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The real effects of capital requirements and monetary policy: evidence from the United Kingdom

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Abstract

We study the effect of changes to UK bank-specific capital requirements on small and medium-sized enterprises (SME) from 1999 to 2005. Following a 1% rise in capital requirements, SME asset growth (and investment) contracts by 3.5% to 6.9% (12%) in the first year of a new bank-firm relationship, but this effect declines over time. These results are robust to a number of different fixed effects specifications and measures of capital requirement changes that are orthogonal to balance sheet characteristics by construction. Banks with tight capital buffers are the most significant transmitters of this shock. Monetary policy only affects the asset growth of small bank borrowers, but has a similar impact on the same sectors as capital requirements. There is evidence that these instruments reinforce each other when tightened, but only for small banks. Firms that borrow from multiple banks and operate in sectors with alternative forms of finance are less (equally) affected by changes in capital requirements (monetary policy).

Key words: Capital requirements, firm-level data, SMEs, relationship lending, macroprudential and monetary policy.

JEL classification: G21, G28.

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Introduction

Current financial reform proposals focus on the introduction of counter-cyclical capital buffers (Basel III) to maintain financial stability and increase the resilience of financial institutions to adverse shocks. Yet the impact and transmission mechanism of these tools is still not well understood (Galati and Moessner, 2013). This should not be surprising, since regulators in most countries imposed a fixed capital requirement, in accordance with Basel I, in the past. But as extensively documented in Francis and Osborne (2012), Aiyar, Calomiris and Wieladek (2014a,b,c) and Turner (2009), UK regulators adjusted *bank-specific* capital requirements *over time*.[1] In this paper we exploit this unique regulatory regime to test if changes in bank-specific capital requirements affect the asset growth of the SME's that borrow from the affected banks. We then examine whether firm or bank characteristics affect the transmission of this regulatory change. For comparison, we also study the impact of monetary policy on asset growth and test if these tools reinforce/dampen each other. Finally, we examine whether access to alternative sources of finance, either via multiple bank relationships or sectoral presence of foreign branches and capital markets, can mitigate the impact of this policy.

For capital requirements to affect the real economy, through the impact on loan supply, three conditions need to be satisfied: i) Bank equity needs to be more expensive than bank debt; ii) Capital requirements need to be a binding constraint on a bank's choice of capital and iii) borrowers need to have limited access to other sources of finance. The first condition implies a failure of the Miller-Modigliani (1958) theorem for banks, as otherwise changes in the capital requirement do not need to affect a financial institution's balance sheet. But economic theory provides good reasons for why condition i) should be satisfied, such as asymmetric information (Myers and Majluf, 1984) and the

[1] In principle, these had to address legal, operational and interest rate risks, which were not allowed for in Basel I. In practice, the regulatory decisions on capital requirements for each bank were based on organization structures, it systems and reporting procedures, rather than financial and balance sheet analysis. Aiyar et al. (2014a,b,c) argue that these institutional characteristics allow to treat changes in capital requirements as exogenous with respect to credit supply.

difference in tax treatment between debt and equity.[2] Regarding condition ii), clearly banks can always choose to hold a smaller capital buffer (*i.e.* the difference between the actual capital ratio and the capital requirement), which means that changes in requirements may not affect them in any way. But banks with already tight capital buffers will not have this option, which implies that capital requirements will have a greater impact on the actual capital ratio of banks with binding capital buffers. A cursory examination of our data (Figure 1) suggests that this indeed case. Several empirical studies, namely Ediz et al (1998), Alfon et al (2005), Francis and Osborne (2012) and Bridges et al (2014), also demonstrate that capital requirements were binding in these data with a regression framework. Finally, empirical work documenting the impact of adverse shocks to capital on loan growth, as in Bernanke (1983) and Peek and Rosengren (1997, 2000), implicitly provides support for this assumption. This suggests that changes to capital requirements were most likely a binding constraint on UK bank's capital choices during the 1999-2006 period.

If conditions i) and ii) hold, the affected bank will have to either raise capital from outside investors, grow capital through retained earnings or cut back on risk-weighted assets, which implies a fall in lending to private non-financial corporate (PNFC) firms. Several studies use the same underlying FSA capital requirements data to test this last implication in *bank level* data. Aiyar, Calomiris and Wieladek (2014a), Bridges et al (2014) and Francis and Osborne (2012) find a loan contraction of 5.7, 5.6 [3] and 5 per cent, following a one hundred basis points increase in capital requirements, respectively. Our results suggests that this leads to a decline in asset growth of an SME borrower of about 3.5 to 6.9%, depending on the specification, in the *first year of a new bank-firm relationship*. This effect then declines over time, as the length of the relationship increases. This is consistent with the literature on *relationship lending* that argues that banks, when hit by a shock, would not be cutting lending to their long-time customers,

[2] See Aiyar, Calomiris and Wieladek (2014a) for an extensive discussion of economic theory and empirical evidence in support of the validity of the first assumption.

[3] To make their results comparable to the other studies, the authors kindly provided us with a figure that refers to the effect over one year and is a weighted average of their results for the commercial real estate and other PNFC lending categories.

but rather to the newly acquired borrowers. We find a reduction in current liabilities of a similar magnitude, which suggests that the reduction in asset growth is indeed the result of a decrease in funding. More importantly, we also find a similar effect for firms' investment, implying that the increase in capital requirement has a negative effect on firms' real, tangibles activities. Those SME's that borrow from banks with tight capital buffers experience a large contraction (10-19%), while those borrowing from banks with larger capital buffers are not affected. This is not surprising, since we argue that changes in capital requirements tend to bind for the former, but not the latter (Figure 1). We also find that firms in the commercial real estate sector experience the largest contraction, consistent with findings on loan supply to this sector reported in Bridges et al (2014).

Compared to the previous *bank* level studies, inference in our study is based on many thousands of firm-level observations per financial institution and capital requirement change, meaning that econometric bias due to reverse causality is much less likely than in the other studies. More importantly, geographical(122)-sector(400)-time (8) (for a total of 152,000 fixed effects) or *bank-year* or even *bank-sector-area-year* dummies allow us to control for loan demand and any other unobservable variable at the *bank* level, including its credit portfolio, defined by the area and 4digitsector the bank lends in, better than in most previous work.[4] Anecdotal evidence supports the assumption that capital requirement changes were exogenous with respect to bank balance sheets during this period. We explore this hypothesis by regressing 23 bank variables that regulators were potentially looking at when setting requirements on the change in capital requirement. We then select the regressors with the highest explanatory power and explore how much of the overall variation (R^2) in capital requirement changes they explain. This exercise suggests that balance sheet characteristics explain only about a third of the total variation in capital requirement changes. The residuals from this

[4] The nature of our data does not allow us to use the *firm*time* fixed-effect pioneered by Kwhaja and Mian (2008), *i.e.* comparing the lending done to the *same* firm by differently affected banks (see data section 2.2 for details). However, like in all studies that employ this methodology (Jimenez et al (2012,2014,2015), Iyer et al. (2014), Brun et al. (2013) among others), we find that going from an aggregate time effect to a very heterogenous sector-,area-,time effect does not significantly alter any of the baseline results. This suggests that highly disaggregated unobserved heterogeneity is usually not a concern in partial equilibrium reduced-form regressions in big data bank lending applications.

regression will be orthogonal to balance sheet characteristics by definition. We can therefore also use them as a measure of capital requirement changes, which were the result of changes in non-balance sheet risk. When we use the alternative capital requirement measure, instead of the actual changes in capital requirements, to examine the SME's asset growth impact, we find very similar effect to the baseline model across a number of specifications.

Ours is not the only study that examines the impact of prudential regulations on real economic activity using firm-level data. Jimenez et al. (2015) provide evidence for the impact of dynamic loan provisioning, an alternative macroprudential instrument in Spain, on lending growth and employment outcomes with a detailed loan-level dataset on loans granted and applied for by firms operating in Spain. They find that in good times, 2001Q2-2008Q1, although credit committed declines, there is no change in credit available to firms, total assets or employment. They do find some negative effect of macroprudential policy on employment growth and firm survival for the borrowing firms, but only in bad times (2008-2012). Compared to Jimenez et al. (2015), we only analyse capital requirement changes in good times (1998-2006) and we still find negative effects on asset growth, but only in the first two years of a new bank-firm relationships. Brun et al (2013) use credit-registry data from France to examine the impact of the introduction of Basel II in 2007-2008 on French firms and banks. In comparison, all of our regulatory changes occurred before the global financial crisis (before 2007), which makes it easier to attribute any effect we find to regulatory changes, as other major bank shocks were absent during this period.

We also contribute to the discussion on the relationship between monetary and prudential policy. The UK experience is helpful in this respect, because banking supervision and monetary policy were undertaken by two independent institutions, the FSA and Bank of England, respectively. UK monetary policy did not explicitly take into account capital requirements of individual institutions.[5] It is therefore unlikely that national authorities co-ordinated capital requirements and monetary policy to affect real activity. This means that we can use a monetary policy shock measure constructed from an estimated Taylor rule for the UK over this period. Our results suggest that monetary policy has an independent effect on SME asset growth via the length of the firm-bank relationship, akin to our findings for capital requirements. In the UK, monetary policy is used to stabilise fluctuations in output and prices and return inflation to target at the 2-year horizon. White (2006) argues that even if price stability is achieved, monetary policy can still lead to sectoral imbalances, with potentially adverse financial stability implications. Indeed, one rationale for active prudential policy is that it can affect such imbalances within the economy more directly and hence in a less socially costly manner than monetary policy. Indeed, our results suggest that monetary policy tends to affect riskier firms more and that the commercial real estate sector, which is highly leveraged, is affected the most. These are also the firms that are mostly affected by changes in capital requirements. Similarly to previous work by Kashyap and Stein (2000), our results suggest that monetary policy does not affect large banks. Capital requirements, on the other hand, affect either type of bank.

Finally, we also contribute to the discussion on the interaction between monetary policy and prudential regulation. Economic theory does not provide a consensus on whether these two policies reinforce or dampen each other (Borio and Zhu (2008), Van den Heuvel (2002, 2005)) and empirical evidence on their interaction is scant (Aiyar et al. (2014c)). We find that these instruments reinforce each other when monetary policy is tightened, but only for small banks. This is because small banks are more likely to be sensitive to realised interest rate risk coming from a monetary policy tightening (Landier, Sraer, Thesmar (2013)). If interest rate risk materializes, small banks' capital buffers will decline following a monetary policy tightening and this in turn will amplify the effect of

[5] The UK had an inflation-targeting monetary policy regime at that time. The analysis and debate of UK monetary policy issues were published on a quarterly basis in the Inflation Report during this time period. The word 'capital requirement' does not appear in the inflation report during this time period.

changes in capital requirements. This effect is not present for large, well-diversified institution that can hedge interest rate risk more effectively.

But the effect on the borrowing firms could be mitigated if alternative sources of funding are readily available (condition iii). Indeed, Aiyar, Calomiris and Wieladek (2014a,b) show that at *bank* and *sector* level, branches of foreign banks, which are not subject to UK capital regulation, are such a source of credit substitution. These *leakages* can offset around 40% of the fall in lending from UK banks. Moreover, in our dataset we can distinguish between SMEs with *single bank* and *multiple banking relationships*. Intuitively, single-bank firms should be more affected by changes in capital requirements as they cannot easily substitute funding away from the affected bank, while firms with multiple banks can. Consistent with the credit substitution hypothesis, we find that only the former, but not the latter type of SMEs is affected. Of course, there could still be credit substitution in form of trade credit from other PNFC firms. This particularly likely in sectors with access to either foreign branch or capital market finance. Again, we find that firms operating in sectors with a high fraction of foreign branch finance contract assets to a smaller extent after a capital requirement change. Importantly, we find that monetary policy 'gets in all the cracks' in the sense that all firms, regardless of the number of banking relationships, are affected equally. [6]

The advantage of our framework and the time period is that it is easier to interpret changes in capital requirements during a time period without any other major shocks to the banking system. We examine the impact of microprudential capital requirement changes in a partial equilibrium regression framework. The results may of course not be generalizable to macroprudential regulatory changes to capital requirements in a general equilibrium framework. Regardless, in the absence of other evidence, we argue that the estimates provided in this paper could still be informative for policy makers and economic theory.

[6] It is of course possible that this would be different with a macroprudential capital requirement.

The remainder of the paper is structured as follows: Section 2 describes the UKs regulatory regime and the data. Section 3 describes the empirical approach for each of the proposed hypotheses, presents the results and examines them for robustness. Section 4 concludes.

2. UK capital requirement regulation and data

In this section we describe the UK's regulatory regime and the detailed firm-level database that make our investigation possible.

2.1 Bank-level data

Bank-specific capital requirements in most countries were set at a fixed value at or above the minimum of 8 per cent of risk-weighted assets since the introduction of Basel I in 1988. But in the UK, regulators varied bank-specific capital requirements, otherwise known as minimum trigger ratios, to address operational, legal or interest rate risk, which were not accounted for in Basel I (Francis and Osborne, 2012).[7] Individual financial institutions were subject to different capital requirements over time and these were subject to review either on an on-going basis or every 18-36 month. This regulatory regime was first implemented by the Bank of England, with the Financial Services Authority (FSA) taking over in 1997. The FSA based regulatory decisions for banks on a system of guidelines called ARROW (Advanced Risk Responsive Operating frameWork), which covers a wide array of criteria related to operational, management, business as well as many other risks. The ARROW approach is risk-based approach, where all type of risks were assessed by regulators, but anecdotal evidence suggests that there was greater emphasis on operational, as oppose to balance sheet and credit, risk. For example, in his high-level review into UK financial regulation prior to the financial crisis of 2008, lord Turner, the chief executive of the FSA, concluded that: "Risk Mitigation Programs set out after ARROW reviews therefore tended to focus more on *organisation structures, systems and reporting procedures, than on overall risks in business models*" (Turner, 2009).

[7] A trigger ratio is the technical term for capital requirement, since regulatory intervention would be triggered if the bank capital to risk-weighted asset ratio fell below this minimum threshold.

Similarly, the inquiry into the failure of the British bank Northern Rock concluded that "under ARROW I there was *no requirement on supervisory teams to include any developed financial analysis* in the material provided to ARROW Panels" (FSA, 2008). Moreover, in the same report it was recognized that '*the FSA is short of expertise in some fundamental areas, notably prudential banking experience and financial data analysis*' (FSA, 2008). For example, when FSA regulators examined the risk management practices at a financial institution, they would ask a list of questions such as "*How is risk management organized at the firm? Who is responsible? Are they competent? Are they independent? How regularly do they communicate with senior management?*" (FSA (2001)). Based on this anecdotal evidence, we therefore argue that capital requirement changes are plausibly exogenous with respect to banks' balance sheet risks.[8] To verify if this is indeed the case, we test whether balance sheet characteristics available to supervisors, at the time the decision was made, can predict capital requirement changes. We find that this is the case, but balance sheet characteristics explain only about a third of capital requirement changes. We also use the residuals from this regression as an alternative measure of changes in capital requirement, which is orthogonal to balance sheet conditions by definition. This allows us to verify if our exogeneity assumption is a serious problem for econometric inference.

The Bank of England has kindly made these regulatory returns data, collected from the BSD3 form, available for our investigation. We collect data on a total of 67 regulated banks' lending to UK PNFCs. Our study covers the time period 1999 to 2006, for two reasons. First, the data after 2006 may have been affected by the start of the UK banking crisis associated with failure of the bank Northern Rock in 2007Q3. Furthermore, prior to 2008Q1, UK regulators relied on the risk-weights associated with Basel I. Unlike the Basel II risk weights, which were adopted thereafter and can be calculated using banks' Internal Risk Based (IRB) models, these risk weights assigned a weight of 100% to PNFC loans, regardless of individual loan characteristics. This

[8] Of course, we are not claiming that capital requirement changes are the result of a randomized experiment. We do not need such a strong statement for our analysis to work. All we need is that the regulatory changes are not affected by the behavior of firms borrowing from these banks.

additional regulatory margin would add a further layer of complexity to our analysis. To better isolate how changes in capital requirements affect bank behaviour, we therefore choose the sample period to finish in 2006. Regulated institutions were affected by 100 capital requirement changes during this time, all of which are shown in Figure 2, with summary statistics provided in Table 2. All of the other lender level balance sheet data was provided by the Bank of England's Statistics and Regulatory Data Division.[9] The control variables we derive from these data are described in greater detail in Table 1, with the corresponding summary statistics provided in Table 2.

2.2. Firm-level data

Firm level data come from the *Bureau Van Dijk Financial Analysis Made Easy* (BvD FAME) database, based on companies' filings with Companies House, the UK's firm registry. The key aspect of this dataset is that it contains the names of the banks (*chargeholders*) that have secured loans (*charges)* against each firm. According to Companies House, a charge is defined as the security, such as land, property or financial instruments a company provides as collateral for the loan.[10] While technically the charge is the collateral for the loan, we will use the term as a synonym for the loan itself and hence an indicator for the presence of lending relationship. The bank-firm relationships in BvD FAME are obtained from the charges registered with Companies House. Charges are legally required to be registered with Companies House twenty-one days after the loan has been created. It is in the interest of the lender to register the charge within the deadline, as otherwise it would not be able to seize the collateral if the company became insolvent. Indeed, the Bank of England surveyed one of the UK's 5 largest lenders in 2013 to examine whether these data actually reflect bank-firm relationships. This was the case for 99.8% of the firm-bank relationships in this dataset, which suggest a high degree of accuracy. Finally, it is important to point out that we only observe the initial charge: Additional funds or re-financing against the same asset later in time does not appear in the dataset. Similarly, we do not observe the maturity date for the vast majority of loans.

[9] All banks operating in the United Kingdom are legally required to provide this information to the Bank of England.
[10] http://www.companieshouse.gov.uk/about/gbhtml/gp3.shtml#ch9

There is are some important dimensions in which the data are different from the Credit Registry data used in other work (Jimenez et al. (2012, 2014, 2015), Iyer et al. (2014), Brun et al. (2013), Gobbi and Sette (2012)); in particular, they do not contain information on the amount of credit provided by each bank, nor the interest rate charged on each loan. The maturity date of the loan is often missing too. Therefore, we cannot use the firm*time fixed-effects identification pioneered by Khwaja and Mian (2008), as we do not have data on the loan amounts each bank provides to each firm.[11] However, the most important feature of the data is that it allows us to link the banks in our sample to individual firms. Moreover it does so at a specific point of time, which means that we can calculate the length of the bank-firm relationship based on actual transactions rather than on survey data (Petersen and Rajan (1994)). Finally, in order to examine our results for robustness, the firm-bank panel nature of the data also allows us to include bank-time effects, which fully control for endogeneity bias at the bank level.

There is a total of 331,000 private, non-financial firms (PNFC) with charges registered in BvD FAME (June 2007 version) and financial data for 1998 to 2006.[12] We match a total of 252,992 firms to 67 banks with capital requirements data. The matched sample is broadly consistent with the 331k sample of firms with registered charges (see Table 1A in Appendix A). Most of the firms in our sample are *small*: 92% have assets below $2.8mil, the official threshold for "small" companies as defined by Companies House.[13] This generates some missing items in the firm-level data, as firms classified as "small" do not need to report a Profit and Loss (P&L) account and only need to file an abridged version of the balance sheet. Indeed, an examination of the summary statistics in Table 2, reveals that, other than age and the length of the relationship, Total Assets is the

[11] Most of the companies in our sample (88%) have single bank relationship anyways.

[12] Note that a charge may have been registered before 1998 and still show up in BvD FAME. We exclude those charges created before 1990, thus the maximum observed length of a bank-firm relationship in our data is 16 years.

[13] According to Companies House, to be a small company, at least two of the following conditions must be met:

- annual turnover must be £5.6 million or less;
- the balance sheet total must be £2.8 million or less;
- the average number of employees must be 50 or fewer.

most highly populated balance sheet variable, while Turnover and Employees are under-represented. But information on the sector, postcode, date of incorporation and relationship characteristics are available for all firms, including small ones. We also have a very good coverage of current liabilities - which include short—term debt, trade credit and taxes - and of credit scores, as these are calculated by an external credit rating agency, CRIF Decisions Ltd, on the basis of both financial and non-financial information (directors' and shareholders' history, County Court Judgements)

Figure 3 shows that a single-bank relationship is dominant, which is not surprising in light of the fact that most of the firms in our sample are small.[14] However, even if only 12% of firms have a banking relationship with multiple institutions, given our large sample size we still have 26,800 firms with two banks and 2,700 firms with three banks. The number of multiple banking firms is therefore sufficiently large to allow us to run separate regressions for these entities. Given their access to multiple banks, we would expect these firms to be less affected by a change in the capital requirement of only one of their relationship banks, relative to single-bank firms, as the other bank can provide a source of credit substitution. The results confirm this expectation. In general, UK firms as whole are less reliant on public debt and equity – and more on bank lending – than the US corporate sector: bank lending represents 65% of total corporate debt in the UK and only 25% in the US (Bank of England (2011)). This high degree of bank dependence makes the UK's PNFCs relatively more susceptible to bank-level shocks.

Another characteristic of the UK banking system is that it's very concentrated: Figure 4 shows that the top 5 banks provide credit to 91% of firms in our sample. This naturally begs the question of whether we have enough variation in our independent variable of interest, the change in banks' minimum capital requirements, given that most of the sample is dominated by five institutions. We show that the results are robust to

[14] This is different from Braggion and Ongena (2014), who document that UK firms used to have single bank relationship before banking deregulation in 1971 and since then engage in multiple banking. Whereas Braggion and Ongena (2014) focus on listed firms, which tend to be larger and more transparent, we mostly have data on small firms, who are not listed and tend to have single banking relationship. Also, the fact that most small firms only have one bank is consistent with data from Italy (Balduzzi, Brancati, Schiantarelli (2015)).

dropping one of the large banks at a time or all of them at once. Next, Figure 5 shows the distribution of firms by sector. The Commercial Real Estate (CRE) sector is the most dominant (37%) followed by Wholesale and Retail Trade (17%), Construction (13%) and Manufacturing (12%). Together these sectors comprise 79% of the companies in our sample. Table 3 shows the growth rate of firms' total assets broken down by sector. All sectors exhibit positive asset growth on average, consistent with the growth in the UK economy over this period. Among the top five sectors by size, the sectors involved in the UK housing boom of the 2000s, the CRE and construction sector, grew faster on average. However, it is important to notice that even in these sectors, a substantial number of firms (more than 25%) have negative asset growth.

In conclusion, our dataset contains 252,000 UK PNFC firms, borrowing from 67 different banks with capital requirement changes between 1998-2006. Most firms are *small*, meaning that they borrow from a *single bank*, especially one of the big 5, and they are concentrated in the CRE, Wholesale and Retail, Construction and Manufacturing sector. We know both the age of the firm and the *length* of the relationship with each bank. We have good coverage of firms' credit scores and share of current liabilities over total assets, but not of turnover, profits or number of employees. Finally, the 4digit SIC code and postcode area where these firms operate, allows us to construct sector*area*year fixed effects to control for unobserved heterogeneity among firms.

3. Empirical approach and results

In this section we describe the empirical framework to test each of the proposed hypotheses and report the results.

3.1 Empirical strategy

Economic theory suggests that changes in capital requirements can affect the asset growth of individual firms in several ways. Given that equity is expensive and capital requirements are a binding constraint on an individual lender's choice of capital, a bank may use retained earnings to rebuild its capital ratio or reduce risk-weighted assets, either at the external or internal margin. Specifically, it could reject the loan of a larger borrower and lend to a borrower with a smaller loan requirement, or just offer a smaller loan to the same borrower. Given the lack of interest rates, loan terms and loan applications in our dataset, we cannot distinguish between all of these channels. But all of them predict a negative impact on borrower asset growth following a rise in capital requirements of the main relationship bank. Thus, ideally and intuitively, we would test this hypothesis with the following regression:

$$\Delta\, ln\, Y_{i,t} = \alpha_i + \delta_1 \Delta KR_{j,t} + + \varphi FC_{i,j,t} + \gamma BC_{j,t} + T_{hkt} + e_{i,j,t} \qquad (1)$$

where $\Delta\, ln\, Y_{i,t}$ is the change in the natural logarithm of total assets (or investment, current liabilities) at time t of firm i that took out a loan from bank *j*. $\Delta KR_{j,t}$ is change in the minimum capital requirement ratio of bank *j* associated to firm i at time *t*. [15] $L_{i,j,t}$ is the length of the relationship between firm i and bank *j* measured in years.[16] $FC_{i,j,t}$ is a vector of firm characteristics for firm *i*, borrowing from lender *j* at time *t*. $BC_{j,t}$ is a vector of bank characteristics for bank *j* at time *t*, all of which are listed in Table 1. α_i is a firm fixed effect to account for firm unobservable time-invariant characteristics. [17] T_{hkt} is a vector of sector-area year effects to account for unobservable geographical and sector differences, in particular in loan demand and unobservable firm characteristics, among the 122 areas and 400 sectors that UK firms operate in over time. $e_{i,j,t}$ is assumed to be a normally distributed error term. All standard errors are clustered by firm in each regression specification.

However, the estimated $\widehat{\delta_1}$ from equation (1) would not be significant if the direct effect of capital requirements on borrowing firms were not linear but it was dependent on the length of bank-firm relationship. Economic theory suggests that this could be important: Bolton et al (2014) find that relationship banks attenuate negative shocks to

[15] For firms with a single bank relationship (88% of the sample), we just use the $\Delta KR_{j,t}$ of the main relationship bank *j*. But for banks with multiple relationships, we take an average of $\Delta KR_{j,t}$ across all banks that lend to firm *i*

[16] This variable takes the value of zero in the first year of the relationship, the value of one in the second year of the relationship and so forth. For multiple-banking firms, it is equal to the average length of each relationship.

[17] Note that for firms with single bank relationships, the firm fixed effect will also be a bank fixed effect.

individual firms during a crisis. [18] In our case there is no financial crisis shock, but rather a regulatory shock that would negatively affect bank lending. Since we expect the change in capital requirements to affect disproportionately more firms with short-relationships, we should interact the change in capital requirements with a non-linear function of length. For example, we could use a log-specification as in Balduzzi, Brancati, Schiantarelli (2015):

$$\Delta \ln Y_{i,t} = \alpha_i + \delta_1 \Delta KR_{j,t} + \delta_2 \Delta KR_{j,t} * \ln(L_{i,j,t}) + \mu \ln(L_{i,j,t}) + \varphi FC_{i,j,t} + \gamma BC_{j,t} + T_{hkt} + e_{i,j,t}$$

Here we expect $\widehat{\delta_1} < 0$ and $\widehat{\delta_2} > 0$, meaning that the initial negative effect of capital requirement changes on firms' asset growth is attenuated as the length of the bank-firm relationship increases. Alternatively and equivalently, we could use the following:

$$\Delta \ln Y_{i,t} = \alpha_i + \delta_1 \Delta KR_{j,t} + \delta_2 \Delta KR_{j,t} * \frac{1}{1+L_{i,j,t}} + \mu \frac{1}{1+L_{i,j,t}} + \varphi FC_{i,j,t} + \gamma BC_{j,t} + T_{hkt} + e_{i,j,t} \qquad (2)$$

This would imply a $\widehat{\delta_1} > 0$ and $\widehat{\delta_2} < 0$. The specific convex and decreasing functional form for the length $(\frac{1}{1+L_{i,j,t}})$ has the same property of the log-specification allowing for the effect of capital requirements changes to be largest in the first year of a new bank-firm relationship ($L_{\mathrm{i,j,t}} = 0$) and then quickly decline over time. We choose this specific functional form so we can draw inference of the negative impact of capital requirements using $\widehat{\delta_2}$ and experiment with different type of fixed-effects, such as *bank-year* effects, that absorb the main effect δ_1. However, as Table 3 shows, choosing the log-specification or $\frac{1}{1+L_{i,j,t}}$ is identical in terms of estimated magnitudes.

In this study we aim to identify the loan *supply* effect of changes in capital requirements, and hence it is important to control for loan *demand*. This is a challenging task in most empirical studies. But the detail of the dataset in this paper allows us to follow the approach presented in Aiyar, Calomiris, Hooley, Korniyenko and Wieladek

[18] There is also a "dark" side to relationship lending: once the relationship is established, banks extract monopoly rents from their borrowers (Sharpe (1990)). Ioannidou and Ongena (2010), using data on the Bolivian credit registry, find that banks charge a lower interest rate at the beginning of the relationship and eventually increase it sharply. Our estimates are consistent instead with the "positive" view of relationship lending.

(2014), who exploit geographical information on the loan destination and use geographical time dummies to control for loan demand. To the extent that loan demand varies over time and across geographical boundaries, this allows them to interpret their estimates as supply effects. Information on the sector and location of the firm in our sample is very detailed: we know the full 4-digit *Standard Industry Classification (SIC)* code and the full postcode for all our firms. There is a total of 400 4-digit sectors and 2,757 full postcodes which we group in 122 postcode areas. For example, we can distinguish between farming of cattle in Birmingham and farming of poultry in Coventry. This high level of granularity allows us to construct 4digitSIC*postcodearea*year fixed-effects (150,753 fixed effects) to control as much as possible for sector specific loan demand and to hence compensate for the lack of firm level controls.[19] To the extent that loan demand shocks differ across UK areas and sectors over time, the regression estimates on the change in capital requirements can thus be interpreted as a supply effect. Importantly, our matched firm-bank-year panel also allows us to use *bank-year* effects to control for any unobserved, time-varying omitted variable at the bank level. Going even further, we can construct *bank-sector-area-year* effects, to ease concerns that capital requirement changes are correlated with the credit portfolio of a specific bank. Indeed, with *bank-sector-area-year* effects we're fully controlling for each bank-borrower type pair, where the borrower type is identified by the sector and area where the firms operate.

3.2 Results

Table 3 presents estimates of equations (1)-(2) for the growth rate of total assets. All specifications include firm fixed effects and either *year* or *sector×area×year* fixed-effects. In column (1) we show that the direct effect of capital requirement changes on firms' total assets is not significant on its own. However, when the change in capital requirement is interacted with a non-linear function of length, such as in columns (2)-(6),

[19] We will see that the difference between controlling for year level fixed effects (7 FE) and the 4digSIC*area*year fixed effect (150k FE) is usually not important in our baseline regressions. Our work has this in common with other papers: For example, in Jimenez et al. (2012) the difference between year and firm*year fixed effects is minimal.

we can clearly see that there is a significant effect. In column (2), where we interact with $ln(L_{i,j,t})$, the coefficient on $\Delta KR_{j,t}$ is negative and equal to -0.0252: it implies that for a 1 percentage point rise in capital requirements, the growth rate of assets declines by 2.5% in the first year of the relationship. This effect is then attenuated for firms with longer relationships, as the coefficient on the interaction term $\Delta KR_{j,t} * ln(L_{i,j,t})$ is positive. In column (3) we explore the alternative specification using $\frac{1}{1+L_{i,j,t}}$ interaction. The coefficient on $\Delta KR_{j,t}$ is positive and equal to 0.08, but the interaction term is negative and equal to -0.034: it implies that following a 1 percent rise in the capital requirement of bank *j*, firms *i's* asset growth declines by -2.6%(-0.034+0.08) in the first year of bank-firm relationship. This is very similar to the estimate obtained with the log-specification. Again this effect becomes weaker as the length of the relationship increases ($\frac{1}{1+L_{i,j,t}}$ is a decreasing function of length). This is consistent with a "positive" view of relationship lending whereby the affected bank does not cut lending to its long-term customers, but rather offer worse credit conditions to new borrowers. Interestingly, the coefficients barely change when the aggregate time effect (7 fixed effects) is replaced with a more highly disaggregated *sector×area×year* effect (around 150,000 fixed effects): this stability of the main coefficient of interest is consistent with the findings in other empirical banking papers that use micro level data to examine similar issues (Jimenez et al. (2012, 2014, 2015) etc).

One concern with the regressions in columns (2) and (3) is the lack of firm characteristics. In particular, one may worry that our result is not picking up an effect on *new borrowers*, but rather the fact that banks may decide to cut lending or offer worse credit conditions to *young firms,* which also tend to be riskier. In column (4) and (5) we therefore control for firms credit ratings, the share in current liabilities over total assets (as a proxy for short-term debt exposure) and the age of the firm[20]. These characteristics enter either contemporaneously or lagged. The coefficient increases to -0.07 with lagged

[20] Note that since the length of the relationship and the age of the firm are collinear with the firm fixed-effect (they both deterministically increase by one every year) we do not control for age as a continuous variable, but rather with dummies defined by quartiles of age.

characteristics but remains highly significant. The coefficient on the interaction term is remarkably stable in column (5) compared to columns (3) and (4), while it is larger in column (6). Column (6) is our preferred specification in so far as contemporaneous firm characteristics at time t in column (5) are simultaneously determined with the growth rate of assets at time t and thus more likely to be endogenous than the lagged characteristics.

Figure 6 plots the partial effect of a change in $\Delta KR_{j,t}$ on the growth rate of total assets ($\delta_1 + \delta_2 \frac{1}{1+L_{i,j,t}}$) for the estimates in column (6) of Table 3. The negative effect of a 1 percent increase in capital requirements is greatest during the first year of the impact at -.076 and then dissipates quickly over time, with an effect that is not significantly different from zero by the time 3 years have passed from the beginning of the relationship. Note that the smooth shape of the function is given by the convex function chosen for length ($\frac{1}{1+L_{i,j,t}}$), where the effect is largest for the new borrowers and quickly dissipates, but this is borne out in the data too. A non-parametric regression, interacting $\Delta KR_{j,t}$ with 15 dummies, one for each year of length, yields a broadly similar shape (Figure 7).

Next, we explore the robustness of our results to a large battery of sample selection, model and exogeneity assumptions in Table 4.

Our results also hold if we change the dependent variable to investment, *i.e.* the growth rate of tangibles normalized by lagged assets. This is important because it means that we can say with confidence that capital requirements have real, tangible effects on SME firms that borrow from the bank the first time and are not just a substitution effect within total assets (for example a decrease in account receivables). The concurrent decline in current liabilities also indicates that the decline in total assets is driven by a fall in short-term debt, such as bank debt.[21]

[21] Current liabilities also include trade credit and taxes, so they are a somewhat noisy proxy for short-term bank debt.

The interpretation of equation (1) hinges on two important econometric assumptions. First, we interpret $L_{i,j,t}$ as the length of the bank-firm relationship. But this could just reflect the age of the firm. In that case our regression estimates would imply that older, perhaps safer, firms are less affected by capital requirement changes. We explore this in the third row of Table 4 by replacing $L_{i,j,t}$ with age in our baseline regression. The results are not statistically significant anymore. This supports our interpretation of $L_{i,j,t}$ as the length of the firm-bank relationship. Second, we assume that changes in capital requirements can affect the asset growth of the borrowing firm, even after the relationship has been established ($\Delta KR_{j,t}$ can be different from zero in any year of the bank-firm relationship). While we do not directly observe re-financing or changes in loan terms, we wanted to allow for this possibility explicitly. But this may not be the right assumption to make if firms only take out a single loan with fixed terms. Alternatively, we could fix the change in capital requirements at its value in the first period of the relationship ($\Delta\overline{KR}_j$). These results, shown in row 5 of table 5, are very similar to the baseline model. Since this specification is akin to a restricted version of the baseline model, this suggests that this restriction is not binding, which is why we proceed with the baseline model. In row (4) we also show that our results are broadly robust to aggressive windsorisation, indicating that the results are not driven by outliers. In row 6 we show that the results also hold for those loans for which we have a maturity date. This is important because the results may not be due to relationship lending but to a more mechanical reason: firms can repay their loans over time, and this would also make the impact on the asset growth of the *average* borrower smaller.[22] However this does not seem to be the case.

A separate econometric issue is sample selection among both banks and firms. Table 2 suggests that our sample is dominated by small firms: the median firm has total assets of £350,000. With very small firms there is always the risk that our sample might contain entities which exist purely on paper (*shell companies*). To explore if this is an

[22] The Bank of England surveyed one of the largest five lenders in the UK in 2013 to examine whether their 2012 loan book entries correspond to those in our database. This exercise suggested that about 30 percent of firms paid the debt back within a year.

issue, we exclude the smallest bottom half (total asset size below £350,000) and three-quarters (total asset size below £914,000) of the firm population in rows (7) and (8) of Table 5. To explore whether ownership structure matters, we estimate the base-line regression separately for firms where either more or less than 50% belongs to the same owner in the following two rows ((9) and (10)). As shown in Figure 4, 91% of the firms in the sample have relationships with 5 large UK banks. The remaining 9% have relationships with 62 different banks. One potential issue is that our results in this paper are due to the presence of one or two banks in the sample. To examine if this is the case, we dropped each large bank from the sample in rows (11) through (16). In rows 17 and 18 we re-estimate the main model excluding either all the small (61) or large banks (6). The following two rows show this result with bank-year effects. Our main result is robust to all of these perturbations.

The main identification assumption in this paper, which we discussed extensively in section 2 is that changes in capital requirements were not determined by the quality of individual banks balance sheets. Although there is anecdotal evidence to support this assumption, it is plausible that we are wrong. In that case our results could be the result of a third omitted variable. The firm-bank nature of our data allows to us examine if this is an issue. In row (21), we include additional bank balance sheet variables, namely bank size, the retail deposit to total asset ratio, the liquidity (government bonds and cash) to total asset ratio and one-year lag and one-year lead of the change in write-offs as additional variables in our model. The results remain unchanged. It is of course plausible that the omitted variable is not picked up in this vector of observable control variables and only weakly correlated with them. For this reason, we replace the *sector×area×year, with bank×year,* effects in the specification in row (22). The regression estimate on $\Delta KR_{j,t} * \frac{1}{1+L_{i,j,t}}$ is -.051, which is very similar to the base-line regression. Rows (23)-(25) take this even further by allowing the bank-year effects to vary with the sector (*bank×sector×year*) or area (*bank×area×year*) where the bank is lending or both simultaneously (*bank×sector×area×year).* The results are remarkably stable to the inclusion of all these fixed-effects. This is especially comforting for us because it means that an institution's credit portfolio, as measured by its exposure to borrowers in a specific sector and area, is not correlated with changes in capital requirements. Overall this suggests that omitted variable bias at the bank-balance sheet level, and hence endogeneity from that source, does not seem to be a significant issue, consistent with our main identification assumption of an exogenous change in capital requirements with respect to credit conditions.

Finally, we re-examine our assumption that $\Delta KR_{j,t}$ is exogenous with respect to balance sheet variables in rows (26)-(35). For this purpose, we collect 23 balance sheet variables that supervisors could have taken into account in their regulatory decisions. We then use single, multiple and Bayesian Model Averaging regression models to identify the most of capital requirement changes. Please see appendix B for more detail. It turns out the strongest predictors for changes in capital requirements are changes in interest rate risk, profit&losses and change in total lending, all scaled by total assets in the previous period. These variables alone explain 30% of the R^2 in the changes in the capital requirements. When we include the change in capital buffer in a second regression, the explained R2 rises to 36%, but we treat this result with caution, since this variable could be correlated with changes in capital requirements almost by construction. Regardless, we find that balance sheet characteristics can at most explain up to 36% of the variation in capital requirement changes. This suggests that the vast majority of capital requirement changes are due to non-balance sheet risk, in line with our initial assumption. Furthermore, the residuals of these predictive regressions will be orthogonal to balance sheet characteristics by construction. We therefore use the residuals from these regressions as two alternative measures of $\Delta KR_{j,t}$, called ΔKR_Exog1 and ΔKR_Exog2. These should be more reflective of changes in capital requirements due to operational, as oppose to credit, risk. When we use these alternative, certainly "more" exogenous measures of capital requirements, the results are remarkably similar to the base-line results, also when we include bank-year fixed effects.

Overall Table 4 suggests that the results obtained with the base-line model are robust to sample selection issues at firm and bank level, more aggressive windsorisation, more restrictive econometric modelling choices, omitted variables bias and exogeneity assumptions about $\Delta KR_{j,t}$.

3.3 Interaction with Bank and Firm Characteristics

Economic theory suggests that *bank characteristics* may affect the adjustment to changes in capital requirements. Banks with a greater fraction of retail deposit funding, which is subject to deposit insurance and hence subsidised, will find equity relatively more expensive than debt and hence may adjust lending to a greater extent. Those with a lot of liquid assets, given that these are not information sensitive, can sell them for a profit and hence grow capital back through retained earnings. Changes to capital requirements should also affect highly-leveraged banks relatively more. While banks with high levels of capital can always choose to have a smaller capital buffer, it is unlikely that this would be an option for banks whose capital buffer is already tight.[23] Consequently, a constant capital buffer is a necessary condition for changes in capital requirements to affect banks' actual capital choices and hence loan supply. Indeed, Figure 1 suggests that capital choices of banks in the bottom size quartile of capital buffers co-move almost contemporaneously with requirements, unlike those in the top quartile. Finally, banks with a large trading, relative to the banking, book may always divert capital from one to the other, which may make the impact smaller.[24]

We examine all of these hypotheses in Table 5, by interacting our main variables of interest with each of these variables in a non-linear fashion with two dummy variables, taking the value of one when the variable is either in the top (high) or bottom (low) quartile. We report the marginal effects for the high and low category in the last two

[23] An item referred to as 'the capital planning buffer' was also part of his regulatory regime. This buffer was a minimum accepted threshold for a bank's capital buffer. Banks were likely to receive greater regulatory scrutiny if the bank's capital buffer fell below the actual planning buffer.

[24] In particular, the trading book was subject to internal model based risk weights. It is therefore likely that moving assets from the banking to the trading book would lead to a reduction in the risk weight for a given asset/loan and hence reduce the capital required for that asset/loan implicitly. As a result entities with larger trading books should be less affected by capital requirement changes.

columns. For robustness, we also examine interaction terms in a linear way. This exercise suggests that *only the capital buffer*, both linearly and non-linearly, is a statistically significant variable that affects the transmission of capital requirements: Banks with tight capital buffers transmit this shock to a much greater extent. With the linear interaction, the results show that firms in the first year of a relationship with bank with a zero buffer experience a decline of around 10% of asset growth (+0.0253-0.132). For any 1% increase in the buffer, the effect on new borrowers is attenuated by 1.4%. With the non-linear interaction, the results are similar. There is a monotonic ordering where banks with large (medium) [small] capital buffers are not (more) [most] affected by changes in capital requirements. Note that the magnitude of the effects for borrowers from banks with small buffers (-19%) depends on the presence of the more disaggregated sector-area-year fixed-effects; with year fixed-effects only (not shown) the effect is more moderate, around -10%.[25]

Firm characteristics may affect the way in which a firm reacts to a loan supply contraction. Clearly, firms which are more debt reliant should be affected to a greater extent. Similarly, older firms are probably more likely to be able to rely on trade networks for trade finance, which would allow them to mitigate any loan supply effects. Finally, riskier firms may also adjust in a different way. These regressions, shown in the lower panel of Table 5, do not suggest a significant degree of variation by individual firm characteristics, as opposed to the strong results by bank characteristics. Of course, firms operating in any given industry may share characteristics: for example commercial real estate and construction firms are probably more likely to be debt reliant. When we split our data by sector, the results suggest that changes in capital requirements have the biggest impact in the commercial real estate (CRE) sector. This is consistent with the evidence in Bridges et al. (2014), who also find that lending to this sector shrinks the most in response to higher capital requirements.

[25] These results are available from the authors upon request.

3.4 Monetary and capital requirement policy

We also contribute to two debates on the relationship between monetary and capital requirements policy. The objective of monetary policy is to stabilise fluctuations in output and inflation. White (2006) argues that even with inflation at target, monetary policy may contribute to the build-up of sectoral imbalances, which may lead to future financial instability.[26] Indeed, an important assumption in the current debate is that macroprudential policy can, not only raise the resilience of the financial system, but also help to address the build-up of sectoral imbalances more directly. A second hotly debated issue is whether monetary and capital requirement policy should be co-ordinated. Clearly if each instrument has one target, and the effects of both instruments are completely orthogonal, no co-ordination is necessary. In that world monetary policy would only focus on price stability, while capital requirement policy would only address financial stability. However, if one instrument affects the transmission of the other, then this will need to be taken into account by the corresponding policy committee to avoid under or overshooting of the target, which can have socially undesirable consequences. Whether co-ordination is desirable or not therefore depends on whether these instruments reinforce the effects of each other. In this section we first explore the impact of monetary policy on asset growth, examine which sectors react the most and finally we study if these two policy instruments dampen or reinforce the effects of each other.

To explore all of these questions, we need a suitable measure of UK monetary policy. Unlike for the United States (Romer and Romer, 2004), there is no commonly accepted narrative measure of UK monetary policy shocks.[27] We need therefore construct our own measure of monetary policy shocks (surprises) in this paper. During this time period, the UK's Monetary Policy Committee objective was to maintain inflation close to target, while minimising output volatility, at the two year horizon. Hence we estimate

[26] Taylor (2009) also argues that US monetary policy, though inflation was at target, contributed to the eventually unsustainable US housing boom ahead of the 2008-2009 recession.

[27] Cloyne and Huertgen (2014) employ this type of approach following Romer and Romer (2004) for the UK, although for a different sample period and focusing on the macroeconomic effects of monetary policy. At the time of writing this series was not yet ready to be shared publicly.

Taylor rule coefficients using the Bank of England's official forecasts of CPI inflation and real GDP growth two years ahead. This yields Taylor rule coefficients of 1.5 and 1, respectively. Yet agents will of course have formed an expectation of the forecast announcement and may have already reacted to the monetary policy decision. It is for this reason that it is necessary to isolate the unexpected component of the Bank of England's official forecasts. Measuring ex-ante expectations of these forecasts before is clearly not an easy task. But the Bank of England does survey professional forecasters on their forecasts two years ahead, one week before the official Bank of England forecast is announced. We use the mean of this survey of professional forecasters as our measure of ex-ante expectations of the Bank of England's official forecast. Subtracting the expectations for the forecast from the announced forecast in that time period, yields CPI inflation and real GDP growth forecast surprises. Applying the Taylor rule coefficients to these two surprises and adding them leads to our proposed measure of a monetary policy shock.

Using different Taylor rule coefficients does not make a substantial difference to this series of monetary policy shocks. As Figure 8 shows, all these measures co-move closely, with the exception of alternative 4, whose smooth shape is given by the lagged Bank rate coefficient. See appendix C for more details. This approach has two conceptual advantages. Since we are measuring monetary policy surprises, it is unlikely that agents would have already reacted to them. Secondly, to the extent that professional forecasters' forecasts and the Bank of England's official forecast are affected by common factors to the same degree, their difference will be free from common factors and hence less likely to be affected by omitted variable bias from other macroeconomic factors. Finally, Figure 1C in appendix C shows that our proposed monetary policy surprise series is correlated with bank rate, especially during tightening and loosening cycles, when monetary policy surprises are likely to be the greatest. The large monetary policy surprise in 2001Q3 evident in Figure 1C and Figure 8 is largely due to the MPC emergency meeting (*i.e.* not scheduled) following the 9/11 terrorist attack. At that time growth forecasts were revised downward and the bank rate was lowered, which explains the sharp drop in our monetary policy measure.

The proposed measure of UK monetary policy surprises has strong conceptual foundations: It is a surprise measure and unlikely to be affected by other macroeconomic common factors by construction. It also co-moves with the actual bank rate strongly, especially during tightening and loosening cycles, when monetary policy shocks are likely to be the largest. As discussed in the previous paragraph, the proposed series also allowed us to reproduce two well-known results from the US empirical banking literature. All of this suggests that while the proposed series is probably not a perfect measure of 'true' monetary policy surprises, it is likely highly correlated with them, and therefore suitable for our analysis in this paper.

From an econometric modelling perspective, we assume that monetary policy affects asset growth of the borrowing firms through exactly the same channels as changes in capital requirements. That is we again assume that firms will be less affected by monetary policy changes as the bank-firm relationship becomes longer over time. Given the aggregate nature of monetary policy, the main effect of monetary policy shocks will be absorbed by the time fixed-effects, so only the interaction with the function of length will show up in the regressions.

Table 6 explores the impact of monetary policy on asset growth within our econometric framework. Columns (1) – (2) show that monetary policy has an independent negative effect on individual firm's asset growth. While this effect is smaller when we include bank year effects (from -6.9% to -3.9%), it is still statistically significant at the 1% level. These firm-level multipliers are similar in magnitude to those found in Aiyar et al. (2014c) for bank lending to PNFC firms. In terms of the macro impact of monetary policy, since the fraction of new relationships is about 20% of total relationships each year, it would give an effect on aggregate output of about 0.8-1.38% for any 1% rise in the bank rate, which is a reasonable number. We then split the sample by large and small banks. A common finding in bank level studies of monetary policy is that monetary policy affects small banks substantially more than large ones (Kashyap and Stein, 2000; Cetorelli and Goldberg, 2012). We define a *large bank* as one of the top 6 UK banks (as found in Table 5, rows (11)-(16)), while a *small bank* is any of the remaining 61 banks in the sample. Our results are consistent with the studies above: monetary policy has a much smaller effect on firms that borrow from large banks. With bank year effects, the impact on large bank borrowers is not statistically significant anymore.

Finally, we explore interactions with firm credit risk and a big bank dummy. These suggests that monetary policy has a much bigger impact on the riskiest firms, which is consistent with the risk-taking channel of monetary policy as proposed by Borio and Zhu (2008). Dell'Arricia, Laeven and Suarez (2013) find similar evidence for US banks. As before, big banks are less affected than small banks and this difference is starker once we include bank year effects.

We then re-estimate the model for each sector separately in rows 11 - 20. This suggests that monetary policy mostly impacts firms in the commercial real estate sector (CRE), like capital requirements do: this is the most leveraged sector of the economy, hence it is the most likely to respond to policy changes. Similarly, firms in the other sector are also affected and these results are robust to including bank year effects. Interestingly, these are the same sectors that are affected by changes in capital requirements and with a similar magnitude. In other words, these results support the idea that prudential policy can effectively address sectoral imbalances which may arise as a result of monetary policy.

3.5 The Interaction of Monetary Policy and Capital Requirements

We also contribute to the discussion on the interaction of macro-prudential and monetary policy. Economic theory suggests that these two instruments should affect the transmission of each other, but the direction of the effect is unclear. We identify two main theories that examine the extent to which these two instruments affect the transmission of each other: the <u>bank capital</u> and <u>risk-taking</u> channel of monetary policy.

The bank capital channel predicts that monetary policy and changes in capital requirements reinforce the effects of each other. Van den Heuvel (2002, 2005) shows that an unexpected monetary policy contraction can lead to a smaller capital buffer, as a result of realised interest rate risk. Landier, Sraer and Thesmar (2013) show that US banks are significantly exposed to interest rate risk and that lending by the most exposed banks is more sensitive to monetary policy, especially for small and unhedged institutions. This occurs due to the natural maturity mismatch on banks' balance sheets between assets (*long duration*) and liabilities (*short duration*), so that an increase in the interest rate causes profits and hence the capital buffer to decline. Therefore a coincident rise in capital requirements will have a larger impact on the loan supply, as it is likely to be more binding.

The risk-taking channel, on the other hand, implies that the sign of the interaction may be asymmetric, depending on the sign of monetary policy. In an environment where banks target a fixed nominal return, a monetary policy expansion and the associated reduction in interest rates may lead to a search for yield and a rise in bank leverage (Borio and Zhu (2008), Adrian and Shin (2011)). One example of a search for yield is related to residential mortgages. In the UK, mortgage market products are homogenous and most mortgages are either variable or fixed for two or three years (Miles, 2005). This suggests that monetary policy, through the impact on short-term rates, has a powerful effect on the profitability of mortgages. A monetary expansion might therefore lead a bank to rebalance its portfolio from mortgages to PNFC loans, which are more bespoke and hence may allow banks to charge greater spreads above the cost of capital. And given that PNFC loans had twice the risk-weight of mortgages under Basel I, such a rebalancing is likely to lead to a significant reduction in the capital buffer.[28] Since banks with tight capital buffers are more likely to cut back risk-weighted assets, a change in capital requirements will have a greater impact on loan supply in this situation. On the other hand, during periods

[28] Empirical evidence from Spain (Jimenez et al. (2014)) and Bolivia (Ioannidou et al. (2014)) points out that a lower overnight rate induces lowly capitalized banks to take on more risk than highly capitalized banks, where the risk is measured with the presence of a bad credit history with nonperforming loans and a higher subsequent probability of default. This may be an additional reason for a reduction in capital buffers.

of monetary policy tightening, mortgage lending would become relatively more profitable and there is likely to be more rebalancing away from PNFC lending. In that situation a rise in capital requirement may be less binding on the actual capital ratio and therefore have a smaller impact on the loan supply. In other words, it is plausible that the sign of the interaction between these two instruments depends on the sign of the monetary policy action.

Despite the interest of the economic theory literature, empirical work that attempts to test these different transmission mechanisms is still scarce. Aiyar, Calomiris and Wieladek (2014c) is one of the first studies to undertake this task with UK bank-level balance sheet data. Across a large number of different specifications, they do not find any statistical evidence that these two tools reinforce each other.

The results from this specification are shown in Table 7. Column (1) shows that once monetary policy is included, a 100 basis points rise in monetary policy ($MPshock_t$) leads to an asset growth contraction of about 6.89%, roughly the same magnitude as a rise from capital requirements. Importantly, capital requirement changes still have an independent negative effect of a similar magnitude as before, even after controlling for monetary policy shocks. Column (2) adds the interactions between the monetary policy and capital requirements, together with the function of length with *year fixed-effects*. Note that both $\Delta KR_{i,t} \times MPshock_t$ and $\Delta KR_{i,t} \times MPshock_t \times \frac{1}{1+L_{i,j,t}}$ have a significant effect on the growth rate of assets, suggesting that using both at the same time would have an additional negative interaction for firms with short relationships. However the effect is not robust to the use of other fixed-effects, such as *bank*-year in column (3) and *sector×area×year* in column (4). This suggests that, at first sight, there is no consistent interaction between the two, confirming the results in Aiyar, Calomiris and Wieladek (2014c) and contrary to the bank capital channel in Van den Heuvel (2002). Column (4) allows the interactions to vary with the sign of the monetary policy measure, by interacting the corresponding coefficients with a dummy variable ($TIGHT_t$) that takes the value of one during a monetary policy tightening and zero otherwise. We do this interaction to test the risk-taking channel of monetary policy and allow for an asymmetry of the effect of monetary policy between a tightening and a loosening, possibly in interaction with capital requirement changes. The results do indicate that only monetary policy tightening negatively affect firms' growth rate of assets, but the interaction with capital requirements still yields no significant results. The results are robust to the alternative measures of monetary policy described in Appendix C (not shown, available upon request). We also tried triple and quadruple interactions with other variables such as firm credit risk or the term structure of interest rates, as suggested in Thakor (1996), but the results are contradictory and not robust.

As shown in Table 6, monetary policy has a larger effect on small banks. In Table 8, we explore whether the interaction with changes in capital requirements also depends on bank size. To reduce the number of interactions and have a more intuitive interpretation of the results, here we are only interacting with the monetary policy stance as defined by the dummy $TIGHT_t$ rather than the quantitative monetary policy surprise series as before[29]. The results clearly indicate, either with sector-area or bank-year fixed effects, that only small banks are negatively affected by changes in capital requirements with a monetary policy tightening. The effects are large and they imply that, for new borrowers of small banks, a 1% increase in capital requirements reduces asset growth by 13% (-0.22+0.053+0.055-0.016) or 9.7% (-0.204+0.0978+0.0850-0.0756), depending on the type of fixed-effects, following a monetary policy tightening. The result does not hold for large banks, for which there is no significant interaction between capital requirements and tight monetary policy. This is perhaps not that surprising, since monetary policy affects big banks to a much smaller degree in our sample.

In conclusion, monetary policy has an independent negative effect on individual firm asset growth. Our results suggest that the sectors which react to monetary policy, are also the ones most affected by changes in capital requirements. We also find a strong additional negative interaction with capital requirements for small banks, following a

[29] However, the results are very similar if we interact with $MPshock_t$ as before and are available upon request

monetary policy tightening. The result is intuitive insofar as small banks' capital buffers are more sensitive to interest rate risk than those of large banks and the smaller the buffer, the larger the impact of capital requirement changes, as shown in Table 5.

3.5 Does credit substitution affect the real impact of changes to capital requirements?

So far we have examined to which extent changes in capital requirements on individual banks affect the asset growth rate of the borrowing firms within a partial equilibrium framework. But in general equilibrium, alternative sources of credit, such as other banks, capital markets or trade credit, which are not affected by capital requirement changes, may offset these effects. The presence of these mitigating channels means that while microprudential changes in capital requirements may affect the individual bank loan supply, it is unclear if there is any impact on the asset growth rate of borrowing firms. Previous work has used bank-level data to test for the presence of such credit substitution for PNFC firms operating in the UK from 1999 -2006. Aiyar, Calomiris and Wieladek (2014a) and Aiyar, Calomiris and Wieladek (2014b) find evidence for credit substitution from foreign branches, but not from capital markets.

In Table 9, we interact the fraction of either foreign branch[30] lending (*BLnonreg*) or capital markets finance (equity + bond issuance, *Nonbank*) over the lending done by all UK-resident bank (*BLreg*), that are subject to capital requirements, at the sector level with our main coefficients of interests. The bottom panel of Table 9 shows the partial derivative of total asset growth with respect to changes in capital requirements for a zero or one standard deviation increase in either *BLnonreg/BLreg* or *Nonbank/BLreg*. We can see that for firms in sectors with a high dependence on lending by foreign branches (*i.e.* one standard deviation increase in the ratio of foreign branches' lending over regulated lending) the effects of capital requirements is reduced by 1.5% without including lagged firm characteristics (column (1)) and by 3% including these (column (3)). Increasing capital markets finance over regulated lending (columns (2) and (4)) has no significant

[30] Foreign branches are banks which are not subject to UK capital requirements, but rather to capital regulation in their home countries. In the UK, foreign branches are a large part of the banking system.

effect instead. These regression results overall confirm the previous findings in Aiyar, Calomiris and Wieladek (2014b) that credit substitution from foreign branches, in these data, is substantially stronger than that from capital markets.[31]

However, given that we analyse bank-specific capital requirement changes and that these are mostly uncorrelated in the cross-section, our data allow for a much more direct and powerful test of the credit substitution hypothesis. If financial entities, which were not affected by changes in capital requirements, truly are a source of credit substitution, then firms with multiple bank relationships should not be affected by capital requirement changes to only one of their relationship banks. However we expect monetary policy to affect all firms, regardless of the number of banking relationships. This is consistent with the idea that since monetary policy is an aggregate policy instrument, it affects all firms and banks regardless of whether they have access to alternative sources of credit or not. Note that to run this test we need to somewhat modify our baseline specification. In fact, in regression model (1), we are not distinguishing between single- or multiple-banking firms: $\Delta KR_{j,t}$ is the average of the capital requirement changes of all banks related to one particular firm in the case of multiple banks. In this section we want to allow the capital requirement of each bank to affect the asset growth of the related firm individually, rather than as an average. We can only undertake this exercise for those firms which have exactly two relationship banks, as there are too few banks with three relationships banks or more [32]. The results from this exercise are presented in Table 10, across three alternative measures of monetary policy, one per column. We interact both capital requirement changes and monetary policy shocks with $bank2dummy$, that takes value one if the firm has two relationship banks and 0 otherwise. In that case we are interacting with the capital requirement of the second bank. The results are clearly saying that while changes in capital requirements do not affect firms with two banks (the partial effect is not statistically significant), monetary

[31] This conclusion does of course not have to hold in other time periods and datasets. Adrian, Colla and Shin (2013) show the presence of strong substitution between bank and capital market finance for US firms during the 2009-2011 period. This might be also due to the fact that bank lending in the UK is much more bank-dependent and SMEs typically do not access capital markets.
[32] Unfortunately, we also do not have enough firms for which *both* banks change capital requirement *at the same time*. In that case we would expect to see an effect of capital requirement changes even if these firms have multiple banks.

policy clearly affects all firms equally, as the difference between the two groups (the interaction coefficient with $bank2dummy$) is not statistically significant.

Thus estimated on the same sample, monetary policy affects all of the firms through the length of the relationship, regardless whether the firm is a single or multiple relationship bank. This is a novel result in the empirical literature and it carries important policy implications for the use of monetary versus prudential policy.

4. Conclusion

Countries around the world have introduced macroprudential regulation to increase the resilience of the financial system to socially costly financial crises. One proposed instrument, which is also embedded in Basel III, is a time-varying capital requirement. But, to date, there is only little understanding of how this instrument will affect the real economy. The UK's unique regulatory regime, where banks were subject to time-varying capital requirements, together with a new firm-bank level database, covering all reporting real economy firms in the UK between 1998 to 2006, allows us to provide a first empirical examination of this important question. Unlike in other countries, UK Banks were subject to time-varying capital requirements, which varied by institution and over time. In this paper we examine whether this loan supply effect carries through to the asset growth rate of individual firms. We also compare our effect to monetary policy and examine if credit substitution may offset some of these effects.

Our results suggest that that an increase in a bank's capital requirement of about 100 basis points leads to a decline in the asset growth rate of the borrowing firm of about 3.9% to 6.9% in the first year of new bank-firm relationship and a similar effect is found for a 100bps rise in monetary policy. SME's borrowing from banks with tight (loose) capital or liquidity buffers are more (less) affected by the regulatory change. Consistent with evidence from banks' balance sheet in Bridges et al. (2014), firms in the commercial real estate sector experience the largest reduction. These results are robust a large number of perturbations, including different types of fixed effects to account for omitted variable and residuals from regressions of balance sheet characteristics on changes in capital requirement to account for potential issues of endogeneity.

We also contribute to the discussion on the relationship between monetary and prudential policy. In particular, we find that monetary policy has an independent effect on borrowers asset growth and that large banks are not affected, in line with Kashyap and Stein (2000) and Citorelli and Goldberg (2012) for the US. One rationale behind prudential policy is to limit the rise of sectoral imbalances which may arise from the regular conduct of monetary policy. Interestingly, we find that capital requirements and monetary policy affect the same sectors with a similar magnitude, providing support for this proposition. There is also a powerful negative interaction between capital requirements and a tightening of monetary policy, but only for small banks, whose buffers are more likely to respond to interest rate risk. Since these interactions are only present for small banks, imperfect co-ordination of these policies may not necessarily result in large macroeconomic costs.

Our results also suggest that SME's with multiple bank relationships are not affected by changes in capital requirements to only one of their relationship banks. But a tightening in monetary policy affects *all* firms, regardless of the number of their bank relationships. This suggests that while prudential policy tools such as capital requirements affect *single*-bank firms only, *i.e.* those that cannot easily find alternative sources of external finance, monetary policy shocks "get in all the cracks". The extent to which this will be a problem with macroprudential policy, which should in principle affect all banks equally, is not well understood yet and an interesting area of future research.

APPENDIX A – Data

BvD FAME data come from the June 2007 CD-ROM. The search strategy filters firms according to the following criteria:

- Exclude Financial and Insurance companies (2003 SIC codes from 6500 to 7000)
- At least one year of Total Assets
- At least one registered charge

This yields a total of 331,996 companies operating in the UK. Note that the search strategy deliberately does not consider filters on turnover, employees or other variables, as this would create a reporting bias in favour of medium and large companies.
We match a total of around 252k firms to 67 banks for which we have capital requirements data. Notice that we do not match all of the 331k firms because either the *chargeholder* is a non-bank (private citizens, finance companies or other funds) or because the c*hargeholder* is a branch of foreign bank, not subject to UK capital regulation.
In a few cases the name of a bank is listed under *chargeholder* for a charge with a firm although the bank is only acting as an agent for another lender. Large banks often times act as agents for another lender to monitor and screen the borrower in exchange for a fee. Therefore, it may not be correct to match a firm to these agent-acting banks, as capital requirements changes at the agents' level should not affect credit conditions for the borrower (although sometimes the bank is listed as "Agent acting for itself"). Other such non clear roles are "security trustee", "agent trustee". There are only 2,581 firm-year observations with such unclear roles: we can safely exclude them from the regressions and the results are not affected.

Table 1 A: Comparison between matched and non-matched sample

Variable	Observations	Mean	10th	50th	90th
331k non matched sample					
Total Assets	1,921,170	3.56	.02	.26	1.9
Turnover	471,825	8.7	.02	.37	8.3
Employees	185,720	183.6	3	37	234
252k matched sample					
Total Assets	1,1485,854	2.47	.053	.35	2.37
Turnover	286,789	7.69	0.036	0.52	10.8
Employees	126,575	156	3	45	243

Table 1A shows some summary statistics for the two samples, the 331k non matched sample and the 252k matched sample. Rather than looking at the means, which are not robust to outliers, we can look at the other percentiles of the distribution to see that the two samples are broadly consistent across the two

Appendix B – Exogeneity of $\Delta \boldsymbol{KR}_{j,t}$

An important assumption within our regression framework is the exogeneity of changes in the capital requirement with respect to bank balance sheet variables. Our argument that this condition is satisfied relies on anecdotal evidence from speeches of senior policy makers and FSA reports during the time that regulatory changes took place. In this section we explore this assertion formally. Specifically, we test if bank balance sheet variables that supervisors had access to at the time of the regulatory decision can statistically predict the regulatory change. If this is the case and the predictors can explain a high fraction of the variation in capital requirements, then our initial assertion would have been invalid. If all relevant balance sheet variables have been included in the predicting model, then the residual will reflect capital requirement changes that reflect non-balance sheet risk. We can therefore use the residual to verify if the results change when we use these "non-balance sheet based" capital requirement changes in our model. We have collected 23 such variables. Their description and summary statistics are given in table 1B, respectively. The first 12 variables are taken from the regulatory returns form, the BSD3 form. This is the form that records capital requirements. It also records the balance sheet risk variables that supervisors could have used in their judgement to change the capital requirement of a given institution. Of course, supervisors could have also relied on additional balance sheet information that was not recorded on the BSD3 form. We therefore also use additional balance sheet information, such as the change in different sectors, scaled by lagged total assets, and the change in the liquid asset and core funding ratio. We then explore if changes in capital requirements can be predicted by any of these variables with the following regression framework:

$$\Delta KR_i = \beta X_i + \varepsilon_i$$

Where ΔKR_i is the non-zero change in capital requirement for bank i *and* X_i is the matrix of exogenous variables that helps to predict this particular instance of ΔKR_i. Under the assumption that the information set contained in the vector of predictors X_i, the residual, ε_i, will reflect capital requirement changes due to non-balance sheet risk, which are exogenous with respect to balance sheet items. We use this regression framework in three different ways to isolate the predictors of changes in capital requirements. First we regress each individual predictor against the change in the capital requirement with a single regression. Then we run a multi-variate regression, including all of the predictors together. However, we have little information on whether supervisors looked at these indicators together or individually to form their judgement about a capital requirement change. Given 23 variables, there are over 8 million regression models that could be explored for this purpose. We therefore follow the Bayesian Modelling approach proposed in the economic growth literature[33] to explore all of these possible model combinations. Due to the inherent model uncertainty, we only use predictors which are found to be statistically significant in all three approaches. Table 2B presents the results from this exercise. This suggests that the change in total loans scaled by total assets in the previous period, change in total interest rate risk and total profits&losses scaled by risk-weighted assets in the previous period are the most important predictors of capital requirement changes.

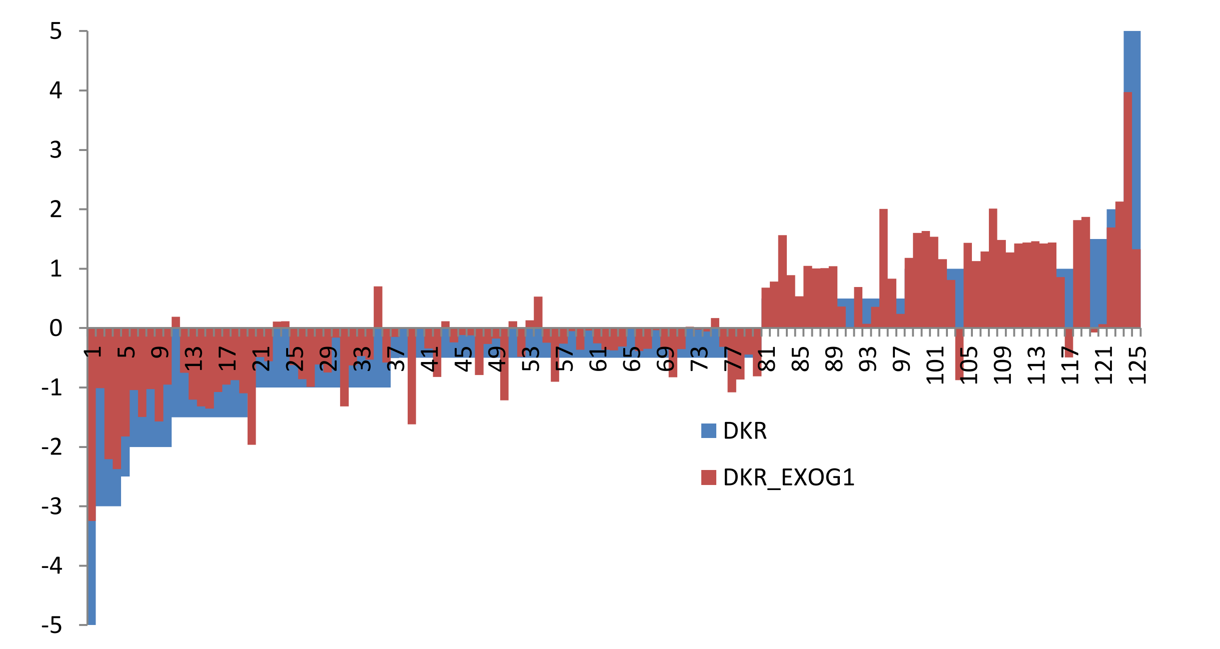
Note that all of these variables are contemporaneous. This is because supervisors probably anticipate and see these developments shortly before they actually happen. This is a standard assumption in the monetary policy reaction function literature as well.

[33] See Doppelhofer, Miller and Sala-i-Martin (2004).

As a result we are left with the reduced/restricted regression equation shown in table 3B. The first regression is the restricted model with the variables suggested by table 1. In the second regression, we also add another important control variable: The change in the capital buffer. We abstained from including this variable in the original variable selection framework since, given a constant capital to risk-weighted asset ratio, the change in the capital buffer will be correlated with the change in the capital requirement almost by construction. All of these balance sheet predictors are highly statistically significant in both regression models. Balance sheet characteristics therefore do predict changes in capital requirements. But, according to the R^2 , the can only explain 30 – 36% of the variation in capital requirements. Assuming that we included all relevant balance sheet variables, this means that between 64% to 70% of the variation in capital requirement changes is due to non-balance sheet risk, which is consistent with the anecdotal evidence that there was a relatively greater focus on non-balance sheet risk during this period.

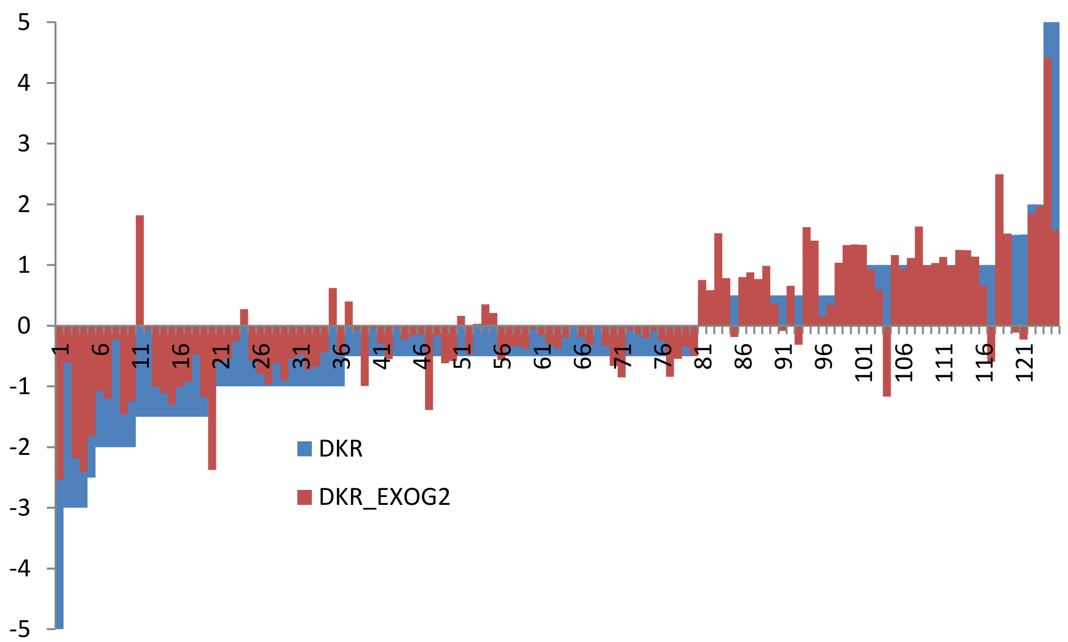
Given that the residuals of these regressions are orthogonal to the balance sheet characteristics by definition, we can use them as measures of non-balance sheet risk capital requirement changes. We refer to the residuals obtained from columns (1) and (2) as ΔKR_Exog1 and ΔKR_Exog2, respectively. Charts 1B and 2B show these residuals plotted against actual capital requirement changes. Consistent with the regression results, this suggests that the majority of capital requirement changes were due to non-balance sheet risk. It is therefore not surprising that the results ΔKR_Exog1 and ΔKR_Exog2 are very similar to those obtained with the baseline specification.

CHART 1B: DKR vs DKR_Exog1



Source: Authors Calculation.

CHART 2B: DKR vs DKR_Exog2



Source: Authors Calculation.

<u>Details on Bayesian Model Averaging</u>

In this section we provide more detail on our implementation of Bayesian Model Averaging. We have up to 23 (k) possible predictors of the change in capital requirements, but only some of these predictors seem to matter the most for regulatory decision. The economic growth literature has proposed Bayesian Model Averaging to objectively determine which variable has the highest explanatory power. We follow this approach here to select the best predictors of changes in capital requirements based on their posterior inclusion probabilities.

The idea underlying Bayesian Model Averaging is to consider the results for all the models which include all possible combinations of the regressors and average them. In our case there are 2^k or up to 8,388,608 models. The weights in the averaging are given by the posterior model probabilities $p(M|y)$ where M is the model and y is the data. In order to compute the posterior model probabilities by means of Bayes rule, two elements are required. First, we need the posterior distribution of the parameters in each model M, which is used to derive the marginal likelihood $p(y|M)$. Second, we need to specify the prior distribution of the models $p(M)$. With marginal likelihood and model prior distributions at hand, the model posterior probabilities can be derived as

$$p(M|y) \propto p(y|M)p(M) \quad (9)$$

As to the setup of the priors, we follow Fernandez, Ley and Steel (2001). In particular, for each model, we compute the posterior probability distribution of the parameters by assuming an uninformative prior on the variance of the residuals and on the intercept. For the remaining regression coefficients we use the g-prior of Zellner (1986), setting $g = \frac{1}{\max(N,k^2)}$ .We set a uniform prior for the distribution of the models.[34] Since we only have up to 8,388,608 models, we follow Magnus, Powel and Pruefer (2010) and evaluate each one of them to obtain the exact likelihood, without having to rely on MCMC methods for approximation. High posterior inclusion probabilities indicate that, irrespective of which other explanatory variables are included, the regressor has a strong explanatory power. We argue that this is therefore an efficient and objective way to select the best predictors of the changes in capital requirements.

Table 1B: Summary Statistics for the ΔKR Predictors

Variable	Obs	Mean	Std. Dev.	25th	50th	75th
Regulatory Returns (BSD3 form) data						
Δ Government Investment	125	0.0084	1.0819	0.0000	0.0000	0.0000
Δ MBS Investment	125	0.0194	0.2409	0.0000	0.0000	0.0000
Δ Writeoffs	125	-0.0476	0.5288	-0.1611	-	0.0000
Δ Provisions	125	0.0677	0.4254	-0.0345	0.0102	0.0762
Δ Profits & Losses on FX	125	0.0014	0.0887	0.0000	0.0000	0.0000
Δ Profits & Losses on Investment	125	0.0152	0.1089	0.0000	0.0000	0.0000
Δ Total Profits & Losses	125	-0.0497	0.9741	0.0000	0.0000	0.2897
Δ Counterparty Risk	125	-0.0019	0.0680	0.0000	0.0000	0.0000
Δ Total FX Risk	125	-0.0007	0.0136	0.0000	0.0000	0.0000
Δ Total Interest Rate Risk	125	-0.0011	0.1004	0.0000	0.0000	0.0000
Δ Total Equity Risk	125	0.0010	0.0186	0.0000	0.0000	0.0000
Δ Total Commodity Risk	125	-0.0008	0.0064	0.0000	0.0000	0.0000
Δ Banking Book Leverage	125	0.0327	1.3134	-0.5096	0.0726	0.6670
Additional Balance Sheet Data						
Δ Financial Lending	125	0.0607	0.9273	-0.0195	0.0000	0.2666
Δ PNFC Lending	125	0.2466	1.1135	-0.0053	0.0048	0.4718
Δ Household Lending	125	0.4422	0.8937	0.0000	0.0000	0.5722
Δ Total Lending	125	2.3734	14.3987	-0.6243	1.9757	7.6196
Δ Wholesale/Retail Sales Lending	125	-0.0148	0.1907	-0.0228	0.0000	0.0048
Δ Construction Lending	125	0.0198	0.1006	0.0000	0.0000	0.0192
Δ Manufacturing Lending	125	0.0203	0.2871	-0.0025	0.0000	0.0192
Δ Commercial Real Estate	125	0.0915	0.5076	-0.0041	0.0000	0.2380
Δ Liquid Assets	125	-0.1340	5.9648	-2.4044	-0.0474	1.5102
Δ Corefunding	125	0.8735	3.2828	-0.1999	0.1510	1.6050

Note: Δ in regulatory returns (balance Sheet) data are scaled by lagged risk-weighted (total) assets.

[34] In practical terms, Bayesian Model Averaging is implemented with the STATA BMA function documented in De Luca and Magnus (2011).

Table 2B: Models for selecting DBBKR predictors

Variable	Single Regression		Multiple		BMA		
DKR	Coeffcient	T-	Coeffcient	T-	Coeffcient	T-	PIP
Δ Government	-0.180	-.68	-0.055	-.55	-0.010	-.24	0.09
Δ MBS Investment	-0.105	-.22	0.188	0.4	0.001	0.01	0.04
Δ Writeoffs	0.543	2.51	0.093	0.44	0.014	0.19	0.07
Δ Provisions	0.939	3.58	0.208	0.66	0.093	0.37	0.16
Δ Profits & Losses on	-0.987	-.75	1.722	1.21	0.016	0.05	0.05
Δ Profits & Losses on	-1.083	-.01	0.875	0.78	0.067	0.19	0.07
Δ Total Profits & Losses	-0.456	-.04	-0.316	-2.3	-0.347	-.23	0.9
Δ Counterparty Risk	-1.944	-.14	-3.389	-.95	-0.811	-.52	0.27
Δ Total FX Risk	24.170	2.91	14.658	1.45	0.804	0.21	0.08
Δ Total Interest Rate	2.080	1.81	3.251	2.47	3.646	2.76	0.95
Δ Total Equity Risk	-2.254	-0.36	-14.920	-1.93	-2.759	-0.48	0.24
Δ Total Commodity	19.97	1.09	-8.400	-0.46	0.006	0	0.04
Δ Banking Book	-0.000125	-0.14	0.000	0.2	0.000	-0.11	0.05
Δ Financial Lending	-0.276	-2.23	-0.002	-0.02	-0.007	-0.17	0.06
Δ PNFC Lending	-0.119	-1.13	0.052	0.31	0.003	0.1	0.06
Δ Household Lending	-0.228	-1.77	-0.091	-0.73	-0.010	-0.21	0.08
Δ Total Lending	-0.031	-4.06	-0.043	-4.01	-0.039	-4.5	1
Δ Wholesale/Retail	-0.564	-0.92	-0.689	-1.11	-0.025	-0.14	0.06
Δ Construction Lending	1.378	1.19	2.987	2.49	0.975	0.74	0.42
Δ Manufacturing	-0.131	-0.32	0.084	0.19	0.012	0.12	0.05
Δ Commercial Real	-0.315	-1.38	-0.143	-0.53	-0.002	-0.05	0.05
Δ Liquid Assets	0.0199	1.02	0.021	1.05	0.001	0.2	0.07
Δ Corefunding	-0.0482	-1.36	-0.011	-0.32	0.000	-0.05	0.04

Table 3B: Regressions to Remove Balance sheet risk DKR Changes

	(1)	(2)	(3)	(4)
Δ Total Profits & Losses	-0.456***	-0.455***	-0.397***	-0.292***
	(0.113)	(0.112)	(0.103)	(0.102)
Δ Total Interest Rate Risk		2.059*	3.875***	2.921***
		(1.083)	(1.051)	(1.034)
Δ Total Lending			-0.0373***	-0.0331***
			(0.00737)	(0.00711)
Δ Capital Buffer				-0.158***
				(0.0431)
Observations	125	125	125	125
R squared	0.117	0.143	0.293	0.364

This table presents the results for predicting 125 changes in capital requirements with risk and lending variables. Movements in capital requirements that are not explained by these variables, i.e. the residuals from these regressions, are attributed to non-balance sheet risks. Specification (3) is implied by the model selection procedure in table XX. We refer to the residuals from this specification as DKR_exog one. The change in the capital buffer can also explain a significant variation of the R2 (7%), but this could because the variables might be correlated by construction. As a result we include this variable in column 4 separately and refer to the residual from column (4) as DKR_exog2 for the rest of this paper.

APPENDIX C - MONETARY POLICY MEASURE

In this appendix we describe our proposed measure of monetary policy surprises. Ideally, we would use an established measure of monetary policy shocks for the UK, similar to the measure proposed by Romer and Romer (2004) for the US.[35] Obtaining an equivalent measure for the UK is not straightforward, given the presence of seven different monetary policy regimes between 1972 to 1997.[36] After 1997, the Bank of England gained operational independence to choose policy to implement the inflation target set by the government. During the time period of relevance here, 1998-2007, the Bank of England used Bank rate, the overnight rate on sterling denominated central bank reserve assets, as it main policy instrument. Following the remit set out in the Bank of England Act, the Monetary Policy Committee's main objective was to keep inflation on at target at the 2-year horizon, subject to minimising volatility in output. Bank rate was therefore set to ensure that the inflation forecast returns to the target within 2 years. Indeed, this is one explanation for why fitting a simple Taylor rule to Bank Rate during this time period does not yield statistically significant coefficients (see column (1) Table 2C). On the other hand, regressing Bank rate on the official 2-year inflation and real GDP forecasts yields coefficients of approximately 1.5 and 1, which is consistent with the Taylor principle.

$$i_t = \rho + \varphi_\pi \pi_t^{BOE\_f} + \varphi_y y_t^{BOE\_f} + \varepsilon_t$$

Ideally, we would like a measure of monetary policy surprises, since economic agents may have already reacted in anticipation of the Bank of England's inflation and GDP forecasts, $\pi_t^{BOE\_f}$ and $y_t^{BOE\_f}$, announcement. To isolate the surprise, and unanticipated, component of $\pi_t^{BOE\_f}$ and $y_t^{BOE\_f}$, it is therefore necessary to measure agents's expectation of $\pi_t^{BOE\_f}$ and $y_t^{BOE\_f}$, $E_{t-1}\pi_t^{BOE\_f}$ and $E_{t-1}y_t^{BOE\_f}$. While measuring expectations is not easy, the Bank of England surveys professional forecasters, regarding their inflation ($\pi_t^{PROF\_f}$) and real GDP ($y_t^{PROF\_f}$) forecasts, the week before the Bank's official forecast is announced. We use the mean of these professional forecasters forecast as our measure of agents' expectation of the MPC's forecast. Subtracting this actual announced forecast from this expectation yields inflation and real GDP growth forecast surprises. Ie. $\pi_t^{SUR\_f} = \pi_t^{BOE\_f} - E_{t-1}\pi_t^{BOE\_f}$ and $y_t^{SUR\_f} = y_t^{BOE\_f} - E_{t-1}y_t^{BOE\_f}$. We then construct the interest rate surprise measure, $i_t^{SUR\_f}$, based on the following equation:

$$i_t^{SUR\_f} = \varphi_{i_{t-1}} i_{t-1}^{SUR\_f} + (1 - \varphi_{i_{t-1}})(\varphi_\pi \pi_t^{SUR_f} + \varphi_y y_t^{SUR_f})$$

For our first measure of the interest rate surprise, we set $\varphi_{i_{t-1}}$ to zero and φ_π to 1.5 and φ_y to 1. This approach has several advantages. First, since surprises are unanticipated by definition, we should only observe a reaction after the monetary policy surprise has been realised. But there is always a risk that this surprise and out LHS side variable are driven by third, common, macroeconomic factors. However, so long these common factors affect the anticipation and the forecast itself roughly to the same extent, they will be subtracted out and this will not be a problem. Finally, the monetary policy committee does not react to the asset growth of individual PNFC firms, which means that we can rule out reverse causality as well. In summary, we have

[35] Cloyne and Huertgen (2014) employ this type of approach following Romer and Romer (2004) for the UK, although for a different sample period and focusing on the macroeconomic effects of monetary policy. At the time of writing this series was not yet ready to be shared publicly.

[36] Nelson (2001) that documents all UK monetary policy regimes during this period.

derived a measure that is unanticipated, and is probably not subject to omitted variable bias from common factors and or reverse causality. As a result it is therefore a valid instrument to examine the impact of monetary policy surprises on the asset growth rate of individual firms through the bank relationship channel. Finally, figure 1C shows our measure together with bank rate. This suggests that the two series co-move especially during tightening and loosening cycles, when surprises are likely to be larger, with an overall correlation coefficient of .53.

To ensure that our results are robust to different Taylor rule coefficients we also constructed this monetary policy series with alternative Taylor rule coefficients, all of which are shown in table 1C. The resulting time series, along the change in bank rate, are shown in Figure 8. A popular modification of the Taylor rule is the introduction of a lagged interested rate term for allow for persistence in interest rates. We therefore add a lagged interest rate term to the baseline model with persistence of a half, which we refer to as 'alternative time series 4' for the rest of this paper. The resulting five different measures of monetary policy are all displayed in Figure 8. They are all clearly highly correlated with the proposed baseline measure. Only the 'alternative Taylor rule 3' and 'alternative Taylor rule 4' are markedly different from each other. We therefore use the baseline as our baseline measure of monetary policy, with 'alternative Taylor rule 3, and 'alternative Taylor 4' as two additional robustness checks

While no monetary policy measure is likely to be perfect, given the properties of our measure and the co-movement with bank rate during tightening and loosening cycles, we believe that our measure does indeed reflect monetary policy surprises and it hence suitable for our purposes.

Figure 1C: UK Bank rate and Interest rate surprise

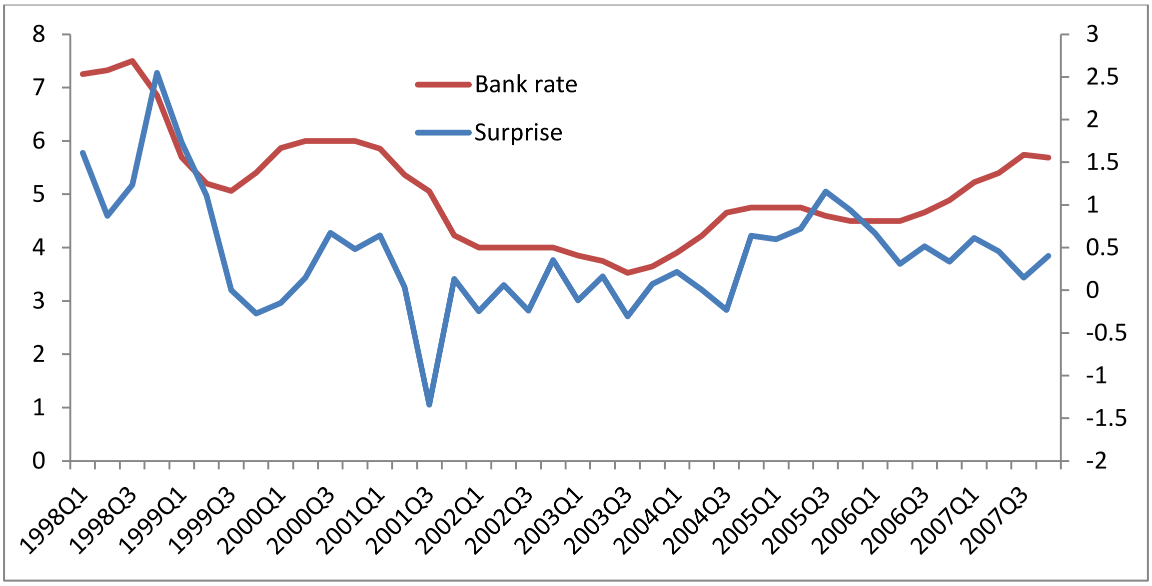


Table 1C

Taylor Rule Coefficients	Base -line	Alternative Rule 1	Alternative Rule 2	Alternative Rule 3	Alternative Rule 4
φ_{π}	1.5	2	2.5	2.5	1.5
φ_{y}	1	1	1	2	1
$\varphi_{i_{t-1}}$	-	-	-	-	0.5

Table 2C

VARIABLES	bankrate	bankrate	bankrate
CPI Inflation	-0.127		
	(0.338)		
Real GDP Growth	-0.233		
	(0.463)		
Annual CPI Inflation		-0.172	
		(0.257)	
Annual Real GDP Growth		-0.131	
		(0.160)	
2-Year Inflation Forecast			1.447***
			(0.459)
2-Year Real GDP Growth Forecast			0.987**
			(0.427)
Constant	5.277***	5.527***	-0.955
	(0.419)	(0.714)	(1.715)
Observations	40	37	40
R-squared	0.009	0.025	0.258

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

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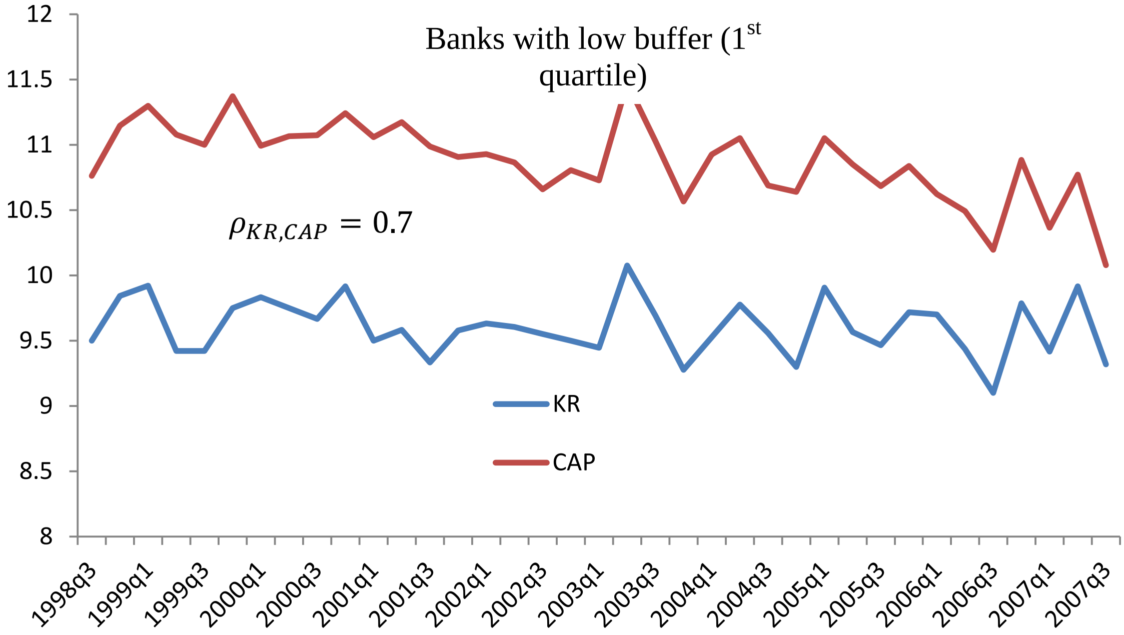
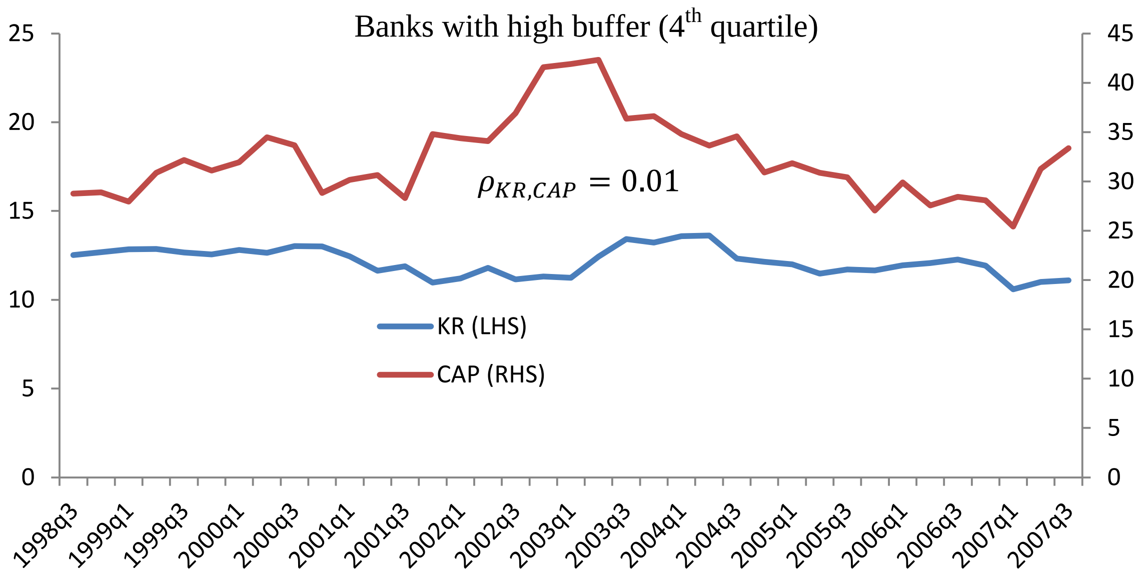
FIGURES

Figure 1: Capital Requirements and Capital Ratios by Bank Buffer





This figure contains the average capital requirement (KR) and capital ratio (TotCapital/RWA) for banks with low buffer (1st quartile) in the upper panel and high buffer (4th quartile) in the lower panel. Note: in the lower panel the capital ratio (red line) is on the right-hand-side scale. Source: regulatory forms (BSD3) at a quarterly frequency

Figure 2: Number of Capital Requirement Changes

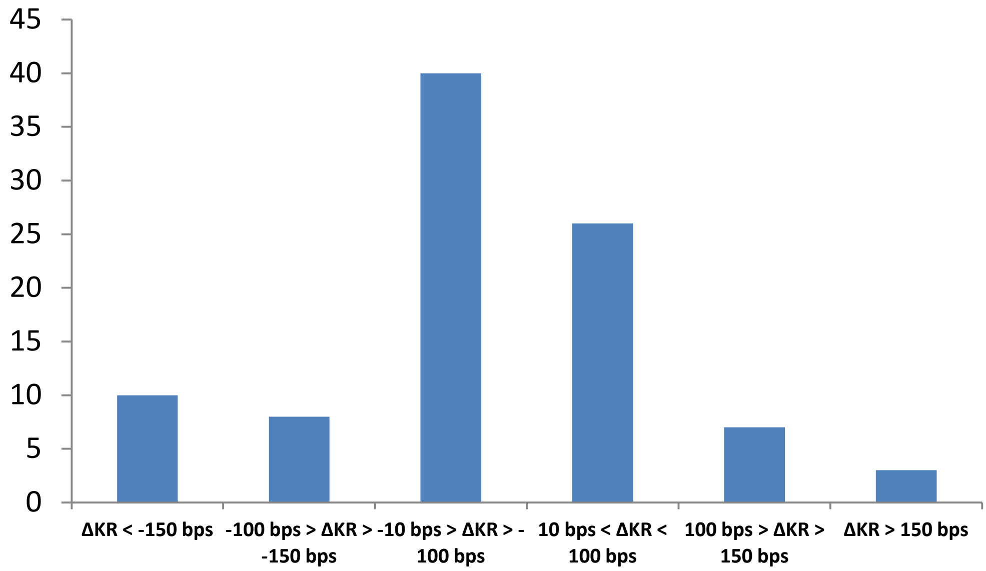


Figure 3: Number of Banks by Firm

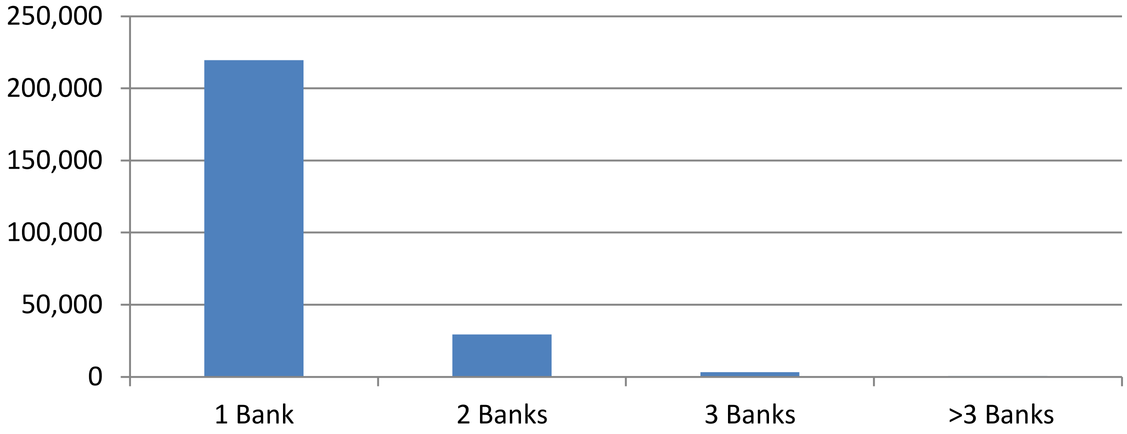


Figure 4: Market Shares by Bank

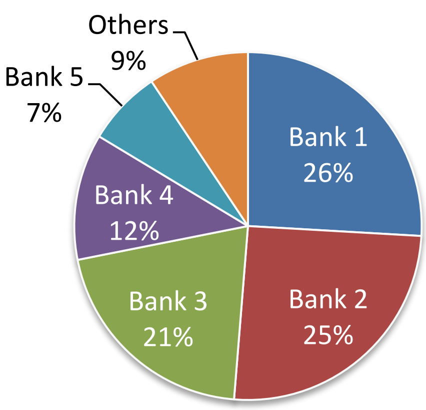


Figure 5: Number and Percentage of Firms by Sector

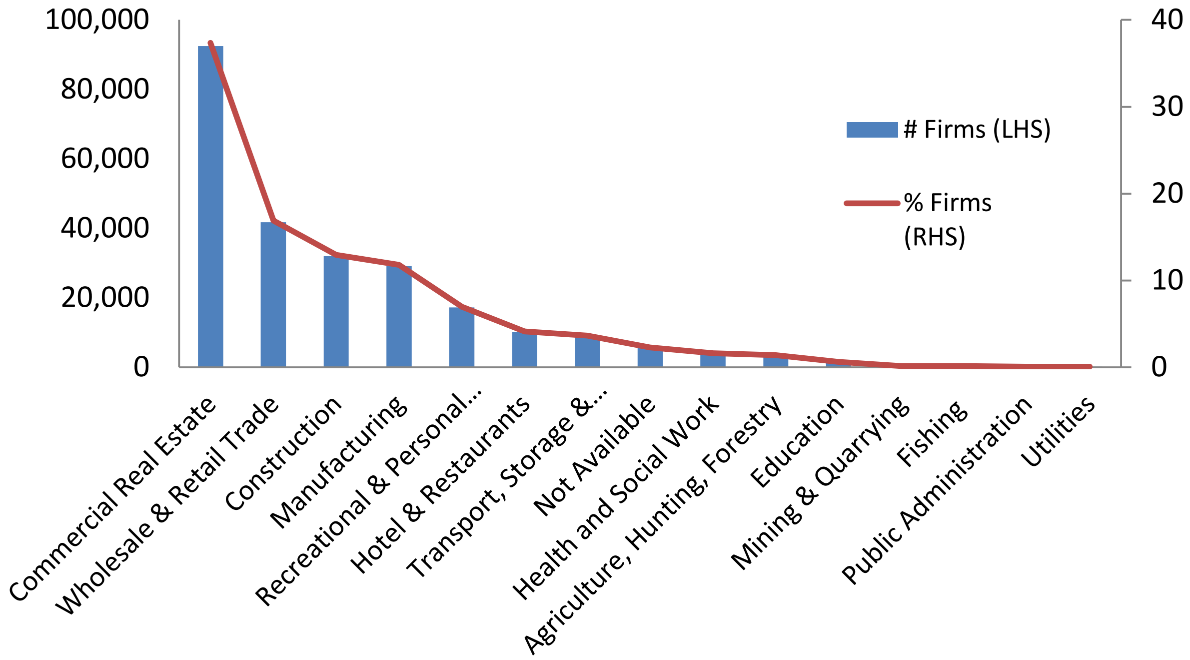
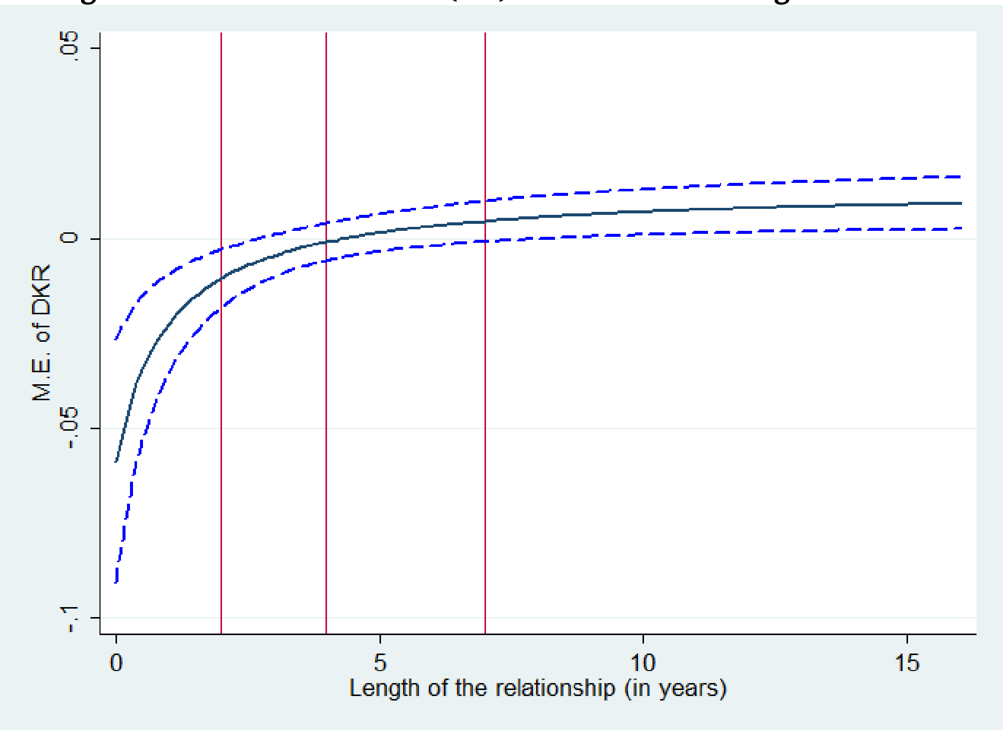
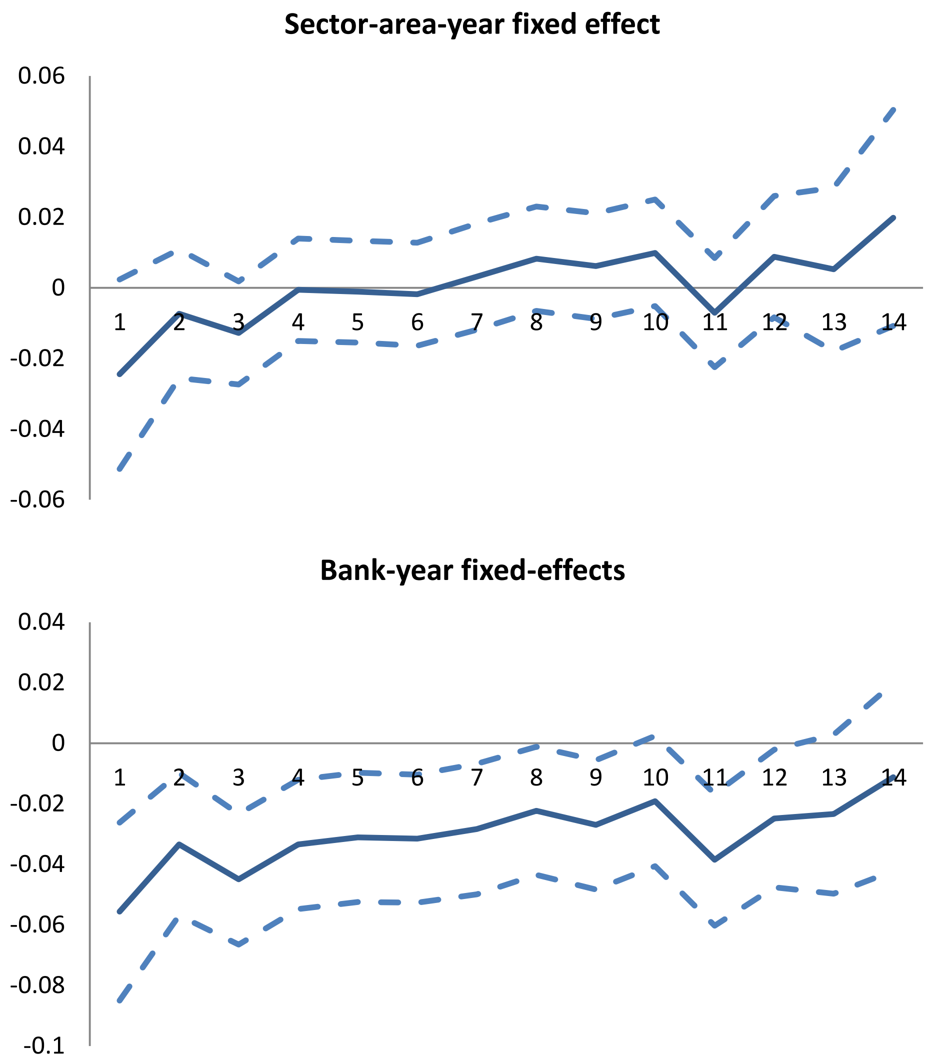


Figure 6: Marginal Effect of ΔKR on $\Delta ln(TA)$ as a function of length



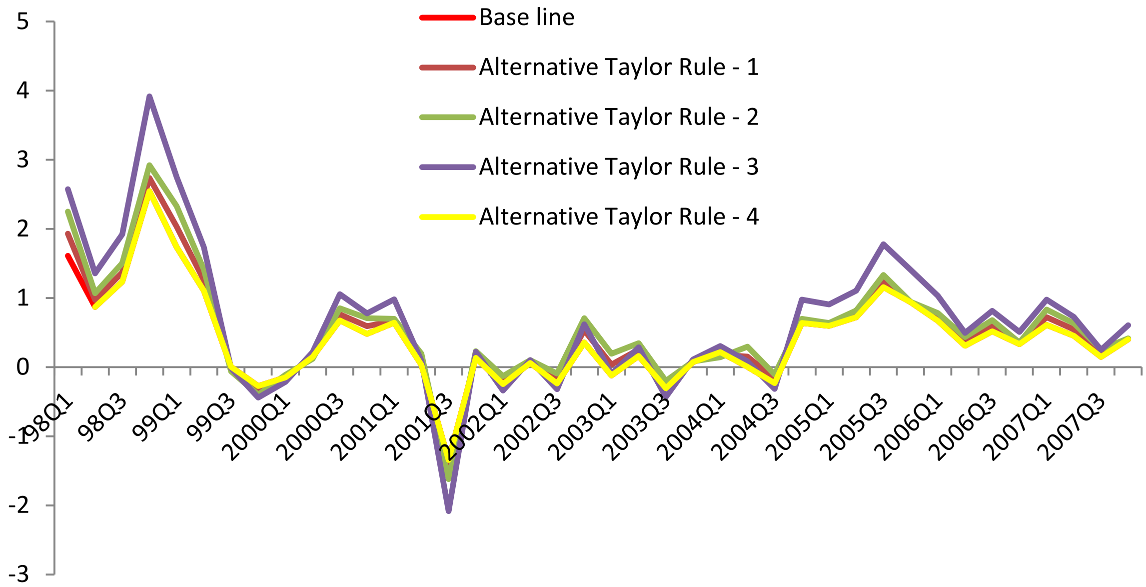
This figure contains the plot of the marginal effect of a change in capital requirement on the growth rate of assets for the baseline specification including lagged firm controls. The partial derivative is a function of length and the vertical red lines represent the 25th (2years), median (4) and 75th (7) percentiles of the distribution of the firm-bank relationship length in years. Dashed lines show the 95% confidence interval around the point estimate.

Figure 7: Non-parametric regression



This figure plots the coefficients from a non-parametric regression of a change in capital requirement interacted with 15 dummies, one for each year of length, on firms' growth rate of assets. The upper panel contains 4digitsect*area*year fixed effect, while the lower panel includes also bank*year effects. The dashed lines show the 95% confidence interval around the point estimate.

Figure 8: Monetary Policy (Taylor Rule) and Alternatives



This figure plots the estimated Taylor rule for the UK economy: a linear combination of the surprises to CPI inflation and GDP forecast using the estimated Taylor coefficients of 1.5 and 1 for the baseline. Alternative Taylor rules with different coefficients are specified in Table 1C

TABLES

Table 1: Variable Description

Variable	Definition	Notes
Firm and bank-firm relationship data (BvD FAME)		
Δln(TA)	Growth rate of Total Assets	
CredScore	Credit Score (QuiScore)	From 0 (worst credit score) to 100 (best). Calculated by CRIF Decision Solutions Ltd., see Table 1a for details.
Currliab/TA	Current Liabilities over Total Assets ratio	Current liabilities include: Short Term Loans & Overdraft, Trade Creditors and Taxes & Dividends
Age	Years since date of incorporation	
Length	Years since creation of the loan	
BvD Independence Indicator	Description of ownership structure	From A (no shareholder with more than 25% ownership) to D (one shareholder with >=50%)
Turnover	Turnover (Sales) in £ mil.	
Employees	Number of Employees	
Bank level data (BSD3 and QFS forms)		
KR_t	Minimum ratio for capital-to-risk weighted assets (RWA) for the banking book.	The BSD3 form provides this information for Banks. QFS provides it for Building societies.
ΔKR_t	Yearly change in KR	
$\Delta Woff_t$	Yearly change in writeoffs rate: nbpa550t/nhd510	nbpa550t – Loan writeoffs nhd510 – Total Risk Weighted Assets
Bank Size	Natural log of (BT40)	BT40 – Total Assets
Bank Liquidity	Liquid to Total Assets ratio: (BT21+ BT32D)/BT40	BT21 - Cash ; BT32D – Holdings of Government Stock
Core Funding	Deposits to Total Assets ratio: (BT2H +BT3H)/BT40	BT2H – Retail Sight Deposits, BT3H – Retail Time Deposits
Buffer	Actual Capital Ratio (nhd40/nhd510) - KR	nhd40 – Total Eligible Capital, nhd510 - RWA
Sector level data on external finance		
BLreg	Regulated Lending	Lending by UK resident banks and UK subsidiaries of foreign banks
BLnonreg	Non-regulated Lending	Lending by UK branches of foreign banks
Nonbank	Non bank external finance	Sum of equity and corporate bonds outstanding

Table 2: Summary Statistics

Variable	Observations	Mean	Std.	25th	50th	75th
Firm and bank-firm relationship data (firm-year panel BvD FAME)						
Total Assets(£mil.)	1,146,711	2.12	53.75	0.137	0.351	0.914
Δln(TA)	967,551	0.076	0.406	-0.072	0.029	0.204
Turnover (£mil.)	285,434	4.54	13.42	0.143	0.522	2.431
Employees (#heads)	125,453	146.5	1049.6	11	44	103
CredScore	1,107,154	50.2	22.60	35	48	64
Currliab/TA	1,139,230	0.63	0.553	0.307	0.547	0.809
Age	1,413,935	12.57	13.68	4	8	16
Length	1413944	4.82	3.922	2	4	7
Bank level data (bank-year panel)						
KR_t (%)	520	11.55	3.07	9	10	13
ΔKR_t (bps.)	520	-4.1	57.2	0	0	0
$\Delta Woff_t$ (bps.)	516	1.655	97.5	-3.972	0	6.365
Size (in £mil.)	520	32,590	89,367	710	4,104	15,100
Liquidity (bps)	520	161	271	1	43	220
Core Funding (%)	520	51.04	30.79	21.92	53.52	79.57
Buffer (%)	517	16.58	33.32	3.1	6.8	14.4
External Finance Dependence data (sector-year panel)						
BLnonreg/BLreg	126	0.52	0.65	0.04	0.26	1.18
Nonbank/BLreg	126	1.29	2.7	0.006	0.2	4.4

Table 3 – Capital Requirements and Firms' Growth Rate of Assets

	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta KR_{i,t}$	0.008	-0.0252***	0.008***	0.007**	0.009**	0.013***
	(0.065)	(0.008)	(0.00266)	(0.00368)	(0.00367)	(0.00437)
$\ln(1 + L_{i,t})$		-0.151***				
		(0.004)				
$\Delta KR_{i,t} \times \ln(1 + L_{i,t})$		0.013***				
		(0.004)				
$1/(1 + L_{i,t})$			0.425***	0.425***	0.412***	0.459***
			(0.00411)	(0.00571)	(0.00590)	(0.0113)
$\Delta KR_{i,t} \times 1/(1 + L_{i,t})$			-0.0340***	-0.0352***	-0.0369***	-0.0721***
			(0.00946)	(0.0130)	(0.0131)	(0.0197)
$CredScore_{i,t}$					0.001***	
					(5e-5)	
$Currliab/TA_{i,t}$					-0.154***	
					(0.004)	
$CredScore_{i,t-1}$						0.001***
						(4.6e-5)
$Currliab/TA_{i,t-1}$						0.130***
						(0.005)
Young					0.03***	0.018***
					(0.003)	(0.002)
Old					0.007**	0.003
					(0.003)	(0.003)
Firm Fixed Effects	X	X	X	X	X	X
Year Fixed Effects			X			
SIC4dig×Postcode ×Year Fixed Effects	X	X		X	X	X
Observations			969,052	968,012	927,310	871,423
N of firms			212,894	212,894	208,316	196,980
R squared			0.338	0.444	0.462	0.445

This table presents the results for the baseline regression. The dependent variable is the log difference of total assets by firm *i* between time *t* and *t-1*. All specifications include firm fixed-effects and either year fixed effects (column (1)) or SIC4dig*postcodearea*year fixed-effects (columns (2)-(5)). $\Delta KR_{i,t}$ is the change in capital requirements between year *t* and year *t-1* for the bank lending to firm *i* (it is averaged over all banks lending to firm *i* at time *t* in case of multiple banks). $L_{i,t}$ is the length of the relationship between firm *i* and its banks, measured in years since the creation of the loan. $CredScore_{i,t}$ is the Credit Score (QuiScore) of the firm, measured on a scale of 0 (worst risk) to 100 (no risk). $Currliab/TA_{i,t}$ is the ratio of Current Liabilities (short term debt, trade credit, taxes and dividends) over total assets. $Young, Old$ are dummies for the age of the firm, at the 25th and 75th percentiles (4 and 16 years old respectively). Finally, $\Delta \ln(TA)_{i,t-1}$ is the lagged growth rate of total assets (lagged dependent variable). For statistical significance, we use the following convention throughout: *** p<0.01, ** p<0.05, * p<0.1. Firm-clustered standard errors are reported in parenthesis.

Table 4: Robustness of Baseline Results

Robustness Exercise	$\Delta KR_{i,t}$	$1/(1+L_{i,t})$	$\Delta KR_{i,t} \times 1/(1+L_{i,t})$
Alternative Dependent Variables			
(1) Investment ($\Delta Tang/TA_{t-1}$)	0.0227*	1.056***	-0.120**
(2) Current Liabilities (*Δlog(Currliab)*)	0.0162**	0.408***	-0.0797***
Econometric Assumptions			
(3) Replace $L_{i,t}$ by firm age	0.00415	0.739***	-0.0430
(4) Dependent variable winsorised at 5%&95%	0.007**	0.321***	-0.038***
(5) Time-invariant capital requirement change		0.478***	-0.0715***
(6) Only loans with maturity date (obs=75,844)	0.0167	0.311***	-0.105**
Sample Selection – Firms			
(7) Exclude small firms (size in bottom 50%)	0.015***	0.502***	-0.093***
(8) Exclude small firms (size in bottom 75%)	0.019**	0.504***	-0.116***
(9) Exclude firms with < 50% single ownership	0.0146**	0.436***	-0.0654**
(10) Exclude firms with > 50% single ownership	0.0151**	0.500***	-0.0835**
Sample Selection – Banks			
(11) Excluding Big Bank1	0.013***	0.469***	-0.07***
(12) Excluding Big Bank2	0.014***	0.461***	-0.071***
(13) Excluding Big Bank3	0.017***	0.474***	-0.098***
(14) Excluding Big Bank4	0.015***	0.468***	-0.086***
(15) Excluding Big Bank5	0.013***	0.457***	-0.077***
(16) Excluding Big Bank6	0.012***	0.445***	-0.072***
(17) Excluding all Big banks together	0.025***	0.514***	-0.123***
(18) Including Big banks only	0.01*	0.45***	-0.055**
(19) Excluding Big banks (Bank – year effect)	-0.01	0.55***	-0.0793**
(20) Including Big banks only (Bank – year effect)	-0.021*	0.452***	-0.041**
Endogeneity - Omitted Variable Bias			
(21) Include Bank level control variables	0.0138***	0.462***	-0.074***
(22) Include Bank-year effects	-0.0185*	0.471***	-0.0508***

(23) Include Bank-Sector-year effects	-0.0191*	0.465***	-0.0586***
(24) Include Bank-Area-year effects	-0.0144	0.467***	-0.0438***
(25) Include Bank-Sector-Area-year effects	-0.0320**	0.493***	-0.0590**
Endogeneity – ΔKR *Exogeneity Assumption*			
(26) ΔKR_EXOG1	0.00964**	0.462***	-0.0550***
(27) ΔKR _EXOG2	0.0132**	0.465***	-0.0772***
(28) ΔKR _EXOG1 with bank year effects	0.000694	0.467***	-0.0426***
(29) ΔKR _EXOG2 with bank year effects	0.00426	0.469***	-0.0605***
(30) ΔKR _EXOG1 with bank sector year effects	0.0022	0.463***	-0.0433***
(31) ΔKR _EXOG2 with bank sector year effects	0.00513	0.465***	-0.0587***
(32) ΔKR _EXOG1 with bank area year effects	0.00495	0.465***	-0.0366**
(33) ΔKR _EXOG2 with bank area year effects	0.00942	0.467***	-0.0527***
(34) ΔKR _EXOG1 bank area sector year effects	-0.00688	0.489***	-0.0473*
(35) ΔKR _EXOG2 bank area sector year effects	-0.00432	0.491***	-0.0629*

The dependent variable is the log difference of total assets by firm i between time t and t-1. All specifications include firm fixed-effects and SIC4dig*postcodearea*year fixed-effects, unless otherwise stated. Lagged firm variables, not shown are: $CredScore_{i,t-1}$, $Currliab/TA_{i,t-1}$ and $young, old$ Standard errors are clustered by firm

Table 5: Bank and Firm Characteristics

Linear Bank Interaction	$\Delta KR_{i,t}$	$1/(1+L_{i,t})$	$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	$\Delta KR_{i,t} \times Variable$	$\Delta KR_{i,t} \times 1/(1+L_{i,t}) \times Variable$
(1) Core Funding Ratio	0.0159	0.402***	-0.0744	-0.0000345	0.0000433
(2) Liquidity Ratio	0.0263**	0.494***	-0.140***	-0.00817	0.0417*
(3) Leverage	0.0113	0.510***	-0.04	0.000151	-0.00225
(4) Capital Buffer	0.0253***	0.474***	-0.132***	0.00277**	.01417**
(5) Banking Book in Total	0.00232	0.0485	0.0565	0.0126	-0.139
Non-Linear (quartile dummy) Bank Interaction	$\Delta KR_{i,t}$	$1/(1+L_{i,t})$	$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	$\partial\Delta\ln(TA)/\partial KR$ For High Value	$\partial\Delta\ln(TA)/\partial KR$ For Low Value
(6) Core Funding Ratio	0.0108*	0.447***	-0.0459	-.064***	-.25
(7) Liquidity Ratio	0.0201***	0.470***	-0.0731**	-.033	-.34
(8) Leverage	0.0107**	0.463***	-0.0549**	-.104***	-.53*
(9) Capital Buffer	0.00955**	0.466***	-0.0536**	-.046	-.19***
(10) Banking Book in Total	0.00539	0.513***	-0.0339	-.32***	-.026
(11) Big Bank –Sec*area*year	0.0241***	0.502***	-0.124***	-.038**	-.10***
(12) Big Bank – Bank*year	-0.00993	0.528***	-0.0883**	-.06***	-.098***
Non-Linear (quartile dummy) Firm Interaction					
(13) Age	0.013***	0.469***	-0.07***	-0.03	-0.05
(14) Credit Risk	0.014***	0.461***	-0.071***	-0.063**	-0.045*
(15) Leverage	0.017***	0.474***	-0.098***	-0.05	-0.056*
Estimate Model by Sector					
(16) Commercial Real Estate	0.0204**	0.575***	-0.116***		
(17) Wholesale	0.00309	0.343***	-0.0163		
(18) Manufacturing	0.000795	0.263***	-0.0314		
(19) Retail	0.019*	0.434***	-0.069		
(20) Other	0.0152**	0.427***	-0.0571*		

Table 6: Monetary Policy (MP Measure -1)

	$1/(1+L_{i,t})$	$MP_t \times 1/(1+L_{i,t})$
Baseline regression		
(1) With sector*area*year effects	0.467***	-0.062***
(2) With bank year effects	0.478***	-0.039***
Split Sample by Bank size		
(3) Big Banks – Sector*area*year effects	0.447***	-0.0378**
(4) Small Banks – Sector*area*year effects	0.534***	-0.146***
(5) Big Banks – Bank*year effects	0.453***	-0.0204
(6) Small Banks – Bank*year effects	0.563***	-0.103***

Non-Linear (quartile dummy) Interaction	$1/(1+L_{i,t})$	$\Delta MP_t \times 1/(1+L_{i,t})$	$\partial\Delta\ln(TA)/\partial MP$ For High Value	$\partial\Delta\ln(TA)/\partial MP$ For Low Value
(7) Credit Risk – Sec*area*year	0.425***	0.00573	-0.159***	-0.0444**
(8) Credit Risk – Bank*year	0.438***	-0.0490***	-0.0833***	-0.0329*
(9) Big Bank – Sec*area*year	0.516***	-0.115***	-.065***	-0.115***
(10) Big Bank – Bank*year	0.545***	-0.104***	-.035**	-0.104***

Split Sample by Sector	$1/(1+L_{i,t})$	$MP_t \times 1/(1+L_{i,t})$
(11) Commercial Real Estate – Sec*area*year effects	0.588***	-0.116***
(12) Wholesale – Sec*area*year effects	0.364***	-0.0473*
(13) Manufacturing – Sec*area*year effects	0.287***	-0.0619*
(14) Retail – Sec*area*year effects	0.437***	-0.0322
(15) Other – Sec*area*year effects	0.439***	-0.0663**
(16) Commercial Real Estate – Bank*year effects	0.615***	-0.0627**
(17) Wholesale – Bank*year effects	0.363***	-0.0312
(18) Manufacturing – Bank*year effects	0.293***	-0.039
(19) Retail – Bank*year effects	0.455***	-0.00181
(20) Other – Bank*year effects	0.453***	-0.0632**

Table 7 – The Interaction with Monetary Policy – MP Measure 1

	(1)	(2)	(3)	(4)	(5)
$\Delta KR_{i,t}$	0.0112**	0.00800**	-0.0241**	0.00908	0.0190*
	(0.00438)	(0.00363)	(0.00993)	(0.0044	(0.0103)
$1/(1+L_{i,t})$	0.466***	0.473***	0.476***	0.469***	0.495***
	(0.0115)	(0.00972)	(0.00969)	(0.0115	(0.0169)
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	-0.0608***	-0.0405**	-0.0394**	-.051**	-0.102**
	(0.0198)	(0.0171)	(0.0173)	(0.0205	(0.0464)
$MPshock_t \times 1/(1+L_{i,t})$	-0.0689***	-0.0593***	-0.0444***	-.067***	0.300*
	(0.0176)	(0.0141)	(0.0145)	(0.0175	(0.163)
$\Delta KR_{i,t} \times MPshock_t$		0.0562***	0.110***	0.0422*	
		(0.0157)	(0.0419)	(0.0196	
$\Delta KR_{i,t} \times MPshock_t \times 1/(1+L_{i,t})$		-0.153**	-0.0688	-0.153*	
		(0.0686)	(0.0691)	(0.0822	
$\Delta KR_{i,t} \times TIGHT_t$					-0.0529**
					(0.0221)
$1/(1+L_{i,t}) \times TIGHT_t$					-0.0310
					(0.0189)
$\Delta KR_{i,t} \times 1/(1+L_{i,t}) \times TIGHT_t$					0.213**
					(0.0949)
$MPshock_t \times 1/(1+L_{i,t}) \times TIGHT_t$					-0.359**
					(0.164)
$\Delta KR_{i,t} \times MPshock_t \times TIGHT_t$					0.0145
					(0.124)
$\Delta KR_{i,t} \times MPshock_t \times 1/(1+L_{i,t})$					0.0576
$\times TIGHT_t$					(0.548)
Lagged Firm Controls	X	X	X	X	X
Year Effects		X			
Bank Year Effects			X		
SIC4dig×Postcode ×Year Effects	X			X	X
Observations	870,168	871,131	871,131	871,131	870,168
N of firms	196,980	196,980	196,980	196,980	196,980
R-Squared	0.445	0.334	0.334	0.334	0.445

This table presents the results for the interaction with monetary policy. The dependent variable is the log difference of total assets by firm i between time t and t-1. All specifications include firm fixed-effects and the SIC4dig*postcode-area*year fixed-effects (the difference with the year fixed-effects is minimal and we do not report it). All specifications include the following lagged firm variables, not shown: $CredScore_{i,t-1}$, $Currliab/TA_{i,t-1}$ and $young, old$. All specifications are fully saturated, but the some double interactions coefficients are not shown. $MPshock_t$ is a monetary policy shock constructed from a Taylor rule fitted on UK data (see Appendix C for details). $TIGHT_t$ is a dummy variable that takes value one if the monetary policy surprise is positive and zero otherwise. Given that $MPshock_t,, TIGHT_t$ are aggregate time shocks, they are absorbed by the time fixed-effects, so their coefficients are not shown in the regression. Firm-clustered standard errors are reported in parenthesis.

Table 8 – The Interaction with Monetary Policy – Large and Small Banks

	Sector Area Time Effects			Bank Year Effects		
	All	Small Banks	Big Banks	All	Small Banks	Big Banks
$\Delta KR_{i,t}$	0.00819*	-0.0162	0.00979**	-.0268**	-0.0756**	-0.0234*
	(0.00446)	(0.0197)	(0.00472)	(0.0115)	(0.0315)	(0.0125)
$1/(1+L_{i,t})$	0.474***	0.561***	0.450***	0.480***	0.587***	0.452***
	(0.0108)	(0.0253)	(0.0124)	(0.0105)	(0.0230)	(0.0120)
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	-0.0475**	0.0552	-.0607***	-.0447**	0.0850	-.0569***
	(0.0207)	(0.0782)	(0.0220)	(0.0202)	(0.0729)	(0.0211)
$\Delta KR_{i,t} \times TIGHT_t$	0.00581	0.0527**	-0.0149	0.0285	0.0978**	0.0181
	(0.00883)	(0.0237)	(0.0116)	(0.0226)	(0.0419)	(0.0318)
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	-0.0171	-0.221**	0.0925*	0.00289	-0.204**	0.0925*
$\times TIGHT_t$	(0.0386)	(0.0953)	(0.0497)	(0.0374)	(0.0878)	(0.0478)
Partial effects for new borrowers (length=0)						
∂TA/∂DKR	-0.04**	0.03	-0.05***	-0.07***	0.009	-0.08***
∂TA/∂DKR$\vert TIGHT_t = 1$	-0.05**	-0.129***	0.026	-0.04	-0.968**	0.03
Lagged Firm Controls	X	X	X	X	X	X
Bank Year Effects				X	X	X
SIC4dig×Postcode×Year Effects	X	X	X			
Observations	870,168	196,981	673,187	871,131	197,887	673,244
R-squared	0.363	0.444	0.363	0.334	0.376	0.329

This table presents the results for the interaction with monetary policy between large and small banks. The dependent variable is the log difference of total assets by firm *i* between time *t* and *t-1*. All specifications include firm fixed-effects and the SIC4dig*postcodearea*year or bank-year fixed-effects All specifications include the following lagged firm variables, not shown: $CredScore_{i,t-1}$, $Currliab/TA_{i,t-1}$ and $young, old$. All specifications are fully saturated, but the some double and triple interactions coefficients are not shown. $MPshock_t$ is a monetary policy shock constructed from a Taylor rule fitted on UK data (see Appendix C for details). $TIGHT_t$ is a dummy variable that takes value one if the monetary policy surprise is positive and zero otherwise. Given that $MPshock_t$, , $TIGHT_t$ are aggregate time shocks, they are absorbed by the time fixed-effects, so their coefficients are not shown in the regression. Firm-clustered standard errors are reported in parenthesis.

Table 9 – Impact of Capital Requirements by Sectoral Bank Dependence

Dependent Variable: Growth Rate of Assets - Δln(TA)

	(1)	(2)	(3)	(4)
$\Delta KR_{i,t}$	0.019***	0.007**	0.03***	0.015***
	(0.007)	(0.00392)	(0.008)	(0.004)
$1/(1+L_{i,t})$	0.511***	0.429***	0.561***	0.460***
	(0.01)	(0.006)	(0.019)	(0.012)
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	-0.0703***	-0.036***	-0.134***	-0.079***
	(0.0236)	(0.014)	(0.0360)	(0.021)
$BLnonreg/BLreg$	5-e4		0.017***	
	(0.01)		(0.006)	
$\Delta KR_{i,t} \times BLnonreg/BLreg$	-4.38e-4***		-5.34e-4***	
	(1.52e-4)		(1.81e-4)	
$1/(1+L_{i,t}) \times BLnonreg/BLreg$	-0.003***		-0.003***	
	(2.12e-4)		(3.80e-4)	
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	0.001**		0.002**	
$\times BLnonreg/BLreg$	(5.49e-4)		(8.27e-4)	
Nonbank/BLreg		-0.004		0.005*
		(0.004)		(0.003)
$\Delta KR_{i,t} \times Nonbank/BLreg$		-3.46e-5		-5.50e-5
		(3.25e-4)		(3.65e-5)
$1/(1+L_{i,t}) \times Nonbank/BLreg$		-1.96e-4***		-1.89e-4**
		(5.20e-5)		(1e-5)
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$		4.26e-5		2.15e-4
$\times Nonbank/BLreg$		(1.16e-4)		(1.62e-4)
Partial effects for new borrowers (length=0)				
∂TA/∂DKR	-0.0507***	-0.0281**	-0.1039***	-0.0639***
∂TA/∂DKR for a one std.dev.increase in (BLnonreg) or (Nonbank)	-0.0338***	-0.0274**	-0.0721***	-0.0491***
Firm Fixed Effects	X	X	X	X
SIC4dig×Postcode ×Year Effects	X	X	X	X
Lagged Firm Controls			X	X
Observations	954,167	954,167	860,355	860,355
N of firms	209,340	209,340	193,900	193,900
R squared	0.443	0.443	0.443	0.443

This table presents the results for the interaction with alternative sources of finance. The dependent variable is the log difference of total assets by firm i between time t and t-1. All specifications include firm fixed-effects and the SIC4dig*postcode- area*year. Lagged firm controls are: $CredScore_{i,t-1}$, $Currliab/TA_{i,t-1}$ and $young, old$. $BLnonreg/BLreg$ is the ratio between the lending by branches of foreign banks (not FSA regulated) and that by UK resident banks (FSA regulated) at the sector level. $Nonbank/BLreg$ is the ratio between capital market funding (equity+bonds outstanding) over the lending by UK resident banks. Firm-clustered standard errors are reported in parenthesis.

Table 10 – The Impact of Multiple Banks on Capital Requirement and Monetary Policy Transmission

	MP-1	MP-2	MP-3
	(1)	(2)	(3)
$\Delta KR_{i,t}$	0.0126***	0.0126***	0.0136***
	(0.00465)	(0.00465)	(0.00464)
$1/(1+L_{i,t})$	0.436***	0.434***	0.445***
	(0.0122)	(0.0120)	(0.0124)
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	-0.0611***	-0.0609***	-0.0655***
	(0.0215)	(0.0215)	(0.0214)
$MPshock_t \times 1/(1+L_{i,t})$	-0.0457**	-0.0313**	-0.0843***
	(0.0180)	(0.0122)	(0.0155)
$\Delta KR_{i,t} \times bank2dummy$	-0.0163	-0.0162	-0.0150
	(0.0129)	(0.0129)	(0.0129)
$\Delta KR_{i,t} \times 1/(1+L_{i,t})$	0.0400	0.0398	0.0300
$\times\, bank2dummy$	(0.0582)	(0.0582)	(0.0582)
$MPshock_t \times 1/(1+L_{i,t})$	-0.0284	-0.0200	-0.0345*
$\times\, bank2dummy$	(0.0224)	(0.0152)	(0.0193)
Partial effects for new borrowers (length=0)			
$\partial\Delta\ln(TA)/\partial KR$ for 2 Relationship Bank	-.024	-.024	-.035
$\partial\Delta\ln(TA)/\partial MP$ for 2 Relationship Bank	-.075***	-.051***	-.12***
Firm Fixed Effects	X	X	X
SIC4dig×Postcode×Year	X	X	X
Lagged Firm Controls	X	X	X
Observations	849,228	849,228	849,228
N of firms with two banks	26,952	27,319	26,952
R squared	0.448	0.448	0.448

This table presents the results for firms with multiple (two) banks relationship on the effects of capital requirements and monetary policy. The dependent variable is the log difference of total assets by firm i between time t and t-1. All specifications include firm fixed-effects and the SIC4dig*postcode-area*year fixed-effects. $bank2dummy$ is a dummy that takes value one if the firm has two banks and interacts with the change in capital requirement for that bank. Firm controls are: $CredScore_{i,t}$, $Currliab/TA_{i,t}$ and $young, old$.