Capital Controls and Risk Misallocation: Evidence from a Natural Experiment

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Foreign currency debt has led to many crises in emerging markets. However, in the past decade, firms in emerging economies have drastically increased their foreign currency borrowing, making them significantly exposed to depreciation shocks. To reduce the exposure of firms to external shocks, central banks have increased their use of capital controls. In this paper I study whether capital controls can have the unintended consequence of inducing banks to lend more in foreign currency and less in local currency to domestic firms. I exploit heterogeneity in the strictness of capital controls across Peruvian banks to provide novel evidence of the effect of capital controls on banks foreign currency lending. Using a unique dataset that includes all foreign exchange transactions and loans given by Peruvian banks, I find that capital controls encourage banks to make more foreign currency loans to domestic firms. I describe a new mechanism to explain these findings, in which capital controls induce local banks to shift exchange rate exposure away from foreigners and onto domestic firms. This is worrisome as the literature shows that depreciation shocks have led to significant reductions in investment and employment for these firms.

Key words: capital controls, macroprudential policies, carry trade, currency risk

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I. Introduction

During the past decade, firms outside the US have quadrupled their US dollar debt. For instance, dollar denominated debt for firms outside the US has reached 9.8 trillion dollars, of which more than 30% is held by firms in emerging markets alone (McCauley et al., 2015). The currency mismatch caused by having debt in dollars but revenues in local currency severely exposes firms to depreciation shocks of the local currency, leading to significant reductions in profits, investment and employment after a depreciation shock.[1]

An important source of depreciation shocks are sudden reversals of capital flows. Since the 2008 financial crisis, there has been greater concern about reversals of capital flows because the low dollar rates have spurred inflows to emerging markets. These inflows aim at earning the interest rate differential between the emerging market currency and the dollar (a strategy called carry trade). As these flows are speculative short-term flows, they can easily revert. To prevent such sudden outflows, economists widely recommend the use of capital controls on carry trade inflows.[2] As a result, an increasing number of countries are using capital controls on inflows to smooth flows across time.

However, despite the concern about dollarization of firms' debt and the wide use of capital controls to mitigate exchange rate risk of these firms, very little consideration has been given to the effect of capital controls on foreign currency borrowing of firms. In this paper I use the implementation of capital controls in Peru to provide causal evidence that capital controls can have the unintended consequence of inducing banks to lend more in foreign currency to domestic firms. To the best of my knowledge, this is the first paper to show that capital controls on inflows can induce banks to lend more in dollars. This is important because part of the reason to set capital controls to begin with is to mitigate the losses the economy faces due to

[1] See Carranza et al. (2003), Echeverry et al. (2003), Pratap et al. (2003), Aguiar (2005), Cowan et al. (2005), Gilchrist and Sim (2007), Hardy (2018), Verner and Gyongyosi (2018) for evidence on the damage caused to the financial capacity of these firms after a depreciation shock.

[2] See Mendoza (2010), Ostry et al. (2010),Farhi and Werning (2013), Rey (2013), Brunnermeier and Sannikov (2015). Many countries followed this advice, including Brazil, Indonesia, Peru, South Korea and Thailand. One example of this consensus is that, in fact, even the International Monetary Fund (IMF) changed its stance on capital controls and as of 2012, has supported their imposition. For more examples, see the letter that more than 200 economists sent to US Officials asking them to remove penalties to countries setting capital controls in trade agreements. See www.ase.tufts.edu/gdae/policy_research/CapCtrlsLetter.pdf

currency mismatches. If capital controls are increasing the exposure firms have to the exchange rate, central banks should take into account for this side effect to assess correctly the potential benefits of introducing capital controls on inflows.

Why could capital controls on inflows induce local banks to lend more in foreign currency to local firms? Given that these capital controls have been set mostly in emerging markets in the past decade, I center the analysis in an emerging market context. In various emerging markets, banks have two main features. First, banks have a large share of their deposits in foreign currency (Catão and Terrones, 2016). Second, because of regulation, banks need to match their foreign currency liabilities with foreign currency assets (Canta et al., 2006). That is, if banks borrow more in foreign currency, such as in dollars, they need to lend in dollars or find another dollar asset to match their dollar liabilities. When capital controls are absent, banks can match their dollar liabilities with dollar assets by buying dollars using forward contracts with foreign investors. However, when capital controls are in place, capital controls limit these kinds of transactions between banks and foreign investors. As banks need to match the two sides of the balance sheet, they could respond to capital controls by increasing their foreign currency lending to local firms.[3]

Peru offers a great laboratory to test whether banks respond to capital controls by lending more in dollars and lending less in domestic currency. In the aftermath of the 2008 financial crisis, Peru, as many other developing countries, imposed capital controls to cope with carry trade inflows. For capital controls on carry trade inflows to work, they need to block the two channels though which foreign investors engage in carry trade. Foreign investors engage in carry trade by either buying domestic short-term bonds or acquiring domestic currency forward securities (and selling dollars). Following this rationale, capital controls in Peru

[3] In partially dollarized economies, there is a stock of dollars in the economy, in addition to capital flows, that needs to be hedged. When banks can hedge their foreign currency liabilities by buying dollars in the forward market, there is no reason for domestic banks to be more sensitive to sudden stops and exchange rate movements as banks can hedge both the flow and the stock of dollars. Therefore, there would be no need for banks to transfer the exchange rate risk to firms given that the exchange rate exposure of banks would already be hedged. Although closing capital markets can also reduce dollar inflows, the economy still has a stock of dollars that needs to be hedged.

consisted of (1) preventing foreign investors from buying short-term securities and (2) setting limits on holdings of forward contracts.[4] [5]

The way the limits on forward contracts were implemented provides an identification strategy for my empirical work. Because foreign investors took forward positions against local banks, limits were imposed on local banks' forward holdings. Given that each bank had a different percentage utilization of this limit at the time these caps were announced, capital controls were not binding for all banks. Only a fraction of banks were forced to reduce their forward holdings because their holding positions were above the cap. The rest were less affected by the cap and could even increase their forward holdings. I use the banks above the cap as treated banks and the rest as the control group. Exploiting the variation in the use of forward limits across banks, I identify the effect of capital controls on banks' lending pattern of soles (the Peruvian currency) and dollars. I use difference-in-differences to compare differences in treated banks' lending to that of banks in the control group before and after the implementation of forward limits. To isolate local banks' credit supply from firms' credit demand, I compare how the two groups of banks change lending to the *same* firm.

My identification strategy rests on the credibility of the following three assumptions. First, banks should not anticipate the imposition of capital controls. I verify this assumption by showing that banks' strategies before capital controls announcement were *opposite* from those they would have followed if they knew capital controls were going to be imposed. Hence, it seems banks did not know capital controls were going to be announced.[6] Second, banks in the control group should be a valid counterfactual for those

[4]Examples of countries that set restrictions on the currencies forward market are Brazil, Colombia and Korea. Malaysia also did so in 1994. Between 2006-2008, Thailand set reserve requirements on currencies sold against baht.

[5]Foreigners did the carry trade by selling dollars and buying soles (the Peruvian currency) forward to local banks. This means that at maturity the local banks would hand in soles in exchange for dollars. In practice, these two flows are netted and only the profits are settled. In this sense, there are almost no physical flows. However, these transactions have similar implications than actual inflows of soles in terms of interest rates and exchange rates. This is because given that local banks are the counterparties of the foreigners, and given that local banks need to hedge exchange rate risk, when banks buy dollars forward from foreign investors (absent of capital controls), banks need to unwind these transactions. They often do this is in spot market by selling dollars (buying soles) in the spot market and investing in short-term securities. Hence, the foreign exchange appreciates and there is downward pressure to soles interest rates. In the case of a reversal of these "inflows", the opposite occurs.

[6]To prevent a fire sale, if banks anticipate the imposition of capital controls, banks should decrease their holdings of forward contracts smoothly before the imposition of capital controls. However, banks kept increasing their forward holdings previous to the announcement of capital controls.

in the treatment group.I perform various checks that suggest this condition is valid. These checks include testing balance on observables (including market shares in the foreign exchange (FX) and credit market) and pre-trends as well as exploring possible explanations for pre-existing dispersion of forward holdings. Third, capital controls should be exogenous to prevent my results from capturing the factors leading to the imposition of capital controls rather than to capital controls themselves. Although capital controls in Peru were an endogenous response to carry trade inflows, my identification strategy is still valid as long as the underlying factors that led Peru to set capital controls affect all banks and firms in the same way.

To implement my identification strategy, I rely on unique, confidential data provided by the Peruvian bank regulator, the Superintendence of Banks and Insurance Companies (SBS). My dataset includes the universe of forward contracts of all Peruvian banks, which I use to compute forward holdings and determine whether a bank is in the treated or control group. To determine the effects on bank lending, the Peruvian bank regulator also provided loan-level data of all commercial lending activities of banks. Hence, I observe all loans banks lend to firms across time.

Using this dataset, I find evidence supporting the key contribution of the paper, which is to show that capital controls induce local banks to lend more in dollars and less in soles. In particular, I find that treated banks lent 10% more in dollars and 20% less in soles during the year following capital controls.

However, as firms borrow from both groups of banks, these estimates are silent about the effect of capital controls on firms' overall currency composition of debt.[7] Then, to assess whether the greater dollar lending of treated banks could have contributed to an overall increase in dollarization of its borrowers, I estimate the effect that firms' dependence on treated banks has on firms' debt dollarization. For this estimation, I aggregate loans in dollars and soles at the firm level and use the share of debt with treated banks to proxy for the dependence on treated banks. With this proxy for dependence on treated banks, I generated two groups

[7] For example, as firms borrow from treated and control banks, the results showing that treated banks increased their dollar lending compared to banks in the control group is compatible with a story in which firms are borrowing in dollars from a treated bank and using these dollars to repay a debt with banks in the control group. In this case, the firms' net dollar borrowing would be unchanged.

of firms: those whose share of debt with treated banks is above/ below the median. I used propensity score matching to make firms in the two groups as similar as possible before the introduction of capital controls.[8]

The greater dollar borrowing of firms does not lead to greater exchange rate risk when the firms that are borrowing in dollars also have revenues in dollars. As revenues in dollars typically come from exports, I collected all exports done by all firms in Peru from the tax collection agency (SUNAT). I find that firms increasing their debt dollarization are not exporters. Moreover, given that I observe the universe of forward and swap contracts of all banks and all firms in Peru, I show that not only are the firms borrowing in dollars non-exporters, they are also not using hedging instruments to hedge their greater exchange rate risk derived from greater dollar liabilities. Therefore, treated banks responded to the imposition of capital controls by shifting dollar liabilities to firms without dollar assets and hence, these results suggest banks increased exchange rate risk for these firms.

These results suggest that, indirectly, banks chose to increase their exposure to the exchange rate by lending dollars to firms that might not repay if the domestic currency depreciates. This result is non-obvious. Although capital controls limit the amount banks can hedge dollar liabilities with forward contracts, banks could still comply with the regulation that forces them to hedge by either reducing dollar liabilities or increasing dollar assets in the US (buying bonds of US firms or US Treasury bonds). However, I show that the Peruvian banks did not decrease its dollar liabilities. Moreover, they did not increase their hedging with US investments. The only adjustment banks made after the imposition of capital controls was to increase their dollar lending to domestic firms and households.

The conclusions of this paper are likely to apply more broadly, including developed economies. First, the setting in countries outside the US is very similar to that of Peru. Like banks in Peru, banks in various developed and developing countries hold more than 20% of their liabilities in foreign currency.[9] To mitigate

[8] I used propensity score matching to mitigate concerns about selection between firms that borrow mostly from treated banks and those that borrow less from treated banks. However, given that I do not have firms' balance sheet information, I matched mostly borrowing patterns before the imposition of capital controls. The data limitation at the firm level stresses that the main contribution of the paper is to show the effect of capital controls on bank lending rather than the results at the firm level.

[9] See BIS locational banking data

risks on the banking system, regulators usually force banks to match, to some extent, these foreign currency liabilities with foreign currency assets (Canta et al., 2006). Given that capital controls on inflows imposed broadly in emerging economies in the last decade are similar to those imposed in Peru in the sense that they limit risk sharing between local banks and foreign investors, capital controls can have the negative consequence of inducing banks to lend more in foreign currency to domestic firms. These results could also be extrapolated to developed economies as these economies have also been imposing macro prudential policies that may have unintended consequences of increasing exchange rate risk to firms (such as those shown in Ahnert et al. (2018)).

I.A. Related Literature

The main contribution of this paper is to the literature in international macroeconomics that aims at understanding the effects of capital controls. Although the economic discussions on capital controls go back to World War I, when they were first incorporated, to the best of my knowledge, this is the first paper showing capital controls can increase the country's exposure to the exchange rate by inducing domestic banks to lend more in foreign currency to domestic firms. This highlights the benefits from capital market deregulation and global risk sharing that have been studied by Chari and Henry (2004), Gourinchas et al. (2010) and Maggiori (2017). However, I am not suggesting capital controls should not be set, as the literature has shown there are very valid reasons to introduce them. My contribution is to show a new (negative) side effect that needs to be balanced with the additional benefits and costs that have been studied in the literature.

The general view on the benefits and costs of capital controls has changed over time. For nearly 50 years economists seemed to agree capital controls prevented an efficient allocation of resources (Neely, 1999).[10] However, since the 2008 financial crisis, economists have been advocating for the introduction of capital controls on both inflows and outflows with the purpose of smoothing flows across time. The reason is that

[10] Between World War I until the collapse of Bretton Woods, it was thought that capital controls could help countries restore stability and growth after a recession by using capital controls to prevent outflows. However, since the collapse of Bretton Woods and until recently, the consensus was that capital controls prevented an efficient allocation of resources.

capital controls can allow central banks to have independent monetary policy,[11] mitigate financial instability, [12] and decrease overborrowing.[13] Some papers, on the other hand, have also shown that capital controls can have limited stabilization effects[14], can be hard to implement and can increase the cost of funding for firms[15].

There is another set of literature that suggests that the cost highlighted in this paper could be an important one. Although I have limited data on the firm side to assess the cost after a depreciation shock on real outcomes, Verner and Gyongyosi (2018) shows that a depreciation shock of 30%[16] decreases disposable income of households borrowing in foreign currency, which ends up decreasing aggregate demand and hence increasing unemployment by 0.4 to 0.9 percentage points. In terms of investment, Aguiar (2005) shows that exposure of firms to foreign currency debt reduced investment rates from positive 1% to negative 3.6% in Mexico in 1995 after a significant devaluation of the local currency. In sum, many economists have stressed the importance of reducing foreign currency borrowing of firms as foreign currency borrowing significantly magnifies crises (Mishkin (1999), Krugman (1999)).

I.B. Outline of the paper

In Section II, I present background information on capital controls and outline a possible channel through which capital controls can affect banks' lending decisions. Starting from Section III, I turn to test the channel outlined in Section II. I start by presenting the data in Section III. Then, in Section IV I discuss the identification strategy and show supporting evidence for the paper's key contribution. This is, I show that banks affected by the capital controls (treated banks) responded to capital controls by lending more

[11] See Shambaugh (2004), Rey (2013), Davis and Presno (2017), Amador et al. (2016)

[12] See Tobin (1978), Ostry et al. (2012), Farhi and Werning (2013), Korinek and Sandri (2016)

[13] See Jeanne and Korinek (2010b), Jeanne and Korinek (2010a), Mendoza (2010), Bianchi (2011), Schmitt-Grohe and Uribe (2012), Brunnermeier and Sannikov (2015)

[14] For instance, Forbes et al. (2015) show that capital regulation has a limited effect on exchange rates, capital flows and macroeconomic volatility, while Edwards (1999) shows that controls on inflows are not very effective in achieving monetary policy independence.

[15] See Forbes (2005), Desai et al. (2006), Forbes (2007), Alfaro et al. (2017)

[16] This magnitude is close to the one experienced in Peru between 2013 and 2015

in dollars and less in domestic currency, regardless of whether the firm has dollar revenues (exporter) or hedges dollar liabilities using financial instruments. Section V complements the findings in Section IV by evaluating the impact of capital controls on firms. I show suggestive evidence that the greater dollar lending from treated banks increased firms' exposure to the exchange rate in the short run. This is problematic for these firms as a large body of work has documented that these firms substantially suffer after a depreciation shock. Finally, in Section VI I show that Peru and other emerging economies share similar characteristics. Then, although this paper is grounded in Peru, the institutional setting of other emerging markets suggests that the conclusions of this paper could apply to more economies. Section VII concludes.

II. Background Information on Capital Controls and Lending Market in Peru

As I study the effect of capital controls on carry trade inflows on bank lending using Peru as laboratory, this section explains the institutional background that affects the main two variables of this paper: (1) capital controls and (2) bank lending in Peru. Nevertheless, to understand Peru's capital controls regulation, it is important to first review the conditions under which, in general, capital controls on carry trade inflows work. I do this next.

II.A. Capital Controls on Carry Trade Inflows

When there are carry trade inflows, investors want to earn the interest rate differential between the dollar and the emerging market currency. There are two alternative ways in which foreign investors engage in carry trade. The first alternative consists of borrowing dollars, buying emerging market currency (the domestic currency) with these dollars in the spot market and buying domestic currency short-term bonds. I refer to this channel as the bond channel. The second alternative consists of using forward contracts. Foreign investors get an asset in domestic currency and liability in dollars by buying domestic currency against dollars using forward contracts. I refer to this channel as the forward channel.

For capital controls to be effective in reducing currency volatility and allowing the central bank to have greater control over its monetary policy, capital controls must block the two channels through which carry trade is done.

Without restrictions on the bond channel, the currency can face larger volatility because the domestic currency appreciates when foreign investors purchase domestic currency bonds but can suddenly revert when foreign investors decide to do the carry trade somewhere else and hence sell the domestic currency bonds and purchase dollars. In this situation, the Central Bank's ability to control the interest rate is also capped because when the Central Bank wants to keep local interest rates high to control inflation, it also attracts carry trade inflows that set downward pressure to interest rates.

The same occurs when there are no restrictions on the forward channel. This happens because the main counterparties of the foreign investors are domestic banks as they are the market makers. As the bond channel is only blocked for foreign investors, domestic banks can do the transactions involved in the bond channel. For instance, during carry trade inflows, foreign investors want to buy the domestic currency forward and sell dollars. Then, domestic banks will sell the domestic currency forward (buy dollars forward) but unwind this transaction using the spot market. That is, the domestic banks purchase the domestic currency in the spot market and purchase domestic currency assets, such as lending in domestic currency to domestic firms or buying short-term bonds.[17] This makes the the domestic currency appreciate and sets downward pressure to interest rates. Then, when foreign investors unwind their positions, the domestic banks can also unwind these positions and create currency volatility and make monetary policy harder to control.[18] Given this, the carry trade transactions with forward contracts are still considered as "inflows" of soles, although in practice, the dollar and soles net at maturity and only the profits are settled.

[17] In this paper I show evidence that before the imposition of capital controls, banks used their purchases of dollar forward to hedge their dollar liabilities and therefore convert their dollar liabilities (households' deposits) to lend in domestic currency to firms.

[18] It is very likely that domestic banks will unwind the positions taken in the forward market. In emerging markets this is almost automatic because banks need to hedge their currency exposure. Hence, if they take positions in the forward market, they need to unwind them.

II.B. Implementation of Capital Controls on Carry Trade Inflows in Peru

Consistent with Section II.A, capital controls in Peru restricted each of the channels through which carry trade could be done. For this, Peru set restrictions on carry trade flows in two stages.

In the first stage, Peru blocked the bond channel between January and April 2008 using two regulations. The first prevented foreign investors from buying short-term bonds by setting high fees (4% over notional) whenever foreign investors bought the Central Bank's certificates of deposit.[19] The second prevented domestic banks from acting as intermediaries for foreign investors by setting 40% (later on raised to 120%) reserve requirements when banks received short-term funds (less than two years) from foreign investors. Absent of these high reserve requirements, local banks could act as intermediaries of foreign investors by obtaining dollar deposits from foreign investors and buying the Central Bank's certificates of deposit for them. Hence, the combination of reserve requirements and fees over purchases of the Central Bank's certificates of deposit effectively blocked the bond channel of carry trade.

In the second stage, having effectively blocked the bond channel, Peru then limited the forward channel because foreign investors could trade forward contracts with local banks to obtain carry trade payoffs. To block the forward channel, in January 2011,[20] Peru set limits on the local banks' holdings of forward contracts.

These forward limits were different across banks and were computed as the maximum between 40% of equity and 400 million soles (equivalent to 144 million dollars in January 2011).[21] The announcement of

[19] These securities were the most common fixed income security foreign investors used for the carry trade because they are the safest Peruvian short-term fixed income securities in soles.

[20] The reason not to impose restrictions on forward contracts in 2008, when restrictions on foreign investors certificates of deposit purchases were announced, was that soon after this regulation, the start of the financial crisis led to outflows in emerging markets. It was only after mid 2010, when inflows to emerging markets resumed, that the Central Bank saw the imposition on local banks' forward holdings necessary.

[21] To prevent banks that are not using their forward limit to intermediate flows to banks that are above their forward limit, the limit is computed as the total dollars forward holdings a bank has with respect to *all* counterparties (including other banks). As the forward limit applies to local banks' forward holdings, regardless of the banks' counterparties' residency, the definition of capital controls is not the same as in Ostry et al. (2012) ("measures that treat transactions between residents and non-residents less favorably than among residents"). However, by arguing there have been many interpretations for capital liberalization, Brockmeijer et al. (2012) suggest that there have been many interpretations for capital controls. For instance, in the broadest sense, capital controls are taken as any limitation on capital flows. In this sense, Brockmeijer et al. (2012) mention that the European Union does not take into account whether capital controls discriminate based on residency. Therefore, my definition of capital controls

these controls occurred on January 24th 2011, but banks had until April 2011 to adjust their holdings below the threshold. Thus, banks that were surpassing their limit as of this date had until April to unwind the necessary trades to achieve net forward positions that were within the regulatory bounds. The heterogeneity in how binding these forward limits were for different banks just before the announcement of forward limits, shown in Figure 1, allows me to identify the effects of capital controls on bank's lending behavior.[22]

II.C. Banking System in Peru: Deposits, Loans and Foreign Exchange Hedging

As I study how capital controls affect banks' incentives to lend dollars and soles in Peru, this subsection describes briefly the main characteristics of Peru's banking system. However, when discussing external validity in Section VI, I show that Peru's banking system is very similar to that of other emerging markets.

In Peru, banks receive deposits and provide loans in both dollars and soles. Figure 2 plots the time series of Peruvian banks' share of dollar loans (loan dollarization) and share of dollar deposits (deposit dollarization) between 2000 and 2014. Deposit and loan dollarization were both above 70% in the early 2000s as the economy had experienced persistent high inflation until late 1990s (Contreras et al., 2017). The drastic monetary and fiscal reforms applied in 1990s halted hyperinflation, and since the Central Bank implemented inflation targeting in 2002, it has successfully kept annual inflation below 3%. As a result, deposit and loan dollarization has been falling since then (Contreras et al., 2017).

Although the ratio of dollar loans and the ratio of dollar deposits have been decreasing consistently for more than a decade, these ratios still remain above 30%. In particular, the years before the imposition of capital controls, dollarization of deposits and bank loans were 30 and 40%, respectively. Interestingly, the downward trend in loan dollarization not only halted at the time capital controls were imposed but grew 6%

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is consistent with this broader definition. Moreover, this broader definition should not be much different from Ostry et al. (2012) given that approximately 40% of the local banks' volume in the forward market is done with foreign investors. However, forward limits need a broader definition that did not distinguish between counterparties' residency to make these limits effective and hence prevent less constrained banks from intermediating forward holdings for constrained banks.

[22] To preserve confidentiality, I use a kernel density plot rather than showing the actual percentage utilization of forward limits for each bank.

the year after the imposition of capital controls. This is an important difference compared to the average growth rate of -6% that the share of dollar loans had experienced since the 2000s. In fact, this growth rate has been the highest growth rate in more than 20 years. In this context, I study how capital controls *increase* dollarization of firms' debt.

Deposit dollarization poses risks to banks' balance sheets. Therefore, by regulation, banks need to hedge exchange rate risk by matching their dollar liabilities (such as dollar deposits) with dollar assets. The Peruvian bank regulator sets foreign exchange hedging requirements in the form of explicit limits to banks' exchange rate exposure, as well as capital requirements for mismatches (Canta et al., 2006). These hedging requirements have been in place since 1999, and as shown in Figure 3, local banks barely have any currency mismatch. The gray line shows the net dollar liabilities of banks' balance sheets (before forward contracts) as a percentage of equity. The red line shows the net total assets obtained from forward contracts as a percentage of equity. As the net dollar assets from forward contracts matches almost perfectly the net dollar liabilities arising from the banks' balance sheets, banks are nearly completely hedged against exchange rate movements.

II.D. How Can Capital Controls Affect Banks' Balance Sheets?

This subsection builds on the setting described in subsection II.C, where banks in emerging economies have a significant share of their deposits in dollars but they have to match these dollar liabilities with dollar assets. This subsection studies how, in this context, capital controls affect local banks' balance sheets.

The regulation that banks need to match their dollar assets with dollar liabilities[23] means that capital controls will force banks above the limit (at the time capital controls are announced) to decrease their forward holdings to be within the limits when capital controls come into effect. As these banks still need to match their dollar liabilities with dollar assets, they need to either decrease their dollar liabilities or increase other

[23] Technically, the regulation also adds off-balance sheet forwards and swaps. Then, for illustration purposes, I take purchases of dollar forward as dollar assets and sales of dollar forward as dollar liabilities.

dollar assets. Figure 4 shows that banks did not decrease their dollar liabilities. In fact, this figure, which shows the normalized sum of all banks' dollar liabilities of banks across time, shows that dollar liabilities remained almost constant the year after the capital controls came into effect.

Then, if dollar liabilities are not decreasing, but banks have to reduce their share of dollars forward, then banks are increasing other types of dollar assets. Which dollar assets did banks acquire after capital controls were announced? Banks have many options to increase dollar assets. The only asset they could not increase beyond the limit imposed by capital controls was forward contracts. Then banks could purchase US Treasury bonds, US corporate bonds, deposit dollars in the Central Bank or lend dollars to domestic firms. However, I find that Peruvian banks opted to match dollar liabilities with dollar assets by mainly increasing lending of dollars. This is shown in Table I. This table shows the dollar assets as percentage of dollar liabilities of the aggregate Peruvian banking system in December 2010 (pre capital controls) and December 2011 (post capital controls). It shows that while in December 2010 6% of their dollar liabilities were hedged with purchases of dollars forward, by December 2011 almost none of their dollar liabilities were hedged with forward contracts. Instead, in 2011, banks increased by 6% the share of their domestic dollar loans as percentage of their dollar liabilities. Moreover, none of the other types of dollar assets seemed to change much after the imposition of capital controls. Then, it seems that banks substituted hedging by buying dollars forward with hedging by lending dollars.

In Peru, lending dollars and buying dollars with forward contracts are two actions that seem to have behaved as substitutes over time. This is seen in Figure 5. This figure plots the normalized share of dollar liabilities that is hedged by lending dollars (red line) and the normalized share of dollar liabilities that is hedged by taking positions in the derivatives market (dotted gray line). For comparison, I normalized the series as of the capital controls announcement. The plot shows the negative co-movement between dollar loans and long dollar derivatives positions across time. In particular, as the long dollar derivatives positions decreased after the announcement of capital controls, dollar lending as a share of total dollar liabilities increased by nearly 10% in the year following capital controls.

Substituting away from hedging dollar liabilities with forward contracts also affects the soles side of the balance sheet. This is a characteristic that is nested when hedging with forward contracts but that is not nested when hedging with other forms of dollar assets. This occurs because when a bank hedges by purchasing dollars forward, the bank is agreeing to receive dollars in the future in exchange for soles in the future and hence generating a liability in soles. As, by regulation, dollar assets need to be matched with dollar liabilities, then soles assets are also matched with soles liabilities. Then, everything else equal, as capital controls induce banks to decrease their purchases of dollar forwards, and with that their soles liabilities, capital controls could induce banks to decrease soles lending because banks have to decrease their soles assets. To do this, banks can reduce dollar interest rates and increase soles interest rates. Unfortunately I do not have data on interest rates but the results in section IV are consistent with this mechanism.

III. Peruvian Data and Summary Statistics

From this section onwards, the focus of the paper will be to study whether the predictions of my theoretical argument hold. For this purpose, I combine Peruvian data on bank loans, forward contracts and firms' exports.

Credit Register: The credit register collected by the Superintendence of Banks and Insurance Companies (SBS), the Peruvian Bank Regulator, constitutes the main dataset I use to evaluate the impact of capital controls on banks' lending behavior. The sample period goes from January 2010 to December 2012 and is recorded at the firm-bank-month level. This confidential dataset contains the monthly balances of all commercial loans outstanding in dollars and soles made by the universe of the Peruvian financial system.

Panel A of Table II shows the summary statistics at the bank level for the change in soles loans, dollar loans, and total loans between December 2010 and May 2011. This constitutes the period between one month before the regulation was announced and one month after the regulation became effective. These statistics show that the average soles and dollar bank loan balance increased by 10 and 11%, respectively, with a large dispersion across banks. These banks comprise the full sample of the thirteen commercial and non-government owned banks operating in Peru. [24]

Unfortunately the number of banks is Peru is small. Moreover, it is a concentrated market. Hence, the power in the results from Section IV will come from having many firms having relationship with few banks. This problem is present in most papers related to banking that yield results based on bank-firm observations because, as shown in Figure A.1 in the Appendix, the banking system across the world is a highly concentrated market.

Panel B of Table II contains the summary statistics of Panel A, but collapsed at the firm level. The discrepancy between Panel A (which aggregates at the bank level) and Panel B shows the high heterogeneity in the credit behavior of the almost 14,000 firms in my sample. Although the SBS collects this information for all firms, because of regulatory constraints, the SBS could only hand in data for firms classified as "medium," "large," and "corporate" according to the SBS size classification.[25] For simplicity, I refer to the medium firms as "small," the large as "medium," and corporate firms as "large firms".

Finally, Panel C of Table II shows the summary statistics collapsed at the bank-firm level. An important aspect of Panel C of Table II is that it shows that the average number of bank relationships that firms have is 2.4 banks per firm. In fact, more than 70% of the firms in my sample have more than one bank relationship. As will be discussed in Section IV.B, this will help to isolate demand for credit from supply of credit (Khwaja and Mian, 2008).

[24] I drop government-owned banks, Agrobanco and Banco de la Nacion, from the sample. I also drop Deutsche Bank Peru because it did not have commercial banking. I only take financial institutions classified as banks (13 out of 60) as these institutions have the same regulations. For details on the differences between banks and other financial institutions, see the SBS law 26702.

[25] The medium firms are those that have had a total debt balance with the financial system greater than 300,000 soles (approximately 92,000 dollars) but have annual sales below 20 million soles (approximately 6.1 million dollars). The large firms are those that have annual sales between 20 and 200 million soles, while the corporate firms are those that have yearly sales above 200 million soles. - See Resolucion SBS 11356-2008.

Banks started reporting this classification in 2010. However, the SBS has reconstructed the firm size for the previous years by using the 2010 definition for each firm. For those firms that ceased to exist, the firm classification for the years before 2010 corresponds to the current definition of size classification. Analysis of the data show that each firm's classification has remained constant across the sample.

Forward Contracts: The forward contracts dataset contains all of the forward contracts outstanding for the universe of banks in Peru. This is a compulsory and confidential report sent on a weekly basis to the SBS. It contains details such as the notional and currency bought, the currency sold, the starting date, the maturity date and counterparty.

Table II, Panel A, shows that on the last reporting date available before the announcement of capital controls (two days before the announcement), banks were using 49% of their forward limit on average, with a standard deviation of 52%.

Bank Information: As will be discussed in Section IV.C, the identification strategy I describe in IV.B is only valid only when the banks in the control group are comparable to those in the treated group. One of the ways I test (and then suggest) that banks in the treated and control group are similar is to compare their observable characteristics. These include deposits, assets, profitability and liquidity. These variables, except for liquidity ratios, have been collected from publicly available balance sheets that are published in the SBS website. The liquidity ratios have been taken from regulatory reports banks submit to the SBS. The SBS defines these ratios as liquid assets over liquid liabilities.[26]

Exporter and Importer Data: The firms that will be affected under a depreciation shock when borrowing in dollars are those that do not have revenues in dollars. As the firms balance sheets are not available for non-public firms, I proxy whether a firm has revenues or additional costs in dollars by using the FOB (Free on Board) value of all exports and imports made by Peruvian firms. This data is collected by the SUNAT.

Additional Datasets: I use market data (such as exchange rates, libor, forward prices, interbank dollar and soles rates) obtained from Bloomberg.

[26]The liquid assets are cash, funds in the central bank and local financial system, interbank lending, central bank and government securities, certificates of deposit of the local banking system and investment-grade bonds. The liquid liabilities are term deposits (up to 360 days), tax liabilities, interbank borrowing, securities issued that expire within 360 days and accounts payable for short selling. The liquid assets and liquid liabilities are specified in the SBS regulation: Resolucion SBS 9075-2012.

IV. Effect of Capital Controls on Banks' Soles and Dollar Lending

Using the data presented in Section III, this section studies whether capital controls induce banks to (1) lend less in domestic currency and (2) lend more in dollars. For this, I first address how an ideal experiment would identify the effect of capital controls on bank lending. I argue that Peru offers a setting close to the ideal one and therefore, proceed to explain the identification strategy, its validity and results. The results I present in this section provide evidence supporting the key contribution of the paper, which is to show that capital controls induce local banks to substitute domestic currency lending for dollar lending.

IV.A. Contrast between Ideal Experiment and Peru's Natural Experiment

Ideally, to estimate the effect that capital controls on bank lending, one would randomly assign capital controls across banks and then compare the outcomes of banks affected by capital controls to those that were not.

The imposition of capital controls on carry trade inflows in Peru resembles this scenario because in Peru, capital controls affected local banks differently. At the time of the capital controls announcement, banks had different percentage utilizations of the imposed forward limit. Those with greater the percentage utilization of the forward limit had greater exposure to capital controls. However, it is likely that the banks that were way below the limit were not constrained. Given that on average the banks that were below the limit were using 26% of the limit while those above the limit were using 123%, in the main analysis I take the banks affected by the capital controls to be those banks that were forced to reduce their forward holdings because they were surpassing the imposed limit at the time capital controls were announced. I refer to these banks as treated banks. In contrast, the rest of banks, the control banks, were below their limit when capital controls were announced and could even increase their forward holdings (up to the regulatory limit). In a robustness analysis I take alternative definitions of whether capital controls bind, which include using the percentage utilization of forward limit itself as capital controls. [27]

There is a difference between the introduction of capital controls in Peru and the environment in which capital controls are randomly assigned across banks. This is that the Peruvian government did not *randomly* allocate capital controls across banks. Then, three conditions are needed for the identification strategy that uses the Peruvian setup to yield similar results than those that would have arisen if banks had been randomly assigned capital controls. These conditions require that in the Peruvian setup, (1) banks do not anticipate the imposition of capital controls, (2) banks in the control group are a valid counterfactual for those in the treatment group and (3) capital controls are exogenous. Section IV.C discusses these conditions and shows evidence that Peru offers a setup close to the ideal experiment.

IV.B. Methodology

Having the imposition of capital controls in Peru as a laboratory, I identify the effect of capital controls on bank lending by using difference-in-differences and comparing the changes in lending between treated and control banks after the imposition of capital controls. For this, I construct the percentage of the forward holdings limit utilization on the last reporting period before the announcement of capital controls[28] as a proxy for the intensity of capital controls treatment. Then, I define treated banks as those that were forced to reduce their forward holdings because they were using more than 100% of their limit.[29] The rest of banks are in the control group. Using these two groups of banks, I estimate the following regression specification:

[27] Ideally, banks in the control group would be fully unconstrained. However, in this case banks in the control group are not fully unconstrained as they are still capped in the amount of holdings they can have. In terms of the regression analysis, this would make it harder to find an effect of capital controls on credit because the treatment group is being compared to a pseudo treated group. Hence, this makes the findings to be a lower bound of the effect of capital controls on bank lending.

[28] The last reporting date before capital controls announcement was January 22, 2011; capital controls were announced on January 24, 2011.

[29] In other words, banks in the treated group are those where: $\left(\frac{\text{Net long dollar fwds}_{b,\text{Jan 22nd 2011}}}{\text{Regulatory limit}_{b,Jan2011}}\right) > 100\%$. Else, banks are in the control group.

$$
\begin{aligned}
y_{bft} =&\beta_0 + \beta_1 CC_b + \beta_2 \text{Post CC}_t + \beta_3 CC_b * \text{Post CC}_t + \text{Bank FE} + \Psi X_{bf} \\
&+ \text{Firm} * \text{Date FE} + \upsilon_{bft}
\end{aligned}
\tag{1}
$$

where y is the outcome of bank b, firm f and month t. The outcome variables $y_{b,f,t}$ are: (1) The percentage of dollar credit with respect to the total credit (2) The log(Credit in dollars + 1) and (3) The log(Credit in soles + 1). In terms of the regressors, CC_b captures a bank's treatment status (equal to 1 for treated banks and 0 for the control group) and Post CC_t is a dummy variable that takes the value of 1 after capital controls' announcement and 0 before. I also include bank fixed effects, bank-firm relationship controls ($X_{b,f}$) and firm$\times$date fixed effects.

I include firm$\times$ date fixed effects (Khwaja and Mian, 2008), where date is recorded at a monthly frequency. I do so because I am interested in capturing the effect of capital controls on banks' lending behavior, that is, on banks' supply of loans, and thus I need to disentangle banks' supply of loans from firms' demand for them. Adding firm$\times$ date fixed effects absorbs possible demand for loans firms have and allows the main coefficient of interest, β_3 to capture the firm-level differences in treated banks' outcome variable y compared to that of the control group after the implementation of capital controls. Then, in essence, adding firm$\times$date fixed effects constrains the sample of Equation (1) to include only firms that borrow from banks in the treated and control group and allows β_3 to compare how treated and control banks change lending to the *same* firm.[30]

The assumption when introducing firm *times* date fixed effects is that when firms demand dollar loans, they will ask for quotes from all the banks with which they have a relationship and take the loan with the lowest rate. However, if the firm only asks for a quote from only one of the banks with which it has a relationship, β_3 could reflect the preference of a firm to deal with a particular bank, say the treated bank, and

[30] As of the remaining coefficients, β_0 subsumes economic conditions that affect untreated banks before the imposition of capital controls. β_1 is the difference in the average between the outcome variable of treated banks versus that of the control group before capital controls. β_2 captures the outcome variable after the imposition of capital controls for the control banks in contrast to the pre capital controls period.

not the treated bank's credit supply. To control for a possible preference of a firm to deal with a particular bank, I include bank-firm relationship controls, $X_{b,f}$. These controls absorb specific bank-firm (observable) relationship factors. These controls are composed of the length of the relationship[31] between a bank and a firm as well as the percentage of credit that a firm was receiving from a bank as of December 2010.

Besides adding bank-firm relationship controls, in Section IV.E I show that treated banks were not better than banks in the control group in providing dollar loans nor in trading foreign exchange forwards. This alleviates the concern that firms might prefer to only quote dollar loans with treated banks because treated banks can be more specialized in providing dollar loans. In particular, I show that although the market share of treated banks in the dollar lending market and forward market is slightly higher than the banks in the control group, my results do not change when I reduce the sample of banks to only the main four banks (where in this case the two banks in the control group have higher market share in the dollar lending market and foreign exchange forward contracts).

Hence, the previous methodology allows me to study how capital controls affect credit supply of dollars and soles. However, because I focus the analysis on firms that borrow from both treated and control banks, these results are silent about the effect of capital controls on the total borrowing in dollars and soles of firms. For example, if the results show that on average firms borrowed more dollars from treated banks than control banks after the imposition of capital controls, these firms might have not increased their overall dollar borrowing because they could be borrowing more dollars from treated banks but instead used these dollars to repay pending debts with control banks. Then, Section V aggregates the analysis to the firm level and studies the effect of capital controls on firms' overall exchange rate exposure.

IV.C. Validity

As mentioned in Section IV.A, the identification strategy presented in the previous section only resembles that of randomly assigning capital controls across banks when the following three conditions hold. First,

[31] This is computed as the number of months in which there is non-zero credit balance between a bank and a firm

banks should not anticipate the imposition of capital controls. Second, banks in the control group should be a valid counterfactual for the treatment group. Finally, capital controls should be exogenous. This section discusses these conditions and shows how I cope with potential problems that arise regarding these conditions.

1. Banks should not anticipate the imposition of capital controls: If banks cannot anticipate the introduction of forward limits, then banks' initial forward holdings do not reflect strategic behavior of banks with respect to capital controls. Next I show evidence that suggests banks did not know that this regulation was going to be announced in January 2011.

Consider banks actually anticipated this regulation. When banks anticipate that forward holdings are going to be capped, banks with forward holdings above the anticipated threshold know that if they do not adjust their forward holdings to be within the regulatory bounds, they will be forced to reduce their holdings after the regulation occurs. Therefore, these banks would be subject to a fire sale. Thus, when banks anticipate this regulation, the optimal strategy is to reduce their forward holdings slowly before the regulation takes place. Moreover, even if banks did not know exactly what the limit was going to be, one would expect that banks would be cautious and would not increase more their forward holdings before capital controls are imposed.

However, Figure 6, which plots the normalized forward holdings of the two groups of banks across time, shows that banks were increasing their holdings in the weeks previous to the announcement of capital controls. Hence, it is unlikely that banks knew that this regulation was going to be announced in January 2011. Furthermore, if banks knew that capital controls were going to be imposed and therefore treated banks reduced their forward holdings before the actual imposition of controls, treated banks' forward positions would resemble more those of the control group at the time capital controls were imposed. This makes it harder to find any effect of the imposition of capital controls when comparing the lending behavior of treated with non-treated banks.

2. Banks in the control group should be a valid counterfactual for those in the treatment group: For banks in the treatment and control group to be comparable, I require that the lending growth rate of treated banks would have been the same as that of banks in the control group if capital controls had not been imposed (parallel trends assumption holds). To provide evidence on this, I check (1) balance on observables, (2) pre-trends, (3) possible reasons for the pre-existing dispersion of forward holdings and (4) that my results are not driven by specific matching between firms and banks.

First, banks' balance sheet characteristics are shown in Table III. This table shows that despite the significant differences in the initial percentage use of their forward limit as of December 2010, as well as the change in the percentage of credit in dollars given between December 2010 and May 2011, there are not large differences in the point statistics in terms of market shares (both in FX and credit), profitability, assets, liquidity ratios and share of dollar and soles deposits. The t-statistics are reported for completeness. However, these cannot be taken seriously given the few number of observations.

Nevertheless, the treated group is slightly larger and has greater market share in forwards and credit. Although the differences do not seem to be very large, the concern if, for some reason, firms decide to borrow more in dollars starting in January 2011, the fact that they borrow more from banks in the treated group could be because these banks specialize a bit more in dollar lending. Later on, I mitigate this concern by studying the effect of capital controls when constraining the sample to the largest four banks. [32] The summary statistics for these four banks is reported in Panel B of Table III. Two of these are in the control group and the other two are in the treated group. In this case, contrary to what happens with the whole sample of banks, on average, the banks in the treated group are smaller and have a lower market share in FX forwards and credit than those in the control group. However, as I show in Section IV.E, my results remain unchanged when I only use a sample of the four largest banks.

[32] In this case, the largest four banks (two of which are in the control group and two in the treated group) cover 80% of the commercial credit. This is a common obstacle the literature faces when studying banks as can be seen in Figure A.1 in the Appendix. This figure shows that the assets of the five largest banks as a share of total banking assets around the world are generally greater than 50%.

Second, Figure 7 suggests that banks in the treated and control group had similar trends in the share of dollar lending before the introduction of capital controls. This figure plots the normalized share of dollar lending for banks in the treated and control groups across time. Although both groups of banks had similar ratios of dollar lending before the announcement of capital controls, these ratios diverge post capital controls. While the treated banks had a significant increase in the percentage of dollar loans given to firms, banks in the control group did not increase this ratio after the regulation.

However, the increase in the treated banks' share of dollar lending post capital controls could have been due to specific characteristics of treated banks rather than due to capital controls. For example, treated banks could have received more deposits in dollars or its clients could have demanded more credit in dollars than those in the control group. In this case, the changes in the share of dollar lending shown in Figure 7 cannot be directly attributed to capital controls.

To isolate the effect of capital controls from observable bank characteristics that affect credit supply (bank lending), as well as to disentangle bank credit supply from firm credit demand, I estimate Equation (1) using leads and lags (with respect to December 2010) of the treatment rather than a single pre/post capital controls treatment effect.[33] The regression specification with leads and lags allows me to analyze whether the treatment effect changes across time and whether there were already differences between the treatment and control group that are not accounted in the bank fixed effects nor firm×date fixed effects.

Figure 8 plots the coefficient of the treatment effect across time. Before the announcement of capital controls (coefficients in gray), one cannot reject the null hypothesis that lending trends in the treated and control group were the same. This holds for the percentage of credit in dollars (Panel A), credit in dollars (Panel B) and credit in soles (Panel C). It is only after the introduction of capital controls that these trends diverge.

[33] I estimate: $y_{b,f,t} = \alpha_0 + \alpha_1 CC_b + \beta_t \sum_{\substack{\tau=-12 \\ \neq 0}}^{\tau=12} \mathrm{CC}_b \times \mathbb{1}\left[t=\tau\right] + \text{Bank FE} + \Psi X_{b,f} + \text{Firm FE} * \text{Time FE} + \upsilon_{b,f,t}$

where y is the outcome variable. This regression is similar to Equation (1). However, instead of having a unique coefficient associated with the interaction between CC_b and Post CC, I use a dummy variable for each date (each month-year). Then each β_t is associated with the interaction between how binding were capital controls for a bank (= 1 when the bank was surpassing its limit and 0 otherwise) and a indicator function that takes the value of 1 at t and 0 otherwise. The omitted dummy is December 2010 ($\tau = 0$), which is the date capital controls were announced.

In particular, after capital controls, treated banks increased the share of dollar loans compared to the control group. This increase comes from both an increase in dollar lending and a decrease in soles lending. These results derive from a bank supply channel rather than firm demand of loans, as the analysis centers in comparing bank lending to firms that have a relationship with treated and non-treated banks.

Third, although both groups of banks are similar in terms of balance sheets and parallel trends hold, the two groups of banks were different in terms of the percentage use of their forward limit to begin with. If the reason for banks to have different forward holdings is unobserved and correlates with the change in credit supply behavior post capital controls, then the results in the previous plots could be due to the unobserved factor driving the differences across forward holdings rather than the introduction of capital controls. This would make $\widehat{\beta}_3$ in Equation (1) biased. However, to invalidate the results presented in this paper, one has to explain why the two groups of banks' lending trends were similar in the pre-capital controls period but different starting exactly at the introduction of capital controls.

To mitigate this concern, I analyze whether the forward holdings of banks came from the inelastic demand of counterparties to trade with the same bank they have been doing or whether they seem to reflect the own trading strategies of banks. If the forward holdings of banks are driven from the inelastic demand of the banks' counterparties to trade with the same bank, then the local banks are only intermediaries. Therefore, their initial holdings of forward contracts could be mostly determined by the actions of the counterparties and can be taken as somewhat exogenous to the bank. [34]

To do this analysis, I exploit the fact that the dataset that contains all outstanding forward contracts includes details such as the local bank's name as well as the traded day and counterparty name. Using this dataset, I obtain the results in Table IV. This table shows that before the imposition of capital controls (before 2011) there was a 60-70% probability that a counterparty trades forward contracts with the same local bank as it

[34] Relationship stickiness has been studied in different markets. For example, Chodorow-Reich (2014) shows that a prior lead lender of a borrower in the syndicated market has a 71 percentage point higher probability of being a new lead lender for a new loan.

did in the previous trade.[35] Then, given that local banks use the spot market to unwind forward holdings (as shown in Figure 9), is plausible their forward positions are predetermined by their counterparty relationships and hence, could be uncorrelated with the error term, $\upsilon_{b,f,t}$.

Finally, it is probable that there is not a random sorting between banks' clients and banks. A bank that is more exposed to capital controls could be increasing its dollar lending relative to soles just because its clients are demanding more dollar loans in relation to soles after capital controls were set.

To address this problem and to isolate firm demand for loans from bank lending (credit supply) effects, Equation (1), uses firm $*$ date fixed effects. Then $\widehat{\beta}_3$ captures only how banks with different exposure to capital controls change their lending *to the same firm at a particular month* in the period post capital controls. This is possible given that 70% of the firms in the sample have multiple bank relationships.

Using a specification that includes firm$\times$ date fixed effects, I find that including firm$\times$date fixed effects yields very similar results than when using a specification that does not include firm$\times$ date fixed effects (Table V). This suggests that my results are not driven by a particular sorting between firms and banks.

3. Capital controls should be exogenous: An unobservable factor that could be worrisome is the underlying factor that made the Peruvian government set capital controls to begin with. Capital controls were a response to carry trade inflows. The problem is that the changes in banks' lending could be due to the economic conditions the government was reacting to, rather than capital controls. If these economic conditions are observable, then one can control for these. If they are unobservable, the estimate of the coefficient of interest,

[35] This probability is the result of the following regression, which is similar to Chodorow-Reich (2014), but in the forward contract market:

$$\begin{aligned}\text{Bank Traded}_{b,c,t} =& \rho_0 + \rho_1 \text{Previous Bank Traded}_{b,c,t-1} + \text{Bank FE}_b + \text{Bank FE} \times \text{Month FE}_{b,t} + \\ &+ \text{Bank FE} \times \text{Cpty Type FE}_{b,c} + \upsilon_{b,c,t}\end{aligned}$$

where the regression is at the bank (b), counterparty (c) and trade date(t) level. The dependent variable, Bank Traded is a dummy variable that takes 1 if the counterparty c trades with bank b at trade date t. If not, it is zero. The variable "Previous Bank Traded" is also a dummy variable that takes 1 if the counterparty c traded with bank b the last time it traded forward contracts. Given that a counterparty could trade with a bank because of the bank's market share, I control for bank fixed effects, to remove the overall market share of bank b from the estimate of ρ_1. The interaction of bank fixed effect and month fixed effect controls for whether a bank was particularly active during a certain time window. Finally, because banks can specialize in a particular type of client, such as foreigners, pension funds or firms, I use bank $\times$ counterparty type fixed effects.

$\widehat{\beta}_3$ would be biased. However, as long as these observable and unobservable factors affect all firms and banks in Peru in the same way, $\widehat{\beta}_3$ will be unbiased.[36]

To mitigate these concerns, Equation (1) corrects for observable and unobservable economic characteristics that are common in the pre capital controls period, as well as in the post capital controls period by using a dummy to capture the dates after capital control (Post CC_t coefficient). I also study the effect of capital controls on a very narrow window (January 2010 to December 2011), which reduces the possibility of having additional factors affecting credit that could be confused with capital controls.

IV.D. Results: Effect of capital controls on banks' dollar and soles lending

Having addressed the validity of my identification strategy, this section shows the results. The main take-away is that the estimation of Equation (1) shows that in contrast to control banks, treated banks: (1) increased dollar lending by 10 to 20% and (2) decreased soles loans by 10 to 20% during the year following the imposition of capital controls. I show these results hold for firms of different sizes and are not driven by exporters or firms that use hedging instruments. Moreover, these results are robust to a variety of modifications, discussed in Section IV.E.

Table V presents these results. This table shows the estimates of the effect of capital controls on (1) the share of dollar loans (columns 1-4), (2) dollar loans (columns 5-8), and (3) soles loans (columns 9-12). The first column of each dependent variable shows the simplest version of Equation (1), without bank fixed effects and bank-firm relationship controls, as well as without firm$\times$date fixed effects. The second column of each dependent variable adds bank fixed effects and bank-firm relationship controls. The third column shows the regression controlling for demand effects but without adding any bank fixed effects or bank-firm

[36] If the inflows were specific to the Peruvian economy, they can possibly be correlated with banks in the Peruvian financial system. In this case there could exist unobservable factors that affect banks differently and that are correlated with the imposition of capital controls. This biases $\widehat{\beta}_3$. However, Bloomberg's EM-8 Carry Trade Index (FXCTEM8), which tracks the carry returns of eight developing countries (which do not include Peru), shows that emerging economies saw inflows at the same time Peru did (see Figure 10). This suggests that carry flows into Peru were unrelated to specific Peruvian market conditions and rather driven by global carry returns amid low US interest rates. This reduces the concerns about the correlation between capital controls exposure variable, CC_b, and unobservable market factors. Hence, $\upsilon_{b,f,t}$ should not include market conditions that affect banks and firms in Peru in different ways.

relationship controls, while the fourth column shows the regression that fully controls for bank fixed effects and bank-firm relationship controls as well as properly controls for credit demand effects (which are the main results).

The different specifications show the coefficients and statistical significance of the coefficient of interest (shaded row in Table V) are stable. First, regardless of the specification, Table V shows that the share of dollar loans (columns 1-4) increased for treated banks compared to non-treated banks after capital controls. The difference across specifications is the magnitude, which oscillates between 50 basis points and 150 basis points, having the greatest magnitudes when properly accounting for credit demand. Moreover, the coefficient is statistically significant at 1% in all cases except when not adding any controls or accounting for credit demand effects.

When using dollar loans and soles loans as dependent variable, the coefficient of interest is also stable and statistical significant. Most coefficients show that treated banks increased dollar loans by 8-9% (columns 4-7) compared to non-treated banks after capital controls. The converse is seen when using soles loans as a dependent variable. Treated banks decreased soles lending by 16-22%.

The results of Table V hold for different firm sizes. However, the treated banks substituted more strongly soles for dollar loans to large firms. While the treated banks increased the share of loans in dollars by 260 - 360 basis points more than banks in the control group during the year after the imposition of capital controls, this coefficient was only around 150 basis points for small firms. [37]

[37] This is shown in Table A.I in the Appendix, where I repeat the analysis of Table V splitting the sample by firm size. This increase in the share of dollar loans is explained by both an increase in dollar loans as well as a fall in the soles loans. For the largest firms, while the treated banks increased the share of dollar loans by around 50% more than banks in the control group after capital controls, they reduced soles loans by 25-50% compared to the control group. The majority of these estimates are significant at the 5% level. For smaller firms, treated banks supplied between 12-28% more dollar loans compared to banks in the control group in the capital controls period and decreased soles loans by 15-20% when compared to banks in the control group. The majority of estimates are significant at the 1% level. Given that the small firms outnumber the medium and large firms, it is possible that there is more power when limiting the study to the small firms. There does not seem to be a large nor significant effect on medium firms, though.

Moreover, Table A.II in the Appendix shows that the results are invariant to excluding exporter firms (those who export more than 100,000 dollars per month). Thus, treated banks were lending dollars to firms who did not have revenues in dollars (have no exports) to plausibly hedge greater dollar liabilities.

However, non-exporters could still hedge the exchange rate risk that arises from borrowing dollars by using hedging instruments. I show that my results are not driven by treated banks lending to firms that hedged dollar loans with derivatives contracts. Taking advantage that the forwards dataset contains the tax identifier of the counterparty with which the bank traded forward contracts, I run the main regression specification excluding firms that bought dollars using either forward contracts or cross currency swaps between 2011 and 2013. The assumption is that firms that bought dollars forward or used cross currency swaps to change debt in dollars to soles did so to hedge dollar liabilities rather than for other purposes. Table A.III in the Appendix presents the results and shows that the results from Table V are not due to treated banks lending dollars to firms that would then engage in forward contracts to hedge the exchange rate risk they incur by borrowing dollars from treated banks.

A natural question that follows is why would banks lend dollars to firms whose revenues are in domestic currency and that would face financial constraints in the event of a depreciation shock? [38]

A possibility is that banks assume the government will intervene heavily in the market to prevent a large depreciation of its currency (due to dirty float) or that the government will end up helping firms repay their dollar loans to prevent a deep recession (Burnside et al. (2001), Schneider and Tornell (2004)). If banks consider the government will bail out firms or will prevent a depreciation shock, then banks will lend dollars to firms without worrying about depreciation risks. Therefore, dirty float and implicit government guarantees can contribute to moral hazard and could explain the banks' risk-taking behavior.

[38]Banks could have hedged instead by only buying dollar bonds such as Treasury bonds. Indeed Figure 11 shows that the decrease forward holdings (gray line) after the imposition of capital controls was coupled with an increase in investment in dollar denominated securities. However, banks did not hedge all of their dollar liabilities by buying dollar securities. Instead, banks hedged a significant fraction of dollar liabilities by lending dollars to firms that do not have dollar revenues.

In Peru, there are various reasons for banks to expect the government to intervene heavily to prevent a large depreciation. First, the Peruvian Central Bank has consistently shown concern about the exchange rate. Indeed, the Central Bank publicly states that it will intervene in the exchange rate market to prevent a financial crisis in the event that a depreciation shock significantly increases non-performing loans (Rossini and Quispe, 2014).[39] Second, these exchange rate interventions have been important. In Peru, the Central Bank has been responsible for 15-33% of the average monthly volume traded in the spot exchange rate market when Peru experienced outflows between June 2008 and February 2009.[40] This has made the Peruvian soles much less volatile than the currencies of other emerging economies. Finally, when the Central Bank purchased dollars to slow down the currency appreciation during inflows, it accumulated a large stock of foreign reserves (reaching more than 60 billion - 30% of GDP- in 2012), improving the Central Bank's ability to intervene in the exchange rate market in the event of a large depreciation.

IV.E. Robustness

To further confirm the validity of my results, I perform several robustness checks that are found in the Appendix . I find my results are robust to a variety of different samples and specifications. Next I describe briefly these robustness checks and results.

The first robustness check consists of redoing the previous analysis using the four largest banks in the sample, as these banks are more homogeneous than including all banks. Table A.IV in the Appendix shows that restricting the sample to only the largest four banks yields very similar results to the ones discussed in Section IV.D. This is also the case when dividing the sample by firm size (Table A.V in the Appendix).

The second check consists of conducting placebo tests. I redo the main regression analysis but instead of covering the period from January 2010 to December 2011, it covers from January 2009 to December 2010.

[39] Also see Central Bank of Peru, `www.bcrp.gob.pe/about-the-bcrp/frequently-asked-questions.html`, Tashu (2014).

[40] Figure A.3 displays the Central Bank's exchange rate interventions in the spot market. For spot traded volumes, see end of day summary of open market operations, Central Bank of Peru, `www.bcrp.gob.pe`

This exercise simulates as if capital controls had been announced one year earlier (i.e. January 2010) but keeps constant banks in the treated and control group. The results are in Table A.VI in the Appendix. As capital controls had not been introduced in 2009, my results do not hold in this sample.

The third check consists of studying the effects of capital controls on bank lending when sorting banks based on their intensity of the capital controls treatment. If capital controls are the driver behind my results, then when sorting banks on the intensity of capital controls, I should find that the results I have presented in Section IV.D get stronger as capital controls bind more. Indeed this is the case. [41]

The fourth check I do is to use alternative capital controls definitions. Instead of using a dummy variable that takes 1 for banks with capital controls above their limit, I use as treatment the following two alternatives: (1) the percentage use of forward limit as of January 2011 as a continuous variable and (2) a dummy variable that takes the value of 1 for banks that were above the median percentage use of forward holdings at the time of capital controls announcement. As shown in Table A.VII in the Appendix, my results continue to hold with these specifications.[42]

Finally, I perform tests to check the validity of the standard errors in my regressions. A first concern is that the standard errors could be potentially underestimated because the effects of the treatment on the dependent variable are long lasting. This happens as the data resembles one of repeated observations. A solution is to collapse the time series information into a pre-treatment and post-treatment (Bertrand et al., 2004) so for each element in the cross-section (in this case bank-firm credit) there are only two time observations. When applying this solution, Table A.VIII in the Appendix shows my results remain significant at the 10% level, with most of them remaining significant at the 5% level.

[41] Splitting banks into terciles and comparing the second and third tercile with the first (i.e. those banks in the lowest 33% of the distribution when sorting them by how binding capital controls were on its announcement) shows that banks in the top tercile and second tercile decreased credit in soles by 30% and 15%, respectively, when compared to those banks in the first tercile. As for dollar lending, although there is no significant difference between the share of dollar debt and the increase in dollar credit between banks in the second and first tercile after the imposition of capital controls, banks in the top tercile increased credit in dollars by 8% more than those in the first tercile. These results are plotted in Figure A.2 in the Appendix.

[42] Table A.VII in the Appendix replicates the benchmark regression specification after controlling for firm×date fixed effects and using bank and bank-firm relationships controls (Table V, Column (4)) but using the alternative capital controls definitions.

A second concern regarding standard errors is that due to the small number of date and bank observations, the main regression results are only clustered at the firm level. This accounts for the time series correlation that occurs within firms. However, is very likely to have correlation across firms and banks for the same date. This cross-section correlation across firms and banks can be accounted for by clustering by date. When doing so, Table A.IX in the Appendix shows most coefficients remain statistically significant at the 1% level while the remaining are significant at the 5% level. Similarly, when adding bank clusters to account for the time series correlation that occurs at the bank level, Table A.X in the Appendix shows the results for both percentage of credit in dollars and credit in soles remain statistically significant at the 1% level when accounting for demand effects.

IV.F. Further evidence on the effect of capital controls on bank lending: Interest rates evidence

Having found that my analysis using loan balances is robust to various modifications, in this section I analyze interest rate data to show further evidence of the effect of capital controls on bank lending.

Studying how interest rates behave across banks after the imposition of capital controls is informative on the effect of capital controls on bank lending because for firms to accept borrowing more in dollars from treated banks instead of soles, treated banks need to offer firms loan conditions that make borrowing dollars more attractive than borrowing soles. Thus, the results from the previous subsections suggest that capital controls decrease dollar rates and increase soles rates.

Unfortunately the credit registry does not have information on interest rates banks charge firms. Thus, the supporting evidence of decreases in dollar rates and increases in soles rates is only suggestive.

To understand how capital controls could affect interest rates, I look into correlations between forward holdings and interbank interest rates in dollars and soles. This correlation is informative because it is at the core of the mechanism that makes capital controls induce banks to lend more dollars.

This mechanism is that banks hedge their stock of dollar deposits by generating dollar assets. Without capital controls, buying dollars using forward contracts was a way to do so. As buying forward contracts does not change banks' present cashflow (forward contracts are settled in the future -at maturity-), banks still have dollar deposits that they can use to invest. As these are already hedged, banks need to invest in domestic assets to remain hedged. Banks demand for soles assets should set downward pressure on soles rates. On the contrary, when capital controls are present and they force banks to decrease banks' holdings of forward, banks will need to use dollar deposits to buy dollar assets (instead of soles). This should set downward pressure on dollar rates. Hence, there should be a positive correlation between holdings of dollar forward and dollar rates while negative correlation between forwards and soles rates.

Figure 12 confirms this hypothesis. Panel A shows that there is a positive correlation between Peruvian banks' forward holdings and the spread of Peruvian dollar interbank rate against one month libor.[43] Similarly, Panel B shows that there is a negative correlation between forward holdings and the spread of soles interbank rate against the Peruvian Central Bank's interest rate target. Therefore, after the implementation of capital controls (gray area in Figure 12), which forces banks to decrease their forward holdings, dollar interbank rates decreased while soles rates increased.

V. Effect of Capital Controls on Firms' Exposure to the Exchange Rate

From this section onward, I shift the focus of the paper away from banks and onto firms. This section studies the effect of capital controls on dollarization of firms' debt to understand whether firms increased their exchange rate exposure as a consequence of capital controls.

Studying the effect of capital controls on firms' total debt dollarization does not derive directly from the results on bank lending. As firms borrow from both treated and control banks, firms' changes in debt dollarization need to aggregate their borrowing across treated and control banks. Hence, I aggregate loans at

[43] I plot the spread rather than the level of interest rates because I want to capture movements in interest rates that are not explained by changes in libor or Peru's Central Bank target rate.

the firm-month level and use difference-in-differences to study whether the greater dollar lending of treated banks led to a greater overall dollar borrowing of the firms that rely on treated banks. That is, I compare the changes in borrowing between firms that were "most exposed" to treated banks and those "less exposed" after capital controls were set. To split firms into "more exposed" or "less exposed" to treated banks, I use two exposure measures. The first one is the percentage of a firm's debt that relies on treated banks, while the second splits the firms into two groups based on this measure: those that have an above median exposure ("Exposed firm" = 1) to treated banks, and those below the median ("Exposed firm" = 0). I used pre capital controls announcement (December 2010) values of the percentage of a firm's debt that relies on treated banks to assign whether or not a firm is "exposed."

Using these definitions of exposure, I estimate Equation (2) to compute whether capital controls had an overall effect on firms' bank loan dollarization:

$$\begin{aligned} y_{f,t} = &\alpha_0 + \alpha_1 \text{Exposed firm}_f + \alpha_2 \text{Exposed firm}_f \times \text{Post CC} + \\ &+ \text{Firm Size} \times \text{Industry FE} \times \text{Date FE} + \psi X_f^{\text{bank}} + \upsilon_{f,t} \end{aligned} \tag{2}$$

Similar to the regressions in Section IV.B, the dependent variable in Equation (2), y, is either (1) the share of total bank loans (credit) that is in dollars, (2) log(Credit in dollars + 1) or (3) log(Credit in soles + 1). The difference with Section IV.B, though, is that in this case, these variables are at the firm (f) and month (t) level, while in Section IV.B these variables were at the firm-bank-month level.

α_2 is the coefficient of interest as it captures how much more a firm that is exposed to a treated bank changed its credit in the post capital controls period in contrast to a firm that was less exposed to a treated bank. To isolate changes in firms' borrowing behavior that could be due to bank-specific characteristics (other than capital controls), $X_{f,t}^{\text{bank}}$ controls for the firms' exposure to different variables at the bank level that can affect firms' borrowing. These bank-specific variables are the shares of dollar and soles deposits over total assets, dollar and soles liquidity ratios, log of total assets and return over assets. To capture a firm's exposure to each of these variables, I use the share of debt with a specific bank as weight. Finally, for the comparison between more and less exposed firms, I compare only firms within the same industry and firm size at each point in time by using firm size$\times$industry$\times$month fixed effect.

Table VI presents the results of Equation (2). The first three columns use whether a firm's exposure (share of its debt that is borrowed from a treated bank) is above or below the median, while the last three columns ("Continuous Exposure") use directly the percentage of a firm's debt that relies on treated banks. The results in this table suggest that capital controls did have an effect at the firm level. In particular, those firms that were more exposed to treated banks increased their total loans in dollars by 13-20%. Thus, firms did not use the dollars supplied by treated banks to pay out dollar debt at a different bank. However, in terms of soles loans, even though at the firm-bank level the treated banks also decreased soles lending, the firms were able to substitute this decrease by leveraging more in soles from a non-treated bank.

However, the concern with this regression is that the "more exposed" and "less exposed" firms do not need to be similar. Thus, the driving factor in the results could be due to differences between these two groups of firms. Unfortunately, I do not have information of the firms' balance sheet to assess how different are these two groups of firms. Then, to reduce the concerns that the results from the previous regression could be due to differences in firms' characteristics, I used propensity-score matching to build a subset of comparable firms for which to do the analysis. Although matching helps alleviate concerns about the comparability between the two groups of firms, lack of firms' balance sheet data limits the analysis that can be done to show firms that were "more exposed" to treated banks were not statistically different from those that were less exposed to treated banks. However, although these limitations reinforce that the contribution of this paper is at the bank level and not at the firm level, the firm-level regression depicted in Equation (2) is still helpful to show suggestive evidence of the total effect of capital controls on firms overall debt dollarization.

The way I created a set of comparable firms is by using, for each industry, propensity-score matching to match "less exposed" firms that have similar covariates the year before the imposition of capital controls than the “more exposed” firms. For this matching, I considered the “less exposed” (“more exposed”) firms to be those below (above) the median of share of debt with treated banks the month before the imposition of capital controls. The covariates I matched consist of variables that summarize the borrowing behavior of firms in 2010[44] as well as dummy variables for whether the firm is an exporter, importer and its firm size. I narrowed the analysis to only those firms that share common support in terms of the propensity scores between the “less exposed” and “more exposed” firms. Figure 13 plots the overlap in terms of the propensity scores between the “less exposed” firms (untreated) and the “more exposed” (treated) firms. It shows that there is an important overlap between these two.

After using propensity score matching to make the “less exposed” and “more exposed” firms more comparable, the differences in covariates between the “less exposed” and “more exposed” firms during the year before the imposition of capital controls decreased significantly. This is seen in Table A.XI in the Appendix. This table shows the mean of the unmatched and matched samples for all the covariates used in the matching. Before matching, these two groups of firms were very different in terms of their borrowing patterns, size and foreign trade activity. After matching firms, the less and more exposed firms show very similar borrowing patterns the year before capital controls, and are similar in size and foreign trade activity. Indeed, as a result of this matching, Figure 14, which plots the coefficients of the effect of exposure to treated banks across time, shows there are no pre-trends in the dollars and soles borrowing between the “less exposed” and “more exposed” firms before the imposition of capital controls.

Besides showing there are no pre-trends, Figure 14 shows that the “more exposed” firms increased their dollar borrowing after the imposition of capital controls but they did not change their soles borrowing. Table (VII) presents the results of Equation (2) when using only matched firms. It shows that firms increased around 10-17% their borrowing in dollars the year after the imposition of capital controls. Nevertheless, when using as definition of exposure whether a firm has above or below median share of debt with treated

[44] The variables summarizing the borrowing behavior of firms are: the 2010 monthly borrowing, 2010 average and the 2010 year over year average of soles and dollar borrowing as well as its share. For this, I only take firms that existed during all months in 2010.

banks, this coefficient is statistically insignificant. Looking into Figure 14, it seems this could be the case, because although more exposed firms increased their dollar borrowing by a peak of approximately 30% by September 2011, this dollar borrowing reverted back to pre-capital controls levels after September.

Moreover, as seen in Table A.XII in the Appendix, the results on dollar borrowing strengthen when redoing the previous analysis using matched firms but excluding exporters and those firms that have bought dollars using forward contracts or swaps. Hence, the firms that increased their total dollar leverage were not those that held dollar revenues from exports or hedged the dollar liabilities using financial instruments.

VI. External Validity

Do the results of this paper hold broadly in emerging markets? To answer this question, it would be optimal to replicate the results of this paper using data for different emerging economies. Unfortunately, as replicating this paper for other countries requires detailed, confidential data to which I do not have access, this paper is constrained to analyzing the imposition of capital controls in Peru. Nevertheless, this section shows suggestive evidence that other emerging economies comply with the conditions required for banks to respond to capital controls by substituting lending in local currency for lending in dollars.

In this paper I showed that capital controls can induce banks to lend more dollars and lend less domestic currency when capital controls are introduced in an economy where (1) banks have dollar liabilities and (2) banks hedge the currency mismatch between assets and liabilities. Next I discuss that these two characteristics are common in various emerging economies.

1. Banks have dollar liabilities: As seen in Figure 15, in many countries outside the US, banks hold a significant share of dollar liabilities. The total dollar liabilities of banks outside the US are approximately 10 trillion dollars (see Shin (2012)). This magnitude is such, that it is comparable to the total dollar liabilities of banks in the US. Dollarization of banks' liabilities is even worse in various developing countries (such as Peru) because its households save partially in dollars to hedge against inflation and country instability.[45]

Interestingly, Peru's bank deposit dollarization experience not only shares a common origin to that of other emerging economies (household hedging) but also shares a common evolution across time. During 1990s inflation was high not only in Peru but across emerging markets. Hence, deposit dollarization in the world (more specifically in Latin America and emerging Europe) peaked during the 1990s but as emerging economies' economic fundamentals strengthened during the 2000s, deposit dollarization has fallen since then (Catão and Terrones, 2016). As banks overall deposit dollarization decreased, their dollar lending also subsided (Dalgic, 2018). Yet, although lower than in the 1990s, deposit and bank lending dollarization remains above 20% in many of these economies.

2. Banks hedge exchange rate risk: A common practice across bank regulators in the world is to enforce that local banks hedge the exchange rate risk that arises from mismatches between their assets and liabilities. Canta et al. (2006) provides evidence of this argument as they show that more than 40 countries have direct limits on the total exchange rate risk banks are permitted to have. These limits are often augmented by setting additional capital requirements for banks with exchange rate exposure.

VII. Conclusion

This is the first paper (to the best of my knowledge) to show that capital controls on carry trade inflows can increase dollarization of firms' debt. I show that capital controls induce local banks to substitute lending in local currency for lending in dollars. This occurs because given that households in many emerging markets save partially in dollars, local banks in these economies use foreign currency markets to hedge dollar deposits. However, when countries introduce capital controls to prevent carry trade inflows, they have to restrict a wide set of transactions to prevent regulatory arbitrage. One of these transactions include the use of currency forward contracts, given that foreign investors use forward contracts to earn the interest rate

[45] See Catão and Terrones (2016), Rajan and Tokatlidis (2005) Rappoport (2009), Dalgic (2018)

differential between the dollar and emerging market currency. Therefore, when banks face capital controls, they will hedge dollar deposits by lending dollars rather than domestic currency to domestic firms.

To test this prediction, I use novel and confidential data. I take advantage of a natural experiment in Peru, where the intensity of capital controls treatment varied across banks, to show causal evidence that capital controls increased banks' dollar lending and decreased banks' soles lending by 10-20%. Importantly, banks increase dollar lending to firms that do not have dollar revenues and are not hedging their dollar liabilities by acquiring hedging securities. Hence, capital controls can lead to an overall increase in firms' exposure to the exchange rate. This is very problematic because various other studies have documented that it is exactly the greater foreign currency borrowing of firms that has generated large reductions in investment and employment in these firms.

Further research is needed to understand the aggregate effect on the overall effect that capital controls have on reducing losses from currency mismatches between assets and liabilities given that these depend on both the changes in the exchange rate and in the exposure of firms.

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Table I: Banks' Aggregate USD Assets as Percentage of Liabilities

This table shows the dollar assets as percentage of dollar liabilities of the aggregate Peruvian banking system in December 2010 (pre capital controls) and December 2011 (post capital controls) for different dollar asset classes. This means, that for example, 60.02% of all dollar liabilities are matched with dollar assets by lending dollars to firms and households. Among the dollar asset classes, I have included dollars forward, which technically are an off-balance sheet account. I have done this because this is another way banks can match their dollar liabilities with off balance sheet dollar "assets".

	Share of USD Liabilities + Equity(%)	
	December 2010	December 2011
Assets		
USD Loans	60.2%	66.2%
USD Cash	28.0%	28.6%
USD Forward	6.1%	0.3%
USD Investments	3.4%	3.4%
USD Other	2.1%	1.7%
Total (including forwards)	99.8%	100.2%

Table II: Summary Statistics: Banks Balance Sheets

This table reports the summary statistics of the main variables that will be used in the benchmark regression shown in Table V. Panel A shows these statistics at the bank level. The percentage of forward limit represents how binding was the capital control restriction two days before the announcement of this regulation. The percentage change in soles, dollar and total credit has been measured between one month after the regulation came into effect (May 2011) and one month before the regulation was announced (December 2010). The banks' balance sheet variables have been measured as of December 2010. Panel B displays summary statistics at the firm level only. These report the percentage change in credit in soles and dollars over December 2010 to May 2011. It also displays the average percentage of credit in dollars that firms had as of December 2010 and the change in this percentage between December 2010 to May 2011. It also shows the number of bank relationships that the firm keeps on average each month, both for dollar and soles credit. Panel C reports similar statistics but at the firm-bank level. The length of firm-bank relationship is reported in years.

	Mean	Median	SD	P5	P95	N
Panel A. Banks						
FX Forwards						
% Fwd Limit (All Banks)$_{22Jan2011}$	48.80	61.35	52.28	0.00	149.71	13
Market Share: FX Forwards (%)$_{2010m12}$	7.69	4.61	9.37	0.00	24.07	13
Δ Credit						
Ch PEN Credit (%)	10.16	5.29	27.60	-24.70	93.10	13
Ch. USD Credit (%, Constant FX)	11.30	12.20	7.88	-9.58	21.98	11
Ch. Total Credit (%, Constant FX)	15.21	10.18	24.59	-9.67	93.10	13
Ch. USD Ratio (%)	1.37	1.21	2.33	-0.98	7.46	11
Other Bank Variables						
ROA$_{2010m12}$ (%)	0.02	0.02	0.01	-0.02	0.03	13
Total Assets$_{2010m12}$ (Billion PEN)	13.73	4.46	19.81	1.13	67.11	13
Liq.Ratio PEN$_{2010m12}$ (%)	42.16	37.84	17.33	18.87	64.37	13
Liq.Ratio USD$_{2010m12}$ (%)	45.02	45.55	10.41	28.22	58.02	13
PEN dep./Assets$_{2010m12}$ (%)	37.71	37.21	12.30	10.58	60.34	13
USD dep./Assets$_{2010m12}$ (%)	26.49	30.24	13.00	1.87	41.81	13
Panel B. Firm						
Δ Credit						
Ch. PEN Credit (%)	-10.92	-7.22	156.70	-206.69	170.92	8005
Ch. USD Credit (%, FX: 2005m2)	-10.01	-9.18	115.71	-131.37	111.09	9553
Ratio USD$_{2010m12}$ (%, Constant FX)	65.64	85.51	39.01	0.00	100.00	13238
Ch. Ratio USD (bp, FX: 2005m2)	-0.30	0.00	18.95	-24.24	22.72	11423
Bank Relationship						
# USD Bank Relationships (by month)	2.05	2.00	1.30	1.00	5.00	11304
# PEN Bank Relationships (by month)	1.87	2.00	1.12	1.00	4.00	9945
# Bank Relationships (by month)	2.41	2.00	1.43	1.00	5.00	13223
Panel C. Bank-Firm Relationship						
Δ Credit						
Ch. PEN Credit (%)	-7.79	-6.98	142.26	-178.94	168.06	12896
Ch. USD Credit (%, Constant FX)	-4.83	-10.49	121.72	-127.25	142.99	17419
Ratio USD (%, FX: 2005m2)	62.73	95.45	44.34	0.00	100.00	28622
Ch. Ratio USD (bp, Constant FX)	0.12	0.00	18.92	-20.11	22.08	23463
Bank Relationship						
Length Firm-Bank USD Rel$_{2010m12}$ (years)	2.25	2.00	2.21	0.00	6.00	28622
Length Firm-Bank PEN Rel$_{2010m12}$ (years)	1.72	1.00	2.02	0.00	6.00	28622
Length Firm-Bank Rel$_{2010m12}$ (years)	2.67	2.00	2.11	0.00	6.00	28622
% Credit with each bank$_{2010m12}$	44.81	34.45	35.56	2.19	100.00	23290
% PEN Credit with each bank$_{2010m12}$	56.53	55.40	38.11	1.86	100.00	13770
% USD Credit with each bank$_{2010m12}$(FX: 2005m2)	51.07	44.46	36.91	2.46	100.00	17803

Table III: Difference of Means Between Banks Using Above its Forward Limit (Treated Banks) vs Those Below (Banks in Control Group)

This table compares balance sheet statistics of the banks for which the capital controls was binding (treated banks) versus those for which it was not. I define capital controls to be binding when, as of two days previous to the capital controls announcement date, the banks were using more than the regulatory limit announced two days later. This limit was the maximum between PEN 400 million (USD 144 million as of Jan 2011) and 40% of the bank's equity. While Panel A displays these statistics using all banks, Panel B narrows the analysis to only the biggest four banks. For both panels, the percentage change in soles, dollar and total credit has been measured between one month after the regulation came into effect (May 2011) and one month before the regulation was announced (December 2010). The banks' balance sheet variables have been measured as of December 2010.

	Control Group		Treated Banks			
	Mean	N	Mean	N	T-stat	β
Panel A. All Banks						
FX Forwards						
% Fwd Limit (All Banks)$_{22Jan2011}$	26.37	10.00	123.55	3.00	-4.67	-97.17***
Market Share: FX Forwards (%)$_{2010m12}$	5.83	10.00	13.89	3.00	-1.35	-8.06
Δ Credit						
Ch PEN Credit (%)	15.61	10.00	-8.00	3.00	1.34	23.61
Ch. USD Credit (%, Constant FX)	10.04	8.00	14.66	3.00	-0.85	-4.62
Ch. Total Credit (%, Constant FX)	16.99	10.00	9.30	3.00	0.46	7.69
Ch. USD Ratio (%)	0.35	8.00	4.08	3.00	-3.37	-3.74***
Other Bank Variables						
Market Share: PEN Credit$_{2010m12}$ (%)	7.15	10.00	9.50	3.00	-0.37	-2.35
Market Share: USD Credit$_{2010m12}$ (%)	7.30	10.00	9.01	3.00	-0.20	-1.71
Market Share: Total Credit$_{2010m12}$ (%)	7.23	10.00	9.24	3.00	-0.27	-2.02
ROA$_{2010m12}$ (%)	0.02	10.00	0.01	3.00	0.67	0.01
Total Assets$_{2010m12}$ (Billion USD)	4.56	10.00	5.97	3.00	-0.29	-1.40
Liq. Ratio PEN$_{2010m12}$ (%)	40.27	10.00	48.46	3.00	-0.70	-8.19
Liq. Ratio USD$_{2010m12}$ (%)	44.45	10.00	46.93	3.00	-0.35	-2.49
PEN dep./Assets$_{2010m12}$ (%)	39.79	10.00	30.78	3.00	1.12	9.00
USD dep./Assets$_{2010m12}$ (%)	23.70	10.00	35.82	3.00	-1.49	-12.12
Panel B. Main 4 Banks						
FX Forwards						
% Fwd Limit$_{22Jan2011}$	62.31	2.00	126.79	2.00	-2.81	-64.48
Market Share: FX Forwards (%)$_{2010m12}$	21.24	2.00	18.46	2.00	0.46	2.78
Δ Credit						
Ch PEN Credit (%)	6.96	2.00	0.34	2.00	2.22	6.62
Ch. USD Credit (%, Constant FX)	11.21	2.00	13.90	2.00	-1.81	-2.70
Ch. Total Credit (%, Constant FX)	10.28	2.00	10.79	2.00	-0.56	-0.51
Ch. USD Ratio (%)	0.72	2.00	2.40	2.00	-2.61	-1.68
Other Bank Variables						
Market Share: PEN Credit$_{2010m12}$ (%)	26.29	2.00	13.09	2.00	5.57	13.20**
Market Share: USD Credit$_{2010m12}$ (%)	32.06	2.00	12.98	2.00	1.86	19.08
Market Share: Total Credit$_{2010m12}$ (%)	29.33	2.00	13.03	2.00	2.53	16.29
ROA$_{2010m12}$ (%)	0.02	2.00	0.03	2.00	-0.67	-0.00
Total Assets$_{2010m12}$ (Billion USD)	18.67	2.00	8.34	2.00	1.91	10.34
Liq. Ratio PEN$_{2010m12}$ (%)	56.30	2.00	53.77	2.00	0.22	2.54
Liq. Ratio USD$_{2010m12}$ (%)	42.03	2.00	41.69	2.00	0.05	0.34
PEN dep./Assets$_{2010m12}$ (%)	35.05	2.00	30.14	2.00	2.25	4.90
USD dep./Assets$_{2010m12}$ (%)	31.53	2.00	32.82	2.00	-0.51	-1.29

Table IV: Probability of trading a forward contract with the same bank as was done in the previous trade

This table shows the estimated probability that a counterparty trades forward contracts with the same bank the counterparty did in the previous forward contract trade between 2007 and 2010. This probability is the result of the following regression:

$$\begin{aligned}\text{Bank Traded}_{b,c,t} = & \rho_0 + \rho_1 \text{Previous Bank Traded}_{b,c,t-1} + \text{Bank FE}_b \text{Bank FE} \times \text{Month FE}_{b,t} + \\ & + \text{Bank FE} \times \text{Cpty Type FE}_{b,c} + \upsilon_{b,c,t}\end{aligned}$$

The regression is at the bank, counterparty and trade date level. The dependent variable, Bank Traded is a dummy variable that takes 1 if the counterparty c trades with bank b at trade date t. If not, it is zero. The variable "Previous Bank Traded" is also a dummy variable that takes 1 if the counterparty c traded with bank b the last time it traded forward contracts.

Given that a counterparty could trade with a bank because of the bank's market share, I control for bank fixed effects, to remove the overall market share of bank b from the estimate of ρ_1. The interaction of bank fixed effect and month fixed effect controls for whether a bank was particularly active during a certain time window. Finally, because banks can specialize in a particular type of client, such as foreigners, pension funds or firms, I use bank $\times$ counterparty type fixed effects.

Column (1) estimates the regression without any fixed effects, Column (2) uses only bank fixed effects, Column (3) adds bank$\times$ month fixed effects to the regression in Column (2), and finally Column (4) has the previous fixed effects plus bank$\times$counterparty class fixed effects. T-statistics are in parenthesis. Standard errors have been clustered by bank, counterparty and date. ***, ** and * denote significance at 1%, 5% and 10% respectively.

	Traded with Bank			
Previous bank traded	0.729***	0.655***	0.645***	0.620***
	(16.98)	(15.43)	(14.59)	(11.36)
Bank FE	No	Yes	Yes	Yes
Bank x Date(mo) FE	No	No	Yes	Yes
BkCptyClass	No	No	No	Yes
Cluster	Date, Bank, Cpty	Date, Bank, Cpty	Date, Bank, Cpty	Date, Bank, Cpty
Date Clusters	999	999	999	999
Bank Clusters	17	17	17	17
Cpty Clusters	876	876	876	876
Observations	196098	196098	196098	196098
Adjusted R2	0.531	0.551	0.553	0.560

Table V: Effect of Capital Controls on Credit Supply

This table presents the main regression results: the effect of capital controls on the share of dollar loans, dollar loans and soles loans. The first column of each dependent variable shows the estimates for Equation 1 but without bank and bank-firm relationship controls, as well as without Firm×Date fixed effects. The second column adds bank fixed effects and bank-firm relationship controls. The coefficient on the capital controls dummy was dropped due to collinearity with the bank fixed effects. The third and fourth column display the results when using Firm×Date fixed effects, without and with bank fixed effects and bank-firm controls respectively. Post CC is dropped due to collinearity with Firm×Date fixed effects. The coefficient of interest in Equation 1, $\widehat{\beta}_3$, is associated with the interaction variable "CC × Post CC". This is highlighted in gray. The capital controls variable, the forward limit, takes the value of 1 for the banks that were using above 100% of its limit as of January 22nd 2011 and 0 otherwise. January 22nd 2011 corresponds to two days before the announcement of the capital controls and is the last reporting date before the announcement of the controls. The bank-firm relationship controls are: (1) the length of the firm-bank relationship and (2) the percentage of credit that a firm receives from a bank. The length of the firm-bank relationship is computed as the number of months in which there is non-zero credit balance between a bank and a firm up to December 2010. Standard errors are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$ [Const. FX]				Log(USD Credit + 1)×100 [Const. FX]				Log(PEN Credit + 1)×100			
CC * Post CC	0.573	1.043***	1.488***	1.390***	8.977*	8.755**	23.24***	9.953**	-6.301	-16.27***	-12.07**	-22.06***
	(0.367)	(0.330)	(0.379)	(0.359)	(4.812)	(4.351)	(4.995)	(4.679)	(4.771)	(4.710)	(5.125)	(5.204)
CC	8.373***		6.002***		26.71***		-24.99***		-212.8***		-218.0***	
	(0.461)		(0.448)		(5.959)		(5.809)		(6.233)		(6.372)	
Post CC	-2.201***	0.205			-24.54***	19.67***			24.75***	18.88***		
	(0.232)	(0.193)			(3.078)	(2.539)			(2.827)	(2.656)		
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N Firm Cluster	19,296	12,414	12,866	7,314	19,296	12,414	12,866	7,314	19,296	12,414	12,866	7,314
Observations	654,012	533,098	533,603	420,788	654,012	533,098	533,603	420,788	654,012	533,098	533,603	420,788
Adjusted R2	0.01	0.05	0.41	0.45	0.00	0.12	0.38	0.50	0.03	0.05	0.33	0.40

Table VI: Firm level regression of Capital Controls Exposure on Credit

This table presents the results of the regression shown in Equation 2. This regression is similar to that in Equation 1 but collapses the data at the firm-month level. The first three columns ("Continuous exposure") use firm's exposure as the share of its debt that is borrowed from treated bank in December 2010. The last three columns use as exposure whether a firm's debt with treated banks in December 2010 was above or below the median. Standard errors are in parenthesis. Standard errors have been clustered by firm and date. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	Continuous Exposure			Above / Below Median Exposure		
	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$	Log(USD+1)$\times$100	Log(PEN+1)$\times$100	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$	Log(USD+1)$\times$100	Log(PEN+1)$\times$100
Post CC * Exposure	0.973	19.85**	3.564	0.310	13.71***	6.870
	(0.675)	(8.688)	(12.05)	(0.401)	(5.238)	(6.416)
Exposure	9.157***	92.84***	-115.5***	2.192***	132.2***	134.5***
	(1.322)	(15.99)	(20.64)	(0.772)	(9.055)	(10.97)
Exposure to Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Firm Size * Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	214225	214225	214225	214225	214225	214225
Adjusted R2	0.152	0.152	0.0991	0.148	0.167	0.109
N Firm Cluster	9350	9350	9350	9350	9350	9350

Table VII: Firm level regression of Capital Controls Exposure on Credit After Matching

This table presents the results of the regression shown in Equation 2 after matching "more exposed" firms with "less exposed" firms the year before capital controls. The first three columns ("Continuous exposure") use firm's exposure as the share of its debt that is borrowed from treated bank in December 2010. The last three columns use as exposure whether a firm's debt with treated banks in December 2010 was above or below the median. Standard errors are in parenthesis. Standard errors have been clustered by firm and date. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	Continuous Exposure			Above / Below Median Exposure		
	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$	Log(USD+1)×100	Log(PEN+1)×100	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$	Log(USD+1)×100	Log(PEN+1)×100
Post CC * Exposure	0.370	17.20*	-1.283	-0.0831	10.50	0.568
	(0.847)	(10.46)	(14.33)	(0.578)	(6.585)	(9.294)
Exposure	4.550***	-56.00***	-220.2***	-2.065**	-5.635	43.99***
	(1.677)	(17.80)	(25.78)	(1.020)	(9.808)	(14.86)
Exposure to Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Firm Size * Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	194245	194245	194245	194245	194245	194245
Adjusted R2	0.169	0.154	0.146	0.168	0.153	0.135
N Firm Cluster	6170	6170	6170	6170	6170	6170

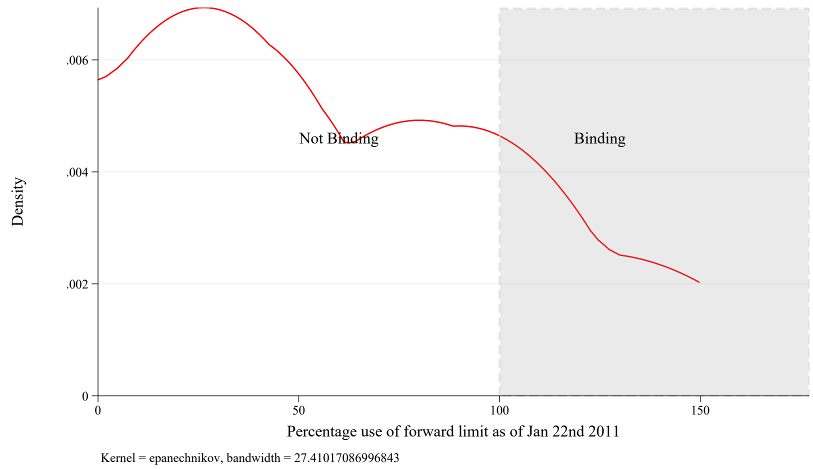


Figure 1: Kernel Density of Forward Holdings on January 22nd 2011

The image above shows the kernel density of the percentage use of the forward limit ($\frac{\text{Net forward holdings}_{b,22Jan2011}}{\text{Regulatory limit}_b}$) of all banks in the Peruvian financial system as of January 22nd 2011. This is the last reporting date available before the announcement of the regulatory limit, which occurred two days after. Then, it shows how constrained were different banks at the time the capital controls were announced. When the banks are surpassing 100% of their forward limit ("binding" area in the plot), they have to reduce their net long forward holdings. I have used the kernel density rather than plot the actual percentage use of the forward limit to keep confidentiality of banks.

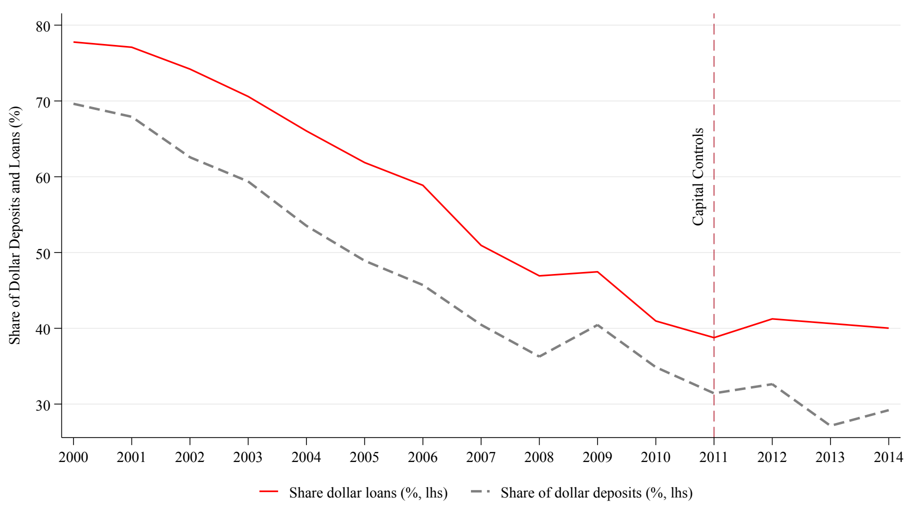


Figure 2: Dollarization of Deposits and Loans in the Peruvian Banking System

This figure plots the share of deposits in dollars (gray line) and the share of loans banks make in dollars (red line) between 2000 and 2014. Although deposit and bank lending dollarization have decreased consistently, when forward limits were set in Peru, in 2011, the downward trend in deposit and lending dollarization stopped. Moreover, loan dollarization started to increase. I study the sample between 2010 and 2012, where dollarization rates of deposits and loans were around 30% and 40%, respectively.

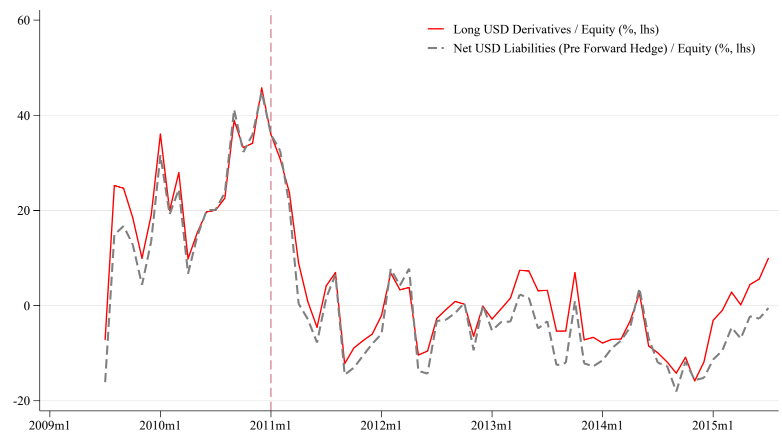


Figure 3: Currency Exposure and Derivatives Hedging

This figure shows the net dollar liabilities of banks' balance sheets (before derivatives) and their net dollar assets with derivatives. Both measures are shown as percentage of equity. This plot shows that banks hedge the balance sheets' net dollar liabilities with net dollar assets from derivatives contracts. The net dollar liabilities (gray dotted line) is computed as dollar liabilities minus dollar assets in the banks' balance sheets without incorporating derivative contracts. On the other hand, the net dollar assets with derivatives (red line) is computed as the total forward and swaps holdings of long dollars minus the total forward and swaps holdings of short dollars.

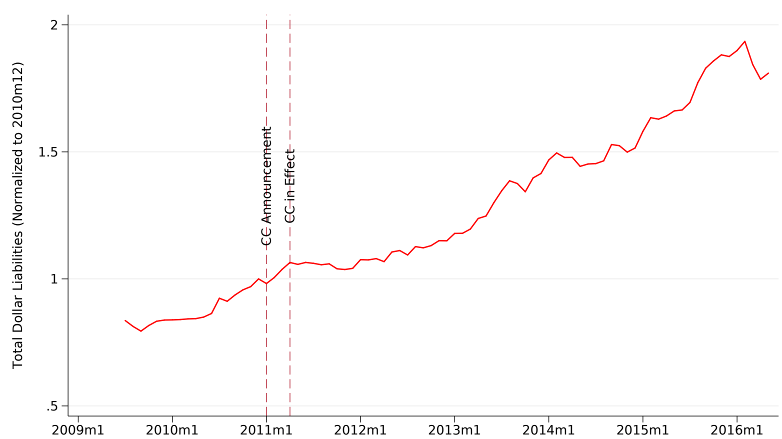


Figure 4: Normalized Aggregate Dollar Liabilities of the Peruvian Banking System

This figure shows the aggregated dollar liabilities of the Peruvian banking system. I have normalized the aggregate dollar liabilties to December 2010, the month before capital controls were announced.

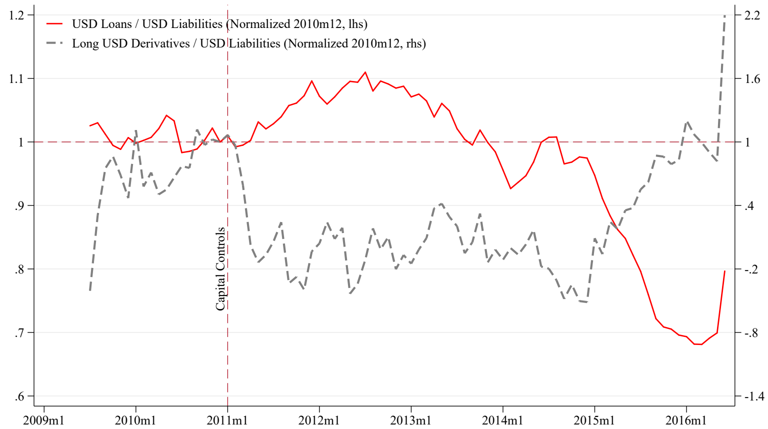


Figure 5: Dollar loans versus Derivatives Hedging

This figure shows the share of dollar liabilities hedged with dollar loans (red line) versus hedged by buying dollar forwards (gray dotted line). Both series have been normalized to December 2010, the month before the imposition of capital controls.

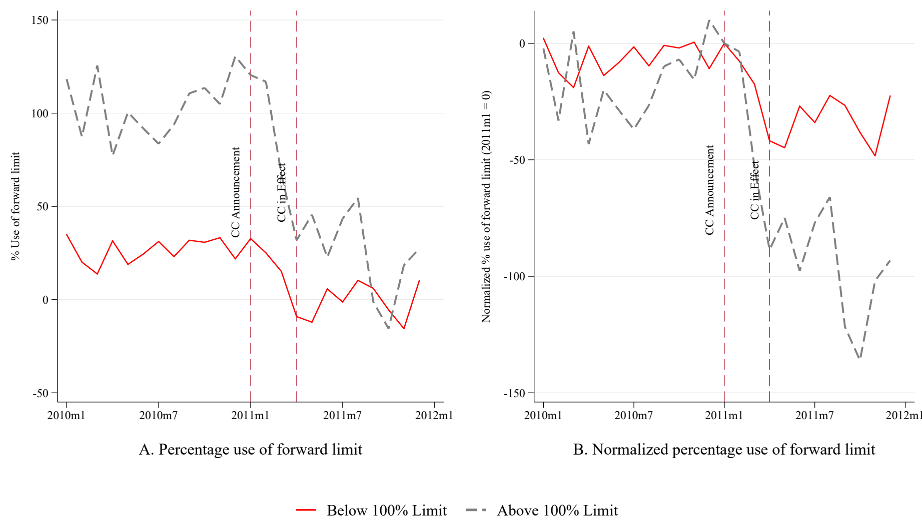


Figure 6: Percentage Use of Forward Limit

Panel A plots the evolution of the percentage use of the forward limit imposed in January 2011. For comparison, for the time before the capital controls, the percentage use of forward limit represents what would have been the fraction of the forward cap that would have been used if the capital controls had been in place. This is, for all periods t, the percentage use of forward limit is: $\frac{\text{Net forward holdings}_{bt}}{\text{Regulatory limit}_{b,Jan2011}}$. The plot splits banks into two groups: (1) Those which were above 100% use of its own forward limit as of January 2011 (gray dotted line), and (2) those that were below the maximum limit (red line). The first vertical line displays the time of the capital controls announcement, while the second shows the time in which capital controls came into effect. For comparison purposes, Panel B replicates Panel A but the forward limit in both groups has been demeaned so that limit is 0 in January 2011.

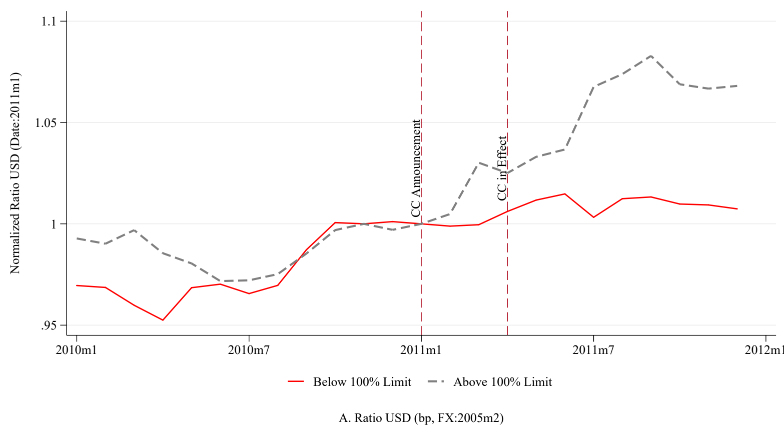


Figure 7: Normalized Ratio USD Credit

Figure 7 compares the percentage of credit given in dollars by banks which had net long forward holdings above the regulatory threshold (capital controls bind -gray dotted line-) versus those that were below the threshold (red line). This is: $\frac{\sum \text{Credit in Dollars}_{b\in<100\%,b\in\geq100\%}}{\sum [\text{Credit in USD + Credit in PEN}]_{b\in<100\%,b\in\geq100\%}}$. For illustration purposes, this ratio has been normalized by dividing it by the corresponding value as of January 2011.

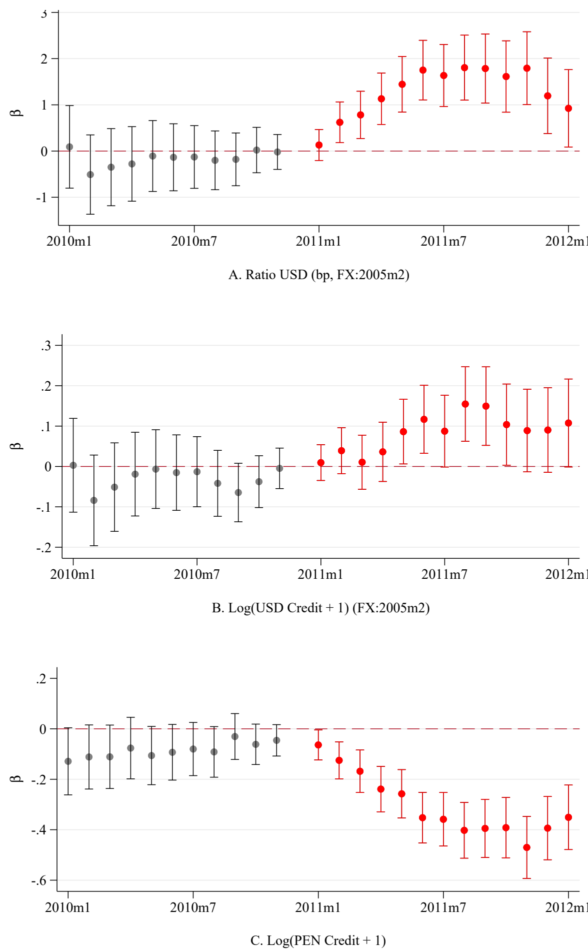


Figure 8: Testing Parallel Trends

This figure plots the $\widehat{\beta_t}$ coefficient of:

$$y_{b,f,t} = \alpha_0 + \alpha_1 CC_b + \beta_t \sum_{\substack{\tau=-12 \\ \neq 0}}^{\tau=12} \text{CC}_b \times \mathbb{1}\left[t=\tau\right] + \text{Bank FE} + \Psi X_{b,f} + \text{Firm FE} * \text{Time FE} + \upsilon_{b,f,t}$$

where y refers to the USD credit ratio $\left(\frac{\text{Credit in USD}}{\text{Total Credit}}\right)$ for Panel A, to the log of USD and PEN credit for Panel B and C, respectively. This regression is similar to Equation 1. However, instead of having a unique coefficient associated to the interaction between CC_b and Post CC, I use a dummy variable for each date (each month-year). Then each β_t is associated to the interaction between how binding were the capital controls for a bank ($= 1$ when the bank was surpassing its limit and 0 otherwise) and a indicator function which takes the value of 1 at t and 0 otherwise. The omitted dummy is January 2011 ($\tau = 0$), which is the date in which the capital controls were announced.

The β_t coefficients represent how much more credit balance (or percentage of USD credit for Panel A) a treated bank is providing to a specific firm in relation to: (1) its credit balance as of December 2010 and (2) a bank in the control group. The set of bank-firm relationship controls, $X_{b,f}$, are those used in Equation 1.

The plot uses 90% confidence intervals, where the errors have been clustered at the firm level.

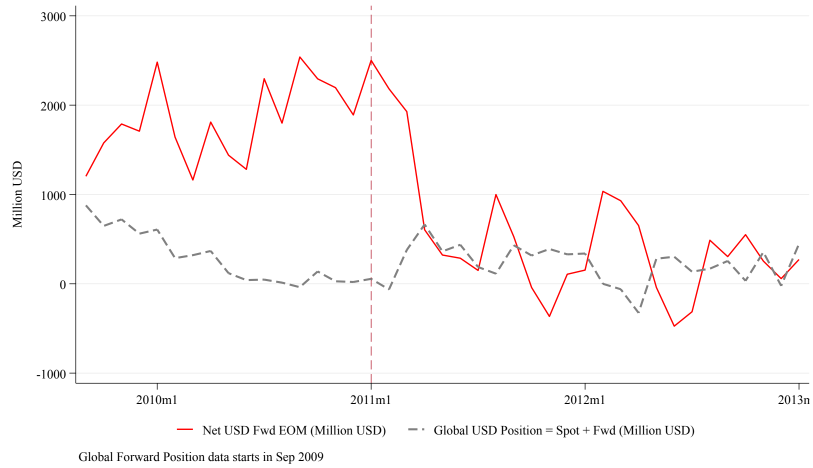


Figure 9: Net long USD forward holdings and net long USD

This figure plots local banks' net long USD forward position and the total FX position. The latter is computed as the total FX position adding spot and forward transactions of PEN/USD. A total FX position that is close to zero means that local banks have almost no FX exposure. Then, if the local banks is long USD forward (so having FX risk in its forward positions), a close to zero total FX position means that the local bank must be short USD in the spot market so as for the total FX exposure to be close to zero.

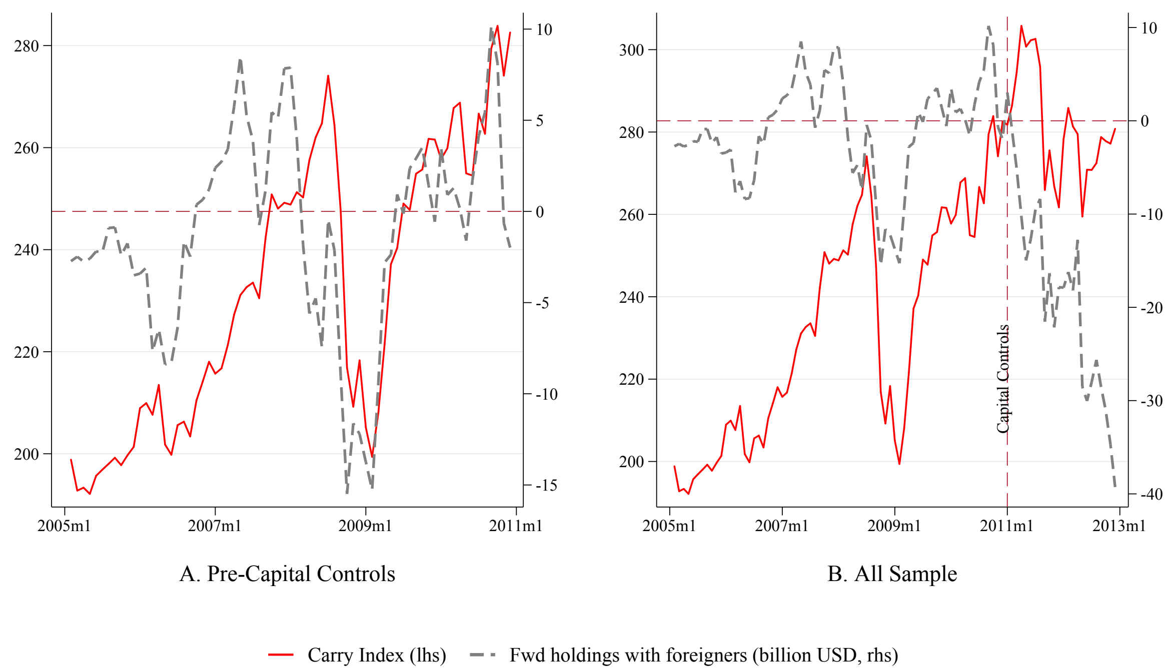


Figure 10: Forward holdings with Foreign Investors and Global Carry Returns

These figures juxtapose carry returns in EM-8 countries and net long USD forward position local banks have against foreign investors. Positive values of USD forward holdings implies local banks are long USD and short PEN against foreign investors. For scale purposes, Panel A plots these series before the capital controls (restrictions on forward holdings) set in, while Panel B plots for all the sample.

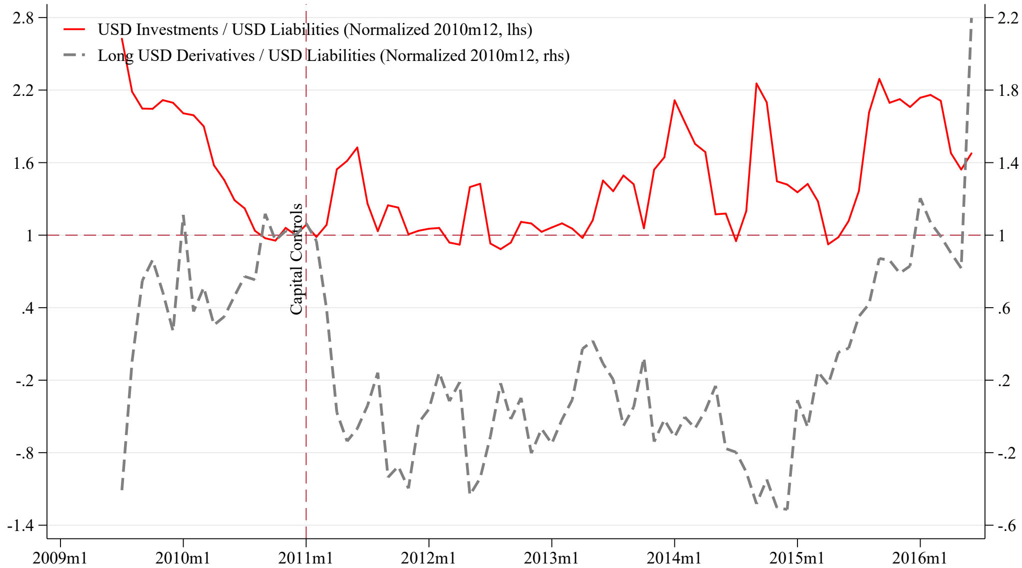
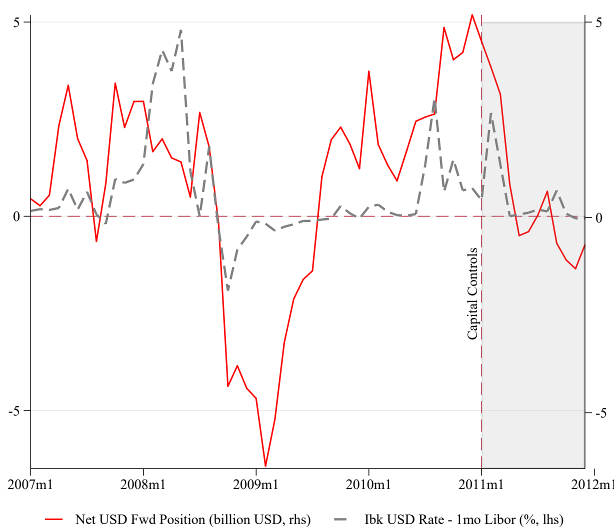
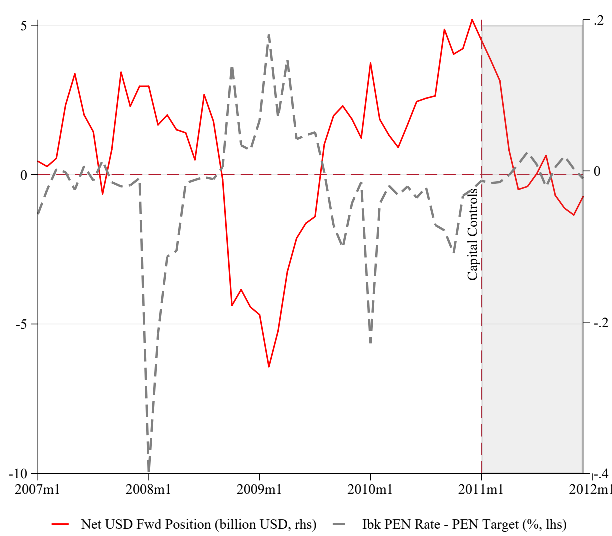


Figure 11: Investment in dollar securities and derivatives hedging

This plots shows the normalized share of dollar liabilities hedged by buying securities denominated in dollars (red line) and the normalized share of dollar liabilities hedged by taking positions in the derivatives market (gray line). As can be seen, at the time of the imposition of capital controls, the lower share of dollar liabilities hedged with derivatives was compensated by the greater share of dollar liabilities hedged by buying dollar securities.



A. USD



B. PEN

Figure 12: Forward Holdings and Interbank Rates

Figure 12 plots forward holdings against interest rate spreads. Panel A displays long dollar forward positions (rhs) against the interest rate spread of the dollar interbank rate and 1 month libor (lhs). On the other hand, Panel B displays long dollar forward positions against the interest rate spread of the soles interbank rate and the Central Bank Target rate.

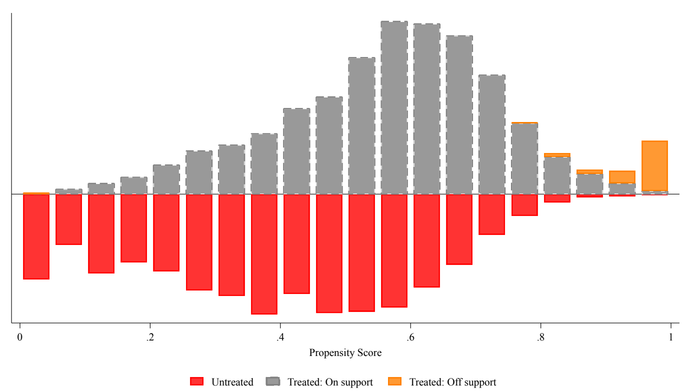


Figure 13: Propensity Scores of Less Exposed (Control) and More Exposed (Treated) Firms

This plot shows the propensity scores for "more exposed" (treated) and "less exposed" (control) firms. As it can be seen, there is common support along the different propensity scores for both treated and control firms.

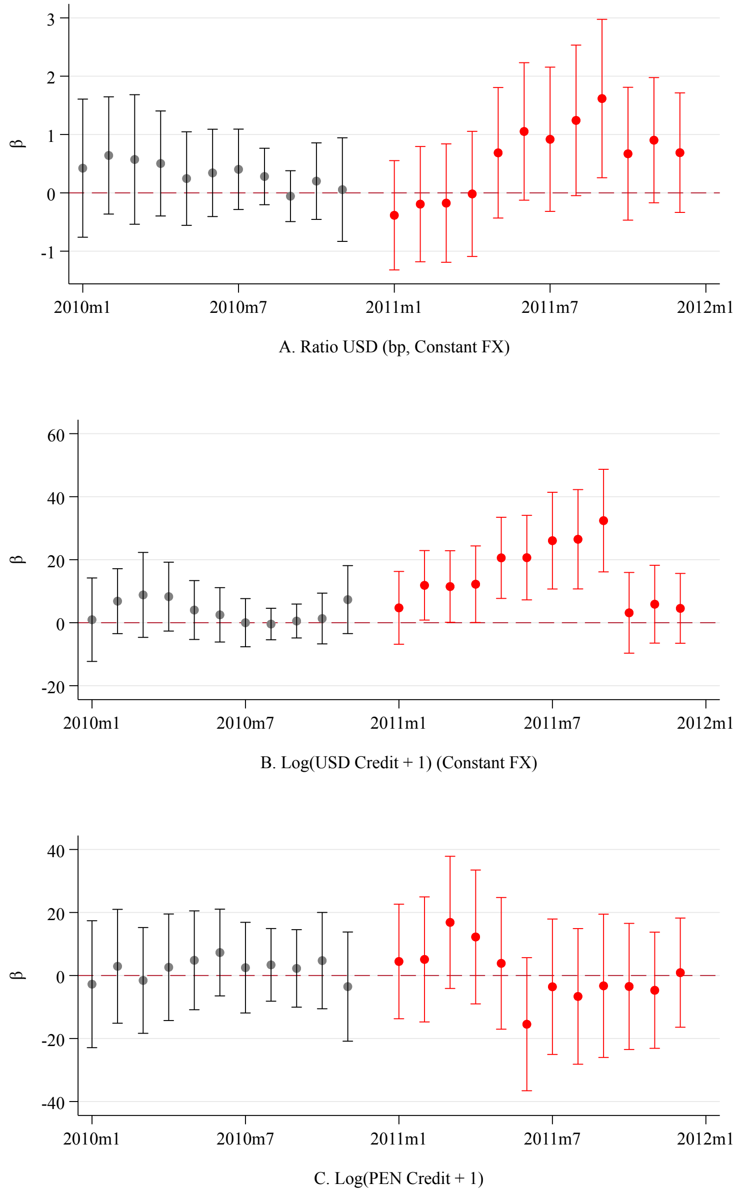


Figure 14: Testing Parallel Trends at the Firm Level

This figure plots the $\widehat{\beta}_t$ coefficient of:

$$y_{f,t} = \alpha_0 + \alpha_1 \text{Exposed firm}_f + \alpha_t \sum_{\substack{\tau=-12 \\ \neq 0}}^{\tau=12} \text{Exposed firm}_f \times \mathbb{1}[t=\tau] + \text{Firm Size} \times \text{Industry FE} \times \text{Date FE} + \psi X_f^{\text{bank}} + \upsilon_{f,t}$$

where y refers to the log of USD and PEN loans for Panel A and B, respectively. This regression is similar to Equation 2 where the results were shown in Table VII. However, instead of having a unique coefficient associated to the interaction between Exposed firm$_f$ and Post CC, I use a dummy variable for each date (each month-year). The is regression has been done using the matched sample and the exposure variable uses whether a firm's share of debt with a treated bank in December 2010 is above or below the median.

The plot uses 90% confidence intervals, where the errors have been clustered at the firm level.

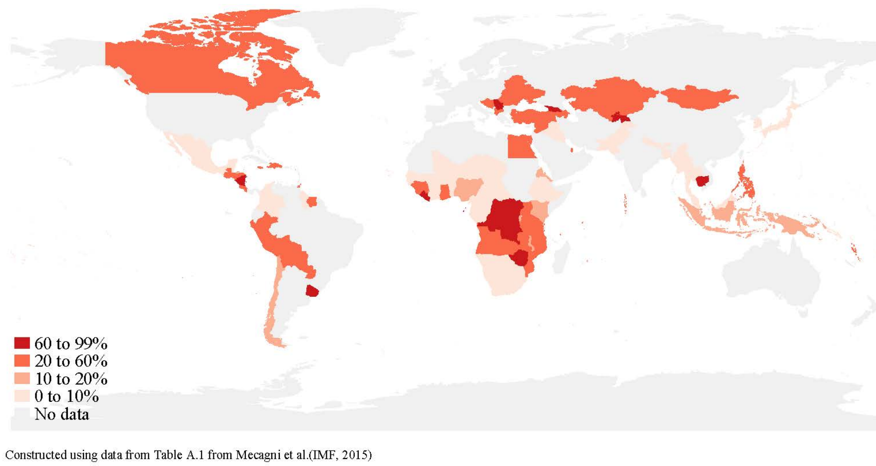


Figure 15: Average Percentage of Foreign Currency Deposits in the Local Banking System (2007 - 2011)

This figure has been constructed using the 2007-2011 averages from Table A.1. in Mecagni et al. (2015). It shows the percentage of foreign currency deposits in the local banking system of various countries.

APPENDIX

Table A.I: Effect of Capital Controls on Credit Supply: By firm size

This table replicates Table V, but splits the sample based on firm size. Standard errors are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$ [FX:2005m2]				Log(USD Credit + 1)×100 [FX:2005m2]				Log(PEN Credit + 1)×100			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel A. Large Firms												
CC * Post CC	2.301	3.157*	2.640*	3.382**	45.86*	62.98**	51.07**	51.58*	-31.86	-24.08	-41.42	-51.74*
	(1.608)	(1.693)	(1.462)	(1.490)	(27.28)	(29.53)	(25.55)	(26.86)	(26.88)	(28.88)	(26.12)	(28.40)
CC	6.289***		3.162*		8.043		-38.33		-227.6***		-199.3***	
	(2.097)		(1.707)		(31.60)		(27.01)		(36.42)		(34.36)	
Post CC	0.907	1.457			23.41	24.72			10.80	-12.69		
	(1.044)	(0.904)			(16.87)	(15.21)			(15.70)	(14.04)		
N Firm Cluster	295	269	240	195	295	269	240	195	295	269	240	195
Observations	20,632	18,056	19,006	16,258	20,632	18,056	19,006	16,258	20,632	18,056	19,006	16,258
Adjusted R2	0.01	0.04	0.61	0.68	0.00	0.04	0.49	0.58	0.03	0.05	0.48	0.56
Panel B. Medium Firms												
CC * Post CC	-0.324	0.0932	-0.00218	0.473	-8.660	-4.476	-4.752	-7.513	-6.096	-11.51	-5.910	-14.73
	(0.675)	(0.633)	(0.666)	(0.663)	(9.288)	(8.722)	(9.492)	(9.317)	(10.89)	(10.89)	(11.62)	(11.96)
CC	8.428***		6.591***		21.32*		-31.07***		-250.6***		-266.4***	
	(0.916)		(0.817)		(11.87)		(11.39)		(15.68)		(15.68)	
Post CC	1.212***	1.097***			26.23***	25.47***			-0.283	8.937		
	(0.408)	(0.375)			(5.609)	(5.274)			(6.452)	(6.309)		
N Firm Cluster	1,883	1,738	1,541	1,233	1,883	1,738	1,541	1,233	1,883	1,738	1,541	1,233
Observations	108,750	98,151	97,008	85,064	108,750	98,151	97,008	85,064	108,750	98,151	97,008	85,064
Adjusted R2	0.01	0.03	0.48	0.54	0.00	0.02	0.37	0.48	0.04	0.09	0.35	0.47
Panel C. Small Firms												
CC * Post CC	0.655	1.150***	1.776***	1.610***	12.62**	10.25**	28.11***	13.22**	-5.246	-16.52***	-13.90**	-24.16***
	(0.433)	(0.388)	(0.460)	(0.433)	(5.556)	(4.956)	(5.917)	(5.469)	(5.391)	(5.267)	(5.826)	(5.848)
CC	8.056***		6.003***		15.99**		-22.56***		-204.6***		-205.7***	
	(0.541)		(0.545)		(6.849)		(6.906)		(6.827)		(7.032)	
Post CC	-2.729***	-0.0463			-28.46***	15.02***			31.31***	20.60***		
	(0.269)	(0.224)			(3.473)	(2.890)			(3.195)	(2.966)		
N Firm Cluster	17,118	10,407	11,085	5,886	17,118	10,407	11,085	5,886	17,118	10,407	11,085	5,886
Observations	524,630	416,891	417,589	319,466	524,630	416,891	417,589	319,466	524,630	416,891	417,589	319,466
Adjusted R2	0.01	0.06	0.38	0.42	0.00	0.12	0.33	0.46	0.03	0.05	0.32	0.37

Table A.II: Effect of Capital Controls on Credit Supply: Excluding Exporters

This table presents the main regression results (Table V) but excluding from the sample exporter firms (those exporting more than $100,000 in any given month). The dependent variable for the first four columns is the dollar credit ratio, for the next four is dollar credit and for the last four is soles credit. The first column of each dependent variable shows the estimates for Equation 1 but without bank fixed effects and bank-firm relationship controls, as well as without Firm×Date fixed effects. The second column adds bank fixed effects and bank-firm relationship controls. The third and fourth column display the results when using Firm×Date fixed effects, without and with bank fixed effects and bank-firm controls respectively. The coefficient of interest in Equation 1, $\widehat{\beta_3}$, is associated with the interaction variable "CC × Post CC". This is highlighted in gray. The capital controls variable, the forward limit, takes the value of 1 for the banks that were using above 100% of its limit as of January 22nd 2011 and 0 otherwise. January 22nd 2011 corresponds to two days before the announcement of the capital controls and is the last reporting date before the announcement of the controls. The bank-firm relationship controls are: (1) the length of the firm-bank relationship and (2) the percentage of credit that a firm receives from a bank. The length of the firm-bank relationship is computed as the number of months in which there is non-zero credit balance between a bank and a firm starting in February 2005 (the starting date of the dataset) up to December 2010. Standard errors are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$ [FX:2005m2]				Log(USD Credit + 1)×100 [FX:2005m2]				Log(PEN Credit + 1)×100			
CC * Post CC	0.622	1.073***	1.480***	1.437***	10.01**	8.604*	23.52***	10.74**	-6.758	-16.70***	-11.83**	-21.71***
	(0.385)	(0.346)	(0.402)	(0.380)	(5.007)	(4.499)	(5.252)	(4.891)	(4.952)	(4.881)	(5.339)	(5.407)
CC	8.390***		6.262***		24.94***		-23.57***		-211.4***		-219.4***	
	(0.483)		(0.477)		(6.215)		(6.160)		(6.383)		(6.541)	
Post CC	-2.388***	0.0104			-27.01***	16.49***			26.57***	20.46***		
	(0.242)	(0.204)			(3.190)	(2.663)			(2.934)	(2.760)		
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N Firm Cluster	19,008	12,165	12,592	7,104	19,008	12,165	12,592	7,104	19,008	12,165	12,592	7,104
Observations	613,999	498,113	496,942	389,639	613,999	498,113	496,942	389,639	613,999	498,113	496,942	389,639
Adjusted R2	0.01	0.05	0.40	0.44	0.00	0.11	0.36	0.48	0.03	0.05	0.33	0.39

Table A.III: Effect of Capital Controls on Credit Supply: Excluding Firms with Hedging Instruments

This table presents the main regression results (Table V) but excluding from the sample firms that between 2011 and 2013 could have hedged greater dollar loans by using forward contracts and cross currency swaps. In particular I exclude all firms that bought dollars using forward contracts and which swapped dollar debt for soles. The assumption is that these instruments were used to hedge dollar loans. The dependent variable for the first four columns is the share of dollar loans, for the next four is dollar credit and for the last four is soles credit. The first column of each dependent variable shows the estimates for Equation 1 but without bank fixed effects and bank-firm relationship controls, as well as without Firm$\times$Date fixed effects. The second column adds bank and bank-firm relationship controls. The third and fourth column display the results when using Firm$\times$Date fixed effects, without and with bank fixed effects and bank-firm controls respectively. The coefficient of interest in Equation 1, $\widehat{\beta_3}$, is associated with the interaction variable "CC $\times$ Post CC". This is highlighted in gray. The capital controls variable, the forward limit, takes the value of 1 for the banks that were using above 100% of its limit as of January 22nd 2011 and 0 otherwise. January 22nd 2011 corresponds to two days before the announcement of the capital controls and is the last reporting date before the announcement of the controls. The bank-firm relationship controls are: (1) the length of the firm-bank relationship and (2) the percentage of credit that a firm receives from a bank. The length of the firm-bank relationship is computed as the number of months in which there is non-zero credit balance between a bank and a firm starting in February 2005 (the starting date of the dataset) up to December 2010. Standard errors are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$ [FX:2005m2]				Log(USD Credit + 1)$\times$100 [FX:2005m2]				Log(PEN Credit + 1)$\times$100			
CC * Post CC	0.397	0.875**	1.426***	1.210***	10.75**	11.18**	26.68***	12.08**	-1.494	-10.88**	-7.677	-16.11***
	(0.391)	(0.352)	(0.408)	(0.387)	(5.110)	(4.632)	(5.375)	(5.070)	(5.010)	(4.937)	(5.375)	(5.449)
CC	8.248***		5.885***		25.40***		-23.74***		-207.8***		-210.6***	
	(0.492)		(0.484)		(6.320)		(6.254)		(6.505)		(6.651)	
Post CC	-2.107***	0.242			-26.07***	16.13***			21.50***	14.88***		
	(0.244)	(0.204)			(3.229)	(2.692)			(2.948)	(2.757)		
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N Firm Cluster	18,412	11,677	12,115	6,732	18,412	11,677	12,115	6,732	18,412	11,677	12,115	6,732
Observations	600,163	485,827	483,709	377,592	600,163	485,827	483,709	377,592	600,163	485,827	483,709	377,592
Adjusted R2	0.01	0.05	0.41	0.45	0.00	0.12	0.37	0.48	0.03	0.05	0.34	0.41

Table A.IV: Effect of Capital Controls on Credit Supply Using Only the Largest 4 banks

This table presents the main regression results: the effect of capital controls on the dollar credit ratio, dollar credit and soles credit. The first column of each dependent variable shows the estimates for Equation 1 but without bank fixed effects and bank-firm relationship controls, as well as without Firm×Date fixed effects. The second column adds bank fixed effects and bank-firm relationship controls. The third and fourth column display the results when using Firm×Date fixed effects, without and with bank fixed effects and bank-firm controls respectively. The coefficient of interest in Equation 1, $\widehat{\beta_3}$, is associated with the interaction variable "CC × Post CC". This is highlighted in gray. The capital controls variable, the forward limit, takes the value of 1 for the banks that were using above 100% of its limit as of January 22nd 2011 and 0 otherwise. January 22nd 2011 corresponds to two days before the announcement of the capital controls and is the last reporting date before the announcement of the controls. The bank-firm relationship controls are: (1) the length of the firm-bank relationship and (2) the percentage of credit that a firm receives from a bank. The length of the firm-bank relationship is computed as the number of months in which there is non-zero credit balance between a bank and a firm starting in February 2005 (the starting date of the dataset) up to December 2010. Standard errors are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$ [FX:2005m2]				Log(USD Credit + 1)×100 [FX:2005m2]				Log(PEN Credit + 1)×100			
CC * Post CC	0.364	0.985***	1.565***	1.252***	6.640	8.614*	25.24***	8.506*	-3.905	-14.92***	-10.50*	-20.24***
	(0.390)	(0.349)	(0.418)	(0.393)	(5.124)	(4.596)	(5.547)	(5.170)	(5.033)	(4.924)	(5.572)	(5.599)
CC	8.872***		6.664***		7.095		-55.19***		-251.3***		-276.5***	
	(0.496)		(0.499)		(6.445)		(6.515)		(6.620)		(6.919)	
Post CC	-2.076***	0.155			-22.07***	19.94***			23.77***	19.73***		
	(0.244)	(0.211)			(3.257)	(2.785)			(3.069)	(2.921)		
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N Firm Cluster	18,758	12,023	11,775	6,558	18,758	12,023	11,775	6,558	18,758	12,023	11,775	6,558
Observations	545,503	450,633	415,533	330,929	545,503	450,633	415,533	330,929	545,503	450,633	415,533	330,929
Adjusted R2	0.01	0.05	0.41	0.44	0.00	0.11	0.37	0.49	0.04	0.05	0.36	0.39

Table A.V: Effect of Capital Controls on Credit Supply Using Only the Largest 4 Banks: By firm size

This table replicates Table V, but splits the sample based on firm size. Standard errors are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$ [FX:2005m2]				Log(USD Credit + 1)×100 [FX:2005m2]				Log(PEN Credit + 1)×100			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel A. Large Firms												
CC * Post CC	2.492	3.685**	1.791	2.352	56.00*	69.72**	41.32	29.74	-15.54	-16.76	-7.417	-20.10
	(1.703)	(1.732)	(1.643)	(1.580)	(29.50)	(31.22)	(28.73)	(29.85)	(28.77)	(30.19)	(28.46)	(31.08)
CC	6.566***		2.610		-3.077		-92.11***		-287.1***		-294.9***	
	(2.309)		(2.011)		(36.59)		(34.76)		(41.39)		(39.41)	
Post CC	1.700	1.279			30.86	23.26			-18.59	-25.30*		
	(1.165)	(1.063)			(19.47)	(18.06)			(16.51)	(14.99)		
N Firm Cluster	288	262	231	180	288	262	231	180	288	262	231	180
Observations	14,951	13,539	13,104	11,479	14,951	13,539	13,104	11,479	14,951	13,539	13,104	11,479
Adjusted R2	0.01	0.03	0.63	0.70	0.00	0.02	0.52	0.59	0.04	0.06	0.50	0.55
Panel B. Medium Firms												
CC * Post CC	-0.725	-0.570	-0.358	-0.0768	-7.107	-10.32	-2.907	-13.03	-3.813	-8.460	-1.134	-11.50
	(0.721)	(0.671)	(0.757)	(0.751)	(9.636)	(9.049)	(10.55)	(10.35)	(11.63)	(11.35)	(13.00)	(13.13)
CC	9.988***		7.932***		7.754		-63.44***		-316.6***		-353.6***	
	(0.992)		(0.913)		(13.04)		(12.66)		(16.88)		(17.14)	
Post CC	1.712***	1.580***			31.41***	32.47***			-2.501	7.041		
	(0.453)	(0.426)			(6.070)	(5.766)			(7.205)	(6.962)		
N Firm Cluster	1,849	1,694	1,451	1,141	1,849	1,694	1,451	1,141	1,849	1,694	1,451	1,141
Observations	88,356	81,436	75,514	67,350	88,356	81,436	75,514	67,350	88,356	81,436	75,514	67,350
Adjusted R2	0.01	0.03	0.49	0.54	0.00	0.02	0.39	0.49	0.06	0.08	0.40	0.47
Panel C. Small Firms												
CC * Post CC	0.516	1.195***	2.003***	1.622***	9.837*	11.12**	30.28***	13.62**	-3.859	-15.69***	-15.75**	-24.64***
	(0.457)	(0.409)	(0.501)	(0.470)	(5.894)	(5.245)	(6.536)	(6.027)	(5.667)	(5.525)	(6.290)	(6.264)
CC	8.363***		6.535***		-5.106		-51.18***		-237.8***		-255.4***	
	(0.578)		(0.601)		(7.356)		(7.682)		(7.225)		(7.594)	
Post CC	-2.694***	-0.169			-27.31***	14.02***			31.46***	21.82***		
	(0.281)	(0.243)			(3.676)	(3.167)			(3.451)	(3.265)		
N Firm Cluster	16,621	10,067	10,093	5,237	16,621	10,067	10,093	5,237	16,621	10,067	10,093	5,237
Observations	442,196	355,658	326,915	252,100	442,196	355,658	326,915	252,100	442,196	355,658	326,915	252,100
Adjusted R2	0.01	0.05	0.37	0.42	0.00	0.11	0.31	0.45	0.04	0.05	0.34	0.37

Table A.VI: Effect of Capital Controls on Credit Supply - Placebo Regressions

Table A.VI reports the results of the main regression shown in Table V but instead of covering the period from January 2010 to December 2011, it covers from January 2009 to December 2010. This exercise simulates as if the capital controls had been announced one year earlier (i.e. January 2010) but keeping constant the banks in the treated and control group. Standard errors are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively.

	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$ [FX:2005m2]				Log(USD Credit + 1)$\times$100 [FX:2005m2]				Log(PEN Credit + 1)$\times$100			
CC * Post CC	0.150	-0.540	-0.117	-0.447	0.0888*	-0.0287	0.0249	0.0412	0.200***	0.264***	0.129***	0.266***
	(0.383)	(0.392)	(0.273)	(0.280)	(0.0492)	(0.0499)	(0.0351)	(0.0361)	(0.0504)	(0.0553)	(0.0384)	(0.0420)
CC	8.223***		5.795***		0.178***		-0.304***		-2.328***		-2.289***	
	(0.464)		(0.421)		(0.0596)		(0.0543)		(0.0631)		(0.0593)	
Post CC	-2.492***	-0.549**			-0.339***	0.0262			0.0981***	0.0670**		
	(0.236)	(0.225)			(0.0303)	(0.0285)			(0.0295)	(0.0307)		
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
RUC	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
N Firm Cluster	16,886	12,414	16,261	12,210	16,886	12,414	16,261	12,210	16,886	12,414	16,261	12,210
Observations	589,860	452,960	589,235	452,756	589,860	452,960	589,235	452,756	589,860	452,960	589,235	452,756
Adjusted R2	0.01	0.04	0.58	0.64	0.00	0.11	0.56	0.64	0.03	0.05	0.54	0.60

Table A.VII: Alternative Capital Controls Definitions

This table replicates the benchmark regression specification after controlling for firm×date fixed effects and using for bank and bank-firm relationships controls (Table V, Column (4)). The first three columns use the treatment variable to be the percentage use of forward limit as of January 2011, which is a continuous variable. The last three columns use as treatment variable a dummy variable which takes the value of 1 for banks that were above the median percentage use of forward holdings at the time of the capital controls announcement. T-statistics are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively.

	% Use Fwd Limit - Jan 22nd 2011			Above/Below Median % Fwd Lim - Jan 22nd 2011		
	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$	Log(USD+1)×100	Log(PEN+1)×100	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$	Log(USD+1)×100	Log(PEN+1)×100
CC * Post CC	0.813*	3.915	-20.62***	1.358***	10.71**	-19.17***
	(1.84)	(0.69)	(-3.19)	(3.82)	(2.29)	(-3.68)
CC	10.74***	27.74**	-298.0***	10.41***	46.89***	-258.0***
	(10.51)	(2.08)	(-16.81)	(12.20)	(4.22)	(-17.43)
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Relationship Controls	Yes	Yes	Yes	Yes	Yes	Yes
Date * Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
St.Dev [CC * Post CC]	0.459	0.459	0.459	0.379	0.379	0.379
St.Dev Dep.Var	42.97	558.5	583.6	42.97	558.5	583.6
Observations	420788	420788	420788	420788	420788	420788
Adjusted R2	0.451	0.495	0.399	0.452	0.496	0.400
N Firm Cluster	7314	7314	7314	7314	7314	7314

Table A.VIII: Effect of Capital Controls on Credit Supply: Collapsing into Pre and Post Capital Controls

This table displays the results of the benchmark regression, Equation (1), but after collapsing the data into a pre and post capital controls period, following the suggestions in Bertrand et al. (2004). This way, for each element in the cross-section (in this case bank-firm credit) there are only two time observations. Therefore I do not use firm×date fixed effects, but only firm fixed effects. T-statistics are in parenthesis. Standard errors have been clustered by firm. ***, ** and * denote significance at 1%, 5% and 10% respectively.

	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$ [FX:2005m2]				Log(USD Credit + 1)×100 [FX:2005m2]				Log(PEN Credit + 1)×100			
CC * Post CC	0.879**	1.356***	0.858**	1.046***	10.13*	11.59***	13.18***	8.314**	-13.25**	-23.11***	-9.572*	-18.16***
	(2.18)	(4.13)	(2.56)	(3.66)	(1.89)	(2.66)	(2.92)	(2.19)	(-2.47)	(-4.68)	(-1.94)	(-3.91)
CC	8.402***	10.03***	6.684***	8.565***	32.71***	28.62**	-9.800	51.46***	-201.8***	-223.0***	-211.9***	-195.2***
	(16.11)	(11.15)	(13.44)	(11.03)	(4.79)	(2.47)	(-1.47)	(5.12)	(-28.72)	(-15.32)	(-30.08)	(-14.43)
Post CC	-1.122***	0.160	-0.0966	-0.188	-14.40***	20.84***	-0.298	0.927	15.44***	25.26***	6.050**	10.11***
	(-4.67)	(0.85)	(-0.53)	(-1.18)	(-4.40)	(8.25)	(-0.12)	(0.43)	(4.96)	(9.05)	(2.23)	(3.95)
Bank Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
St.Dev [CC * Post CC]	0.361	0.346	0.362	0.348	0.361	0.346	0.362	0.348	0.361	0.346	0.362	0.348
St.Dev Dep.Var	44.21	43.67	44.14	43.63	583.4	572.8	582.5	572.4	584.2	587.8	584.2	587.9
Observations	55391	49653	53565	48788	55391	49653	53565	48788	55391	49653	53565	48788
Adjusted R2	0.00847	0.0544	0.536	0.585	0.000943	0.125	0.505	0.628	0.0268	0.0492	0.472	0.542
N Firm Cluster	14304	12414	12478	11549	14304	12414	12478	11549	14304	12414	12478	11549

Table A.IX: Effect of Capital Controls on Credit Supply : Cluster Firm and Date

This table replicates Table V but using different clusters. T-statistics are in parenthesis. Standard errors have been clustered by firm and date (month-year). ***, ** and * denote significance at 1%, 5% and 10% respectively.

	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$ [FX:2005m2]				Log(USD Credit + 1)$\times$100 [FX:2005m2]				Log(PEN Credit + 1)$\times$100			
CC * Post CC	0.573	1.036***	1.488***	1.374***	8.977*	8.642**	23.24***	9.694**	-6.301	-16.40***	-12.07**	-22.03***
	(1.71)	(3.22)	(4.07)	(4.14)	(2.00)	(2.16)	(4.45)	(2.34)	(-1.32)	(-3.12)	(-2.50)	(-3.85)
CC	8.373***	9.931***	6.002***	8.045***	26.71***	21.50*	-24.99***	35.01***	-212.8***	-235.1***	-218.0***	-202.9***
	(18.44)	(11.49)	(13.60)	(12.01)	(4.55)	(1.98)	(-4.31)	(3.94)	(-33.73)	(-16.57)	(-35.27)	(-16.97)
Post CC	-2.201***	0.206	0	0	-24.54***	19.70***	0	0	24.75***	19.03***	0	0
	(-6.39)	(1.60)	(.)	(0.00)	(-6.51)	(5.36)	(.)	(0.00)	(5.32)	(4.49)	(.)	(0.00)
Bank Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
St.Dev [CC * Post CC]	0.366	0.358	0.381	0.375	0.366	0.358	0.381	0.375	0.366	0.358	0.381	0.375
St.Dev Dep.Var	43.99	43.05	43.96	42.97	573.7	559.1	573.6	558.5	579.1	586.2	576.1	583.6
Observations	654012	533098	533603	420788	654012	533098	533603	420788	654012	533098	533603	420788
Adjusted R2	0.00868	0.0506	0.414	0.452	0.00100	0.117	0.382	0.496	0.0295	0.0515	0.331	0.400
N Firm Cluster	19296	12414	12866	7314	19296	12414	12866	7314	19296	12414	12866	7314
N Date Cluster	24	24	24	24	24	24	24	24	24	24	24	24

Table A.X: Effect of Capital Controls on Credit Supply : Cluster Firm, Date and Bank

This table replicates Table V but using different clusters. T-statistics are in parenthesis. Standard errors have been clustered by firm, date and bank. ***, ** and * denote significance at 1%, 5% and 10% respectively.

	$\frac{\text{USD Credit}}{\text{Total Credit}}\times 100$ [FX:2005m2]				Log(USD Credit + 1)$\times$100 [FX:2005m2]				Log(PEN Credit + 1)$\times$100			
CC * Post CC	0.573	1.036**	1.488**	1.374***	8.977	8.642	23.24**	9.694	-6.301**	-16.40***	-12.07***	-22.03***
	(0.80)	(2.20)	(2.49)	(4.60)	(0.78)	(0.94)	(2.47)	(1.11)	(-2.77)	(-3.83)	(-5.45)	(-4.32)
CC	8.373***	9.931***	6.002**	8.045***	26.71	21.50	-24.99	35.01**	-212.8***	-235.1***	-218.0***	-202.9***
	(4.41)	(7.89)	(3.06)	(9.34)	(1.14)	(1.32)	(-0.68)	(2.26)	(-6.20)	(-20.65)	(-5.73)	(-23.24)
Post CC	-2.201**	0.206	0	0	-24.54**	19.70***	0	0	24.75***	19.03***	0	0
	(-2.67)	(1.05)	(.)	(0.00)	(-2.41)	(5.41)	(.)	(.)	(19.17)	(5.29)	(.)	(0.00)
Bank Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Relationship Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Date * Firm FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
St.Dev [CC * Post CC]	0.366	0.358	0.381	0.375	0.366	0.358	0.381	0.375	0.366	0.358	0.381	0.375
St.Dev Dep.Var	43.99	43.05	43.96	42.97	573.7	559.1	573.6	558.5	579.1	586.2	576.1	583.6
Observations	654012	533098	533603	420788	654012	533098	533603	420788	654012	533098	533603	420788
Adjusted R2	0.00868	0.0506	0.414	0.452	0.00100	0.117	0.382	0.496	0.0295	0.0515	0.331	0.400
N Firm Cluster	19296	12414	12866	7314	19296	12414	12866	7314	19296	12414	12866	7314
N Date Cluster	24	24	24	24	24	24	24	24	24	24	24	24
N Bank Cluster	13	13	12	12	13	13	12	12	13	13	12	12

Table A.XI: Matching of firm covariates: Means of matched and unmatched samples

This table compares the mean of the different covariates before and after matching "more exposed" firms to "less exposed" firms. The "more exposed" ("less exposed") firms are those that had above (below) median share of debt with treated banks the month before the imposition of capital controls (December 2010). For simplicity, the table below refers to the "less exposed" firms as control and the "more exposed" firms as treated. The covariates shown are those that were used for propensity score matching. The matching was done for the year before the imposition of capital controls.

Variable	Unmatched Means		t-stat	Matchd Means		t-stat
	Treated	Control		Treated	Control	
Share of USD loans (%)						
2010m1	71.7	65.3	8.09	71.5	70.1	1.81
2010m2	71.8	65.4	8.12	71.6	69.8	2.4
2010m3	71.6	65.5	7.77	71.4	69.7	2.3
2010m4	71.3	65.3	7.66	71.2	69.6	2.19
2010m5	71.2	65.2	7.61	71.1	69.5	2.08
2010m6	71.1	65.1	7.6	71.0	69.6	1.87
2010m7	71.3	65.1	7.76	71.2	69.7	1.94
2010m8	71.5	65.4	7.76	71.3	70.1	1.6
2010m9	71.4	65.5	7.46	71.3	70.1	1.56
2010m10	71.7	65.6	7.67	71.5	70.3	1.66
2010m11	71.7	65.5	7.87	71.6	70.5	1.43
2010m12	71.4	65.1	7.99	71.4	70.4	1.36
Log(USD + 1)						
2010m1	1282.4	1108.3	18.59	1274.0	1257.2	2.06
2010m2	1284.9	1108.3	18.91	1276.2	1257.4	2.33
2010m3	1287.0	1110.3	19.04	1278.4	1257.3	2.6
2010m4	1291.9	1112.0	19.57	1282.9	1263.3	2.47
2010m5	1293.0	1108.1	19.95	1283.8	1263.2	2.58
2010m6	1292.4	1104.6	20.11	1282.9	1260.2	2.8
2010m7	1295.6	1103.0	20.65	1285.9	1264.8	2.63
2010m8	1298.5	1100.6	21.24	1289.0	1271.7	2.19
2010m9	1297.7	1100.7	21.02	1288.7	1275.0	1.73
2010m10	1302.8	1100.9	21.69	1293.5	1281.5	1.54
2010m11	1305.7	1100.1	22.11	1296	1285.2	1.4
2010m12	1302.5	1094.4	22.12	1293.1	1282.8	1.32
Log(PEN + 1)						
2010m1	938.6	840.2	8.51	933.5	933.3	0.02
2010m2	939.7	838.7	8.72	936.0	942.6	-0.57
2010m3	953.3	839.7	9.87	947.7	953.7	-0.53
2010m4	962.4	841.5	10.55	956.5	960.1	-0.32
2010m5	965.8	849.4	10.19	960.1	960.9	-0.07
2010m6	970.6	846.4	10.86	964.2	963.0	0.11
2010m7	971.4	843.9	11.13	964.5	964.8	-0.03
2010m8	970.5	838.6	11.49	964.2	961.8	0.22
2010m9	970.3	832.4	11.99	963.1	961.4	0.16
2010m10	964.5	828.6	11.76	957.3	960.7	-0.3
2010m11	970.2	828.9	12.24	962.6	963.1	-0.04
2010m12	970.1	827.4	12.32	960.9	964.2	-0.28
Other covariates						
Av. Share USD loans 2010	71.5	65.34	8.08	71.35	69.95	1.93
Av. PEN loans 2010 ('000 PEN)	1339.7	701.12	7.67	1106.9	1024.1	1.01
Av. USD loans 2010 ('000 PEN)	4362.1	1638.8	13.99	3114.1	2904.2	1.3
Av. YOY share USD loans 2010	-0.0063	-0.00762	0.38	-0.00585	-0.00647	0.17
Av. YOY USD loans 2010	18.7	-13.7	7.08	17.9	18.9	-0.25
Av. YOY PEN loans 2010	32.8	1.6	5.35	30.7	30.7	0.01
Av. YOY Total loans 2010	14.1	-5.4	12.12	13.8	16.8	-1.32
Share of Medium Firms	20.5	14.5	7.56	19.4	17.9	1.76
Share of Small Firms	76.3	83.7	-8.91	78.4	79.7	-1.5
Share of Exporters	20.5	14.4	7.65	19.6	18.1	1.78
Share of Importers	46.7	31.6	15	46.1	45.9	0.17

Table A.XII: Firm level regression of Capital Controls Exposure on Credit

This table presents the results of the regression shown in Equation 2 after excluding exporters and firms that have bought dollars with forwards or swap contracts. This is, this table replicates Table VII after excluding exporters and firms that could be hedging dollar liabilities. The first three columns ("Continuous exposure") use firm's exposure as the share of its debt that is borrowed from treated bank in December 2010. The last three columns use as exposure whether a firm's debt with treated banks in December 2010 was above or below the median. Standard errors are in parenthesis. Standard errors have been clustered by firm and date. ***, ** and * denote significance at 1%, 5% and 10% respectively. The sample period goes from January 2010 to December 2011, where the capital controls announcement was made in January 2011.

	Continuous Exposure			Above / Below Median Exposure		
	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$	Log(USD+1)$\times$100	Log(PEN+1)$\times$100	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$	Log(USD+1)$\times$100	Log(PEN+1)$\times$100
Post CC * Exposure	0.698	20.01*	3.971	0.189	12.36*	6.359
	(0.930)	(11.63)	(15.43)	(0.634)	(7.479)	(9.677)
Exposure	4.957***	-51.30***	-222.5***	-2.448**	-12.50	40.85***
	(1.827)	(19.57)	(27.16)	(1.127)	(11.08)	(15.79)
Exposure to Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Firm Size * Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	163690	163690	163690	163690	163690	163690
Adjusted R2	0.163	0.116	0.134	0.162	0.116	0.123
N Firm Cluster	5251	5251	5251	5251	5251	5251

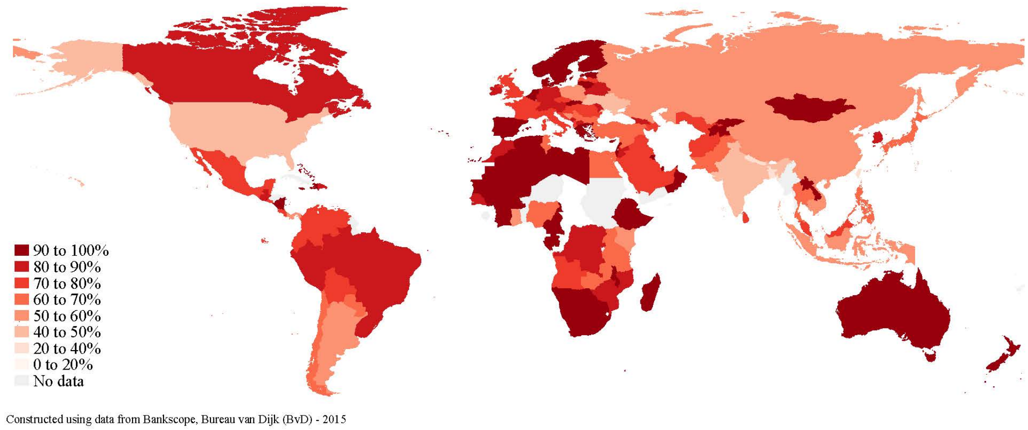


Figure A.1: Five Bank Asset Concentration (2015)

This figure has been constructed from World Bank data, which uses Bankscope, Bureau van Dijk. It shows the percentage of the banks' system assets that are held by the largest five banks.

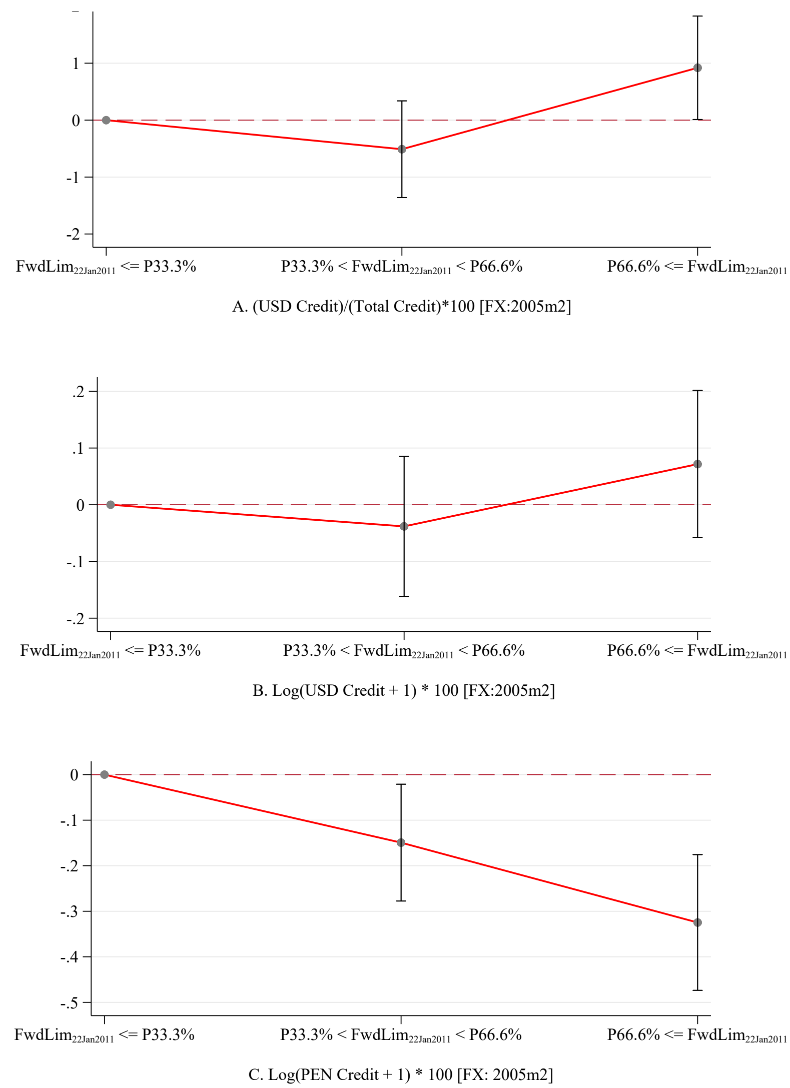


Figure A.2: Testing Monotonicity of Capital Controls on Credit

Figure A.2 plots coefficients similar to β_3 in Equation 1, but where the dummy CC_b has been split into tertiles. Specifically, it plots the $\widehat{\gamma_\tau}$ coefficient of the following regression:

$$
\begin{aligned}
y_{b,f,t} = & \beta_0 + \beta_1 \text{Post CC}_t + \beta_\tau \sum_{\tau=2}^{\tau=3} \mathbb{1}\left[b \in Tercile = \tau\right] + \gamma_\tau \sum_{\tau=2}^{\tau=3} \mathbb{1}\left[b \in Tercile = \tau\right] \times \text{Post CC} + \text{Firm} * \text{Date FE} \\
& + \Gamma X_b + \Psi X_{b,f} + \upsilon_{b,f,t}
\end{aligned}
$$

where $y_{b,f,t}$ is the share of dollar credit in Panel A, the log of credit in dollars in Panel B and the log of credit in soles in Panel C. $\mathbb{1}\left[b \in Tercile = \tau\right]$ is an indicator function that takes 1 when the bank b was located in the tercile τ with regards to its forward limit at the time of the capital controls announcement. The terciles represent how binding the capital controls were for each bank. The third tercile is the most binding. The first tercile has been omitted due to collinearity. Post CC is a dummy that is equal to 1 after December 2010 and 0 before. The coefficient of interest, $\widehat{\gamma_\tau}$ measures how much greater change in credit a bank that is in the second or third tercile provides with respect to the banks in the first tercile after the imposition of the capital controls. The regression controls for firm$\times$date fixed effects and bank and bank-firm relationship controls.

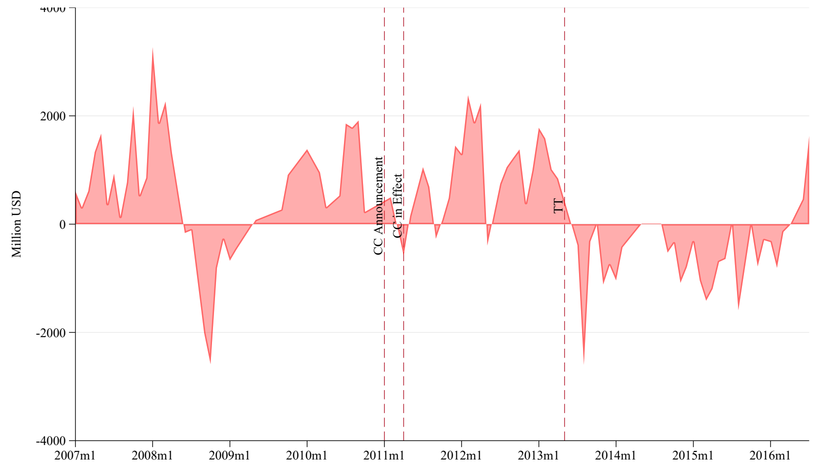


Figure A.3: Peruvian Central Bank's Exchange Rate Interventions

This plot shows the Peruvian Central Bank's spot interventions in the dollar-sol exchange rate market. The negative numbers mean that the Central Bank was selling dollars and buying soles, while the positive numbers represent the Central Bank's purchases of dollars against soles.

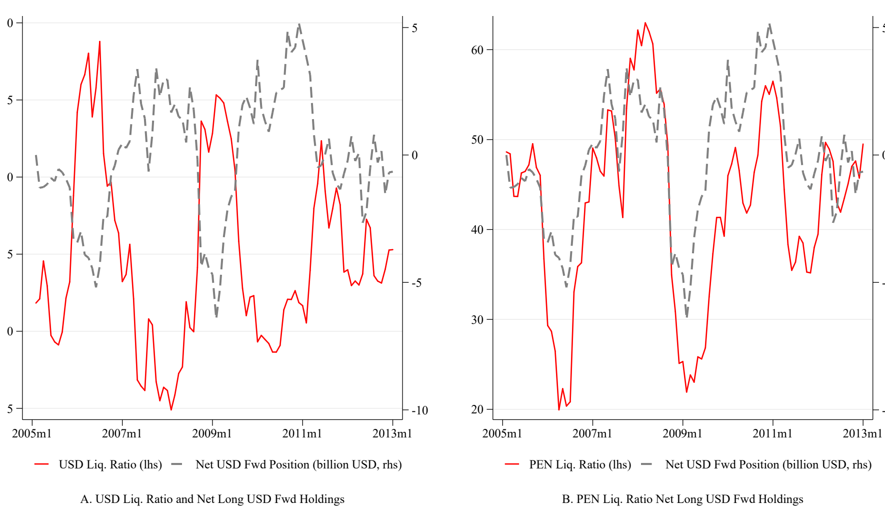


Figure A.4: USD and PEN Liquidity Ratios and USD Forward holdings

This figure juxtaposes the net long USD forward position that local banks have and the average liquidity ratios in PEN and USD. The USD (PEN) liquidity ratio is computed as the ratio of short term USD (PEN) assets and short term USD (PEN) liabilities. Forward contracts are not part of the liquidity ratio.In this figure, Panel A shows the local banks forward position and USD liquidity ratio. Panel B does the same but using PEN liquidity ratio.