MACROPRUDENTIAL FX REGULATIONS AND SMALL FIRMS: UNINTENDED CONSEQUENCES FOR CREDIT GROWTH

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Abstract

Macroprudential FX regulations aim to reduce systemic currency-mismatch risks, yet their distributional effects on firms' access to credit remain poorly understood. This paper studies Peru's 2014 dedollarization policy, which sharply increased reserve requirements on banks' foreign-currency liabilities in proportion to their dollar lending to nontradable firms. Exploiting cross-sectional variation in banks' exposure and using administrative loan-level data covering the universe of firms, I find that moving from the median to the 75th percentile of exposure reduces growth in total new loans by roughly 10 percentage points for micro and small firms, with no significant effects for medium or large firms. Larger firms absorb the shock by reallocating borrowing across banks and into local currency credit, whereas micro firms experience sharp declines in both dollar and total credit, higher borrowing costs, and modest employment losses. The results highlight a trade-off between macroprudential objectives and credit access for small firms.

Keywords: macroprudential FX regulations, currency mismatch, small firms, emerging markets, borrowing constraints, bank lending channel.

JEL classification: E43, E58, F31, F38, F41.

Resumen

La regulación macroprudencial en moneda extranjera busca reducir los riesgos sistémicos derivados del descalce de monedas; sin embargo, sus efectos distributivos sobre el acceso de las empresas al crédito siguen siendo poco comprendidos. En este documento se estudia la política de desdolarización aplicada en Perú en 2014, que aumentó de forma pronunciada los requisitos de reservas sobre los pasivos en moneda extranjera de los bancos en proporción a su volumen de préstamos en dólares a empresas no transables, es decir, aquellas no involucradas en el comercio internacional. Aprovechando la variación transversal en la exposición de los bancos y utilizando los datos administrativos sobre préstamos que cubren el universo de empresas, se concluye que pasar del percentil mediano al percentil 75 de exposición reduce el crecimiento de nuevos préstamos totales de aproximadamente 10 puntos porcentuales para las micro y pequeñas empresas, sin efectos significativos para las empresas medianas o grandes. Las empresas de mayor tamaño absorben el impacto reasignando su endeudamiento entre los bancos y hacia el crédito en moneda local, mientras que las microempresas experimentan fuertes caídas tanto en el crédito en dólares como en el crédito total, mayores costes de financiación y pérdidas de empleo. Los resultados ponen de manifiesto una disyuntiva entre los objetivos macroprudenciales y el acceso al crédito para las empresas más pequeñas.

Palabras clave: regulaciones macroprudenciales en moneda extranjera, descalce de monedas, pequeñas empresas, mercados emergentes, restricciones de crédito, canal de préstamo bancario.

Códigos JEL: E43, E58, F31, F38, F41.

1 Introduction

Nontradable firms[1] in emerging economies frequently rely on dollar-denominated borrowing, exposing their balance sheets to exchange-rate fluctuations (Bruno and Shin, 2015; McCauley et al., 2015) and, indirectly, exposing their lenders to higher credit risk (Niepmann and Schmidt-Eisenlohr, 2022). Because these risks can amplify financial instability, many emerging economies have introduced macroprudential foreign-exchange (FX) regulations that limit banks' incentives to lend in dollars or that raise the cost of dollar funding.[2]

While these measures aim to mitigate systemic vulnerabilities, their distributional consequences for firms' access to credit remain poorly understood. In particular, if foreign-currency borrowing serves as a means for financially constrained firms to access *cheaper* credit, FX-targeted regulations may impose disproportionate costs on smaller borrowers.[3] In that sense, the same policies that reduce systemic risk could also tighten firm-level financing constraints, especially for those borrowers lacking access to multiple lenders and credit in multiple currencies.

This paper provides empirical evidence on such distributional trade-offs using the case of Peru's 2014 credit dedollarization policy, a particularly strict and binding macroprudential intervention that directly targeted banks' foreign-currency lending to nontradable firms. Announced in December 2014, the regulation increased the reserve requirement rate on banks' dollar liabilities in proportion to the ratio of their December 2015 stock of dollar loans to their September 2013 level. This gave banks one year to reduce their dollar lending in order to avoid the increase in reserve requirements. A key feature of the policy is that its intensity varied across banks. Institutions more reliant on dollar funding as a share of assets, and therefore holding larger stocks of dollar loans, faced a stronger increase in reserve requirements.[4]

The stated objective of this intervention was to reduce currency-mismatch risks on domestic firm's balance sheets while preserving credit flows for trade-related activities, which were explicitly exempt from the policy rule (Castillo et al., 2016).

[1] Defined as firms that are neither exporters nor importers.

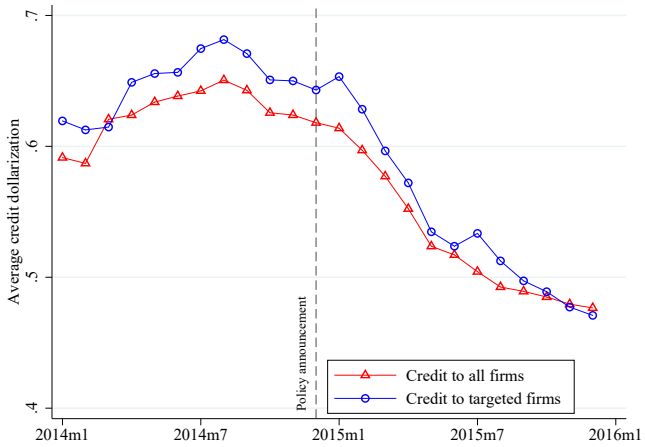
[2] Examples include Peru, Bulgaria, Croatia, and Romania (see the IMF 2017 Macroprudential Policy Survey).

[3] See DiGiovanni et al. (2021), Ivashina et al. (2023) and Acosta-Henao et al. (2025) for evidence of deviations from uncovered interest parity using bank loan rates.

[4] Banks in partially dollarized economies typically align the currency composition of their assets and liabilities; see, for example, Keller 2019, Brown et al. 2014, and Tobal 2018.

Figure 1 shows the main objective was achieved. Banks' average dollarization ratio of commercial lending fell by more than 10 percentage points within a year of the announcement. When considering only non-tradable firms with a pre-policy history of dollar debt—the actual target of the policy—the decline in dollarization is similarly pronounced, confirming that the policy tightened FX credit precisely for the currency mismatched firms.

Figure 1: Banking System Average Credit Dollarization Ratio

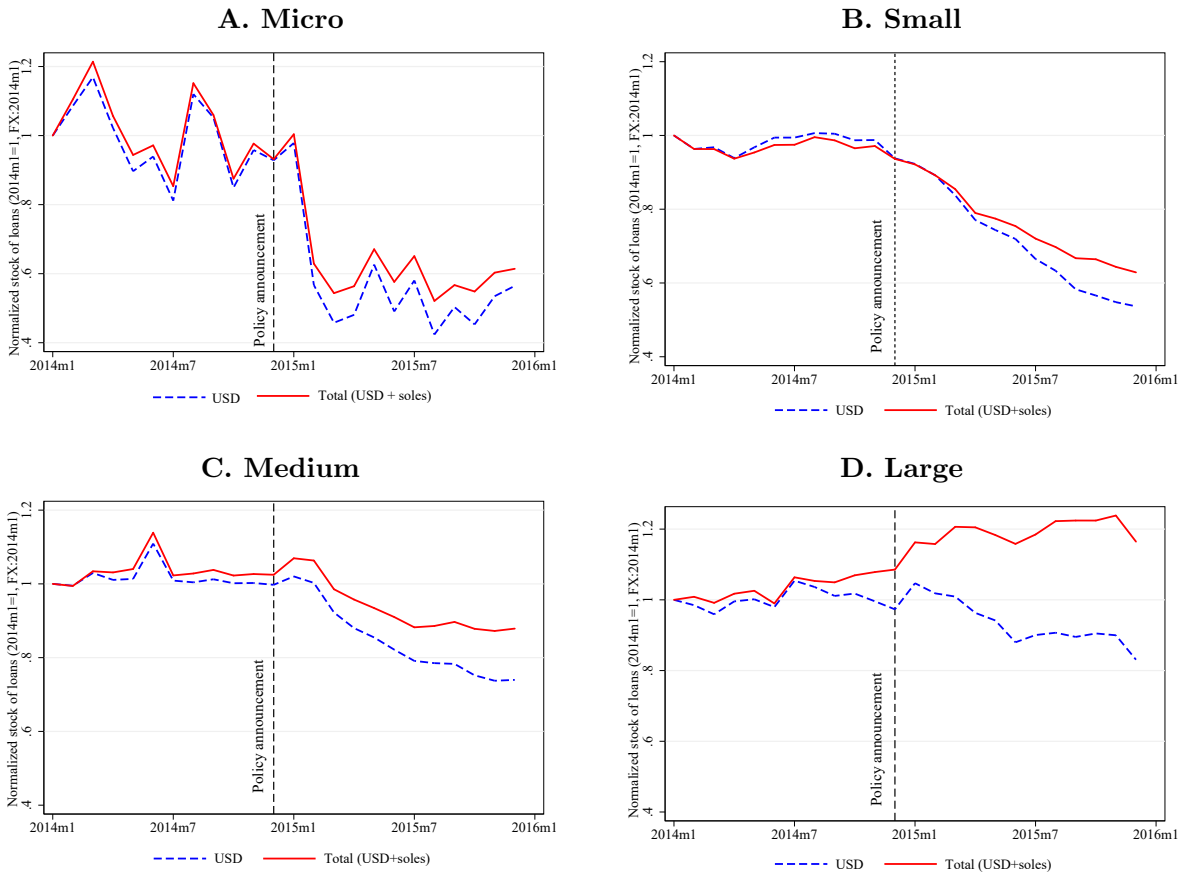


Notes: Figure 1 plots the evolution of the average dollarization ratio of banks' commercial credit portfolios. The series with hollow triangles includes all firm loans, while the alternative series restricts the sample to non–trade-related credit to firms with strictly positive dollar debt (targeted firms) at the policy announcement.

However, Figure 2 shows that the contraction in dollar lending was substantially sharper for smaller firms. Moreover, once total credit is considered (dollar plus local-currency, sol), the decline persists for micro, small, and medium firms, indicating only partial substitution into sol-denominated loans. In contrast, total lending to large firms remains broadly stable, consistent with an adjustment that occurs primarily through a shift in the currency composition of their debt rather than a reduction in overall borrowing.

I exploit the cross-sectional variation in banks' exposure to the regulation to identify its effects on firms' credit dynamics. In particular, I estimate a difference-in-differences specification to test whether higher exposure leads to a sharper contraction in dollar lending to smaller firms relative to larger ones. In addition, I track the growth of new total loan issuance (in dollars and soles) to assess whether affected firms contemporaneously substituted from dollar to local currency borrowing in response to the policy.

Figure 2: Evolution of Dollar and Total (Sol + USD) Loans by Size Category



Notes: Figure 2 shows the evolution of the normalized stock of outstanding dollar loans for targeted firms (dashed blue line) and the normalized stock of total credit (sol + dollar) for the same sample of targeted firms (solid red line). Panels correspond to each firm-size category.

The analysis combines several data sources: a confidential credit register on the universe of loans to non-financial firms, compiled by the financial regulator (SBS) and accessed through the Central Reserve Bank of Peru (BCRP); publicly available bank balance-sheet data collected by the Peruvian financial regulator (SBS); firm-level administrative records from the tax authority (SUNAT); and monthly reports on banks' foreign-currency liabilities from the Central Bank of Peru (BCRP). Merging these sources allows me to observe, at monthly frequency, both the evolution of dollar and sol-denominated lending and the characteristics of firms and banks that may shape their exposure to the policy.

The validity of my identification strategy relies on the following assumptions: First, firms of different sizes were not endogenously sorted across banks with varying exposure to the regulation. I show that, before the policy, the size composition of banks' loan portfolios is uncorrelated with

their exposure, even after conditioning on banks' capitalization, credit risk, and loan-to-asset ratios.

Second, differently exposed banks act as valid counterfactuals. I verify that credit from differently exposed banks follows parallel pre-trends, for each size category, and that banks are balanced on key observables that could shape lending behavior.

Third, the estimated effects in this paper capture the impact of the 2014 credit dedollarization policy itself rather than other concurrent macroeconomic or regulatory developments. Although the policy was implemented amid a depreciatory trend of the sol that began well before the announcement,[5] the absence of differential pre-trends across banks with varying exposure rules out the possibility that these external macroeconomic conditions drive the results. Moreover, earlier dedollarization measures—mainly reserve-requirement adjustments introduced in 2013—had no persistent effects on banks' lending behavior during my sample period. I also rule out that post-policy macroeconomic or regulatory shocks explain my main findings.

Fourth, banks did not anticipate the dedollarization policy. While the recent history of gradual dedollarization initiatives makes it impossible to completely rule out policy expectations or information leakages, the evidence does not support anticipatory behavior. If banks had foreseen the regulation, they would arguably have begun contracting foreign-currency lending before the announcement, given their highly dollarized liabilities. Instead, dollar lending contracts only after the policy is introduced. Once firm-size heterogeneity and fixed effects are accounted for, pre-policy trends in both dollar and total loans are statistically indistinguishable across banks with different exposure levels, supporting the assumption of no anticipatory responses.

Lastly, demand shifts are not correlated with banks' exposure to the regulation. Because most micro, small, and many medium firms borrow from a single bank, including firm-time fixed effects would eliminate the bulk of the sample. Instead, I include granular location–industry–month fixed effects to absorb local and sectoral demand shocks. Such fixed effects provide a solid alternative to control for demand variation in credit registries with mostly single-bank borrowers (Degryse et al., 2019).

Results using bank-firm level data show that the policy generated a significant intensive margin

[5] This depreciation episode started after the U.S. "taper tantrum" in May 2013 and lasted until the December 2015 policy "liftoff."

contraction in the supply of new dollar loans, with heterogeneous effects across firm size. I find that a 10 percent increase in a bank's exposure to the regulation—roughly the shift from the median to the 75th percentile of the exposure distribution—reduced the growth rate of new dollar lending by about 10.8 percentage points for micro firms and 12.9 percentage points for small firms. Effects for medium firms are modest ($\approx$ 3.2 p.p.) but not statistically significant at conventional levels, while large firms experience no significant contraction. Thus, the decline in dollar credit is concentrated among the smallest and most financially constrained borrowers. Moreover, results show no evidence of significant substitution toward (relatively more expensive) local-currency borrowing from the same bank.

Firms could also have mitigated the supply contraction by borrowing from less-exposed banks. To examine this channel, I aggregate credit across all lender relationships and estimate the effect of a firm's exposure—defined as the loan-share-weighted average of its banks' exposure—on the growth rate of new lending.

At the firm level, the results show no full offset of the shock. A 3 percent increase in exposure—moving from the median to the seventy-fifth percentile—reduces dollar loan growth by about 4.1 percentage points for micro firms and by 9.7 percentage points for small firms. Medium firms experience a milder and statistically insignificant decline of 1.4 percentage points, while large firms remain unaffected.

To directly test whether access to multiple lenders mitigates the impact of the policy, I exploit cross-sectional variation in the number of pre-policy bank relationships. Interacting exposure with the number of bank relationships shows that each additional lender attenuates the negative policy effect on new dollar loan growth by about 0.9 percentage points. This mitigating effect applies primarily to small and medium firms, which have meaningful variation in the number of lenders. Importantly, there is no corresponding increase in sol-denominated lending, indicating that intensive-margin substitution occurred within dollar credit across banks rather than through a shift into local-currency loans.

Beyond the contraction in loan volumes, the policy also affected firms' likelihood of obtaining new loans. Moving from the median to the 75th percentile of the firm-exposure distribution reduces

the probability of issuing a new dollar loan by about 0.17 p.p. for small firms, 0.30 p.p. for medium firms, and 0.19 p.p. for large firms, with no significant change for micro firms. This pattern indicates that micro firms, despite substantial intensive-margin contractions, did not exit the credit market but continued borrowing at much smaller scales, reflecting their limited ability to substitute into more expensive sol loans. By contrast, the probability of issuing sol-denominated loans increases significantly with firm size, offsetting the extensive-margin contraction in dollar-loan issuance for small, medium and large firms.

To shed light on the mechanisms underlying these heterogeneous quantity responses, I examine the behavior of lending rates across currencies and firm sizes. Using bank-month average interest rate data by size segment, I find that more exposed banks raised interest rates on dollar loans to micro firms by about 1.6 percentage points, while rates for small, medium, and large firms remained unchanged. No significant effects are found for sol-denominated lending rates across any size group. These patterns indicate that the policy tightened financing conditions primarily for the most constrained borrowers. Micro firms experienced both lower borrowing volumes and higher interest rates, consistent with a contraction in credit supply combined with very limited scope to reallocate borrowing across lenders or currencies. In contrast, small firms saw a sizable drop in dollar lending without a rise in borrowing costs, likely because part of their dollar-loan demand shifted toward sol credit, mitigating upward pressure on interest rates. Similarly, medium and large firms exhibited stable lending rates despite changes in loan composition, consistent with their greater financial flexibility and their ability to substitute across banks and currencies in response to the policy.

I also examine whether the credit contraction induced by the policy had real effects on firms. Using cross-sectional variation in exposure and firm-level employment changes between 2014 and 2015, I find that more exposed micro and small firms experienced modest but statistically significant reductions in employment growth. Moving from the median to the 75th percentile of exposure lowered employment growth by about 0.3 p.p. for micro firms and 0.26 p.p. for small firms, with no effects for medium or large firms. These results indicate that the policy's real impacts were concentrated among the most financially constrained firms—those facing the sharpest credit contractions and the least scope for substitution—while larger firms absorbed the shock without

meaningful employment adjustments.

Because universe-level investment data are unavailable, I complement these results with a back-of-the-envelope calculation using survey-based information on firm-level investment in 2014. Under a conservative assumption that bank credit finances 20 percent of investment, the estimated credit contraction implies an investment decline of roughly 15 percent for micro firms and about 9 percent for small firms.

Finally, I examine potential spillovers to firms engaged in international trade, which were excluded from the credit dedollarization targets. Although these firms experienced a notable decline in the dollarization of their outstanding loans, the policy did not affect the dynamics of new lending: neither new dollar nor new sol credit changed significantly. This suggests that the policy successfully reduced the stock of dollar exposures in the financial system without disrupting credit flows to the tradable sector.

The heterogeneous effects of the policy across firm sizes are robust to a wide range of tests. First, I rule out contamination from prior reserve-requirement adjustments and earlier phases of Peru's dedollarization program: placebo estimations using pre-2014 data show no effects correlated with banks' exposure and progressively shorter pre-treatment windows leave the main results unchanged. Second, the findings are not driven by concurrent external shocks. Truncating the sample before the December 2015 U.S. monetary tightening, as well as placebo tests around the August 2015 adjustment of banks' forward-position limits, yields the estimates virtually unchanged. Third, pre-existing borrower risk during the depreciation of the sol does not account for the heterogeneous responses: firms' 2014 non-performing-loan ratios are uncorrelated with the exposure of their lending banks. Fourth, nonbank financial institutions (NBFI) did not absorb the shock. NBFI credit growth shows no systematic response to the policy and offsets at most a small fraction of the decline in bank dollar lending to small firms. Finally, disaggregating by loan type confirms that the contraction is concentrated in cash-flow loans, consistent with banks reducing lending in the riskiest segments rather than uniformly tightening all categories of loans.

Relationship with the literature. This paper contributes to several strands of the literature on macroeconomics and international finance. First, the findings in this paper strengthens the

empirical foundation of the unified theory for dollar dominance in trade and finance (Gopinath and Stein, 2021). By showing that restrictions on dollar lending disproportionately affect small firms, the findings highlight two core mechanisms emphasized in this literature: (i) the role of cheaper dollar credit in expanding financial access—linked to deviations from uncovered interest parity (UIP)[6]—and (ii) the risks created by unhedged dollar borrowing. Relatedly, the policy's stronger impact on non-asset-backed loans suggests that factors like funding costs and access to dollar liquidity are also key in determining firms' ability to secure financing.

Second, to the best of my knowledge, this is the first paper to examine the distributional effects of macroprudential FX regulation on firm financing. This gap reflects both limited administrative microdata—particularly for small firms—and the need for an appropriate policy setting. Peru's 2014 credit dedollarization rule is uniquely suited for this purpose: it tightened the relative cost of dollar borrowing only for nontradable firms (those choosing to bear FX risk), allowing direct insight into the trade-off between exchange-rate exposure and access to cheaper credit. Moreover, Peru is a representative partially dollarized emerging economy, with a significant portion of financial intermediation in foreign currency.[7]

Third, the paper relates to a large literature documenting asymmetric firm responses to financial and macroprudential interventions—such as capital controls (Varela, 2018; Alfaro et al., 2017; Andreasen et al., 2024; De Gregorio et al., 2000; Forbes, 2007; Larrain and Stumpner, 2017) or monetary policy shocks (Gertler and Gilchrist, 1994; Ciccarelli et al., 2015). These studies emphasize that small firms are more exposed to policy tightening because they lack access to external finance or capital markets. However, existing work does not examine how such interventions affect the currency composition of firm borrowing or the degree of substitution across currencies, nor how these substitution margins vary across firm-size. More recently, Chen and Lee (2023) show that European SMEs experienced a pronounced post-Global Financial crisis productivity slowdown relative to large firms. Their findings highlight that credit tightening disproportionately impairs smaller firms' investment in intangibles and thus their growth potential relative to large firms. My results contribute to this literature by showing that similar size-dependent vulnerabilities arise

[6] Recent empirical work using granular bank-level interest rate data also documents UIP deviations in emerging markets: see DiGiovanni et al. (2021) for Turkey , Ivashina et al. (2023) for Peru and Acosta-Henao et al. (2025) for Chile.

[7] See Dalgic (2024) for stylized facts on financial dollarization in emerging markets.

not only in crisis periods, but also under targeted macroprudential FX regulation, and through a different channel: the inability of small firms to substitute across lenders or currencies when dollar funding conditions tighten.

Regarding real effects, my findings are in line with Siemer (2019), who shows that credit supply disruptions during the Global Financial Crisis disproportionately reduced employment growth among small and young firms in the United States. Likewise, the modest employment declines I document for micro and small firms—alongside no effects for larger firms—highlight that credit tightening primarily transmits to real activity through the most financially constrained firms.

Fourth, the paper contributes to the literature on the currency choice of debt by unhedged firms (Basso et al., 2011; Brown et al., 2011; Allayannis et al., 2007; Bruno and Shin, 2015; Ranciere et al., 2010; Salomao and Varela, 2022; Acosta-Henao et al., 2025). Prior work shows that currency mismatch can relax borrowing constraints and support firm growth (Ranciere et al., 2010; Salomao and Varela, 2022). The policy examined here directly reduced the relative cheapness of dollar borrowing for firms bearing exchange-rate risk. The fact that micro and small firms experience the strongest contraction in credit provides new evidence that dollar borrowing—and the associated currency mismatch—plays a key role in easing borrowing constraints and shaping firms' incentives to choose dollar-denominated debt.

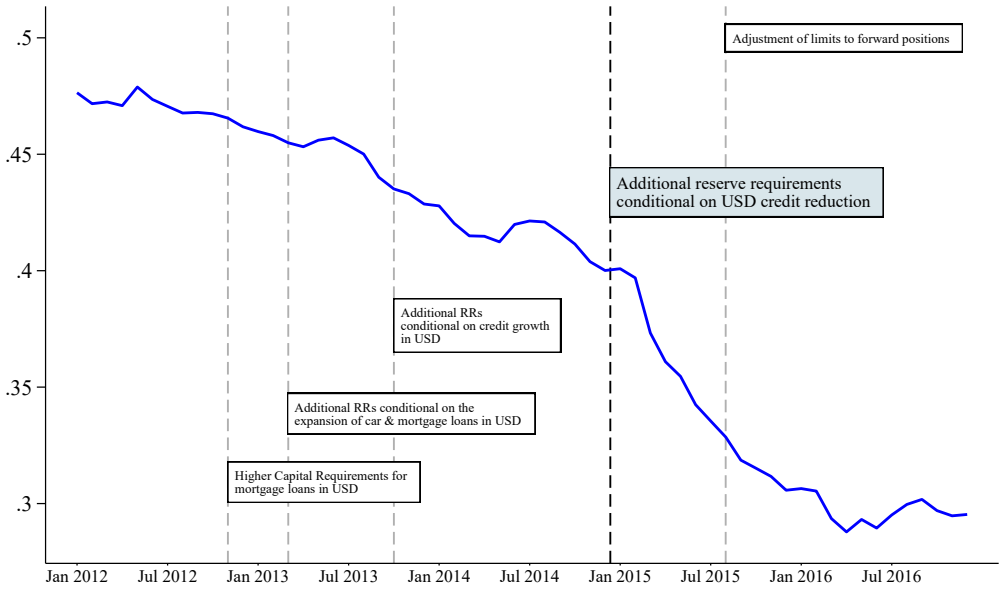
Lastly, the paper contributes to the literature on the unintended consequences of macroprudential FX regulation (Keller, 2019; Ahnert et al., 2021; Aiyar et al., 2014; Cerutti et al., 2017; Reinhardt and Sowerbutts, 2015). This body of work primarily focuses on regulatory arbitrage or the partial shift of FX risk from the banking sector to other sectors of the economy, such as investors and borrowers. To my knowledge, this paper is the first to address the unintended consequences of macroprudential FX regulations from a distributional perspective.

The rest of the paper is organized as follows. Section 2 describes the institutional features of the dedollarization policy. Section 3 presents two stylized facts on the use of dollar loans in Peru. Section 4 introduces the data. Section 5 outlines the identification strategy, and Section 6 presents the main results. Section 7 discusses a series of robustness checks. Section 8 concludes.

2 Institutional Framework

This section describes the institutional setting and policy framework underlying the regulatory intervention analyzed in this paper. In March 2013, the Central Bank of Peru (BCRP) launched a dedollarization program aimed at reducing financial dollarization within the banking system. The macroprudential rationale behind this initiative was to mitigate the systemic externalities arising from agents' decisions—both firms and households—to borrow in foreign currency and incur currency mismatches. In the event of a sharp depreciation, such mismatches can amplify their balance sheet vulnerabilities and deteriorate banks' asset quality, potentially triggering broader financial instability.

Figure 3: Evolution of credit dollarization in the banking system



The analysis in this paper focuses on the December 2014 intervention, which constituted the most stringent phase of the dedollarization program (See Figure 3). This policy aimed to increase the cost of financial intermediation in foreign currency by raising the remunerated reserve requirement (RR) rate on banks' dollar liabilities (i.e., FX reserve requirements).[8] Importantly, the increase in the FX RR was conditional on banks meeting specific credit dedollarization targets.[9] Unlike earlier RR adjustments, the 2014 regulation directly linked the increase in FX RR to actual

[8] FX reserve requirements are remunerated at the one-month London Interbank Offered Rate (LIBOR) minus 50 basis points.

[9] The policy applied to all loan categories, including consumer, mortgage, and business credit.

reductions in banks' foreign-currency loan portfolios. This design represented a significantly stricter and more credit-focused step in the BCRP's dedollarization strategy.

The Policy Rule. The increase in the FX RR rate was heterogeneous across banks and depended on the size of their stock of dollar loans, excluding credit granted to tradable firms.[10] The policy rule operated as follows. In December 2014, banks were informed that by December 2015 (the deadline), they would be subject to an additional reserve requirement (RR) rate, τ_b, on their dollar liabilities. The magnitude of this increase was proportional to each bank's stock of dollar loans at the deadline, $D_b^{Dec2015}$, normalized by its stock of dollar loans in September 2013 (the benchmark), $D_b^{Sep2013}$, provided that this ratio exceeded 0.9:

$$\tau_b = \begin{cases} 0.3 \times \left(\frac{D_b^{Dec2015}}{D_b^{Sep2013}} - 0.9 \right) & if \quad \frac{D_b^{Dec2015}}{D_b^{Sep2013}} > 0.9 \\ 0 & o/w \end{cases} \tag{1}$$

This rule implies that, by the deadline, banks failing to reduce their stock of dollar loans to at least 90% of the benchmark level would face an increase in the reserve requirement rate on their dollar liabilities. The magnitude of the increase is proportional to the deviation of $D_b^{Dec2015}$ from $D_b^{Sep2013}$.[11]

Banks' Exposure to the Policy. Based on this rule, banks' exposure to the policy varied along two dimensions. First, banks more reliant on dollar funding had to allocate a larger share of their assets to meet the higher FX RR rate, giving them stronger incentives to avoid the regulation and to cut back on new dollar lending. Second, among banks with similar funding structures, those with a stock of dollar loans further from the regulatory benchmark faced a larger potential increase in the FX RR rate by the deadline, and thus stronger incentives to reduce their dollar loan portfolios. These two dimensions of exposure are highly correlated. Banks in Peru, like those in most dollarized emerging economies, maintain tight currency matching on their balance sheets to limit exchange-rate risk;[12] as a result, banks with larger expansions of dollar lending tend to

[10] The policy also excluded operations with maturities longer than four years exceeding US$10 million.

[11] This summarizes the main policy rule. Further institutional details are available in Circular N° 006-2015-BCRP (Central Bank of Peru, `https://www.bcrp.gob.pe/en`) and discussed in Castillo et al. (2016). The regulation also set an intermediate target for July 2015 (95% instead of 90%), but since both deadlines were announced in December 2014, the effective treatment period runs from December 2014 to the final stricter deadline in December 2015.

[12] See Keller (2019) for evidence on Peru, and Canta et al. (2006) and Tobal (2018) for other emerging economies.

hold a higher share of dollar liabilities as well. Consequently, the regulation operated much like a progressive tax on dollar liabilities: more dollarized banks, holding larger stocks of both dollar assets and liabilities, were more likely to exceed the regulatory threshold and face higher RR rates applied to a larger liability base.[13]

To avoid potential endogeneity, and because both indicators capture the same underlying variation, I use banks' reliance on dollar funding at the time of the policy announcement (December 2014) as the main exposure indicator (see Section 5).[14]

Additional adjustments in Reserve Requirements. In December 2014, the Central Bank of Peru (BCRP) complemented the previously described policy rule with a sharp increase in the marginal reserve requirement on foreign-currency deposits, raising it from 50 percent to 60 percent, and again to 70 percent in February 2015. The stated objective of this intervention was to complement the credit dedollarization measures by discouraging banks from funding themselves with dollar deposits and encouraging the attraction of local-currency deposits, rather than directly targeting credit growth.[15]

As shown in Figure 4, this unconditional tightening of dollar marginal reserve requirements coincided with a continued easing of local-currency reserve requirements—a trend that had been underway since early 2013. The result was an unprecedented differential widening of roughly 21.5 percentage points between foreign- and local-currency marginal reserve requirements within just two months.[16]

This discontinuous jump occurred simultaneously with the conditional increase in FX RR, and the exposure measure described earlier—reliance on dollar funding—would also capture banks' exposure to this differential change in reserve requirements.

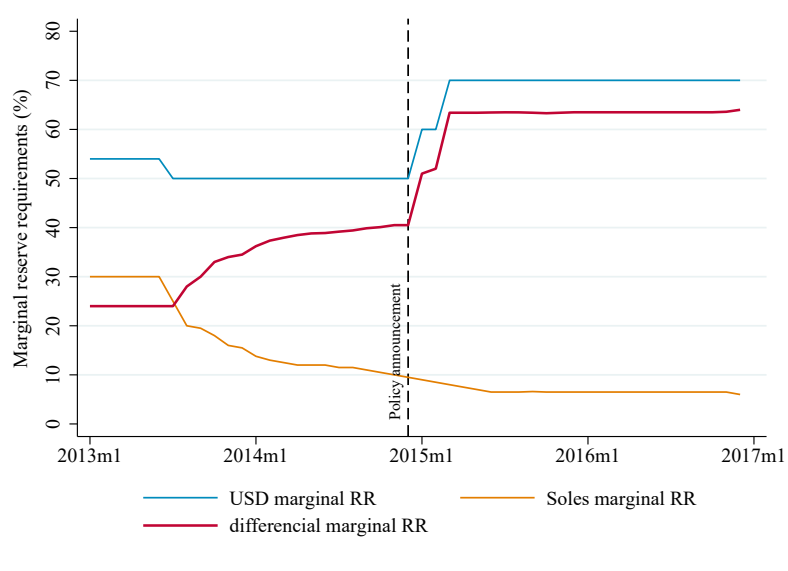
[13]For illustration, let $D_b^{Sep2013} = 100$ and assume that bank b's dollar liabilities equal its dollar loans ($L_b = D_b$). The total cost associated with the RR increase is defined as $C_b = \tau_b L_b$. Bank 1, with $D_1^{Dec2015} = 95$, faces $\tau_1 = 0.3(0.95 - 0.90) = 0.015$ and $C_1 = 0.015 \times 95 = 1.425$, whereas a more dollarized bank, Bank 2, with $D_2^{Dec2015} = 110$, faces $\tau_2 = 0.3(1.10 - 0.90) = 0.060$ and $C_2 = 0.060 \times 110 = 6.600$. Thus, while τ_b rises proportionally to the excess credit exposure of Bank 2 relative to Bank 1—i.e., $(1.10 - 0.90)/(0.95 - 0.90) = 4$—the total burden increases more than proportionally for Bank 2, since both the rate and the (matched) liability base rise with dollarization ($C_2/C_1 > 4$).

[14]Figure A.1, shows that reliance on dollar funding remained stable around the policy announcement, whereas the distance to the benchmark responded endogenously. The main empirical results remain virtually unchanged when using the latter as an alternative exposure indicator (see Appendix B).

[15]BCRP, Inflation Report, January 2015.

[16]The second-largest differential increase was 10 percentage points, observed in October 2004.

Figure 4: Marginal reserve requirements



Notes: Figure 4 shows the evolution of marginal reserve requirement rates on dollar and sol liabilities, as well as the differential marginal reserve requirement between currencies.

Overall, the empirical results in this paper can thus be interpreted as capturing the combined impact of these two contemporaneous measures—the conditional increase in FX reserve requirements linked to credit-dedollarization targets and the simultaneous, discontinuous widening of the differential between FX and local-currency reserve requirements—which together reinforced banks' incentives to reduce foreign-currency lending and funding.

Other relevant macroprudential actions sourrounding the policy. As shown in Figure 3, since 2012 the Peruvian authorities implemented a series of prudential policies aimed at reducing credit dollarization. The first measure—introduced in November 2012 by the Superintendency of Banks (SBS)—focused primarily on *mortgage* and *car* loans, through higher capital requirements for dollar lending to these segments. Similarly, in February 2013, the Central Bank of Peru (BCRP) raised reserve requirement rates on FX liabilities for banks whose mortgage and car loans exceeded specified thresholds. In September 2013, the Central Bank introduced the first policy during this period that directly targeted business loans. This measure increased the reserve requirement on banks' dollar liabilities conditional on the expansion of total dollar credit not exceeding specific thresholds.[17]

[17]Specifically, additional reserve requirements rose by 1.5 percentage points when total outstanding dollar credit (excluding trade-related credit) exceeded 1.05 times the benchmark balance set in September 2013, by 3.0 percentage points when it exceeded 1.10 times, and by 5.0 percentage points when it surpassed 1.15 times the benchmark balance.

To avoid potential confounding effects from earlier dedollarization measures, the sample used in my empirical analysis is restricted to one-year windows before and after the implementation of the December 2014 credit dedollarization policy: January 2014–December 2015.[18] Although the September 2013 adjustment lies outside this period, its design—linking reserve requirements to banks' foreign-currency lending—raises the possibility of persistent effects that could contaminate the estimated impact of the December 2014 policy. I address these concerns in robustness checks of Section 7.

Finally, it is worth discussing the capital control measures implemented in January 2011, analyzed in Keller (2019). Although these policies were not primarily designed to influence credit dollarization, but rather to curb short-term capital inflows, they had unintended consequences for banks' foreign-currency intermediation. In particular, by restricting the use of forward contracts—which banks had relied on to hedge foreign dollar liabilities while extending loans in soles—the policy increased banks' incentives to lend in dollars instead. As Keller (2019) documents, this led treated banks to expand dollar credit even to unhedged firms (i.e., those without foreign-currency revenues), thereby increasing the exposure of the corporate sector to exchange rate risk.

This episode is conceptually related but opposite in direction to the policy analyzed in this paper. While the 2011 capital control unintentionally increased dollar lending by limiting banks' ability to hedge, the 2014 credit dedollarization program explicitly sought to reduce dollar lending by raising reserve requirements conditional on FX credit reduction.

Moreover, since the 2011 policy was implemented three years before the sample period used in this study, and its estimated effects on dollar credit had dissipated by the last quarter of 2011 (Keller, 2019), confounding effects unlikely. However, the financial regulator (SBS) further adjusted the limits on forward positions in August 2015, near the end of the post-policy period analyzed in this paper (Figure 3). This measure could, in principle, have affected banks' incentives to lend in foreign currency, thereby influencing the main estimates of the policy's effect. Robustness checks in Section 7 account for the implications of this later adjustment to forward limits.

[18] Evidence from the IBRN cross-country database on prudential policy instruments (Cerutti et al., 2017) confirms that no other macroprudential instruments—such as capital requirements, loan-to-value (LTV) limits, exposure limits, or interbank concentration regulations—were adjusted during this period (January 2014–December 2015). See Figure A.2.

2.1 Policy Objectives and Trade-offs

The 2014 dedollarization policy achieved its intended macroprudential goal of reducing foreign-currency credit among firms not engaged in international trade—precisely those most exposed to exchange-rate risk. As shown in Figure 1, dollar lending to these firms fell sharply following the policy. Table 1 further shows that this decline occurred across all firm-size categories, including among firms with a pre-policy history of dollar borrowing, implying a sizable reduction in potential currency mismatches and balance-sheet exposure to exchange-rate fluctuations.[19]

Table 1: Credit Dollarization of Business Loans (Excluding Trade Loans)

	All firms		USD borrowers	
Firm size	**2014**	**2015**	**2014**	**2015**
Micro	34.1	16.5	89.5	50.3
Small	25.4	14.4	69.7	54.1
Medium	58.2	42.3	73.8	55.9
Large	48.7	32.5	52.4	33.6

Notes: Table reports, for December 2014 and December 2015, (i) the share of total commercial loans denominated in foreign currency for all firms by size segment, and (ii) the corresponding share computed only for firms with strictly positive dollar debt ("targeted firms"). Both measures exclude trade-related loans.

Since the adoption of inflation targeting in 2002, the Peruvian Central Bank has allowed for exchange-rate fluctuations of up to 7 percent within a two-week window. Such flexibility implies that currency mismatches on firms' balance sheets can translate into solvency risk. Estimates by the Superintendency of Banks (SBS) using confidential balance sheet data show that, among large firms, a 20 percent exchange-rate depreciation would render borrowers representing 25.6 percent of total dollar credit insolvent, while a 10 percent depreciation would do so for firms accounting for 6.5 percent of dollar credit (Ivashina et al., 2023). Although comparable data for micro, small, and medium firms are unavailable, their weaker balance sheets and limited access to hedging instruments suggest that similar exchange rate shocks would likely generate even higher default risks. In this sense, the policy successfully reduced systemic exposure to exchange-rate risk—one of its core macroprudential goals.

At the same time, borrowing in dollars is, on average, about two percentage points cheaper than in soles[20], and even more so for the smallest firms (see Figure A.3). Dollar credit thus constitutes an

[19] Throughout the paper, I follow the size definitions used by the Peruvian financial regulator (SBS): *micro*, *small*, *medium*, and *large*. See Table A.2 for details.

[20] This is documented by Ivashina et al. (2023) using large firms' loan interest rates.

important source of relatively low-cost financing, particularly for financially constrained borrowers. Dedollarization policies therefore face an inherent trade-off: while reducing systemic risk and lowering firms' exposure to exchange rate fluctuations, they can inadvertently constrain credit access if the supply of dollar loans tightens without an adequate substitution toward sol-denominated credit. This risk is most pronounced for small and younger firms that are highly bank-dependent (Custódio et al., 2013; Gertler and Gilchrist, 1994), maintain few lending relationships, and for whom access to relatively more expensive sol credit can become more binding than for larger firms.

3 Two Stylized Facts on Firms' use of Dollar Loans

Before turning to the empirical strategy, this section documents two stylized facts about firms' use of dollar credit in the Peruvian banking system. These patterns provide context for understanding the mechanisms behind the policy effects analyzed later in the paper.

FACT 1: Firms borrow in dollars despite lacking effective hedges.

Firms borrowing in dollars can limit their exposure to exchange-rate risk in three ways: (1) through financial hedging using FX derivatives, (2) by adjusting sales prices in response to exchange-rate changes, or (3) by naturally matching dollar liabilities with dollar revenues (e.g., exporters).

In practice, few firms hedge effectively. Table 2 shows that only about 4 percent of medium-sized and 18 percent of large firms with dollar debt use FX derivatives, while smaller and nontradable firms almost never do. The use of derivatives is somewhat higher among tradable firms, but overall remains very limited.[21]

Nontradable firms are also unlikely to hedge through pricing or revenue matching. They lack dollar income, and price pass-through to consumers is incomplete across sectors. Figure A.4 shows that dollar loans are widely distributed across nontradable industries—mainly Commerce, Manufacturing, Real Estate, Transport, and Electricity—where domestic-currency pricing predominates. Even in these sectors, price indices show only limited comovement with exchange-rate fluctuations (Figure A.5).

The third channel—natural hedging—is largely irrelevant in this context because the policy

[21] Consistent with evidence from other emerging markets; see Alfaro et al. (2022).

targeted nontradable firms, which by definition lack dollar receivables from exports to hedge their dollar liabilities.[22]

Despite this exposure, many firms still borrow in dollars, possibly reflecting the relatively lower cost of dollar credit compared with sol-denominated loans (see Figure A.3).[23]

Table 2: Share of firms issuing FX derivatives contracts

	Firms with USD debt		Tradable firms		NT firms with USD debt	
Size	Obs.	Share (%)	Obs.	Share (%)	Obs.	Share (%)
Micro	0	0.00	0	0.00	0	0.00
Small	24	0.16	0	0.00	24	0.17
Medium	546	3.89	125	6.43	421	3.47
Large	397	17.59	177	26.50	221	13.89

Notes: Table 2 reports the number and share of nonfinancial firms engaging in FX derivatives contracts with commercial banks, by firm size category. Columns (1)–(2) include firms with outstanding USD-denominated debt. Columns (3)–(4) restrict the sample to tradable firms, and Columns (5)–(6) report results for nontradable firms with USD debt. All figures correspond to December 2014 (policy announcement date).

FACT 2: Dollar borrowing behavior differs across firm sizes.

Two clear regularities emerge from the data on unhedged firms—defined as nontradable borrowers that do not use FX derivatives. First, larger firms are more likely to access dollar credit. As shown in Table 3, the share of unhedged firms borrowing in dollars rises sharply with firm size, from roughly one in four micro firms to more than four in five large firms (column 2). Using alternative indicators of firm size confirms the same pattern: Figure 5 shows that the share of firms with dollar debt increases monotonically across employment and age quantiles.

Table 3: Firm size and debt dollarization

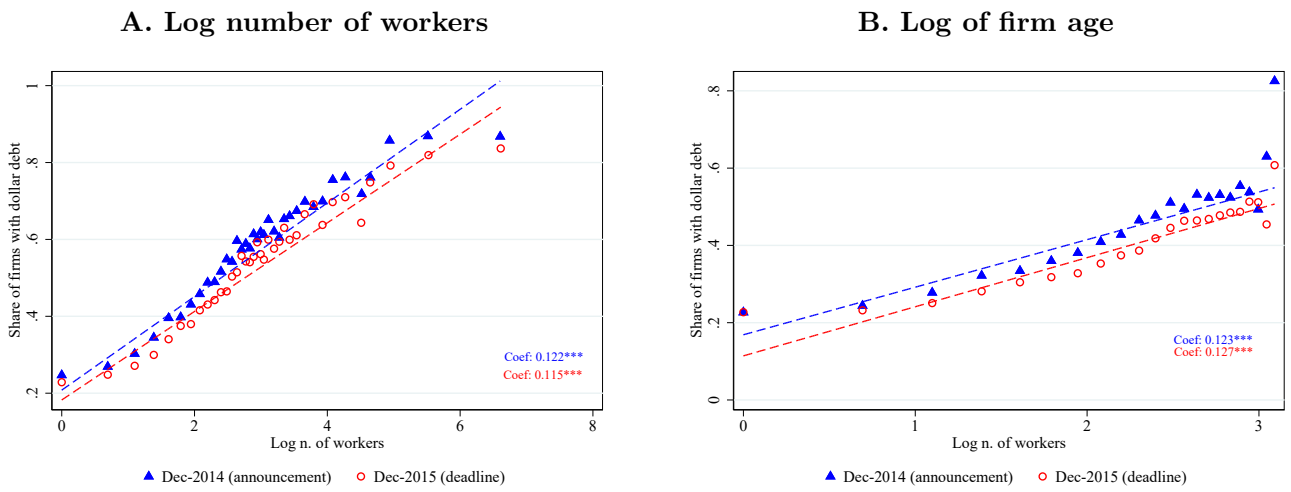
Size	% USD debt (unhedged firms' debt)	% unhedged firms with USD debt	Av. debt dollarization (unhedged firms with USD debt)
Micro	36.11	24.05	81.72
Small	25.89	29.86	72.31
Medium	59.54	66.79	70.19
Large	51.63	84.78	68.92

Notes: Table 3, column (1) reports the share of dollar denominated loans to unhedged firms—defined as non-exporters and non-importers that do not issue FX derivatives. Column (2) shows the share of *unhedged* firms that borrow in dollars, and Column (3) the average debt dollarization among *unhedged* firms with dollar debt. All figures correspond to December 2014 (policy announcement date).

[22] Evidence from a comparable partially dollarized emerging economy, Chile, shows that even natural hedging is limited; see Alfaro et al. (2022).

[23] The literature attributes this differential either to deviations from uncovered interest parity in macro rates Salomao and Varela (2022), DiGiovanni et al. (2021) or to a "dollar deposit discount" in emerging markets Dalgic (2024), Bocola and Lorenzoni (2020), Ivashina et al. (2023), Gopinath and Stein (2021).

Figure 5: Binscatter (mean value): Share of unhedged firms with dollar debt and size



Notes: Panels A and B of Figure 5 show binned scatter plots of the average share of unhedged firms borrowing in dollars (y-axis) against the quantiles of the number of workers and firm age (x-axis), respectively. The series include all unhedged firms active at the policy announcement date (December 2014, blue filled triangles) and at the policy deadline (December 2015, red hollow circles). Dashed lines represent linear fits. The number of quantiles in Panel B is 100.

Second, among unhedged firms that decide to borrow in dollars—which are mostly the firms impacted by the regulation—small firms have higher dollarization ratios on average compared to large firms (column 3 of Table 3). Figure 6 also illustrates this inverse relationship across employment and age quantiles. In other words, firms with fewer workers or shorter credit histories, once they borrow in dollars, rely more heavily on foreign-currency debt. As a result, these firms are more exposed to exchange-rate risk.

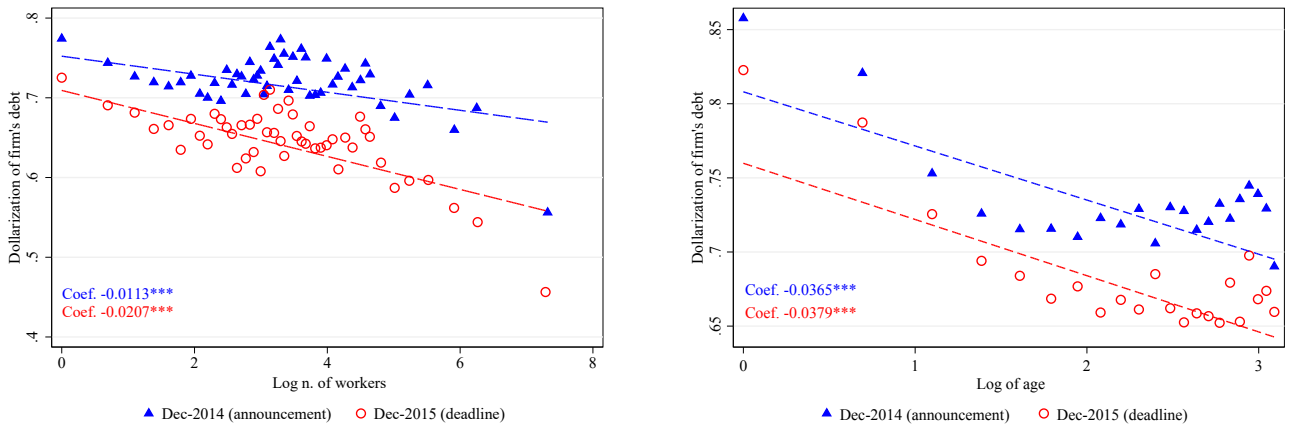
This pattern is consistent with financial frictions shaping firms' currency choices. For small and young firms, cheaper dollar loans can partially relax borrowing constraints, enabling them to access larger credit volumes than they could otherwise obtain in local currency. In contrast, larger and typically less constrained firms, able to borrow in both currencies, maintain lower average dollarization ratios.[24]

Taken together, these facts suggest systematic differences in dollar borrowing behavior across firm sizes. The empirical analysis therefore accounts for potential size-related demand patterns to more cleanly identify heterogeneous supply responses to the policy.

[24] Importantly, this pattern is likely not driven by industry composition. Dollar borrowing among unhedged firms is widespread across sectors. As shown in Figure A.6, both the share of dollar loans and the share of unhedged firms with dollar debt are distributed broadly across industries within each size group.

Figure 6: Binscatter (mean value): Dollarization ratio of unhedged firms and size

A. Log number of workers **B. Log of firm age**



Notes: Panels A and B of Figure 6 show binned scatter plots of firms' debt dollarization ratio (y-axis) against the logarithm of the number of workers and the logarithm of firm age (x-axis), respectively. The sample includes all unhedged firms borrowing in dollars and active at the policy announcement date (December 2014, blue filled triangles) and at the policy deadline (December 2015, red hollow circles). Dashed lines represent linear fits. The number of quantiles in Panel B is 100.

4 Data and Summary Statistics

To implement the empirical analysis, I combine four primary data sources. First, a confidential credit register on the universe of loans to non-financial firms, compiled by the Superintendency of Banks (SBS) and accessed through the Central Reserve Bank of Peru (BCRP). Second, banks' monthly balance sheets reported to the SBS. Third, administrative firm-level data from the tax authority (SUNAT). Fourth, Central Bank reports on the dollar liabilities of financial institutions.

Credit Register. This is my main dataset. It allows me to construct the outcome variables: the growth rate of new dollar loans and total loans. This database contains monthly balances for the universe of outstanding business loans, in dollars and soles, made by all entities in the financial system. It also contains a detailed classification of the type of loan; in particular, whether the loan is classified as *credit for trade activities*, granted to finance commercial activities related to exports or imports. It also classifies the loans based on the size of the borrower.[25] The sample period covers January 2014 to December 2015, spanning 12 months before and 12 months after the December 2014 policy announcement.

Data on Banks' Balance Sheets. This dataset contains monthly balance sheets for the

[25] See Table A.2 for a definition.

universe of financial institutions, as reported to the financial regulator (SBS).[26] From this source, I construct bank-level ratios such as the net interest margin (NIM), core funding, coverage, liquidity, equity, capitalization, non-performing loans (NPL), and loan-to-asset ratios, as well as the share of dollar loans—by both volume and number—across firm-size segments. These variables are used in the covariate balance analysis, while capitalization, NPL, and loan-to-asset ratios are also included as controls in the empirical specification.

Dataset on Formally Registered Firms. This dataset contains annual information on the universe of active firms registered with the tax authority (SUNAT). It includes each firm's five-digit industrial classification and six-digit geographic location, which are used to control for loan-demand shocks through fixed effects. It also reports firms' sales range,[27] number of workers, and year of establishment, which I use to construct firm age.

Peruvian Central Bank Reports. The bank exposure indicator for December 2014 is calculated using bank's amount of dollar liabilities that is subject to reserve requirements (TOSE I), obtained from compulsory monthly reports sent by financial institutions to the BCRP.[28]

Sample Construction. The credit register classifies firms based on an *SBS code*, while SUNAT classifies firms using a taxpayer identification number (RUC). To merge both datasets, I use a confidential dataset that links the SBS code with the RUC. My empirical strategy relies on the universe of nonfinancial, formally registered firms (i.e., all firms that have a RUC).[29] *Credit for trade activities* is excluded from the analysis, because the regulation targets nontradable firms that are exposed to currency-mismatch risk. Thus, I exclude all firms that issued at least one dollar loan classified as credit for trade activities during the period of analysis.[30] I also exclude all service exporters operating in the tourism sector.

My analysis focuses only on banks in the financial system, excluding all other non-bank financial

[26] Accessible at `https://www.sbs.gob.pe/app/stats_net/stats/EstadisticaBoletinEstadistico.aspx?p=1`

[27] The database defines 15 sales intervals, each containing between 2% and 13% of firms in the sample.

[28] Accessible at `https://www.bcrp.gob.pe/docs/Estadisticas/Cuadros-Estadisticos/cuadro-020.xlsx`

[29] I exclude borrowers without a taxpayer ID who receive business loans under a personal ID, since it is not possible to separate firm-related borrowing from personal use. Their lower formality likely makes them even more vulnerable to the policy, so the estimated effects should be interpreted as a lower bound for small firms.

[30] This restriction limits the possibility that the results are driven by banks reclassifying loans as trade-related in order to avoid the regulation.

institutions.[31] Section 7.4 discusses the importance of these non-bank financial institutions in lending to small firms, and checks whether they absorbed the negative supply shock by exposed banks on lending to smaller firms. To keep banks comparable and avoid confounding effects, I exclude from the main sample those specialized institutions that lend almost exclusively to large corporations or to micro and small firms, and that together account for only a minimal share of total loans in the banking system.[32] Appendix B checks robustness to the inclusion of these institutions.

Additional Datasets. To examine the heterogeneity of the main results across loan types, I use a confidential dataset from the SBS that classifies all loans in the Credit Register according to the type of collateral used in each transaction. Asset-based loans include those secured by real estate, other collateral with stand-alone titles, deposits, or other liquid financial securities. Within the group of non-asset-based loans, I distinguish cash-flow loans (revolving lines and fixed-term loans), leasing, and other loans.

I also use confidential data on foreign-exchange derivatives collected by the SBS, which cover all outstanding contracts between banks and non-financial firms. The dataset reports the notional amount of forwards, cross-currency swaps, and options for each firm.

Summary Statistics. Table A.1 presents summary statistics for the main variables used in the empirical analysis. Panel A reports the dependent variables from the baseline specification (Section 5). The first four columns correspond to the year before the policy announcement (2014), and the next four to the year after (2015). The average monthly growth rates of both dollar and total loans declined among the three smallest firm-size categories. Panel B summarizes banks' average financial ratios in the year preceding the policy, and Panel C reports firm–bank relationship statistics by firm size as of 2014. On average, micro firms maintain relationships with only one bank, and small firms with fewer than two. In the sample, roughly 98% of micro firms, 70% of small firms, and 40% of medium firms borrow from a single bank, compared with less than 1% of large firms.

[31] Such as municipal and rural savings and credit unions.

[32] These banks account, together, for less than 1.9% of total dollar borrowing in the banking system before and after the policy.

5 Identification Strategy

5.1 Empirical Specification

I estimate the impact of the 2014 dedollarization policy using a difference-in-differences design with a continuous treatment. Specifically, I compare the evolution of credit granted by banks with different levels of exposure to the policy before and after its implementation. Bank exposure, $Exposure_b$, is defined as the ratio of dollar funding to total assets, measured in the month of the policy announcement. To capture heterogeneous effects across firm sizes, I estimate the following specification:

$$y_{f,b,t} = \beta Exposure_b \times Post_t + \sum_{s=2}^{4} \delta^s Exposure_b \times Post_t \times Size_f^s \tag{2}$$
$$+ Controls_{b,t} + \alpha_{f,b} + \alpha_{s(f),t} + \alpha_{g(f),i(f),t} + \epsilon_{f,b,t}$$

where $y_{f,b,t}$ denotes the outcome for firm f borrowing from bank b in month t. The dependent variable is either (1) the growth rate of *new foreign-currency loans*, (2) the growth rate of *new total loans* (foreign-currency + local-currency) or (3) the growth rate of *new sol loans*. Dollar values are converted into soles using the January 2014 exchange rate to remove valuation effects due to exchange-rate fluctuations.[33]

The indicator $Post_t$ equals 1 for months after the December 2014 policy announcement, and 0 before. $Size_f^s$ is a dummy for firm-size category $s \in$ {micro, small, medium, large}, with micro firms as the reference group.

The term $Controls_{b,t}$ includes interactions of month dummies with pre-treatment (2014) bank-level characteristics—specifically, bank bins based on capitalization (equity-to-risk-weighted assets), credit risk (non-performing loan ratio), and business model (loan-to-asset ratio). Importantly, Figure 7 shows that, after conditioning on these bank bins, bank exposure to the policy is statistically orthogonal to other key bank characteristics, including the net interest margin, core funding ratio, coverage ratio, liquidity ratios in dollars and soles, equity ratio, and the size composition of banks' loan portfolios.

[33] This valuation adjustment avoids mechanically capturing changes in total credit caused by currency depreciation.

Unobserved, time-invariant heterogeneity in bank-firm relationships is absorbed by bank-firm fixed effects, $\alpha_{f,b}$, which the literature shows to be an important factor determining the terms of credit contracts (Petersen and Rajan 1994; Degryse and Van Cayseele 2000; Santos and Winton 2008; Duqi et al. 2018; among others). Consistent with the patterns documented in Section 3, dollar borrowing behavior varies systematically with firm size. To account for potential size-specific demand shocks that could correlate with bank exposure, I include size–month fixed effects, $\alpha_{s(f),t}$.

The limited number of multiple bank–firm relationships among *micro*, *small*, and *medium*-sized firms implies that including firm–month fixed effects—which absorb firm-specific demand shocks—would reduce the effective sample size by about 85% (see Section 4). Because this restriction would disproportionately retain larger firms, the main specification does not include firm–month fixed effects (Khwaja and Mian, 2008). Instead, following Degryse et al. 2019, I include location–industry–month fixed effects to capture time-varying shocks that may influence credit demand at the sectoral or regional level, such as changes in local economic conditions or exchange-rate-induced fluctuations in borrowers' funding needs.

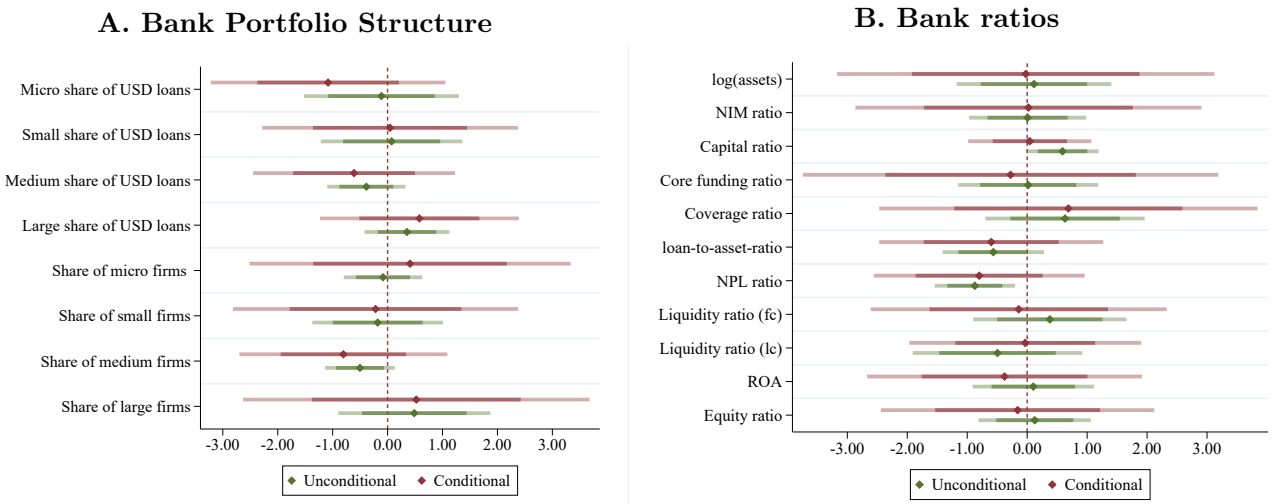
5.2 Validity

The validity of my identification strategy rests primarily on five assumptions. The first is that firms of different sizes are not endogenously allocated across banks with different levels of exposure. To validate this assumption, I examine the distribution of firm sizes—measured by both the volume of credit and the number of firms—within the portfolios of differently exposed banks in the year preceding the policy implementation.

Panel A of Figure 7 reports that the unconditional correlation coefficient between the exposure measure and the share of small firms in differently exposed banks is statistically indistinguishable from zero. This result remains unchanged even when comparing banks within similar groups—such as those with above- or below-median capitalization, loan-to-asset (business model), or non-performing loan (NPL) ratios. Hence, there is no evidence that the heterogeneous effects of the policy could be driven by an endogenous sorting of firms of different sizes across banks with varying levels of exposure.[34]

[34]Figure A.7 shows substantial cross-bank variation in the exposure measure.

Figure 7: Bank Covariate Balance



This figure presents coefficient estimates with 95% (dark bars) and 99% (light bars) confidence intervals for the effect of a one–standard-deviation increase in bank exposure across standardized bank-level variables. "Unconditional" estimates compare banks with different exposure levels, while "conditional" estimates compare banks within groups defined by capitalization, credit risk, and business model—based on the median values of the capital ratio (equity-to-risk-weighted-assets), NPL ratio, and loan-to-assets ratio, respectively. Panel A shows correlations between bank exposure and the share of USD loans (by volume and number) to firms of different sizes. Covariates in Panel B include the net interest margin (NIM), core funding, coverage, liquidity, and equity ratios. Specifically, the NIM is financial income over average productive assets; core funding is deposits over liabilities; coverage is loan-loss provisions over non-performing loans; liquidity is liquid assets over short-term liabilities (in dollars and soles); and equity is equity over total assets.

The second assumption is that differently exposed banks serve as valid counterfactuals—that is, in the absence of the de-dollarization policy, the evolution of credit from differently exposed banks to firms of different sizes would have followed similar trends. I assess the validity of this assumption in two ways. First, I verify the absence of pre-trends by analyzing how the treatment effect on dollar and total loans evolves over time across firm size groups, and whether, before the announcement, there is already a significant declining trend that is not captured by bank controls and the fixed-effects structure in my main specification. Second, I verify that banks are balanced in terms of key observables, including size, core funding, net interest margin (NIM), liquidity coverage, equity, and profitability ratios. Panel B of Figure 7 shows this is true after accounting for observable characteristics such as capitalization, NPL ratio (credit risk), and loan-to-asset ratio (business model), which I use as controls in my main specification in Equation 2. Thus, pre-existing differences in banks' characteristics are unlikely to drive my results.

While monetary and prudential interventions are often responses to broader macroeconomic developments, my third assumption is that the estimated effects in this paper capture the impact

of the 2014 credit dedollarization policy itself rather than other concurrent factors.

The policy was implemented amid a depreciatory trend of the local currency, common across emerging economies after the U.S. "taper tantrum" in mid-2013. However, this trend preceded the regulation and was already well internalized by the markets. By ruling out differential pre-trends across bank exposure and firm-size groups, I show that credit dynamics were not systematically different before the policy announcement, even after depreciation expectations had adjusted.[35]

Moreover, while other macroprudential and monetary policy actions took place in surrounding years, none coincide in timing, intensity, or design with the 2014 regulation. As discussed in Section 7, earlier reserve-requirement adjustments in 2013 were modest, targeted consumer credit, and/or conditioned on credit growth rather than reduction. Other macroprudential instruments—such as loan-to-value ratios, capital requirements, or exposure limits—remained unchanged throughout the period of analysis.

The fourth identifying assumption is that banks did not anticipate the dedollarization policy. If banks had foreseen the measure, they could have adjusted their portfolios in advance, biasing the estimated effects. While the recent history of gradual dedollarization initiatives makes it impossible to completely rule out policy expectations or information leakages, the evidence does not support the presence of anticipatory behaviour. Given their highly dollarized liabilities, banks expecting the regulation would have had incentives to gradually reduce FX lending before the announcement. Instead, the decline in dollar lending occurs only after the policy was introduced.

The apparent pre-policy divergence between dollar and total loans in Panels C and D of Figure 2 reflects compositional differences rather than anticipation effects. For small firms, which borrow predominantly in a single currency, dollar and total loans move almost identically before the policy. In contrast, for larger, multi-bank and multi-currency firms, total loans naturally follow a different trend from dollar loans. This divergence is further amplified by the sol's depreciation, which increased demand for local-currency credit and caused total loans to grow more rapidly. Importantly, the identification strategy does not rely on the levels or slopes of (normalized) dollar and total loans coinciding, but only on the absence of differential pre-trends across banks with different

[35] Section 7.3 discusses this identification threat in greater detail and provides corresponding robustness checks.

exposure to the regulation.

In the empirical analysis that follows, I confirm that pre-policy trends in dollar and total lending are statistically indistinguishable across banks with different levels of exposure once relevant observables and fixed effects are accounted for, supporting the assumption of no strategic anticipatory behavior.

Finally, my fifth identifying assumption is that shifts in firms' loan demand are uncorrelated with banks' exposure to the policy. In this setting, most *micro*, *small*, and many *medium*-sized firms maintain a single banking relationship (Table A.1). Consequently, absorbing firm-level demand variation with firm–time fixed effects would exclude almost all smaller firms and reduce the sample size by roughly 85 percent.[36] To address this concern, I include highly granular location–industry–month fixed effects, covering 1,008 districts (6-digit geographic classification) and 266 industries (5-digit industrial classification). These fixed effects capture localized and sector-specific demand shocks—such as regional economic fluctuations, or exchange-rate-driven shifts in input costs—that could influence credit demand. This approach follows Degryse et al. (2019), who show that such fixed effects provide a solid alternative to control for demand shocks in credit registries with mostly single-bank borrowers.

6 Results

Table 4 reports the estimated effects of the 2014 dedollarization policy on the growth rate of (i) *new dollar loans* (columns 1–2), (ii) *new total loans* (columns 3–4), and (iii) *new sol-denominated loans* (columns 5–6). For each dependent variable, the first column includes pre-policy bank controls, time, and bank–firm fixed effects, while the second column additionally incorporates industry–location–month and size–month fixed effects, following the main specification in Equation 2.

The estimates show that the negative effect of the policy on the growth rate of *new dollar loans* increases with banks' exposure to the regulation. For *micro firms* (the omitted category), the coefficient on the interaction between the policy shock and bank exposure is negative and statistically significant at the 5% level across both specifications. Quantitatively, a 10% increase

[36] Khwaja and Mian (2008) first introduced firm fixed effects to disentangle loan-supply from loan-demand factors in bank–firm datasets.

in bank exposure to the policy (approximately the change from the median to the 75th percentile of the exposure distribution) reduces the average growth rate of *new dollar loans* by about 10.8 percentage points in the year following the policy announcement (column 2).

For *small firms*, the triple interaction term between the policy shock, exposure, and firm size is negative but not individually significant. However, the joint effect—computed as the sum of the coefficients on *Exposure*×*Shock* and *Exposure*×*Shock*×*Small*—is negative and significant at the 1% level, implying an overall reduction in dollar credit of roughly 12.9 percentage points for a 10% increase in bank exposure. This suggests that the contraction in dollar lending was somewhat stronger for small firms than for micro firms, though both groups experienced substantial declines in credit supply.

For *medium firms*, the estimated triple-interaction coefficient is positive, implying an attenuated effect relative to micro firms. The implied overall reduction in credit for this group is approximately 3.2 percentage points, but it is not statistically significant at conventional levels (p-value = 0.11).

In contrast, for *large firms*, the triple interaction term is positive and statistically significant at the 5% level, indicating that the negative policy effect on dollar credit growth is substantially weaker for large firms. Indeed, the total effect of the policy for large firms—obtained as the sum of *Exposure*×*Shock* and *Exposure*×*Shock*×*Large*—is close to zero and statistically insignificant across both specifications.

Overall, these results indicate that, following the policy announcement, more exposed banks curtailed the supply of *new dollar loans* more sharply than less exposed banks, with the contraction concentrated among *micro*, *small*, and, to a lesser extent, *medium*-sized firms.

To assess whether firms facing a contraction in dollar credit supply substituted toward (relatively more expensive) sol-denominated loans, columns (3)–(4) examine the growth of *new total loans* (dollar + sol) using the same firm–month observations as in columns (1)–(2). The results remain qualitatively similar: for *micro* firms, an increase in bank exposure from the median to the 75th percentile is associated with a decline of about 10.8 percentage points in the growth rate of new total loans; for *small* firms, the corresponding reduction is 9.8 percentage points and statistically significant. As with dollar loans, the estimated effects for *medium* and *large* firms are small

and statistically insignificant.

Finally, columns (5)–(6) use as the dependent variable the growth rate of *new sol-denominated loans*, computed for the same firm–month observations in which new dollar loans are issued. Results confirm the absence of a significant increase in sol lending, indicating that the contraction in dollar credit was not offset by a simultaneous expansion in local-currency loans.

Table 4: Impact of the 2014 Dedollarization Policy on Credit Growth: Bank-Firm level analysis

	Δ log(New FX Loans)		Δ log(New Total Loans)		Δ log(New Sol Loans)	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × *Post*	-0.839**	-1.084**	-0.660*	-1.082**	-1.139	0.364
	(0.420)	(0.539)	(0.388)	(0.523)	(1.546)	(2.713)
× Small	-0.116	-0.211	-0.209	0.099	0.378	-2.992
	(0.510)	(0.689)	(0.474)	(0.647)	(1.986)	(3.161)
× Medium	0.569	0.758	0.653	1.022*	0.875	-1.642
	(0.434)	(0.561)	(0.400)	(0.541)	(1.609)	(2.754)
× Large	1.012**	1.216**	0.777*	0.961*	1.335	-0.936
	(0.479)	(0.602)	(0.431)	(0.577)	(1.627)	(2.814)
Joint test (p-val):						
Small:	0.002	0.005	0.003	0.019	0.585	0.179
Medium:	0.079	0.110	0.964	0.760	0.612	0.082
Large:	0.485	0.652	0.569	0.653	0.706	0.497
Controls × Month	Yes	Yes	Yes	Yes	Yes	Yes
Bank × firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	No	Yes	No	Yes	No
Size × Month FE	No	Yes	No	Yes	No	Yes
Industry × Location × Month FE	No	Yes	No	Yes	No	Yes
Observations	157,492	117,150	126,724	91,350	25,042	11,436
R^2	0.411	0.546	0.460	0.589	0.486	0.695

Notes: The dependent variable in columns (1)–(4) is the monthly growth rate of: new dollar loans (1)–(2), new total loans (3)–(4) and new sol loans (5)–(6). Dollar amounts are converted to soles at the January 2014 exchange rate to adjust for valuation effects. *Exposure* × *Post* captures the effect of the policy on Micro firms, which is the omitted category. The following rows capture the differential effect (relative to micro firms) of the policy for the rest of size segments. Joint test reports the p-value of the F-test that the overall effect of the policy on the rest of size segments, i.e. the sum of the coefficients of *Exposure* × *Post* and *Exposure* × *Post* × *Size* is equal to 0. The sample excludes trade-related loans and covers the period from 2014m1 to 2015m12 at a monthly frequency. Post starts in December 2014. Lower-order interactions terms corresponding to *Exposure* × *Post* × *Size* are included in all regressions. Coefficients of *Exposure* × *Size* are absorbed by the inclusion of bank-firm FE. Coefficients on *Post* × *Size* in columns (2), (4), and (6), are absorbed by the inclusion of size–month fixed effects. In columns (1), (3), and (5) they are not reported for brevity. Standard errors are clustered at the firm level and reported in parenthesis.*** p<0.01, ** p<0.05, * p<0.1.

Dynamics of treatment effect. Panels A.1–A.4 of Figure 8 show the estimated dynamic effect of bank exposure on the growth rate of new *dollar* and *total* loans by size category, using the following specification:

$$y_{f,b,t} = \sum_{\substack{\tau=-5 \\ \tau\neq-1}}^{6} \beta_\tau^z \big(Exposure_b \, 1[t=\tau]\big) + \sum_{s\neq z} \sum_{\substack{\tau=-5 \\ \tau\neq-1}}^{6} \delta_\tau^s \big(Exposure_b \, 1[t=\tau] \, Size_f^s\big) \tag{3}$$

$$+Controls_{b,t} + \alpha_{f,b} + \alpha_{s(f),t} + \alpha_{g(f),i(f),t} + \epsilon_{f,b,t}$$

The coefficients β_τ^z depict the dynamic response for the baseline size category z; for any other size $s \neq z$, the dynamic effect is $\beta_\tau^z + \delta_\tau^s$. Event time is measured in bimonthly bins, and coefficients are normalized to the last pre-policy bin ($\tau = -1$). Over the pre-policy period ($\tau \leq -2$), the estimates show no evidence of systematic differential trends for more exposed banks across sizes.[37]

Because the policy is not immediately binding upon announcement, its effects may not be fully realized until the deadline. I therefore report the cumulative response as the running sum of contemporaneous coefficients:

$$\hat{\gamma}_\tau^z = \begin{cases} \sum_{t=0}^{\tau} \hat{\beta}_t^z, & \tau \geq 0, \\ \sum_{t=\tau}^{-2} \hat{\beta}_t^z, & \tau < 0, \end{cases} \tag{4}$$

which is normalized to the last pre-policy bin ($\tau = -1$).

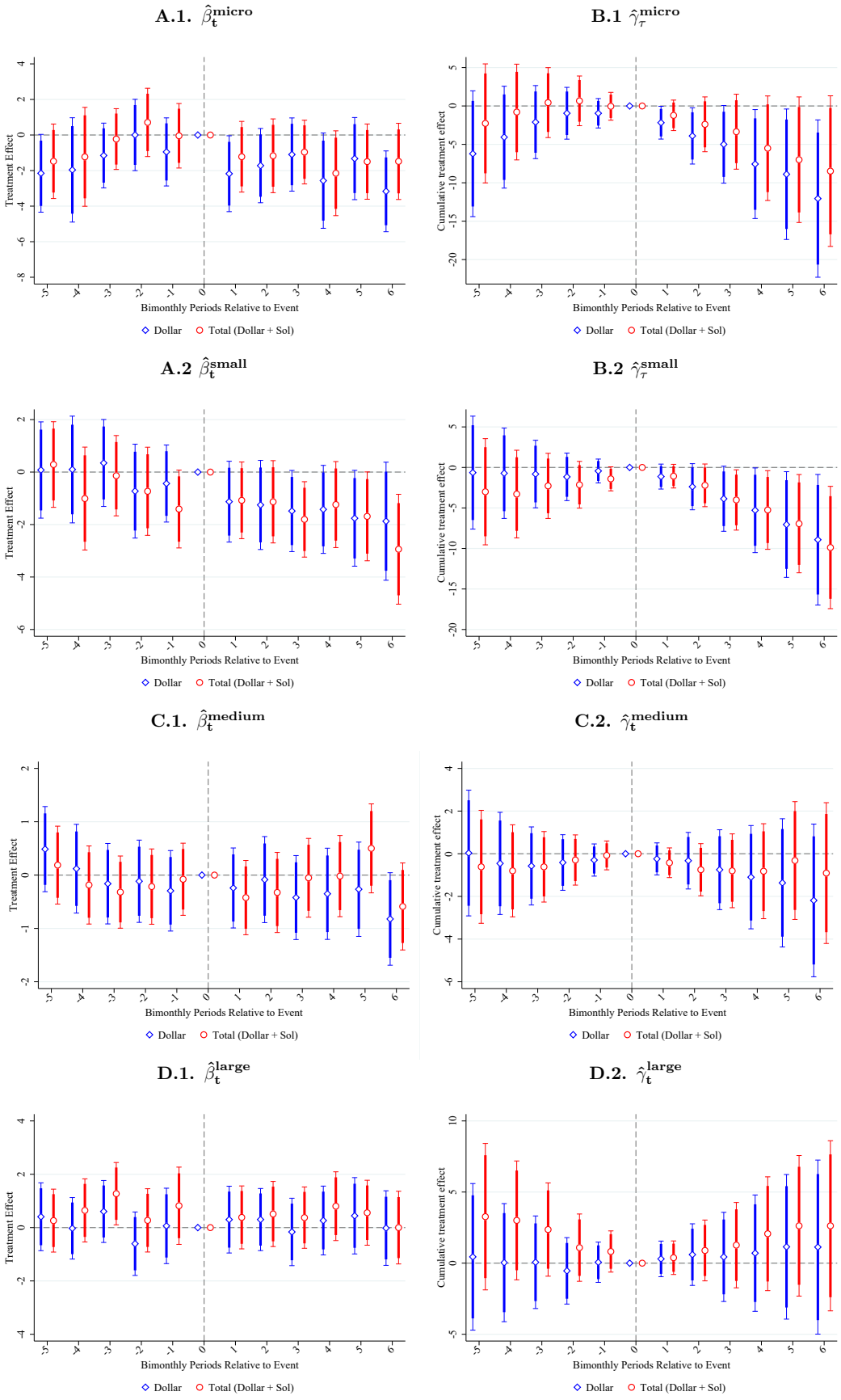
Over the pre-policy period, there is no evidence of pre-trends in the cumulative response: for all $\tau \leq -2$, the estimated coefficients $\hat{\gamma}_\tau^z$ are statistically indistinguishable from zero across all firm-size categories, with no sign of an already declining pattern in dollar-loan growth prior to the policy. Panel B of Figure 8 shows that, following the announcement, the cumulative path turns downward for *micro* and *small* firms and becomes statistically significant at short horizons, increasing in magnitude toward the deadline. For *medium* firms, the pattern is directionally similar but less precisely estimated, while for *large* firms the cumulative effects on both *dollar* and *total* loans remain statistically indistinguishable from zero throughout the estimation window, consistent with the insignificant average effects reported earlier.

6.1 Firm-Level Effects

After the policy implementation, firms could have mitigated its impact by reallocating borrowing from more exposed to less exposed banks. To assess whether such substitution occurred, I aggregate credit across all bank relationships at the firm level and estimate the effect of a firm's

[37] I use bimonthly bins to increase observations per event-time×size cell and improve precision. Monthly bins have fewer observations since they rely on firms issuing new loans in that exact month, which tends to yield noisier estimates—especially with industry×location×month fixed effects.

Figure 8: Testing Parallel Trends



Note: Figure 8 displays dynamic effects by size category. Panels A.1–A.4 plot the estimated coefficients β_t^z, while panels B.1–B.4 plot the cumulative coefficients γ_τ^z. In each panel, blue dots correspond to new *dollar* loans and red dots to new *total* (dollar + sol) loans. Thick `rspikes` indicate 90% confidence intervals; thin `rcaps` indicate 95% confidence intervals.

overall exposure to the policy on the growth rate of its new loans.

The main firm-level specification is given by:

$$y_{f,t} = \beta Exposure_f \times Post_t + \sum_{s=2}^{4} \delta^s Exposure_f \times Post_t \times Size_f^s \tag{5}$$

$$+ Controls_{f,t} + \alpha_f + \alpha_{s(f),t} + \alpha_{g(f),i(f),t} + \epsilon_{f,t},$$

where $y_{f,t}$ denotes the monthly growth rate of new dollar, total and sol loans, granted to firm f in month t. The variable $Exposure_f$ measures the weighted average exposure of the banks lending to firm f before the policy announcement, computed as:

$$Exposure_f = \sum_b \frac{L_{bf,2014}}{L_{f,2014}} \times Exposure_b, \tag{6}$$

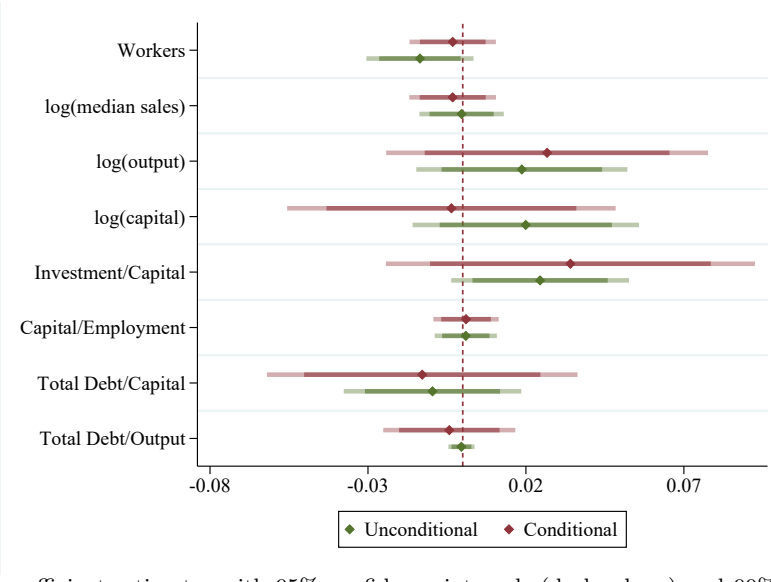
where $L_{bf,2014}$ is the outstanding dollar credit granted by bank b to firm f at the end of 2014, and $L_{f,2014}$ is the total dollar debt of firm f in that period.

The term $Controls_{f,t}$ includes interactions between month dummies and firm-level quintiles based on pre-policy characteristics. These quintiles are computed using weighted averages of the same pre-policy bank ratios used in Equation 2—capitalization, non-performing-loans, and loan-to-asset ratios—weighted by each bank's share of dollar credit to the firm. This procedure flexibly accounts for heterogeneity in firm-level exposure to banks with different balance-sheet structures. To control for firm unobservables I include firm fixed effects, α_f. To account for size-specific demand shocks, I include size–month fixed effects, $\alpha_{s(f),t}$, and to capture local and sectoral demand fluctuations, I include location–industry–month fixed effects, $\alpha_{g(f),i(f),t}$.

Importantly, as shown in Figure 9, after conditioning on industry–location and size fixed effects, the firm-level exposure measure is statistically orthogonal to key firm characteristics such as employment, sales, leverage, capital stock, and productivity-related ratios (investment-to-capital and capital-to-employment). This confirms that firms are ex ante comparable, ruling out endogenous sorting or structural differences as drivers of the observed effects.

Table 5 presents the firm-level estimates. Column (1) reports the effect of the policy on the

Figure 9: Firm Covariate Balance



This figure reports coefficient estimates with 95% confidence intervals (darker bars) and 99% confidence intervals (lighter bars) for the effect of a one–standard-deviation increase in firm exposure, as defined in equation (6), on a range of firm-level characteristics. All variables are standardized to have a mean of zero and a standard deviation of one. "Unconditional" estimates compare firms with different exposure levels without controlling for fixed effects, while "conditional" estimates include location–industry and size fixed effects. Covariates comprise the number of workers, the logarithm of median sales within each sales range, total production, capital stock, the investment-to-capital and capital-to-employment ratios, and leverage indicators such as debt-to-capital and total-debt-to-output ratios. Measures of capital, investment, and output are drawn from the 2014 Encuesta Económica Anual (EEA) and Encuesta Nacional de Empresas (ENE) conducted by the National Institute of Statistics and Informatics (INEI).

growth rate of new dollar loans in a specification with firm and month fixed effects, while column (2) adds size–month and industry–location–month fixed effects. Results indicate that for *micro firms*, a 3% increase in firm exposure to the policy—roughly equivalent to moving from the median to the 75th percentile of the firm-exposure distribution—reduces the average growth rate of new dollar loans by about 4.1 percentage points in the year following the policy (column 2). For *small firms*, the corresponding decline is larger—approximately 9.7 percentage points—and statistically significant. For *medium firms*, the estimated reduction in dollar credit growth is modest (around 1.4 percentage points) and statistically insignificant, while *large firms* appear unaffected.

These effects are smaller in magnitude than the corresponding bank–firm–level coefficients suggesting partial attenuation of the policy's impact through reallocation of dollar borrowing toward less exposed banks. For micro firms, however, the scope for reallocation is very limited. Because most maintain only one lending relationship, their firm-level exposure closely mirrors the exposure of their primary bank. As a result, a 10 percent increase in bank exposure maps almost proportionally into firm exposure, and the corresponding coefficients are of similar magnitude.

Columns (3)–(4) report results for the growth rate of *new total loans* (dollar plus sol). For *micro firms*, the estimated decline is about 5 percentage points (column 4), comparable in size—and slightly larger—than the effect on dollar loans, suggesting that tighter dollar credit was not offset by an expansion in sol lending from other banks. For *small firms*, the reduction in the growth rate of new total loans is roughly 8.1 percentage points, somewhat smaller than for dollar loans but still statistically significant, suggesting limited substitution toward local-currency credit. For *medium* and *large* firms, the estimated effects on total loans remain small and statistically insignificant.

Finally, columns (5)–(6) present results for the growth rate of *new sol loans*. The coefficients are not statistically significant, confirming that the contraction in dollar credit was not accompanied by a contemporaneous, significant expansion in local-currency lending.

Table 5: Impact of the 2014 Dedollarization Policy on Firm Credit Growth: Firm-level analysis

	Δ log(New FX Loans)		Δ log(New Total Loans)		Δ log(New Sol Loans)	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × *Post*	-1.005**	-1.356**	-0.783*	-1.681**	-2.183	-0.486
	(0.512)	(0.686)	(0.460)	(0.686)	(1.513)	(2.690)
× Small	-1.330**	-1.880**	-1.084**	-1.028	0.363	-1.036
	(0.567)	(0.813)	(0.526)	(0.817)	(1.782)	(3.017)
× Medium	0.482	0.904	0.483	1.127	1.943	1.535
	(0.521)	(0.691)	(0.465)	(0.694)	(1.550)	(2.744)
× Large	0.927	1.027	0.181	1.065	2.117	2.269
	(0.792)	(1.038)	(0.720)	(1.014)	(1.756)	(3.150)
Joint test (p-val):						
Small:	4.48e-09	1.67e-07	8.12e-07	1.38e-05	0.096	0.420
Medium:	0.032	0.210	0.213	0.175	0.631	0.350
Large:	0.901	0.689	0.310	0.449	0.943	0.271
Controls × Month	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	No	Yes	No	Yes	No
Size × Month FE	No	Yes	No	Yes	No	Yes
Industry × Location × Month FE	No	Yes	No	Yes	No	Yes
Observations	90,520	58,544	69,956	42,972	18,232	7,083
R^2	0.438	0.578	0.492	0.630	0.508	0.717

Notes: The dependent variable in columns (1)–(4) is the monthly growth rate of: new dollar loans (1)–(2), new total loans (3)–(4) and new sol loans (5)–(6) aggregated at the firm level. Dollar amounts are converted to soles at the January 2014 exchange rate to clean for valuation effects. *Exposure* × *Post* captures the effect of the policy on Micro firms, which is the omitted category. The following rows capture the differential effect (relative to micro firms) of the policy for the rest of size segments. Exposure is defined as in Equation 6. Joint test reports the p-value of the F-test that the overall effect of the policy on the rest of size segments, i.e. the sum of the coefficients of *Exposure* × *Post* and *Exposure* × *Post* × *Size* is equal to 0. The sample excludes trade-related loans and covers the period from 2014m1 to 2015m12 at a monthly frequency. Post starts in December 2014. Lower-order interactions terms corresponding to *Exposure* × *Post* × *Size* are included in all regressions. Coefficients of *Exposure* × *Size* are absorbed by the inclusion of firm FE. Coefficients on *Post* × *Size* in columns (2), (4), and (6), are absorbed by the inclusion of size–month fixed effects. In columns (1), (3), and (5) they are not reported for brevity. Standard errors are clustered at the firm level and reported in parenthesis.*** p<0.01, ** p<0.05, * p<0.1.

Reallocation across bank relationships. To assess directly whether multiple banking relationships help attenuate the impact of the regulation, I interact a firm's pre-policy number of bank relationships with the *Exposure* × *Post* term. The estimated specification is:

$$y_{f,t} = \beta Exposure_f \times Post_t \times NrBankRelations_f + \gamma Post_t \times NrBankRelations_f \qquad (7)$$

$$+Controls_{f,t} + \alpha_f + \alpha_{s(f),t} + \alpha_{g(f),i(f),t} + \epsilon_{f,t},$$

Table 6 shows that the estimated coefficient on the triple interaction, $\hat{\beta}$, is positive and statistically significant when the dependent variable is the growth rate of new dollar loans (column 1) or total loans (column 2). This implies that each additional bank relationship reduces significantly the negative effect of the policy on dollar credit growth. Specifically, a three-percentage-point increase in firm exposure—equivalent to moving from the median to the 75th percentile of the exposure distribution—reduces dollar credit growth by roughly 5.7 percentage points for a single-bank firm. The same shock would lead to a contraction of only about 4.8 percentage points for a firm with two bank relationships. This indicates that more diversified firms were better able to reallocate borrowing toward less-exposed lenders, mitigating the policy's impact on credit supply. In contrast, when the dependent variable is the growth rate of new sol loans (column 3), the interaction is not statistically significant, suggesting that at the intensive margin, reallocation across banks occurred primarily within dollar-denominated credit rather than through substitution into sol loans.

Overall, these results indicate that pre-existing multi-bank relationships served as an effective buffer against the policy shock, primarily for firms most exposed to tighter credit conditions.

6.2 Extensive-Margin Effects

To complement the intensive-margin analysis, I examine whether the 2014 policy affected firms' likelihood of issuing new loans. I estimate the following extensive margin specification:

$$NewLoan^c_{f,t} = \beta Exposure_f \times Post_t + \sum_{s=2}^{4} \delta^s Exposure_f \times Post_t \times Size^s_f \qquad (8)$$

$$+\alpha_f + \alpha_{s(f),t} + \alpha_{g(f),i(f),t} + \epsilon_{f,t},$$

Table 6: Impact of the 2014 Dedollarization Policy on Credit Growth: Firm-level analysis with heterogeneity by number of bank relationships

	Δ log(New FX Loans)	Δ log(New Total Loans)	Δ log(New Sol Loans)
	(1)	(2)	(3)
Exposure × *Post*	-1.907***	-2.018***	-0.706
	(0.589)	(0.599)	(1.592)
Exposure × *Nr. of bank relations* × *Post*	0.290**	0.332**	0.445
	(0.147)	(0.143)	(0.346)
Controls × Month	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes
Observations	58,544	42,972	7,083
R^2	0.578	0.630	0.717

Notes: The dependent variable in column (1) is the monthly growth rate of new dollar loans, in column (2) the growth rate of new total loans, and in column (3) the growth rate of new sol loans, all aggregated at the firm level. *Exposure* × *Post* captures the effect of the 2014 dedollarization policy, where *Exposure* is defined as in Equation 6. "Nr. of bank relations" is the pre-treatment measure of a firm's bank access, defined as the *maximum* number of banks with which the firm maintained lending relationships in 2014 (the year prior to the policy announcement). The triple interaction term, *Exposure* × *Post* × *Nr. of bank relations*, captures heterogeneity in the policy effect by pre-policy bank access. Lower-order interactions corresponding to this triple interaction term are all included in the regressions. *Exposure* × *Nr. of bank relations* is absorbed by firm fixed effects and the estimated coefficient for *Nr. of bank relations* × *Post* is not reported for brevity. The sample excludes trade-related loans and covers the period from 2014m1 to 2015m12; *Post* begins in December 2014. Standard errors are clustered at the firm level and reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

where, $NewLoan^c_{f,t}$, is an indicator equal to one if firm f receives at least one new loan in currency c, from any bank in month t, and zero otherwise.

The sample includes all firms with any positive borrowing prior to the policy announcement, excluding post-policy entrants. Table 7 reports the estimates for the probability of obtaining a new loan denominated in dollars (columns 1–2), denominated in soles (columns 3–4), and for any new loan regardless of currency (columns 5–6).

Results show that the probability of issuing new dollar loans declines significantly for small, medium, and large firms, but remains unchanged for micro firms. A three–percentage-point increase in firm exposure—equivalent to moving from the median to the 75th percentile of the exposure distribution—reduces the probability of issuing new dollar loans by roughly 0.17 percentage points for small firms, 0.30 percentage points for medium firms, and 0.19 percentage points for large firms in the preferred specification (column 2). In contrast, the probability for micro firms remains close to zero and statistically insignificant. This pattern suggests that micro firms—while heavily affected in loan volumes at the intensive margin—did not exit the credit market altogether. Instead, they continued borrowing, though at substantially reduced scales. Likely related to soles loans relatively

higher cost binding enough to limit reallocation incentives.

Conversely, the probability of issuing new sol loans for small, medium and large firms increases significantly with firm size, indicating that larger firms were able to reallocate part of their borrowing toward local-currency credit, consistent with greater financial flexibility and lower switching costs among larger borrowers (columns 3–4).

In net terms, the probability of obtaining any new loan (in either currency) rises significantly for small and even more for medium firms in the preferred specification (column 6), suggesting that this last group most effectively offset the contraction in dollar borrowing through new sol issuances.

These heterogeneous responses are consistent with a hierarchy of financial constraints that are more binding for smaller firms. Micro firms, facing the tightest constraints, absorbed the negative supply shock through a contraction in dollar credit volumes, with no scope for substitution toward more expensive sol loans or adjustment at the extensive margin. High fixed costs of switching currencies or limited credit histories in soles likely prevented meaningful reallocation, leading these firms to continue borrowing but at substantially smaller scales. Small firms, while still constrained, displayed somewhat greater flexibility. They partially reallocated dollar borrowing toward less-exposed banks. And, along the extensive margin, increasing the issuance of new local-currency loans, attenuating—but not offsetting—the overall decline in total lending volumes. Medium firms, which were less constrained, show only mild evidence of contraction in dollar lending and no significant change in overall credit volumes, suggesting some adjustment through existing multi-bank relationships and primarily along the extensive margin by substituting new dollar loans with sol loans, thereby maintaining total borrowing. Finally, large firms, which face virtually no binding financial constraints, appear to have adjusted smoothly to the policy's intended compositional shift from dollar to local-currency credit, without any friction or change in total credit volumes.

6.3 Effect on interest rates

To further assess the mechanisms underlying these patterns, I examine the behavior of lending rates across currencies and firm sizes. If the policy primarily tightened financing conditions through supply-side channels, more constrained borrowers should have faced higher borrowing costs, whereas

Table 7: Impact of the 2014 Dedollarization Policy on the Extensive Margin: Firm-level analysis

	Pr(New FX Loans)		Pr(New Sol Loans)		Pr(New Total Loans)	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × *Post*	0.008	0.003	0.001	0.003	0.007	0.005
	(0.006)	(0.006)	(0.008)	(0.009)	(0.009)	(0.010)
× Small	-0.059***	-0.056***	0.054***	0.061***	0.005	0.014
	(0.006)	(0.007)	(0.009)	(0.011)	(0.010)	(0.012)
× Medium	-0.100***	-0.095***	0.092***	0.099***	0.018	0.029**
	(0.007)	(0.008)	(0.012)	(0.013)	(0.013)	(0.014)
× Large	-0.053*	-0.060**	0.128***	0.122***	0.082*	0.068
	(0.028)	(0.029)	(0.043)	(0.046)	(0.045)	(0.049)
Joint test (p-val):						
Small:	0.000	0.000	0.000	0.000	0.050	0.004
Medium:	0.000	0.000	0.000	0.000	0.007	0.001
Large:	0.095	0.041	0.002	0.005	0.047	0.129
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	No	Yes	No	Yes	No
Size × Month FE	No	Yes	No	Yes	No	Yes
Industry × Location × Month FE	No	Yes	No	Yes	No	Yes
Observations	1,915,831	1,762,867	1,915,831	1,762,867	1,915,831	1,762,867
R^2	0.267	0.331	0.228	0.306	0.249	0.324

Notes: The dependent variable in columns (1)–(3) is a binary indicator equal to one if a firm issues a new loan in a given month and zero otherwise. Columns (1), (2), and (3) report results for the probability of issuing new dollar, sol, and total (dollar plus sol) loans, respectively. The sample includes all firms with any positive borrowing prior to the policy announcement, excluding post-policy entrants. *Exposure* × *Post* captures the effect of the 2014 dedollarization policy on *micro* firms, which is the omitted size category, while the following rows capture the differential effects for other size groups. Exposure is defined as in Equation 6. Joint test reports the p-value of the F-test that the overall effect of the policy on the rest of size segments, i.e. the sum of the coefficients of *Exposure* × *Post* and *Exposure* × *Post* × *Size* is equal to 0. The sample excludes trade-related loans and covers 2014m1-2015m12. *Post* begins in December 2014. Lower-order interactions terms corresponding to *Exposure* × *Post* × *Size* are included in all regressions. Coefficients of *Exposure* × *Size* are absorbed by the inclusion of firm FE. Coefficients on *Post* × *Size* in columns (2), (4), and (6), are absorbed by the inclusion of size–month fixed effects. In columns (1), (3), and (5) they are not reported for brevity. Standard errors are clustered at the firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

unconstrained firms—able to reallocate borrowing across banks or currencies—should exhibit stable interest rates despite shifts in loan composition. To test this hypothesis, I use publicly available bank-level average interest rate data by size segment and estimate a specification analogous to Equation 2, but at the bank–size–month level and with a less demanding fixed-effects structure given the smaller number of observations. Specifically, I estimate:

$$i^c_{b,s,t} = \beta Exposure_b \times Post_t + \sum_{s=2}^{4} \delta_1^s Exposure_b \times Post_t \times Size^s + \sum_{s=1}^{4} \delta_2^s Exposure_b \times Size^s \quad (9)$$

$$+ \sum_{s=1}^{4} \delta_3^s Post_t \times Size^s + Controls_{b,t} + \alpha_b + \alpha_s + \alpha_t + \epsilon_{b,s,t}$$

where $i^c_{b,s,t}$ denotes the average interest rate on loans granted by bank b to firms in size segment

s, in currency c, and at month t. Table 8 reports the estimated effects of the policy, $\hat{\beta}$ and $\hat{\delta}_1^s$, on average lending rates in dollars (column 1), soles (column 2), and the interest rate differential between the two currencies (column 3). Results show that more exposed banks increased interest rates on *dollar loans* by about 1.6 percentage points for micro firms following the policy, while rates for larger firms remained unchanged. This pattern implies that funding cost pressures were primarily transmitted to the smallest and most credit-constrained borrowers, who lacked alternative financing sources. By contrast, there is no significant change in rates on sol loans across any size group, and the estimated interest rate differential between sol and dollar loans narrowed only for micro firms, reflecting the relative increase in their dollar borrowing costs.

Taken together, these results confirm that the policy's impact on lending rates was highly heterogeneous across firm sizes. For micro firms, both dollar borrowing volumes declined and lending rates rose sharply, indicating that these borrowers faced tighter and more costly credit conditions—a pattern consistent with a contraction in credit supply combined with limited scope to reallocate borrowing toward other lenders or currencies.

In contrast, small firms experienced a significant contraction in dollar credit without an accompanying increase in interest rates. This pattern suggests that shifts in dollar loan demand may have dampened potential price effects. In particular, small firms partially reallocated their dollar borrowing from more exposed banks toward sol-denominated loans, likely mitigating upward pressure on lending rates.

Similarly, for medium and large firms, the absence of price effects likely reflects offsetting supply and demand adjustments: banks reallocated funding toward local-currency lending in response to the policy, while financially stronger firms reduced their demand for dollar loans. Importantly, these demand shifts are not pre-existing confounders but part of the adjustment margin triggered by the policy itself—firms responding to tighter dollar credit supply by altering their borrowing composition. As a result, equilibrium interest rates remained broadly unchanged even as the composition of borrowing shifted toward soles. Overall, these patterns suggest that the policy tightened financing conditions primarily for the most constrained firms, while larger and better-connected borrowers adjusted smoothly through portfolio reallocation across banks and currencies.[38]

[38] Though suggestive and relying on aggregate data, these findings align with evidence from a related study using

Table 8: Impact of the 2014 Dedollarization Policy on Bank-Level Average Interest Rates by Firm Size Segment

	Interest rate (FX, %)	Interest rate (Sol, %)	IR differential (Sol–FX, p.p.)
	(1)	(2)	(3)
Exposure × *Post*	1.552***	0.093	-1.454***
	(0.333)	(0.382)	(0.391)
× Small	-1.573***	-0.104	1.471***
	(0.338)	(0.387)	(0.397)
× Medium	-1.570***	-0.199	1.364***
	(0.336)	(0.385)	(0.395)
× Large	-1.584***	-0.144	1.432***
	(0.337)	(0.386)	(0.396)
Joint test (p-val):			
Small:	0.722	0.876	0.807
Medium:	0.689	0.046	0.099
Large:	0.504	0.356	0.703
Controls × Month	Yes	Yes	Yes
Size FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Observations	611	609	609
R^2	0.906	0.935	0.751

Notes: The dependent variables are bank-size-level average lending interest rates in: dollars (column 1), soles (column 2), and the interest rate differential (soles minus dollars, column 3). *Exposure* is the bank ratio of dollar funding to total assets in december 2014. *Exposure* × *Post* measures the effect of the 2014 dedollarization policy on bank interest rates for the omitted size category (micro). Subsequent rows report heterogeneous effects by size segment. Lower-order interactions terms corresponding to *Exposure* × *Post* × *Size* are included in all regressions. Coefficients of *Exposure* × *Size* and *Post* × *Size* are not reported for brevity. Sample period: 2014m1–2015m12. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

6.4 Real Effects

To examine whether the credit supply shock associated with the 2014 dedollarization policy had real effects, I analyze firm-level employment growth between 2014 and 2015. Specifically, I estimate the following cross-section specification:

$$\Delta logWorkers_f = \beta Exposure_f + \sum_{s=2}^{4} \delta^s Exposure_f \times Size_f^s \tag{10}$$

$$+\alpha_{s(f)} + \alpha_{g(f),i(f)} + \alpha_{ageterciles(f)} + \alpha_{salesterciles(f)} + \epsilon_{f,t},$$

where the dependent variable, $logWorkers_f$, is the annual change in the log number of workers employed by firm f between 2014 and 2015. This specification includes fixed effects for firm size, industry-location, age and sales terciles.

granular loan-level data for Chile, which documents similarly heterogeneous interest rate responses to macro-financial shocks across firm sizes Acosta-Henao et al. (2025).

Table 9 presents cross-section estimates relating firms' exposure to the policy to the annual change in their log number of workers over this period. The variable *Exposure* is defined as in equation (6), capturing firms' weighted average exposure to banks affected by the regulation and taking a value of zero for firms without any pre-policy dollar borrowing. The *Exposure dummy*, instead, equals one for firms that held any dollar-denominated debt before the policy and zero otherwise.

Results show that more exposed firms experienced significantly lower employment growth in the year following the policy. Quantitatively, a three-percentage-point increase in exposure—equivalent to moving from the median to the 75th percentile of the exposure distribution—is associated with a decline in employment growth of about 0.3 percentage points for micro firms and 0.26 percentage points for small firms. In column (2), using the binary exposure measure, micro firms that borrowed in dollars before the policy grew roughly 0.1 percentage points less in employment than firms without prior dollar borrowing; for small firms, the corresponding difference is about 0.08 percentage points. For medium and large firms, estimated effects are statistically insignificant.

These findings suggest that the policy's real effects were concentrated among the most financially constrained firms, which faced tighter credit conditions and limited substitution possibilities across lenders or currencies. In contrast, larger and better-diversified firms appear to have absorbed the policy shock without meaningful employment adjustments, implying that the policy achieved its de-dollarization goal without broad labor-market costs.

Because universe-level data on firm investment are unavailable, I rely on survey-based information from 2014 to perform a simple back-of-the-envelope calculation, detailed in Appendix 8. Using these data and mapping the estimated credit contraction into investment changes under alternative values of θ—the share of investment typically financed through bank credit, taken from World Bank enterprise surveys—the exercise provides suggestive evidence that the policy may have led to a moderate decline in investment among smaller firms. Under a conservative assumption that 20% of investment is financed through bank credit, investment for micro firms is estimated to have fallen by roughly 15%, and by about 9% for small firms.

Table 9: Impact of the 2014 Dedollarization Policy on Annual Employment Growth (2014–2015)

	$\Delta log(workers)$		
	(1)		(2)
Exposure	-0.113*	*Exposure dummy*	-0.034*
	(0.066)		(0.021)
× Small	0.027	× Small	0.007
	(0.071)		(0.022)
× Medium	0.061	× Medium	0.020
	(0.078)		(0.024)
× Large	-0.019	× Large	-0.022
	(0.156)		(0.024)
Joint test (p-val):			
Small	0.002		0.001
Medium	0.215		0.282
Large	0.341		0.208
Size FE	Yes		Yes
Industry × Location FE	Yes		Yes
Age terciles FE	Yes		Yes
Sales terciles FE	Yes		Yes
Observations	42,560		42,560
R^2	0.134		0.134

Notes: The dependent variable is the annual growth rate of firm's employment (number of workers) between 2014 and 2015. *Exposure* is defined as in equation (6). *Exposure dummy* is a binary variable that takes the value of 0 if the firm did not borrow in dollars in pre-policy period and 1 if it did. Standard errors are clustered at the industry–location level. *** p<0.01, ** p<0.05, * p<0.1.

6.5 Spillovers on the tradable sector

Although trade-related loans were formally excluded from the 2014 credit dedollarization targets, several channels could have transmitted the policy's effects to firms engaged in international trade. First, the simultaneous increase in reserve requirements on foreign-currency liabilities tightened dollar funding conditions across the banking system, potentially affecting credit supply even in exempt segments. Second, banks could have reclassified some exposures as "trade-related" to circumvent the credit target, temporarily redirecting FX lending toward exporters. Finally, tighter credit conditions for nontradable firms may have propagated indirectly to tradable sectors through input–output or client–supplier linkages. This section examines whether, on net, these mechanisms produced measurable effects on credit supply to tradable firms.

Table 10 shows that trade-related lending is highly concentrated among medium and large firms. Together, these two groups represent more than 85% of all firms that issued at least one trade-related loan (64.25% medium and 21.98% large) and account for almost the entirety of trade-

Table 10: Trade-Related Lending by Firm Size

	Firms with trade-related loans		Credit dollarization of firms with trade-related loans (%)	
	% of all trade-related firms	% of total trade-related loan volume	2014	2015
Micro	1.20	0.05	92.94	71.77
Small	12.58	0.34	82.10	62.54
Medium	64.25	18.00	81.77	65.01
Large	21.98	81.61	76.18	54.87

Notes: The table summarizes the composition and dollarization of trade-related lending across firm sizes. Column (1) reports the share of each firm-size segment among all firms that issued at least one trade-related loan, using December 2014 data (shares sum to 100%). Column (2) shows the corresponding share of total trade-related loan volume in December 2014. Columns (3)–(4) report the share of dollar-denominated credit in these firms' total loans before (as of Dec. 2014) and after (as of Dec. 2015) the policy announcement. All values are expressed as percentages of totals within the population of trade-related firms.

related loan volume (approximately 99.6%). By contrast, micro and small firms together constitute only about 14% of trade-oriented firms and less than 0.4% of trade-related credit volume.

Columns (3)–(4) show that, despite being exempt from the credit dedollarization targets, trade-oriented firms experienced a sizable decline in credit dollarization between 2014 and 2015—on the order of 15–20 percentage points. To determine whether this decline reflects changes in new loan origination or simply the amortization of existing FX debt, I replicate the firm-level analyses from Sections 6.1 and 6.2 using firms with trade-related loans.

Table A.3 shows that this reduction did not translate into a significant contraction in new dollar lending or a corresponding expansion in sol lending at the intensive margin (Panel A). Unlike nontradable medium and large firms—which adjusted the currency composition of new loans despite stable total credit volumes—tradable firms display no significant response at the extensive margin either (Panel B).

Overall, estimated coefficients have the same signs as those for nontradable firms but are smaller and not statistically significant, suggesting no spillover effects. The most consistent interpretation is that the contemporaneous (unconditional) increase in marginal reserve requirements on foreign-currency liabilities raised dollar funding costs systemwide, prompting banks to gradually rebalance their portfolios away from FX exposures even in exempt segments. Since trade loans remained outside the calculation of the credit dedollarization target, banks had incentives to maintain credit flows to exporters while adjusting the currency composition of their broader balance sheets, for example, by allowing existing dollar loans to amortize, shortening maturities on dollar credit, or

expanding sol lending to post-treatment entrants.

7 Robustness checks[39]

7.1 Accounting for previous adjustments in FX reserve requirements

A potential concern is that the estimated effects of the December 2014 dedollarization policy might partially capture the influence of earlier regulatory changes, such as the reserve requirement increases implemented in March 2013 or the September 2013 first phase of the dedollarization program. To address this, I conduct a placebo exercise in which I re-estimate the main specification using the same continuous bank-level exposure measure but over an earlier sample (January 2013–December 2014). This allows testing whether those 2013 policy changes had differential effects correlated with banks' exposure to the 2014 policy. The results show no significant effects on either new dollar lending or total credit growth, indicating that earlier measures did not generate systematic average or heterogeneous lending responses correlated with exposure to the 2014 policy or firm size (see Table A.4).

To further ensure that the main findings are not contaminated by earlier policy actions, I also re-estimate the main specification using progressively shorter pre-policy windows—nine, six, and three months before December 2014. If the results were driven by lingering effects of previous measures, the estimated coefficients should weaken as the pre-treatment window narrows. As shown in Figure 10, the coefficients on the interaction term *Exposure* × *Post* remain broadly stable across specifications and firm-size groups, with micro firm estimates becoming slightly more pronounced in shorter windows.[40] These results confirm that the estimated effects are not driven by earlier regulatory interventions but instead reflect the tightening episode associated with the December 2014 dedollarization policy.

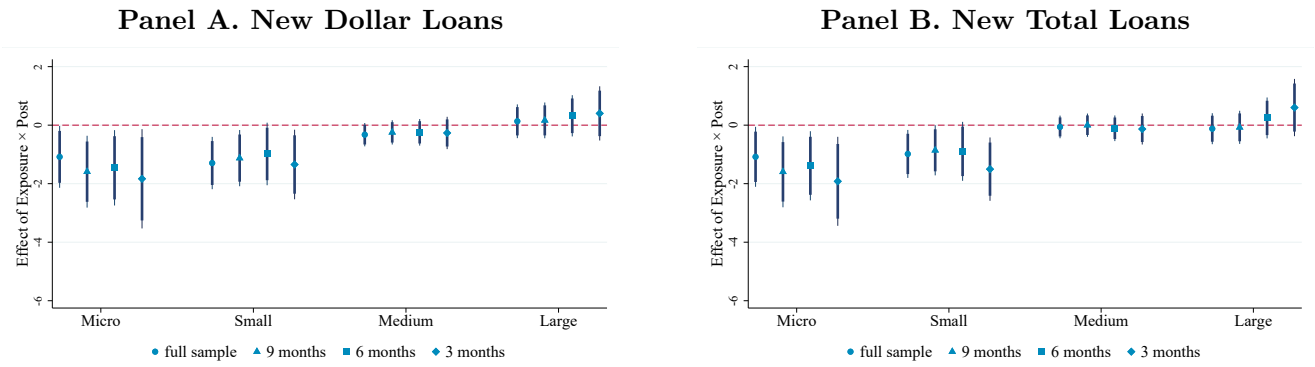
7.2 Accounting for additional post-treatment developments

U.S. Monetary Policy. At its December 2015 meeting, the Federal Open Market Committee (FOMC) raised the target range for the federal funds rate by 25 basis points—the first increase

[39] Additional robustness checks—including alternative exposure measures, size indicators, and sample definitions—are included in Appendix B.

[40] See also Table A.5 in the Appendix.

Figure 10: Impact of the 2014 Dedollarization Policy on Credit Growth: Robustness to Alternative Pre-Treatment Windows



Notes: Figure 10 plots coefficient estimates of the interaction term *Exposure* × *Post* with 90% (thick bars) and 95% (thin bars) confidence intervals, using the preferred specification in Equation 2. Each set of estimates corresponds to regressions with different pre-treatment windows, starting in January, April, July, and October 2014. Results are shown separately for different firm-size groups when the dependent variable is the growth rate of new dollar loans (Panel A) and new total loans (Panel B).

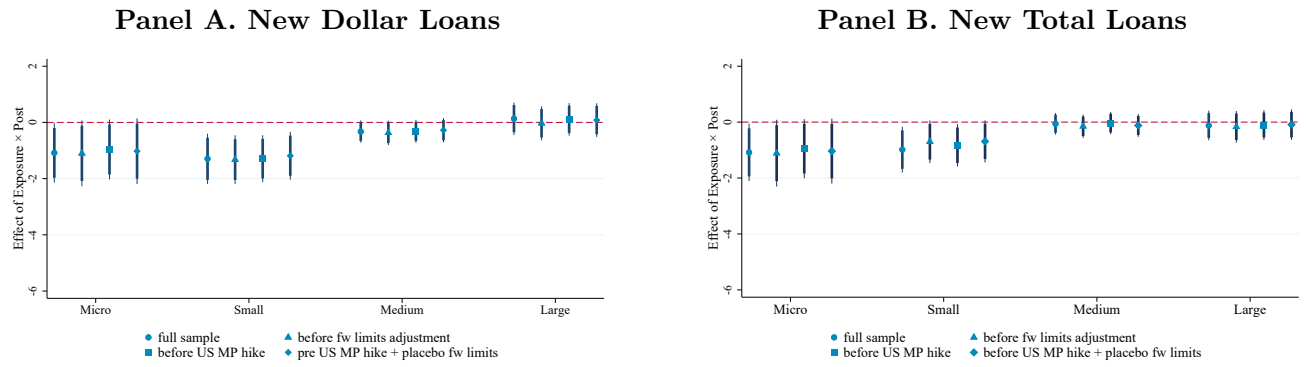
since June 2006—bringing the effective rate from 0.12 to 0.24 percent. This monetary tightening coincided with the policy deadline to reduce the stock of dollar credit to at least 90 percent of the September 2013 threshold. As a result, the last month of the sample may concentrate part of the policy's lending adjustment, but also a higher cost of non-core dollar funding resulting from the U.S. monetary policy contraction. To address this, I re-estimate the main specification truncating the post-treatment period at November 2015, one month before the U.S. rate hike. The results remain qualitatively unchanged, with estimated coefficients on micro firms about 88 percent of the estimated magnitude when using the full sample (see Figure 11 and columns 3 and 7 in Table A.6). This indicates that, even under a conservative scenario, the December 2015 U.S. policy rate increase accounts for only a small fraction of the total estimated effect on the smallest size segment, confirming that the observed changes in credit are primarily driven by the domestic dedollarization policy rather than by external monetary conditions.

Adjustment of Forward Limits. On August 26, 2015, the financial regulator also modified the limits on banks' net forward positions, a change that could have influenced the incentives for foreign-currency intermediation (Keller, 2019). To ensure that my estimates are not driven by this adjustment, I first restrict the post-treatment window to eight months after the policy announcement, isolating the average effect before the forward-limit modification. As shown in Figure 11 and columns (2) and (5) of Table A.6, the estimated coefficients remain stable in magnitude relative to

the full-sample estimates.

Additionally, I include a placebo triple interaction between bank exposure and a dummy that equals one after August 2015, in the sample truncated before December 2015 (to also net out potential U.S. policy effects). The placebo term is statistically insignificant across all firm-size groups, and the main treatment coefficients remain virtually unchanged. Overall, these results confirm that neither the adjustment in forward limits nor the U.S. monetary policy normalization materially affected the estimated impact of the December 2014 dedollarization policy.

Figure 11: Impact of the 2014 Dedollarization Policy on Credit Growth: Robustness to Alternative Post-Treatment Windows



Notes: Figure 11 plots coefficient estimates of the interaction term *Exposure* × *Post* with 90% (thick bars) and 95% (thin bars) confidence intervals, using the preferred specification in Equation 2. Each set of estimates corresponds to regressions with alternative post-treatment windows: (i) truncated one month before the August 2015 adjustment of forward limits, (ii) truncated one month before the December 2015 increase in the U.S. policy rate, and (iii) including a placebo term for August 2015. Results are shown separately by firm-size group when the dependent variable is the growth rate of new dollar loans (Panel A) and new total loans (Panel B).

7.3 Depreciatory Context

This policy was implemented in a period of a depreciatory trend of the sol and most currencies in emerging economies. This period started after the US Taper Tantrum announcement in May 2013,[41] and ended in December 2015 with the policy liftoff.

By ruling out pre-trends, I am showing that there was no significantly different trend of the loans granted *by differently exposed banks* a year before the policy was announced and after the market was already expecting a depreciation of the sol (see Figure A.8).

[41] This was the first time Federal Reserve officials mentioned a possible curtailment of its large-scale asset purchase program. Market participants updated their expectations on when the Federal Reserve starts increasing its policy rate after keeping it at near zero levels in December 2008 as a response to the GFC.

An additional concern is that small firms borrowing in dollars may cope worse with a depreciation than large firms. Since changes in firms' outcomes evolve gradually, it is possible that, by the time of the policy announcement, small firms had already accumulated larger losses. If so, banks may have reduced dollar lending to small borrowers more sharply than to large ones, independently of the regulation. If this alternative mechanism were driving the empirical findings, then prior to the policy the loan performance of small firms—relative to large ones—should already be worse in banks that were more exposed to the regulation. I test this hypothesis by estimating the following specification:

$$NPL_{f,b,t} = \beta Exposure_b + \sum_{s=2}^{4} \delta^s Exposure_b \times Size_f^s \tag{11}$$
$$+ Controls_{b,t} + \alpha_f + \alpha_{s(f),t} + \alpha_{g(f),i(f),t} + \epsilon_{f,b,t}$$

where $NPL_{f,b,t}$ is the share of non-performing dollar loans firm f has with bank b in month t during 2014. The coefficients β and δ^s jointly capture the correlation between bank exposure, firm size, and firms' non-performing loan ratios in the year prior to the policy, controlling for relevant covariates and fixed effects. Table A.7 shows that, across all size groups, firms' NPL ratios are not statistically related to bank exposure. Thus, even if small and large firms differed in performance before the announcement, these differences are not correlated with banks' exposure to the policy. This evidence rules out pre-existing differential borrower risk as an alternative explanation for the heterogeneous effects documented in the paper.

7.4 Non-bank financial institutions

Banks dominate credit intermediation in Peru, accounting for at least 90 percent of dollar loan volumes across size segments (see Table A.8). Moreover, the vast majority of firms that borrow from banks do not maintain relationships with any non-bank financial institution (NBFI), a pattern that holds across all size categories (see Table A.9).

NBFIs also differ systematically from banks. They tend to operate in narrow geographic markets, specialize in specific products such as factoring or leasing, and rely on more expensive funding sources. As a result, they charge higher interest rates and have very limited capacity to supply dollar-denominated loans. Given these structural differences, NBFIs are neither close

substitutes for banks nor meaningful alternative lenders for firms relying on bank credit before the policy. It is therefore unlikely that bank-dependent firms—especially smaller ones—could have offset the contraction in dollar lending from exposed banks by shifting to NBFIs.

To assess whether firms substituted toward less-exposed nonbank financial institutions (NBFIs), I aggregate all NBFI credit at the firm level and estimate the same specification in Equation 5 using the growth rate of new NBFI loans as the dependent variable.[42] Specifically, I estimate:

$$NBFI_{f,t} = \beta Exposure_f \times Post_t + \sum_{s=2}^{4} \delta^s Exposure_f \times Post_t \times Size_f^s \tag{12}$$

$$+Controls_{f,t} + \alpha_f + \alpha_{s(f),t} + \alpha_{g(f),i(f),t} + \epsilon_{f,t},$$

where, $NBFI_{f,t}$ denotes the growth rate of new loans (dollar, sol or total) issued by nonbank financial institutions to firm f in month t, and is set to zero in months in which no new loan is issued. Results in Table A.10 show no significant effect of the policy on NBFI credit growth, indicating that nonbank lenders did not materially offset the reduction in bank credit following the policy. The only exception is for small firms, which exhibit a modest but statistically significant increase in the growth rate of new dollar loans. Quantitatively, given that NBFI dollar lending represented about 6 percent of total new dollar loans issued by small firms before the policy, this positive response offsets at most one-fifth of the estimated 8.1 percentage-point decline in bank credit (see Table 5).[43] Overall, substitution toward NBFIs appears limited, suggesting that the tightening in bank dollar lending was only partially absorbed by nonbank intermediaries.

7.5 Type of loans

A key distinguishing feature of commercial lending is the type of collateral that underlies each loan, which is closely linked to its risk profile and recovery value (Ivashina et al., 2022). Since the composition of collateralized versus uncollateralized credit differs systematically across

[42] Since only a small fraction of firms borrow from NBFIs, the purely intensive-margin specification yields few observations and limited precision. To retain information on both issuance and non-issuance periods, I recode missing values of new NBFI loan growth as zero, treating months with no new NBFI credit as zero growth. This definition of the dependent variable captures potential substitution to NBFI loans both at the intensive and extensive margins.

[43] At end-2014, banks accounted for about 94 percent of new dollar lending to small firms, with NBFIs supplying the remaining 6 percent. Although average NBFI loans are similar in amount ($\approx 93,000$ soles vs. $\approx 84,000$ soles), their infrequent issuance limits their aggregate impact.

firm sizes, comparing aggregate lending responses could confound policy effects with compositional changes in loan types. To address this, I re-estimate the main bank-firm specification separately for asset-backed and non–asset-backed loans—the latter including cash-flow loans (revolving lines and fixed-term loans) as well as leasing contracts.[44]

Results in Table A.12 show that the negative effects of the policy on dollar loans are concentrated in cash-flow loans, whereas asset-backed and leasing credit remain broadly unaffected.[45] For micro firms, higher bank exposure is associated with a pronounced reduction in new cash-flow lending, while small firms show a modest increase in leasing loans.

The stronger effects for cash-flow loans can be explained by the nature and quality of collateral. Asset-backed loans are secured by physical assets such as machinery or property with verifiable resale values, making them easier to recover and less sensitive to funding shocks. By contrast, cash-flow loans often rely on "soft" collateral such as receivables, inventory, or personal guarantees, which are harder to liquidate and more procyclical in value. For small firms in particular, these forms of collateral are insufficient to maintain access to dollar credit when funding conditions tighten. Leasing, though formally classified as non–asset-backed in the data, is economically similar to secured credit since the lessor retains legal ownership of the asset, facilitating recovery. Taken together, these results suggest that the observed size-differential effects of the policy were driven primarily by a contraction in unsecured, cash-flow–based lending, rather than by uniform tightening across all credit types.

8 Conclusion

This paper provides new evidence that macroprudential FX regulations—while effective in reducing financial dollarization—can amplify financing disparities across the firm-size distribution. Exploiting cross-sectional variation in banks' exposure to Peru's 2014 credit dedollarization policy, and drawing on matched administrative data covering the universe of bank–firm credit relationships,

[44] See Table A.11 for the composition of credit by loan type.

[45] Due to the limited number of observations in some categories, I recode missing growth rates as zero within relevant subsamples: for asset-backed and non–asset-backed loans, zeros are assigned when total dollar loan growth is positive but the specific loan type is not issued; for leasing and cash-flow loans, zeros are assigned within the subset of firms issuing non–asset-backed credit. This approach retains information on both issuance and non-issuance months and helps capture extensive-margin effects within each group.

I document that the tightening of dollar credit supply fell disproportionately on micro and small firms. Larger firms, by contrast, mitigated the shock through their greater ability to substitute across banks and currencies. The adjustment occurred primarily through reductions in unsecured, non–asset-backed lending, with little offset from nonbank financial institutions.

These results highlight a fundamental distributional trade-off inherent in FX-targeted macroprudential regulation. Policies that restrict foreign-currency lending successfully reduce systemic currency-mismatch risks but may unintentionally curtail access to external finance for the most constrained firms—precisely those for whom cheaper dollar loans play a key role in relaxing borrowing constraints. The documented declines in employment among exposed micro and small firms underscore that these credit-supply effects extend to the real economy.

From a policy perspective, the findings suggest that dedollarization efforts in partially dollarized economies may need to be complemented with measures that support smaller firms' transition toward local-currency financing—such as targeted credit guarantees or liquidity facilities in domestic currency. More broadly, because small and young firms constitute the backbone of firm dynamics and productivity growth, sustained tightening of their financing conditions may slow aggregate productivity and contribute to rising market concentration over time (IMF, 2021). Ensuring that macroprudential frameworks preserve financial stability without disproportionately burdening smaller borrowers is therefore essential for both inclusive and resilient economic growth.

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Appendix

A. Additional Tables and Figures

Table A.1: Summary Statistics

Panel A: Bank–Firm Level

	2014						2015					
Firm size	**Mean**	**Median**	**SD**	**P5**	**P95**	**N**	**Mean**	**Median**	**SD**	**P5**	**P95**	**N**
Micro												
New dollar loans	0.587	0.273	0.664	0.000	2.052	20,429	0.569	0.256	0.656	0.000	2.052	16,622
New total loans	0.553	0.306	0.594	0.000	1.791	18,475	0.522	0.268	0.582	0.000	1.791	14,640
Small												
New dollar loans	0.486	0.198	0.606	0.000	2.052	25,095	0.447	0.167	0.591	0.000	2.052	22,621
New total loans	0.395	0.146	0.523	0.000	1.791	19,386	0.362	0.115	0.509	0.000	1.791	16,560
Medium												
New dollar loans	0.409	0.170	0.551	0.000	1.982	39,467	0.387	0.145	0.545	0.000	1.918	33,497
New total loans	0.343	0.151	0.460	0.000	1.576	33,696	0.319	0.126	0.452	0.000	1.508	27,148
Large												
New dollar loans	0.461	0.210	0.581	0.000	2.052	10,584	0.474	0.223	0.587	0.000	2.052	9,419
New total loans	0.376	0.179	0.477	0.000	1.746	9,151	0.369	0.170	0.476	0.001	1.742	7,770

Panel B: Bank-level Covariates (2014)

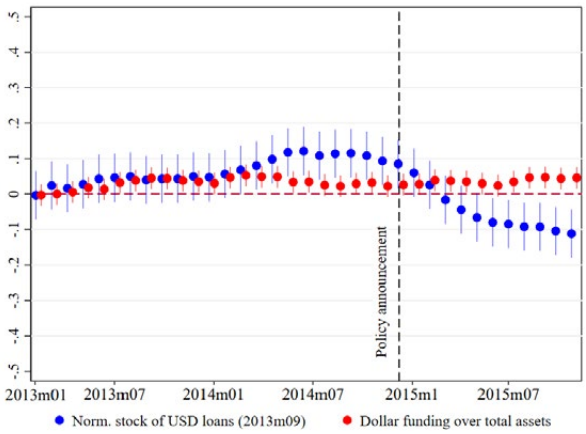
Variable	**Mean**	**Median**	**SD**	**P5**	**P95**	**N**
Capital ratio	0.137	0.132	0.014	0.121	0.156	9
Loans-to-assets	0.682	0.703	0.045	0.599	0.730	9
Non-performing loans	0.021	0.022	0.008	0.004	0.031	9

Panel C: Nr. of Bank–Firm Relationships (Dec. 2014)

Firm size	**Mean**	**Median**	**SD**	**P5**	**P95**	**N**
Micro	1.03	1.00	0.17	1.00	1.00	20,724
Small	1.39	1.00	0.64	1.00	3.00	47,862
Medium	2.07	2.00	1.14	1.00	4.00	20,008
Large	2.65	2.00	1.67	1.00	6.00	2,518

Notes. Table A.1 summarizes key variables used in the analysis. **Panel A** reports bank–firm–level monthly growth rates of new dollar and total loans for firms that issued new dollar loans during 2014–2015, by size segment. **Panel B** presents summary statistics for banks' financial ratios (2014). **Panel C** shows the distribution of the number of bank–firm relationships per firm by size segment (Dec. 2014).

Figure A.1: Average Change in the Normalized Stock of Dollar Loans and the Share of Dollar Funding to Assets (95% CI)



Notes: Figure A.1 plots the monthly change in banks' average normalized stock of dollar loans (blue dots), denoted γ_t in:

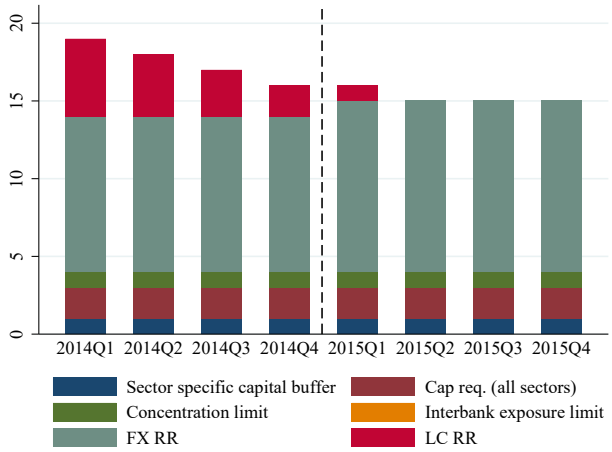
$$\frac{D_{b,t}}{D_b^{Sep2013}} = \gamma_t \sum_{i=2}^{36} 1[t=i] + \text{BankFE} + \varepsilon_{b,t},$$

and the monthly change in the average share of dollar liabilities (subject to reserve requirements) to total assets (red dots), denoted θ_t in:

$$\frac{\text{USD Liabilities}_{b,t}}{\text{Assets}_{b,t}} = \theta_t \sum_{i=2}^{36} 1[t=i] + \text{BankFE} + \varepsilon_{b,t}.$$

Both series are plotted with 95% confidence intervals.

Figure A.2: Evolution of Main Macroprudential Policies (2014–2015)



Notes: Figure A.2 shows the cumulative index of macroprudential policy actions—coded as +1 for tightenings and 1 for easings—over the period 2014–2015. The index is constructed from the IBRN cross-country database on prudential policy instruments (Cerutti et al., 2017).

Table A.2: Regulator Firm Size Definition

			Total Debt (USD)	
Size Category	May Include Listed Firms	Annual Sales	Above	Below
Micro	No	—	—	7,142
Small	No	—	7,142	107,142
Medium	No	Below 7 million USD	107,142	—
Large	Yes	Above 7 million USD	—	—

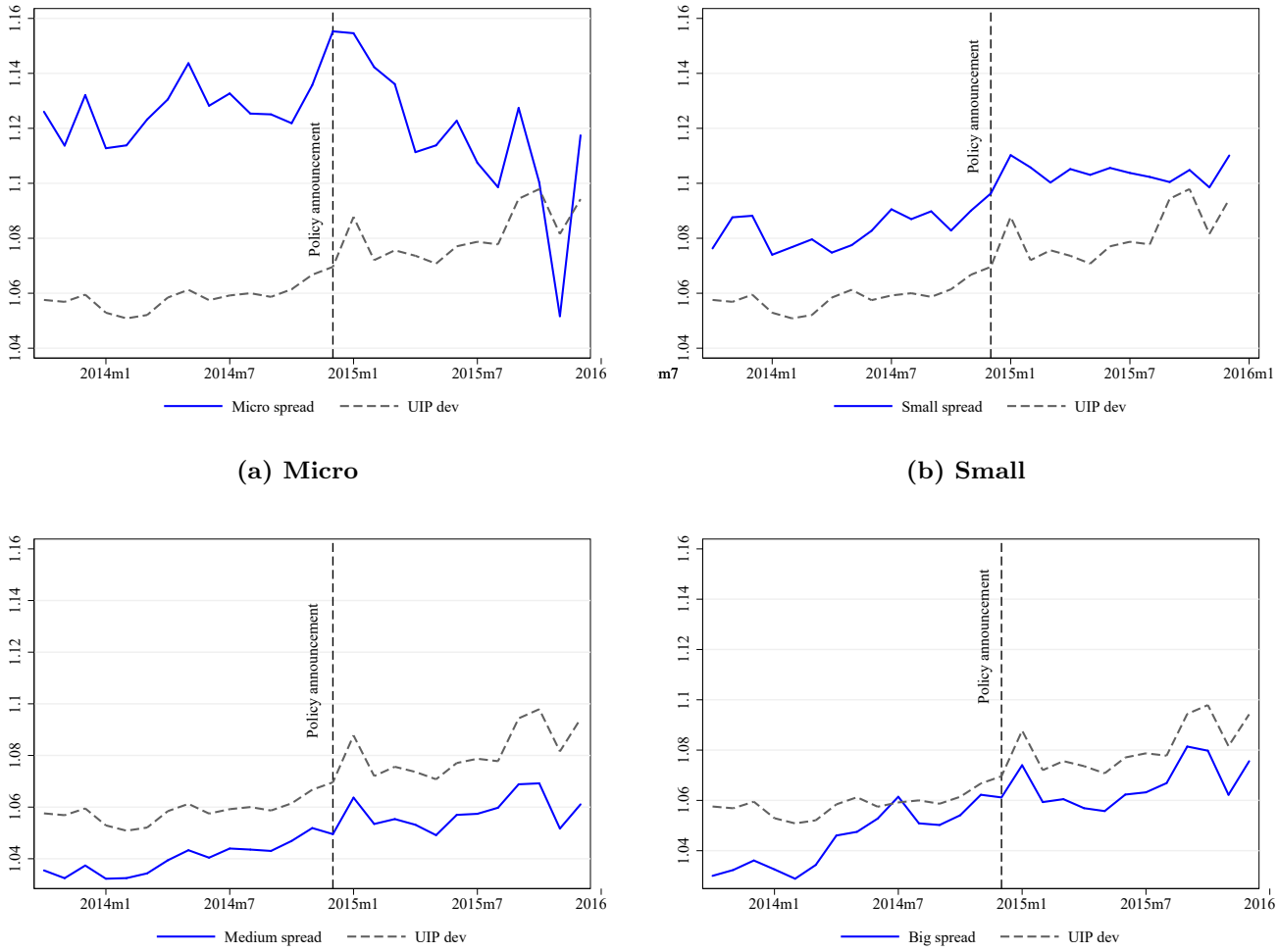
Source: Superintendencia de Banca, Seguros y AFP (SBS), Resolution SBS No. 11356–2008.

Notes: Table A.2 presents the firm size classification used in the empirical analysis, based on the official definition established by the Peruvian financial regulator (Resolution SBS No. 11356–2008). The regulator defines five size categories: *corporate, large, medium, small*, and *micro*.

Corporate firms are those with annual sales above 200 million soles (approximately US$71.4 million), while *large* firms have annual sales between 20 and 200 million soles or have access to capital markets. Because the number of corporate firms is limited, the analysis groups corporate and large firms together under the category *large*.

Medium firms have annual sales below 20 million soles and typically maintain total debt with the financial system greater than 300,000 soles. *Small* and *micro* firms have total indebtedness below 300,000 soles and 20,000 soles, respectively. These firms generally employ fewer than 100 workers and report annual sales below 6 million soles and 570 thousand soles, respectively (based on data from SUNAT, the tax authority).

Figure A.3: Average UIP deviations using bank loan interest rates

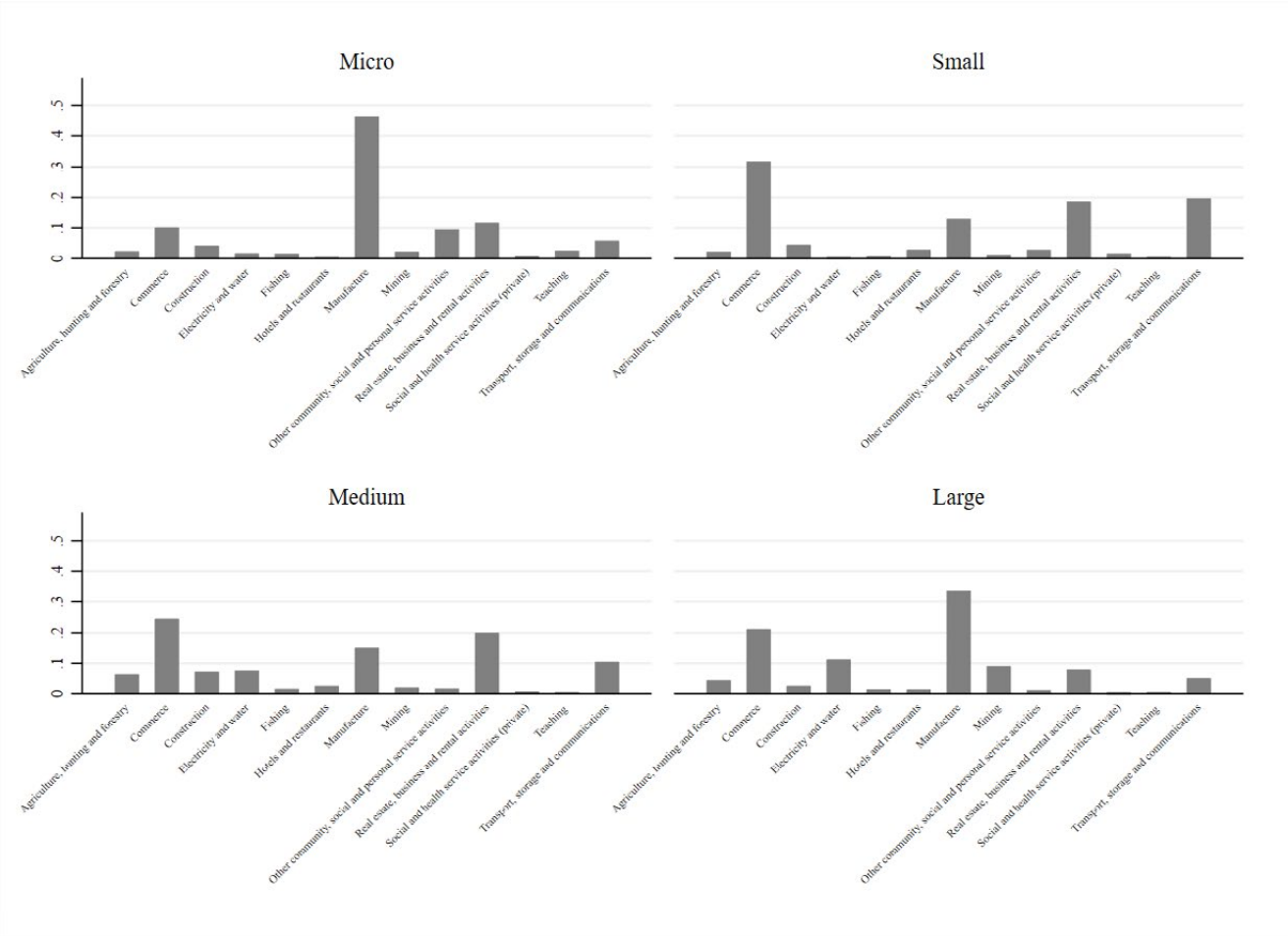


(a) Micro **(b) Small**

(c) Medium **(d) Large**

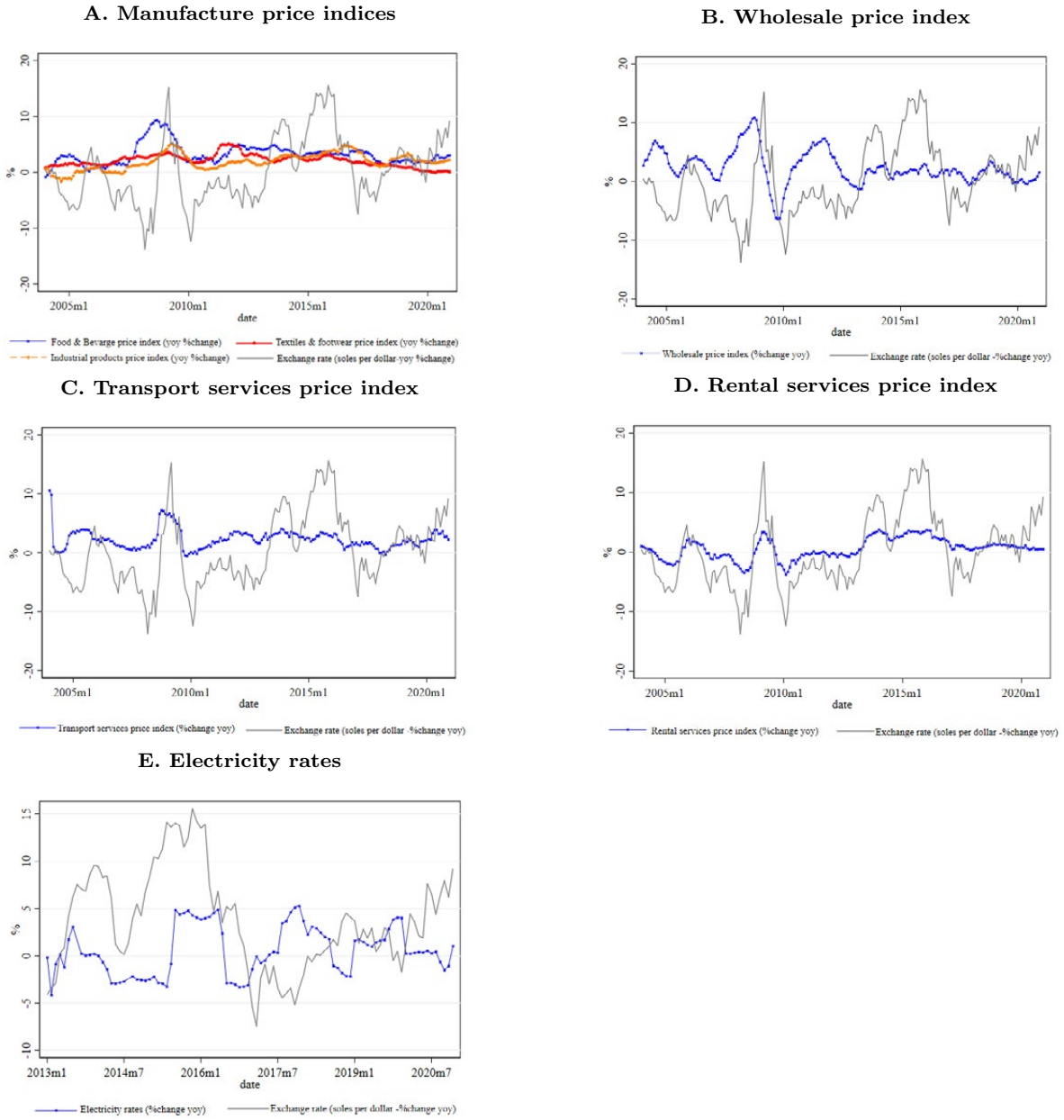
Notes: The dashed gray line plots deviations from uncovered interest parity (UIP), $\text{UIPdev}_t = \frac{e_t}{\mathbb{E}_t[e_{t+1}]} \cdot \frac{1+r_t}{1+r_t^*}$, and the solid blue line plots the average UIP deviation using bank loan rates, $\text{spread}_t^{(\text{size})} = \frac{e_t}{\mathbb{E}_t[e_{t+1}]} \cdot \frac{1+r_t^{L,\text{size}}}{1+r_t^{L*,\text{size}}}$. Here, r_t and r_t^* are one-year Treasury bill rates for Peru and the U.S., respectively; $r_t^{L,\text{size}}$ and $r_t^{L*,\text{size}}$ are average sol- and dollar-denominated bank loan rates by firm size; e_t is the PEN/USD exchange rate and $\mathbb{E}_t[e_{t+1}]$ is the one-year-ahead expected exchange rate (BCRP expectations survey, `https://www.bcrp.gob.pe/estadisticas/encuesta-de-expectativas-macroeconomicas.html`).

Figure A.4: Distribution of dollar loans across industrial sectors



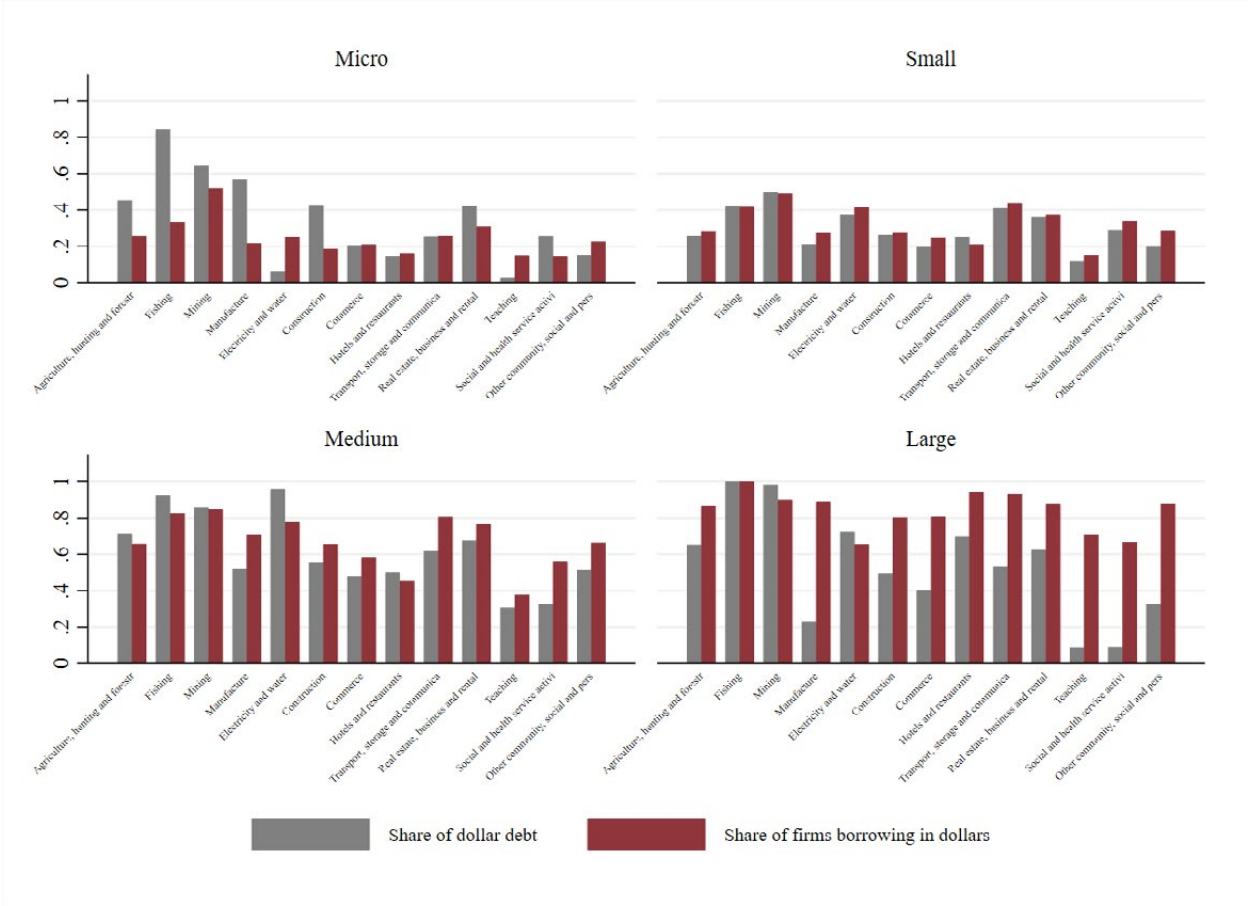
Notes: Figure A.4 shows the distribution of dollar loans across industrial sectors by firm size segment. Dollar lending is broadly distributed across sectors, but it is most concentrated in *Commerce*, *Manufacture*, *Real Estate*, *Transport*, and *Electricity*. Specifically, *micro* firms concentrate 66% of their dollar loans in *Commerce*, *Manufacture*, and *Real Estate*. *Small* and *medium-sized* firms account for 82% and 69% of their dollar loans, respectively, in *Commerce*, *Manufacture*, *Real Estate*, and *Transport*. The *large* firm segment concentrates 66% of dollar loans in *Commerce*, *Manufacture*, and *Electricity*.

Figure A.5: Exchange rate and price indices



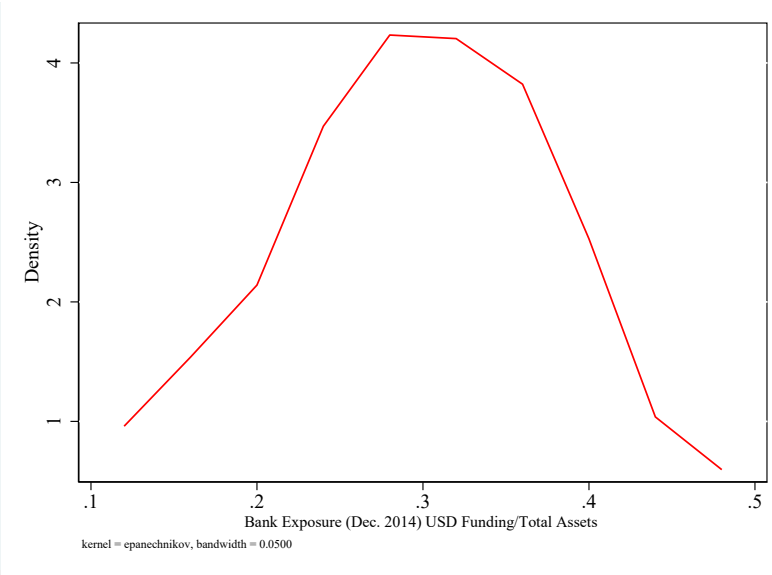
Notes: Figure A.5 shows the monthly evolution of the year-over-year percentage change in the nominal exchange rate (soles per U.S. dollar) and inflation rates across key price indices. Each panel corresponds to one of the main dollarized industrial sectors: (A) *Manufacture*—food and beverage, textiles, and industrial products price indices; (B) *Commerce*—wholesale price index; (C) *Transport services*; (D) *Real estate*—rental services index; and (E) *Electricity rates*.

Figure A.6: Industrial sectors and debt dollarization of unhedged firms



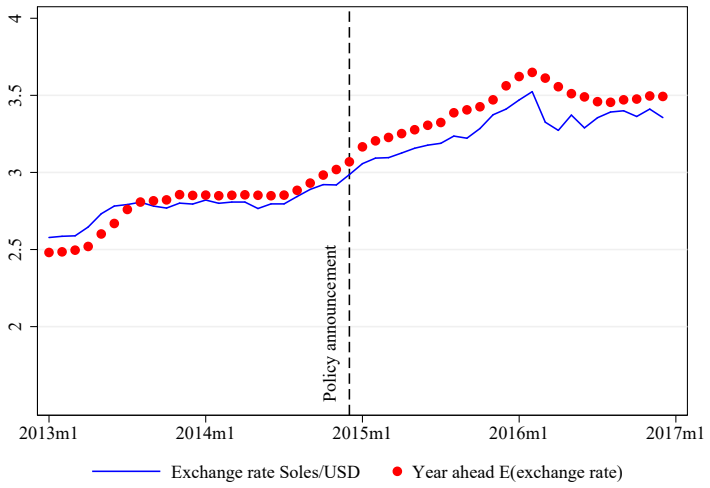
Notes: Figure A.6 shows, by industry and size segment, the debt dollarization ratio (first column) and the share of firms borrowing in dollars (second column) among unhedged firms—defined as non-exporters and non-importers that do not issue FX derivatives. All figures are based on firm-level data as of December 2014 (policy announcement date).

Figure A.7: Kernel Density of the Share of Dollar Funding to Total Assets (December 2014)



Notes: Figure A.7 plots the kernel density of banks' share of dollar funding—subject to reserve requirements—relative to total assets as of December 2014. The figure illustrates the substantial heterogeneity in banks' exposure to the dedollarization policy at the time of its announcement.

Figure A.8: Exchange Rate (Soles/USD) and 12-Month-Ahead Expectations



Notes: Figure A.8 shows the evolution of the nominal exchange rate (soles per U.S. dollar) and the 12-month-ahead exchange rate expectations, based on the Central Reserve Bank of Peru (BCRP) Expectations Survey. Data are available at `https://www.bcrp.gob.pe/estadisticas/encuesta-de-expectativas-macroeconomicas.html`.

Table A.3: Impact of the 2014 Dedollarization Policy on Credit Growth: Firms issuing trade-related loans

Panel A. Intensive margin

	Δ log(New FX Loans)			Δ log(New Sol Loans)		
	(1) All	(2) Micro & Small	(3) Medium & Large	(4) All	(5) Micro & Small	(6) Medium & Large
Exposure × *Post*	-0.128	-2.008	-0.013	-1.681	-5.307	-1.746
	(0.376)	(2.337)	(0.405)	(1.391)	(4.187)	(1.451)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,784	552	13,252	2,725	38	2,503
R^2	0.570	0.710	0.546	0.664	0.812	0.658

Panel B. Extensive margin

	Pr(New FX Loan)			Pr(New Sol Loan)		
	(1) All	(2) Micro & Small	(3) Medium & Large	(4) All	(5) Micro & Small	(6) Medium & Large
Exposure × *Post*	-0.071	-0.079	-0.023	0.053	-0.299	0.181
	(0.111)	(0.222)	(0.142)	(0.144)	(0.240)	(0.196)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	54,552	5,119	46,033	54,552	5,119	46,033
R^2	0.317	0.440	0.314	0.369	0.483	0.368

Notes: Panel A reports intensive-margin results, where the dependent variable is the monthly growth rate of firm-level new credit: dollar loans in columns (1)–(3) and sol loans in columns (4)–(6). Panel B reports extensive-margin results, where the dependent variable is a dummy equal to one if firm f issues a new loan in month t. *Exposure* × *Post* captures the effect of the 2014 dedollarization policy. Lower-order interactions terms corresponding to *Exposure* × *Post* × *Size* are absorbed by fixed effects. Standard errors are robust and clustered at the firm level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: 2013 Reserve Requirement adjustments: Placebo Tests

	Δ log(New FX Loans)			Δ log(New Total Loans)		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × Post 1 (Mar 2013 RR Increase)	0.130		0.379	0.338		0.558
	(0.843)		(0.886)	(0.695)		(0.738)
× Small	-0.195		-0.672	-0.422		-0.928
	(1.085)		(1.127)	(0.972)		(1.000)
× Medium	-0.615		-0.811	-0.640		-0.835
	(0.890)		(0.938)	(0.743)		(0.791)
× Large	-0.795		-0.993	-1.028		-1.194
	(0.949)		(1.007)	(0.817)		(0.886)
Exposure × Post 2 (Sep 2013 Dedollarization)		-0.451	-0.526		-0.355	-0.463
		(0.497)	(0.519)		(0.464)	(0.492)
× Small		0.891	1.023		0.908	1.088*
		(0.666)	(0.691)		(0.623)	(0.644)
× Medium		0.270	0.440		0.257	0.426
		(0.530)	(0.555)		(0.493)	(0.524)
× Large		0.230	0.445		0.122	0.378
		(0.577)	(0.611)		(0.538)	(0.581)
Joint tests (p-values):						
Small – Post 1	0.928		0.691	0.907		0.604
Medium – Post 1	0.132		0.212	0.306		0.386
Large – Post 1	0.124		0.203	0.113		0.202
Small – Post 2		0.356	0.310		0.208	0.157
Medium – Post 2		0.376	0.698		0.597	0.852
Large – Post 2		0.455	0.805		0.397	0.786
Controls × Month	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	120,804	120,804	120,804	97,013	97,013	97,013
R^2	0.541	0.541	0.541	0.579	0.579	0.579

Notes: The dependent variable in columns (1)–(3) is the monthly growth rate of new dollar loans, and in columns (4)–(6) the growth rate of new total loans. Estimates are based on the main bank–firm-level specification using an earlier sample period (January 2013–December 2014). *Post 1* equals one after the March 2013 increase in reserve requirements on dollar liabilities targeting mortgage and car loans, while *Post 2* equals one after the September 2013 dedollarization policy announcement. The exposure measure is the same as in the main analysis, constructed using bank-level foreign-currency loan shares in December 2014. Lower-order interaction terms corresponding to the triple interactions are absorbed by bank-month and size-month fixed effects. Standard errors are clustered at the firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Impact of the 2014 Dedollarization Policy on Credit Growth: Alternative Pre-Treatment Windows

	Δ log(New FX Loans)			Δ log(New Total Loans)		
	(1) 9M Pre	(2) 6M Pre	(3) 3M Pre	(4) 9M Pre	(5) 6M Pre	(6) 3M Pre
Exposure × *Post*	-1.590**	-1.458**	-1.833**	-1.595***	-1.389**	-1.919**
	(0.626)	(0.657)	(0.866)	(0.618)	(0.603)	(0.776)
× Small	0.464	0.473	0.489	0.735	0.494	0.416
	(0.774)	(0.832)	(1.032)	(0.729)	(0.765)	(0.924)
× Medium	1.343**	1.213*	1.566*	1.594**	1.282**	1.788**
	(0.645)	(0.678)	(0.891)	(0.634)	(0.626)	(0.809)
× Large	1.753**	1.778**	2.235**	1.521**	1.636**	2.521***
	(0.688)	(0.737)	(0.974)	(0.673)	(0.692)	(0.912)
Joint test (p-values):						
Small	0.022	0.071	0.027	0.049	0.082	0.007
Medium	0.244	0.291	0.344	0.995	0.635	0.627
Large	0.600	0.374	0.395	0.796	0.489	0.228
Controls × Month	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	99,334	81,716	64,933	77,041	62,737	49,008
R^2	0.554	0.565	0.579	0.598	0.612	0.626

Notes: The dependent variable in columns (1)–(3) is the monthly growth rate of new dollar loans, and in columns (4)–(6) the growth rate of new total loans. Each column adjusts the pre-treatment estimation window to begin 9, 6, or 3 months before the December 2014, while the post treatment period remains unchanged. Dollar amounts are converted to soles at the January 2014 exchange rate to clean for valuation effects. *Exposure* × *Post* captures the effect of the policy on Micro firms, which is the omitted category. The following rows capture the differential effect (relative to micro firms) of the policy for the rest of size segments. Joint test reports the p-value of the F-test that the overall effect of the policy on the rest of size segments, i.e. the sum of the coefficients of *Exposure* × *Post* and *Exposure* × *Post* × *Size* is equal to 0. Post starts in December 2014. The coefficients of *Exposure* × *Size* are absorbed by the inclusion of bank–firm FE. Coefficients on *Post* × *Size* are absorbed by the inclusion of size–month fixed effects. Standard errors are clustered at the firm level and reported in parenthesis.*** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Impact of the 2014 Dedollarization Policy on Credit Growth: Varying Post-Treatment Windows

	Δ log(New FX Loans)				Δ log(New Total Loans)			
	(1) Full Sample	(2) 8M Post	(3) Post 11M	(4)	(5) Full Sample	(6) Post 8M	(7) 11M Post	(8)
Exposure × Post	-1.084**	-1.102*	-0.962*	-1.022*	-1.082**	-1.110*	-0.950*	-1.037*
	(0.539)	(0.598)	(0.544)	(0.595)	(0.523)	(0.609)	(0.539)	(0.593)
× Small	-0.211	-0.222	-0.330	-0.164	0.099	0.410	0.122	0.347
	(0.689)	(0.725)	(0.675)	(0.717)	(0.647)	(0.699)	(0.640)	(0.683)
× Medium	0.758	0.732	0.645	0.746	1.022*	0.961	0.916	0.917
	(0.561)	(0.620)	(0.566)	(0.616)	(0.541)	(0.624)	(0.558)	(0.608)
× Large	1.216**	1.071	1.071*	1.105*	0.961*	0.942	0.845	0.947
	(0.602)	(0.659)	(0.607)	(0.654)	(0.577)	(0.661)	(0.591)	(0.642)
Exposure × Post 2 (Placebo: Aug 2015 FW limits adjustment)				0.250				0.358
				(0.837)				(0.759)
× Small				-0.800				-1.146
				(1.036)				(0.966)
× Medium				-0.416				0.017
				(0.856)				(0.778)
× Large				-0.149				-0.410
				(0.932)				(0.806)
Joint tests (p-values):								
Small	0.005	0.003	0.003	0.005	0.019	0.071	0.032	0.019
Medium	0.110	0.097	0.127	0.110	0.760	0.481	0.865	0.760
Large	0.652	0.921	0.713	0.652	0.653	0.558	0.699	0.653
Small – Post 2				0.411				0.219
Medium – Post 2				0.606				0.223
Large – Post 2				0.830				0.885
Controls × Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	117,150	98,374	112,760	112,760	91,350	77,364	88,071	88,071
R^2	0.546	0.561	0.548	0.548	0.589	0.601	0.591	0.591

Notes: The dependent variable in columns (1)–(4) is the monthly growth rate of new dollar loans, and in columns (5)–(8) that of new total loans. All amounts are converted to soles at the January 2014 exchange rate to net out valuation effects. Columns (1) and (5) correspond to the main specification using the full 2014–2015 sample. Columns (2) and (6) restrict the post-treatment period to eight months (through July 2015). Columns (3)–(4) and (7)–(8) extend the post-treatment window to eleven months and include a placebo term for the August 2015 forward-limit adjustment. *Exposure × Post* captures the policy's effect for micro firms (omitted category), and size interactions report differentials relative to micro firms. Robust standard errors clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Pre-Policy Relationship Between Credit Exposure and Nonperforming Dollar Loans (2014)

Dependent variable:	Non-performing dollar loans ratio	
	(1)	(2)
Exposure	0.632	
	(0.671)	
× Small	-0.470	-0.412
	(0.678)	(0.665)
× Medium	-0.637	-0.568
	(0.671)	(0.658)
× Large	-0.604	-0.512
	(0.671)	(0.659)
Joint test (p-values):		
Small	0.134	0.536
Medium	0.903	0.389
Large	0.415	0.437
Controls × Month	Yes	Yes
Firm FE	Yes	Yes
Bank FE	No	Yes
Size × Month FE	Yes	Yes
Industry × Location × Month FE	Yes	Yes
Observations	525,171	525,171
R^2	0.726	0.727

Notes: The dependent variable is the ratio of nonperforming dollar loans to total dollar loans for firm–bank relationships in 2014, the year prior to the implementation of the dedollarization policy. The coefficient on bank-level *Exposure* captures the conditional correlation between a bank's exposure and the nonperforming loan ratio for micro firms (omitted category). Subsequent rows report the differential correlations for other firm-size segments relative to micro firms. The "Joint test" reports the p-value of the F-test for the null hypothesis that the overall correlation—i.e., the sum of the coefficients on *Exposure* and *Exposure* × *Size*—equals zero. Standard errors are clustered at the firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8: Distribution of loans by type of financial institution

	Micro		Small		Medium		Large	
	Sol	USD	Sol	USD	Sol	USD	Sol	USD
Panel A. Loan Amount								
Banks	71.45	89.25	88.73	90.28	92.45	98.35	99.83	99.94
Municipal	17.93	3.62	8.33	3.09	5.34	1.15	0.06	0.01
Rural	1.42	0.01	0.77	0.12	0.75	0.08	0.05	0.01
Other Financial inst.	8.83	7.12	2.07	6.50	1.41	0.42	0.05	0.04
Edpymes	0.37	-	0.10	-	0.04	-	-	-
Panel B. Number of Firms								
Banks	83.46	93.19	86.21	86.80	93.86	95.58	99.22	99.28
Municipal	11.96	1.12	9.36	3.14	4.69	1.56	0.34	0.05
Rural	1.17	0.04	1.16	0.14	0.60	0.11	0.27	0.04
Other Financial inst.	3.03	5.64	3.08	9.91	0.79	2.75	0.17	0.63
Edpymes	0.38	0.02	0.19	0.01	0.05	-	-	-

Notes: Table A.8 shows the share of loans (Panel A:volume and Panel B: number) granted by each type of financial institution, by currency and size segment. Calculations are made using data from December 2014 (policy announcement). Source: SBS, own calculations.

Table A.9: Firms' relationships with banks and non-bank financial institutions

Share of firms with (%)	Micro	Small	Medium	Large
Relationships with banks only	81.69	70.97	77.18	94.90
Relationships with non-banks only	12.97	8.87	1.74	0.08
Relationships with both banks and non-banks	5.34	20.15	21.08	5.02

Source: SBS, own calculations. Calculations are made using data from December 2014.

Table A.10: Impact of the 2014 Dedollarization Policy on Credit Growth from Non-bank Financial Institutions (NBFIs)

	Δ log(New FX Loans)	Δ log(New Soles Loans)	Δ log(New Total Loans)
	(1)	(2)	(3)
Exposure × *Post*	0.083	0.017	0.018
	(0.060)	(0.013)	(0.013)
× Small	0.007	-0.006	0.002
	(0.058)	(0.019)	(0.016)
× Medium	-0.046	-0.009	-0.016
	(0.062)	(0.015)	(0.013)
× Large	0.061	-0.014	-0.021
	(0.147)	(0.014)	(0.016)
Joint test (p-values):			
Small	0.011	0.538	0.099
Medium	0.436	0.403	0.816
Large	0.321	0.749	0.770
Controls × Month	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes
Observations	655,200	655,200	655,200
R^2	0.194	0.204	0.202

Notes: The estimates follow the firm-level specification in equation (5). The dependent variables in columns (1)–(3) are the monthly growth rates of new dollar loans, new total loans, and new sol loans, respectively, granted by non-bank financial institutions (NBFIs). The estimation sample is identical to that used in the main firm-level analysis, but the outcome variable is replaced with the corresponding NBFI lending. When NBFI lending is missing, its value is recoded as zero, treating months with no new NBFI credit as zero growth. Dollar amounts are converted to soles at the January 2014 exchange rate to remove valuation effects. *Exposure* × *Post* captures the effect of the 2014 dedollarization policy on micro firms (omitted category). Interaction terms report differential effects for other firm-size segments relative to micro firms. The "Joint test" reports the p-value of the F-test for the null hypothesis that the total policy effect for each size group—i.e., the sum of the coefficients on *Exposure* × *Post* and *Exposure* × *Post* × *Size*—equals zero. The sample excludes trade-related loans and covers the period January 2014–December 2015 at monthly frequency, with *Post* beginning in December 2014. Lower-order interaction terms corresponding to *Exposure* × *Post* × *Size* are absorbed by the fixed effects. Standard errors are clustered at the firm level and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: Composition of overall commercial credit by loan type

	Asset-Based(%)	Non-Asset-Based(%)		
		Cash-flow	Leasing	Others
Panel A. Dollar Loans				
Micro	67.98	13.53	11.28	7.21
Small	79.66	16.05	1.20	3.09
Medium	86.04	6.84	1.88	5.23
Large	81.47	6.05	1.63	10.85
Panel B. Sol Loans				
Micro	30.48	63.43	0.00	6.09
Small	23.80	64.57	6.98	4.65
Medium	55.82	25.71	0.64	17.83
Large	50.87	11.74	0.94	36.45

Notes: Table A.11 shows the composition of loans depending on whether or not they are secured by physical assets (asset-based and non-asset based loans). Within non-asset based, loans are classified in cash-flow loans, leasing, and others. The sample in Panel A, includes the universe of loans in dollars, excluding loans for international trade. Panel B includes the universe of sol loans of the firms included in Panel A. Calculations are made using data from December 2014 (policy announcement).

Table A.12: Impact of the 2014 Dedollarization Policy on the Growth of New Dollar Loans by Type of Collateral

	Non-Asset-Based Loans			Asset-Based Loans
	All	Cash-flow Loans	Leasing	
	(1)	(2)	(3)	(4)
Exposure × *Post*	-0.345	-3.062*	-0.116	-0.019
	(0.405)	(1.798)	(0.118)	(0.360)
× Small	0.082	0.622	0.333*	-0.375
	(0.557)	(4.001)	(0.171)	(0.456)
× Medium	0.320	2.434	-0.156	-0.041
	(0.417)	(2.101)	(0.203)	(0.389)
× Large	0.161	3.710	0.320	0.079
	(0.478)	(2.705)	(0.686)	(0.433)
Joint test (p-values):				
Small	0.485	0.491	0.083	0.156
Medium	0.789	0.575	0.100	0.674
Large	0.458	0.753	0.764	0.800
Bank × Firm FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes
Observations	118,885	16,019	16,019	118,885
R^2	0.563	0.518	0.717	0.550

Notes: The dependent variable in columns (1)–(4) is the monthly growth rate of new dollar loans granted by bank b to firm f, respectively for: loans without collateral, cash flow loans, leasing loans, and asset-backed loans. *Exposure* × *Post* measures the effect of the 2014 dedollarization policy on micro firms (omitted category), and subsequent rows report heterogeneous effects by firm size relative to micro firms. Missing growth rates are recoded as zero within relevant subsamples: for asset-backed and non–asset-backed loans, zeros are assigned when total dollar loan growth is positive but the specific loan type is not issued; for leasing and cash-flow loans, zeros are assigned within the subset of firms issuing non–asset-backed credit. Standard errors are clustered at the firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B. Additional Robustness checks

Use of FX derivatives. An alternative explanation to the size differentiated effects of the policy could be that firms in non-tradable sectors that are hedged through FX derivatives have a more inelastic demand for dollar financing relative to soles financing. The regulation excludes exporters/importers that naturally would have a specific preference for dollar loans to match their dollar receivables or purchases. However, non tradable firms issuing FX derivatives may as well have specific preference for dollar financing (they are willing to pay fee for hedging), and as Table 2 shows, the issuance of FX derivatives is concentrated in medium and large firms. Therefore, in order to rule out that the size differential results capture the unobserved drivers of hedged firms's preference for dollar debt, rather than a borrowing constraints story, I exclude these firms from the analysis and check if the size differential effects persists. Results in Table B.1 show they are almost unchanged.

Alternative indicators of size. The main results rely on the size classification used by the financial regulator (SBS), which serves as the basis for setting differentiated regulatory requirements and is also the segmentation scheme adopted by banks when setting differentiated lending terms (Table A.2). This definition is therefore the most relevant for identifying how the policy affected the commercial credit market. As a robustness exercise, I re-estimate the specification using alternative indicators of firm size—age, employment, and sales terciles—to assess whether the heterogeneous effects depend on the specific classification used. As shown in Table B.2, among the alternative measures, firm age produces results that are qualitatively and quantitatively closest to those obtained under the SBS definition. This consistency suggests that credit history plays a central role in how firms adjusted to the negative supply shock. Younger firms—typically with shorter relationships and credit records—faced tighter constraints, consistent with the policy affecting riskier and less established borrowers.

Alternative indicator of bank exposure. Table B.3 presents results using an alternative measure of bank exposure, defined as the bank's distance from the regulatory benchmark, $\frac{D_b^{Dec2014}}{D_b^{Sep2013}}$, at the time of the policy announcement. The estimates remain both qualitatively and quantitatively consistent with the main bank–firm level results in Table 4, reinforcing the view that the currency composition of banks' assets closely mirrors that of their liabilities.

Alternative clustering. Although the treatment varies at the bank level, the allocation of firms across differently exposed banks can be considered quasi-random (see Figure 7). In the main specification, I cluster standard errors at the firm level to account for serial correlation within firms over time. As a robustness check, I first cluster standard errors at the bank–month level to allow for contemporaneous shocks common to all firms borrowing from the same bank in a given period. As expected, given the limited number of banks,

standard errors increase substantially and statistical significance weakens somewhat. I also re-estimate the specification clustering at the firm–month level to account for firm-specific shocks that could simultaneously affect all lending relationships of a given firm in a particular month. In both cases, the statistical significance of the coefficients still suggest that micro firms experience the sharpest contraction in total credit, and/or larger firms are comparatively less affected in dollar lending (see Table B.4).

Additional exclusions. The baseline analysis excludes two specialized institutions that sit at opposite ends of the exposure–borrower-size distribution. The first is a highly exposed bank whose portfolio consists almost exclusively of large firms. Because its borrowers are concentrated in a single size category and face an unusually high exposure level, including this institution risks distorting the estimated treatment effect for large firms—unless these firms fully reallocate borrowing to less-exposed lenders. The second is a low-exposure bank focused almost entirely on micro and small firms; its limited exposure to the policy mechanically attenuates the estimated contraction for smaller borrowers. These two institutions are therefore not good counterfactuals for the rest of the banking system, as they are structurally different precisely along the firm-size dimension that drives heterogeneity in responses. Nevertheless, Table B.5 shows that including them sequentially does not alter the main qualitative conclusions: the pattern of stronger effects among smaller firms remains, while large-firm loans appear somewhat more affected when the highly exposed, large-firm bank is included—as expected given its portfolio composition. Together, these specialized banks represent less than 1.9 percent of total dollar credit in the banking system, so their exclusion has negligible implications in the aggregate.

Table B.1: Impact of the 2014 Dedollarization Policy on Firm Credit Growth: Excluding Firms with FX Derivatives

	Δ log(New FX Loans)	Δ log(New Total Loans)	Δ log(New Sol Loans)
	(1)	(2)	(3)
Exposure × *Post*	-1.439**	-1.674**	-0.852
	(0.693)	(0.697)	(2.773)
× Small	-1.890**	-0.999	-0.500
	(0.816)	(0.820)	(3.066)
× Medium	0.918	1.120	1.628
	(0.697)	(0.703)	(2.827)
× Large	1.275	1.073	2.592
	(1.115)	(1.085)	(3.254)
Joint test (p-values):			
Small	8.98e-08	1.81e-05	0.477
Medium	0.162	0.189	0.496
Large	0.860	0.507	0.310
Controls × Month	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes
Observations	56,273	41,457	6,713
R^2	0.578	0.630	0.723

Notes: The dependent variable in columns (1)–(3) is the monthly growth rate of new dollar, total, and sol-denominated loans, respectively, aggregated at the firm level. The sample excludes all firms that, within each period, have a FX derivative contract with at least one bank in the banking system. Dollar amounts are converted to soles at the January 2014 exchange rate to remove valuation effects. *Exposure* × *Post* captures the effect of the dedollarization policy on micro firms (the omitted category), while interaction terms capture differential effects by firm size relative to micro firms. "Joint test" reports the p-value of the F-test that the sum of *Exposure* × *Post* and *Exposure* × *Post* × *Size* equals zero for each size group. All regressions include firm fixed effects and the fixed effects indicated. Standard errors are clustered at the firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.2: Impact of the 2014 Dedollarization Policy on Credit Growth: Alternative size related indicators (Horse Race)

	X = Age terciles		X = Sales terciles		X = Workers terciles	
	Δ log(FX)	Δ log(Total)	Δ log(FX)	Δ log(Total)	Δ log(FX)	Δ log(Total)
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × *Post*	-1.306**	-0.915*	-0.440	-1.192***	-0.474	-0.333
	(0.574)	(0.488)	(0.441)	(0.422)	(0.470)	(0.468)
× 2nd tercile	0.887	0.658	-0.427	0.699	-0.407	0.311
	(0.644)	(0.561)	(0.592)	(0.586)	(0.612)	(0.602)
× 3rd tercile	1.041*	0.743	0.091	0.987**	0.237	0.079
	(0.588)	(0.504)	(0.464)	(0.444)	(0.497)	(0.490)
Joint test (p-val):						
2nd tercile	0.186	0.387	0.039	0.254	0.032	0.958
3rd tercile	0.144	0.331	0.053	0.230	0.239	0.179
Controls × Month	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
X × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	116,647	90,865	113,302	88,048	96,248	74,393
R^2	0.546	0.589	0.543	0.587	0.541	0.585

Notes: Each pair of columns reports estimates of the impact of the 2014 dedollarization policy using a different size-related indicator X: age, sales, and number of workers terciles. The dependent variable is the monthly growth rate of new dollar loans (FX) and new total loans (Total) granted by bank b to firm f. *Exposure* × *Post* measures the effect for firms in the first tercile (omitted group), while subsequent rows show heterogeneous effects for higher terciles. Sales terciles are constructed using the median of each sales interval reported by firms to the tax authority (SUNAT), which defines 15 discrete ranges of annual sales. Joint test reports the p-value of the F-test that the overall effect of the policy on the rest terciles, i.e. the sum of the coefficients of *Exposure* × *Post* and *Exposure* × *Post* × *tercile* is equal to 0. The sample excludes trade-related loans and covers the period from 2014m1 to 2015m12 at a monthly frequency. Standard errors are clustered at the firm level and reported in parentheses. All regressions include the indicated controls and fixed effects. Standard errors are clustered at the firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3: Impact of the 2014 Dedollarization Policy on Credit Growth: Robustness to Alternative Exposure Measure

	Δ log(New FX Loans)	Δ log(New Total Loans)	Δ log(New Sol Loans)
	(1)	(2)	(3)
Exposure × *Post (Alt. measure)*	-1.145***	-1.070***	-0.586
	(0.345)	(0.297)	(1.285)
× Small	-0.099	0.191	-0.630
	(0.444)	(0.394)	(1.510)
× Medium	0.922**	1.109***	-0.546
	(0.365)	(0.317)	(1.318)
× Large	1.107***	0.954***	0.426
	(0.369)	(0.319)	(1.324)
Joint test (p-values):			
Small	0.000	0.003	0.219
Medium	0.165	0.798	0.027
Large	0.790	0.375	0.655
Controls × Month	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes
Observations	117,150	91,350	11,436
R^2	0.546	0.589	0.695

Notes: The table reports estimates of the impact of the 2014 dedollarization policy using an alternative exposure measure defined as the bank's distance from the regulatory benchmark the month of the policy announcement, $\frac{D_b^{Dec2014}}{D_b^{Sep2013}}$. The dependent variables in columns (1)–(3) are the bank-firm-level monthly growth rates of new dollar loans, new total loans, and new sol loans, respectively. *Exposure* × *Post (Alt. measure)* captures the effect of the policy on micro firms (the omitted group), while subsequent rows report heterogeneous effects by firm size. Joint test reports the p-value of the F-test that the overall effect of the policy on the rest of size segments, i.e. the sum of the coefficients of *Exposure* × *Post* and *Exposure* × *Post* × *Size* is equal to 0. The sample excludes trade-related loans and covers the period from 2014m1 to 2015m12 at a monthly frequency. Standard errors are clustered at the firm level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.4: Impact of the 2014 Dedollarization Policy on Firm Credit Growth: Robustness to Alternative Clustering

	Δ log(New FX Loans)		Δ log(New Total Loans)		Δ log(New Sol Loans)	
	Bank–Date	Firm–Date	Bank–Date	Firm–Date	Bank–Date	Firm–Date
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × *Post*	-1.084	-1.084*	-1.082*	-1.082**	0.364	0.364
	(0.623)	(0.561)	(0.573)	(0.521)	(2.310)	(2.617)
× Small	-0.211	-0.211	0.099	0.099	-2.992	-2.992
	(0.822)	(0.709)	(0.569)	(0.616)	(3.191)	(3.449)
× Medium	0.758	0.758	1.022	1.022*	-1.642	-1.642
	(0.608)	(0.581)	(0.643)	(0.572)	(2.351)	(2.703)
× Large	1.216*	1.216*	0.961	0.961	-0.936	-0.936
	(0.628)	(0.636)	(0.543)	(0.576)	(2.154)	(2.486)
Joint test (p-values):						
Small	0.194	0.002	0.173	0.025	0.204	0.216
Medium	0.002	0.028	0.669	0.746	0.098	0.047
Large	0.631	0.635	0.507	0.610	0.267	0.454
Controls × Month	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	117,150	117,150	91,350	91,350	11,436	11,436
R^2	0.546	0.546	0.589	0.589	0.695	0.695

Notes: Each pair of columns reports estimates using alternative clustering levels for standard errors. Odd columns (1), (3), and (5) cluster by bank–date, while even columns (2), (4), and (6) cluster by firm–date. The dependent variables are the monthly growth rates of new dollar loans, new total loans, and new sol loans, respectively, at the firm–bank level. *Exposure* × *Post* captures the policy effect for micro firms (the omitted category), and interaction terms report heterogeneous effects across size categories. The Joint test reports the p-value of the F-test that the total policy effect for each size group equals zero. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table B.5: Impact of the 2014 Dedollarization Policy on New Dollar Credit Growth: Including Specialized Banks by Market Segment

	Main Specification	+ Large-Firm Bank	+ Small-Firm Bank	+ Both Specialized Banks
	(1)	(2)	(3)	(4)
Exposure × *Post*	-1.084**	-1.154**	-1.010**	-1.098**
	(0.539)	(0.487)	(0.476)	(0.444)
× Small	-0.211	-0.027	0.425	0.525
	(0.689)	(0.641)	(0.528)	(0.500)
× Medium	0.758	0.857*	0.695	0.805*
	(0.561)	(0.510)	(0.495)	(0.465)
× Large	1.216**	0.807	1.123**	0.740
	(0.602)	(0.527)	(0.547)	(0.488)
Joint test (p-values):				
Small	0.005	0.007	0.031	0.031
Medium	0.110	0.134	0.097	0.118
Large	0.652	0.120	0.701	0.110
Controls × Month	Yes	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes	Yes
Size × Month FE	Yes	Yes	Yes	Yes
Industry × Location × Month FE	Yes	Yes	Yes	Yes
Observations	117,150	117,787	118,007	118,644
R^2	0.546	0.545	0.547	0.546

Notes: Column (1) reports the baseline specification excluding the two specialized banks. Columns (2)–(4) sequentially add the bank specializing in large firms (high exposure), the bank specializing in small firms (low exposure), and then both. The dependent variable is the monthly growth rate of new dollar loans at the firm–bank level. *Exposure* × *Post* captures the effect of the 2014 dedollarization policy on micro firms (omitted category), while the interaction terms show the heterogeneous effects by firm size. The joint test reports the p-value of the F-test that the total policy effect for each size group equals zero. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

C. Back-of-the-Envelope Calculation: Implied impact on investment

To quantify the potential real effects of the policy, I conduct a back-of-the-envelope exercise linking the estimated semi-elasticities of new lending to firms' investment. Because survey data on investment are only available for 2014,[46] I use a proportionality factor θ to map the predicted change in new lending into an implied change in investment, assuming that a fraction θ of new credit typically finances capital expenditures.

Step 1: Baseline new-loan flows. I define each firm's baseline monthly flow of new total loans, $N_{f,0}$, as the value of new dollar plus sol-denominated loans granted in the month immediately before the policy announcement (November 2014), for all firms issuing dollar loans. This measure reflects each firm's borrowing activity at the intensive margin just prior to the introduction of the regulation.

Step 2: Estimated semi-elasticities. From the main firm-level regression in Equation 5, I recover the size-specific coefficients $\hat{\beta}_s = \{\hat{\beta}, \hat{\delta}^{small}\}$ which are statistically significant and I use them to quantify the implied impact on investment for micro and small firms respectively.

Step 3: Predicted change in new lending. For each firm, the semi-elasticity of new lending with respect to bank exposure is

$$\hat{d}_{gf} = \hat{\beta}_{s(f)} \times \text{Exposure}_f,$$

and the implied change in monthly new total lending is

$$\widehat{\Delta \text{NewLoan}}_{f,t} = \left(e^{\hat{d}_{gf}} - 1\right) N_{f,0}.$$

I aggregate this monthly change over the twelve post-policy months to obtain:

$$\widehat{\Delta \text{NewLoan}}_{f,\text{post}} = \sum_{t \in \text{post}} \widehat{\Delta \text{NewLoan}}_{f,t}.$$

Step 4: Mapping lending to investment. I assume that a constant share θ of new credit is used to finance investment projects, while the remainder finances working capital or other short-term needs. I report results for $\theta \in \{0.2, 0.3, 0.4, 0.5\}$, covering a plausible range consistent with firm survey evidence in similar emerging-market economies. The implied change in investment for 2015 is therefore:

$$\widehat{\Delta I}_{f,2015} = \theta \cdot \widehat{\Delta \text{NewLoan}}_{f,\text{post}}.$$

[46] Obtained from the 2014 Encuesta Económica Anual (EEA) and Encuesta Nacional de Empresas (ENE) conducted by the National Institute of Statistics and Informatics (INEI)

Step 5: Implied investment growth. I express the effect as an implied growth rate of investment relative to its pre-policy level, obtained from 2014 survey data:

$$\widehat{g}_{I,f} = \frac{\Delta \widehat{I}_{f,2015}}{I_{f,2014}},$$

replacing missing or zero $I_{f,2014}$ values with the mean of I_{2014} within each industry. I then average these firm-level effects by size category:

$$\widehat{g}_{I,s} = \frac{1}{N_s} \sum_{f \in s} \widehat{g}_{I,f}.$$

Step 6: Results. The back-of-the-envelope calculation suggests that the policy led to a decline in investment for smaller firms. Under a conservative mapping parameter of $\theta = 0.2$, investment among micro firms is estimated to have fallen by about 15%, and by about 9% among small firms. Using higher values of θ in the range 0.3–0.5 yields implied investment declines between 23–39% for micro firms and 14–24% for small firms. These magnitudes are consistent with tighter borrowing constraints for smaller firms, which rely more heavily on bank credit to finance capital expenditures.

Choice of θ. The proportionality factor θ represents the share of fixed investment typically financed through bank credit. According to the 2023 World Bank Enterprise Surveys, Peruvian firms financed on average 39.5 percent of their total fixed-asset purchases with bank loans, compared with an average of 21.9 percent across Latin American economies. Among Peruvian small firms (5–19 employees), the corresponding share is even higher at 40.8 percent, while for Latin America's small firms is 19.4 percent. This is consistent with a conservative range of $\theta \in [0.2, 0.5]$ adopted in the previous calculations.

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