Prudential Capital Controls and Risk Misallocation: Bank Lending Channel

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I identify a novel impact of managing capital flows in emerging markets: Prudential capital controls encourage domestic firms to take more dollar liabilities. This occurs because banks in emerging markets have a fundamental risk problem: households save partially in dollars while firms borrow in local currency. Absent capital controls, banks hedge the associated currency risk with foreign investors. When capital controls are present, banks respond by lending in dollars to domestic firms. I exploit heterogeneity in the strictness of capital controls across Peruvian banks to provide causal evidence of this mechanism and show that it has sizable effects on employment.

Key words: capital controls, macroprudential policies, employment, emerging markets, carry trade, corporate debt, currency risk, bank regulation, bank lending

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I. Introduction

Since World War I, economists have been debating whether countries should set capital controls to regulate international capital flows (Eichengreen, 2008). For nearly 50 years, since the 1970s, the common view has been that capital controls hindered economic growth because they prevented the efficient allocation of capital flows. However, spurred by the 2008 financial crisis, a broad body of research on macroprudential regulation has emerged and with that, a new consensus[1] on capital controls has followed. According to this new consensus, capital controls can be used as a macroprudential policy tool to prevent crises because, by limiting capital inflows, capital controls limit sudden slowdowns or reversals of capital inflows (sudden stops). This view is particularly important after the 2008 financial crisis because, as the low dollar rates attracted short term inflows into developing economies, the probability of sudden stops increased in these economies.[2]

The higher probability of sudden stops is worsened by a scenario in which countries have become more sensitive to sudden stops. Recently, as firms and banks outside the US have been increasing their dollar liabilities, [3] economists are concerned about the economic consequences of a large depreciation. As sudden stops generate large depreciations, sudden stops directly hurt firms and banks outside the US borrowing in dollars.

However, despite the consensus that countries should implement capital controls and the increasing concern about dollarization of banks and firms' liabilities, there is no research linking dollarization to the introduction of capital controls. This paper addresses this void by investigating the effect of capital controls on firms' dollar leverage in an emerging market context. To the best of my knowledge, this is the first paper showing that capital controls on inflows increase dollarization of firms' debt and that this debt dollarization leads to large reductions in employment after a sudden stop. I describe the channel through which capital controls induce firms to take dollar debt and exploit the implementation of capital controls in Peru as a natural experiment to provide evidence that this new channel/mechanism is at work.

[1] 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 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

[2] See Byrne and Fiess (2011), Fratzscher et al. (2012), Ahmed and Zlate (2014), Ghosh et al. (2014)

[3] Dollar liabilities for non-US banks reached $ 10 trillion (Shin (2012), Bruno and Shin (2015), Ivashina et al. (2015), McCauley et al. (2015)). This magnitude is comparable to the dollar liabilities of US banks.

The potential for capital controls to increase dollarization of firms' debt relies on the following observation: Banks in emerging economies have a fundamental risk management problem. While domestic households want to save partially in dollars to hedge against inflation (Catão and Terrones, 2016a), firms that want to match the currency denomination of their revenues need to borrow in domestic currency. As banks intermediate between firms and households, banks are therefore naturally exposed to exchange rate risk. As a result, bank regulators usually require banks to hedge exchange rate risk (Canta et al., 2006). When capital controls are absent, banks can hedge this currency risk with foreign investors by taking positions in the currency forward market. However, when capital controls are present and limit forward transactions, banks can no longer do this. As banks need to match the two sides of the balance sheet, banks could respond to capital controls by (1) lending more in dollars and (2) lending less in domestic currency.[4]

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 short term capital inflows. These inflows aimed at earning the interest rate differential between Peru's currency, soles, and dollars (a strategy named carry trade). Because foreign investors can engage in carry trade by either buying domestic short term bonds or by acquiring forward securities, for capital controls on carry trade inflows to work, they must block both channels. Following this rationale, capital controls in Peru consisted of (1) preventing foreign investors from buying short term securities and (2) setting caps on holdings of forward contracts.[5]

The way the limits on forward contracts were implemented provides identification for my empirical strategy. Because foreign investors took forward positions against local banks, caps were imposed on local banks' forward holdings. Given 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, the treated banks, were forced to reduce their forward holdings because their holding positions were above the cap. The rest, the control group, was not affected by the cap and could even increase their forward holdings. Exploiting the variation in the use of forward limits across banks, I identify the effect of capital controls on banks' lending pattern of soles and dollars. I use difference-in-differences to compare differences in treated banks' lending

[4]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 always 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.

[5]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.

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 show 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 in the treatment group. That is, 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 trend assumption holds). Although this condition is untestable, I perform various checks that suggest this condition is valid. These checks include testing balance on observables and pre-trends as well as exploring possible explanations for pre-existing dispersion of forward holdings. Third, capital controls should be exogenous to prevent that my results capture 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. I make this assumption.

To implement my identification strategy, I rely on unique, confidential data. My data include the universe of forward contracts of all Peruvian banks as well as details on their commercial credit activities from the Peruvian bank regulator. Hence, I observe the percentage utilization of each bank's forward limit as well as the lending activities between banks and firms across time.

Using these data, I test how capital controls affect banks' lending. I find that treated banks lent 10-20% more in dollars and 10-20% less in soles in the year following capital controls. These changes in loans had long lasting effects in the balance of soles and dollar loans (more than 2 years).

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 find how capital controls affected

[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.

[7] For example, if capital controls induce treated banks to lend more in dollars, a firm which wants to borrow in soles can go to a bank in the control group to get a loan in soles. Therefore, if firms were not allowed to substitute across banks, such as in the case where capital controls apply to all banks, the effect that capital controls would have on firm outcomes would be even larger than the ones I show in this paper.

dollarization of firms' debt, 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. Among firms of the same size and in the same industry, I find that firms with the highest dependence on treated banks, those firms whose share of debt with treated banks is above the median, borrowed 17-26% more in dollars than firms below the median.

Although I find that capital controls induce firms to borrow more in dollars, this finding does not necessarily mean that firms have more exchange rate risk. If only firms that have revenues in dollars (such as exporters) are borrowing more in dollars, then these firms will not have greater exchange rate risk because their dollar liabilities are matched with dollar assets. However, I show that my results are not driven by exporters. Moreover, the firms borrowing in dollars were not hedging dollar loans with derivatives contracts. Therefore, treated banks responded to the imposition of capital controls by shifting dollar liabilities to firms without dollar assets and hence, increasing exchange rate risk for these firms.

Absent of implicit government guarantees, such as government interventions to prevent exchange rate from depreciating and firms from defaulting on their dollar loans, greater exchange rate risk for firms implies higher probability of non-performing loans for banks when the domestic currency depreciates. Firms will have difficulties repaying their dollar loans because their revenues, denominated in domestic currency, are now worth less in dollar terms. Then, the finding that banks seem to be ignoring the risk of greater non-performing loans suggests that implicit government guarantees are plausibly in place (Burnside et al. (2001), Schneider and Tornell (2004)). This is likely as the Peruvian Central Bank intervenes frequently in the exchange rate market to minimize volatility (Rossini and Quispe, 2014). Then, treated banks could opt to increase dollar loans to firms that do not have dollar revenues rather than to invest in other dollar assets, such as Treasury bonds.

Similarly, for firms to take additional exchange rate risk, it must be treated banks must have been decreasing dollar rates or increasing soles rates to induce firms to borrow more in dollars. Unfortunately I do not have the interest rates at which loans were granted to test whether this holds. However, I find that as banks reduce their forward holdings (which is what capital controls force treated banks to do), the dollar interbank interest rate decreases while soles interbank rate increases.[8] This correlation, in addition to the evidence that treated

[8]More specifically, to isolate interest rate movements from movements in Libor or in Peru's Central Bank target rate, these correlations use the spread between the dollar interbank interest rate and Libor and the spread between soles interbank rate and Peru's Central Bank target rate.

banks are able to increase dollar lending with respect to control banks suggest treated banks decreased dollar lending rates and/or increased soles lending rates in contrast to control banks.

Finally, having found that capital controls increase firms' exchange rate risk, I proceed to assess the economic importance of my findings by quantifying how this greater exchange rate risk affects employment after a depreciation shock. Using monthly employment data for all firms in Peru, I estimate the changes in employment that occur to firms borrowing only from treated banks (relative to firms borrowing also from control banks) after a depreciation shock.[9] I control for importers and exporters and only compare firms in the same industry and of the same size. I find that firms borrowing only from treated banks decreased total employment by 6-7%. Among workers, 11% of permanent workers lost their job but this was partly compensated by hiring more temporary/ outsourced workers. This suggests that the greater dollarization of treated firms' debt made them financially constrained after the depreciation shock. Then firms partially substituted permanent workers for temporary ones as temporary workers are usually cheaper as firms do not have to provide them benefits such as vacations, health or unemployment insurance.

I.A. Related Literature

This paper connects various strands of literature in finance and macroeconomics, including research on (1) capital controls and macroprudential policies, (2) risk management, (3) intermediary asset pricing and (4) propagation of financial frictions on firms' real outcomes.

1. Capital Controls and Macroprudential Policies: This paper contributes to the capital controls and macroprudential policies literature by being the first paper to show that capital controls increase dollarization of firms' debt. However, although there is no research on the effect of capital controls on dollarization of firms' debt, there is a wide literature on capital controls. The literature shows there are both positive and negative sides to implementing capital controls.

On the positive side, previous research has shown that capital controls allow central banks to have independent monetary policy.[10] Literature has also shown that capital controls decrease overborrowing[11] and mitigate financial instability.[12]

[9] The depreciation shock I study occurred after May 2013, when the Federal Reserve announced the gradual withdrawal of liquidity (taper tantrum) and there was a sudden outflow of capital across emerging economies.

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

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

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

On the negative side, some of these benefits do not seem to hold broadly: 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. Furthermore, capital controls are not only hard to implement, but they increase the cost of funding for firms (Forbes (2005), Desai et al. (2006), Forbes (2007)), in particular for smaller firms (Alfaro et al. (2017)).

More broadly, this paper also complements Ahnert et al. (2018), who show that other macroprudential policies can induce firms to issue greater foreign currency debt.

2. Risk Management: This paper also contributes to the risk management literature by adding further evidence that greater access to capital markets can help economies hedge risks. Similar benefits from capital market de-regulation and global risk sharing are shown in Chari and Henry (2004), Gourinchas et al. (2010) and Varela (2015) and Maggiori (2017).

3. Intermediary Asset Pricing: The effect that capital controls could have on interest rates speaks directly to asset pricing literature. I show that limiting forward positions of intermediaries (banks) seems to impact interest rates faced in the economy, highlighting, as in Du et al. (2016), the importance that bank regulation has in explaining price dynamics.

4. Financial Frictions and Real Outcomes: Finally, this paper also adds to the growing literature on the effect of financial frictions on real outcomes by showing that banks respond to capital controls by shifting exchange rate risk to firms. This ultimately affects real outcomes as the greater exchange rate risk forces firms to reduce employment after a depreciation shock. Examples of other papers that study how other financial frictions affect firms include Bentolila et al. (2013), Greenstone et al. (2014), Duygan-Bump et al. (2015), and Bottero et al. (2015), Benmelech et al. (2016) and Cingano et al. (2016).

I.B. Outline of the paper

In Section II, I present background information on capital controls. I use this information to show a possible channel through which capital controls can affect banks' lending decisions in Section III. Starting from Section IV, I turn to test the channel outlined in Section III. I start by presenting the data in Section IV. Then, in Section V I discuss the identification strategy and show that banks affected by the capital controls (treated banks) responded to capital controls by lending more in dollars and less in domestic currency, regardless of whether the firm has dollar revenues (exporter) or hedges dollar liabilities using financial instruments. As

showing that capital controls affect banks' lending is the main contribution of this paper, Section V is the most important section. Starting at Section VI I shift attention away from banks and onto firms. In Section VI I show that the greater dollar lending from treated banks increased firms' exposure to the exchange rate and in Section VII I show that because of the greater exposure to the exchange rate that firms borrowing from treated banks reduced employment after a depreciation shock. Finally, in Section VIII 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 IX 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

For capital controls to be effective in decreasing carry trade inflows, capital controls must block all channels through which carry trade is done. There are two alternative ways in which foreign investors engage in carry trade.

When there are carry trade inflows, investors want to earn the interest rate differential between the dollar and the emerging market currency. To do this, the first alternative foreign investors have consists on borrowing dollars, buying emerging market 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. Therefore, capital controls must block the bond and forward channel.

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 can 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.[13] 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 2 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.

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,[14] 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).[15] The announcement of 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

[13] 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.

[14] 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.

[15] 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 if capital controls discriminate based on residency. Therefore, my definition of capital controls 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.

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.

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

As I study how capital controls affect bank's 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 VIII, 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 1992 and 2017. Deposit and loan dollarization were both above 70% in early 1990s as the economy had experienced persistent high inflation since 1970s and hyperinflation in the late 1980s (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. Then, given that banks were already providing dollar loans to firms, this paper studies 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. Specifically, this figure shows that although banks can have dollar holdings with forward contracts (gray line), their net exchange rate exposure (sum of their dollar holdings in forwards and their dollar holdings in spot - the red line-) this is practically null. In other words, when banks buy dollars using forward contracts, they sell dollars in the spot market.

III. Possible Channel Though Which Capital Controls Could Affect Bank Lending

Why could capital controls change local banks' lending behavior in foreign and domestic currency in a context such as the one described in the previous section? Using a toy example, this section illustrates a possible channel through which capital controls can affect banks' lending decisions and formalizes this channel by building a model in Appendix A.I. This exercise allows me to draw predictions that guide the empirical strategy and that I test in the next sections.

The setting I present assumes (1) banks have dollar liabilities (deposits) and (2) banks hedge the currency mismatch between assets and liabilities. These assumptions are consistent with the environment described in Section II in which Peruvian banks operate and therefore allow me to draw predictions that can be tested in the rest of the paper. Although the empirical analysis is grounded in Peru, in Section VIII I show that in various emerging markets banks hold dollar liabilities and are forced to hedge currency mismatches. Hence, the analysis presented in this section can be extrapolated to studying different emerging economies.

Using a setting in which the two previous assumptions hold, I consider what happens when capital controls on carry trade inflows are set. A scenario in which capital controls work implies that the bond channel described in Section II.A is blocked and banks have restrictions on the forward market. As Peru set limits to forward positions of banks after it had already blocked the bond channel, in this section I take as given that the bond channel is blocked and study the effect of limiting forward holdings of banks.

III.A. Toy example showing capital controls: (1) Increase firms' foreign currency loans (2) Decrease firms' domestic currency loans

What could be the consequences of setting capital controls on dollarization of firms' loans when (1) banks have dollar liabilities and (2) banks hedge the currency mismatch between assets and liabilities? A possible consequence is that capital controls induce firms to substitute debt in domestic currency for foreign currency. Figure 4 shows a toy example to capture the basic intuition underlying this hypothesis.

Figure 4 illustrates an environment in which an emerging economy bank has 100 dollar liabilities which the bank receives from domestic households. The bank has two options: lend dollars (abbreviated as USD in Figure 4) or lend domestic currency, "soles", to firms (abbreviated as PEN in Figure 4).[16] For simplicity,

[16] In this example for simplicity, I am considering the bank only has dollar deposits. However, in Peru and other emerging markets banks also have domestic currency deposits. Adding soles deposits to this example does not change at all any of the consequences or intuition so I abstract from adding soles deposits to simplify the example and notation. This means that to be hedged, the bank will only lend dollars. If the bank also had soles deposits, the bank can lend the soles deposits in soles and the dollar deposits in dollars without altering any of the intuition of the example.

this economy's interest rates in both currencies are zero and its spot and forward exchange rate is 2 soles per dollar.

Assuming the bank regulator does not allow the bank to have exchange rate risk, the bank has two options to hedge this risk. First, as shown in Panel A, the bank can lend firms 100 dollars. Second, as displayed in Panel B, the bank can commit to buy 100 dollars in the future at a predetermined exchange rate of 2 soles per dollar.

Under the first option (Panel A), when the bank lends 100 dollars to firms, the bank will not have exchange rate risk because there is no currency mismatch between the bank's assets and its liabilities. However, if the bank converts 100 dollars into 200 soles, the bank has exchange rate risk because the bank owes dollars to households but will receive soles from firms when the loan matures.

On the other hand, under the second option (Panel B), the bank enters a contract with foreign investors to buy 100 dollars against 200 soles when the dollar deposit matures. In the context of capital inflows, as the one addressed in this paper, foreign investors want to have assets in soles and liabilities in dollars.[17] Then, as the bank needs dollar assets and foreign investors want dollar liabilities, foreign inverstors allow the bank to hedge its dollar deposits. Given that there is no exchange of cashflows at inception of the forward contract, the bank can dispose of 100 dollar deposits to lend. As the bank hedged its dollar deposits with forward contracts, the bank will then lend only in soles. The bank does not have exchange rate risk because, as Panel B shows, the bank's assets match the bank's liabilities in each currency.

However, when capital controls on carry trade inflows are present, the second option is constrained. This is shown in Panel C of Figure 4. In this case, capital controls limit the forward holdings of local banks to 25 dollars. Then, given that the bank can only hedge 25 dollars with forward contracts, the bank needs to generate 75 dollar assets to hedge exchange rate risk. The bank will then lend 75 dollars to firms. The remaining 25 dollar deposits are exchanged to soles and lent in soles. Two predictions arise when comparing this outcome with the one in Panel B, Figure 4: (1) loans in soles drop and (2) loans in dollars increase when the country sets capital controls. The model in Appendix A.I rationalizes these predictions using a similar setting to this toy example.

I test these two predictions in Section V and later on, in Section VII, I discuss the effects on employment.

[17] Foreign investors want assets in soles and liability in dollars when there are inflows. This is the setting considered in the paper because it is only in this situation that capital controls on inflows are binding. Although inflows occur because the interest rate differential between soles and dollars makes foreign investors want to engage in this investment strategy, for simplicity, in this example I am abstracting from interest rates. Incorporating interest rates will still make my argument hold, but the bank needs to hedge the future value of the deposits.

IV. 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 employment.

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 February 2005 to October 2015 and is recorded at the firm-bank-month level. However, given that the Peruvian government introduced regulations that attempted to reduce dollar bank loans in 2013, to study the effect of the 2011 forward limits on bank lending I only consider the sample until December 2012. 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 I 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.[18]

Panel B of Table I 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.[19] For simplicity, I refer to the medium firms as "small", the large as "medium" and corporate firms as "large firms".

[18] 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). For details on the differences between banks and other financial institutions, see the SBS law 26702.

[19] 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 which 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.

Finally, Panel C of Table I shows the summary statistics collapsed at the bank-firm level. An important aspect of Panel C of Table I 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 V.B, this will help to isolate demand for credit from supply of credit (Khwaja and Mian, 2008).

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 I, Panel A, shows that on the last reporting date available before the announcement of capital controls (2 days before the announcement), banks were using 49% of their forward limit on average, with a standard deviation of 52%.

Bank Information Used as Bank Controls: As will be discussed in Section V.B, I condition on different bank characteristics (deposits, assets, profitability and liquidity) when estimating the effect of capital controls on credit supply. 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.[20]

Exporter and Importer Data: The firms that will be affected under a depreciation shock when borrowing in dollars are those which 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 and the sample provided was from January 2007 to September 2016.

Employment Data: Finally, to study the effect of capital controls on employment, I use the monthly employment data collected by the Tax Revenue Agency (SUNAT) for all firms operating in Peru. Although only employment in the previous 12 months is available in the SUNAT's website, the SUNAT provided me with monthly employment data between January 2007 and December 2016 for all firms. As the firms are identified using its unique tax identifier, I could merge it with the credit data from the SBS.

[20]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.

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

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

Using the data presented in Section IV, 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 should be conducted to have identification of the effect of capital controls on bank lending. I argue that Peru offers a setting close to the second best ideal one and therefore, proceed to explain the identification strategy, its validity and results. As the main contribution of this paper is to show that local banks respond to capital controls on inflows by substituting domestic currency lending for dollar lending, this is the most important section of the paper.

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

Ideally, to estimate the direct effect of capital controls on banks' supply of dollar and domestic currency loans, one would randomly assign capital controls across countries and compare outcomes of countries with and without capital controls. Unfortunately, this setup is not possible. A second best alternative would then be to randomly assign capital controls across banks within one country and then compare the outcomes of banks affected by capital controls to those that were not. This is a second best because this comparison is across banks and hence, abstains from general equilibrium effects that occur at the country level.

Nonetheless, given that countries do not set capital controls randomly and that there is no reliable counterfactual at the country level, I use a setup that resembles the second best experiment to identify the effect of capital controls on bank lending. The setup I use is the imposition of capital controls on carry trade inflows in Peru.

The similarity between using the introduction of capital controls in Peru and the environment in which capital controls are randomly assigned across banks is that, in Peru, capital controls affected local banks in different degrees. There were banks, which I refer to as treated banks, that were forced to reduce their forward holdings because they were surpassing the imposed limit at the time capital controls were announced. In contrast, the rest of banks, the control banks, could even increase their forward holdings (up to the regulatory limit) because they were below their limit when capital controls were announced. Hence,

capital controls did not affect all banks in the same way as the control banks were less constrained than the treated banks. [21]

However, 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 V.C discusses these conditions and shows evidence that Peru offers a setup close to the second ideal experiment.

V.B. Methodology

Having the imposition of capital controls in Peru as 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[22] as a proxy for the intensity of capital controls treatment. Then, I define treated banks as those which were forced to reduce their forward holdings because they were using more than 100% of their limit.[23] The rest of banks are in the control group. Using these two groups of banks, I estimate the following regression specification:

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

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[24] (2) The log(Credit in dollars + 1) and (3) The log(Credit in

[21] 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.

[22] The last reporting date before capital controls announcement was January 22nd 2011. Capital controls announcement was on January 24th 2011.

[23] 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.

[24] To compute this ratio, the credit in dollars has been converted to soles using the exchange rate of February 2005 across all time periods. This prevents mechanical changes in the dollar debt ratio due to changes in the exchange rate.

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 controls (X_b) and bank-firm relationship controls ($X_{b,f}$). The set of bank controls, X_b, include $\frac{\text{Soles Deposits}}{\text{Total Assets}}$, $\frac{\text{Dollar Deposits}}{\text{Total Assets}}$, log(total assets), return over assets (ROA) and dollar and soles liquidity ratios. I take the December 2010 values (pre capital controls) for all these variables. On the other hand, the bank-firm relationship controls, $X_{b,f}$, resume specific bank-firm (observable) relationship factors. These controls are composed of the length of the relationship[25] 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.

Finally, given that I am interested in capturing the effect of capital controls on banks' lending behavior, that is, on banks' supply of loans, I need to disentangle banks' supply of loans from firms' demand for them. For this reason, I include firm$\times$ date fixed effects (Khwaja and Mian, 2008). Adding these 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.[26]

Although having firms that borrow from both treated and control banks enables me to separate credit demand from supply, it also makes the results of the effects of capital controls on bank lending to differ from those on total firms' borrowing. 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 not have increased their overall dollar borrowing because they could be borrowing more from treated banks but to repay pending debts with control banks. Then, Section VI aggregates the analysis to the firm level and studies the effect of capital controls on firms' overall exchange rate exposure.

[25] This 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

[26] 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.

V.C. Validity

As mentioned in Section V.A, the identification strategy presented in the previous section only resembles that randomly assigning capital controls across banks when the following three conditions hold. First, 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. Then, 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 5, 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 on 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, Peruvian banks' balance sheet characteristics are shown in Table II. 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 terms of profitability, assets, liquidity ratios and share of dollar and soles deposits. However, the t-statistics of Table II have to be taken with caution as, similar to other papers studying banks, the banking system tends to be a very concentrated industry with very few large banks.[27]

Second, Figure 6 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 capital controls' announcement, 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. Then, the changes in the share of dollar lending shown in Figure 6 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.[28] The regression specification with leads and lags allows to analyze whether the treatment

[27] In this case, the largest four banks (2 of which are in the control group and 2 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.2 in the Appendix. This figure shows that the assets of the five biggest banks as a share of total banking assets around the world is generally greater than 50%.

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

where y is the outcome variable. This regression is similar to Equation (1). However, instead of having a unique coefficient associated 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 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 December 2010 ($\tau = 0$), which is the date in which capital controls were announced.

effect changes across time and whether there were already differences between the treatment and control group that are not accounted in the bank controls nor firm×date fixed effects.

Figure 7 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. 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 which have 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 were the two groups of banks' lending trends similar in the pre-capital controls period but different starting exactly at the introduction of capital controls.

To mitigate this concern, I exploit 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, to analyze whether the forward holdings of banks came from the inelastic demand of counterparties to trade with the same bank they have been doing so rather than an unobserved factor that could explain the changes in bank lending post capital controls.[29] If this is the case, and the local banks are only intermediaries, then their initial holdings of forward contracts could be mostly determined by the actions of the counterparties.

Table III 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 he did in the previous trade.[30]

[29] Relationship stickiness have 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 percent point higher probability of being a new lead lender for a new loan.

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

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

Then, given that local banks use the spot market to unwind forward holdings (as shown in Figure 3), 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.

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 credit and subsequent employment effect could be due to the economic conditions to which 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, $\widehat{\beta}_3$ 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.[31]

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). For instance, a common change in economic conditions after the introduction of capital controls relates to foreign investors' appetite for Peruvian currency. In private conversations with local banks' traders, they argued that the introduction of

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.

[31] 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 8). 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.

capital controls reduced foreign investors' demand for soles. Foreign investors feared that the introduction of capital controls meant future regulatory changes in the exchange rate market, as well as signaled that the Central Bank could be more aggressive in preventing the soles from appreciating. Given that foreign investors buying soles in the forward market profit when the soles appreciates, fears that the Central Bank could intervene more heavily to prevent the soles appreciation deterred them partially from further soles purchases. The result of this is that, as seen in Figure 5, banks forward holdings declined for both control and treated banks, though for treated banks declined further as they need to comply with the regulatory limit.

Moreover, studying the effect of capital controls on a very narrow window (January 2010 to December 2011) reduces the possibility of having additional factors affecting credit that could be confused with capital controls.

V.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 V.E.

Table IV 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 and bank-firm relationship controls, as well as without firm$\times$date fixed effects. The second column of each dependent variable adds bank controls and bank-firm relationship controls. The third column shows the regression controlling for demand effects but without adding any bank or bank-firm relationship controls, while the fourth column shows the regression that fully controls for bank 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 IV) are stable. First, regardless of the specification, Table IV shows that the share of dollar loans (columns 1-4) increased for treated banks compared to non-treated 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 nor accounting for credit demand effects.

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

The results of Table IV 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. [32]

Moreover, Table A.I in the Appendix shows that the results are invariant to excluding exporter firms (those who export more than 100,000 dollars per month). Then, 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 which 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 which bought dollars using either forward contracts or cross currency swaps between 2011 and 2013. The assumption is that firms which 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.II in the Appendix presents the results and shows that the results from Table IV 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.

[32] This is shown in Table A.III in the Appendix, where I repeat the analysis of Table IV 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 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.

The results after excluding exporters and firms with hedging instruments are consistent with the findings on the effect of capital controls on employment that I present in Section VII. In that section I show that firms that were borrowing from treated banks at the time of the capital controls announcement and that increased dollar borrowing, decreased employment after a depreciation shock. This would not be the case if treated banks were lending to hedged firms given that if so, firms might not need to decrease employment after a depreciation shock.

V.E. Robustness

To further confirm the validity of my results, I perform several robustness checks that are found in the Appendix A.I.C. 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 on redoing the previous analysis using the four largest banks as these banks are more homogeneous than including all banks. Table A.V in the Appendix shows that restricting the sample to only the largest four banks yields very similar results to the ones discussed in Section V.D. This is also the case when dividing the sample by firm size.

The second check consists on 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. This exercise simulates as if capital controls had been announced one year earlier (i.e. January 2010) but keeping 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 on 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 V.D get stronger as capital controls bind more. Indeed this is the case. [33]

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)

[33] 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 of 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.3 in the Appendix.

the percentage use of forward limit as of January 2011 as a continuous variable and (2) 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 capital controls announcement. As shown in Table A.X in the Appendix, my results continue to hold with these specifications.[34]

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.IX in the Appendix shows my results remain significant at the 10% level, with most of them remaining significant at the 5% level.

A second concern regarding standard errors is that due to 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 a same date. This cross-section correlation across firms and banks can be accounted for by clustering by date. When doing so, Table A.VII 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.VIII 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.

V.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 turn to 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

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

attractive than borrowing soles. Then 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. Then, 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. Absent of 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 to 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 to 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 9 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 1 month libor.[35] 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 9), which forces banks to decrease their forward holdings, dollar interbank rates were decreasing while soles rates increasing.

V.G. Discussion

Up to now I have presented multiple evidence that capital controls induce treated banks to substitute soles lending for dollar lending and that this substitution occurred regardless of whether the firm that banks are lending to has dollar assets to hedge dollar liabilities. Then, a natural question that arises is why would banks

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

lend dollars to firms whose revenues are in domestic currency and that would face financial constraints in the event of a depreciation shock?

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)). Then, if banks consider the government will bailout 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 be explaining the banks' risk taking behavior.

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 shown consistently shown concern about the exchange rate. Such is the case, that the Central Bank publicly states that they intervene in the exchange rate market is to prevent a financial crisis in the event that a depreciation shock significantly increases non-performing loans (Rossini and Quispe, 2014).[36] 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.[37] Finally, the purchases of dollars that the Central Bank did trying to slow down the currency appreciation during inflows, made the Central Bank accumulate a large stock of foreign reserves (reached 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.

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

From this section onwards, 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. In the next section, this analysis is relevant to understand how a large depreciation shock driven by sudden outflows affects employment of those firms borrowing from treated banks.

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

[37] Figure A.4 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

Studying the effect of capital controls on firms' total debt dollarization does not derive directly from the results ontrols 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 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. This 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 on two groups based on this measure: those which have an above median exposure ("Exposed firm" = 1) to treated banks, and those below the median ("Exposed firm" = 0).

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

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

Similar to the regressions in Section V.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 V.B, though, is that in this case, these variables are at the firm (f) and month (t) level, while in Section V.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. 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×industry×month fixed effect.

Although adding firm size×industry×month fixed effect helps in making "more exposed" and "less exposed" firms more comparable, there could be various factors at the firm level that explain the changes in their loans. Then, unlike the results presented in the previous section, this regression is not causal. Furthermore, lack of firms' balance sheet data limit 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.

Nonetheless, though 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.

Table V presents the results of Equation (2). The first three columns use whether a firm's exposure (share of its debt that is borrowed from 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 17-26%. Then, 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. This caused the percentage of debt in dollars to increase by 100-200 basis points.

Moreover, as seen in Table A.XI in the Appendix, the results remain stable when redoing the previous analysis 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.

An important aspect to consider when interpreting these results is their relative magnitude in contrast to the first best ideal setting described in Section V.A. In the first best ideal experiment, capital controls are applied countrywide. Then, all banks in the country are treated and need to increase dollar lending to hedge their dollar liabilities. Not all firms, on the other hand, might find optimal to borrow more dollars given the exchange rate risk this action entails. However, unlike the environment studied in this paper where there are treated and control banks, firms cannot borrow more in soles by switching from treated to control banks as all banks in the ideal setup are treated. Therefore, because in this paper firms can switch between treated and control banks, firms can mitigate the effects of capital controls and so, my results on firms' outcomes are a *lower bound* to those that would have occurred if firms borrowing from treated banks could not turn to control banks for soles funding.

VII. Effect of Capital Controls on Employment

Having shown that capital controls increased firms exposure to the exchange rate, in order to assess the economic importance of capital controls, this section briefly studies how the dollarization of firms' debt

generated by capital controls can affect employment after a sudden outflow of capital that led to a depreciation shock. Although there is a wide literature showing the effects of depreciations on the financial conditions of firms that hold dollar debt,[38] the purpose of this section is to quantify the effects of capital controls on employment. Then, in contrast to other papers, I only examine debt dollarization that can be attributed to capital controls. For this, I compare how employment between firms borrowing from treated banks and those borrowing from control banks after a depreciation shock. I find that after a depreciation of 30% on the local currency, firms borrowing from treated banks decreased employment by 6-11% more than those not borrowing from treated banks.

A possible explanation for this decrease in employment is that given that capital controls increase firms' dollar borrowing but firms' revenues are in domestic currency, a depreciation shock impairs firms' capacity to repay their dollar debt. This could spur cost reductions, including worker layoffs.

A first glance at the employment data suggests that capital controls had a significant damaging effect on employment. I split firms that were only borrowing from treated banks as of December 2010 and those that also relied on other banks. Figure 10 juxtaposes the normalized moving average of employment (left hand side axis) for both sets of firms with the exchange rate (right hand side axis). This plot shows that when soles started to depreciate after the Taper Tantrum in May 2013 (blue area), firms that were only borrowing from treated banks at capital controls' announcement (treated firms -dotted gray line-), decreased employment. On the other hand, although the non-treated firms (red line) had a similar employment growth pattern as the treated firms before the Taper Tantrum, the non-treated firms continued to increase employment after the Taper Tantrum.[39]

To have a more rigorous assessment of the consequences of capital controls on employment, I use difference in differences to estimate how employment changed for firms which borrowed from treated banks after a depreciation shock. Specifically, I use the depreciation in emerging markets that followed the Federal Reserve's announcement on liquidity withdrawal in May 2013 (taper tantrum). In Peru's case, the soles depreciated 30% between May 2013 and October 2015.[40]

To implement this estimator, I sort firms by their exposure to capital controls before the depreciation shock. Noticing that capital controls have persistent effects (Figure 7), I sort firms into two groups: (1) those that borrowed in dollars because they were borrowing only from treated banks at the time of capital controls'

[38] See Krugman (1999), Feldstein (1999), Céspedes et al. (2004), Kim et al. (2015)

[39] The sample of firms only includes firms that have been functioning across all years. This prevents that the small number of workers that characterize firm entry to be confused with lower employment.

[40] I use October 2015 because this is the last period I have in the SBS credit dataset.

announcement and (2) those that did not borrow in dollars because they were not connected to banks exposed to capital controls. The difference in difference estimator then captures the effect of capital controls by comparing the change in employment from these two groups of firms. Having this, I regress:

$$\begin{aligned}\log(\mathrm{N})_{f,t} = &\theta_0 + \theta_1 \text{Firm Exposure}_f + \theta_2 \text{Firm Exposure}_f \times \text{Post TT}_t + \Gamma X_f^{bank} \\ &+ \text{Industry * Firm Size * Date FE} + ImporterFE + ExporterFE + \zeta_{f,t}\end{aligned} \tag{3}$$

where $log(N)$ refers to the monthly number of workers. The sample goes from January 2007 to October 2010. The firm exposure variable takes the value of 1 for those firms which borrowed only from treated banks at the time of capital controls' announcement, and zero otherwise. The variable "Post TT" is a dummy that is 0 before May 2013 and 1 after. X_f^{bank} refers to the specific bank variable controls (the same as in the previous sections) but weighted by the percentage of credit each firm had with that bank. The sum across all banks from which the firm was borrowing generates a firm level variable that summarizes the bank controls at which the firm was exposed to as of December 2010. Given that changes in the exchange rate could directly change exporters and importers revenues, I use importer and exporter fixed effects.[41] Finally, I control for industry$\times$firm size$\times$ date fixed effects.

However, these results should be taken as suggestive rather than as causal evidence of the effects of capital controls on employment. This is because, besides firm size and industry, I lack of reliable information on the firm side and, contrary to the analysis of capital controls on bank lending, I cannot use firm fixed effects to control for firm unobservable characteristics as this is a firm-level analysis.

The results of this regression, displayed in Table VI, show that firms borrowing from banks that were most exposed to capital controls on January 2011, decreased the total number of workers by 6-7% after May 2013, when the currency depreciated more than 30%. This result suggests that the depreciation tightened financial constraints for treated firms because capital controls induced banks with which the treated firms had relationship with to lend more in dollars.[42]

The change in treated firms' employment was not homogeneous across employers with different contracts. The decrease in employment is even larger when considering workers that had a permanent contract with the

[41] Given that small exports (imports) could introduce noise in the sample, I define an exporter (importer) as a firm that exports (imports) more than 100,000 dollars per month. However, the results are robust to including all sizes of exports (imports).

[42] Consistent with this, the treated firms increased dollar loans over the non-treated between the announcement of capital controls but before the taper tantrum, making treated firms more exposed to the exchange rate. This shown in Table VII. This regression only captures changes in lending for those firms that existed since 2007 because I took firms that existed throughout the sample to prevent that my results are driven by changes in the distribution of entry and exit of firms.

company (approximately 11%). The total employment effect is lower because the firms substituted workers that had permanent contracts (typically more expensive due to regulatory benefits) for outsourced workers (eg. contractors).

VIII. 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 Section III, 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 11, 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 (see Shin (2012), Ivashina et al. (2015)). This magnitude is such, that it is comparable to the total dollar liabilities banks in the US have. Dollarization of banks' liabilities is even worse in various developing countries (such as Peru) because its households save partially in dollars to hedge against against inflation and country instability.[43]

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

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

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 a list of 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.

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

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%.

Although the main contribution of this paper is to show dollarization of firm debt as novel impact of capital controls, to assess the importance of this finding, I analyze the implications of this dollarization on employment after a large depreciation episode that occurred due to a sudden stop. I find that capital controls decrease employment by 6-10% because given that domestic firms have revenues in domestic currency, the large depreciation made firms more financially constrained. This can induce firms to reduce costs by reducing employment.

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: 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 IV. 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 has been measured from 2005 to December 2010 and 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
Credit						
Ch PEN Credit (%)	10.16	5.29	27.60	-24.70	93.10	13
Ch. USD Credit (%, FX: 2005m2)	11.30	12.20	7.88	-9.58	21.98	11
Ch. Total Credit (%, FX: 2005m2)	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
Bank Controls						
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						
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$_{2010m12}$ (%, FX: 2005m2)	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						
Credit						
Ch. PEN Credit (%)	-7.79	-6.98	142.26	-178.94	168.06	12896
Ch. USD Credit (%, FX:2005m2)	-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, FX: 2005m2)	0.12	0.00	18.92	-20.11	22.08	23463
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 II: 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)$_{22\text{Jan}2011}$	26.37	10.00	123.55	3.00	-4.67	-97.17***
Credit						
Ch PEN Credit (%)	15.61	10.00	-8.00	3.00	1.34	23.61
Ch. USD Credit (%, FX: 2005m2)	10.04	8.00	14.66	3.00	-0.85	-4.62
Ch. Total Credit (%, FX: 2005m2)	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***
Bank Controls						
ROA$_{2010\text{m}12}$ (%)	0.02	10.00	0.01	3.00	0.67	0.01
Total Assets$_{2010\text{m}12}$ (Billion PEN)	12.82	10.00	16.76	3.00	-0.29	-3.94
Liq. Ratio PEN$_{2010\text{m}12}$ (%)	40.27	10.00	48.46	3.00	-0.70	-8.19
Liq. Ratio USD$_{2010\text{m}12}$ (%)	44.45	10.00	46.93	3.00	-0.35	-2.49
PEN dep./Assets$_{2010\text{m}12}$ (%)	39.79	10.00	30.78	3.00	1.12	9.00
USD dep./Assets$_{2010\text{m}12}$ (%)	23.70	10.00	35.82	3.00	-1.49	-12.12
Panel B. Main 4 Banks						
FX Forwards						
% Fwd Limit$_{22\text{Jan}2011}$	62.31	2.00	126.79	2.00	-2.81	-64.48
Credit						
Ch PEN Credit (%)	6.96	2.00	0.34	2.00	2.22	6.62
Ch. USD Credit (%, FX: 2005m2)	11.21	2.00	13.90	2.00	-1.81	-2.70
Ch. Total Credit (%, FX: 2005m2)	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
Bank Controls						
ROA$_{2010\text{m}12}$ (%)	0.02	2.00	0.03	2.00	-0.67	-0.00
Total Assets$_{2010\text{m}12}$ (Billion PEN)	52.45	2.00	23.41	2.00	1.91	29.03
Liq. Ratio PEN$_{2010\text{m}12}$ (%)	56.30	2.00	53.77	2.00	0.22	2.54
Liq. Ratio USD$_{2010\text{m}12}$ (%)	42.03	2.00	41.69	2.00	0.05	0.34
PEN dep./Assets$_{2010\text{m}12}$ (%)	35.05	2.00	30.14	2.00	2.25	4.90
USD dep./Assets$_{2010\text{m}12}$ (%)	31.53	2.00	32.82	2.00	-0.51	-1.29

Table III: 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 IV: Effect of Capital Controls on Credit Supply

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 and bank-firm relationship controls, as well as without Firm×Date fixed effects. The second column adds bank and bank-firm relationship controls. The third and fourth column display the results when using Firm×Date fixed effects, without and with bank 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 controls are: $\frac{\text{Soles Deposits}}{\text{Total Assets}}$, $\frac{\text{Dollar Deposits}}{\text{Total Assets}}$, log(Total Assets), Return over Assets (ROA) and dollar and soles Liquidity Ratios. All these use the December 2010 values. 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. T-statistics 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.

	$\frac{\text{USD Credit}}{\text{Total Credit}}$×100 [FX:2005m2]				Log(USD Credit + 1)×100 [FX:2005m2]				Log(PEN Credit + 1)×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***
	(1.56)	(3.14)	(3.92)	(3.83)	(1.87)	(1.99)	(4.65)	(2.07)	(-1.32)	(-3.48)	(-2.36)	(-4.23)
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***
	(18.15)	(11.43)	(13.38)	(11.80)	(4.48)	(2.00)	(-4.30)	(3.96)	(-34.13)	(-16.70)	(-34.21)	(-17.28)
Post CC	-2.201***	0.206	0	0	-24.54***	19.70***	0	0	24.75***	19.03***	0	0
	(-9.50)	(1.07)	(.)	(.)	(-7.97)	(7.75)	(.)	(.)	(8.75)	(7.16)	(.)	(.)
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

Table V: 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 use whether a firm's exposure (share of its debt that is borrowed from 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. T-statistics 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.

	Above / Below Median Exposure			Continuous Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$	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	1.194*	17.56**	2.138	2.089**	29.60**	-2.183
	(2.02)	(2.53)	(0.25)	(2.33)	(2.72)	(-0.17)
Exposure	9.077***	-18.37**	-253.0***	15.81***	-83.68***	-526.5***
	(13.29)	(-2.43)	(-25.32)	(14.27)	(-6.57)	(-32.70)
Industry * Firm Size * Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	152401	152401	152401	152401	152401	152401
Adjusted R2	0.133	0.141	0.143	0.140	0.143	0.198
N Date Cluster	24	24	24	24	24	24
N Firm Cluster	10859	10859	10859	10859	10859	10859

Table VI: Effect of Capital Controls on Employment

This table shows the regression results of Equation (3). The firm exposure variable takes the value of 1 for those firms which borrowed only from treated banks at the time of the capital controls announcement, and zero otherwise. The variable "Post TT" is a dummy that is 0 before May 2013 and 1 after. X_f^{bank} refers to the specific bank variable controls (the same as in the previous sections) but weighted by the percentage of credit each firm had with that bank. The sum across all banks from which the firm was borrowing generates a firm level variable that summarizes the bank controls at which the firm was exposed to as of December 2010. Finally, I control for industry×firm size× date fixed effects as well as for exporter and importer fixed effects. T-statistics are in parenthesis. Standard errors have been clustered by firm and date. ***, ** and * denote significance at 1%, 5% and 10% respectively.

	Log(Total Workers)× 100				Log(Permanent Workers)×100				Log(Outsourced Workers)× 100			
Firm Exp * Post TT	-6.613***	-6.831***	-7.559*	-7.763*	-11.00***	-11.25***	-11.36**	-11.59**	5.767	5.815	8.380	8.595
	(-4.39)	(-5.66)	(-1.88)	(-1.96)	(-4.66)	(-4.85)	(-2.29)	(-2.35)	(0.82)	(0.83)	(1.20)	(1.24)
Firm Exp	-33.85*	-54.46***	-26.43**	-41.02***	-31.62	-53.87**	-23.25*	-37.22**	-21.63	-28.41	-31.68***	-42.32***
	(-1.77)	(-2.65)	(-2.03)	(-2.98)	(-1.57)	(-2.47)	(-1.67)	(-2.51)	(-1.28)	(-1.60)	(-2.80)	(-3.41)
Post TT	15.27***	15.64***	0	0	25.68***	26.16***	0	0	-8.812***	-8.903***	0	0
	(9.54)	(9.55)	(.)	(.)	(11.61)	(11.65)	(.)	(.)	(-3.80)	(-3.85)	(.)	(.)
Bank Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Exporter FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Importer FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm Size * Industry * Date FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
N Firm Cluster	2797	2797	2694	2694	2797	2797	2694	2694	2778	2778	2674	2674
N Date Cluster	93	93	93	93	105	105	105	105	93	93	93	93

Table VII: Effect of Capital Controls on Share of Dollar Loans of Treated Firms

This table shows the changes on the share of dollar debt that treated firms (those borrowing only from treated banks at the capital controls announcement) after the announcement of capital controls but before the taper tantrum.

	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$ [FX:2005m2]				Log(USD Credit + 1)×100 [FX:2005m2]				Log(PEN Credit + 1)×100			
Firm Exp * Post CC/Pre TT	0.178*	0.207***	0.215***	0.250***	1.696	2.421**	1.958**	2.367***	-0.508	-0.169	-2.244	-2.401
	(1.81)	(3.93)	(2.84)	(4.75)	(1.32)	(2.28)	(2.11)	(2.75)	(-0.27)	(-0.11)	(-1.19)	(-1.48)
Firm Exp	1.863	-9.086	8.948	-1.139	-268.6***	-381.3***	-161.8**	-255.2***	-491.4***	-379.7***	-518.6***	-399.9***
	(0.33)	(-1.51)	(1.54)	(-0.18)	(-3.72)	(-5.08)	(-2.02)	(-3.07)	(-6.33)	(-4.35)	(-6.14)	(-4.35)
Post CC, Pre TT	0.0198***	0.178***	0	0	-0.159***	1.637***	0	0	-0.537***	-1.267***	0	0
	(12.56)	(7.07)	(.)	(0.00)	(-10.34)	(6.64)	(.)	(0.00)	(-15.64)	(-2.74)	(.)	(0.00)
Bank Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Exporter FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Importer FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm Size * Industry * Date FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
N Firm Cluster	2797	2797	2694	2694	2797	2797	2694	2694	2797	2797	2694	2694
N Date Cluster	73	73	73	73	73	73	73	73	73	73	73	73

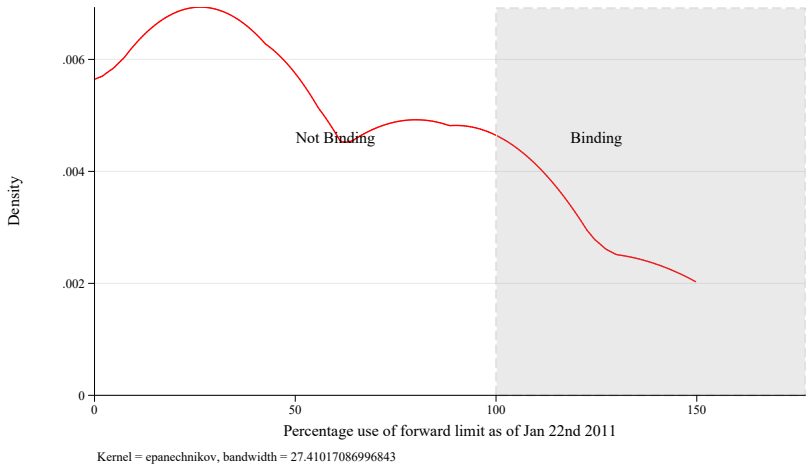


Figure 1: Kernel Density of Forward Holdings on January 22nd 2011

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.

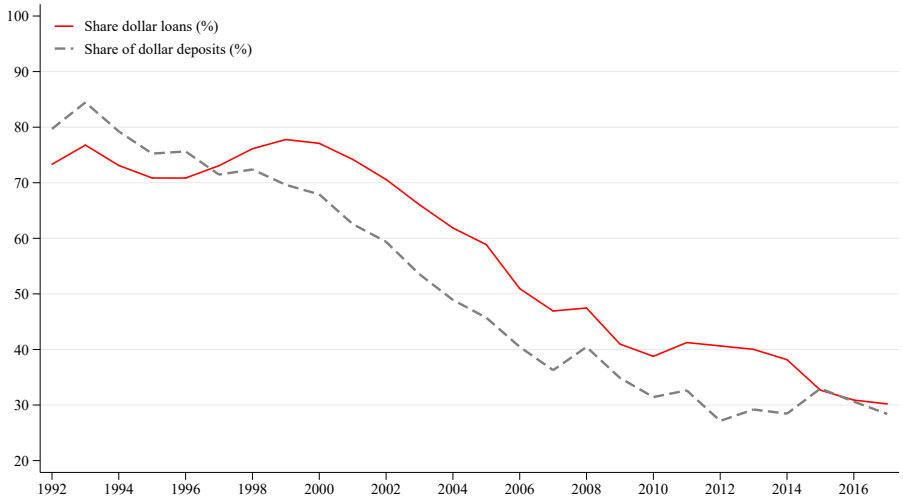


Figure 2: Dollarization of Deposits and Loans in the Peruvian Banking System

This plot juxtaposes the share of deposits in dollars in the banking system (gray line) with the share of loans banks make in dollars (red line) between 1992 and 2017. Although deposit and bank lending dollarization have decreased consistently, when forward limits were set in Peru, in 2011, dollarization rates of deposits and loans were still above 30%. More specifically, I study the sample between 2010 and 2012, where dollarization rates of deposits and loans were around 30% and 40%, respectively.

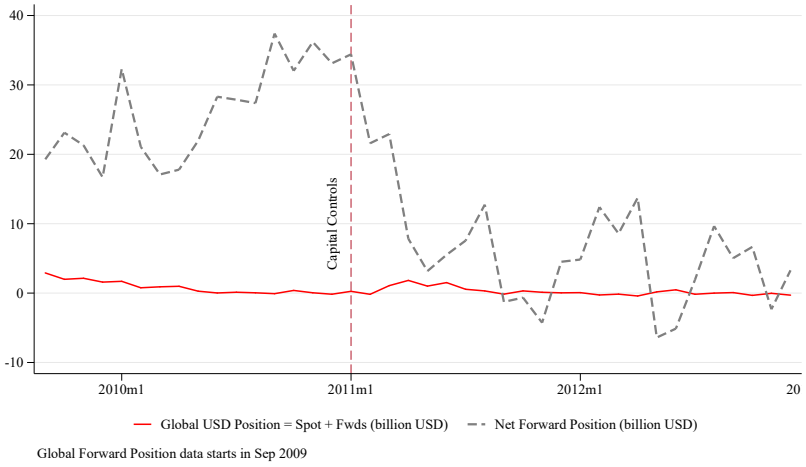


Figure 3: Net long USD forward holdings and net long USD

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.

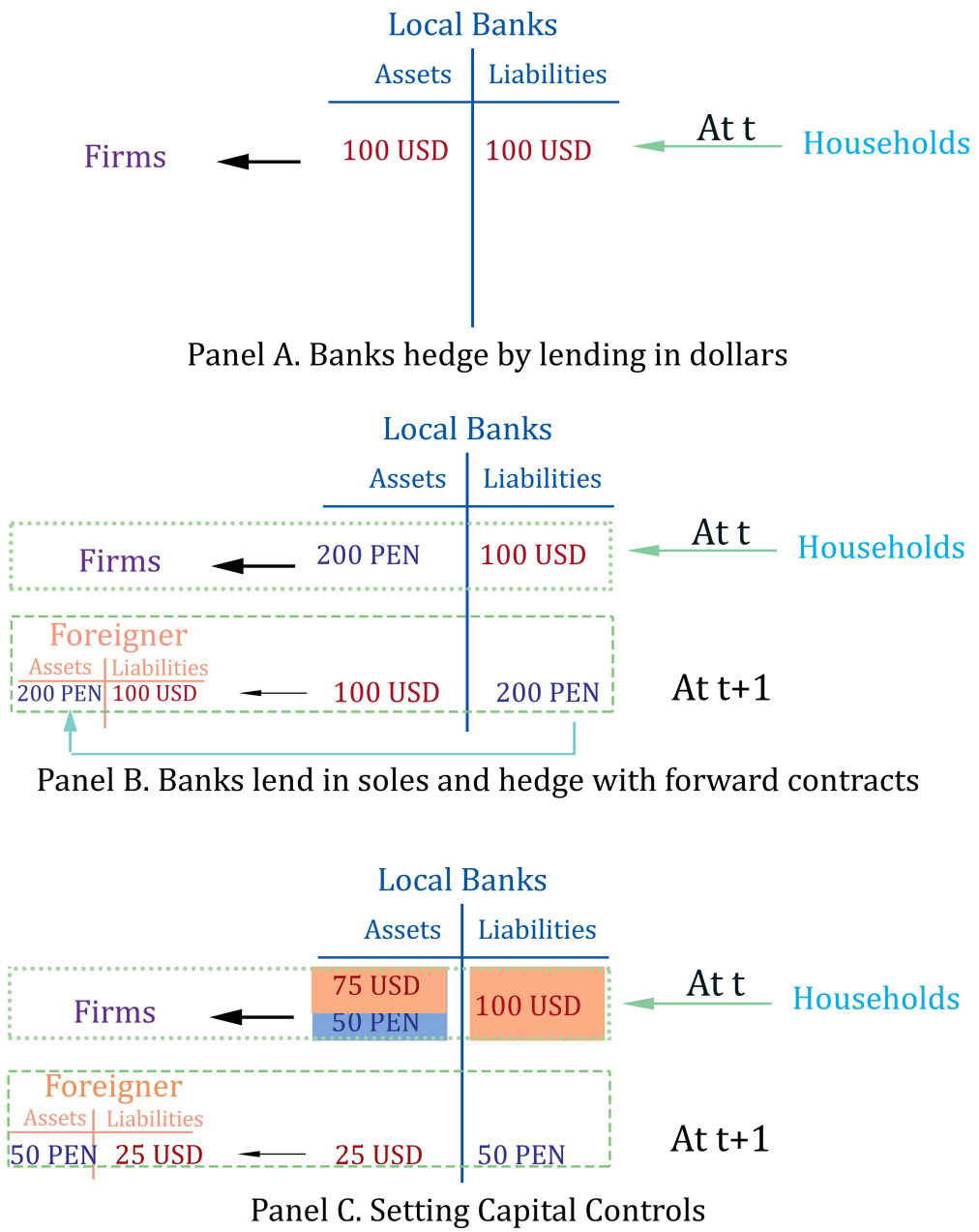


Figure 4: Balance sheets of local banks when foreign investors want to earn the interest rate differential between PEN and USD

Panel A displays an example of the balance sheet of a local bank when households deposit 100 dollars and the bank cannot access foreign markets. Panel B shows the balance sheet of a local bank when the bank has access to foreign markets and lends in domestic currency. The bank hedges exchange rate risk by buying dollars in the forward market (assuming a forward and spot exchange rate of 2 soles per dollar). Panel C shows what happens after capital controls are introduced. As these cap the total dollar forward holdings banks can have to 25 dollars, the bank will lend the remaining 75 in dollars. Only the equivalent of the 25 dollars hedged with forward contracts are lent in soles. Therefore, this figure shows that when capital controls are set, banks increase lending in dollars and decrease in soles.

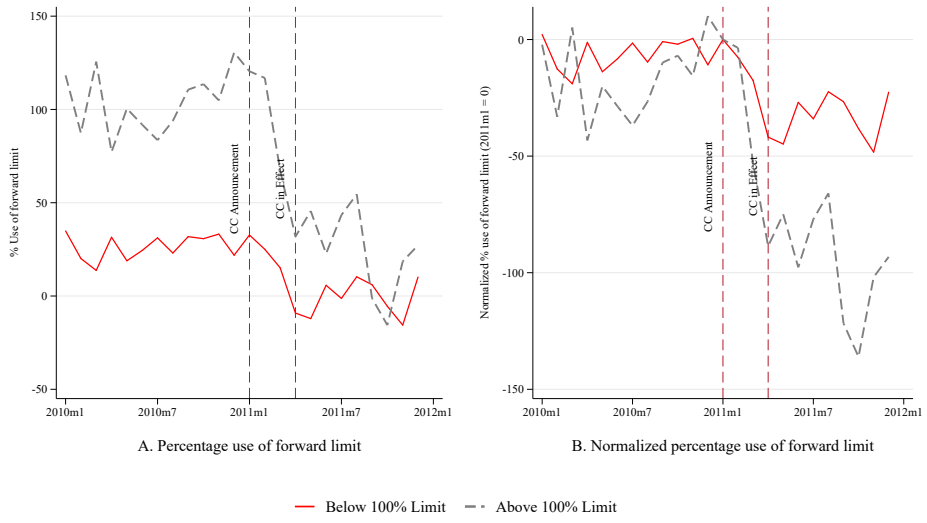


Figure 5: Percentage Use of Forward Limit

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 6: Normalized Ratio USD Credit

Figure 6 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.

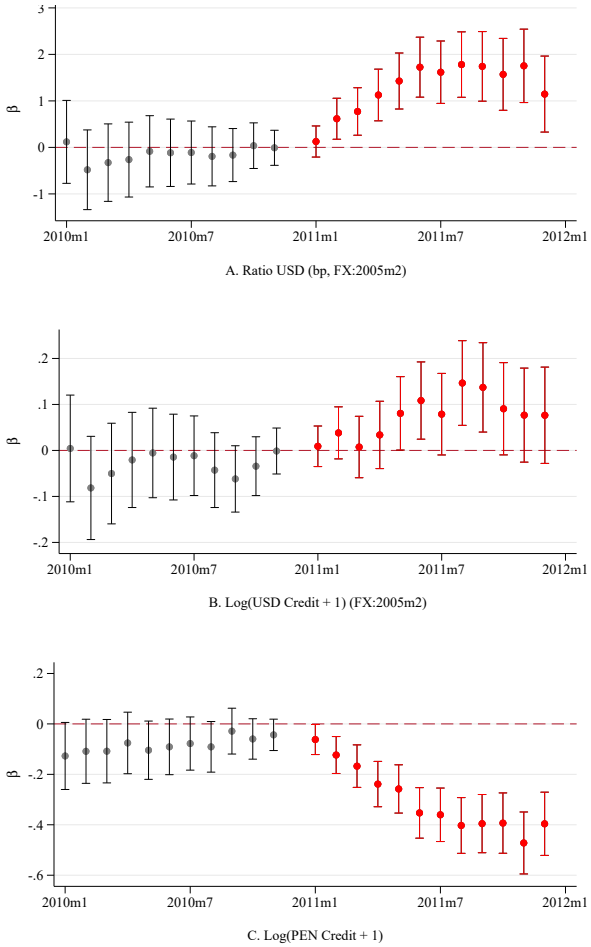


Figure 7: Testing Parallel Trends

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

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

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 controls, X_b and the 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.

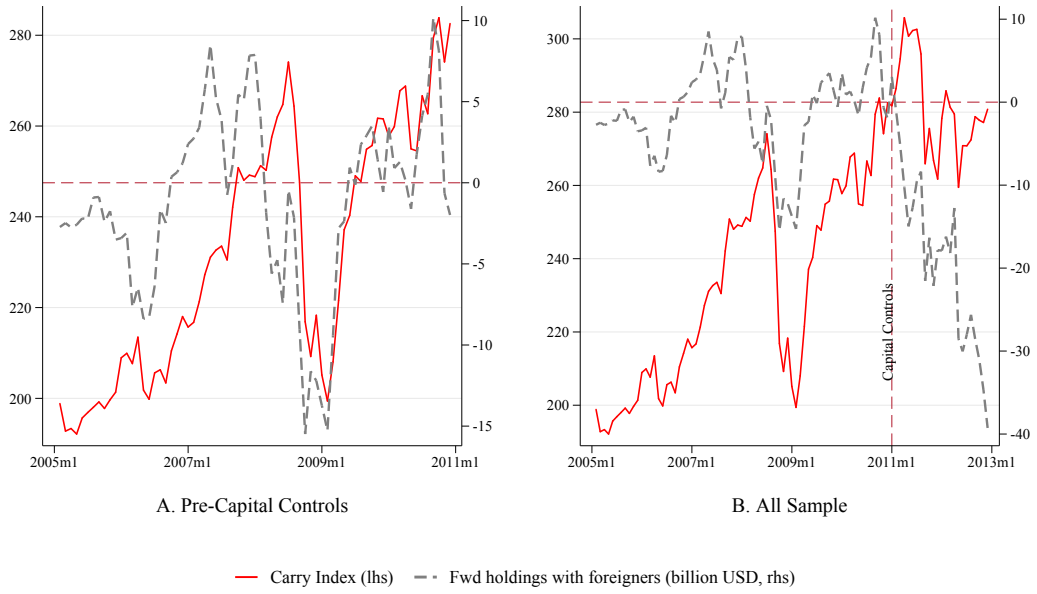


Figure 8: Forward holdings with Foreign Investors and Global Carry Returns

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.

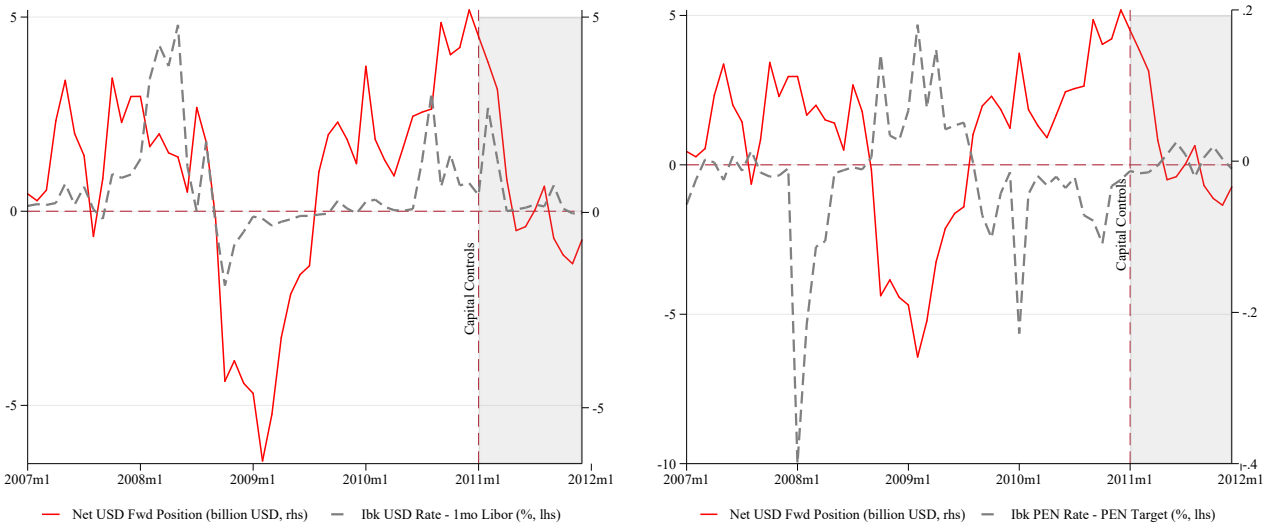


Figure 9: Forward Holdings and Interbank Rates

Figure 9 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.

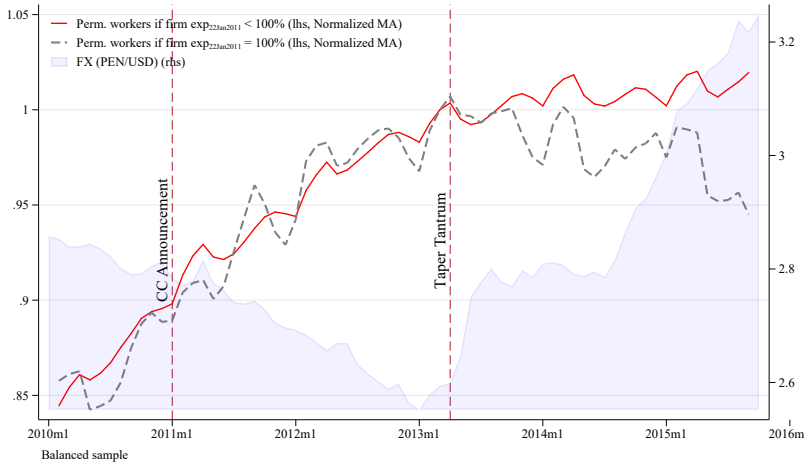


Figure 10: Normalized Moving Average of Permanent Employment

This plot suggests that capital controls had a significant damaging effect on employment. The left hand side axis of Figure 10 shows the normalized moving average of permanent employment, while the right hand side axis reflects the Soles/USD exchange rate. The moving average at time t reflects the average of employment at $t-1$, t, $t+1$. The red and dotted gray line depict the moving average of employment for treated and control firms, while the blue area shows the evolution of the exchange rate. The treated firms are those that had 100% of their debt with a bank for which the capital control was binding as of the capital controls announcement. The dotted gray line refers to all other firms. To avoid employment reflecting entry and exit of firms, I balanced the sample so that employment reflects the evolution of employment of those firms that were in operation throughout the sample years. For ease of comparison of the treated and control firms, I have normalized employment by dividing employment in each group by its respective value as of May 2013 (Taper Tantrum - month in which the PEN started to rapidly depreciate-).

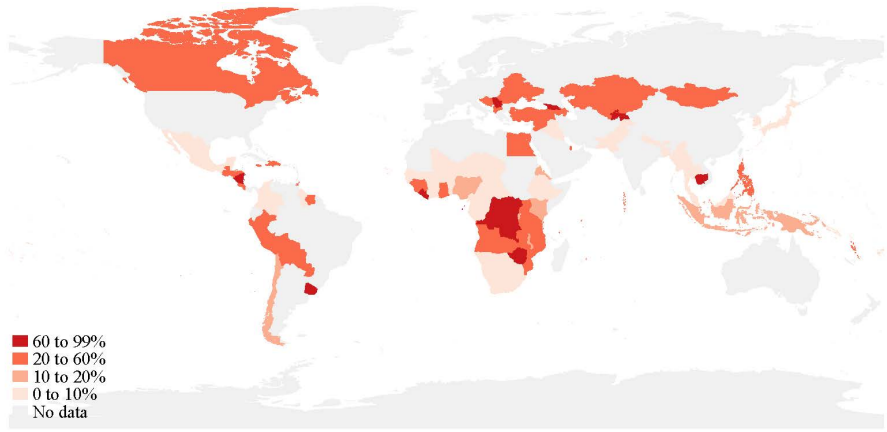


Figure 11: Average Percentage of Foreign Currency Deposits in the Local Banking System (2007 - 2011)

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

A.I. Model

Consider a two period ($t = 1,\ 2$) partial equilibrium model. This model contains a small open economy (domestic economy) populated with a firm and a local bank. Outside this economy, there is a risk neutral foreign investor.

The foreign investor can invest in the domestic economy by buying domestic currency, soles, against the foreigner's currency, dollars. At $t = 1$, the foreign investor forms expectations about the $t = 2$ soles per dollar exchange rate, s_2. Then, compares these expectations with the forward exchange rate, s_1^f, which is the $t = 1$ market value of s_2. If the foreign investor expects that the exchange rate s_2 at $t = 2$ is going to be lower than s_1^f, the foreign investor will use forward contracts at $t = 1$ to sell dollars against soles at $t = 2$ because this strategy allows the foreign investor to sell each dollar at $t = 2$ for s_1^f soles instead of at the lower expected price, $\mathbb{E}(s_2)$ soles.

On the other hand, in the domestic economy, the firm borrows soles and/or dollars at $t = 1$ from the bank to produce good y, which is sold in soles at $t = 2$ in soles. The loans also mature at $t = 2$. Soles and dollars are perfect substitutes as factors of production and hence the firm demands dollar loans only when borrowing in dollars is the cheapest option.

The bank finances the firm's l_1 soles and l_1^* dollar loans with d_0 soles and d_0^* dollar deposits that the bank is born with, as shown in the bank's $t = 1$ budget constraint:

$$s_1 l_1^* + l_1 = s_1 d_0^* + d_0 \tag{A.1}$$

where s_1 is the exchange rate soles per dollar.

At $t = 1$ the bank also decides how many f_1 dollars to buy in exchange for $s_1^f f_1$ soles at $t = 2$ to the foreign investor using forward contracts, where s_1^f is the soles per dollar forward exchange rate and is determined at $t = 1$. At $t = 2$, in addition to the dollars bought, the bank receives back the loans and pays back the deposits. The bank receives gross rates $R_1^{*,l}$ and R_1^l for its dollar and soles loans, while pays predetermined gross rates $R_0^{*,d}$ and R_0^d for its dollar and soles deposits, as shown in the bank's $t = 2$ budget constraint:

$$l_1 R_1^l + s_2 l_1^* R_1^{*,l} + s_2 f = s_2 d_0^* R_0^{*,d} + d_0 R_0 + s_1^f f_1 + \pi \tag{A.2}$$

where π are the bank's profits and the exchange rate s_2 is unknown at $t = 1$.

When choosing how much to lend in either currency, the bank must comply with two regulations. First, the bank must hedge its $t = 2$ dollar liabilities ($d_0^* R_0^{*,d}$) by either lending $l_1^* = \frac{d_0^* R_0^{*,d}}{R_1^{*,l}}$ dollars to the firm

(the present value of the dollar liabilities) or by buying $f_1 = d_0^* R_0^*$ dollars forward to the foreign investor. Mathematically, at $t = 2$, the following hedging constraint must hold:

$$l_1^* R_1^{*,l} + f_1 = d_0^* R_0^{*,d} \tag{A.3}$$

Second, I assume that capital controls are in place. As discussed in Sections II.A and II.B, this entails: (1) limiting the forward channel by setting a cap $\bar{F}$ to the bank's purchases of dollar forward (i.e. $f_1 < \bar{F}$) and (2) restricting the bond channel. Effectively, restricting the bond channel prevents the foreign investor from lending soles and dollars to the local bank and firm. Then, the maximum dollar liabilities the bank has to hedge are the ones the bank was born with. Hence, $f_1 < d_0^* R_0^{*,d}$. Mathematically, this adds capital controls restriction to the bank's optimization problem:

$$f_1 \leq min\left(\bar{F}, d_0^* R_0^{*,d}\right) \tag{A.4}$$

I incorporate these regulations into the bank's profit maximization problem. In this maximization problem, the bank chooses how much to lend in soles and dollars, the dollar lending rate ($R_1^{*,l}$) and how many dollars to buy in the forward market to maximize profits taking prices s_1, s_1^f, $R_0^{*,d}$, R_0^d, R_1^l, $R_1^{*,l}$ as given and subject to two budget constraints ((A.1) and (A.2)) and regulations ((A.3) and (A.4)).

Optimal supply of loans and forward contracts: Solving the bank's optimization problem, the optimal amount of dollars f the bank buys in the forward market is given by:

$$\underbrace{\left(\underbrace{s_2\, R_0^{*,d} - s_1^f R_0^{*,d}}_{\text{Forward transaction}} + \underbrace{s_1\, R_1^l \left\{\frac{R_0^{*,d}}{R_1^{*,l}}\right\}}_{\text{Soles loan to firm}}\right)}_{\text{Benefit of hedging } R_0^{*,d} \text{ with forwards}} - \underbrace{\left(s_2\; R_1^{*,l} \left\{\frac{R_0^{*,d}}{R_1^{*,l}}\right\}\right)}_{\substack{\text{Benefit of hedging} \\ R_0^{*,d} \text{ by lending dollars}}} = \lambda\, s_1\, R_0^{*,d} \tag{A.5}$$

where $\lambda \geq 0$ is the capital controls multiplier.

Equation (A.5) shows the trade-off (from $t = 2$ perspective) between hedging $R_0^{*,d}$ dollars (the future value of 1 dollar deposit) by buying $R_0^{*,d}$ dollars using forward contracts or lending dollars.

This equation shows that when the bank decides to hedge $R_0^{*,d}$ dollars using forward contracts, the bank receives $R_0^{*,d}$ dollars at $t = 2$ (worth $s_2\, R_0^{*,d}$ soles at $t = 2$) in exchange for paying $s_1^f R_0^{*,d}$ soles to the foreign investor ("forward transaction" term). Since forward contracts only affect the bank's budget constraint at $t = 2$, from the perspective of $t = 1$, the bank still has deposits to lend. Then, the bank lends the present value of $R_0^{*,d}$ dollar liabilities, $\frac{R_0^{*,d}}{R_1^{*,l}}$, in soles at a gross interest rate R_1^l ("soles loan to firm" term). On the

other hand, the opportunity cost of hedging using forward contracts and lending in soles is the foregone benefit of lending in dollars ("benefit of hedging by lending dollars" term in Equation (A.5)).

A.I.A. Equilibrium interest rates

Using the previous setting, what happens when there are inflows? To simulate this case, assume that at $t = 1$ there is a shock to the foreign investor's expectations of $t = 2$ exchange rate, s_2, such that his expectation of s_2 is lower than the forward price of soles per dollar ($\mathbb{E}(s_2) < s_1^f$). When this is the case, the profit maximizing strategy is to engage in forward contracts to sell dollars at s_1^f. Equilibrium is reached when the forward exchange rate s_1^f drops so that $\mathbb{E}(s_2) = s_1^f$.

What is the optimal equilibrium hedging strategy for the bank when the forward exchange rate s_1^f drops to a new equilibrium? There are three different scenarios to consider: (1) Without restrictions in the bond and forward channel, (2) With binding restrictions in the bond channel ($\lambda > 0$ and $f_1 = d_0 R_0^*$) (3) With binding restrictions in the bond and forward channel ($\lambda > 0$ and $f_1 = \bar{F}$).

First, when there are no restrictions in the bond and forward channel, $\lambda = 0$ in Equation (A.5), making the exchange rate s_1 and rates R_1^l and $R_1^{*,l}$ adjust so the bank is indifferent between hedging dollar liabilities with forwards or lending dollars to firms.

Second, when restrictions on the bond channel bind, $\lambda > 0$ and the bank hedges all its dollar liabilities buying dollars forward. This is because, by Equation (A.5), the benefit of hedging with forwards is greater than lending dollars to the firm. In other words, the dollar return of lending soles is greater than the dollar return of lending dollars $\left(s_1 \left\{ \frac{R_1^l}{s_1^f} \right\} > R_1^{*,l} \right)$ so the bank does not lend dollars.

However, for zero dollar loans to be an equilibrium, it must also match the firm's demand of dollar loans. Since the benefits of the bank are the losses of the firm, zero dollar loans is not an equilibrium because since the firm is risk neutral, it is profitable for the firm to borrow dollars rather than soles when the expected cost of borrowing dollars is lower than that of borrowing soles. Then, market clearing in the loan market forces the domestic dollar rate to increase (does not have to be the same as the international dollar rate when there are restrictions on the bond channel and the bank cannot borrow from abroad), and/or soles rate to decrease and/or exchange rate s_1 to appreciate until the bank is indifferent between hedging alternatives.

Finally, when there are restrictions in the bond and forward channel, $\lambda > 0$ but the bank only hedges $f = \bar{F}$ with forward contracts. The bank hedges the remaining dollar liabilities by supplying dollar loans. The positive supply of dollar loans leads to a lower dollar rate when there are restrictions on the bank's forward holdings than when there are not.

A.I.B. What is the equilibrium loan outcome if banks can invest in other dollar assets?

The previous example assumed banks could hedge dollar liabilities by using only two dollar assets: (1) dollars bought in the forward market and (2) lending dollars to domestic firms. However, in reality, banks have more dollar assets to choose from. For instance, a bank can buy risk free bonds in dollars such as US Treasury bonds or corporate bonds of US firms. Then, when a bank cannot hedge dollar liabilities with forward contracts because capital controls are in place, the bank can hedge dollar liabilities with dollar risk free bonds. Therefore, there is no need for the bank to increase dollar loans to domestic firms.

Indeed, there is a key reason for banks to plausibly prefer hedging dollar liabilities by buying US firms' dollar bonds rather than by lending dollars to firms that are identical to US firms with the exception that operate in the domestic market. This reason is that US firms have revenues in dollars while domestic firms have revenues in domestic currency. Then, a depreciation shock of the domestic currency will not impair the US firm's ability to repay their dollar debt, but will impair the domestic firms' ability to repay their dollar loans. Hence, banks face greater risk of having firms defaulting on their dollar bank loans than US firms.

Assume that because of the greater default risk that occurs when lending to domestic firms, banks respond to capital controls by buying US Treasury bonds rather than lending dollars to domestic firms. This decreases banks' total lending to firms. Then, under this scenario, the theoretical prediction is that (1) dollar loans stay the same and (2) domestic currency loans decrease.

Although the previous argument is feasible, the empirical analysis shows that banks *increase* dollar loans after the imposition of capital controls. As discussed in Section V.G, this result seems to suggest the presence of implicit government guarantees (Burnside et al. (2001), Schneider and Tornell (2004)).

A.I.C. Robustness

Parallel Trends Assumption and Placebo Tests: One of the main concerns when employing a difference in difference identification strategy is that the treatment and control group are not comparable. Section V.B has shown that the parallel trend assumption holds and the summary statistics on Table II show that the banks in the treatment and control group have similar covariates. However, to further confirm the results, I repeat the main regressions using only the four largest banks.[44] Table A.IV and Table A.V show that restricting the sample to only the largest four banks yields very similar results to the ones discussed in Section V.D. This is also the case when dividing the sample by firm size.

[44]This was possible because only two of the largest four banks were using more than 100% of their forward limit by capital controls announcement date.

Similarly, analyzing the effect of capital controls on credit during the pre-capital controls period also helps confirm the validity of capital controls as a shock to the banking system. Table A.VI reports the results of the main regression shown in Table IV 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 capital controls had been announced one year earlier (i.e. January 2010) but keeping constant the banks in the treated and control group.

If the results presented in the previous section are not driven by already existing differences in the banks that were affected by capital controls with respect to those that were not, the coefficients should be statistically insignificant in this period.

Table A.VI shows that there is no predictive power for the effect of capital controls on the percentage of total credit that is in dollars. After controlling for firm$\times$date fixed effects as well as bank and bank-firm controls, there is also no predictive power over the change in dollar lending. The signs of the coefficients are also very dependent on the specification, as either adding fixed effects or bank and bank-firm relationship controls shift the coefficients' signs. As of soles lending, the treated banks were increasing soles lending in 2010 with respect to 2009 and with respect to the banks in the control group. This is the opposite effect than the one found in Table IV.

The last result could mean that the decrease in soles lending seen the imposition of capital controls (Table IV) are only due to the treated banks adjusting their soles credit because in the pre capital controls period, they were lending relatively more than the banks in the control group (Ashenfelter and Card, 1985). Another possibility is that one could actually expect that the coefficients have the opposite sign that those reported in Section V.D. The latter occurs because if the previous results are driven because the treated banks were forced to decrease their long dollar holdings of forward contracts, then one could expect the inverse results on credit if these treated banks were increasing their forward holdings before the imposition of capital controls.

To disentangle both explanations, one needs to pin down the underlying reason why the treated banks decreased credit in soles. If the treated banks decrease credit in soles because of the need to reduce their forward holdings after the imposition of capital controls, then one would expect that the reduction in credit in soles is a function of their forward holdings. In particular, the closer a bank is to the constraint, the more this bank will need to reduce its forward holdings and therefore, its credit in soles.

Figure A.3, Panel C, shows that the percentage decrease of soles credit after the imposition of capital controls monotonically increases as a function of how binding were capital controls constraint. Splitting the 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 the 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. In contrast, Panel A and B show that 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, the banks in the top tercile increased credit in dollars by 8% more than those in the first tercile.

The previous plot, coupled with the placebo results for dollar and share of dollar debt suggest that the reason for the treated banks to decrease credit in soles in the post period when compared to the banks in the control group was due to the imposition of capital controls.

Alternative Capital Control Definitions: Although the main regressions use capital controls, the treatment variable, as a dummy variable which takes the value of 1 for banks that were surpassing their forward limit by capital control's announcement date, the results are robust to changes in the definition of the treatment variable. Table A.X replicates the benchmark regression specification after controlling for firm×date fixed effects and using for bank and bank-firm relationships controls (Table IV, 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 capital controls announcement. The magnitude of the coefficients and its statistical significance show that the results shown in Section V.D are robust to different specifications of the treatment variable.

Standard Errors: A known problem when using a difference in difference approach is that if the effects of the treatment on the dependent variable are long lasting, the standard errors could be potentially underestimated. This happens as the data resembles one of repeated observations. One of the solutions suggested in Bertrand et al. (2004) is to collapse the time series information into a pre-treatment and post-treatment. This way, for each element in the cross-section (in this case bank-firm credit) there are only two time observations. Table A.IX displays the results of the benchmark regression, Equation (1), but after collapsing the data into a pre and post capital controls period.[45] The results from this analysis also reinforce the robustness of the regression using the time series. Indeed all coefficients are statistically significant at the 10% level, with the majority being statistically significant at the 5% level.

I also conduct robustness checks using different clustering options (Petersen, 2009). Due to 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

[45] Therefore I do not use firm×date fixed effects, but only firm fixed effects.

firms and banks for a same date. This cross-section correlation across firms and banks can be accounted for by clustering by date. Despite of having only 24 different dates, Table A.VII re-estimates the benchmark regressions clustering by both firm and date. After accounting for demand effects, most coefficients remain statistically significant at the 1% level while the remaining are significant at the 5% level. Table A.VIII adds bank clusters to account for the time series correlation that occurs at the bank level. In this case, 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.

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

This table presents the main regression results (Table IV) 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 and bank-firm relationship controls, as well as without Firm×Date fixed effects. The second column adds bank and bank-firm relationship controls. The third and fourth column display the results when using Firm×Date fixed effects, without and with bank 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 controls are: $\frac{\text{Soles Deposits}}{\text{Total Assets}}$, $\frac{\text{Dollar Deposits}}{\text{Total Assets}}$, log(Total Assets), Return over Assets (ROA) and dollar and soles Liquidity Ratios. All these use the December 2010 values. 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. T-statistics 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	1.064***	1.480***	1.416***	10.01**	8.452*	23.52***	10.42**	-6.758	-16.79***	-11.83**	-21.64***
	(1.62)	(3.07)	(3.68)	(3.72)	(2.00)	(1.88)	(4.48)	(2.13)	(-1.36)	(-3.44)	(-2.21)	(-4.00)
CC	8.390***	11.65***	6.262***	8.815***	24.94***	45.49***	-23.57***	47.79***	-211.4***	-248.1***	-219.4***	-203.0***
	(17.37)	(10.87)	(13.13)	(10.49)	(4.01)	(3.22)	(-3.83)	(4.51)	(-33.12)	(-14.88)	(-33.55)	(-13.92)
Post CC	-2.388***	0.0113	0	0	-27.01***	16.54***	0	0	26.57***	20.59***	0	0
	(-9.87)	(0.06)	(.)	(.)	(-8.47)	(6.20)	(.)	(.)	(9.06)	(7.45)	(.)	(.)
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
N Firm Cluster	19008	12165	12592	7104	19008	12165	12592	7104	19008	12165	12592	7104

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

This table presents the main regression results (Table IV) 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 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 and bank-firm relationship controls, as well as without Firm×Date fixed effects. The second column adds bank and bank-firm relationship controls. The third and fourth column display the results when using Firm×Date fixed effects, without and with bank 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 controls are: $\frac{\text{Soles Deposits}}{\text{Total Assets}}$, $\frac{\text{Dollar Deposits}}{\text{Total Assets}}$, log(Total Assets), Return over Assets (ROA) and dollar and soles Liquidity Ratios. All these use the December 2010 values. 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. T-statistics 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	0.403	0.829**	1.381***	1.170***	11.11**	10.07**	26.53***	11.75**	-1.990	-11.57**	-7.376	-16.04***
	(0.99)	(2.27)	(3.24)	(2.89)	(2.11)	(2.12)	(4.75)	(2.24)	(-0.39)	(-2.28)	(-1.33)	(-2.86)
CC	8.262***	12.93***	6.142***	9.213***	23.45***	61.69***	-22.65***	54.24***	-206.8***	-261.0***	-212.3***	-202.0***
	(16.21)	(11.29)	(12.12)	(9.84)	(3.59)	(4.04)	(-3.46)	(4.55)	(-31.23)	(-14.75)	(-31.23)	(-12.77)
Post CC	-2.245***	0.105	0	0	-27.91***	13.88***	0	0	23.04***	16.38***	0	0
	(-8.90)	(0.49)	(.)	(.)	(-8.43)	(4.97)	(.)	(.)	(7.59)	(5.75)	(.)	(.)
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
N Firm Cluster	18211	11508	11927	6593	18211	11508	11927	6593	18211	11508	11927	6593

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

This table replicates Table IV, but splits the sample based on firm size. T-statistics 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)
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
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Panel A. Large Firms												
CC * Post CC	2.301	3.468**	2.640*	3.624**	45.86*	65.37**	51.07**	53.78**	-31.86	-27.25	-41.42	-53.68*
	(1.43)	(2.06)	(1.81)	(2.42)	(1.68)	(2.22)	(2.00)	(2.00)	(-1.19)	(-0.94)	(-1.59)	(-1.89)
CC	6.289***	6.386**	3.162*	4.016**	8.043	4.676	-38.33	-21.98	-227.6***	-240.7***	-199.3***	-225.3***
	(3.00)	(2.36)	(1.85)	(1.99)	(0.25)	(0.12)	(-1.42)	(-0.77)	(-6.25)	(-5.20)	(-5.80)	(-5.63)
Post CC	0.907	1.232	0	0	23.41	23.05	0	0	10.80	-10.44	0	0
	(0.87)	(1.35)	(.)	(.)	(1.39)	(1.50)	(.)	(.)	(0.69)	(-0.74)	(.)	(.)
St.Dev [CC * Post CC]	0.366	0.361	0.373	0.369	0.366	0.361	0.373	0.369	0.366	0.361	0.373	0.369
St.Dev Dep.Var	42.69	42.39	42.81	42.55	619.4	606.2	621.7	608.2	709.2	713.6	720.2	724.9
Observations	20632	18056	19006	16258	20632	18056	19006	16258	20632	18056	19006	16258
Adjusted R2	0.00699	0.0146	0.608	0.669	0.00164	0.0266	0.485	0.577	0.0257	0.0388	0.481	0.552
N Firm Cluster	295	269	240	195	295	269	240	195	295	269	240	195
Panel B. Medium Firms												
CC * Post CC	-0.324	0.0950	-0.00218	0.465	-8.660	-4.530	-4.752	-7.707	-6.096	-11.53	-5.910	-14.78
	(-0.48)	(0.15)	(-0.00)	(0.70)	(-0.93)	(-0.52)	(-0.50)	(-0.83)	(-0.56)	(-1.06)	(-0.51)	(-1.24)
CC	8.428***	10.58***	6.591***	7.993***	21.32*	40.45**	-31.07***	25.39	-250.6***	-240.8***	-266.4***	-224.9***
	(9.21)	(7.28)	(8.07)	(7.18)	(1.80)	(2.15)	(-2.73)	(1.62)	(-15.98)	(-9.73)	(-16.99)	(-10.15)
Post CC	1.212***	1.091***	0	0	26.23***	25.44***	0	0	-0.283	8.973	0	0
	(2.97)	(2.91)	(.)	(.)	(4.68)	(4.83)	(.)	(.)	(-0.04)	(1.42)	(.)	(.)
St.Dev [CC * Post CC]	0.373	0.371	0.383	0.383	0.373	0.371	0.383	0.383	0.373	0.371	0.383	0.383
St.Dev Dep.Var	39.14	38.63	38.84	38.29	506.6	491.1	498.6	481.4	632.6	636.4	638.9	643.7
Observations	108750	98151	97008	85064	108750	98151	97008	85064	108750	98151	97008	85064
Adjusted R2	0.0101	0.0254	0.482	0.534	0.000764	0.0177	0.369	0.477	0.0357	0.0861	0.351	0.467
N Firm Cluster	1883	1738	1541	1233	1883	1738	1541	1233	1883	1738	1541	1233
Panel C. Small Firms												
CC * Post CC	0.655	1.125***	1.776***	1.574***	12.62**	9.907**	28.11***	12.72**	-5.246	-16.49***	-13.90**	-23.92***
	(1.51)	(2.90)	(3.86)	(3.63)	(2.27)	(2.00)	(4.75)	(2.32)	(-0.97)	(-3.13)	(-2.39)	(-4.09)
CC	8.056***	10.71***	6.003***	15.77***	15.99**	63.58***	-22.56***	157.1***	-204.6***	-211.8***	-205.7***	-251.6***
	(14.88)	(6.44)	(11.02)	(5.36)	(2.33)	(3.00)	(-3.27)	(3.79)	(-29.97)	(-6.71)	(-29.26)	(-5.95)
Post CC	-2.729***	-0.0259	0	0	-28.46***	15.29***	0	0	31.31***	20.57***	0	0
	(-10.16)	(-0.12)	(.)	(.)	(-8.19)	(5.28)	(.)	(.)	(9.80)	(6.93)	(.)	(.)
St.Dev [CC * Post CC]	0.364	0.354	0.381	0.374	0.364	0.354	0.381	0.374	0.364	0.354	0.381	0.374
St.Dev Dep.Var	44.73	43.84	44.77	43.85	569.7	556.7	566.1	552.5	561.2	566.8	552.3	557.2
Observations	524630	416891	417589	319466	524630	416891	417589	319466	524630	416891	417589	319466
Adjusted R2	0.00807	0.0604	0.380	0.422	0.000819	0.119	0.329	0.457	0.0288	0.0499	0.316	0.373
N Firm Cluster	17118	10407	11085	5886	17118	10407	11085	5886	17118	10407	11085	5886

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 and bank-firm relationship controls, as well as without Firm×Date fixed effects. The second column adds bank and bank-firm relationship controls. The third and fourth column display the results when using Firm×Date fixed effects, without and with bank 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 controls are: $\frac{\text{Soles Deposits}}{\text{Total Assets}}$, $\frac{\text{Dollar Deposits}}{\text{Total Assets}}$, log(Total Assets), Return over Assets (ROA) and dollar and soles Liquidity Ratios. All these use the December 2010 values. 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. T-statistics 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***
	(0.93)	(2.82)	(3.75)	(3.19)	(1.30)	(1.87)	(4.55)	(1.65)	(-0.78)	(-3.03)	(-1.89)	(-3.61)
CC	8.872***	14.57***	6.664***	13.15***	7.095	117.6***	-55.19***	96.86***	-251.3***	-273.0***	-276.5***	-269.0***
	(17.89)	(18.90)	(13.36)	(16.28)	(1.10)	(12.42)	(-8.47)	(9.95)	(-37.96)	(-26.17)	(-39.96)	(-24.67)
Post CC	-2.076***	0.155	0	0	-22.07***	19.94***	0	0	23.77***	19.73***	0	0
	(-8.49)	(0.74)	(.)	(.)	(-6.78)	(7.16)	(.)	(.)	(7.74)	(6.75)	(.)	(.)
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	0.384	0.374	0.404	0.398	0.384	0.374	0.404	0.398	0.384	0.374	0.404	0.398
Observations	545503	450633	415533	330929	545503	450633	415533	330929	545503	450633	415533	330929
N Firm Cluster	18758	12023	11775	6558	18758	12023	11775	6558	18758	12023	11775	6558

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

This table replicates Table IV, but splits the sample based on firm size. T-statistics 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)
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
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
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
	(1.46)	(2.13)	(1.09)	(1.49)	(1.90)	(2.23)	(1.44)	(1.00)	(-0.54)	(-0.56)	(-0.26)	(-0.65)
CC	6.566***	-5.489	2.610	-8.577***	-3.077	-139.7**	-92.11***	-166.3***	-287.1***	-216.3***	-294.9***	-218.6***
	(2.84)	(-1.57)	(1.30)	(-3.04)	(-0.08)	(-2.47)	(-2.65)	(-3.44)	(-6.94)	(-3.19)	(-7.48)	(-3.29)
Post CC	1.700	1.279	0	0	30.86	23.26	0	0	-18.59	-25.30*	0	0
	(1.46)	(1.20)	(.)	(.)	(1.58)	(1.29)	(.)	(.)	(-1.13)	(-1.69)	(.)	(.)
St.Dev CC	0.398	0.389	0.410	0.404	0.398	0.389	0.410	0.404	0.398	0.389	0.410	0.404
Observations	14951	13539	13104	11479	14951	13539	13104	11479	14951	13539	13104	11479
N Firm Cluster	288	262	231	180	288	262	231	180	288	262	231	180
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
	(-1.01)	(-0.85)	(-0.47)	(-0.10)	(-0.74)	(-1.14)	(-0.28)	(-1.26)	(-0.33)	(-0.75)	(-0.09)	(-0.88)
CC	9.988***	1.867	7.932***	1.546	7.754	-31.28	-63.44***	-28.29	-316.6***	-201.0***	-353.6***	-214.5***
	(10.07)	(1.17)	(8.69)	(1.06)	(0.59)	(-1.48)	(-5.01)	(-1.54)	(-18.76)	(-7.77)	(-20.63)	(-8.43)
Post CC	1.712***	1.580***	0	0	31.41***	32.47***	0	0	-2.501	7.041	0	0
	(3.78)	(3.71)	(.)	(.)	(5.18)	(5.63)	(.)	(.)	(-0.35)	(1.01)	(.)	(.)
St.Dev CC	0.387	0.383	0.400	0.398	0.387	0.383	0.400	0.398	0.387	0.383	0.400	0.398
Observations	88356	81436	75514	67350	88356	81436	75514	67350	88356	81436	75514	67350
N Firm Cluster	1849	1694	1451	1141	1849	1694	1451	1141	1849	1694	1451	1141
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***
	(1.13)	(2.92)	(4.00)	(3.45)	(1.67)	(2.12)	(4.63)	(2.26)	(-0.68)	(-2.84)	(-2.50)	(-3.93)
CC	8.363***	17.77***	6.535***	17.20***	-5.106	156.8***	-51.18***	141.9***	-237.8***	-286.5***	-255.4***	-288.3***
	(14.47)	(20.04)	(10.88)	(17.86)	(-0.69)	(14.78)	(-6.66)	(12.38)	(-32.91)	(-24.83)	(-33.63)	(-23.55)
Post CC	-2.694***	-0.169	0	0	-27.31***	14.02***	0	0	31.46***	21.82***	0	0
	(-9.58)	(-0.70)	(.)	(.)	(-7.43)	(4.43)	(.)	(.)	(9.12)	(6.68)	(.)	(.)
St.Dev CC	0.382	0.371	0.405	0.397	0.382	0.371	0.405	0.397	0.382	0.371	0.405	0.397
Observations	442196	355658	326915	252100	442196	355658	326915	252100	442196	355658	326915	252100
N Firm Cluster	16621	10067	10093	5237	16621	10067	10093	5237	16621	10067	10093	5237

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 IV 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. 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	0.150	-0.503	0.632	0.132	0.0888*	-0.0239	0.163***	0.0304	0.200***	0.259***	0.182***	0.161***
	(0.39)	(-1.28)	(1.60)	(0.31)	(1.80)	(-0.48)	(3.18)	(0.56)	(3.97)	(4.67)	(3.38)	(2.64)
CC	8.223***	10.34***	5.370***	7.400***	0.178***	0.305***	-0.413***	0.308***	-2.328***	-2.497***	-2.361***	-2.038***
	(17.71)	(11.05)	(11.77)	(10.48)	(2.99)	(2.67)	(-7.01)	(3.30)	(-36.89)	(-16.26)	(-36.70)	(-17.36)
Post CC	-2.492***	-0.552**	0	0	-0.339***	0.0254	0	0	0.0981***	0.0693**	0	0
	(-10.56)	(-2.45)	(.)	(.)	(-11.19)	(0.89)	(.)	(.)	(3.32)	(2.26)	(.)	(.)
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	0.361	0.376	0.376	0.396	0.361	0.376	0.376	0.396	0.361	0.376	0.376	0.396
Observations	589860	452960	478844	351187	589860	452960	478844	351187	589860	452960	478844	351187
N Firm Cluster	16886	12414	11302	7314	16886	12414	11302	7314	16886	12414	11302	7314

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

This table replicates Table IV 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)×100 [FX:2005m2]				Log(PEN Credit + 1)×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.VIII: Effect of Capital Controls on Credit Supply : Cluster Firm, Date and Bank

This table replicates Table IV 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)×100 [FX:2005m2]				Log(PEN Credit + 1)×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.IX: 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.X: 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 IV, 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.XI: 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 V after excluding exporters and firms that could be hedging dollar liabilities. The first three columns use whether a firm's exposure (share of its debt that is borrowed from 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. T-statistics 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.

	Above / Below Median Exposure			Continuous Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\frac{\text{USD Credit}}{\text{Total Credit}} \times 100$	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	0.822	19.84**	4.222	1.516	32.53**	-0.390
	(1.30)	(2.57)	(0.46)	(1.61)	(2.74)	(-0.03)
Exposure	9.549***	-13.47	-249.0***	16.47***	-70.42***	-512.0***
	(12.83)	(-1.61)	(-23.65)	(13.88)	(-5.14)	(-30.59)
Industry * Firm Size * Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	134101	134101	134101	134101	134101	134101
Adjusted R2	0.132	0.105	0.148	0.139	0.107	0.202
N Date Cluster	24	24	24	24	24	24
N Firm Cluster	10048	10048	10048	10048	10048	10048

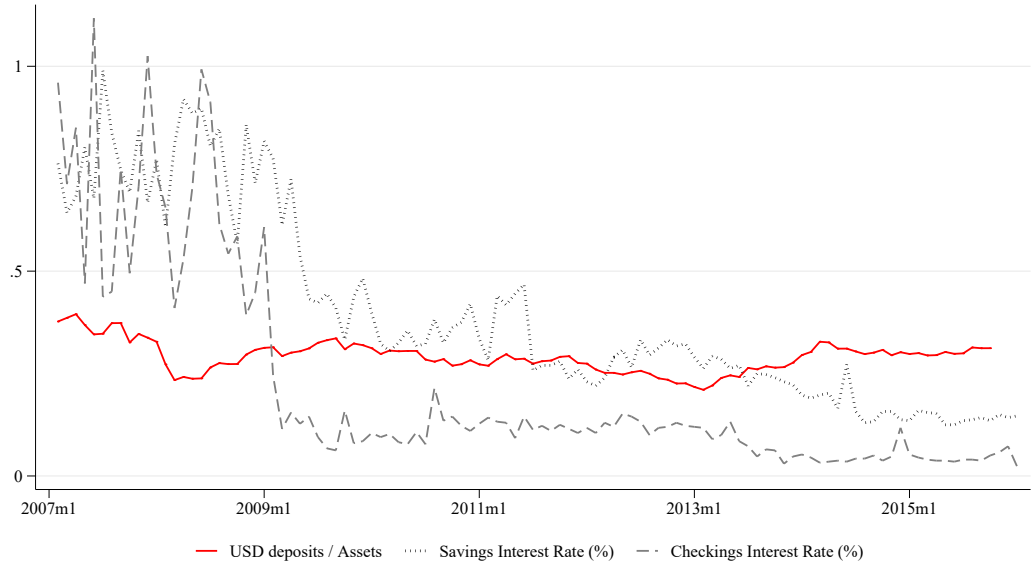


Figure A.1: Dollar checkings and savings interest rates

This figure juxtaposes the average dollar checkings and savings interest rates in Peru with the average share of dollar deposits. The share of dollar deposits is taken from banks' balance sheets while the savings and checkings interest rates are taking from banks' reports to the bank regulator.

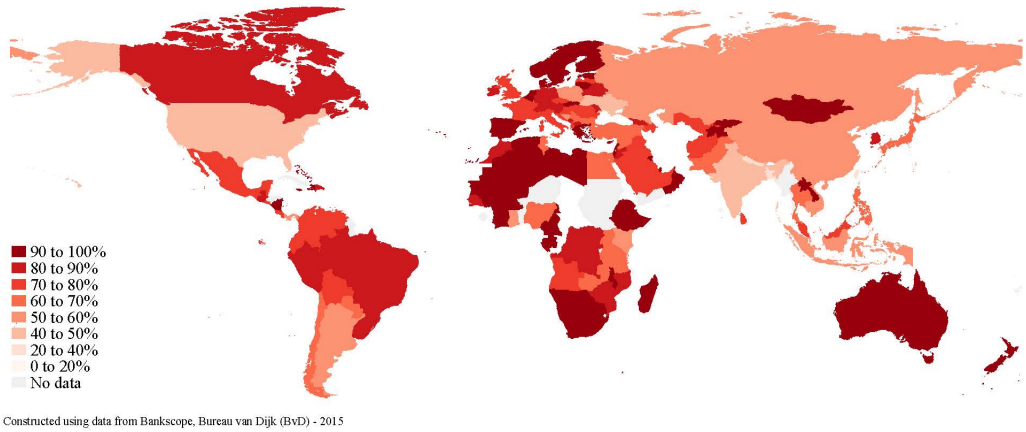
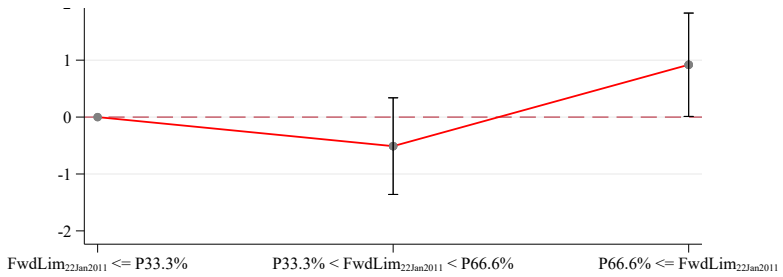
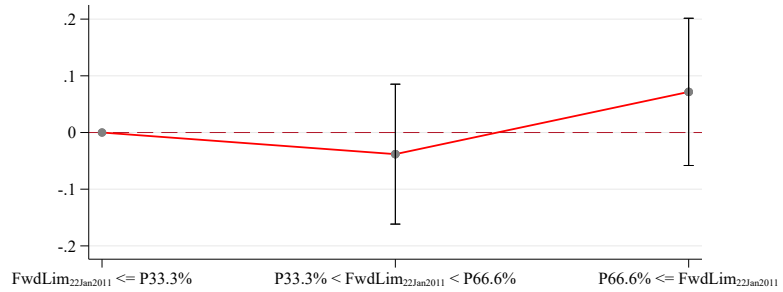


Figure A.2: Five Bank Asset Concentration (2015)

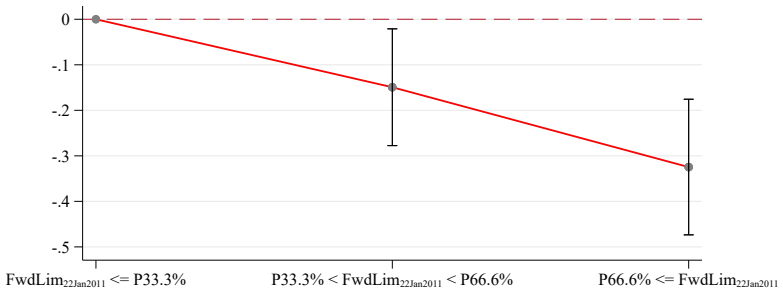
This figure has been constructed from World Bank data, which uses Bankscope, Bureau van Dijk. It shows the percentage of the banks' system assets that are held by the largest five banks.



A. (USD Credit)/(Total Credit)*100 [FX:2005m2]



B. Log(USD Credit + 1) * 100 [FX:2005m2]



C. Log(PEN Credit + 1) * 100 [FX: 2005m2]

Figure A.3: Testing Monotonicity of Capital Controls on Credit

Figure A.3 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:

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

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.

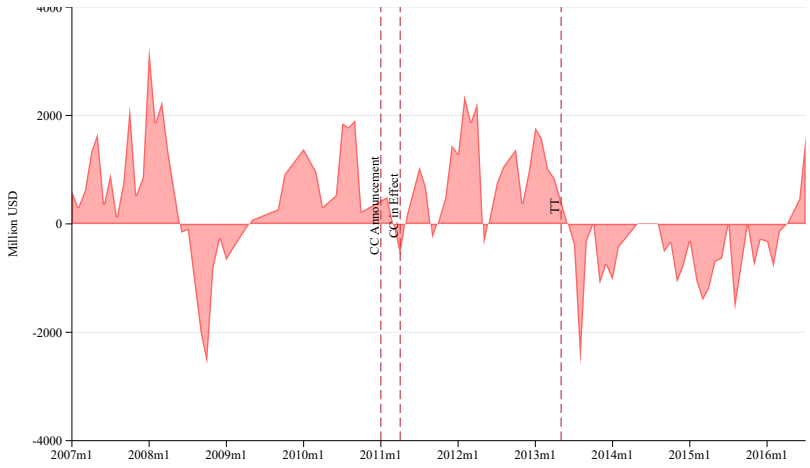


Figure A.4: Peruvian Central Bank's Exchange Rate Interventions

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.

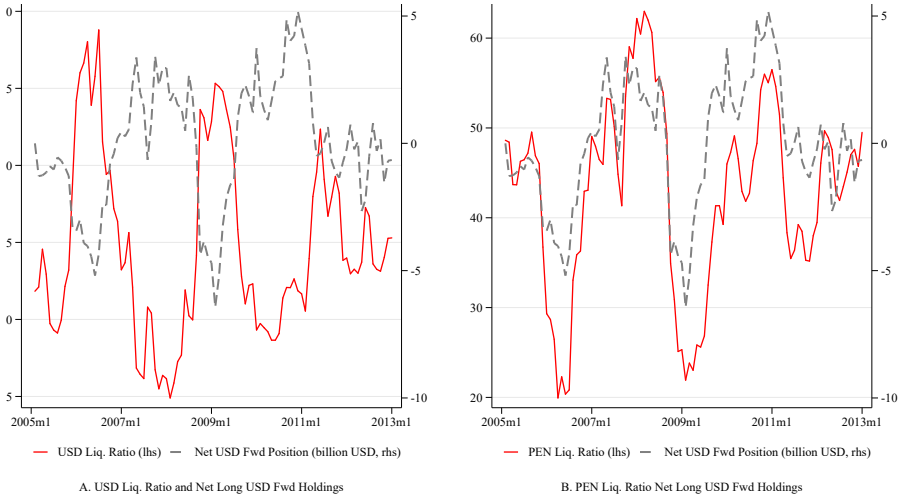


Figure A.5: USD and PEN Liquidity Ratios and USD Forward holdings

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.