Journal of Banking and Finance 135 (2022) 105806

Contents lists available at ScienceDirect

Journal of Banking and Finance

journal homepage: www.elsevier.com/locate/jbf

Bank capital requirements and lending in emerging markets: The role of bank characteristics and economic conditions[☆]

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ARTICLE INFO

Article history:
Received 31 July 2019
Accepted 5 March 2020
Available online 20 March 2020

JEL classification:
E44
G21
G28

Keywords:
Bank lending
Bank regulation
Capital requirements
Emerging markets

ABSTRACT

This paper offers novel evidence on the impact of raising bank capital requirements on lending in an emerging market and explores heterogeneous effects, depending on bank characteristics and economic conditions. Using quarterly bank-level data and exploiting the adoption of bank-specific capital buffers, we find that higher capital requirements are associated with lower credit growth in Peru. But the effect is short-lived and becomes insignificant in about half a year. The impact of capital requirements varies with economic conditions and bank characteristics. The effects is stronger during periods of lower economic growth. Weaker (less profitable, less capitalized and less liquid) banks react more to changes in capital requirements. Our findings are robust to estimating a variety of specification to address concerns about the endogeneity of capital requirements.

1. Introduction

The 2008 Global Financial Crisis (GFC) revealed deep weaknesses in banks' balance sheets and triggered a policy response aimed at strengthening bank regulation, supervision and risk management. The new Basel III regulatory framework therefore put forth substantially higher capital requirements, particularly in good times. This change has generated a lively debate on the costs and benefits of bank capital (see Aiyar et al., 2015; Dagher et al., 2020, among others, for an overview). On the one hand, higher capital requirements can improve banks' loss absorbing capacity and mitigate the pro-cyclicality of leverage, thus helping avoid costly financial crises (Admati and Hellwig, 2014). On the other hand, if banks meet the higher requirements by shrinking their assets rather than by raising equity, an increase in capital requirements could affect the availability and cost of bank lending, possibly dampening real economic activity (Kashyap et al., 2010; Hanson et al., 2011). In this context, in 2017, the Financial Stability Board (FSB) launched the "Framework for Post-Implementation Evaluation of the Effects of the G20 Financial Regulatory Reforms" (FSB, 2017), with the aim of examining the economic costs and benefits of the post-GFC financial sector reforms.

A growing empirical literature has been looking at the effects of changes in capital requirements on bank lending.[1] To address the endogeneity of capital regulation, most studies take advantage of quasi natural experiments or heterogeneity in capital requirements across banks. Notwithstanding a relatively similar approach, these studies yield a remarkably wide range of estimates: from 5 to 10% reduction in bank lending for each percentage point increase in

☆ The authors would like to thank the editors (Thorsten Beck and Stijn Claessens), an anonymous referee, Tobias Adrian, Majid Bazarbash, Paul Collazos, Giovanni Dell' Ariccia, Raphael Espinoza, Nigel Jenkinson, Tumer Kapan, Romain Lafarguette, Aditya Narain, Ananthakrishnan Prasad, Daniel Tarullo, and seminar participants at the IMF, the Superintendencia de Banca y Seguros (SBS) and the Banco Central de Reserva del Peru for insightful comments. We are grateful to the SBS for kindly providing data and guidance. We are particularly indebted to Manuel Luy Molinie, Hesione Moreno Benavente, Edmundo Paredes, and Andrea Fernandez Tinoco. The views expressed are those of the author(s) and do not necessarily represent the views of the ECB, IMF, their Executive Boards, or their management.

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[1] This literature is summarized in Section 2.

https://doi.org/10.1016/j.jbankfin.2020.105806
0378-4266/

bank capital ratio (Aiyar et al., 2014; Fraisse et al., 2019; Mésonnier and Monks, 2015) to little if any effect (Acharya et al., 2018; Cortes et al., 2019; Gropp et al., 2019).

What may explain this divergence of empirical results? Theory suggests that the effects of higher capital requirements on bank credit supply depend on a number of factors. First, these effects are likely to be smaller when the higher capital requirements are less binding for banks, i.e., when banks have capital cushions that can absorb more stringent requirements. Second and related, the effects of higher capital requirements on banks are likely to be smaller when the increases in capital requirements are gradual and anticipated. This feature allows banks to adjust capital smoothly and in advance, thus making the on-impact effect of higher capital requirements smaller, or not binding. Finally, the effects of higher capital requirements on banks are likely to be smaller when banks are profitable, thus enabling them to use retained earnings to increase capital, and when macroeconomic and macrofinancial conditions are accommodative, resulting in higher bank profitability and better access to funding markets.

The aim of the paper is to investigate the effects of higher capital requirements in an emerging market and explore the heterogeneity of these effects depending on economic conditions and bank characteristics. For this purpose, we use data on Peru's transition to higher capital requirements over 2009–2016. The first reform in capital requirements, announced in July 2008, established a gradual increase of uniform minimum capital requirements from 9.1% to 10% of RWA in four steps between 2009 and 2011. The second reform, announced in July 2011, introduced a bank-specific compulsory capital buffer (with magnitude anchored to bank loan portfolio characteristics) that each bank had to hold on top of the uniform 10% minimum capital requirements.

Peru offers an interesting case study for two key reasons. First, while the increases in bank capital requirements in Peru were meaningful, their introduction was gradual and pre-announced—two features that suggest smaller effects of capital requirements on credit. Having said this, the second reform of capital requirements had substantial time-varying and bank-specific components, enabling better identification, and creating scope to observe the effects of capital requirements on credit, despite their gradual and anticipated introduction. Second, the bulk of the existing evidence on the effects of higher capital requirements are based on advanced economies (mostly in Europe and the United States), during the period of sluggish if stable economic growth, and with many banks experiencing low and uncertain profitability.[2] Peru, a representative emerging market, implemented higher capital requirements during a period of high economic growth and robust but heterogeneous bank profitability—enabling us to test the external validity of traditional results on the effects of higher bank capital requirements in this more buoyant environment.[3]

A key empirical challenge in studying the effects of higher bank capital requirements on bank lending is controlling, to the extent possible, for credit demand. There are two established approaches to doing so. One approach involves the use of data on individual loans, usually sourced from confidential credit registries, and considers borrowers (or clusters of borrowers) with multiple bank relationships. In this way, it is possible to compare the provision of credit by two banks differentially exposed to the policy change to the same borrower, under the assumption that credit demand is homogeneous across banks. However, when this assumption does not hold plausible, this approach can lead to misidentifying credit supply shocks (Paravisini et al., 2017). The second-best solution is to use bank-level data and exploit, when possible, the differential exposure of banks to higher capital requirements. In this case, it is still possible to isolate the effect of the policy on the provision of credit by absorbing global and bank-specific factors that may affect credit demand via time fixed effects, and constructing proxies for time-varying bank-specific credit demand (Aiyar et al., 2014).

In this paper, we adopt the latter approach. Our main analysis is based on quarterly bank-level data and exploits the adoption of bank-specific capital buffers introduced by the second reform to investigate how changes in bank capital requirements affect bank lending. We try to isolate credit supply from credit demand controlling for standard bank characteristics and a time-varying measure of bank loan demand—constructed by weighting sector-level GDP growth by the predetermined bank-level sectoral loan shares. We absorb the effects of time-invariant bank-specific unobservables and common shocks with bank and time fixed effects. The results indicate that higher capital requirements do affect bank credit in Peru, despite their introduction being gradual and largely anticipated: a one percentage point increase in capital requirements is associated with a reduction in loan growth of 4 to 6 percentage points in the same quarter, a magnitude similar to that found by Aiyar et al. (2014) for British banks. However, in contrast to some of the previous literature, we find that this effect is remarkably short-lived and becomes statistically insignificant already in half a year.

Our results also show that the effect of higher capital requirements on loan growth is heterogeneous across bank characteristics and depends on economic conditions. Indeed, we find that the negative association between higher capital requirements and bank credit is substantially stronger in periods of relatively slower economic growth. We find further evidence that bank characteristics and performance shape banks' response to capital requirements, as weaker banks (e.g., less profitable, less liquid, less capitalized, and with no positive retained earnings) react more to changes in capital requirements. Thus, we attribute the short-lived effect of higher capital requirements on loan growth mostly to the strong balance sheets and performance of the Peruvian banks and to the buoyant economic environment.

Our results are robust to estimating different specifications to address concerns about the role of reform anticipation and the endogeneity of capital requirements. In particular, we do not find any evidence that bank credit is adjusted ahead of higher capital requirements, despite the lag between the announcement and the implementation dates. Moreover, we exploit the first reform, which introduced higher—but uniform—capital requirements, to show that even an arguably unanticipated increase in capital requirements led to a relatively small and temporary decline in lending. Finally, we use more granular data at the bank-product-quarter level. In this case, we can exploit the differences in the size of the countercyclical capital buffer within bank and across loan products. The product-specific surcharges can be considered exogenous as long as banks cannot change their existing portfolio allocation in anticipation of the activation of the countercyclical buffer. The product-level data also allow us to better control for credit demand through bank-time fixed effects. The results still show evidence of a contemporaneous—but short-lived—effect of higher bank capital requirements on loan growth.

The findings of this paper have important policy implications. They suggest that higher bank capital requirements can have only limited short-term effects on credit provision when they are implemented gradually and during periods of buoyant economic growth.

[2] Gambacorta and Murcia (2019) summarize a set of studies done in Latin America to estimate the impact of macroprudential policies on credit growth, but only in one country (Argentina) the analysis focuses on bank capital.

[3] The average return on equity in the banking system was about 31%, and GDP grew at an average annual rate of 4.6% during the period of the bank capital requirement reform in Peru. The respective values for the U.K. during the period 1998–2007 analyzed by Aiyar et al. (2014) are 18% and 2.9%. Data on GDP growth are taken from the IMF's World Economic Outlook (October 2018 vintage), while data on return on equity are taken from the World Bank's Global Financial Development Database (July 2018 vintage).

In addition, our results indicate that bank characteristics and, in particular, balance sheet strength matters, as less profitable, less capitalized and less liquid banks react more to changes in capital requirements. We also show that, interestingly, banks do not adjust credit in anticipation of higher bank capital requirements, suggesting that the pre-announcement of higher capital requirements may matter less than their gradualism and the economic environment surrounding their implementation. Our analysis offers other useful insights. First, it points to the limits on the external validity of bank regulation impact studies based on advanced economies for the emerging market and developing economies due to likely differences in the broader economic environment and banks' performance and balance sheet strength. Second, the empirical approach taken in our analysis is instructive in showing how credit demand can be controlled for in situations when loan-level data from credit registers are not available, as is often the case in emerging market and developing countries, or in the presence of data confidentially restrictions.

The rest of the paper is organized as follows. Section 2 reviews the existing literature on capital requirements and bank lending. Section 3 outlines the Peruvian regulatory reforms. Section 4 presents the data and the empirical methodology. Section 5 discusses the main results as well as additional exercises and robustness tests. Section 6 concludes.

2. Related literature

When studying the effects of capital requirements on credit, the literature generally distinguishes between transitory and steady state effects. Transitory effects, analyzed also in this paper, refer to what happens to lending during banks' adjustment to higher capital levels. Steady state effects refer to the implications for lending after banks have fully converged to higher capital levels.[4]

Estimating the steady-state effects of higher bank capital is challenging. The identification from time variation hinges on banks' reactions to exogenous shocks to bank capital, and on considering a large time window around these shocks to capture the steady-state effects. However, this exposes the estimation to multiple confounding factors (e.g., economic slowdown, other policies) that can also affect lending and bias the estimation of the effect of bank capital. The identification from cross-sectional variation in capital is also likely biased because such variation reflects, at least in part, endogenous bank capital choices. Unlike capital, capital requirements usually do not to suffer from such endogeneity. However, they tend to be uniform across banks. Notwithstanding these caveats, the empirical literature finds that a 1 percentage point higher Tier 1 capital ratio is associated with 2.5–13 basis points higher loan rates—a modest effect (Baker and Wurgler, 2015; Barth and Miller, 2018; Francis and Osborne, 2012; Kisin and Manela, 2016; Dell'Ariccia et al., 2017). Moreover, some papers find positive effects of higher steady-state bank capital on loan growth, possibly reflecting banks' increased risk-bearing capacity (Berrospide and Edge, 2010; Buch and Prieto, 2014; Cohen and Scatigna, 2016; Gambacorta and Shin, 2016; Bahaj and Malherbe, 2018).

Since estimating the steady-state effects is challenging, many studies take a theoretical and calibration approach to assessing the long-run effects of bank capital requirements on credit provision and bank risk. The key benefit of higher capital requirement is to reduce banks' excessive risk taking induced by leverage and deposit insurance. Another benefit is to create buffers to absorb future losses. The cost comes from different sources: reduced liquidity creation (Van den Heuvel, 2008), reduced credit supply and output within and beyond the banking sector (Martinez-Miera and Suarez, 2014; De Nicolo et al., 2014; Elenev et al., 2018; Xiang, 2018), and lower competition in the banking sector (Corbae and D'Erasmo, 2019). As a result, the social welfare exhibits an inverse U-shape with respect to bank capital requirement, with the optimal level of capital being theoretically unclear. For example, Van den Heuvel (2008) suggests that higher bank capital implies high welfare costs due to less liquidity creation, while Nguyen (2015) suggests large welfare gains due to less risk taking. Similarly, Kashyap et al. (2010) suggest limited negative effects of higher bank capital requirement on credit supply and output, while Martinez-Miera and Suarez (2014) show that credit supply in normal times can significantly shrink if bank capital requirements are raised from 7% to 14%. Elenev et al. (2018) and Xiang (2018) argue that the existing levels of bank capital are close to optimal in their quantitative DSGE model. Begenau (2019) proposes a mechanism that leads to an opposite DSGE-based result: higher capital requirements can increase credit supply by lowering the cost of funding for banks, which translates into banks' balance sheet expansion.

The literature on the effects of *transition* to higher bank capital—on which our paper builds—employs more robust approaches to obtain empirical identification and often finds substantial effects of higher capital requirements on loan growth. This literature goes back to Peek and Rosengren (1995), who show that banks that were subject to a capital conservation plan during the Basel I implementation in the U.S. lent 2% less than other banks in the year that followed the imposition of the plan. More recent papers focus on the effects of bank-specific capital requirements and on the heterogeneous capital shortfalls following bank stress tests. For example, Aiyar et al. (2014) use bank-specific time-varying capital charges imposed by U.K. regulators. They estimate a bank-level lending regression and show that a 1 percentage point increase in bank capital requirement is associated with 5.7–8% lower bank lending in the following three quarters. Noss and Toffano (2016) study the same regulatory data in a VAR setting that better takes into account the endogeneity of regulatory actions and find a smaller effect: 3.75 percentage point reduction in quarterly lending growth after two quarters that fades to zero after about one year. Fraisse et al. (2019) use loan-level data and exploit the changes in bank capital requirements related to Basel II implementation in France. Their results indicate that a 1 percentage points higher bank capital requirement is associated with 9% lower lending in the following year: a more substantial effect than that in Aiyar et al. (2014). A variety of estimates exists also in the studies that examine the heterogeneity of bank capital shortfalls after stress tests. Mésonnier and Monks (2015) consider the 2011 EBA stress tests and find that a 1 percentage point higher capital shortfall is associated with 1.6% lower bank lending in the following year: a modest but meaningful effect. Still in the context of the EBA framework, Cappelletti et al. (2019) exploit the discontinuity introduced by the selection of "Other Systematically Important Institutions" (O-SII), which are charged additional capital surcharges, and find a transitory reduction in bank lending and a shift towards less risky borrowers, which instead persists over the medium term. In contrast, the papers that exploit differences across banks in their exposure to the US bank stress tests, such as Acharya et al. (2018) and Cortes et al. (2019), find that the stress-tests had small to no effects on overall credit supply, and any effects were limited to the credit to risky borrowers and small businesses.[5]

Other papers focus on the heterogeneous effect of higher capital requirements on distinct types of loans. Imbierowicz et al. (2018) show that, in response to higher capital requirements,

[4] This section builds on and extends the review in Dagher et al. (2020).

[5] Focusing on the gap between the extra capital implied by the supervisory stress tests relative to the banks' own models, Basset and Berrospide (2018) do not find that this capital gap acts as a constraint to loan growth.

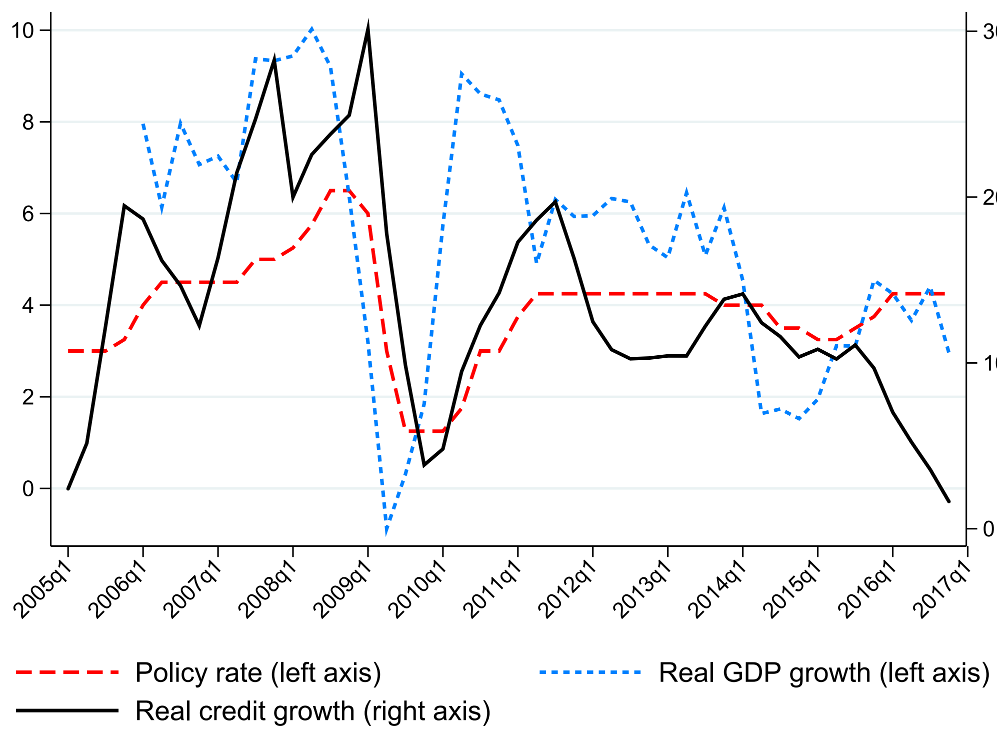


Fig. 1. Macroeconomic setting in Peru, 2005–2016. *Notes*: Quarterly growth rates for credit and GDP are in real terms and year-on-year. Source: Banco Central de Reserva del Peru.

Danish banks retrench more from loans with higher risk weights. Bridges et al. (2014) similarly show that the British banks reduce most the growth of their commercial real estate lending, then corporate lending and, finally, personal lending. De Jonghe et al. (2020) find that Belgian banks retrench more from corporate sectors in which they are less specialized. Using European data from the EBA exercise, Gropp et al. (2019) document particularly strong effects of bank' capital shortfall on their syndicated lending: a 27% reduction to achieve a 1.9 percentage point increase in bank capital, consistent with syndicated loans being arm's length (arm's length lending tends to contract more during periods of distress, Bolton et al., 2016) and less specialized. Behn et al. (2016) show that, during the transition to model-based capital requirements under Basel II, banks reduced the growth of loans whose risk weights increased.

Finally, Mendicino et al. (2019) develop a model that calibrates the trade-off between the long-run benefits of higher bank capital and the transition costs. They suggest that about a quarter of the long-run benefits can be lost to the transition costs, but less so if the transition costs are offset by accommodative monetary policy or if the banking system was particularly risky.

Our paper adds to the aforementioned literature, which examines the consequences of raising capital requirements primarily in advanced economies and following periods of stress, by (i) analyzing the case of an emerging market during a period of economic expansion, and (ii) showing how differences in bank characteristics shape the reaction of credit growth to higher capital requirements.

3. Institutional background: capital reforms in Peru

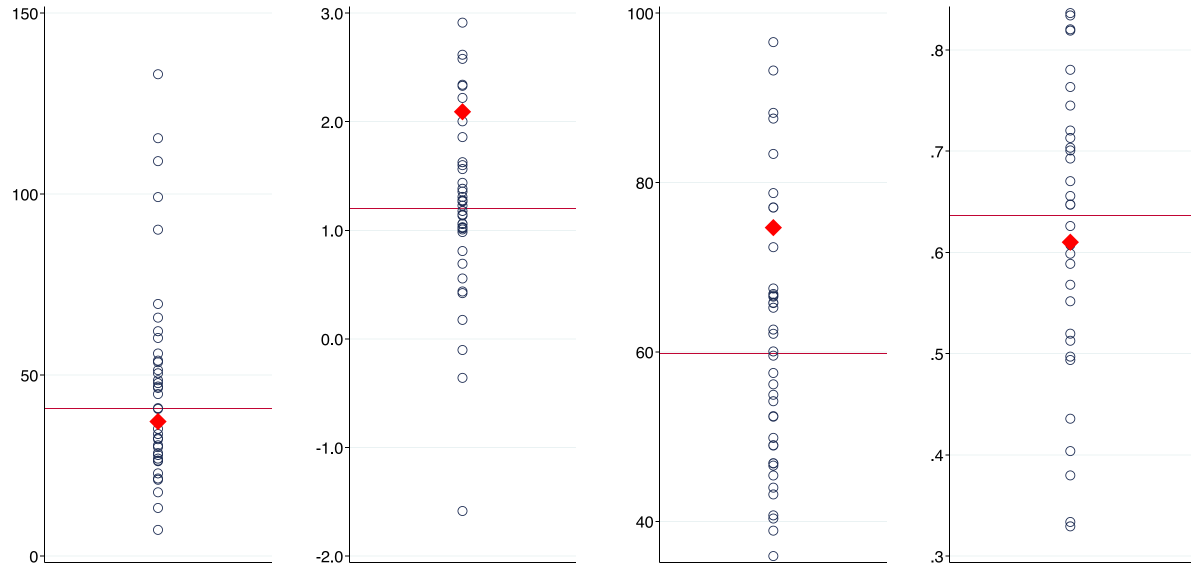
Peru represents a good benchmark for studying the effects of capital requirements in emerging markets. As shown in Fig. 1, Peru has grown at more than 5% per year since 2005, keeping inflation stable at around 3%.[6] Over the 5-year period 2012–2016, GDP per capita in Peru was US$ 5,828, somewhat higher than the average middle-income country (MIC), which stood at US$ 4,744, but much lower than the average for a high income country (HIC) which was US$ 35,702. Bank credit in Peru has grown at rates above 10% per year since 2005. As a result, the average of domestic credit to the private sector between 2012 and 2016 was 37% of GDP, close to the average for MICs (46%) but much lower than in HICs (91%). The degree of bank capitalization in Peru is also very close to that for MICs (bank capital over total assets was 11.5% in Peru and 10.5% in MICs), and higher than for HICs (where it was 8.8%).[7]

When comparing Peru with a sample of other 40 MICs, similarities emerge in terms of financial development, although the banking system in Peru is characterized by high average profitability and relatively high market concentration, two features which could mitigate the effects of capital requirements on credit growth. The level of financial development, as measured by the ratio of private credit over GDP, is close to the median of MICs (Fig. 2, panel a). However, the return on assets of Peruvian banks is higher than 2%, well above the median of MICs (Fig. 2, panel b). A similar picture emerges for the return on equity—in our sample of banks, the average return on equity is at around 27%, a high level also in comparison with other emerging markets. The Peruvian banking system is relatively concentrated: the first three banks account for almost three quarters of total assets (Fig. 2, panel c and Table A1). However, measures of credit market competition show that Peru is closer to the median of MICs (see Fig. 2, panel d).[8]

[6] Data are taken from the IMF's World Economic Outlook (October 2018 vintage), GDP growth is expressed in real terms.

[7] Data are taken from the World Bank's Global Financial Development Database (July 2018 vintage).

[8] The chart plots the H-statistics, that measures the elasticity of banks revenues relative to input prices. Under perfect competition, the H-statistic equals 1, while values closer to 0 indicates a market closer to a monopoly. A similar picture emerges plotting the Boone indicator, a measure of degree of competition based



(a) Private credit (% GDP) (b) Return on assets (c) Concentration (d) H-statistic

Fig. 2. Banking system's characteristics in Peru vs other emerging markets. *Notes*: The charts reports the distribution across a sample of 40 emerging markets: of four variables, measured as averages of 2012–2016: the ratio of credit to the private sector over GDP (in percent, panel a); the return on assets (panel b); market concentration measured by the share of the top 3 largest banks in the country (panel c); and the H-statistics, (panel d). Each circle identifies a country. Peru is highlighted by the shaded square. The horizontal solid line corresponds to the sample median. Source: World Bank's Global Financial Development Database and Financial Structure Dataset.

Starting in 2007, Peru experienced an acceleration in credit growth, which reached levels above 20% annually and pushed the credit-to-GDP ratio above its trend, raising financial stability concerns (Rossini and Quispe, 2017). To moderate credit growth, since the GFC the Peruvian banking regulator—the Superintendencia de Bancos y Seguros (SBS)—raised capital requirements in two phases. During 2009–2011, the SBS raised uniform minimum requirements from 9.1% to 10% of risk-weighted assets (RWA), though in a staggered fashion. In the period 2012–2016, the SBS introduced bank-specific capital buffers. These came on top of the 10% uniform minimum and, depending on the bank, could be as high as 5.6 percentage points (see Figs. 3–5). It is worth noting that the policy rate, after a substantial reduction in response to the GFC, remained relatively stable since 2011, during the implementation of the bank-specific capital requirements (Fig. 1), so our results are unlikely to be confounded by monetary policy.[9]

The first increase in capital requirements, announced in July 2008, stipulated that on July 1, of 2009, 2010, and 2011, uniform minimum capital requirements would be raised from 9.1% to 9.5%, 9.8%, and 10% of RWA, respectively. The second reform of capital requirements, announced in July 2011, introduced a formula determining the bank-specific compulsory capital buffer that each bank had to hold on top of the uniform 10% minimum capital requirements. It was stipulated that, on July 1, of the years 2012 to 2016, banks had to hold 40, 55, 70, 85 and, finally, 100% of the formula-prescribed buffer, respectively.

The second reform established compulsory capital buffers which consist of a non-cyclical and a countercyclical component.[10] The latter is calculated by first obtaining the marginal requirement which results from multiplying banks' direct exposures for each type of credit, by risk weights applied to each exposure. Then the cyclical buffer is calculated by multiplying the marginal requirement by the uniform capital requirement. This buffer is calculated every month. Risk weights stay constant but changes in this capital requirement are driven by changes in the direct exposures to each type of loan (which will vary on a monthly basis) or in the uniform capital requirement (which changes according to the reform calendar).

The noncyclical buffer consists of various concentration risk buffers, a risk-propensity buffer and, for the largest banks, a (very small) systemic risk buffer (see Fig. 4). The concentration buffer includes three components: a measure for individual concentration, one for sectoral concentration and another for regional concentration. Concentration risk from individual largest exposures is measured by the sum of top-20 exposures over risk weighted assets multiplied by the capital requirement for credit risk. This buffer requirement kicks in only when the measure exceeds 5%. Sectoral and regional concentration risk buffers are determined on the basis of Herfindahl-Hirschman indices applied to the banks' loan books, distinguishing between 19 sectors and 8 regions. These concentration rates are then respectively multiplied by the required capital for credit risk to determine the additional buffers. Though the calculation of each concentration buffer is done monthly, the rates for the buffer for individual concentration are adjusted twice a year (June and December) and, for the regional concentration measure, the adjustment related to the share of the population or value added by region is adjusted every 5 years. Hence, concentration buffer measures are slow moving.

The systemic risk buffer considers the size of each institution, measured by its assets, relative to GDP and calculates an effective

on profit-efficiency in the banking market, calculated as the elasticity of profits to marginal costs.

[9] During our sample period, the central bank introduced conditional reserve requirements (RR) as a macroprudential tool explicitly aimed at curbing excessive growth of credit in foreign currency. This policy seems to have generated a change in the currency composition of bank lending, even though aggregate credit growth remained relatively stable (Cabello et al., 2017; Keller, 2018; Gambacorta and Murcia, 2019). For an overview of the macroprudential framework in Peru, see Rossini and Quispe (2017).

[10] See Appendix A for details, templates and formulas for the calculation of each component of the buffer.

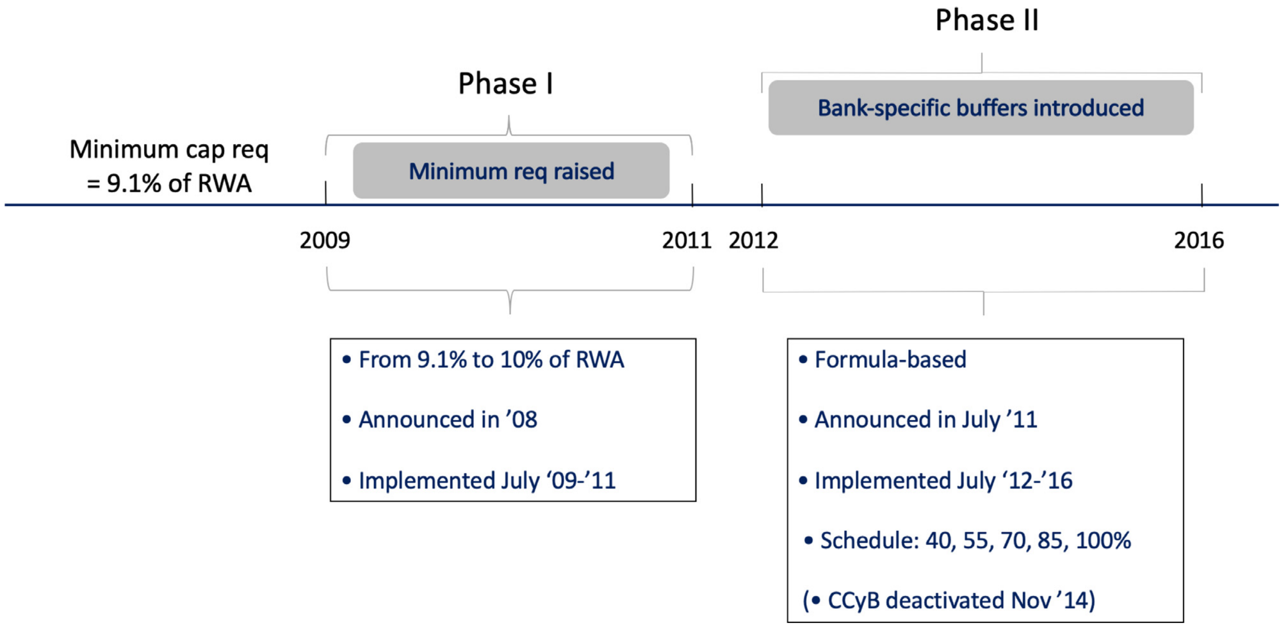


Fig. 3. Timeline of Peruvian capital reforms. *Source*: Superintendencia de Banca y Seguros (SBS).

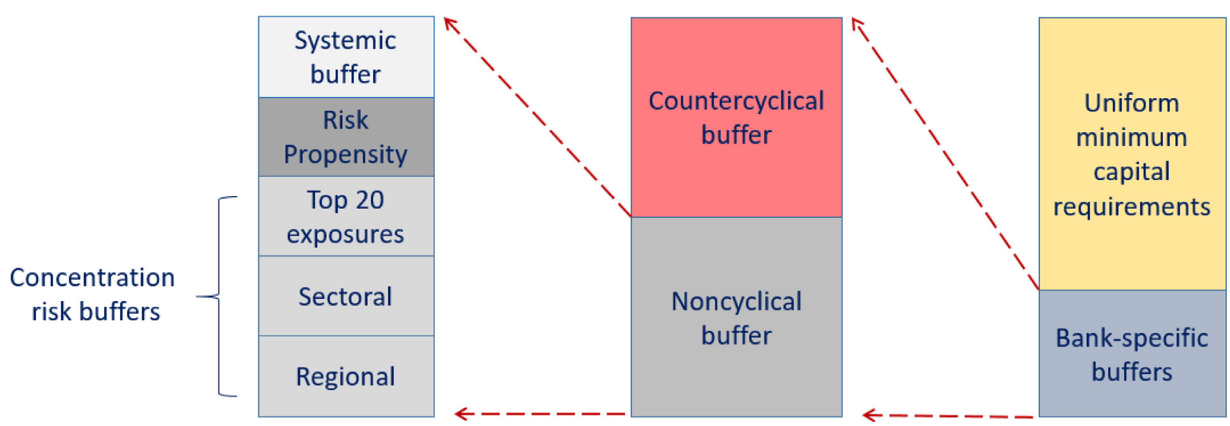
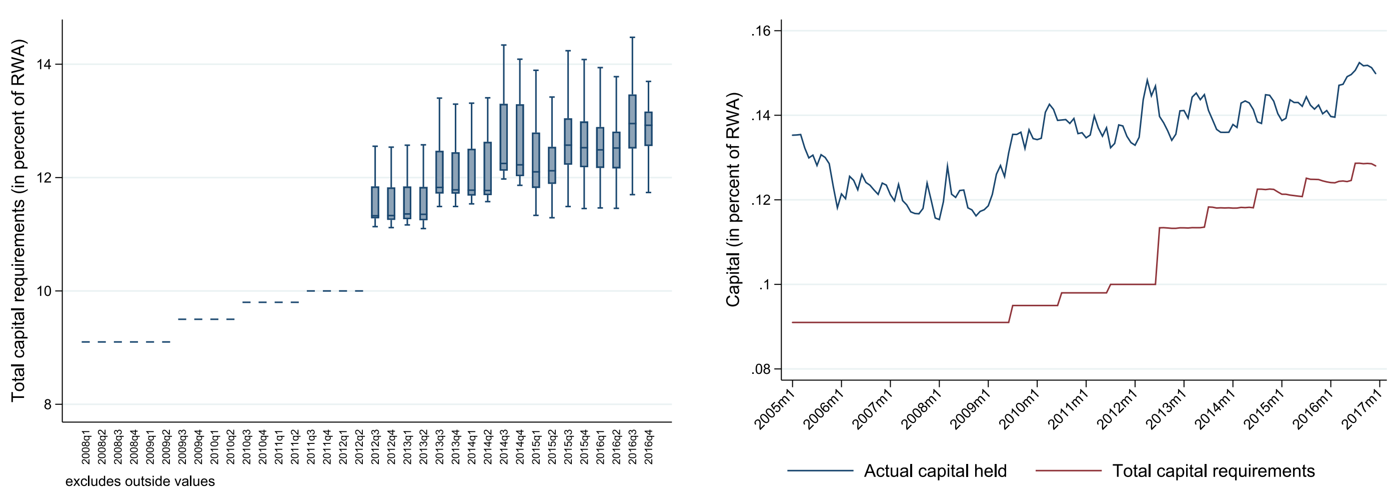


Fig. 4. Components of total capital requirements. *Source*: Superintendencia de Banca y Seguros (SBS).



(a) Bank-specific capital requirements

(b) Actual vs required capital

Fig. 5. Bank capital requirements. *Notes*: The charts reports the evolution of bank capital requirements for the 14 banks in the sample. Panel (a) reports the distribution of bank capital requirements by quarter. Until 2012:q2 there is no variation across banks as capital requirements are uniform. Since 2012:q3, the boxplot reports the interquantile range (the horizontal solid line is the median) and the whiskers correspond to the 10th and the 90th percentiles of the distribution of bank capital requirements. Panel (b) shows the average total capital requirements (solid line) and the average actual capital (dashed line) held at a monthly frequency. Data are weighted by total assets.

requirement which is multiplied by the minimum capital requirement for credit, operational and market risk. The size indicator is calculated once a year.

The risk-propensity buffer is driven by the evolution of a bank's specific loan provisions and risk-weighted assets over the last five years. It affects only a few of the largest banks.

For the purpose of parameter identification, the first reform has the advantage that the initial increase of capital requirements were likely unexpected. Yet those capital requirements were not binding for all banks, a fact which could moderate their effects on bank lending. Also, the uniformity of requirements across banks does not allow us to control in a credible way for other contemporaneous policy and macroeconomic shocks, unless we focus exclusively on the heterogeneous impact of the reform across banks. In that case, we can interact the capital requirements with bank characteristics and control for unobserved common factors by including time fixed effects; we report results following this approach in Section 5.5.

Thus, while we will exploit the first phase of the reform to mitigate endogeneity concerns, our main analysis focuses on the second phase of the reforms when capital buffer requirements were bank-specific and depended on bank portfolio composition. During the second reform, changes in compulsory capital buffers were of two types: annual implementation-related "jumps" and monthly portfolio composition-driven "wiggles". Since the "wiggles" were fully driven by the changes to bank portfolio composition and thus potentially endogenous to bank lending decisions during the previous month, we drop them from the analysis and focus on the implementation "jumps" only. The heterogeneity of capital buffer requirements introduced by the second reform, depending on bank portfolio composition, allows us to add time fixed effects to absorb all common macroeconomic shocks to bank lending, while bank fixed effects control for all time-invariant differences between banks. The only changes and shocks that then remain to be controlled for are those that are both bank- and time-specific, such as profitability, liquidity and, more important, demand for credit. In the empirical specification we deal with this issue by including time varying bank-level indicators of bank health and loan demand.

Furthermore, since the capital surcharges within the CCyB component of the compulsory capital buffer vary across borrower types depending on their CCyB weights, the effects of the compulsory capital buffer are expected to be different across product categories. This enables us to examine the effects of capital requirements on different types of lending *within* the same bank, while controlling for overall bank conditions and using more granular measures of loan demand (see Section 5.5).

Two additional institutional caveats are in order. First, from November 2014, the compulsory buffer for new lending was *de facto* reduced by "switching off" its CCyB component. The non-cyclical component remained, and the countercyclical component remained fully applicable to past lending, including the need to top it up to 85% and 100% of the formula on July 1, of 2015 and 2016, respectively. Second, the Peruvian regulators demonstrated a degree of forbearance to banks' temporary and small breaches of compulsory buffer requirements—six out of the fourteen banks in our sample did breach buffer requirements at some point. This softer implementation may be an additional factor that reduced the effects of higher bank capital requirements on bank lending in Peru.

4. Data and empirical methodology

To investigate the impact of higher bank capital requirements, we collected capital requirement and quarterly balance sheet data for all 16 Peruvian commercial banks, for the period 2005–2016 from the SBS (see Table A1 in the Appendix).[11] From this sample, we dropped two small banks, because of their short histories (they were established in 2012 and 2014, respectively) and associated extreme values for loan growth. Similarly, for newly-established banks with longer histories, we excluded observations for the first 4 quarters since, during their first year of operation, these banks exhibited very high capital ratios resulting from low levels of lending. Finally, we winsorized loan growth at the 1st and 99th percentile, to ensure that our findings are not driven by extreme and unrepresentative outliers of loan expansion and contraction. The final sample of 14 banks is representative of the national banking system. These banks accounted for about 85% of banking system assets in 2016. Quarterly loan growth during the sample period averaged 3.2% in real terms, a number consistent with aggregate statistics reporting 13.7% real annual growth (see Fig. 1; and Table 1 for further summary statistics of the data).

To test for the effects of capital requirements on bank lending, our baseline empirical approach exploits the variation of capital requirements introduced by the second reform across banks and time, building on a well-established approach that regresses bank-level credit growth on changes in (bank-specific) capital requirements (see Aiyar et al., 2014, 2016). The bank-specific capital buffers allow us to exploit deviation from averages, in terms of credit growth and in term of buffer requirements, to identify the effect of higher capital. In practice, we estimate the following regression equation:

$$\Delta LOAN^i_{t+r,t-s} = \beta_{r,s}\Delta KR^i_{t,t-1} + \gamma'\mathbf{X}^i_{t-s} + \alpha D^i_t + \phi^i + \tau_t + \epsilon^i_t \qquad (1)$$

where the dependent variable is loan growth, while the key explanatory variable is the change in the required capital buffer. Formally, $\Delta LOAN^i_{t+r,t-s}$ is defined as the log-difference in the stock of outstanding gross loans of bank i between the end of quarter $t+r$ and the end of quarter $t-s$. The change in the required capital buffer, $\Delta KR^i_{t,t-1}$, is defined as the percentage point difference in bank i's average capital requirement in quarters t versus $t-1$. Recall that, starting in 2012, banks' buffer requirements changed for two reasons. First, on July 1, of each year, there was a jump in requirements due to a step up in reform implementation. Second, there were more minor monthly changes (wiggles) in between these jumps, resulting from the evolution of banks' balance sheets and the monthly recalculation of the requirements. As the jumps can be plausibly considered exogenous (although anticipated) changes to capital requirements, while wiggles were driven by banks' portfolio changes, to mitigate the endogeneity bias we only use jumps when computing ΔKR.[12] In our baseline specification, we consider cumulative credit growth over increasingly longer periods "straddled" around jumps in capital requirements. Comparing credit growth across banks and time reveals if (and by how much) banks faced with higher capital requirements grew their lending slower than banks with lower requirements. Examining increasingly longer periods shows how durable the effect was. However, as our sample ends only six months after the last capital buffer increase of July 2016, the empirical analysis focuses on the short to medium term effects, without tackling the issue of steady-state effects of capital requirements.

The set of time-varying, bank-specific controls, $\mathbf{X}^i_{t-s}$, is relatively parsimonious and includes standard control variables: 1)

[11] While data on capital requirements and bank balance sheets are available originally at a monthly frequency, we collapse them at a quarterly frequency. In this way, we can still preserve the capacity to look at short-term adjustments of bank credit to changes in capital requirements, while at the same time reducing the noise in monthly data and the extreme values that may derive from computing variations over monthly intervals.

[12] Even though in this approach identification is based on the second reform, we estimate our model on the whole sample period 2005–2016. However, as discussed below, our results do not change if we focus on the 2010–2016 period.

Table 1
Variables: definitions and summary statistics. *Notes*: The table shows the list of variables used in the main empirical analysis, their definition and summary statistics. All variables are constructed at a quarterly frequency, on a sample of 14 banks.

Variable	Definition	Mean	SD	Min	Max	Obs
$\Delta LOAN$	Quarter-on-quarter change in real gross loans. Constructed from end-of-quarter loan levels deflated with consumer prices.	0.029	0.053	-0.106	0.233	550
ΔKR	Quarter-on-quarter "jumps" in total capital buffer requirement, expressed as percent of RWA. Jumps are equal to zero, except at step-ups in reform implementation on July 1, of the years 2012 to 2016. Constructed from averages of monthly-levels data in each quarter.	0.091	0.299	0.000	2.553	550
$\Delta NCKR$	Quarter-on-quarter "jumps" in noncyclical capital buffer requirement, expressed as percent of RWA. Jumps are equal to zero, except at step-ups in reform implementation on July 1, of the years 2012 to 2016. Constructed from averages of monthly-levels data in each quarter.	0.036	0.116	0.000	1.284	550
$\Delta CCKR$	Quarter-on-quarter "jumps" in countercyclical capital buffer requirement, expressed as percent of RWA. Jumps are equal to zero, except at step-ups in reform implementation on July 1, of the years 2012 to 2016. Constructed from averages of monthly-levels data in each quarter.	0.055	0.196	0.000	1.933	550
$CAR - KR$	Difference between total regulatory capital held and total capital requirement during quarter, both expressed as percent of RWAs	0.035	0.026	-0.010	0.191	550
Assets	Natural logarithm of total assets (in Soles) on balance sheet at the end of quarter	15.496	1.413	12.236	18.444	550
ROA	Return on assets during quarter, defined as net income divided by total assets	0.005	0.005	-0.045	0.032	550
Liquidity	Ratio of liquid assets and total assets at the end of quarter	0.209	0.097	0.034	0.530	550
RWA	Ratio of risk-weighted assets and total assets at the end of quarter	0.828	0.134	0.383	1.450	550
Demand	Bank-specific, time-varying loan demand, see Eq. (2) for details	0.047	0.033	-0.054	0.124	550
Deposits	Ratio of retail deposits over total liabilities at the end of quarter	0.734	0.104	0.383	0.955	550
Retained earnings	Dummy equal to 1 if retained earningsat the end of quarter are positive, and zero otherwise	0.510	0.500	0.000	1.000	525
ΔROE	Quarter-on-quarter change in return on equity (ROE)	0.000	0.024	-0.081	0.108	550

bank size, measured by the logarithm of total assets; 2) liquidity, defined as the ratio of liquid assets over total assets; 3) profitability, measured by the return on assets; 4) the ratio of risk weighted assets over total assets; and 5) "excess capital," i.e., capital adequacy ratio (CAR) minus capital requirement (KR). For precise definitions and summary statistics, see Table 1.

As we aim to identify the effect of change in capital requirements on loan growth, it is important to recognize that observed changes in bank lending could be the results of supply and demand effects. We can control for all macroeconomic and policy shocks affecting banks equally (e.g., changes in economic growth and monetary policy) through time fixed effects (τ_t). Differences in unobserved (and constant) bank characteristics which may result in different demand for credit are absorbed by bank fixed effects (ϕ^i). To account for changes in loan demand that vary across banks but also over time, we exploit information on the sectoral specialization of lending across banks to construct a bank-specific time-varying proxy for loan demand. Following an approach similar to Aiyar et al. (2014, 2016), we construct the time-varying loan demand measure D_t^i for a given bank i as the weighted average of the sectoral GDP growth, taking the *ex-ante* sectoral loan shares of a given bank as weights. More formally:

$$D_t^i = \sum_s w_{is} \times \Delta GDP_{st} \tag{2}$$

where w_{is} is the predetermined share of the loan portfolio of bank i, in sector s, measured in the first quarter in which bank i enters the sample.[13] We have disaggregated data on lending to 12 sectors (agricultural, manufacturing and service sectors), plus consumer lending and mortgage lending. ΔGDP_{st} is the yearly real growth rate of GDP in sector s, computed as log difference between t and $t-4$, to avoid the seasonality of growth rates at shorter frequencies. For consumer lending we use real GDP growth, while for mortgage lending we use the growth rate of real house prices, again computed year-on-year. For loan demand to adequately capture differential demand across banks, it is critical that banks have a different sectoral specialization. This is indeed the case, as illustrated by Fig. 6, which plots D_t^i over time and points out that loan demand varies along the cycle and it does so differentially across banks. To further validate the construction of the proxy for loan demand, Fig. 7 shows that there is a positive and significant correlation between loan demand and loan growth at the bank level.

5. Results

5.1. Baseline

Table 2 reports the OLS estimates of Eq. (1) for progressively longer straddles around jumps in capital requirements. Standard errors are clustered at the bank level to allow for autocorrelation within banks. In the first column we look at the contemporaneous effect of capital requirements on loan growth, controlling only for loan demand, and bank and time fixed effects. In column 2 we add bank-specific controls. Subsequently, the period over which credit growth is calculated is progressively lengthened: column 3 shows the effect of capital on credit growth over a six-month period, from three months before the jump until three months after, while the effect over a one-year period straddling an increase in capital requirements is reported in column 4. Columns 5–8 mimic this structure presenting weighted regressions where observations are weighted by bank assets to have a more precise sense of the aggregate effects.

Our estimates point to a contemporaneous effect of capital on lending that "washes out" very quickly. Columns 1–2 and 5–6 reveal that a one percentage point increase in capital requirements is associated with a reduction in loan growth of 4 to 6 percentage points in the same quarter, and that this effect survives the introduction of bank-specific controls.[14] However, columns 3–4 and

[13] Results are robust to computing the loan demand measure using time-varying weights, which better reflect the bank portfolio allocation, at the cost of the potential endogeneity of the weights if banks reallocate credit across sectors in anticipation of the changes in capital requirements.

[14] This result is close to what found by Aguirre and Repetto (2017) in Argentina, where bank lending decreased by 4.5% in response to a tightening of the capital buffer (but, interestingly, there was almost no effect at the introduction of the capital buffer).

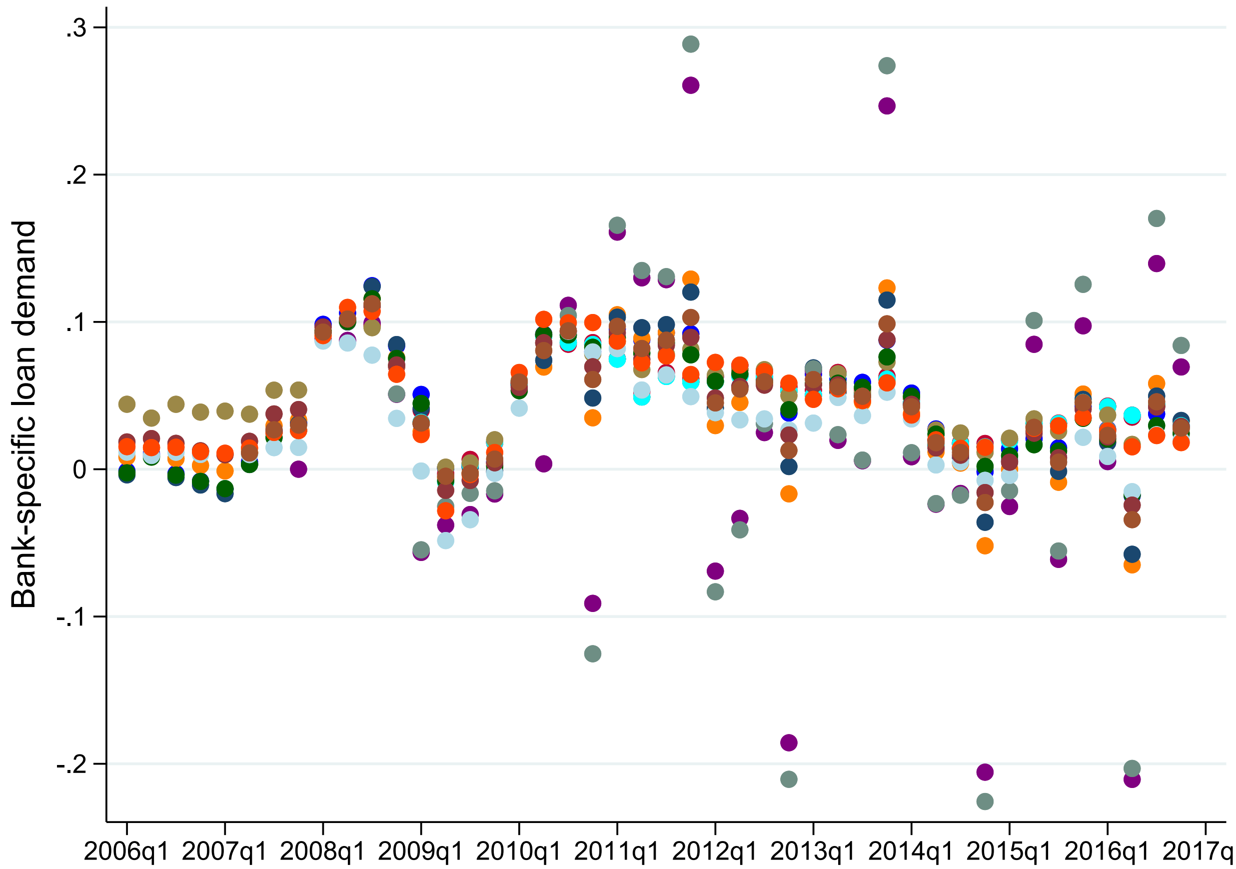


Fig. 6. Bank-specific, Time-varying demand for loans. *Notes*: The chart shows the quarterly evolution of the bank-specific time-varying measure of loan demand (see Eq. (2)). In each quarter, a dot represents one bank.

7–8 show that, for longer periods, the coefficient on ΔKR is not significantly different from zero. This means that, over half a year and more, loan growth does not significantly differ between periods with and without changes in capital requirements. Lengthening the straddle to 6 or 8 quarters—i.e., 3 or 4 quarters on either side of a jump—does not change this conclusion. In that respect, our results are close to Noss and Toffano (2016), who find that the effect of a capital requirement increase in the U.K. fades to zero in about a year, but imply an even shorter-lived effect.[15]

To have a better understanding of the dynamic effect of changes in capital requirements on loan growth we use the local projection approach—pioneered by Jordà (2005)—assuming that the impulse, or "shock," occurs at the time of the jump in the capital requirement and tracing the effect up to one year after the impulse. Fig. 8 depicts the response of cumulative credit growth to a one percentage point rise in capital requirements. The figure confirms that the effect on cumulative credit growth is limited to a single quarter, either looking at unweighted (panel a) or weighted (panel b) regressions.

In interpreting our results—especially when assessing external validity—one should keep in mind two important features of the environment in Peru that may drive them. First, as discussed in Section 1, the introduction of capital buffers took place in the context of a growing economy and strong bank performance, which may have attenuated their impact, given that more profitable banks may find it easier to raise additional capital via retained earnings (De Jonghe et al., 2020). Since raising bank capital is easier in an environment of economic growth and high profitability than in a recession, our findings may not carry over to economies in a less buoyant state. We will test for the role of macroeconomic conditions and bank balance sheet strength in Sections 5.3 and 5.4. Second, it could be possible that, given that the reform was announced a year before its implementation, and considering the gradual implementation over 4 years, banks adjusted their level of capital ahead of the implementation dates. However, bank-level data do not show systematic evidence of such a pattern—see Fig. 5 (panel b), which does not point to any sharp increase in actual capital held by the banking system around July 2011, at the time of the announcement of the second phase of the reform. Moreover, in Section 5.5 we formally look at anticipation effects and rule them out.

5.2. Robustness

Before moving to a number of extensions, we test the robustness of the baseline findings to (i) alternative measures of loan growth, (ii) changes in the set of controls, and (iii) changes in the sample period. Results are reported in Table 3, again both for unweighted (top panel) and weighted (bottom panel) regressions. We start by computing loan growth using either net rather than gross exposures (column 1), or the Davis and Haltiwanger (1992) growth measure (column 2), which is symmetric and bounded, thus mitigating the effect of outliers, and captures the intensive and exten-

[15] As discussed in Section 3, the total change in capital requirements can be decomposed in the changes in the noncyclical and the countercyclical capital buffers. The countercyclical capital buffer is larger than the noncyclical one (Figure A1 in the Appendix) and its change shows a higher variability across banks (Table 1). When testing separately the role of these two components, we find that the average results seem to be driven by the changes in the countercyclical buffers, see Table A2 in the Appendix.

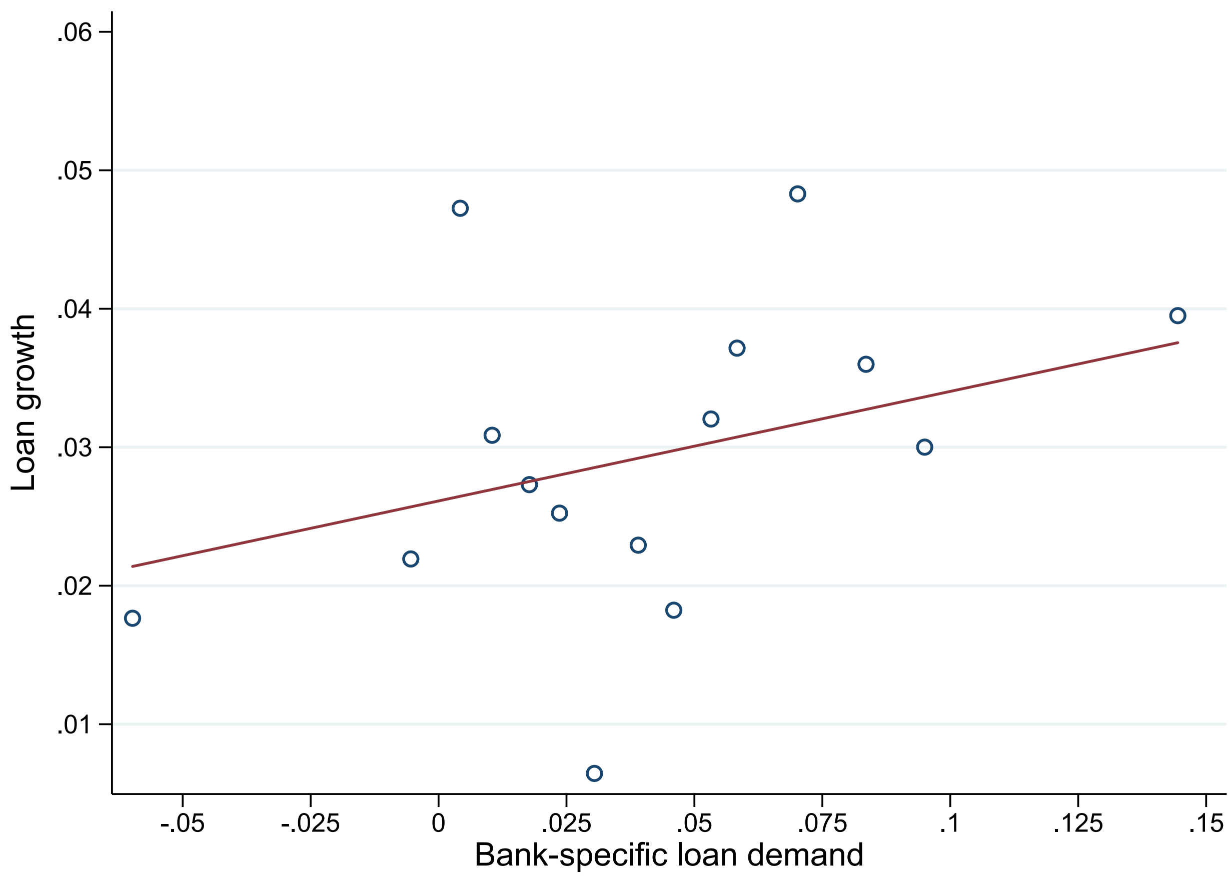
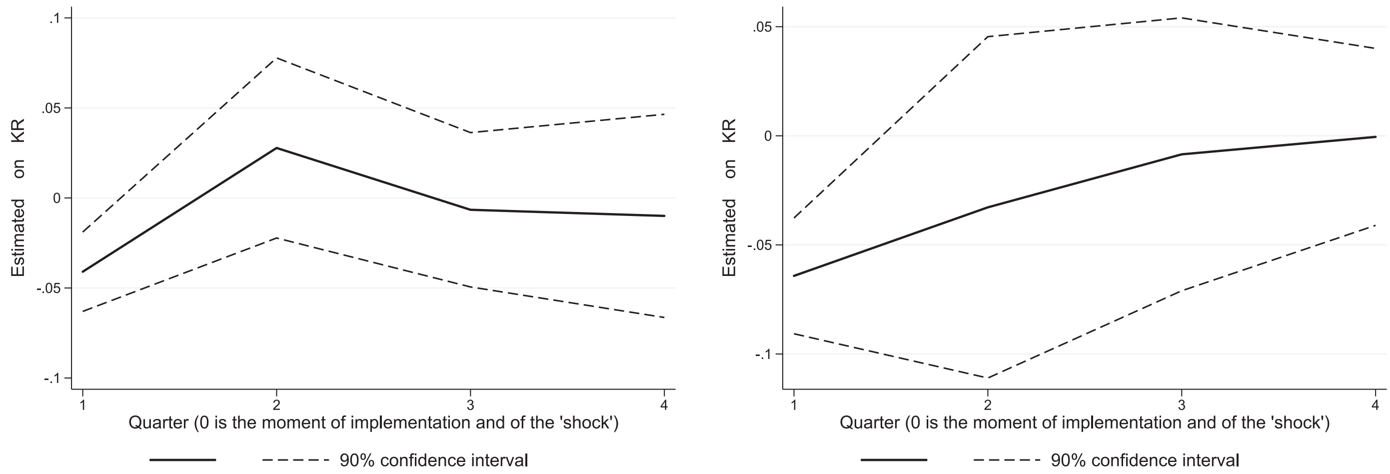


Fig. 7. Bank-specific, Time-varying loan demand and bank lending. *Notes*: A regression of loan growth against loan demand (both measured at time t), controlling for bank and M&As fixed effects, gives a coefficient on the capital requirement variable of 0.203 (p-value of 0.008). To generate the binned scatterplot, starting from the sample of 14 banks, the loan growth, defined as log-difference in stock of outstanding gross loans between t and $t-1$ (y-axis), is regressed against the bank-specific time-varying measure of loan demand (see Eq. (2)) at time t (x-axis) and bank and M&As fixed effects. Then, the x-residuals are grouped into 15 equal-sized bins, then the chart plots, for each bin, the mean of loan growth, within each bin, holding the controls constant. The solid line is the linear fit of the OLS regression of the y-residuals on the x-residuals.

Table 2
Baseline results. *Notes*: The table presents OLS estimates of model (1). The dependent variable is loan growth at the bank-quarter level, calculated as log difference between $t+r$ and $t-s$. All control variables are defined in Table 1. Weighted regressions use total assets as weight. Standard errors, clustered at the bank level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Dep. Var.: $\Delta LOAN_{t+r,t-s}$ $(r,s)=$	(1) (0,1)	(2) (0,1)	(3) (1,1)	(4) (2,2)	(5) (0,1)	(6) (0,1)	(7) (1,1)	(8) (2,2)
$\Delta KR_{t,t-1}$	−0.0446***	−0.0410***	−0.0125	0.0262	−0.0576***	−0.0642***	−0.0553	−0.0416
	(0.010)	(0.013)	(0.028)	(0.045)	(0.013)	(0.016)	(0.046)	(0.058)
$CAR-KR_{t-s}$		−0.1477	0.0666	0.3128		0.0509	0.1484	0.3589
		(0.135)	(0.296)	(0.548)		(0.127)	(0.275)	(0.561)
Assets_{t-s}		−0.0828***	−0.1883***	−0.3016***		−0.0588***	−0.1516***	−0.2750***
		(0.024)	(0.053)	(0.088)		(0.017)	(0.030)	(0.066)
ROA_{t-s}		0.4971	0.6948	1.6132		0.0668	3.3998*	7.0303***
		(0.983)	(1.851)	(2.260)		(1.237)	(1.806)	(2.122)
Liquidity_{t-s}		−0.0016	0.0461	0.0290		0.0782	0.1346*	0.2913**
		(0.061)	(0.073)	(0.152)		(0.059)	(0.065)	(0.114)
RWA_{t-s}		−0.0650	−0.1208	−0.2489		−0.0126	−0.0756	−0.1604
		(0.046)	(0.086)	(0.161)		(0.031)	(0.069)	(0.157)
Demand_{t-s}	−0.0101	−0.0017	−0.0046	0.0032	−0.0059	0.0057	−0.0160	−0.0066
	(0.034)	(0.032)	(0.026)	(0.023)	(0.066)	(0.067)	(0.031)	(0.035)
Observations	550	550	544	516	550	550	544	516
R^2	0.332	0.415	0.485	0.516	0.543	0.565	0.627	0.655
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighted regressions	No	No	No	No	Yes	Yes	Yes	Yes



(a) Unweighted regressions

(b) Weighted regressions

Fig. 8. Impulse response functions. *Notes*: the charts trace the response of loan growth over four quarters as a function of a change in the capital requirement at the beginning of the period ($\Delta KR^i_{t,t-1}$). More formally, the charts plot the estimated β coefficients (solid line) and the associated 95% confidence intervals (dotted lines) from this set of regressions at the bank (i) and quarter (t) level, with $k = 0, \dots, 3$: $\Delta LOAN^i_{t+k,t-1} = \beta_k \Delta KR^i_{t,t-1} + \gamma' \mathbf{X}^i_{t-1} + \alpha D^i_t + \phi^i + \tau_t + \epsilon^i_t$ The set of control variables **X** and loan demand are defined as in the baseline model, see Eq. (1) and Table 2. Standard errors are clustered at the bank level.

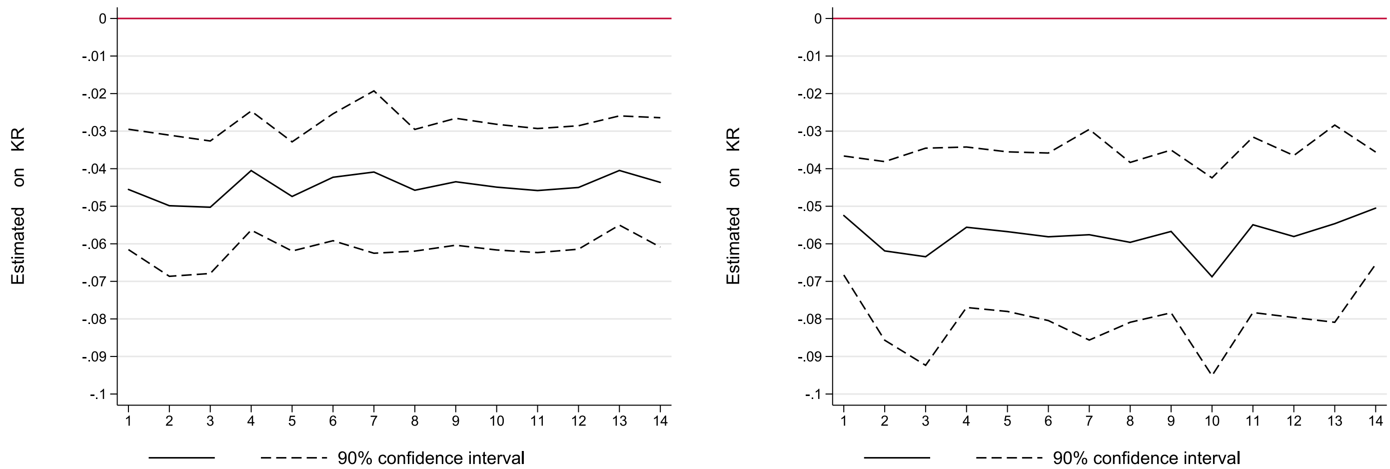
Table 3
Robustness exercises. *Notes*: The table presents OLS estimates of model (1). The dependent variable is loan growth at the bank-quarter level, calculated as log difference between $t + r$ and $t - s$. The top and bottom panels report, respectively, unweighted and (asset) weighted regressions. In column 1 loan growth is computed using net loans, while in column 2 it is computed using gross loans with the Davis and Haltiwanger (1992) measure, see Eq. (3). Columns 3 and 4 exclude, respectively, the demand variable and excess capital. Column 5 includes bank-specific linear trends. Column 6 reports results only for the sample between 2010 and 2016. The set of bank control variables is the same as in the baseline model (Table 2, columns 3–4) and the variables, measured at $t - 1$, are defined in Table 1. Standard errors, clustered at the bank level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Dep. Var.: $\Delta LOAN_{t,t-1}$	(1) Net loans	(2) DH	(3) No demand	(4) No capital	(5) Trends	(6) Post-2009
	Unweighted regressions					
$\Delta KR_{t,t-1}$	−0.0339*	−0.0409***	−0.0410***	−0.0416***	−0.0409**	−0.0401**
	(0.017)	(0.013)	(0.013)	(0.013)	(0.014)	(0.015)
Observations	550	550	550	550	550	385
R^2	0.413	0.416	0.415	0.413	0.415	0.322
			Weighted regressions			
$\Delta KR_{t,t-1}$	−0.0632***	−0.0642***	−0.0642***	−0.0640***	−0.0627***	−0.0588***
	(0.018)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)
Observations	550	550	550	550	550	385
R^2	0.552	0.565	0.565	0.565	0.566	0.427
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank time trends	No	No	No	No	Yes	No

sive margins of lending.[16] Then, we alternatively drop from the set of control variables the measure of demand (column 3) and the excess capital variable (column 4), to rule out the possibility that it can confound the coefficient on ΔKR, as banks can start holding capital before the implementation date. This does not seem to be the case, as the coefficient of excess capital is never significant in the baseline (Table 2). However, it is reassuring to observe that the point estimate of the effect of ΔKR is barely unchanged when dropping either demand or excess capital. Results are also robust to the inclusion of bank-specific time trends to further control for unobserved factors that could generate differences in loan growth across banks (column 5). Finally, we drop the period with the uniform capital requirements and restrict the sample period to 2010–2016. The results show that the estimates of the contemporaneous effect are stable, consistently implying a drop in lending of between 3 and 6 percentage points in response to one percentage point increase in capital requirements (column 6).

We further test the robustness of our main result to the sample composition by estimating Eq. (1) and dropping one bank at the time, to be sure that the average result is not driven by any particular bank. Given the relative concentration of the banking system in Peru and the presence of 4 large banks (Table A1), this could be a relevant concern. However, the estimate of the coefficient that measures the contemporaneous association between the change in capital requirement and loan growth—plotted in Fig. 9 for

[16] Similar results hold when considering the ratio of interbank funding over total liabilities. We have also run other exercises to look at potential sources of heterogeneity across foreign ownership, bank size, revenue mix (the share of non-interest income to total income), portfolio concentration (computing the Herfindahl index across 14 sectors) and specialization in consumer lending, mortgages or SMEs lending, without finding significant and robust results.



(a) Unweighted regressions　　(b) Weighted regressions

Fig. 9. Response to capital requirements excluding one bank at a time. *Notes*: The charts show the estimated coefficients on ΔKR and the 95% confidence intervals, for 14 different banks, each obtained estimating Eq. (1), as reported in Table 2, columns 3 and 4 and excluding one bank at the time, reported on the x-axis. Panel a reports the estimates of unweighted regressions (as in Table 2, column 3), while panel b refers to weighted regressions (as in Table 2, column 4).

unweighted and weighted regressions—remains stable at around 0.04 and is always statistically significant.

5.3. The role of economic conditions

Our baseline results point to a relatively small and short-lived effect of higher capital requirements on bank lending. However, theory suggests that the effect of higher capital requirements can be stronger in less buoyant economic conditions (Kashyap and Stein, 2004; Repullo and Suarez, 2012). Here we test this hypothesis. First, we split the sample identifying banks facing low and high credit demand depending on the bank-specific (time-varying) demand variable being below or above the sample median, respectively. Then, we focus on aggregate economic conditions and we create a dummy that identifies the low growth period between 2014:q1 and 2015:q1, when real GDP growth averaged 1.7% (compared to 4.2% in the other quarters since the introduction of bank-specific capital requirements; see Fig. 2).

In both cases, we find that the average on-impact effect of changes in capital requirements on lending is significantly stronger during downturns (Table 4). In particular, one percentage point increase in capital requirements is associated with a reduction in loan growth of about 8 percentage points in the same quarter when banks face lower demand (column 1) or in period of low growth (column 2), compared to a reduction of 3 to 4 percentage points in other periods. These differences are economically sizable and statistically significant, but they do not translate in any longer-term reduction in loan growth, even during phases of economic downturn (results not reported).

Table 4
Capital requirements, lending and economic conditions. *Notes*: The table presents OLS estimates of model (1). The dependent variable is loan growth at the bank-quarter level, calculated as log difference between $t+r$ and $t-s$. The set of bank control variables is the same as in the baseline model (Table 2, columns 3–4) and the variables, measured at $t-1$, are defined in Table 1. In column 1, the sample is split in high vs low demand according to the bank-specific demand variable being above/below the first quartile of the sample distribution. In columns 2, the low growth dummy is equal to 1 for the period 2014:q1-2015:q1 and zero otherwise. The last row reports the p-value of a t-test for the equality of the coefficient of ΔKR across the high vs low categories. Standard errors, clustered at the bank level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Dep. Var.: $\Delta LOAN_{t,t-1}$	(1)	(2)
$\Delta KR_{t,t-1}$, high demand	−0.0427***	
	(0.013)	
$\Delta KR_{t,t-1}$, low demand	−0.0797***	
	(0.013)	
$\Delta KR_{t,t-1}$, high growth		−0.0342*
		(0.016)
$\Delta KR_{t,t-1}$, low growth		−0.0832**
		(0.034)
Observations	550	550
R^2	0.420	0.416
Bank controls	Yes	Yes
Bank fixed effects	Yes	Yes
Quarter fixed effects	Yes	Yes
Test of equality (p-value)	0.0453	0.2785

5.4. The role of bank characteristics and performance

So far, we have treated the effects of increased capital requirements as homogeneous across banks. However, some banks could be more sensitive than others. First, while unprofitable banks can improve their capital adequacy only by compressing lending or issuing expensive new equity, highly profitable banks can simply retain more earnings, making the transition to higher capital requirements easier, while muting the impact on credit (Cohen and Scatigna, 2016). Second, the cost of capital may be higher for less capitalized banks, which could have a greater incentive to reduce lending. To test these hypotheses we split the sample into high and low profitability banks, depending on ROA being above or below the first quartile of the sample distribution, respectively. Similarly, we define low and high capital banks splitting the sample around the first quartile of the excess capital variable (i.e., the capital adequacy ratio minus capital requirement). Then, we split banks depending on whether they had or not positive retained earnings at $t-1$.

The results, reported in columns 1 to 3 of Table 5 suggest that less profitable banks are more sensitive to changes in capital requirements, consistent with the evidence shown on Belgian banks by De Jonghe et al. (2020). The estimates reported in the first column shows that, for banks in the bottom quartile of the ROA distribution, the contemporaneous contraction of quarterly loan growth in response to a one percentage point increase in

Table 5
Bank lending, capital requirements and bank characteristics. *Notes*: The table presents OLS estimates of model (1). The dependent variable is loan growth at the bank-quarter level, calculated as log difference between $t+r$ and $t-s$. The set of bank control variables is the same as in the baseline model (Table 2, columns 3–4) and the variables, measured at $t-1$, are defined in Table 1. The sample is split in high vs low ROA (column 1), capital (column 2), deposits (column 4) and liquidity (column 5) depending on the continuous variable being above/below the first quartile of the sample distribution. Excess capital is the ratio of the difference between total regulatory capital held and total capital requirement over risk-weighted assets. Deposits are measured by the ratio of retail deposits over total liabilities. In column 2 the sample is split between banks with positive retained earnings at $t-1$ and those with negative or zero retained earnings. In column 6 the sample is split between banks with the share of branches in the city capital (Lima) above (low diversification) or below (high diversification) the sample median. The last row reports the p-value of a t-test for the equality of the coefficient of ΔKR across the high vs low categories. Standard errors, clustered at the bank level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Dep. Var.: $\Delta LOAN_{t,t-1}$	(1)	(2)	(3)
$\Delta KR_{t,t-1}$, high ROA	−0.0444***		
	(0.014)		
$\Delta KR_{t,t-1}$, low ROA	−0.0700***		
	(0.011)		
$\Delta KR_{t,t-1}$, no retained earning		−0.0796***	
		(0.010)	
$\Delta KR_{t,t-1}$, positive retained earnings		−0.0521***	
		(0.010)	
$\Delta KR_{t,t-1}$, high excess capital			−0.0419**
			(0.015)
$\Delta KR_{t,t-1}$, low excess capital			−0.0606***
			(0.016)
Observations	550	525	550
R^2	0.419	0.387	0.416
Test of equality (p-value)	0.0621	0.0480	0.0798
Dep. Var.: $\Delta LOAN_{t,t-1}$	(4)	(5)	(6)
$\Delta KR_{t,t-1}$, high deposits	−0.0374**		
	(0.016)		
$\Delta KR_{t,t-1}$, low deposits	−0.0309*		
	(0.016)		
$\Delta KR_{t,t-1}$, high liquiidty		−0.0262	
		(0.018)	
$\Delta KR_{t,t-1}$, low liquidity		−0.0409***	
		(0.009)	
$\Delta KR_{t,t-1}$, low diversification			−0.0506***
			(0.012)
$\Delta KR_{t,t-1}$, high diversification			−0.0320**
			(0.011)
Observations	550	550	472
R^2	0.428	0.416	0.416
Bank controls	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes
Test of equality (p-value)	0.4760	0.2629	0.0728

capital requirements is 7%. For high profitability banks this number declines to 4.4% and the difference between the two subsamples is statistically significant. Consistent with this result, the estimates reported in column 2 show that lending by banks with positive retained earnings reacts significantly less (almost 3 percentage points) to changes in capital requirements than lending by other banks. When looking at bank capital, results show that low capital banks respond to an increase in capital requirements by cutting lending more aggressively than high capital banks. This result is in line with the international evidence discussed by Deli and Hasan (2017), who show that the negative effect of capital stringency on loan growth becomes quite low for well-capitalized banks. The difference is around 2% and it is statistically significant (column 3). Overall, these results are consistent with the evidence on a large sample of banks in advanced and emerging economies which shows that most banks have been able to increase capital ratios post-GFC through the accumulation of retained earnings and that more profitable and more capitalized banks increased lending more than other banks (Cohen and Scatigna, 2016).

Next, we look at the funding structure. We identify banks with high or low reliance on retail deposits by splitting the sample around the first quartile of the ratio of retail deposits over total liabilities. We find that the response of credit to changes in capital requirements is the same across banks with a different funding structure (column 3).[17]

Moving to the asset side of the balance sheet, we split the sample between low and high liquidity banks, depending on the ratio of liquid assets over total assets being below or above the sample median. In this case, although the test of equality suggests no significant differences between low and high liquidity banks, we find that the negative response of credit to changes in capital requirements is significant only for low liquidity banks (column 5). This result is in line with the international evidence shown by Deli and Hasan (2017) and consistent with the view that more liquid banks can protect their loan portfolio drawing down on their securities portfolio, in a similar way to which they react to tighter monetary policy (Kashyap and Stein, 2000).

Finally, we consider a measure of geographical diversification. Lacking granular data on the geographical allocation of lending, we

[17] Similar results hold when considering the ratio of interbank funding over total liabilities. We have also run other exercises to look at potential sources of heterogeneity across foreign ownership, bank size, revenue mix (the share of non-interest income to total income), portfolio concentration (computing the Herfindahl index across 14 sectors) and specialization in consumer lending, mortgages or SMEs lending, without finding significant and robust results.

construct a dichotomous measure which identifies low and high diversification banks as those with a share of branches located in Lima (the capital of Peru) above and below the sample median, respectively. The results indicate that the response of lending to changes in capital requirements is stronger for banks which have a branch network more concentrated in the city capital (column 6), perhaps because they have less geographically diversified income streams.

5.5. Addressing the endogeneity of capital requirements

The key challenge to tackle in identifying the effect of bank capital on lending is the endogeneity of bank-specific capital requirements to bank portfolio composition.[18] For identification, we only use bank-specific jumps in capital requirements resulting from annual step-ups in reform implementation, and not the endogenous monthly wiggles. Still, even though these step-ups were exogenous in terms of percentage-points of the formula, the associated jumps in capital requirements were not: by adjusting the composition of their balance sheets, banks could affect the size of the jumps in terms of capital over RWAs. We address this concern in three steps. First, we show two exercises that illustrate the lack of (i) anticipation effects, and (ii) strategic behavior by banks in adjusting the composition of their balance sheets to reduce their buffer requirements. Then, we conduct the analysis at the product level, exploiting more granular data and plausibly more exogenous product-specific capital surcharges. Finally, we shift our attention on the announcement of the first reform of minimum capital requirements, which was arguably less anticipated and therefore less subject to endogeneity concerns.

Anticipation effects We start by estimating an alternative regression specification, which considers the effect of higher capital requirements on quarterly loan growth, while progressively allowing for longer adjustment periods, also ahead of the implementation date. Specifically, we estimate the following regression equation:

$$\Delta LOAN^{i}_{t,t-1} = \sum_{s=-n_l}^{+n_f} \beta_s \Delta KR^{i}_{t+s,t+s-1} + \gamma' \mathbf{X}^{i}_{t-1} + \alpha D^{i}_{t} + \phi^{i} + \tau_t + \epsilon^{i}_{t} \tag{4}$$

On the left-hand size we have quarterly loan growth. On the right-hand side, we have lags and forwards to allow for gradual adjustment, lasting from n_f periods before the increase until n_l periods after. OLS estimates for specification (4) are reported in Table 6, which looks at unweighted (columns 1–4) and weighted (columns 5–8) regressions. The first specification reflects the contemporaneous negative effect of a jump in buffer requirements on credit growth, i.e., during the first three months after the rise came into effect. Then, from columns 2 to 4, the number of lags and forwards on ΔKR is progressively increased such that, in the last column, banks can respond to increases in capital requirements up to one year in advance and can continue adjusting for up to one year after the requirements have come into force.

Results indicate that the negative effect of a rise in capital requirements on contemporaneous loan growth survives the introduction of lags and forwards. There is also some evidence of a lagged effect, consistent with the baseline results. However, there is no evidence of anticipation effects, suggesting that, even though the timing of the changes in capital requirements is known, banks do not start adjusting lending before the actual implementation date. When we consider the joint impact of lags and leads there is no significant aggregate effect of a change in buffer requirements on quarterly loan growth. The last two rows of the table reveal that the joint impact of lags and leads, measured by the sum of coefficients β_s, is never statistically different from zero. This means that, statistically speaking, the negative contemporaneous effect is washed out by changes in loan growth that take place in the run up to and after the change in requirement, consistent with the baseline results.

While the lack of anticipation effects provide some reassuring evidence that banks do not adjust their capital position ahead of higher capital requirements, in the following we discuss two additional exercises to deal with any lingering concern that banks may adjust the composition of their portfolio in anticipation of the capital requirements.

Counter-factual capital requirements As a first, descriptive, approach, we calculate the counter-factual capital requirements and look at their dynamics around the implementation of the reform. Virtual buffers are the counter-factual buffers the banks would have faced if the compulsory capital buffer would have been fixed at 40% before July 1, 2012 the first implementation date. If counter-factual capital requirements had been stable up to the announcement in June 2011 but started trending down between the announcement and the implementation date, it would indicate that banks strategically adjusted the composition of their balance sheets to reduce their buffer requirements. Such endogenous reaction by banks would complicate the interpretation of our results, as part of the effect of the introduction of higher capital requirements would have already been reflected in bank assets before the implementation date. However, Fig. 10 suggests, and formal tests confirm (do not reject), that average virtual buffers before and after the announcement were the same, and that there were no downward trends in buffers between announcement and implementation dates. This is reassuring, as it suggests that, in practice, banks did not strategically adjust the composition of their balance sheet, limiting the endogeneity bias of our estimates.

Product-level analysis In Peru, the counter-cyclical capital buffer requirement (CCyB) is product-specific.[19] To calculate a bank's CCyB, every asset is assigned to one of 13 buckets, each of which is associated with an additional risk-weight between zero and 55%. Mortgage loans, for example, are assigned to the 15 percent-bucket. This means that a bank must hold 15% × 10% (the uniform minimum) = 1.5% of the mortgage value as additional capital. This capital surcharge applies both to the flow of new mortgages as well as to the stock of mortgages already on the balance sheet. Assuming capital is more expensive than debt, the CCyB increases the cost of mortgage lending less (more) than it does for other products, which falls in buckets with larger (smaller) capital surcharges.

Our second approach exploits this feature of the CCyB and the granularity of the data, which include outstanding gross loans

[18] The endogeneity of capital requirements would bias our estimates upward. To see this, suppose that the formula required a bank to hold an x percent buffer, before undertaking any adjustments to the composition of its balance sheet. Also suppose that this would have reduced the bank's credit growth by y percentage points. The (exogenous) effect of a one percentage point increase in capital on credit growth was then equal to $\beta = -y/x$. Notice that this is the number we are actually interested in. Now suppose that, by adjusting the composition of its balance sheet, the bank was able to reduce the size of its buffer requirement from x to γx percent ($0 < \gamma \leq 1$). In the best of cases, this balance sheet adjustment had no negative impact on the bank's ability to grow. The reduction of the fall in the bank's credit growth was then fully proportional, such that credit growth also fell by γy—rather than y—percentage points. In that case, the estimate $\hat{\beta}$ for the effect of a one percentage point increase in capital on credit growth is: $\hat{\beta} = -\gamma y/\gamma x = -y/x$, which is equal to β. In all other cases, in the process of adjusting the composition of its balance sheet, the bank would have foregone at least some growth opportunities. The reduction of the fall in the bank's credit growth would then be strictly less than proportional, such that the fall in credit growth, μy, was strictly greater than γy; i.e., $\mu > \gamma$. The estimate then becomes $\hat{\beta} = -\mu y/\gamma x > -y/x = \beta$.

[19] The loan portfolio is disaggregated in non-revolving consumption (term loans), revolving consumer (e.g., credit cards), commercial loans, real estate loans, loans to micro, small, medium and large firms, loans to the financial system, loans to the public sector, loans to multilateral banks, and loans to sovereigns, see Table A4 in the Appendix.

Table 6

Alternative model specification: leads and lags. *Notes*: The table presents OLS estimates of model (4). The dependent variable is loan growth at the bank-quarter level, calculated as log difference between t and $t-1$. The set of bank control variables is the same as in the baseline model (Table 2, columns 3–4) and the variables, measured at $t-1$, are defined in Table 1. Weighted regressions use total assets as weight. The last two rows report the cumulative effect of ΔKR, and the associated p-value of a t-test for this effect to be equal to zero. Standard errors, clustered at the bank level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Dep. Var.: $\Delta LOAN_{t,t-1}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta KR_{t,t-1}$	−0.0410***	−0.0311*	−0.0359*	−0.0343*	−0.0642***	−0.0628***	−0.0706***	−0.0636***
	(0.013)	(0.017)	(0.019)	(0.019)	(0.016)	(0.020)	(0.018)	(0.020)
$\Delta KR_{t-1,t-2}$		0.0578*	0.0530	0.0503**		0.0162	0.0083	−0.0107
		(0.032)	(0.031)	(0.022)		(0.041)	(0.039)	(0.039)
$\Delta KR_{t-2,t-3}$			−0.0434***	−0.0416***			−0.0427**	−0.0376*
			(0.012)	(0.013)			(0.019)	(0.019)
$\Delta KR_{t-3,t-4}$				−0.0375				−0.0173
				(0.031)				(0.032)
$\Delta KR_{t+1,t}$		0.0262	0.0213	0.0400		−0.0061	−0.0141	0.0020
		(0.020)	(0.022)	(0.023)		(0.042)	(0.042)	(0.038)
$\Delta KR_{t+2,t+1}$			0.0070	0.0079			−0.0115	−0.0065
			(0.028)	(0.027)			(0.024)	(0.025)
$\Delta KR_{t+3,t+2}$				0.0132				0.0416
				(0.024)				(0.034)
Observations	550	550	550	537	550	550	550	537
R^2	0.415	0.425	0.429	0.436	0.565	0.565	0.567	0.568
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighted regressions	No	No	No	No	Yes	Yes	Yes	Yes
Cumulative effect of ΔKR		0.0530	0.0019	−0.0020		−0.0305	−0.1306	−0.0920
p-value		0.3215	0.9790	0.9810		0.5299	0.1050	0.3992

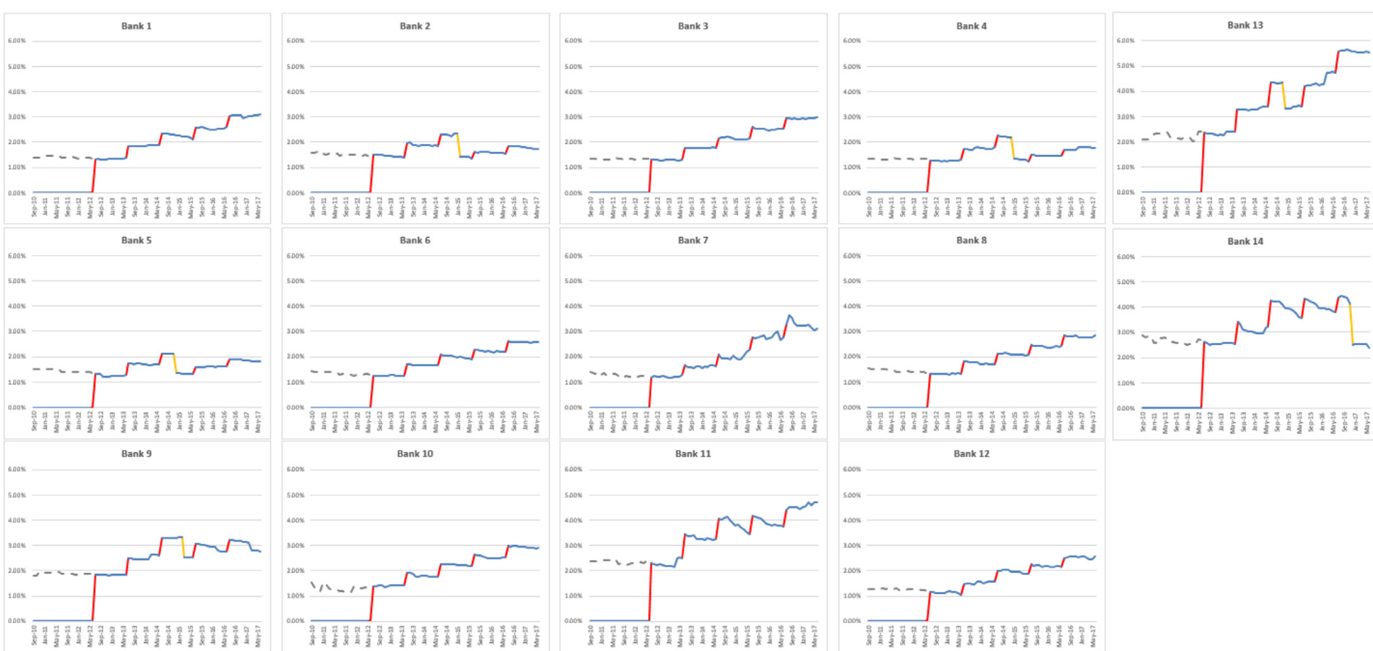


Fig. 10. Capital buffers: virtual and actual. *Notes*: Virtual capital buffers are plotted in the dashed grey line, "wiggles" in the blue solid line, and "jumps" in the red solid line. Drops (in yellow) reflect the release of 60% of a bank's CCyB, which occurred upon the depletion of its counter-cyclical provisions. Since the timing of drops was endogenous, downward jumps are not used for identification. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

for 13 different loan products, to run a set of regressions at the bank-product level. The availability of product-level data gives us a key advantage to better tackle endogeneity of capital requirements, compared to the bank-level regressions. Keeping the same approach as before, we can modify our baseline model—Eq. (1)—to estimate if, and for how long, loan products with higher capital surcharges grew more slowly than products with lower surcharges in response to an increase in the CCyB. Thus, we estimate the following model exploiting within-bank variation:

$$\Delta LOAN^{i,j}_{t+r,t-s} = \beta_{r,s}\Delta CS^{j}_{t,t-1} + \gamma'\mathbf{X}^{i}_{t-s} + \alpha D^{i}_{t} + \phi^{i}\times\eta^{j} + \tau_t + \epsilon^{i,j}_{t} \quad (5)$$

where the dependent variable, $\Delta LOAN^{i,j}_{t+r,t-s}$, is the log-difference in the stock of gross loans of product j held by bank i between the end of quarters $t+r$ and $t-s$. The independent variable of interest, $\Delta CS^{j}_{t,t-1}$, is the change in the product-specific capital surcharge percentage between quarters t and $t-1$. We start by including the same set of bank-specific control variables as in the baseline, together with the proxy for loan demand D^{i}_{t}. Unobserved heterogeneity across bank-product pairs—which may capture bank specialization—is absorbed by bank × product ($\phi^{i}\times\eta^{j}$) fixed effects, while quarter (τ_t) fixed effects control for common shocks, including the timing of the CCyB (de)activation. Then, we take advantage of the additional dimension of the dataset to in-

Table 7
Product-level regressions. *Notes*: The table presents OLS estimates of model (5). The dependent variable is loan growth at the bank-product-quarter level, calculated as log difference between $t+r$ and $t-s$. ΔCS^j is the quarter-on-quarter change in the product-specific capital surcharge. Bank control variables, indexed by i, are defined in Table A3. Weighted regressions use total assets as weight. Standard errors, clustered at the bank-product level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Dep. Var.: $\Delta LOAN^{i,j}_{t+r,t-s}$ $(r,s) =$	(1) (0,1)	(2) (0,1)	(3) (1,1)	(4) (1,1)	(5) (0,1)	(6) (0,1)	(7) (1,1)	(8) (1,1)
$\Delta CS^j_{t,t-1}$	−0.0785*	−0.0881**	−0.0591	−0.0583	−0.0387**	−0.0399*	−0.0083	−0.0091
	(0.042)	(0.043)	(0.051)	(0.054)	(0.019)	(0.021)	(0.020)	(0.027)
$CAR-KR^i_{t-s}$	0.5030		0.4006		−0.0865		0.2297	
	(0.559)		(0.795)		(0.328)		(0.619)	
Assets^i_{t-s}	−0.0075		−0.1159		−0.0368		−0.1283***	
	(0.074)		(0.131)		(0.040)		(0.048)	
ROA^i_{t-s}	−9.8241		1.4256		2.7540		8.1099	
	(9.761)		(7.899)		(3.815)		(5.249)	
Liquidity^i_{t-s}	0.3556*		0.7366**		0.0235		0.1273	
	(0.194)		(0.363)		(0.092)		(0.111)	
RWA^i_{t-s}	0.3369		0.3718		−0.1112		−0.1949	
	(0.231)		(0.388)		(0.163)		(0.290)	
Demand^i_{t-s}	0.6106**		−0.1352		0.0818		−0.0032	
	(0.245)		(0.095)		(0.080)		(0.052)	
Observations	2741	2741	2620	2620	2741	2741	2608	2608
R^2	0.070	0.173	0.102	0.210	0.148	0.194	0.138	0.193
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Quarter fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Quarter fixed effects	Yes	–	Yes	–	Yes	–	Yes	–
Weighted regressions	No	No	No	No	Yes	Yes	Yes	Yes

clude bank × quarter fixed effects, to control for all time-varying bank-specific factors which could drive lending. In this case, the identification is within bank and exploits the differential capital surcharges across loan products. Demand for credit is controlled for as long as we assume that banks face similar (bank-specific, time-varying) shifts in the demand for loans across products. Moreover, given that the capital surcharges are applied to the stock of outstanding loans, the size of product-specific surcharges could be considered exogenous, under the plausible assumption that banks cannot change the allocation of their outstanding loan portfolio in anticipation of the activation of the CCyB.

Table 7 reports the results looking at the contemporaneous (columns 1–2 and 4–5) and lagged (over two quarters, columns 3–4 and 7–8) effects on loan growth. Unweighted and (asset) weighted regressions are presented in columns 1–4 and 5–8, respectively. Standard errors are clustered at the bank-product level, to allow for autocorrelation within bank-product pairs. As in the bank-level analysis, also the product level regressions show that an increase in capital requirements has a negative contemporaneous effect on lending, although this effect does not extend beyond one quarter.[20] Importantly, the contemporaneous negative effect of capital surcharges is robust to the inclusion of bank × quarter fixed effects (columns 2 and 6) and and it is economically large: one percentage point increase in capital is associated with a 8.8% decline in lending (column 2), even though this effect declines to 4% when using weighted regressions (column 6).

First reform of capital requirements Our baseline analysis has focused on the second reform of capital requirements, which has the advantage of setting bank-specific requirements. However, that reform could have been at least partly anticipated, in light of the uniform capital increases announced in July 2008. While we provide evidence suggesting no anticipation effects, to further corroborate our findings, here we zoom in on the announcement of the first reform of capital requirements, which could arguably be considered unexpected.[21] While the new minimum capital requirements are common to all banks, we can still measure the relative exposure of banks to the policy comparing the *ex-ante* actual capital with the requirement, under the assumption that the uniform capital requirement would be more binding for banks with actual capital closer to the requirement. Of course, one limitation of this approach is that we can look at the heterogeneous impact of the first reform, but not its overall impact.

A visual inspection of bank lending around the announcement and the implementation of the first phase of the reform shows that real credit grew less for banks more exposed to the increase in the capital requirement. Splitting the sample of banks around the median of the ratio of total regulatory capital over RWA in June 2008, Fig. 11 shows that real credit grew at similar rates across the two samples before the announcement (and even for the subsequent 2 quarters), while it contracted and then grew less for more exposed banks (e.g., those less capitalized in June 2008) around the implementation date in mid-2009. Then, by the end of 2009, real credit started growing again at a similar rate for the two groups of banks.[22]

Formally, we can look at differential loan growth post-reform for banks *ex-ante* more or less capitalized by estimating the following difference-in-difference model over the period 2007:q1–2009:q4 and around the announcement date:

$$\Delta LOAN^i_{t,t-1} = \beta EXPOSURE^i \times POST_t + \gamma' \mathbf{X}^i_{t-1} + \phi^i + \tau_t + \epsilon^i_t \qquad (6)$$

where the dependent variable is loan growth between the end of quarters t and $t-1$, and the vector $\mathbf{X}^i_{t-1}$ includes the standard set of lagged control variables as in the baseline. The coefficient of interest is β, which measures the difference in loan growth before and after the announcement of the reform (*POST* is a dummy variable equal to 1 after 2008:q2 and 0 otherwise) for banks more or less exposed to the new requirements. In particular, we

[20] The estimation of the impulse response functions confirm the presence of an effect on impact, while capital surcharges have no statistically significant effect on lending in the subsequent quarters, see Figure A2 in the Appendix.

[21] In particular, while the change from 9.1% to 9.5% could have been unexpected, the other staggered increases to finally meet a 10% requirement by 2011 were announced at the start of the reform in July 2008 and hence are less likely to have been unexpected.

[22] Measuring more exposed banks as those in the bottom 75% of the distribution delivers qualitatively similar results, see Figure A3 in the Appendix.

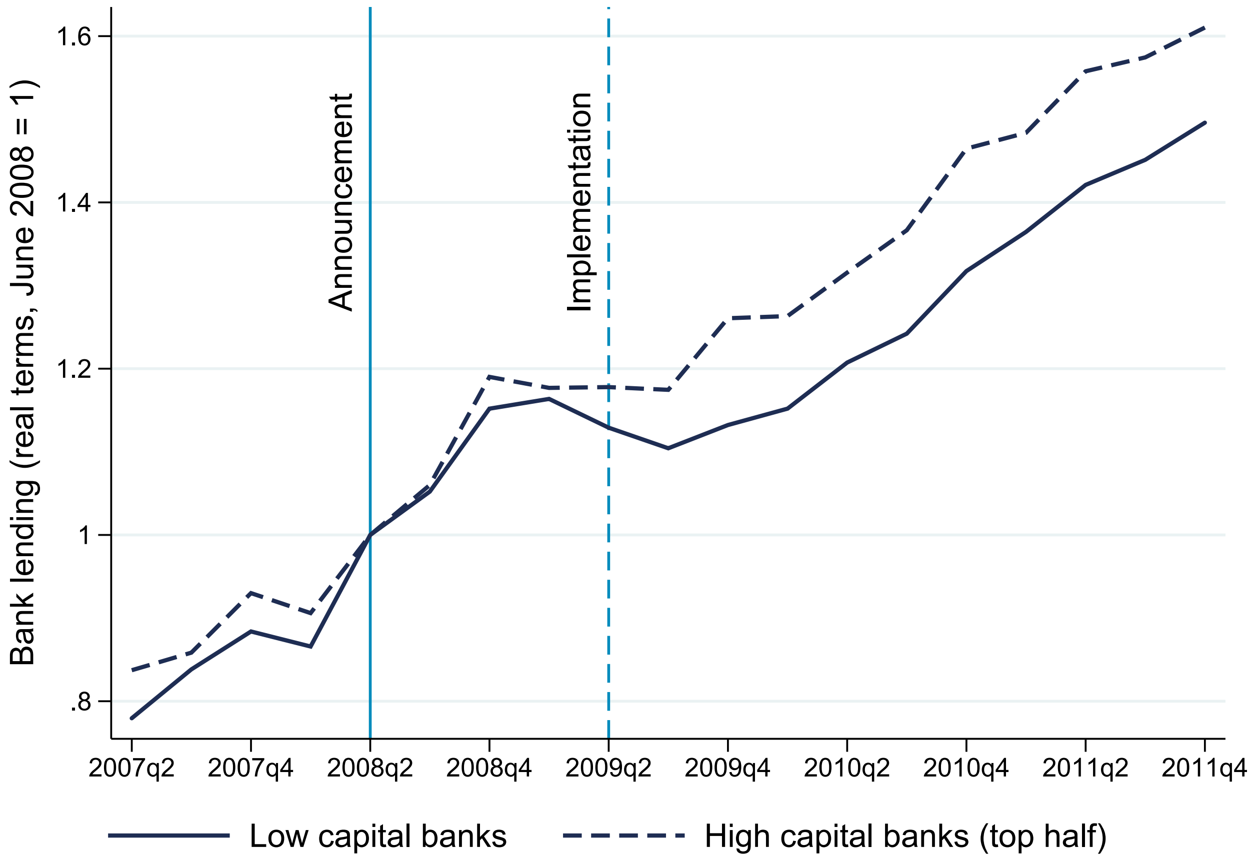


Fig. 11. The first reform of capital requirements and bank lending. *Notes*: The chart plots gross loans (in real terms) and normalized to 1 in 2008:q2 separately for banks with total regulatory capital over RWAs in 2008:q2 below (low capital banks) and above (high capital banks) the sample median. The vertical lines indicate the announcement (July 2008, solid line) and the first implementation date (July 2009, dotted line) of the first phase of the increase in capital requirements.

Table 8
The first reform of capital requirements. *Notes*: The table presents OLS estimates of model (6). The dependent variable is loan growth at the bank-quarter level, calculated as log difference between t and $t-1$. *POST* is a dummy variable equal to 1 after 2008:q2 and 0 otherwise. Excess capital is defined as the difference between total regulatory capital over RWA in June 2008 and the 10% new capital requirement. High capital are dummy variables identifying high capital banks as those with total regulatory capital over RWA in June 2008 above the median (top 50) or the first quartile (top 25) of the sample distribution. Bank control variables, indexed by i, are defined in Table A3. The sample covers the period from 2007:q1 to 2009:q4. Weighted regressions use total assets as weight. Standard errors, clustered at the bank level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Dep. Var.: $\Delta LOAN_{t,t-1}$	(1)	(2)	(3)	(4)	(5)	(6)
Excess capital$^i_{2008:q2}$ × Post$_t$	0.0062			0.0073**		
	(0.005)			(0.003)		
High capital$^i_{2008:q2}$ (top 50) × Post$_t$		0.0337**			0.0212**	
		(0.013)			(0.007)	
High capital$^i_{2008:q2}$ (top 25) × Post$_t$			0.0223			0.0319**
			(0.014)			(0.011)
Assets$^i_{t-1}$	−0.1004***	−0.1083***	−0.1059***	−0.1137***	−0.1187***	−0.1319***
	(0.020)	(0.018)	(0.024)	(0.030)	(0.029)	(0.030)
ROA$^i_{t-1}$	−0.1755	−0.2108	−0.1289	−2.6378	−2.7509	−2.2014
	(0.491)	(0.448)	(0.506)	(1.910)	(1.907)	(1.857)
Liquidity $^i_{t-1}$	0.1180	0.1168	0.1243	0.3295	0.3308	0.3114
	(0.240)	(0.229)	(0.247)	(0.237)	(0.235)	(0.243)
RWA $^i_{t-1}$	−0.1242	−0.1350	−0.1291	−0.1408**	−0.1480**	−0.1268**
	(0.118)	(0.117)	(0.115)	(0.055)	(0.056)	(0.049)
Observations	139	139	139	139	139	139
R^2	0.744	0.754	0.743	0.803	0.805	0.799
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Weighted regressions	No	No	No	Yes	Yes	Yes

measure the exposure to the policy by: (i) a continuous measure of excess capital defined as the difference between total regulatory capital over RWA in June 2008 and the new capital requirement; (ii) dummy variables identifying high capital banks as those with total regulatory capital over RWA in June 2008 above the median or the first quartile of the sample distribution.

Results—reported in Table 8—consistently show that loan growth post-reform is higher for *ex-ante* more capitalized banks, especially when using weighted regressions (columns 4–6). Considering that the uniform increase of capital requirement is 0.9 percentage point (the minimum capital requirement increased from 9.1 to 10%), the results of the last two columns of Table 8 show that the relative reduction in loan growth for less capitalized banks is between 2 and 3%, about a half of the baseline estimates for the second phase of the reform (see Table 2, column 6).

Overall, these results indicate that the unexpected announcement of higher uniform capital requirements slowed down lending by less capitalized banks, even though the effect is relatively small and temporary (as suggested by Fig. 11).

6. Conclusions

While the literature has examined extensively the effect of capital requirements on lending in advanced economies, there is not much evidence for emerging markets. We offer novel evidence based on the case of Peru, a representative emerging market economy. We also contribute to the literature by exploring heterogeneous effects depending on bank characteristics and economic conditions, an issue that has received limited attention but that could influence the impact of capital requirements on lending. Our findings indicate that increased capital requirements have only a temporary effect on lending. Our estimates suggest that a one percentage point increase in required capital buffers is associated with a drop in lending growth by between 4 and 6 percentage points in the quarter in which it comes into effect. Furthermore, over periods of six months and beyond, loan growth does not statistically differ between periods with and without capital increases. Hence, we cannot reject the hypothesis that, after as little as half a year, credit growth is back where it would have been in the absence of a rise in capital requirements.

The apparently low cost of adjusting to higher capital levels in Peru may be due to several factors: the early announcement of reforms, the relatively slow speed of implementation, the solid performance of the economy, and bank characteristics and performance. The reforms were officially announced one year before implementation started, allowing banks time to prepare. Implementation was spread over four years, allowing for a smooth adjustment. However, we do not find evidence of anticipation effects, neither we observe a systematic early adjustment of bank capital to the level implied by the future capital requirements. In addition, when looking at the first reform of capital requirements, whose announcement was arguably more unexpected, we still find evidence of a relatively modest (and temporary) contraction of bank lending. By contrast, our results show that the adjustment is stronger for less liquid, lower capitalized and profitable banks and during periods of less buoyant economic conditions. This evidence suggests that strong economic conditions and bank balance sheet health and performance could be a key factor explaining the short-lived and relatively small reaction of lending to the implementation of higher capital requirements.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.jbankfin.2020.105806.

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