividends) on the capital release is large, it is worthwhile to note that the variation in the change in log dividends is also quite large. Our estimated coefficient implies that a one-standard-deviation increase in the capital release increases dividend growth by less than 5 percent of a standard deviation.

Table 9. Firm-Level Results

	Δ Log (Fin. Assets) (1)	**Δ Log (Dividends) (2)**	**Δ Log (Sales) (3)**	**Δ Log (Capital) (4)**	**Δ Log (Wages) (5)**
$\text{Post}_t \times \text{Capital Release}_{b,2006q4}$	6.311	40.17***	2.043	7.006***	2.247
	(4.302)	(10.58)	(1.734)	(1.741)	(2.901)
N	20,562	47,222	32,707	41,783	32,045
No. of Clusters	97	97	96	97	97
Mean of Dependent Variable	0.0682	0.156	0.0649	0.0177	0.0866
SD of Dependent Variable	0.977	2.281	0.624	0.573	0.608
Mean of Capital Release	0.00713	0.00713	0.00713	0.00713	0.00713
SD of Capital Release	0.00235	0.00235	0.00235	0.00235	0.00235
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: *p $<$ 0.1, **p $<$ 0.05, ***p $<$ 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Post = 1 for 2008 and 2009, and 0 otherwise. Standard errors are clustered at the bank level.

Table 10. Firm-Level Heterogeneity

	Δ Log (Capital) (1)	**Δ Log (Capital) (2)**	**Δ Log (Capital) (3)**	**Δ Log (Capital) (4)**	**Δ Log (Capital) (5)**
$\text{Post}_t \times \text{Capital Release}_{b,2006q4}$	7.006***	10.15***	2.225	10.64***	2.970
	(1.741)	(3.225)	(6.160)	3.819)	(4.796)
N	41,783	20,202	17,818	18,139	19,937
No. of Clusters	97	95	97	95	97
Mean of Dependent Variable	0.0177	0.0163	0.0142	0.0172	0.0134
SD of Dependent Variable	0.573	0.577	0.560	0.586	0.552
Mean of Capital Release	0.00713	0.00713	0.00713	0.00713	0.00713
SD of Capital Release	0.00235	0.00235	0.00235	0.00235	0.00235
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Sample	All	Low Default Risk	High Default Risk	Low Leverage	High Leverage

Note: *p $<$ 0.1, **p $<$ 0.05, ***p $<$ 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Column 1 considers the full sample. Columns 2 and 3 consider subsamples of firms with a 2006 (inverse) z-score below and above the median, respectively. Columns 4 and 5 consider subsamples of firms with a 2006 leverage ratio below and above the median, respectively.

7. Robustness

As discussed in Section 4, our main identifying assumption is that, absent the Basel II transition, outcomes would have been similar both for the banks in consideration (in the bank- and loan-level analysis) and for the firms borrowing from them (in the firm-level analysis), i.e., we assume that the banks and firms with different capital releases would otherwise be on parallel trends. There are at least three threats to this assumption. We now discuss each potential threat and how we examine the robustness of our results.

First, banks with different capital releases could be systematically different in terms of the outcomes we consider. For instance, banks with a larger capital release could be banks with systematically higher lending growth, irrespective of the reduction in capital requirement. We alleviate this concern by estimating period-specific "treatment" effects prior to the transition and confirm that capital release only explains cross-sectional variation in outcomes *after* the transition. The estimated dynamic treatment effects are reported in Section A.1 of the appendix. Reassuringly, we cannot reject the null hypothesis that the capital release has no significant impact on bank outcomes prior to the Basel II transition. This also indicates that anticipation effects are less of a concern in our sample, even though the Basel II transition was announced pre-2007.

Second, even if banks with different capital releases are similar prior to the transition, it could be that they experience different credit demand shocks after the transition, which can lead to different outcomes. This is a concern, as the Basel II transition coincided with the financial crisis where both firms and banks were subject to external shocks. First, at the bank level we find that banks with a lower initial capital ratio expand lending more in response to lower capital requirements (Table 7). This is consistent with the capital release reflecting a relaxation of capital constraints, as opposed to a positive credit demand shock. Second, we use our loan-level data to control for industry-year fixed effects. This allows us to control for potential industry-specific demand shocks in the post-treatment period. All of the loan-level results we report include such industry-year fixed effects. Third, we use information on the effects of capital release on the price of loans (i.e., the interest rate on loans). As we report in Table 4, we find a significant negative effect of capital

release on the effective interest rate paid on the loan. This suggests that the effects we are capturing are associated with an increase in credit supply.

A third threat to identification is confounding credit supply shocks. There are at least two potential sources. The first source is direct exposure to the financial crisis. We note that the credit losses related to the financial crisis were limited for Norwegian banks, and in particular for the banks in our sample, reflecting their smaller size and domestic focus. However, banks in our sample held mark-to-market financial instruments corresponding on average to 5 percent of their total assets. A drop in the value of these assets could induce banks to experience losses. Since banks with a lower capital release on average held more such financial instruments, this could potentially confound our estimates.

In order to investigate the severity of this concern, we study how write-downs during the crisis period correlate with the capital release. The results are shown in Figure A.7 in the appendix. We are unable to reject the null hypothesis of no significant relationship between capital release and subsequent write-downs, which lends support to our claim that differences in the capital release explain the differences in lending behavior.

The financial crisis affected Norwegian banks to some extent through a tightening in liquidity, especially following the collapse of Lehman Brothers in September 2008. The Norwegian authorities, in return, implemented a series of liquidity support measures to alleviate the liquidity crunch.[29] If the liquidity shock and the liquidity support measures are correlated with our measure of capital release, our estimates of the effect of capital requirement reductions could be biased.

We conduct two robustness exercises to tackle this concern. First, our results are robust to including a proxy for banks' liquidity shock exposure as a control (see Table 7, column 5). Second, we reestimate all of our empirical models using a post-treatment sample that stops in 2008:Q3 for the bank-level results and in 2008 for the loan-level analysis due to its annual frequency. Our results are presented in Tables A.1–A.3 in Section A.2 of the appendix. Our results

[29]See Section A.2 of the appendix for a discussion of these liquidity support measures.

using this shorter treatment period are consistent with our baseline results. Although we fail to reject the null hypothesis for some of the outcomes considered, all coefficients remain qualitatively unchanged and the lower significance is likely driven by the short treatment period.

Finally, even if banks with a high capital release are not directly affected by the financial crisis, they could be indirectly affected through changes in competitive pressures in the credit markets they operate in. One potential confounding factor could arise if banks with a high capital release were competing with banks that were directly affected by the financial crisis, i.e., the IRB banks. When their competitors potentially retrench their credit supply, non-IRB banks with a high capital release could expand credit to increase their market share. In order to investigate whether this is confounding our estimates, we exploit the fact that our non-IRB banks primarily operate in geographically segmented markets. For each non-IRB bank, we compute the pre-crisis market share of IRB banks in that bank's regional market and correlate it with the capital release of that bank. The results are reported in Figure A.6 in Section A.2 of the appendix. If anything, our data indicate that banks with a high capital release are less exposed to competition from IRB banks, which suggests that potential effects of competitive pressures do not constitute a confounding effect on our results.

We also conducted several robustness tests involving different samples which include estimating our main bank-level specification using the smaller sample of banks used in the loan-level results (Table A.4); estimating our main bank-, loan-, and firm-level specifications using trimmed samples to ensure our results are not sensitive to the presence of outliers (Tables A.6–A.8); and using a consistent sample of firms for which we observe sales for our firm-level results (Table A.9).

8. Conclusions and Policy Implications

The purpose of our empirical exercise was to investigate whether reducing capital requirements can be a tool for macroeconomic stabilization via a bank lending channel. The use of capital requirements as a stabilization tool has been an important part of the

policy response during the downturn associated with the COVID-19 epidemic. Several countries, including Norway, reduced capital requirements and regulators signalled flexibility in using capital buffers. Shedding light on how capital requirements affect lending and macroeconomic variables is important for macroeconomic policy, particularly in circumstances where monetary policy space is limited.

Our empirical strategy takes advantage of Norwegian banks' transition to Basel II in 2007–08 which led to a sizable and differential reduction in capital requirements at the bank level and coincided with a period of economic downturn. This quasi-natural experiment allowed us to estimate the effects of reductions in capital requirements on bank lending and real economic outcomes.

Our results are broadly supportive of capital requirement reductions playing a positive stabilizing role in a downturn. We find significant and sizable effects of reducing capital requirements on the cross-section of bank lending to both households and firms. Furthermore, the relative expansion of credit at the bank and loan level translates into better relative real economic outcomes at the firm level.

Moreover, our results suggest that the impact of reducing capital requirements is "double state-dependent." Specifically, the overall transmission to the real economy depends on (i) the capitalization of banks and (ii) the financial soundness of firms. First, capital requirement reductions lead to more lending primarily for banks that have lower capitalization. This is consistent with capital requirements being a binding constraint. Second, creditworthiness of the corporate sector matters. Specifically, our analysis suggests that banks respond primarily to "good" borrowers. Credit expansion and the associated increase in capital investment are driven primarily by firms with low default risk and low leverage.

Although it is challenging to infer the macroeconomic impact of lower capital requirements based on our results, our estimates are at least consistent with the hypothesis that capital requirements can be a potent tool if the banking system is or is expected to be constrained by capital requirements. Periods of financial-sector turmoil and likely banking-sector losses are therefore periods when capital requirement reductions would likely be more effective as a stabilization tool. Finally, the effects of capital requirement reductions can

be dampened if borrowers have low creditworthiness or are highly leveraged.

Appendix. Robustness

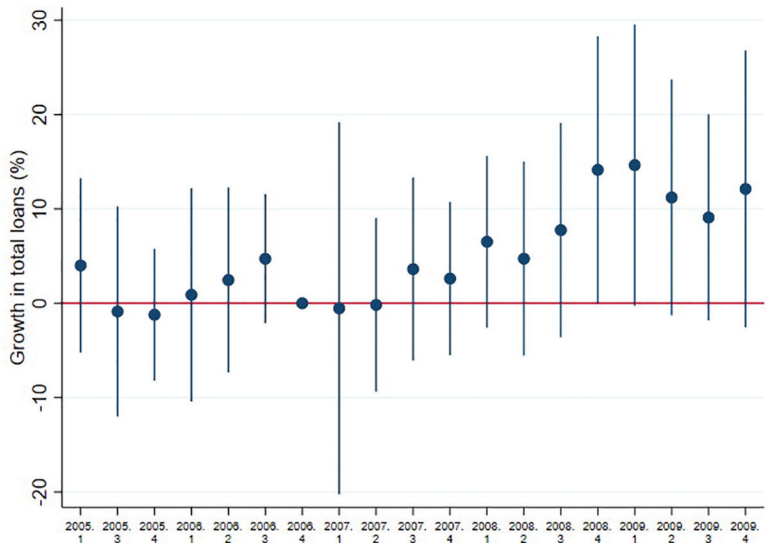
A.1 Parallel Trends

In this section, we report the coefficient estimates from estimating Equation (2) at the bank level.

A.2 Financial Crisis and the Liquidity Freeze

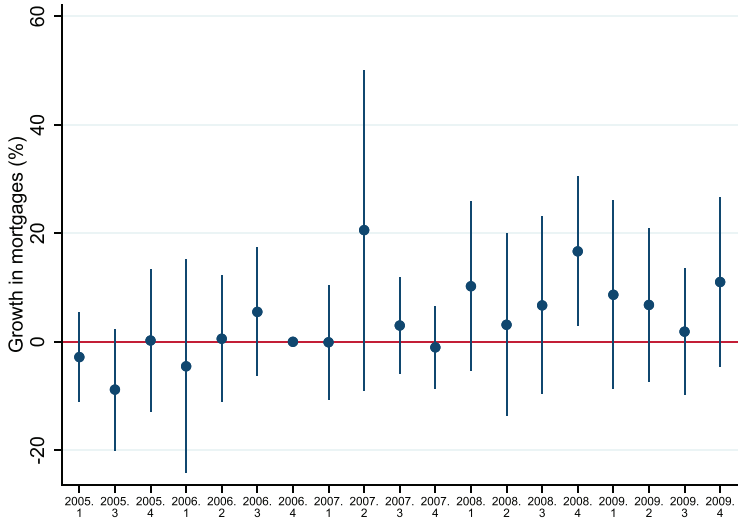
A potential threat to our identification strategy is the potential confounding effects of other shocks that affect banks' credit supply during this period. The period under our study overlaps with the intensification of the global financial crisis which may have affected credit supply through other channels.

Figure A.1. Total Loans and Capital Release



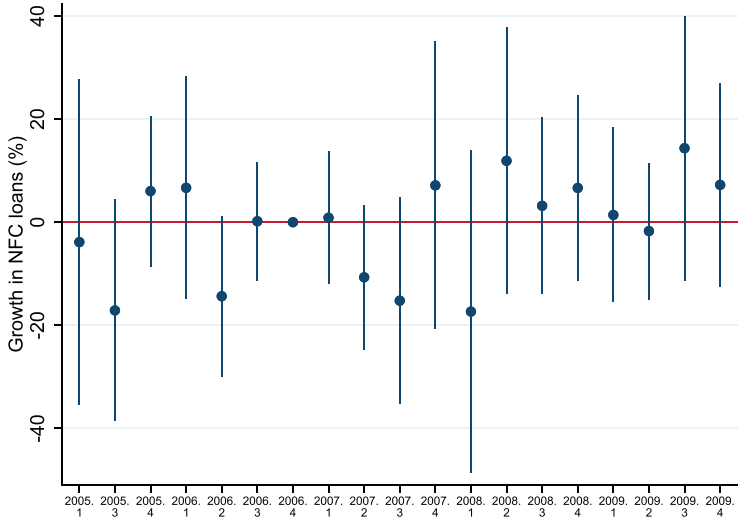
Note: This figure shows the dynamic treatment effects (γ_τ) from estimating Equation (2), using the percentage change in total loans as dependent variable.

Figure A.2. Mortgages and Capital Release



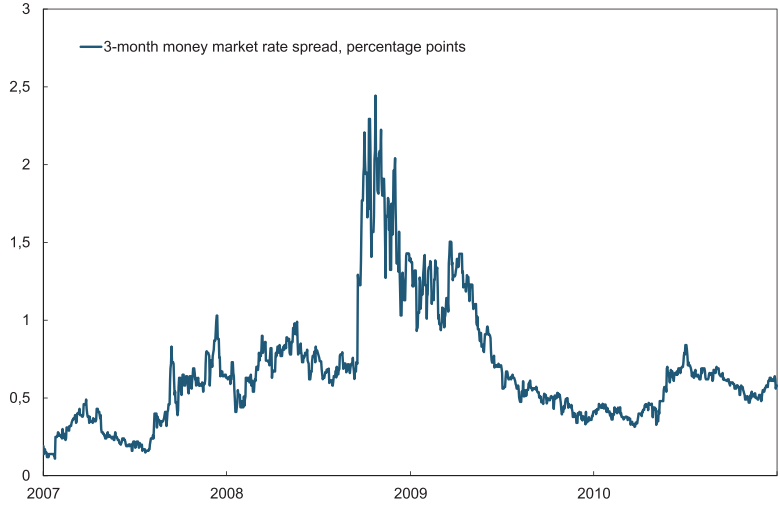
Note: This figure shows the dynamic treatment effects (γ_τ) from estimating Equation (2), using the percentage change in mortgages as dependent variable.

Figure A.3. NFC Loans and Capital Lease



Note: This figure shows the dynamic treatment effects (γ_τ) from estimating Equation (2), using the percentage change in NFC loans as dependent variable.

Figure A.4. Developments in Money Market Rates during the Financial Crisis



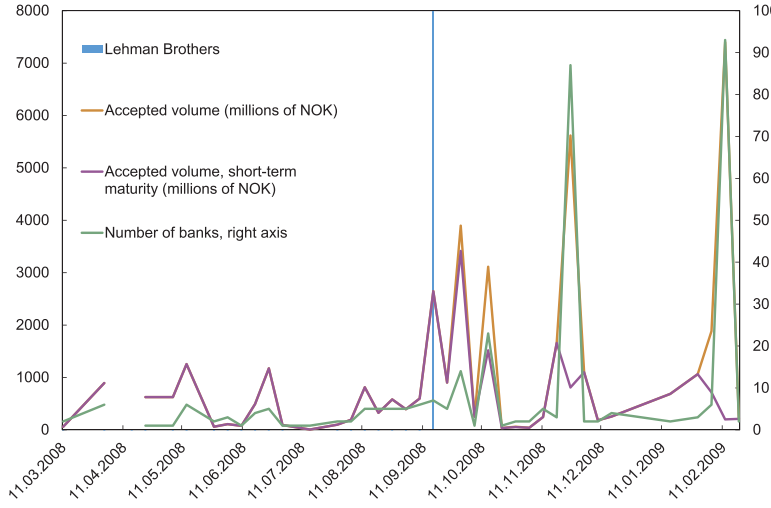
Source: Norges Bank.

An important channel was the significant tightening in liquidity during the crisis. Both the liquidity shock and the liquidity support measures that were announced could affect our results, especially if there is a systematic link with the capital release. In this section, we argue that the liquidity pressures and the support measures became a significant factor only after the bankruptcy of Lehman Brothers in September 2008. We then present results which show that our results are robust to excluding this period.

Figure A.4 shows the three-month Norwegian money market rate spread, i.e., the difference between the rate at which banks lend to each other and the overnight index swap (OIS) rate for the same maturity, during our period of analysis. The money market spread started to increase in the second half of 2007 and remained fairly stable until it jumped significantly soon after the Lehman Brothers bankruptcy on September 15, 2008. The money market spread remained elevated through 2008:Q4–2009:Q1 before gradually coming down later in 2009.

Use of F-loans, which is the main lending tool used by Norges Bank to provide liquidity to the financial system, was limited to

Figure A.5. Developments in F-Loans to Non-IRB Banks during the Financial Crisis



Source: Norges Bank.

only a few of the IRB banks in our sample before Lehman Brothers bankruptcy. F-loans taken up by non-IRB banks were small in terms of magnitude and were mainly short-term (Figure A.5). Norges Bank and the Norwegian government undertook several support measures targeting the liquidity situation following the collapse of Lehman Brothers. F-loans were increased considerably and offered in longer maturities to help strengthen banks' liquidity in Norwegian krone.[30] This shift in liquidity needs and the provision of liquidity after Lehman Brothers is evident in Figure A.5, where we show the weekly take-up of F-loans by non-IRB banks based on proprietary data on F-loans from Norges Bank. The number of banks taking F-loans increases considerably only in November 2008 when Norges Bank offered two-year F-loans which targeted primarily the small banks. The two-year F-loan was announced on October 12 together with an arrangement where the government provided

[30]Norges Bank also provided considerable U.S. dollar liquidity through loans and currency swap arrangements. But these measures targeted the larger and more internationally active banks which are not in our sample.

Table A.1. Bank-Level Results: Short Treatment Period

	Δ Log (Total Loans) (1)	Δ Log (Mortgages) (2)	Δ Log (NFC Loans) (3)
Post = 1 × Capital Release	6.228**	5.683	2.469
	(2.750)	(4.177)	(5.643)
N	3,193	3,163	3,150
No. of Clusters	132	132	132
Bank FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Post = 1 for 2008 and 2009, and 0 otherwise. Standard errors are clustered at the bank level. Sample ends in 2008:Q3.

long-term funding to banks through the exchange of mortgage-backed securities (Norwegian covered bonds) for government paper in swap agreements.[31]

Having identified the Lehman Brothers bankruptcy as the key liquidity shock for our sample of banks, we reestimate our key specifications focusing only on the first three quarters of 2008 for our results using quarterly data and 2008 for our results using annual data. Table A.1 shows our results based on bank-level lending for this shorter sample. Capital release has a similarly large effect on lending based on total loans and mortgages. Our estimate for total loans is statistically significant and confirms our baseline results, while the effects on subcomponents of lending are no longer significant.

We next consider the robustness of our results focusing on the interaction between capital release and bank characteristics. Table A.2 shows that our baseline results hold also for this shorter sample. Capital release has a positive effect on lending, which declines with the initial level of bank capitalization. When we control for banks' exposure to liquidity shocks (column 5), we find that capital release is still associated with higher lending growth but the interaction of capital release with liquidity shock exposure is no longer significant.

Finally we report our short-sample results for loan-level outcomes in Table A.3. Capital release has a positive effect on loan-level

[31] The government paper could be used as collateral for loans from other banks or from Norges Bank, or be sold.

Table A.2. Bank-Level Outcomes, Heterogeneity: Short Treatment Period

	Δ Log (Total Loans) (1)	Δ Log (Total Loans) (2)	Δ Log (Total Loans) (3)	Δ Log (Total Loans) (4)	Δ Log (Total Loans) (5)
Post = 1 × Capital Release	11.86***	−31.22	6.965	8.350	9.031**
	(3.473)	(20.45)	(7.257)	(5.791)	(3.760)
Post = 1 × Capital Release × Equity Ratio, 2007	−80.98**				
	(33.85)				
Post = 1 × Capital Release × Log(Total Assets), 2007		2.359*			
		(1.372)			
Post = 1 × Capital Release × RoA, 2007			−74.69		
			(586.1)		
Post = 1 × Capital Release × Writeoffs/Total Assets, 2007				769.2	
				(677.0)	
Post = 1 × Capital Release × Interbank Borrowing/Total Assets, 2007					6.448
					(5.603)
N	3,177	3,177	3,177	2,993	2,693
No. of Clusters	130	130	130	121	110
Bank FE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes

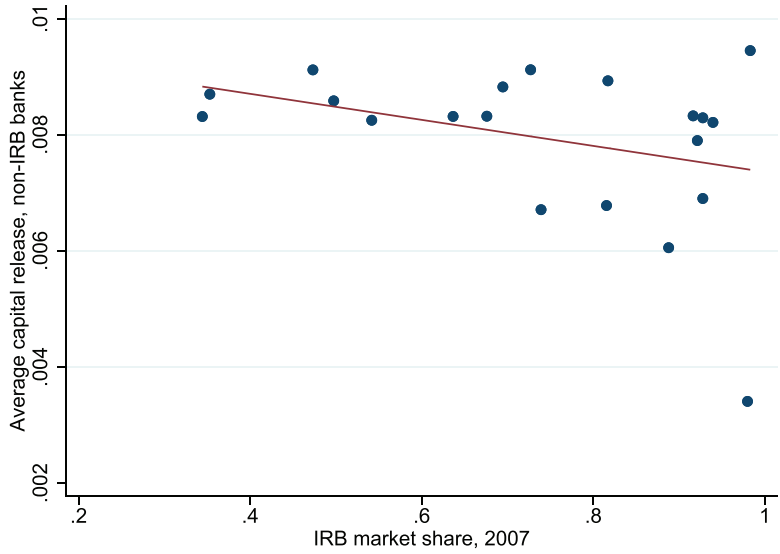
Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using the bank-level growth in total loans as dependent variable. Sample ends in 2008:Q3.

Table A.3. Loan-Level Outcomes: Short Treatment Period

	Credit Growth (1)	Interest Rate (2)	New Relationship = 1 (3)
Post = 1 × Capital Release	5.050	−0.364**	29.38***
	(3.377)	(0.173)	(5.246)
N	50,593	66,161	66,161
No. of Clusters	96	98	98
Bank FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Industry × Year FE	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using the loan-level symmetric credit growth, imputed interest rate, and dummy for new loans as dependent variable, respectively. Sample ends in 2008:Q3.

Figure A.6. Pre-crisis IRB Share and Capital Release



Note: This figure shows a binned scatterplot of the pre-crisis market share of IRB banks for all non-IRB banks and the associated non-IRB banks' capital release. Each dot represents the median in a bin of seven banks.

Figure A.7. Capital Release and Change in Write-offs

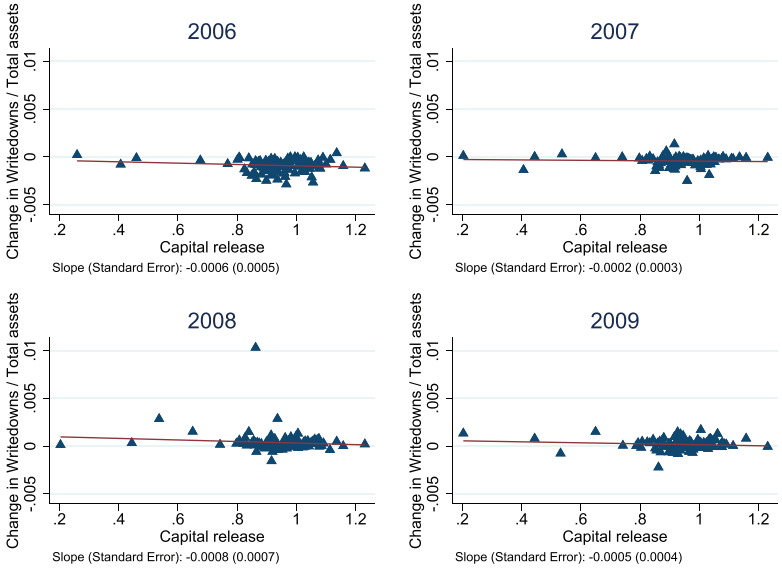
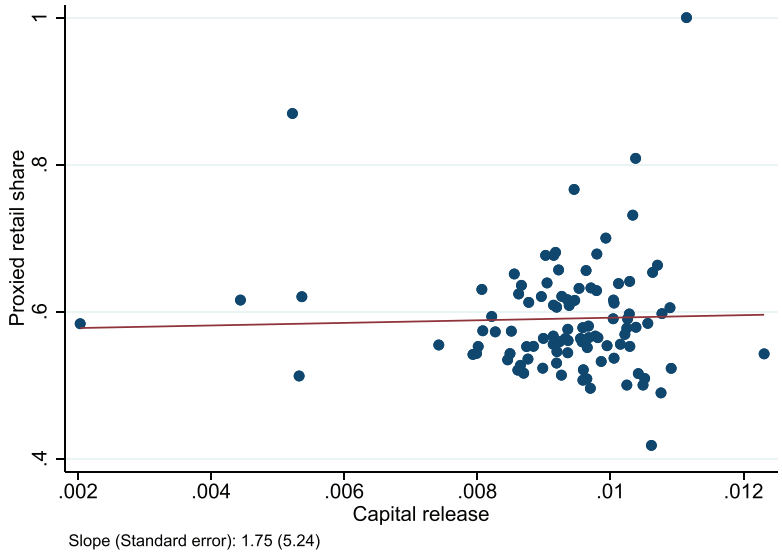


Figure A.8. Imputed Retail Share and Capital Release



lending, but its coefficient is no longer significant. This likely reflects the higher annual variation in lending growth relative to the average over two years in the baseline sample. Our results for the effect of capital release on average effective interest rate and the probability of new lending relationships (extensive margin of lending) remain intact and significant when we use the shorter treatment period.

A.3 Additional Tables

Table A.4. Bank-Level Outcomes: Smaller Bank Sample Used for Loan-Level Results

	Δ Log (Total Loans) (1)	**Δ Log (Mortgages) (2)**	**Δ Log (NFC Loans) (3)**
Post × Capital Release	0.0564***	0.0380**	0.0656*
	(0.0212)	(0.0168)	(0.0388)
N	2,874	2,874	2,874
No. of Clusters	98	98	98
Mean of Dependent Variable	0.0287	0.0272	0.0300
SD of Dependent Variable	0.0321	0.0382	0.102
Mean of Capital Release	0.930	0.930	0.930
SD of Capital Release	0.150	0.150	0.150
Bank FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using the bank-level outcomes. The sample of banks is restricted to be equal to the sample of banks in the loan-level data.

Table A.5. Bank-Level Results: Sample of Active Dividend Payers

	Δ Log (Total Loans) (1)	Δ Log (Mortgages) (2)	Δ Log (NFC Loans) (3)
Post × Capital Release	0.0615***	0.0466**	0.0348
	(0.0150)	(0.0179)	(0.0386)
N	1,300	1,300	1,270
No. of Clusters	44	44	44
Mean of Dependent Variable	0.0293	0.0286	0.0306
SD of Dependent Variable	0.0279	0.0351	0.0697
Mean of Capital Release	0.900	0.900	0.900
SD of Capital Release	0.165	0.165	0.165
Bank FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using the bank-level outcomes. The sample of banks is restricted to contain banks with 2006 dividend payouts.

Table A.6. Bank-Level Results: Trimmed Sample

	Δ Log (Total Loans) (1)	Δ Log (Mortgages) (2)	Δ Log (NFC Loans) (3)
Post = 1 × Capital Release	0.0487***	0.0266*	0.0778**
	(0.0128)	(0.0160)	(0.0348)
N	3,773	3,794	3,727
No. of Clusters	132	132	132
Mean of Dependent Variable	0.0276	0.0258	0.0285
SD of Dependent Variable	0.0271	0.0351	0.0736
Mean of Capital Release	0.921	0.921	0.921
SD of Capital Release	0.160	0.160	0.160
Bank FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using the bank-level outcomes. All dependent variables are trimmed at the 1st and 99th percentiles.

Table A.7. Loan-Level Results: Trimmed Sample

	Credit Growth (1)	**Interest Rate (2)**	**New Relationship = 1 (3)**
Post × Capital Release	9.817***	−0.490***	29.69***
	(2.902)	(0.168)	(5.550)
N	65,990	83,821	86,120
No. of Clusters	98	98	98
Mean of Dependent Variable	−0.0384	0.0603	0.161
SD of Dependent Variable	0.550	0.0340	0.368
Mean of Capital Release	0.00709	0.00709	0.00709
SD of Capital Release	0.00245	0.00245	0.00245
Bank FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Industry × Year FE	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using the loan-level outcomes. All dependent variables are trimmed at the 1st and the 99th percentiles.

Table A.8. Firm-Level Results: Trimmed Sample

	Δ Log (Financial Assets) (1)	**Δ Log (Dividends) (2)**	**Δ Log (Sales) (3)**	**Δ Log (Capital) (4)**	**Δ Log (Wages) (5)**
Post = 1 × Capital Release	8.468**	30.72***	4.860***	5.682***	4.830***
	(3.505)	(10.61)	(1.364)	(1.930)	(1.709)
N	20,050	46,077	31,986	40,780	31,336
No. of Clusters	97	97	96	97	97
Mean of Dependent Variable	0.0646	0.166	0.0721	0.0137	0.0919
SD of Dependent Variable	0.707	1.998	0.390	0.412	0.377
Mean of Capital Release	0.00713	0.00713	0.00713	0.00713	0.00713
SD of Capital Release	0.00235	0.00235	0.00235	0.00235	0.00235
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using firm-level outcomes. All dependent variables are trimmed at the 1st and the 99th percentiles.

Table A.9. Firm-Level Results: Smaller Sample of Firms

	Δ Log (Financial Assets) (1)	**Δ Log (Dividends) (2)**	**Δ Log (Sales) (3)**	**Δ Log (Capital) (4)**	**Δ Log (Wages) (5)**
Post = 1 × Capital Release	6.311	31.81***	2.043	5.771**	3.369
	(4.302)	(10.07)	(1.734)	(2.397)	(2.947)
N	20,562	32,707	32,707	29,987	29,224
No. of Clusters	97	96	96	96	96
Mean of Dependent Variable	0.0682	0.169	0.0649	0.0156	0.0939
SD of Dependent Variable	0.977	2.385	0.624	0.613	0.538
Mean of Capital Release	0.00713	0.00713	0.00713	0.00713	0.00713
SD of Capital Release	0.00235	0.00235	0.00235	0.00235	0.00235
Bank FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Standard errors are clustered at the bank level. This table shows the results from estimating Equation (3) using firm-level outcomes. The sample of firms is restricted to only firms for which the change in log sales is observed.

A.4 Alternative Measures of Firm Risk

In this section, we show how our results related to loan-level reallocation towards safe borrowers are robust to alternative measures of computing firm risk. Specifically, we compute a firm z-score based on an eight-year window, rather than the four-year window reported in the main text.

Table A.10. Loan-Level Heterogeneity

	Credit Growth (1)	**Credit Growth (2)**
$\text{Post}_t \times \text{Capital Release}_{b,2006q4}$	0.550	4.245
	(3.973)	(2.941)
$\text{Post}_t \times \text{Low z-score}_{f,2006} \times \text{Capital Release}_{b,2006q4}$	14.85^{***}	
	(5.243)	
$\text{Post}_t \times \text{Low Leverage}_{f,2006} \times \text{Capital Release}_{b,2006q4}$		10.42^{**}
		(4.797)
N	58,865	58,974
No. of Clusters	98	98
Mean of Dependent Variable	−0.0328	−0.0324
SD of Dependent Variable	0.590	0.590
Mean of Capital Release	0.00709	0.00709
SD of Capital Release	0.00245	0.00245
Bank FE	Yes	Yes
Firm FE	Yes	Yes
Firm Industry × Year FE	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Post = 1 for 2008 and 2009, and 0 otherwise. Standard errors are clustered at the bank level. "Low z-score" is a dummy equal to 1 if the borrower had a 2006 (inverse) z-score below the median, and 0 otherwise. "Low Leverage" is a dummy equal to 1 if the borrower had a 2006 leverage ratio below the median.

Table A.11. Explaining Loan-Level Heterogeneity

	Credit Growth (1)	**Credit Growth (2)**
$\text{Post}_t \times \text{Capital Release}_{b,2006q4}$	0.850	3.926
	(4.020)	(3.145)
$\text{Post}_t \times \text{Low z-score}_{f,2006} \times \text{Capital Release}_{b,2006q4}$	10.93***	
	(2.189)	
$\text{Post}_t \times \text{Low z-score}_{f,2006} \times \text{Low Equity}_{b,2006q4} \times \text{Capital Release}_{b,2006q4}$	56.40***	
	(7.031)	
$\text{Post}_t \times \text{Low Leverage}_{f,2006} \times \text{Capital Release}_{b,2006}$		7.015***
		(2.422)
$\text{Post}_t \times \text{Low Leverage}_{f,2006} \times \text{Low Equity}_{b,2006q4} \times \text{Capital Release}_{b,2006q4}$		24.63***
		(8.376)
N	58,865	58,974
No. of Clusters	98	98
Mean of Dependent Variable	−0.0328	−0.0324
SD of Dependent Variable	0.590	0.590
Mean of Capital Release	0.00709	0.00709
SD of Capital Release	0.00245	0.00245
Bank FE	Yes	Yes
Firm FE	Yes	Yes
Firm Industry × Year FE	Yes	Yes

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Mean and standard deviations are taken over the full sample period (2005–09). Post = 1 for 2008 and 2009, and 0 otherwise. Standard errors are clustered at the bank level. "Low z-score" is a dummy equal to 1 if the borrower had a 2006 (inverse) z-score below the median, and 0 otherwise. "Low Leverage" is a dummy equal to 1 if the borrower had a 2006 leverage ratio below the median.

References

Aiyar, S., C. W. Calomiris, and T. Wieladek. 2014. "Does Macroprudential Regulation Leak? Evidence from a UK Policy Experiment." *Journal of Money, Credit and Banking* 46 (s1): 181–214.

Andersen, H., R. Juelsrud, and A. Kostol. 2020. "Effekter av IRB-metoden pa bankenes utlan til norske foretak." ("Effects of the IRB Approach on Bank Lending to Norwegian Enterprises") Norges Bank Staff Memo No. 1/2020.

Andrade, P., C. Cahn, H. Fraisse, and J.-S. Mésonnier. 2019. "Can the Provision of Long-term Liquidity Help to Avoid a Credit

Crunch? Evidence from the Eurosystem's LTRO." *Journal of the European Economic Association* 17 (4): 1070–1106.

Auer, R., and S. Ongena. 2019. "The Countercyclical Capital Buffer and the Composition of Bank Lending."

Basel Committee on Banking Supervision. 2010a. "Basel III: A Global Regulatory Framework for More Resilient Banks and Banking Systems." Technical Report, Bank for International Settlements.

———. 2010b. "Interim Report: Assessing the Macroeconomic Impact of the Transition to Stronger Capital and Liquidity Requirements." Technical Report, Bank for International Settlements.

Behn, M., R. Haselmann, and P. Wachtel. 2016. "Procyclical Capital Regulation and Lending." *Journal of Finance* 71 (2): 919–56.

Brun, M., H. Fraisse, and D. Thesmar. 2013. "The Real Effects of Bank Capital Requirements." *Dèbats èeconomiques et financiers* (*Economic and Financial Debates*) (Banque de France) 8: 3–26.

Buch, C. M., S. Eickmeier, and E. Prieto. 2014. "In Search for Yield? Survey-based Evidence on Bank Risk Taking." *Journal of Economic Dynamics and Control* 43 (June): 12–30.

Dietsch, M., H. Fraisse, M. Lé, S. Lecarpentier. 2019. "Lower Bank Capital Requirements as a Policy Tool to Support Credit to SMEs: Evidence from a Policy Experiment." EconomiX Working Paper No. 2019-12.

Fraisse, H., M. Lè, and D. Thesmar. 2020. "The Real Effects of Bank Capital Requirements." *Management Science* 66 (1): 5–23.

Gropp, R., T. C. Mosk, S. Ongena, and C. Wix. 2019. "Bank Response to Higher Capital Requirements: Evidence from a Quasi-natural Experiment." *Review of Financial Studies* 32 (1): 266–99.

Imbierowicz, B., J. Kragh, and J. Rangvid. 2018. "Time-Varying Capital Requirements and Disclosure Rules: Effects on Capitalization and Lending Decisions." *Journal of Money, Credit and Banking* 50 (4): 573–602.

Jiménez, G., S. Ongena, J.-L. Peydró, and J. Saurina. 2014. "Hazardous Times for Monetary Policy: What Do Twenty-three Million Bank Loans Say about the Effects of Monetary Policy on Credit Risk-taking?" *Econometrica* 82 (2): 463–505.

———. 2017. "Macroprudential Policy, Countercyclical Bank Capital Buffers and Credit Supply: Evidence from the Spanish Dynamic Provisioning Experiments." *Journal of Political Economy* 25 (6): 2126–77.

Jordà, O., M. Schularick, and A. M. Taylor. 2013. "When Credit Bites Back." *Journal of Money, Credit and Banking* 45 (s2): 3–28.

Juelsrud, R. E., and E. G. Wold. 2020. "Risk-Weighted Capital Requirements and Portfolio Rebalancing." *Journal of Financial Intermediation* 41 (January): Article 100806.

Khwaja, A. I., and A. Mian. 2008. "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market." *American Economic Review* 98 (4): 1413–42.

Kim, D., and A. Santomero. 1988. "Risk in Banking and Capital Regulation." *Journal of Finance* 43 (5): 1219–33.

Mayordomo, S., and M. Rodríguez-Moreno. 2018. "Did the Bank Capital Relief Induced by the Supporting Factor Enhance SME Lending?" *Journal of Financial Intermediation* 36 (October): 45–57.

Schularick, M., and A. M. Taylor. 2012. "Credit Booms Gone Bust: Monetary Policy, Leverage Cycles, and Financial Crises, 1870-2008." *American Economic Review* 102 (2): 1029–61.