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The Effects of Higher Bank Capital Requirements on Credit in Peru

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Abstract

This paper offers novel evidence on the impact of raising bank capital requirements in the context of an emerging market: Peru. Using quarterly bank-level data and exploiting the adoption of bank-specific capital buffers, we find that higher capital requirements have a short-lived, negative impact on bank credit in Peru, although this effect becomes statistically insignificant in about half a year. This finding is robust to estimating different specifications to address concerns about the exogeneity of capital requirements. The fact that the reform was gradual and pre-announced and that banks were highly profitable at the time could explain the short-lived effects on credit.

JEL Classification Numbers: E44, G21, G28

Keywords: bank credit, bank regulation, capital requirements

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GLOSSARY

CAR	Capital Adequacy Ratio
CCyB	Countercyclical Capital Buffer
FSAP	Financial Sector Assessment Program
FSB	Financial Stability Board
HIC	High-Income Country
MIC	Middle-Income Country
MM	Modigliani and Miller
RWA	Risk-weighted Assests
SBS	Superintendencia de Banca y Seguros

I. INTRODUCTION

The 2008 Global Financial Crisis (GFC) revealed deep weaknesses in banks and triggered a policy response aimed at strengthening bank regulation, supervision and risk management. The new Basel III regulatory framework put forth substantially higher capital requirements—particularly in good times—and has generated a lively debate on the costs and benefits of bank capital (see Aiyar et al. 2015 and Dagher et al. 2016, 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 and the associated large economic costs of 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 financial sector reforms implemented in response to the GFC.

A growing empirical literature has been looking at the effects of changes in capital requirements on bank lending. To address the endogeneity of capital regulation, several studies take advantage of quasi-natural experiments or heterogeneity in capital requirements across banks. Studies on advanced economies show that banks reduce credit supply in response to an increase in capital requirements, even though the magnitude of this effect varies depending on the context (see, among others, Aiyar et al. 2014; Behn et al. 2016; Fraisse et al. 2017; Gropp et al. 2018; Mesonnier and Monks 2014).[2] We contribute to this debate by providing novel evidence in the context of an emerging market—Peru—which gradually introduced higher and bank-specific capital requirements over the period 2009–2016.

Peru makes for an interesting case study for several reasons. First, from an external validity standpoint, as the bulk of the existing evidence is based on advanced economies (mostly in Europe and the United States), it is not straightforward to extend the results to emerging markets. Differences in economic and financial development, as well as in the degree of credit market competition, and bank performance between advanced economies and emerging markets could affect how banks react to changes in capital requirements.[3] In this respect, the analysis of the impact of higher capital requirements in Peru is likely to be of relevance for other economies at a

[2] See Section 2 for a review of the literature on bank capital and lending.

[3] Over the period 2012–16, GDP per capita in Peru was US$5,828, somewhat larger than the average middle-income country (MIC), which stood at US$4,744, but much lower than the average for high income country (HIC) which was US$35,702. Similarly, domestic credit to the private sector was, on average, 33 percent of GDP in Peru, close to the average for MICs (46 percent) but much lower than in HICs (91 percent). The degree of bank capitalization in Peru was also very close to that for MICs (bank capital over total assets was 11.5 percent in Peru and 10.5 percent in MIC), and larger than for HICs (where it was 8.8 percent). Finally, bank profitability is also larger in Peru and MICs (returns on equity equal to 13 percent) than in HICs (8 percent). Data are drawn from the Global Financial Development Database (https://www.worldbank.org/en/publication/gfdr/data/global-financial-development-database).

similar stage of development and with similar banking systems. Second, the design of the capital reform implemented in Peru since 2009 through the gradual introduction of time-varying, bank-specific capital requirements facilitates the empirical identification of the effects of bank capital on lending. Finally, Peru has accumulated rich and high-quality data, including extensive information on bank balance sheets and regulatory requirements, which allows for a detailed empirical analysis.

Our main analysis is based on quarterly bank-level data and exploits the adoption of bank-specific capital buffers to investigate the impact of bank capital requirements on bank lending. These additional requirements are economically sizable and large enough to potentially impact lending: depending on bank characteristics, they could be as high as 5.6 percentage points.

Our results indicate that higher capital requirements have a short-lived, negative impact on bank credit in Peru, which becomes statistically insignificant in about half a year. Controlling for standard bank characteristics and absorbing the effects of bank-specific unobservables and common shocks with bank and time fixed effects, we show 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. However, over periods longer than a quarter, the effect on cumulative credit growth becomes statistically insignificant. This finding is robust to estimating different specifications to address concerns about the exogeneity of capital requirements in Peru and to allow for reform anticipation. We also find some evidence suggesting that less profitable banks are more sensitive to changes in capital requirements than more profitable banks.

Broadly speaking, our findings are consistent with the existing literature for advanced economies and point to a limited impact of changes in capital requirements in Peru. The size of the effect we find is similar to Aiyar et al. (2014), even though, in our data, the effect on lending is shorter-lived than in theirs. In that respect, our results are closer to Noss and Toffano (2016), who find that the effect of a capital requirement increase fades to zero in about a year. Unlike the existing literature, and somewhat surprisingly, we do not find that banks consistently retrench more from loans with higher risk weights. We attribute this difference in results to the fact that the increase in requirements in Peru happened while the economy was growing and banks were highly profitable—the average return on equity in the banking system was around 20 percent and GDP grew at an average annual rate of 4.6 percent during the period of the reform. The limited impact of changes in capital requirements could also be due to their gradual and anticipated implementation. High profitability and long lead times made it relatively easy for banks to satisfy the new requirements by retaining earnings rather than by cutting back on lending.

The rest of the paper is organized as follows. Section 2 reviews the existing literature on capital requirements and bank lending. Section 3 discusses the Peruvian regulatory reforms. Section 4 introduces a simple conceptual framework to analyze the effect of capital requirements on credit. Section 5 discusses the data and the empirical methodology. Section 6 presents the main results, and discusses additional exercises and robustness tests. Section 7 concludes.

II. Literature Review

When studying the effects of capital requirements on credit, the literature generally distinguishes between transitory and steady state effects. Transitory effects 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 relies on banks' reactions to exogenous shocks to bank capital. To capture steady-state effects, one needs to consider a large time window around these shocks. However, this makes it difficult to account for all the confounding factors, i.e., bank- and time-specific changes in and shocks to the economy that also affect credit. Identification from cross-sectional variation in capital is likely biased because such variation reflects, at least in part, endogenous bank capital choices. Unlike capital, capital requirements usually do not to suffer from 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, 2017; 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, 2014; Gambacorta and Shin 2016).

Due to the difficulty of estimating steady-state effects, many studies rely on model calibration. The key parameter in these models is the Modigliani and Miller offset: the extent to which a policy-imposed increase in the capital requirement increases the total funding costs of banks. In principle, an increase in a firm's capital reduces its riskiness and, thus, its cost of borrowing. Modigliani and Miller (MM, 1958) have shown that, under a set of ideal conditions, this effect fully offsets any potential increase in the funding cost from a shift in funding structure. An increase in capital is then essentially costless for banks and inconsequential for lending volumes and rates. Therefore, the degree to which MM holds in practice, is crucial. Overall, most studies that allow for the MM offset find extremely small effects of capital requirements on lending rates due to the tax shield for debt: a 1 percentage point higher requirement is associated with a 2 basis points increase in rates (Kashyap et al., 2010; Miles et al., 2013). Studies that assume a more limited MM offset suggest an effect of up to 13 basis points: still reasonably modest (Elliott, 2009; BCBS, 2010). Overall, these calibration results are in line with the findings of the empirical literature.

The literature on transitory effects—the one our approach builds on—employs more robust empirical identification and often finds substantial effects of higher capital requirements on loan growth. This literature goes back to Peek and Rosengren (1995), who study the effects of capital conservation plans during the implementation of Basel I in the U.S. They show that, over a one-

[4] This section builds on and extends the review in Dagher et al. (2016)

year period, banks subject to a capital conservation plan lend 2 percent less than banks not subject to such a plan. More recent papers explicitly focus on the effects of capital requirements. Aiyar et al. (2014) use bank-specific time-varying capital charges imposed by U.K. regulators to address bank-specific risks related to organizational and management practices. 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 percent lower bank lending in the following three quarters. Noss and Toffano (2016) study the same regulatory data in a VAR setting and find a smaller effect. A 15 basis points increase in capital requirements leads to a 0.25 percentage point reduction in quarterly lending growth after two quarters, and it fades to zero after about one year. Fraisse et al. (2017) 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 percent lower lending in the following year. Mesonnier and Monks (2014) consider heterogeneity in capital shortfall in banks following the 2011 EBA stress-tests. They find that a 1 percentage point higher capital shortfall is associated with 1.6 percent lower bank lending in the following year: a more modest effect than that in Aiyar (2014) and in Fraisse et al. (2017).

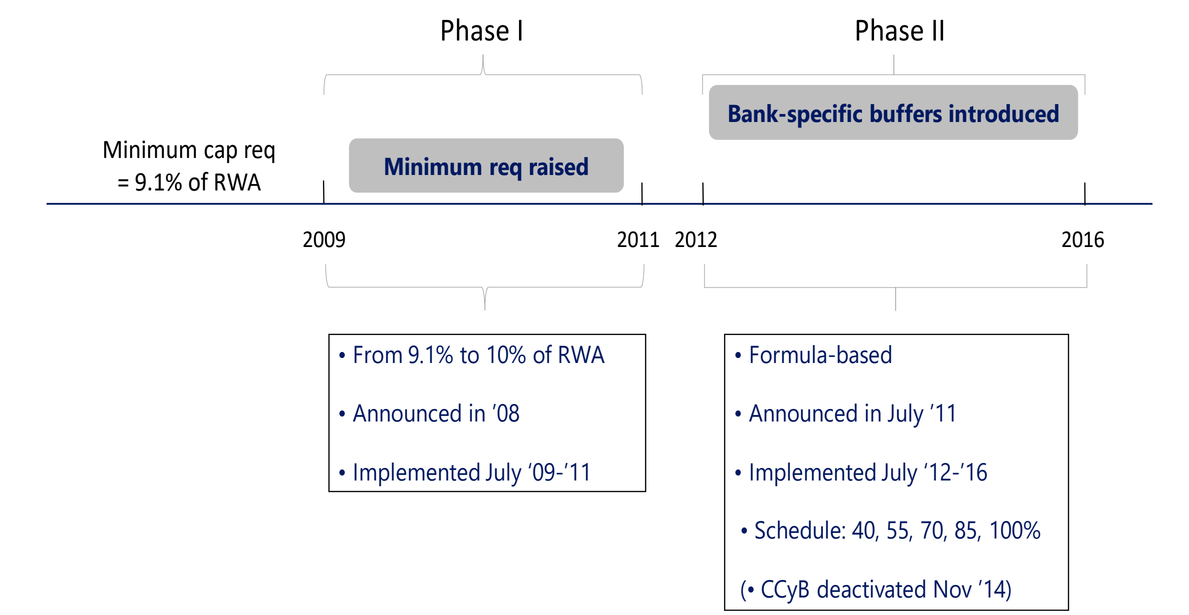
Other papers focus on the heterogenous effect of higher capital requirements on distinct types of loans. Using data for Denmark, Imbierowicz et al. (2018) show that banks retrench more from loans with higher risk weights. Using U.K. data, Bridges et al. (2014) show that banks reduce most the growth of their commercial real estate lending, then corporate lending and, finally, personal lending. This is consistent with stronger effects for riskier loan types. Using European bank data from the EBA exercise, Gropp et al. (2018) show particularly strong effects on banks' syndicated lending: a 27 percent reduction to achieve a 1.9 percentage point increase in bank capital. This is consistent with the fact that syndicated loans both have high-risk weights and are arm's length—arm's length lending tends to contract more during bank distress (see Bolton et al., 2016). Using Belgian data, De Jonghe, Dewachter, and Ongena (2016) show that banks retrench more from corporate sectors in which they are relatively less specialized. Finally, 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.

III. Peruvian Capital Reforms

Since the GFC, the Peruvian banking regulator, SBS, raised capital requirements in two phases. During 2009–2011, the SBS raised uniform minimum requirements from 9.1 percent to 10 percent of risk-weighted assets (RWA). In the period 2012–2016, the SBS introduced bank-specific capital buffers. These came on top of the 10 percent uniform minimum and, depending on the bank, could be as high as 5.6 percentage points (see Figures 1, 2, and 4).

The first reform, announced in July 2008, stipulated that on July 1 of 2009, 2010, and 2011, uniform minimum capital requirements would be raised from 9.1 percent to 9.5 percent to 9.8 percent to of RWA, respectively. Instead of introducing specific numerical increases, the second reform introduced a formula determining the capital buffer that each bank had to hold on top of the uniform 10 percent minimum. The formula was announced in July 2011. It was stipulated that, on July 1 of the years 2012 to 2016, banks had to hold 40, 55, 70, 85 and, finally, 100 percent of the formula-prescribed buffer, respectively. The amount of capital this corresponded to was recalculated monthly. Uniform and banks-specific capital requirements are illustrated in Figure 2.

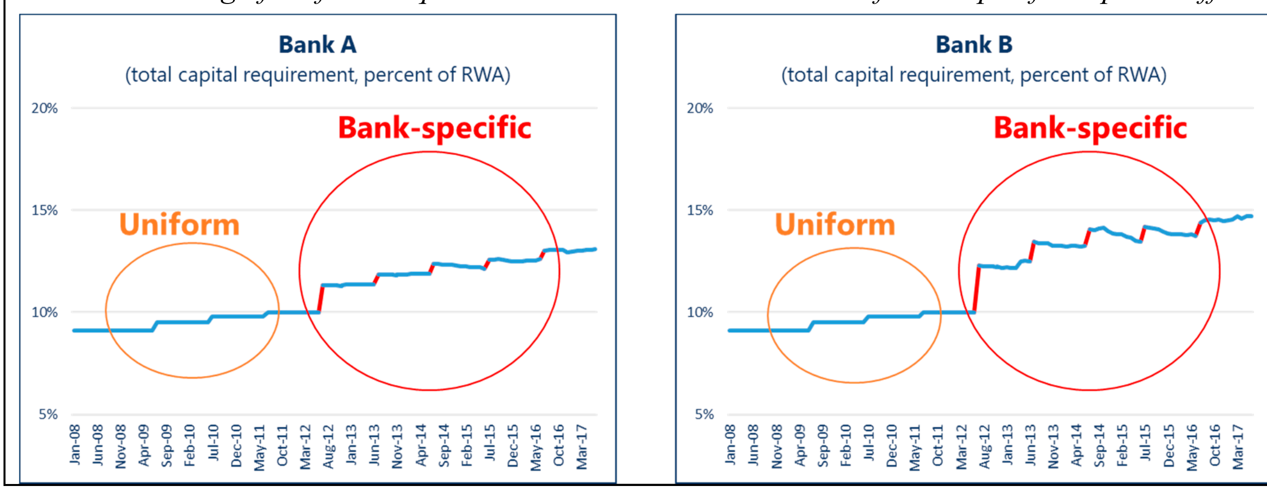
Figure 1. Timeline of Peruvian Capital Reforms



During the second reform, changes in compulsory capital buffers were of two types: annual "jumps" and monthly "wiggles." The annual jumps resulted from step-ups in the implementation schedule on July 1 of each year. The wiggles resulted from the monthly recalculation of the buffer requirements and were driven by the endogenous evolution of banks' balance sheets.

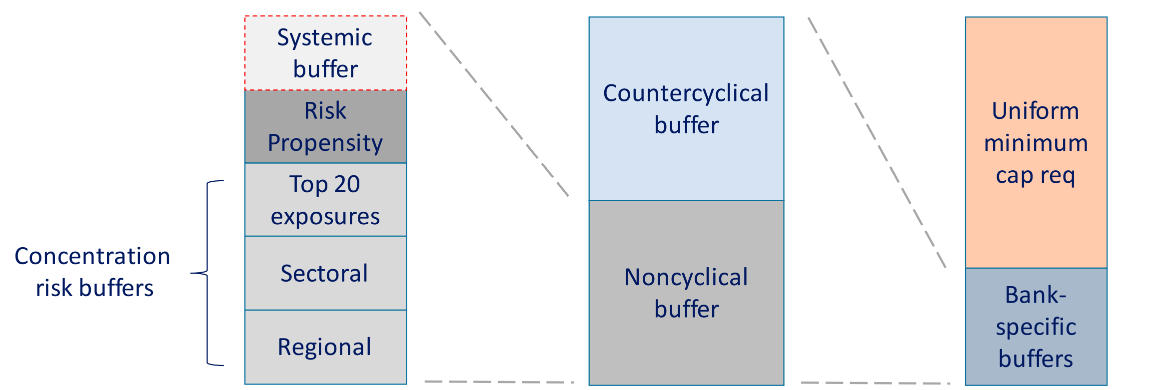
Figure 2. Peru: Two Reform Phases

Phase 1: Raising of uniform requirements. Phase 2: Introduction of bank-specific capital buffers



Compulsory capital buffers consisted of a non-cyclical component and a countercyclical component, which could be switched on and off. The countercyclical capital buffer (CCyB) was "born alive" and remained switched on until the end of October 2014.[5] When it was switched off in November, no additional countercyclical buffers had to be accumulated against new lending. However, already accumulated buffers did not become available and, as reform-implementation continued, countercyclical buffers for past lending had to be topped up to 85 percent and 100 percent of the formula on July 1 of 2015 and 2016, respectively.[6] The noncyclical buffer consisted of various concentration risk buffers, a risk-propensity buffer and, for the four largest banks, a (very small) systemic risk buffer (see Figure 3).[7]

Figure 3. Peru: Components of Total Capital Requirements



Neither reform was a "shock." Both reforms were publicly announced a year before implementation began. Through anticipation, lending could have been affected as early as the announcement itself. However, the empirical analysis below suggests that anticipatory effects on credit growth—if present—were small.

For the purpose of parameter identification, each of the two phases of the Peruvian capital reforms have some advantages and disadvantages. The first phase has the advantage that capital requirement increases were exogenous. Banks could not affect the size nor the timing of the increases, once promulgated. By contrast, capital requirement increases during the second phase of the reforms were potentially endogenous, since these were formula-based and recalculated

[5] For more details about the calculation of the CCyB, see Section VI.D, p.24.

[6] In Peru, countercyclical buffers are only released once a bank has used all its countercyclical *provisions*. Even then, only 60 percent of the accumulated buffer becomes available. Using the remaining 40 percent requires regulatory approval.

[7] 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, respectively. The higher the index, the greater the concentration risk buffer. Concentration risk from individual accounts is measured by the sum of top-20 exposures over RWAs. This buffer requirement kicks in only when the measure exceeds 5 percent. 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 banks: in our sample, only 4 banks were ever subject to it.

monthly.[8] This could have allowed banks to reduce the size of the capital buffer they needed to hold, by rejigging the composition of their balance sheets. The second phase has the advantage that it imposed bank-specific capital increases. To use the uniform increases of the first reform to infer the effect of capital requirements on credit, we would need to control for all other changes in the economy that also affected credit. This is an essentially impossible task. By contrast, the heterogeneity of capital buffer requirements introduced by the second reform allows us to add time fixed effects to our regressions. Time fixed effects absorb (and thus control for) all common macroeconomic shocks to banks and their lending, while bank fixed effects absorb 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 and liquidity.

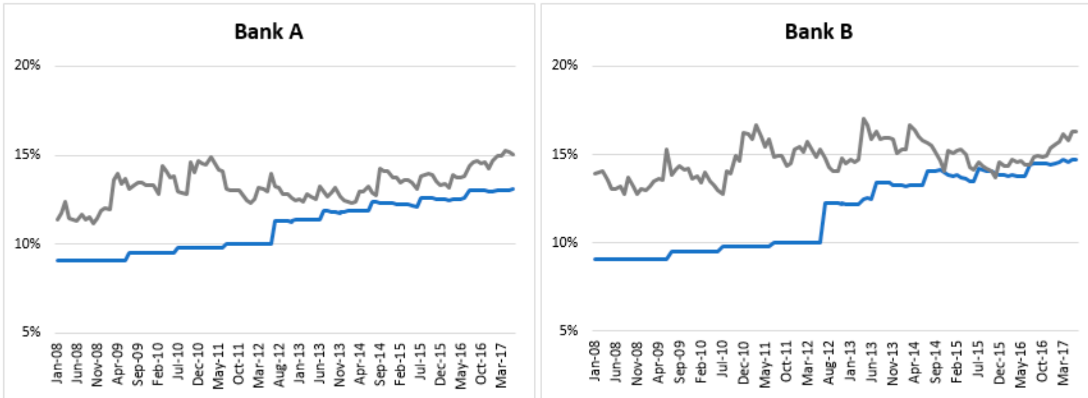
We have chosen to focus on the second reform for identification, despite the endogeneity problem. While we try to address endogeneity concerns in several ways (see Section 6), we believe that "controlling for everything" as would be required if we were to use the first phase of the reform is fundamentally impossible. Furthermore, at the time of the first phase of the reform, the Peruvian regulator also enforced an informal capital adequacy requirement of 11.1 percent until the beginning of the second reform. Since the informal requirement exceeded the formal one (even at the end of the first reform), it is highly unlikely that raising the formal requirement from 9.1 to 10 percent had much of an effect.

Finally, in interpreting our results—especially when assessing external validity—one should keep in mind two main caveats. First, the introduction of capital buffers took place in the context of a growing economy and high bank profitability, which may have attenuated their impact. Between 2011 and 2016, GDP grew at an average annual rate of 4.6 percent, and banks' return on equity averaged 16 percent and 20 percent, in non-weighted and asset-weighted terms, respectively (see also Table A4 in the Appendix). As 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.

Second, it is also important to consider the difference in stringency between capital buffer requirements and minimum capital requirements. In Peru, a bank that breaches minimum capital requirements (i.e., 10 percent of RWA since July 1, 2016) faces immediate and severe regulatory intervention. While the breaching of buffer requirements does lead to extra scrutiny from the regulator, the consequences are less severe. In fact, 6 out of the 14 banks in our sample did breach buffer requirements at some point (see Figure 4 below and Figure A1 in the Appendix). Since our estimates rely on changes in required buffers rather than in minimum capital requirements, arguably, they constitute a lower bound for the effect on lending of increases in minimum capital requirements of the same size.

[8] The only exception was the risk-propensity buffer, which was recalculated annually.

Figure 4. Peru: Actual Versus Required Capital



Note: Actual regulatory capital in grey, required capital in blue.

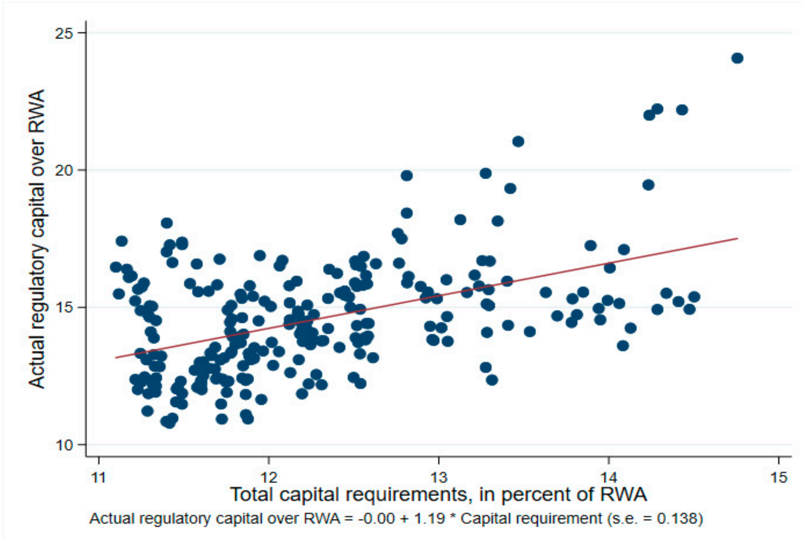
IV. Conceptual Framework

Capital requirements could affect bank credit if these three conditions are satisfied: (i) capital requirements must be—essentially—binding, (ii) additional capital must be costly relative to debt, and (iii) credit demand must be somewhat elastic. If higher capital requirements do not induce higher capital holdings, the requirements would be irrelevant. If capital financing is not costlier than debt financing then, even if requirements are binding, the adjustment to higher capital levels would be swift and costless, without affecting banks' credit supply. Finally, even with binding constraints and costly capital, higher requirements would not affect the volume of credit—unlike its price—if banks could fully pass on cost increases to borrowers, without affecting demand. If one of these conditions fails to hold, we would not expect to observe any relationship between capital requirements and credit. While we can test the first necessary condition, we cannot directly test the other two. In fact, we do not have detailed cost of funds data that would allow us to compare banks' total economic cost of debt versus equity financing, nor do we have the price data (cum instrument) needed to estimate the elasticity of credit demand.

As regard to the first condition, the data for Peruvian banks suggest that capital requirements were, essentially, binding for most banks. A simple scatter plot reveals that, on average, regulatory capital holdings increase with capital requirements—and this positive correlation is statistically significant (see Figure 5).

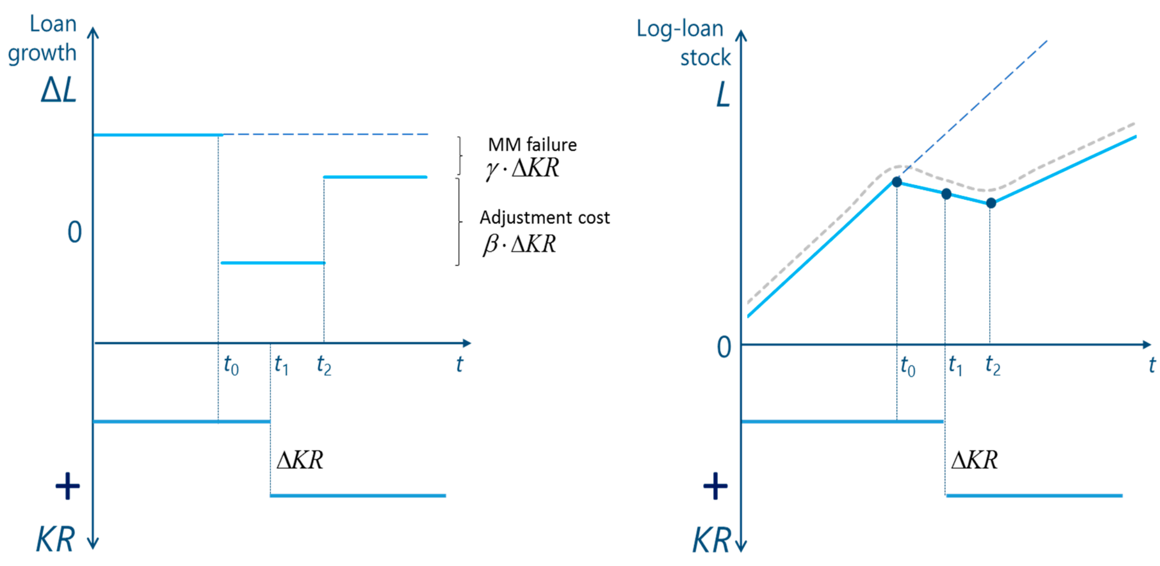
If all three conditions held, higher capital requirements could have had both permanent and transitory effects on credit in Peru. Permanently lower loan growth would imply failure of the MM irrelevance result. MM says that, in the absence of frictions, a bank's capital structure should have no effect on its overall cost of funds and, hence, on its credit supply. (In other words, under MM, the second condition fails.) However, even if capital structure—and, hence, capital requirements—were irrelevant in the long run, a bank could face adjustment costs associated with transitioning to higher levels of capital. During the adjustment period, the bank might temporarily cut back on lending to economize on capital.

Figure 5. Peru: Capital Requirements Versus Capital Held, 2012–2016



In a stylized way, Figure 6 illustrates how loan growth, ΔL, would react to an increase in capital requirements, ΔKR, in the presence of both adjustment costs and MM failure. Suppose it is announced at time t0 that capital requirements will rise by ΔKR percentage points at t1. In Figure 6, the permanent reduction in loan growth due to MM-failure is given by $\gamma \, \Delta KR$. Adjustment costs lead to an additional but transitory drop of credit growth by β ΔKR.

Figure 6. Conceptual Framework to Analyze an Increase in Capital Requirements



Note: Effect on lending of announcement at t_0 that capital requirement rises by ΔKR at t_1.

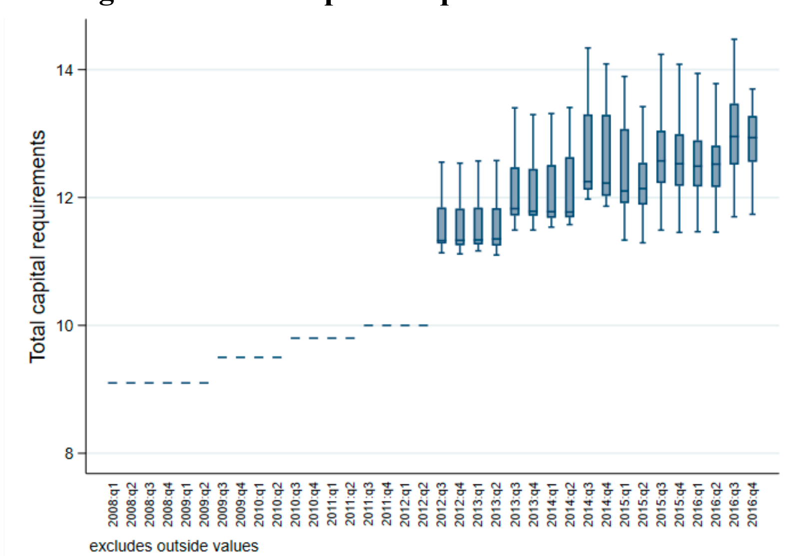
V. Data and Empirical Methodology

To investigate the impact of higher bank capital requirements, we collected capital requirement and balance sheet data for all 16 Peruvian commercial banks, for the period 2005–2016 (see Table A1 in the Appendix). The data were provided by the SBS. To reduce noise, we transformed them from their original monthly frequency to a quarterly frequency. From this sample, we dropped two banks, because of their short histories and associated extreme values for loan growth—i.e., Cencosud and ICBC, established in 2012 and 2014, respectively. 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 first and 99th percentile, to ensure that our findings would not be 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 percent of financial system assets in 2016. Quarterly loan growth during the sample period averaged 3.2 percent in real terms, a number consistent with aggregate statistics reporting 13.7 percent real annual growth. See Table A3 in the Appendix for further summary statistics of the data.

Total capital requirements differed materially over time and across banks. During the first reform phase, capital requirements increased from 9.1 to 10 percent of RWA but remained uniform across banks. During the second reform, capital requirements became bank specific. This is illustrated in Figure 7, which shows the distribution of total capital requirements (i.e., uniform minimum plus buffer) for the 14 banks in our sample. Variation across banks and time is the key ingredient of our identification strategy.

Figure 7. Peru: Capital Requirements over Time



Note: Distribution of total capital requirements across banks and time.

To test for the effects of capital requirements on bank lending, the empirical analysis builds on a well-established approach that regresses credit growth at the bank-level on changes in (bank-specific) capital requirements (see Aiyar et al. 2014). The bank-specific capital buffers of the second reform allow for a "difference-in-differences" approach, whereby only deviations from averages, in terms of credit growth and in terms of buffer requirements, are used to identify the effect of higher capital. In practice, we estimate the following regression equation:

$$\text{For } r, s \in \{0, 1, 2,...\}, \quad \Delta L^i_{t+r,t-s} = \beta_{r,s} \Delta KR^i_{t,t-1} + \delta X^i_{t-s} + \alpha^i + \tau_t + \varepsilon^i_t \qquad (1)$$

where the dependent variable is loan growth, while the key explanatory variable is the change in the required capital buffer. Formally, $\Delta L^i_{t+r,t-s}$ 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*. Since we only use jumps but not wiggles for identification, $\Delta KR^i_{t,t-1} = 0$ for all time periods t other than the third quarter of the years 2012 to 2016. Unobserved bank heterogeneity is absorbed by bank fixed effects, α^i, while all macroeconomic and policy shocks affecting banks equally (e.g., changes in economic growth and monetary policy) are absorbed by time fixed effects, τ_t. This means that the coefficients of interest, $\beta_{r,s}$, are solely estimated off the degree to which an above-average increase in capital requirements leads to an above-average drop in credit growth.[9] Standard errors are clustered at the bank level to allow for autocorrelation within banks.

The set of time-varying, bank-specific controls, X^i_{t-s} , is relatively parsimonious and includes: (1) 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 Tables A2 and A3 in the Appendix.[10] A key implicit assumption of our specification is that changes in the demand for credit are the same for all banks.

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 more slowly than banks with lower requirements. By looking at increasingly longer periods, we can see 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, without tackling the issue of steady-state effects of capital requirements.

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[9] Notice that we do not allow for bank-specific, time-varying shocks, since they would not leave us with any residual variation from which to estimate the coefficients.

[10] The parsimoniousness of the set of controls has the advantage—important from a policy perspective—of facilitating replicability of the analysis in other contexts.

To mitigate endogeneity bias, we only use jumps—but not wiggles—for identification. Recall that banks' buffer requirements changed for two reasons: On July 1 of each year, there was a jump in requirements due to a step up in reform implementation. In between, there were more minor monthly changes (wiggles), resulting from the evolution of banks' balance sheets and the monthly recalculation of the requirements. Clearly, the jumps were more exogenous than the wiggles. Even though identification is based exclusively on the second reform, we use the whole sample period for estimation. Our sample covers the period 2005–2016, while the second reform only started in 2011. Still, the earlier years contain information that allow us to estimate the control variables more accurately. Therefore, we include them in the estimation. However, as discussed below, our results do not change if we focus on the 2010–2016 period.

VI. Results

A. Baseline Results

Table 1 reports OLS estimates for specification (1) for progressively longer straddles around jumps in capital requirements. Unweighted and weighted regressions are presented in adjacent columns where, in the latter, observations are weighted by bank assets to allow the largest banks to drive the results. In addition to $\Delta KR_{t,t-1}^{i}$, columns (1) and (2) only include bank and time fixed effects as regressors, while columns (3) and (4) add bank-specific controls, X_{t-s}^{i}. Subsequently, the period over which credit growth is calculated is progressively lengthened: columns (5)–(6) show 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 is reported in columns (7)–(8).

Our estimates point to a contemporaneous effect of capital on lending that "washes out" very quickly. Columns (1)–(4) 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. However, columns (5)–(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.

We also look at the dynamic effect of changes in capital requirements using the local projection approach—pioneered by Jorda (2005)—assuming that the impulse, or "shock," occurs at the time of the jump in the requirement. Figure 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.

Table 1. OLS Estimates for Specification (1)

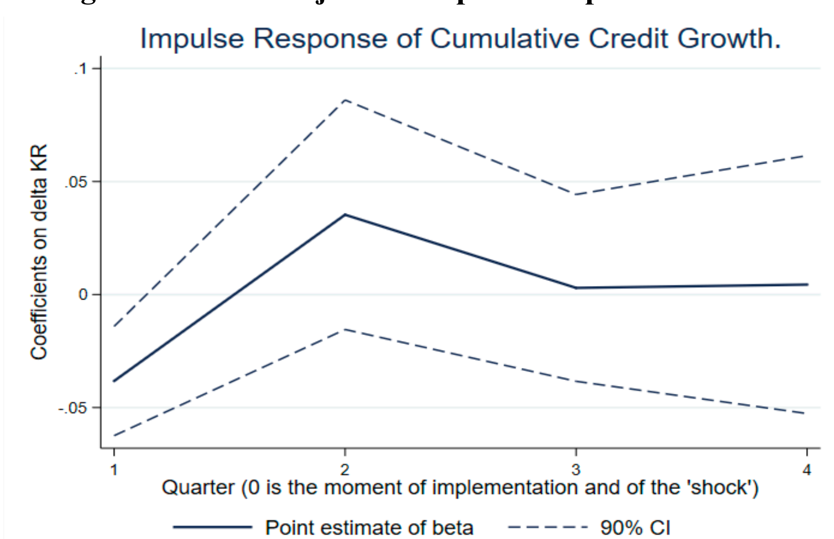
Dep. Var.: $\Delta L_{t+r, t-s}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1q		1q		2q		4q	
Explanatory variables	(*r,s*)=(0,1)		(*r,s*)=(0,1)		(*r,s*)=(1,1)		(*r,s*)=(2,2)	
$\Delta KR_{t, t-1}$	-0.0464***	-0.0584***	-0.0399***	-0.0643***	-0.0090	-0.0545	0.0385	-0.0349
	(0.010)	(0.014)	(0.012)	(0.017)	(0.026)	(0.047)	(0.043)	(0.060)
CAR minus KR, t-s			-0.0011	0.0007	0.0012	0.0018	0.0045	0.0035
			(0.001)	(0.001)	(0.003)	(0.002)	(0.005)	(0.005)
Log Assets, t-s			-0.0811***	-0.0558***	-0.1844***	-0.1443***	-0.2883***	-0.2456***
			(0.021)	(0.016)	(0.049)	(0.031)	(0.084)	(0.068)
ROA, t-s			0.5507	0.2305	0.6754	3.6012*	1.7413	8.0339***
			(0.901)	(1.219)	(1.755)	(1.780)	(2.335)	(2.251)
Liquid / Total Assets, t-s			-0.0087	0.0679	0.0305	0.1088*	-0.0300	0.2057
			(0.060)	(0.055)	(0.073)	(0.059)	(0.148)	(0.120)
RWA / Assets, t-s			-0.0613	-0.0213	-0.1215	-0.1026	-0.2488	-0.2225
			(0.044)	(0.034)	(0.083)	(0.076)	(0.159)	(0.177)
Observations	586	586	581	581	575	575	547	547
R-squared	0.323	0.543	0.423	0.571	0.492	0.625	0.519	0.640
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Weights	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.2436	0.4900	0.3491	0.5157	0.4197	0.5711	0.4478	0.5868

Robust standard errors in parentheses

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

Note: Effect of a jump in required buffers on loan growth, in increasing straddles of up to one year around the jump.

Figure 8. Local Projection Impulse Response Function



Note: Response of cumulative credit growth to a 1 percentage point rise in buffer requirement, assuming the rise and the "shock" both occur at time zero. The estimates are based on unweighted regressions with all the controls of Table 1.

B. Bank Heterogeneity

So far, we have assumed that the effects of increased capital requirements are homogeneous across banks. However, there are reasons to believe that some banks are more sensitive than others. We are concerned that the limited effect of changes in capital requirements could be due to the elevated level of bank profitability in Peru. More profitable banks may find it easier to raise additional capital, since they may be able to do so via retained earnings (De Jonghe, Dewachter, and Ongena 2016). By contrast, the cost of capital may be higher for less capitalized banks, which could have a greater incentive to reduce lending. We test this hypothesis splitting banks into "more profitable" and "less profitable" groups, depending on ROA being above or below the median or the first quartile of the sample distribution, respectively, and allowing the coefficient on the change in capital requirements to vary depending on the bank profitability classification.

Our results suggest that less profitable banks are more sensitive to changes in capital requirements than more profitable banks, consistent with the evidence shown on Belgian banks by De Jonghe, Dewachter, and Ongena (2016). Table 2 shows that, for banks with below-average profitability, the unweighted point-estimate of the contemporaneous contraction of quarterly loan growth in response to a one percentage point increase in capital requirements is 7.4 percent. For banks with above-average profitability this number is 4.9 percent. While the difference appears quite large, it is not statistically significant. Also, at 6.5 and 6.4 percent, respectively, the weighted estimates are almost indistinguishable. When we cut the sample at the 25th percentile rather than at the median, the coefficients are statistically different in the unweighted regression, but not in the weighted one.

C. Robustness

We test the robustness of the baseline findings in Table 1 to alternative measures of loan growth, to only focusing on the period 2010–2016, and to applying the pre-July 2012 11.1 percent informal capital requirement. The results of the robustness exercises are shown in Table 3. First, loan growth is computed using net rather than gross exposures. Second, loan growth is computed using the Davis and Haltiwanger (1992) measure.[11] Third, the sample period is restricted to 2010–2016. Finally, we assume that the uniform minimum capital requirement was 11.1 percent until July 2012. This reflects reports that such a threshold was informally enforced up to the beginning of the second reform phase. Table 3 shows that the estimates of the contemporaneous effect are stable, consistently implying a drop-in lending of between 3 and 6 percentage points per percentage point increase in capital requirements.

[11] This measure is defined as the difference in gross loans between t and *t*-1, divided by average gross loans in t and *t*-1. In this way, the measure is bounded between -2 (exit) and +2 (entry).

Table 2. Heterogeneous Impact of Capital Requirements

Dep. Var.: $\Delta L_{t, t-1}$	(1)	(2)	(3)	(4)
Explanatory variables	*Cut at median*		*Cut at 25th percentile*	
$\Delta KR_{t, t-1}$, *low ROA*	-0.0738***	-0.0649***	-0.0699***	-0.0687***
	(0.019)	(0.016)	(0.011)	(0.015)
$\Delta KR_{t, t-1}$, *high ROA*	-0.0489***	-0.0642***	-0.0435***	-0.0612***
	(0.012)	(0.016)	(0.013)	(0.018)
CAR minus KR, t-1	-0.0011	0.0007	-0.0011	0.0007
	(0.001)	(0.001)	(0.001)	(0.001)
Log Assets, t-1	-0.0797***	-0.0558***	-0.0795***	-0.0556***
	(0.020)	(0.016)	(0.020)	(0.016)
ROA, t-1	0.5088	0.2214	0.5066	0.2043
	(0.907)	(1.256)	(0.904)	(1.210)
Liquid / Total Assets, t-1	-0.0100	0.0681	-0.0113	0.0681
	(0.060)	(0.055)	(0.059)	(0.055)
RWA / Assets, t-1	-0.0654	-0.0213	-0.0651	-0.0218
	(0.046)	(0.034)	(0.045)	(0.034)
Observations	581	581	581	581
R-squared	0.427	0.571	0.427	0.571
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Standard errors	Cluster	Cluster	Cluster	Cluster
Weights	No	Yes	No	Yes
R2adj	0.3517	0.5148	0.3521	0.5150
Test of equality (p-value)	0.1430	0.9374	0.0552	0.4795

Robust standard errors in parentheses

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

Note: The table shows estimates for the contemporaneous effect of buffer requirement increases on loan growth when distinguishing between banks on the basis of ROA.

Table 3. Robustness Estimations

Robustness exercises for the contemporaneous effect of requirement change on loan growth. Columns (1)–(2): net rather than gross exposures. Columns (3)–(4): Davis and Haltiwanger (1992) measure of loan growth. Columns (5)–(6): sample restricted to 2010-2016. Columns (7)–(8): 11.1 percent minimum capital requirement until July 2012.

Dep. Var.: $\Delta L_{t,t-1}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Explanatory variables	Net Loans		DH		'10-'16		11.1% min	
$\Delta KR_{t,t-1}$	-0.0326*	-0.0631***	-0.0399***	-0.0643***	-0.0401**	-0.0589***	-0.0399***	-0.0643***
	(0.015)	(0.018)	(0.012)	(0.017)	(0.015)	(0.015)	(0.012)	(0.017)
CAR minus KR, t-1	-0.0007	0.0008	-0.0011	0.0007	-0.0039	0.0013	-0.0011	0.0007
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)
Log Assets, t-1	-0.0848***	-0.0604***	-0.0814***	-0.0559***	-0.0470	-0.0251	-0.0811***	-0.0558***
	(0.023)	(0.017)	(0.021)	(0.016)	(0.031)	(0.034)	(0.021)	(0.016)
ROA, t-1	0.6701	0.2566	0.5624	0.2369	2.5271*	1.0154	0.5507	0.2305
	(0.854)	(1.281)	(0.904)	(1.222)	(1.387)	(1.332)	(0.901)	(1.219)
Liquid / Total Assets, t-1	0.0065	0.0719	-0.0086	0.0679	-0.0199	0.0640	-0.0087	0.0679
	(0.062)	(0.056)	(0.059)	(0.055)	(0.049)	(0.059)	(0.060)	(0.055)
RWA / Assets, t-1	-0.0495	-0.0245	-0.0613	-0.0213	-0.0534	0.0182	-0.0613	-0.0213
	(0.039)	(0.034)	(0.044)	(0.034)	(0.049)	(0.031)	(0.044)	(0.034)
Observations	581	581	581	581	385	385	581	581
R-squared	0.425	0.562	0.424	0.571	0.322	0.426	0.423	0.571
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Weights	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4. OLS Estimates for Specification (2)

Dep. Var.: $\Delta L_{t,\,t-1}$ Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta KR_{t,\,t-1}$	-0.0399***	-0.0643***	-0.0307*	-0.0633***	-0.0357*	-0.0708***	-0.0344*	-0.0635***
	(0.012)	(0.017)	(0.016)	(0.021)	(0.018)	(0.019)	(0.019)	(0.020)
$\Delta KR_{t-1,\,t-2}$			0.0577*	0.0158	0.0524	0.0079	0.0498**	-0.0106
			(0.031)	(0.041)	(0.031)	(0.040)	(0.021)	(0.040)
$\Delta KR_{t-2,\,t-3}$					-0.0425***	-0.0427**	-0.0415***	-0.0378*
					(0.013)	(0.019)	(0.012)	(0.018)
$\Delta KR_{t-3,\,t-4}$							-0.0375	-0.0164
							(0.031)	(0.032)
$\Delta KR_{t+1,\,t}$			0.0264	-0.0058	0.0211	-0.0139	0.0399*	0.0022
			(0.019)	(0.042)	(0.021)	(0.041)	(0.022)	(0.038)
$\Delta KR_{t+2,\,t+1}$					0.0067	-0.0112	0.0077	-0.0061
					(0.027)	(0.023)	(0.026)	(0.024)
$\Delta KR_{t+3,\,t+2}$							0.0136	0.0417
							(0.022)	(0.035)
CAR minus KR, t-1	-0.0011	0.0007	-0.0011	0.0006	-0.0014	0.0004	-0.0014	0.0006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log Assets, t-1	-0.0811***	-0.0558***	-0.0846***	-0.0575***	-0.0856***	-0.0596***	-0.0853***	-0.0621***
	(0.021)	(0.016)	(0.021)	(0.016)	(0.022)	(0.017)	(0.024)	(0.015)
ROA, t-1	0.5507	0.2305	0.5515	0.2135	0.4726	0.0575	0.4657	0.0499
	(0.901)	(1.219)	(0.916)	(1.218)	(0.889)	(1.201)	(0.957)	(1.154)
Liquid / Total Assets, t-1	-0.0087	0.0679	-0.0051	0.0742	-0.0039	0.0758	-0.0010	0.0737
	(0.060)	(0.055)	(0.067)	(0.055)	(0.064)	(0.054)	(0.062)	(0.057)
RWA / Assets, t-1	-0.0613	-0.0213	-0.0605	-0.0204	-0.0663	-0.0215	-0.0719	-0.0206
	(0.044)	(0.034)	(0.044)	(0.031)	(0.044)	(0.028)	(0.046)	(0.029)
Observations	581	581	572	572	561	561	537	537
R-squared	0.423	0.571	0.430	0.571	0.435	0.578	0.436	0.568
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Weights	No	Yes	No	Yes	No	Yes	No	Yes
Cumulated effect			0.0534	-0.0317	0.0020	-0.1308	-0.0025	-0.0904
p-value			0.2907	0.5272	0.9760	0.1014	0.9766	0.4058

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Effect of a 1 p.p. jump in required buffers on quarterly loan growth, progressively allowing for longer adjustment periods of up to one year before and after the jump.

We also estimate an alternative regression specification, which considers the effect of a jump in required buffers on quarterly loan growth, progressively allowing for longer adjustment periods. Specifically, we estimate the following regression equation:

$$\Delta L_{t,t-1}^{i} = \sum_{s=-n_{lag}}^{n_{forw}} \beta_s \, \Delta KR_{t+s,t+s-1}^{i} + \delta X_{t-1}^{i} + \alpha^{i} + \tau_t + \varepsilon_t^{i} \qquad (2)$$

On the left-hand size we have quarterly loan growth. On the right-hand side, we have lags and forwards on $\Delta KR_{t,t-1}^{i}$ to allow for gradual adjustment, lasting from n_{forw} periods before the increase until n_{lag} periods after. OLS estimates for specification (2) are reported in Table 4. Columns (1)–(2) reflect 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. From columns (3)–(4) onward, the number of lags and forwards on ΔKR is progressively increased such that, in columns (7)–(8), 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 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 negative effect of a rise in capital requirements on contemporaneous loan growth survives the introduction of lags and forwards—if only at 10 percent significance in the unweighted regression. Except for β_{-2}, coefficients on lags and forwards are not significant, or they are significant in the "wrong" direction. More importantly, the last two rows of the table reveal that the joint impact of lags and forward, measured by the sum of coefficients $\sum_{s=-n_{lag}}^{n_{forw}} \beta_s$, 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. Hence, the overall effect of a change in capital requirement on quarterly loan growth is insignificant.

D. Addressing Endogeneity

The results reported so far largely ignore the endogeneity of capital requirements. For identification, we only used 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 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.

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 λx percent, $0 < \lambda \leq 1$. 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 λy—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:

$$\hat{\beta} = -\frac{\lambda y}{\lambda x} = -\frac{y}{x}$$

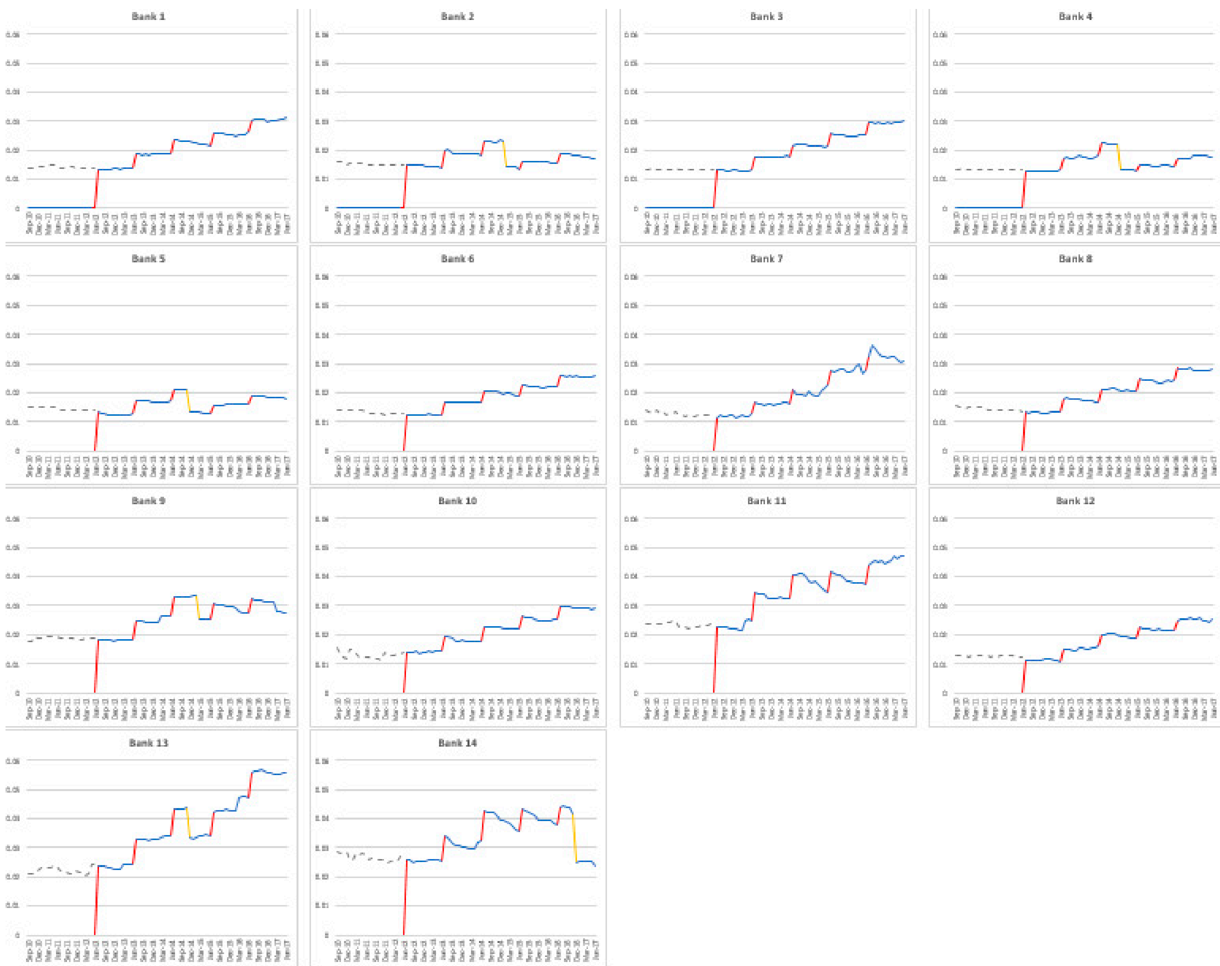
which is equal to β. In all other cases, in the process of adjusting the composition of its balance sheet, the bank would have foregone at least some growth opportunities. The reduction of the fall in the bank's credit growth would then be strictly less than proportional, such that the fall in credit growth, μy, was strictly greater than λy; i.e., $\mu > \lambda$. Estimate $\hat{\beta}$ then becomes:

$$\hat{\beta} = -\frac{\mu y}{\lambda x} > -\frac{y}{x} = \beta.$$

This establishes the claim.

To assess the severity of the endogeneity problem, we calculate "virtual" buffer requirements. Virtual buffers are the (counter-factual) buffers the banks would have had to hold if the requirements had been in place at 40 percent before July 1, 2012 (see Figure 9). We compared these virtual requirements with the ones that came into effect. If virtual requirements had been stable up to the announcement in June 2011 and suddenly 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 endogeneity would complicate the interpretation of our results.

Figure 9. Capital Buffers—Virtual and Actual



Note: Virtual capital buffers in grey, "wiggles" in blue, "jumps" in red. Drops (yellow) reflect the release of 60 percent 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.

Visual inspection of Figure 9 suggests, and formal tests confirm (do not reject), that averages before and after the announcement were the same, and that there was no downward trend between announcement and implementation dates. Furthermore, until the countercyclical buffer was switched off at the end of October 2014, the wiggles in between jumps did not exhibit a downward trend either.[12] This is reassuring. It suggests that, in practice, banks did not strategically adjust the composition of their balance, limiting the endogeneity bias of our estimates.

To further verify the robustness of our findings, we ran panel regressions on the bank-product level that do not suffer from the same kind of endogeneity problem. In Peru, the CCyB is entirely product-specific. To calculate a bank's CCyB, every asset is assigned to one of eight buckets, each of which is associated with an additional risk-weight between zero and 55 percent. Mortgage loans, for example, are assigned to the 15 percent-bucket. This means that when the CCyB is "switched on", a bank must hold 15 percent x 10 percent (the uniform minimum) = 1.5 percent 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, notice that the CCyB decreases the cost of mortgage lending relative to, e.g., large-enterprise lending, which falls in the 25 percent-bucket.

Keeping the same approach as before, we can modify our baseline model—equation (1)—to study if, and for how long, loan products with higher capital surcharges grew more slowly than products with lower surcharges. Thus, we estimate the following model:

$$\text{For } r,s \in \{0,1,2,\ldots\}, \qquad \Delta L^{i,j}_{t+r,t-s} = \Delta CS^{j}_{t,t-1} + \delta X^{i}_{t-s} + \alpha^{i} + \rho^{j} + \tau_t + \varepsilon^{i,j}_{t} \qquad (3)$$

where the dependent variable, $\Delta L^{i,j}_{t+r,t-s}$, 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. The set of bank-specific control variables is the same as in specification (1). Unobserved heterogeneity across bank-product pairs and across time is absorbed by fixed effects.

The greater degree of granularity of the product-level data gives us a key advantage to tackle endogeneity, compared to the bank-level regressions. In fact, the sizes of capital surcharges—but not the timing of their (de)activation—were fully exogenous. The CCyB was (de)activated depending on the state of the economy. To the extent that the economy-wide shocks triggering (de)activation were common to all banks-products, they would be absorbed by the time fixed-effects and would not bias our estimates. Otherwise, controlling for bank-product-specific demand shocks takes on added importance. Standard errors are clustered at the bank-product level, to allow for autocorrelation within bank-product pairs.

Results—reported in Table 5—confirm that the negative effect of capital increases on lending does not extend beyond one quarter. Unweighted and weighted regressions are presented in

[12] When the CCyB was switched off, the buffers were "frozen in place." Since banks' balance sheets continued to grow, mechanically, the wiggles started to trend down.

adjacent columns where, in the latter, bank-product categories are weighted by asset size. The regressions provide weak evidence for a contemporaneous negative impact, but they do not show any effect beyond one quarter. In fact, the capital surcharge appears with a marginally significant positive coefficient in the weighted regression of annual credit growth. This could be due to changes in demand that differed across bank-product pairs.[13] Impulse response functions, as well as regressions of quarterly credit growth with increasingly longer adjustment periods, confirm that adverse effects on lending, if they existed, were short-lived. (See Figure A2. and Table A5 in the Appendix).

Table 5. OLS Estimates for Specification (3)

Dep. Var.: $\Delta L_{t+r,\, t-s}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1q		1q		2q		4q	
Explanatory variables	$(r,s)=(0,1)$		$(r,s)=(0,1)$		$(r,s)=(1,1)$		$(r,s)=(2,2)$	
$\Delta CS_{t,\, t-1}$	-0.0950	-0.0392**	-0.0952	-0.0389**	-0.0821	-0.0074	-0.0083	0.0382*
	(0.068)	(0.019)	(0.068)	(0.019)	(0.064)	(0.020)	(0.096)	(0.020)
CAR minus KR, t-s			0.4765	-0.1245	0.4088	0.1959	2.2704	0.6506
			(0.942)	(0.330)	(1.278)	(0.622)	(2.357)	(0.980)
Log Assets, t-s			-0.0424	-0.0384	-0.1266	-0.1300***	-0.0687	-0.2911***
			(0.101)	(0.041)	(0.175)	(0.049)	(0.305)	(0.095)
ROA, t-s			-16.6507	2.6005	3.5506	7.6853	21.8617	9.2342
			(14.222)	(3.904)	(8.986)	(5.282)	(22.078)	(7.794)
Liquid / Total Assets, t-s			0.5876	0.0288	0.8438	0.1318	1.2254	0.3281*
			(0.385)	(0.093)	(0.519)	(0.111)	(0.896)	(0.174)
RWA / Assets, t-s			0.3810	-0.1197	0.3346	-0.1984	0.7833	-0.3658
			(0.338)	(0.163)	(0.488)	(0.290)	(1.022)	(0.588)
Observations	2,741	2,741	2,741	2,741	2,620	2,608	2,393	2,377
R-squared	0.041	0.158	0.043	0.159	0.075	0.130	0.140	0.197
Product x Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Weights	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	-0.0158	0.1075	-0.0159	0.1067	0.0156	0.0746	0.0813	0.1420

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Effect of a jump in capital surcharges on loan growth, for increasing straddles of up to one year around a jump.

[13] An implicit (key) assumption of our specification is that changes in demand were the same for all bank-products.

VII. Conclusions

While the literature has examined the impact of capital requirements in advanced economies, evidence for emerging market countries is lacking. We offer novel evidence based on the case of Peru. Increased capital requirements appear to have had only a temporary effect on lending in Peru. Our estimates suggest that a one percentage point increase in required capital buffers reduced lending growth by between 4 and 6 percentage points in the quarter in which it came into effect. We do not find evidence of anticipation, nor of lagged effects. Furthermore, over periods of six months and beyond, loan growth did not statistically differ between periods with and without capital increases. Hence, we cannot reject the hypothesis that, after as little as half a year, the level of credit was back where it would have been in the absence of a rise in capital requirements.

This benign outcome suggests that the cost of raising additional capital was quite low for Peruvian banks. Alternatively, capital requirements may not have been binding and/or credit demand was extremely inelastic, allowing banks to pass on higher costs to borrowers without negatively affecting demand. However, as we have seen, the data seem to reject the former hypothesis, while the literature has shown that, even in the short run, demand for credit tends to be quite elastic (see, e.g., Karlan and Zinman, 2013).

The apparently low cost of adjusting to higher capital levels in Peru may be due to: the early announcement of reforms; the relatively slow speed of implementation; and the high profitability of banks. 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. Finally, with a weighted average return on equity of around 20 percent (16 percent unweighted), Peruvian banks were highly profitable, also in comparison with other emerging markets. While unprofitable banks can improve their capital adequacy only by compressing lending or issuing expensive new equity, highly profitable banks need not resort to either of these options: they can simply retain more earnings, making the transition to higher capital requirements easier, while muting the impact on credit.

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Appendix I. Data Description, and Additional Tables and Figures

Table A1. Commercial Banks in Peru as of end-2016.

Bank Name	Total Assets (thousands of Soles)	Rank	Share (in % of total)	Cumulative share (in % of total)
Banco de Crédito del Perú (BCP)	121,913,255	1	34.52%	34.52%
BBVA (ex Banco Continental)	73,068,119	2	20.69%	55.21%
Scotiabank Perú	56,792,357	3	16.08%	71.29%
Interbank	41,482,405	4	11.75%	83.04%
Banco Interamericano de Finanzas (BANBIF)	12,858,287	5	3.64%	86.68%
Mibanco	11,509,224	6	3.26%	89.93%
Banco Financiero	8,780,702	7	2.49%	92.42%
Citibank	5,975,162	8	1.69%	94.11%
Banco GNB	5,289,726	9	1.50%	95.61%
Banco Falabella Perú	4,867,627	10	1.38%	96.99%
Banco Santander Perú	4,783,806	11	1.35%	98.34%
Banco Ripley	2,238,754	12	0.63%	98.98%
Banco de Comercio	1,945,597	13	0.55%	99.53%
Banco Cencosud	681,960	14	0.19%	99.72%
Banco Azteca del Perú	563,288	15	0.16%	99.88%
Banco ICBC	421,146	16	0.12%	100.00%

Table A2. Variable Names, Descriptions, and Units of Measurement

List of variables

Variable shorthand	Definition	Unit of measurement
$\Delta L_{t,\,t\text{-}1}$	Quarter-on-quarter change in real gross loans. Constructed from end-of-quarter loan levels deflated with consumer prices.	percent
$\Delta KR_{t,\,t\text{-}1}$	Quarter-on-quarter "jumps" in 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.	percent
CAR minus KR, t-1	Difference between total regulatory capital held and total capital requirement during quarter *t-1* , both expressed as percent of RWAs	percent
Log Assets, t-1	Natural logarithm of total assets on balance sheet at the end of quarter *t-1*	log of Soles
ROA, t-1	Return On Assets during quarter *t-1* , defined as Net Income divided by Total Assets	percent
Liquid / Total Assets, t-1	Ratio of liquid assets and total assets at the end of quarter *t-1*	percent
RWA / Assets, t-1	Ratio of risk-weighted assets and total assets at the end of quarter *t-1*	percent

Table A3. Summary Statistics

All changes and ratios in percent.

Summary statistics for variables

All changes and ratios expressed in percent. $\Delta KR_{t,\, t\text{-}1}$ only accounts for non-zero values (i.e. implementation periods only). *ROA, t-1* is on a quarterly basis (i.e. total Net Income over the quarter divided by Total Assets at end of quarter).

Variable	Mean	Median	Std. Deviation	Minimum	Maximum	Sample size
$\Delta L_{t,\, t\text{-}1}$	3.161	2.671	5.503	-10.645	23.315	593
$\Delta KR_{t,\, t\text{-}1}$	0.717	0.480	0.507	0.198	2.553	70
CAR minus KR, t-1	3.543	3.044	2.564	-0.959	19.109	616
Log Assets, t-1	15.474	15.223	1.418	12.236	18.444	607
ROA, t-1	0.551	0.486	0.429	-1.393	3.184	602
Liquid / Total Assets, t-1	20.671	19.572	9.662	3.374	53.012	607
RWA / Total Assets, t-1	82.880	84.341	14.143	38.280	160.748	607

Table A4. Peru: Bank Profitability

Profitability during 2011–2016. Annual averages across banks.

	ROA						ROE					
Average	**2011**	**2012**	**2013**	**2014**	**2015**	**2016**	**2011**	**2012**	**2013**	**2014**	**2015**	**2016**
Unweighted	2.16%	3.48%	1.77%	1.75%	2.65%	1.78%	18.09%	19.89%	13.98%	13.19%	15.38%	13.35%
Weighted	2.26%	2.06%	1.90%	1.88%	1.95%	2.03%	22.78%	21.24%	20.04%	19.07%	20.46%	18.48%

Table A5. Peru: Product-level Regressions with Lags and Forwards

The table shows the effect of a 1 p.p. jump in the capital surcharge on quarterly loan growth, progressively allowing for longer adjustment periods of up to one year before and after the jump.

Dep. Var.: $\Delta L_{t, t-1}$								
Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta CS_{t, t-1}$	-0.0952	-0.0389**	-0.0952	-0.0222	-0.1257	-0.0134	-0.0105	0.0008
	(0.068)	(0.019)	(0.065)	(0.017)	(0.078)	(0.020)	(0.092)	(0.022)
$\Delta CS_{t-1, t-2}$			-0.0003	0.0373**	-0.0346	0.0460**	0.0192	0.0349*
			(0.092)	(0.018)	(0.092)	(0.023)	(0.098)	(0.021)
$\Delta CS_{t-2, t-3}$					-0.1055	-0.0321*	-0.0224	-0.0248
					(0.122)	(0.019)	(0.116)	(0.020)
$\Delta CS_{t-3, t-4}$							0.0890	0.0070
							(0.077)	(0.023)
$\Delta CS_{t+1, t}$			0.0365	0.0393*	0.0048	0.0480**	0.0797	0.0586**
			(0.073)	(0.020)	(0.083)	(0.022)	(0.085)	(0.027)
$\Delta CS_{t+2, t+1}$					0.0186	0.0382	0.0983	0.0487*
					(0.097)	(0.024)	(0.107)	(0.026)
$\Delta CS_{t+3, t+2}$							0.1641*	0.0388**
							(0.098)	(0.018)
CAR minus KR, t-1	0.4765	-0.1245	0.5116	-0.1174	-0.6108	-0.2080	-0.3623	-0.2461
	(0.942)	(0.330)	(0.823)	(0.343)	(1.052)	(0.393)	(1.185)	(0.431)
Log Assets, t-1	-0.0424	-0.0384	-0.0356	-0.0373	-0.0884	-0.0336	-0.0717	-0.0250
	(0.101)	(0.041)	(0.087)	(0.042)	(0.105)	(0.044)	(0.121)	(0.048)
ROA, t-1	-16.6507	2.6005	-16.9805	3.2496	-10.4144	2.7779	-6.4096	2.3620
	(14.222)	(3.904)	(15.089)	(4.005)	(17.013)	(4.636)	(17.291)	(4.941)
Liquid / Total Assets, t-1	0.5876	0.0288	0.5237	0.0178	0.4254	0.0474	0.6721	0.0410
	(0.385)	(0.093)	(0.393)	(0.097)	(0.411)	(0.088)	(0.456)	(0.104)
RWA / Assets, t-1	0.3810	-0.1197	0.1665	-0.1103	-0.0244	-0.1033	0.2176	-0.0849
	(0.338)	(0.163)	(0.274)	(0.166)	(0.340)	(0.138)	(0.432)	(0.195)
Observations	2,741	2,741	2,626	2,626	2,511	2,511	2,292	2,292
R-squared	0.043	0.159	0.056	0.159	0.054	0.167	0.071	0.176
Product x Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Weights	No	Yes	No	Yes	No	Yes	No	Yes
Cumulated effect			-0.0591	0.0545	-0.242	0.0868	0.418	0.164
p-value			0.696	0.158	0.388	0.293	0.267	0.182

Robust standard errors in parentheses

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

Figure A1. Peru: Actual Versus Required Capital

Actual regulatory capital in grey, required capital in blue

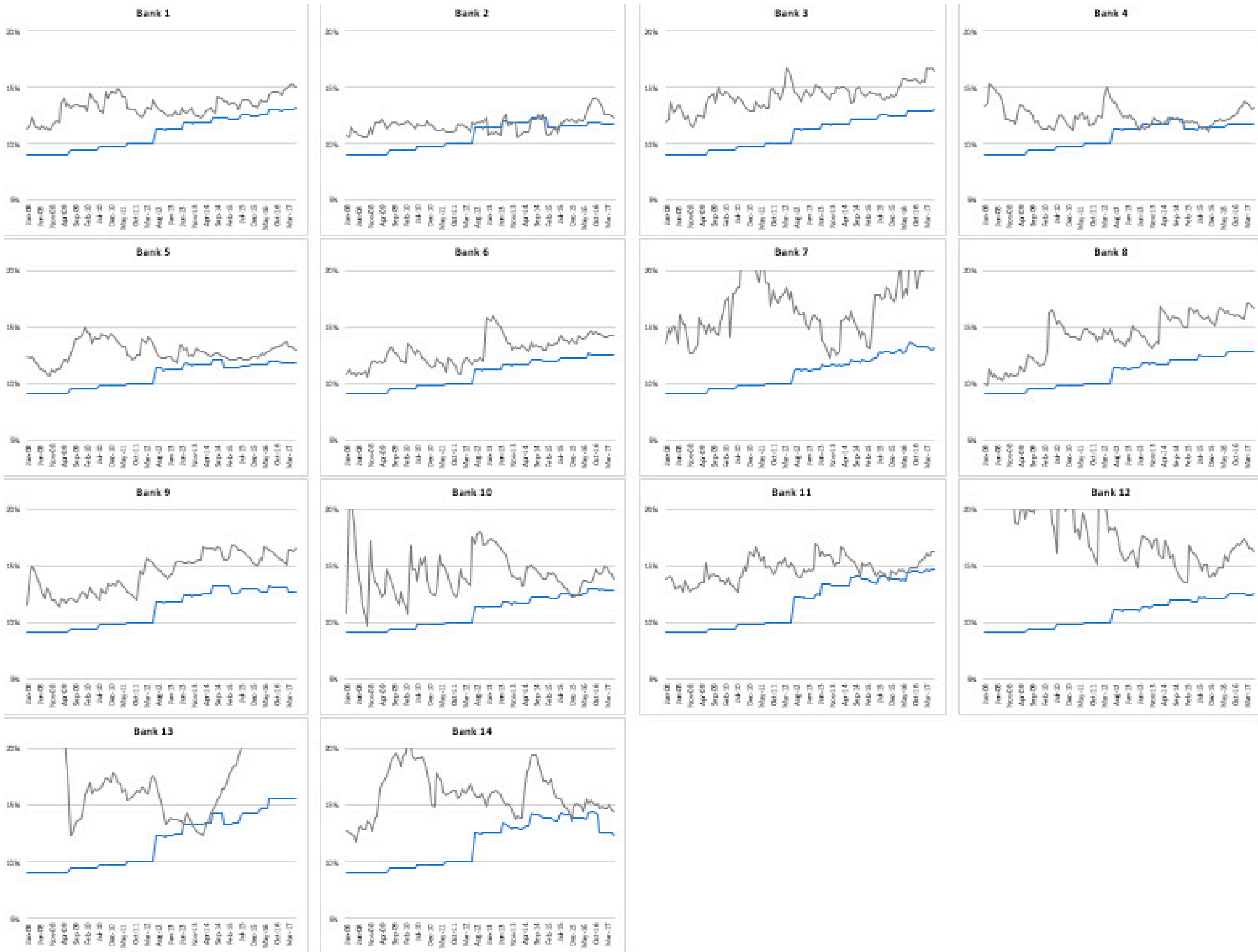

Figure A2. Peru: Impulse Response Function for Product-level Capital Surcharges

Response of cumulative credit growth to a 1 pp. rise in capital surcharge, assuming rise and "shock" both occur at time zero. The estimates are based on unweighted regressions with all the controls of Table 5 in the main text.