The Financial Channel of Tax Amnesty Policies*

Federico Bernini† Paula Donaldson‡
Ezequiel Garcia-Lembergman§ Leticia Juarez¶

September, 2025

Abstract

In the past two decades, more than 30 countries have implemented tax amnesty policies to encourage the declaration and repatriation of hidden assets, with the goal of increasing government tax revenues. While previous literature has focused primarily on the fiscal impact, this paper studies a new channel: the potential expansion of the financial sector resulting from these policies. We examine the macroeconomic effects of Argentina's 2016 Tax Amnesty, one of the largest programs for disclosing hidden assets, through the *financial channel*. This amnesty led to an influx of savings into domestic banks, primarily in dollars, equivalent to 1.4% of GDP. We leverage the heterogeneous exposure of banks and firms to this amnesty-induced financial shock to identify bank responses and spillovers to firms in the private sector. We find that more exposed banks significantly increased their lending compared to less exposed ones. Firms connected to banks with higher exposure experienced increased borrowing, along with a boost in imports of intermediate inputs, exports, and employment. Our findings reveal that tax amnesty policies can stimulate economic growth by expanding the financial sector, demonstrating effects beyond their direct fiscal impact. These results are particularly relevant for countries with underdeveloped financial systems, where the potential for growth through improved access to capital is significant.

JEL Classifications: E5, E6, H26, H31, D31

Keywords: tax amnesty, bank lending channel, credit supply shock,tax evasion, offshore wealth

*Leticia Juarez (Email: leticiaj@umich.edu). We thank Laura Alfaro, Mauro Alessandro, Javier Cravino, Kathryn Dominguez, Juan Herreño, Nir Jaimovich, John Leahy, Andrei Levchenko, Pablo Ottonello, Linda Tesar, Fabian Trottner, Johannes Wieland, Steve Wu, Chenzi Xu. We also thank various conference and seminar participants at AAEP, ESWC, LACEA-Lames, RIDGE and UCSD. All errors are our own. The views expressed in this paper are our own, and do not represent the views of the IADB, its Executive Board or its management.

†Universidad de Buenos Aires

‡University of California - San Diego

§Pontifica Universidad Catolica de Chile

¶Inter-American Development Bank (IADB)

1 Introduction

The size and depth of a country's financial system are central to fostering innovation, facilitating international trade, and promoting economic development. Yet in many economies, a substantial share of domestic savings is concealed offshore or held informally outside the banking system to evade taxes (Alstadsæter et al. (2018)). This practice undermines both public revenue collection and the development of domestic financial markets.[1] In response, tax amnesty policies have become increasingly popular policy tools in both developed and developing countries. These policies are designed to encourage the declaration and repatriation of hidden assets in exchange for reduced penalties. Since 2005, more than 30 countries have implemented a tax amnesty policy. Notable examples - each revealing assets exceeding $50 billion - include those launched in Brazil (2016), Italy (2009), Argentina (2016), and Indonesia (2016).

Recent literature has primarily focused on the fiscal channel of tax amnesties, examining how asset disclosures affect governments' ability to raise revenue in the short and long run. However, one potentially important aspect remains overlooked: the capacity of tax amnesty policies to expand the economy through the financial system. By channeling hidden assets into domestic banks, these policies can increase the supply of bank credit and, in turn, promote investment and growth. We refer to this mechanism as the *financial channel* of tax amnesty policies. Despite its potential relevance, particularly in economies with underdeveloped financial systems, there is currently no evidence documenting this channel.

This paper studies the *financial channel* of tax amnesty policies in the context of Argentina's 2016 Tax Amnesty, one of the largest asset disclosure programs in history. The program generated an influx of savings into domestic banks equivalent to 1.4% of GDP, the vast majority of which were denominated in US dollars. We exploit the heterogeneous exposure of domestic banks to these inflows, combined with variation in firm exposure to affected banks, to identify the response of banks and firms to the amnesty-induced financial shock. We find that more exposed banks significantly increased their credit supply, leading to improvements in firms' financial and real outcomes - particularly in overall bank borrowing, access to dollar credit, imports, exports, and employment. These results suggest that the gains from tax amnesties through the financial system can be substantial, especially in shallow credit markets. Moreover, because the shock shifted the relative supply of dollar- versus peso-denominated credit, we use this episode to study firms' currency composition decisions. We find evidence of at least weak substitution between debt in different currencies and significant heterogeneity between firms. Our estimates have implications for understanding the real effects of relative fluctuations in domestic versus foreign currency funding, both at the aggregate level and across firms with different characteristics.

The Argentinian experience provides an ideal setting for studying the *financial channel* of tax

[1] Alstadsæter et al. (2018) documents that approximately 10% of global GDP is held in tax havens. This estimate is likely conservative, as it does not account for cash holdings concealed outside the formal banking system, such as cash stored at home or in security boxes.

amnesty policies for several reasons. First, the program was remarkably successful, leading to the disclosure of assets equivalent to 21% of GDP[2]. The program allowed citizens to declare previously hidden assets in exchange for a relatively small penalty. Crucially, it offered incentives to repatriate financial assets from foreign bank accounts to domestic banks. As a result, more than 20% of all disclosed bank holdings were deposited in Argentine banks, an amount equivalent to 1.4% of GDP[3]. Second, the vast majority of repatriated savings were denominated in US dollars. In this respect, the tax amnesty resembled an unusual episode of reverse capital flight, offering a unique opportunity to study the impact of foreign currency inflows on the domestic financial system, including changes in bank and firm financial behavior as well as firms' participation in international markets. Third, Argentina is a middle-income economy with a relatively underdeveloped financial system and limited access to credit markets. At the time of the amnesty, domestic bank credit to the private sector was only 13% of GDP, well below the Latin American average of 40%, making the economy particularly suited to benefit from an expansion in credit supply.

Studying the financial channel of tax amnesty policies has been challenging because it is difficult to combine policy variation with comprehensive data. We overcome these challenges by combining bank-to-firm lending data from the Credit Registry of the Central Bank of Argentina with firm-level employment data from the tax authority and trade data from customs records. Our dataset covers the universe of firm-to-bank credit transactions, the currency composition of firm-level credit, bank balance sheets, and firm-level outcomes. A key feature of our data is that it identifies the precise amount of dollar deposits each bank received as a result of the tax amnesty. This level of granularity is rarely available to researchers and is specific to Argentina, where regulatory mandates required amnesty-related deposits to be made into designated special bank accounts.

Our empirical analysis is structured around four steps. First, we assess the effectiveness of the tax amnesty policy in inducing citizens to deposit concealed cash holdings in domestic banks. Second, we analyze how banks responded to the inflow of tax amnesty dollars. Third, we investigate firm-level borrowing responses as well as the effect of the shock on firms' currency choices. Finally, we examine the real effects of the tax amnesty policy, focusing on how it affected firms' performance in labor and international markets.

We start by documenting the significant and unprecedented inflow of dollars into the Argentine banking system due to the tax amnesty. In a two-month window, dollar deposits in domestic banks increased by 50%, while peso deposits remained largely unaffected by the policy. Furthermore, we show that there was considerable variation in how tax amnesty dollars flowed into banks. Banks with a higher proportion of dollar deposits in their liabilities before the amnesty experienced stronger inflows of dollar funds. While the median bank's deposits rose by 2% of their total private sector deposits, some banks experienced increases over 13%, and others saw no additional deposits.

[2]This figure includes both real (e.g. residential properties, businesses, vehicles, art, etc.) and financial (e.g. cash, bank accounts abroad, equity holdings, etc.) assets.

[3]Cash and bank account holdings accounted for 30% of total disclosed assets, making them the second most significant asset category. The first asset category in terms of disclosed values was portfolio investments.

We study the response of bank lending by leveraging this variation in bank exposure to the tax amnesty. We measure bank-level exposure as the ratio of dollar deposits received during the tax amnesty to the bank's total deposits. Importantly, we show that, across exposure levels, banks were balanced in terms of both observable characteristics and pre-amnesty lending trends. To identify the effect of the tax amnesty on bank lending, we employ the within-firm approach popularized by Khwaja and Mian (2008) that leverages variation in exposure across different banks lending to the same firm. By only exploiting within-firm variation, our estimation strategy effectively controls for any potential correlation between firm-specific credit demand shocks and the distribution of bank exposure. We find that bank exposure resulted in a relative increase in lending. On average, banks in the 75th percentile of exposure granted 11% more credit relative to those in the 25th percentile. This effect is more pronounced for credit granted to exporters, which are the firms that typically rely more on dollar-denominated debt.

Next, we study the impact of the tax amnesty on the financial and real performance of firms. For identification, we leverage the fact that not all firms were equally exposed to the increase in credit supply since they were borrowing from different banks before the tax amnesty. We construct firm-level exposure as a weighted average of bank exposure, where the weights correspond to the share of debt with each bank prior to the tax amnesty. The intuition is that there is some rigidity in firm-bank relationships, so a firm is more exposed to the shock if it is already connected to those banks that receive more dollars. Our identification assumption is that, in the absence of the tax amnesty, firms connected to high-exposure banks would have followed a similar trend to firms not connected to them. We provide evidence supporting this assumption by showing that the pre-trends between more exposed and less exposed firms are not significantly different prior to the tax amnesty. In addition, we restrict our comparisons to firms with heterogeneous exposure levels within narrowly defined industries and with the same international trade participation statuses. This approach helps to control for potential confounding effects from any post-amnesty shock that might differentially affect exposed and non-exposed firms due to their preexisting characteristics.

We first test whether the positive effects observed in the *within-firm* analysis extend to the total borrowing of the firms, as it is possible that firms were mainly substituting existing debt across banks. Our findings provide evidence against such substitution. More exposed firms experienced greater credit growth compared to less exposed ones. Our estimates are of similar magnitude to those from the within-firm analysis, which we interpret as evidence of a strong firm borrowing channel rather than a reallocation of existing borrowing to more exposed banks. Specifically, we estimate that an average firm in the 75th percentile of exposure experienced a 9.7% increase in credit relative to a firm at the 25th percentile of exposure. Consistent with our within-firm results, we also find stronger effects among differentially exposed exporters, with the impact of the tax amnesty being more than twice as large as that observed for the average firm.

Next, we evaluate the performance of the dollar credit market in response to the tax amnesty shock, which improved the capacity of banks to issue dollar-denominated credit. In emerging

economies, the availability of foreign currency credit, particularly in dollars, matters for firm performance. Fluctuations in dollar credit availability have been shown to lead to reduced firm investment (Kalemli-Ozcan et al. (2016),Elias (2021)), diminished export dynamism (Paravisini et al. (2015b)), and weaker employment growth (Hardy (2023)). Thus, it is important to assess how tax amnesties, when they channel foreign-currency savings toward domestic banks, contribute to smoothing and deepening access to dollar credit. Given that few firms held dollar debt at the beginning of the sample, our analysis focuses on the extensive margin, examining how changes in foreign currency funding impact firms' participation in this credit market. Our results indicate that firms that were more exposed to the tax amnesty were significantly more likely to borrow in dollars. Specifically, firms in the 75th percentile of exposure had a 3 percentage point higher probability of borrowing in dollars compared to those in the 25th percentile. This represents a 10% increase relative to the unconditional probability of holding dollar debt at the start of the sample.

Next, we examine how the tax amnesty influenced the currency composition of firms' borrowing, shifting our focus from credit growth outcomes to the currency choices made by borrowing firms. Understanding the elasticity of substitution between peso and dollar-denominated debt is crucial to assessing the real and aggregate consequences of fluctuations in dollar credit availability, particularly in emerging economies. Our study provides a causal estimate of this parameter by leveraging the currency-specific nature of the shock. We argue that the amnesty created a shift in the supply of dollar-denominated loans without affecting peso loans, offering a promising quasi-experimental setting to explore this substitution elasticity. We discuss how the institutional regulations of the financial system are essential for this supply shifter to work as proposed and conduct tests that show no differential movements in peso lending rates between more and less exposed banks.

We estimate this elasticity of substitution using firm-level exposure to the tax amnesty as an instrument for dollar credit growth. We find that peso- and dollar-denominated credit are at least weakly substitutable, with elasticity estimates close to -0.7 for the average firm, and not different from -1 at the 10% significance level. However, for exporting firms, the substitution is weaker, with elasticity estimates about half those of the average firm. At a two-year horizon, we cannot reject an elasticity of 0 at the 10% level. Although understanding the drivers of this heterogeneity is beyond the scope of our paper, our results suggest a role for differences in productive processes and tasks in shaping firms' debt portfolio decisions. We believe that investigating what shapes this heterogeneity is a promising research avenue, as it can provide insights into how these differences might affect aggregate outcomes when dollar credit fluctuates.

Lastly, we investigate how increased access to credit affected firm imports, exports, wages, and employment. Our findings suggest that credit access significantly enhances firm performance in these dimensions. Firms with greater exposure to credit were able to increase their importing activities, invest more in capital goods, and expand their exports. This demonstrates the critical role of credit in facilitating firm growth and improving their market reach. In addition, these firms experienced an increase in employment, indicating that credit access supports operational expansion

and contributes to job creation.

More specifically, firms with increased credit access saw a significant increase in total imports, particularly in intermediates and capital goods, highlighting a direct influence on firm investment. Exports also grew significantly, with firms in the lowest export quantile experiencing a 4.04 percentage point increase for every percentage point of exposure. Overall, employment among more exposed firms rose by approximately 2%, reflecting the ability of these firms to hire more workers and expand their operations. However, the impact on wages was not significant, suggesting that increased labor demand was met by a relatively elastic labor supply, preventing substantial wage increases. These results emphasize the benefits of access to credit for firms, contributing to their overall resilience and economic performance.

In summary, these results suggest that tax amnesty policies can expand a country's financial sector, increase access to credit, and foster firm growth in both domestic and international markets by incentivizing the flow of private savings into domestic banks. Policymakers gain two key insights: first, tax amnesty policies have benefits beyond raising government revenue; second, these policies should be designed to not only encourage asset disclosure, but also actively promote the deposit of cash holdings into domestic banks. This is important since many tax amnesty laws stop at disclosure incentives and fail to create channels that direct funds into the domestic financial system.

Related Literature This paper contributes to four strands of the literature.

First, the paper relates to recent literature that studies the consequences of tax amnesties. Previous studies in this literature have primarily focused on the immediate fiscal impacts of these policies on disclosure of hidden assets and its effect on government tax revenues and public spending (Gil et al., 2024; Langenmayr, 2017; Lejour et al., 2022; Londoño-Vélez and Tortarolo, 2022). For instance, Londoño-Vélez and Tortarolo (2022) investigates the fiscal impact of the same policy in Argentina and finds that the tax amnesty led to improved tax compliance, increased government revenue, and expanded social transfers. Gil et al. (2024) studies the importance of deterrence laws and beliefs about future amnesties on incentivizing asset disclosure. Unlike these previous studies, which focus on hidden asset declarations and their fiscal effects, our paper is the first to examine the indirect consequences of the policy through the financial sector and firms in the spillover to private sector firms. We show that tax amnesties can encourage the deposit of hidden assets into domestic banks, and that banks more exposed to these inflows expand their credit supply. Consequently, firms connected to these banks experience improved performance due to enhanced access to credit. Our findings suggest that tax amnesties have the potential to influence economic growth through their effects on banks and firms in the private sector, extending beyond their direct impacts on government tax revenues and spending.

Second, this paper contributes to the literature in macroeconomics that identifies the economic effects of credit supply shocks by isolating the bank lending channel. Papers in this strand include Alfaro et al. (2021); Amiti and Weinstein (2011, 2018); Chodorow-Reich (2014); Federico et al.

(2023); Herreño (2020); Jiménez et al. (2020); Kashyap and Stein (2000); Khwaja and Mian (2008); Mora (2013); Paravisini (2008); Paravisini et al. (2015b); Schnabl (2012); Villacorta et al. (2023), among others. We uncover a new dimension to understand the bank lending channel: the currency of the credit supply shock. Specifically, we exploit the regulatory framework of the tax amnesty policy in Argentina to identify a credit supply shock that was induced by an inflow of deposits in foreign currency and study the lending decisions of banks after the shock, both in pesos and dollars.

Third, this paper relates to the literature on financial development, credit constraints, and firms' performance (Caggese and Cuñat (2013); Chor and Manova (2012); Federico et al. (2023); Kohn et al. (2016, 2023); Leibovici (2021); Levchenko et al. (2010); Manova (2008, 2012); Paravisini et al. (2015a,b)). While most of these studies focus on the role of access to credit in facilitating exporting, our paper extends the analysis to the causal impact on importing. In this context, Kohn et al. (2023) empirically document that financially underdeveloped economies exhibit a slower aggregate response following trade liberalization, attributing this to credit constraints affecting importing possibilities. Additionally, Muûls (2015) documents the relationship between importing and access to credit. We contribute to this literature in two dimensions. First, we combine exposure of banks to the tax amnesty with firm-to-bank data to empirically estimate the causal effect of credit availability, especially in dollars, on firms' importing activities. Second, most of these papers do not have access to data on the currency denomination of the credit. We are able to study how access to dollar credit deferentially affect firms' participation in international markets.

Finally, the paper is also related to the literature exploring the drivers of firms' foreign currency debt choices in emerging economies (such as Allayannis et al. (2003); Brown et al. (2011, 2014); Calvo (2002); Degryse et al. (2012); Galindo et al. (2003); Hardy (2018); Kamil (2012)). The richness of our data and the regulatory environment of the tax amnesty provides an ideal scenario to show novel patterns about the relationship between dollars and pesos debt. In addition, the dollar-specific shock to banks allow us to identify the firm borrowing elasticity between pesos and dollar debt.

The remainder of the paper is organized as follows. Section 2 provides background on tax amnesty programs and details the institutional context of the Argentine case. Section 3 describes the datasets used in the empirical analysis. Section 4 outlines the identification strategy and presents the main results. Finally, Section 5 offers concluding remarks.

2 Tax Amnesties and Argentina's Experience

Tax Amnesties A tax amnesty, as defined by Le Borgne and Baer (2008), is a limited time offer by the government that allows taxpayers to settle their tax liabilities for a defined amount, often including a reduction or forgiveness of interest and penalties. This settlement pertains to previous tax periods and grants taxpayers freedom from legal prosecution related to the disclosed tax liabilities. Le Borgne and Baer (2008) highlight three goals of these programs: (1) raise government

revenue quickly, (2) increase future tax compliance, and (3) incentivize asset repatriation of flight capital for reasons that go beyond immediate revenue and tax compliance motives.

The existing literature has primarily addressed the first two objectives, largely overlooking the indirect effects through asset repatriation. This paper focuses on asset repatriations. We adopt a broad definition of "asset repatriation," which includes inflows into domestic banks from: (a) cash previously held abroad, and (b) cash held domestically but informally outside the banking system (for example, "under the mattress" or in private vaults).[4]

Tax amnesty programs are increasingly used by governments as policy tools, a trend expected to grow due to enhanced measures against cross-border tax evasion, making these programs more likely to succeed.[5] In the past 20 years, over 30 countries have launched tax amnesty programs with varying levels of success in terms of asset disclosure. Notable examples of successful tax amnesty programs include Brazil (2016) with $50 billion disclosed, Chile (2015) with $18.7 billions, Italy (2009) with $102 billion, Indonesia (2016) with $346 billion, Argentina (2016) with $116 billion, and Pakistan with $21 billion. Other countries that implemented national programs are United Kingdom, Russia, Spain, India, among others. Regarding asset repatriation, some of these programs included incentives for repatriation, while others did not. Unfortunately, data on the exact amount of repatriated cash holdings for most of these programs is typically not available.

Argentina's Tax Amnesty In July 2016, Argentina enacted a tax amnesty law allowing citizens who had previously concealed assets from the tax authority to disclose them by paying a modest penalty for past tax evasion. This policy was part of a larger government initiative aimed at achieving budget balance. Specifically, the tax amnesty played an important role by immediately increasing tax revenue through the collection of one-time penalties and by broadening the future tax base.

Argentina's 2016 tax amnesty stands out as one of the largest of its kind worldwide. Disclosed assets amounted to approximately 19% of GDP, effectively doubling the country's wealth tax base. Londoño-Vélez et al. (2022) provide a detailed analysis of the fiscal aspects of this episode. In the following, we highlight the aspects of the amnesty that are particularly relevant for the present study.

Under the policy, citizens could disclose any type of asset subject to wealth taxes, regardless of its geographical location (whether domestic or offshore). Eligible assets included stocks, portfolio investments, residential properties, cars, foreign currency checking accounts, and cash holdings, among others. A significant portion of disclosed assets were held abroad, notably foreign currency checking accounts, real-estate, and financial investments.

In this study, we focus on the disclosure of two types of financial assets: (i) foreign-currency

[4] Although informally held domestic cash is already within the country, its effects resemble those of repatriated funds, as both increase banks' dollar deposit base.

[5] Examples of such measures include the US Foreign Account Tax Compliance Act (FATCA) and the global Common Reporting Standard (CRS) for the automatic exchange of financial account data.

savings held in offshore bank accounts and (ii) foreign-currency cash holdings stored informally outside the domestic financial system (e.g., cash stored at home or in safety deposit boxes). We bundle them together as "repatriated cash holdings" because both increase the dollar deposits in the domestic financial system.

Specifically, citizens who disclose assets held in foreign bank accounts were given two options: keep these assets abroad or repatriate them to a domestic bank. Repatriation was incentivized by a lower penalty rate, encouraging inflows of these funds into Argentina's banking system. In contrast, citizens who disclosed foreign currency cash holdings were required to deposit them in domestic banks, as there was no alternative option available. Both types of assets had a defined time window, August to December 2016, for their repatriation or formal deposit into the domestic banking system. Thus, from the perspective of domestic banks, these two distinct asset classes represented an economically equivalent inflow of foreign currency deposits during the amnesty period.

Citizens who opted to declare cash or repatriate foreign currency had to open a special foreign currency bank account at a bank of their choice to deposit the money. This is convenient because it allows us to observe the exact amount of dollars that entered the bank system because of the tax amnesty. We will refer to the specially created bank accounts as *Tax Amnesty Accounts*. In addition, they were required to park declared funds in those same accounts for six months[6]. The amount of foreign currency deposits (mainly US dollars) that flowed into domestic banks was equivalent to 1.4% of GDP and implied almost a doubling of the stock of private dollar deposits in domestic banks. This occurred within a very short window of around three months as most funds were effectively repatriated between October 2016 and December 2016. This inflow of dollars into domestic banks is what we refer to as the tax amnesty shock throughout the paper.[7]

Figure 1a shows the aggregate stock of dollar deposits held in *Tax Amnesty Accounts*. The inflow of tax amnesty funds was concentrated in the last quarter of 2016 even though the tax amnesty window opened in August 2016. After the 6-month parking period, depositors could transfer their holdings to their standard dollar checking accounts which explains the gradual decline in deposits in Figure 1a.

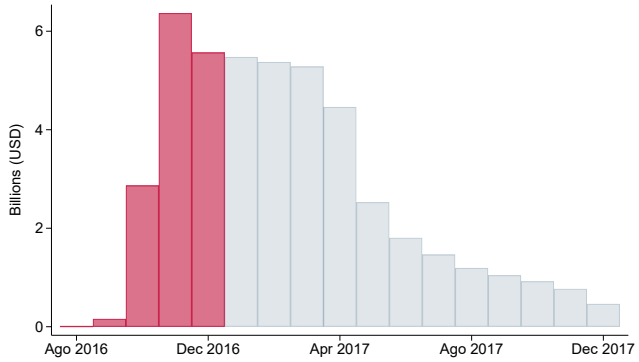
Figure 1b contextualizes the relevance of the tax amnesty shock. It plots the evolution of total private sector dollar deposits. The tax amnesty resulted in a significant increase in dollar-denominated deposits at domestic banks, which doubled. Tax amnesty funds explain more than 80% of the increase in dollar deposits during the last semester of 2016.

[6]There were a number of exceptions to the parking requirement. For example, the requirement could be lifted if funds were applied to purchase government debt securities

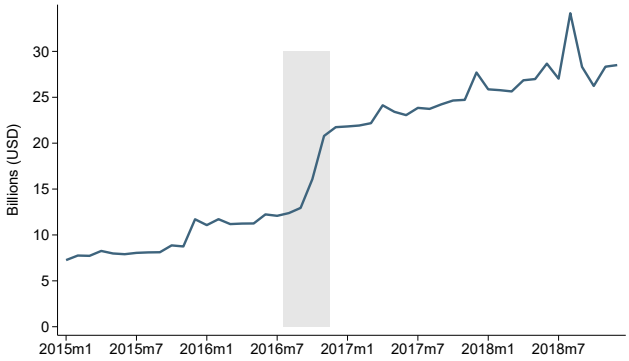
[7]Note that this excludes from the analysis any foreign currency holdings at bank accounts abroad that citizens did not choose to deposit in Argentinian banks.

Figure 1: Tracking Tax Amnesty Dollars

(a) Deposits in *Special Accounts*



(b) Dollar Deposits



Data are in billions of US dollars. Panel (a) shows the evolution of dollar deposits in the specially created bank accounts. The red bars correspond to the months of the tax amnesty window. Each bar corresponds to the aggregate stock of dollar deposits at the end of each month. After the 6-month parking period, funds gradually flow out of the special accounts into traditional dollar checking accounts. Panel (b) shows the evolution of dollar deposits held by the private sector. The shaded region corresponds to the tax amnesty window.

3 Data

We combine four datasets: i) bank-level balance sheet data obtained from the Central Bank of Argentina (BCRA); ii) firm-level data on exports and imports from the Argentinian Customs Office; iii) firm-to-bank data on domestic credit compiled by the Central Bank of Argentina (BCRA); and iv) administrative records on firm-level employment and average wages from Argentina's tax authority (AFIP). All data sets are collected monthly and cover years from 2014 to 2018. We employ bank-level balance sheets to recover information on banks' assets, liabilities, core capital, and profits. These data cover a total of 80 banks operating in Argentina, both publicly and privately owned. We complement balance sheets with bank-level performance indicators (e.g., leverage ratios, return on assets, and average interest rates), which are published by the Central Bank. Importantly, the balance sheet records the amount of dollar deposits that each bank received during the tax amnesty. Banks were required to inform the outstanding stock of deposits in checking accounts that were specially created for tax amnesty.[8]We restrict the analysis to banking institutions that had a participation in total deposits of at least 0.05% during our sample period. This leaves us with 28 domestic banks that take up more than 97% of aggregate deposits.

We use Credit Registry data to obtain firm-level credit indicators. We employ two datasets within the Credit Registry. Our primary source of information contains detailed firm-bank level data on monthly total outstanding loans. This dataset covers all private nonfinancial firms that have debt with domestic banks between 2014 and 2018. We complement this information with a second dataset that started being recorded in August 2015. This additional dataset contains disaggregated information on the currency composition of firm-level debt. For each firm, we observe the level of

[8]Section 4 provides more details on how we use this information to construct the exposure of banks to the tax amnesty shock.

pesos and dollar debt over time. We focus on firms that report debt to banks at some point during the period considered. In addition, we drop firms for which total credit never exceeds 50 US dollars and winsorize the top and bottom 1% of observations to limit the influence of outliers. After this process, we are left with data on 77,777 firms.

Lastly, we gather information from several sources to obtain a comprehensive picture of the consequences of the tax amnesty on firms' real performance. We use comprehensive customs data covering the universe of firm-level export and import transactions. This data set covers the period 2014 to 2018. It provides monthly information on the value (in US dollars) of exports and imports for each firm, categorized by country (origin/destination) and product at the 6-digit level. We link this dataset to fiscal files generated by the Argentina's tax authority (AFIP) using unique firm identifiers to enrich our analysis. This allows us to obtain additional information on formal employment, average wages, and firms' main sector of activity. Consequently, we are able to construct firm-level measures of employment, exports, imports, and payroll for the years 2014 to 2018.

4 Empirical Analysis

We empirically examine the impact of the tax amnesty in Argentina through the financial channel. Specifically, we analyze to what extent the resulting capital inflow influenced banks' credit supply and, in turn, how the expanded credit access affected output of firms.

We begin by constructing measures of banks' and firms' exposure to the tax amnesty. The empirical analysis then proceeds in two steps. First, we examine the bank lending channel, assessing whether more exposed banks expand their credit supply by more. Second, we analyze the impact of the increase in credit supply on firm-level outcomes, including borrowing, trade (imports and exports), and employment.

4.1 Bank and Firm Exposure

Bank-level Exposure The balance sheet of domestic banks records the stock of deposits held in the special accounts created during the tax amnesty. For each bank b, we compute the average deposits reported in these special accounts between October 2016 and December 2016. We refer to this object as the *tax amnesty funds* received by bank b. Next, we measure the exposure of bank b to the tax amnesty as the ratio between its *tax amnesty funds* and its total private sector deposits in the quarter preceding the tax amnesty. Formally,

$$S_b = \frac{Tax\ Amnesty\ Funds_b}{D_{b,0}^{Total}} \times 100 \tag{1}$$

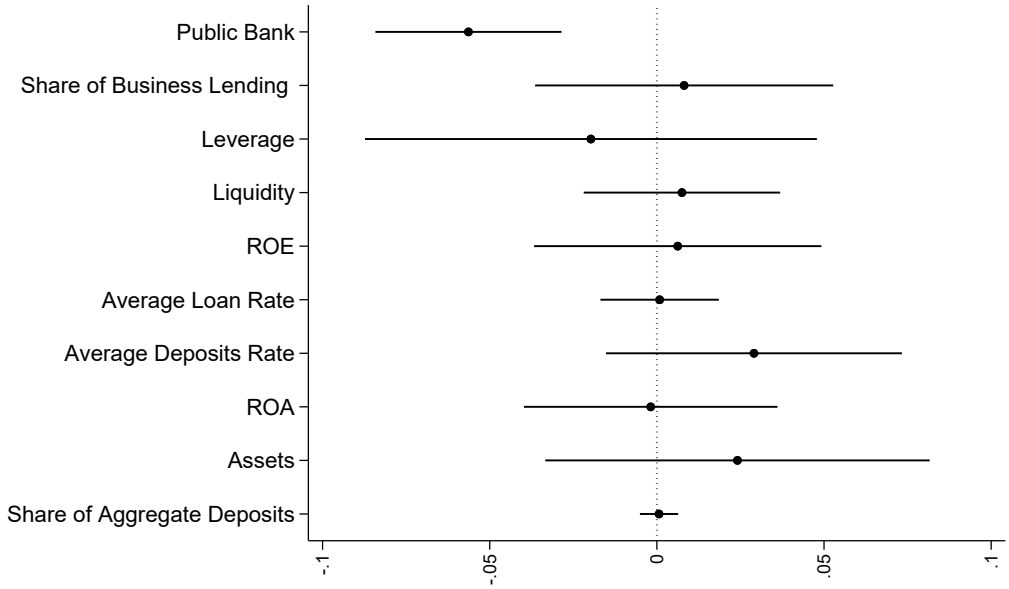
where $D_{b,0}^{Total}$ are the average total deposits in 2016Q2. S_b measures the importance of the inflow of dollars due to the tax amnesty relative to the existing liquidity of each bank. Banks experienced

varying levels of exposure to the tax amnesty funds, with an average exposure of 10% and a standard deviation of 10 percentage points. For the most exposed banks, the liquidity shock represented nearly 20% of their preexisting deposit liabilities. The difference in exposure between a bank in the 75th percentile of exposure and one in the 25th percentile of exposure is 10 percentage points.

Bank-level differences in inflows were partly shaped by the existing banking relationships of households. Three pieces of evidence support this. First, the variation in bank exposure is positively correlated with the share of dollar deposits a bank held before the amnesty (Appendix Figure 18). Second, at the municipality level, inflows are concentrated in areas with higher pre-existing dollar deposits, indicating households deposited funds in banks where they had prior relationships. Third, most individuals transferred funds from special amnesty accounts to other accounts within the same bank after the holding period ended, suggesting that they already had accounts there (Appendix Figure 18).

Although our identification strategy does not rely on balance in pre-amnesty characteristics, examining the relationship between these characteristics and our exposure measure is informative. Figure 2 presents the coefficients from individually regressing a comprehensive set of baseline bank characteristics on tax amnesty exposure. With the exception of public ownership, for which we explicitly control in all specifications, we find no systematic differences between banks with higher and lower exposure.

Figure 2: Banks Characteristics - Balance Test

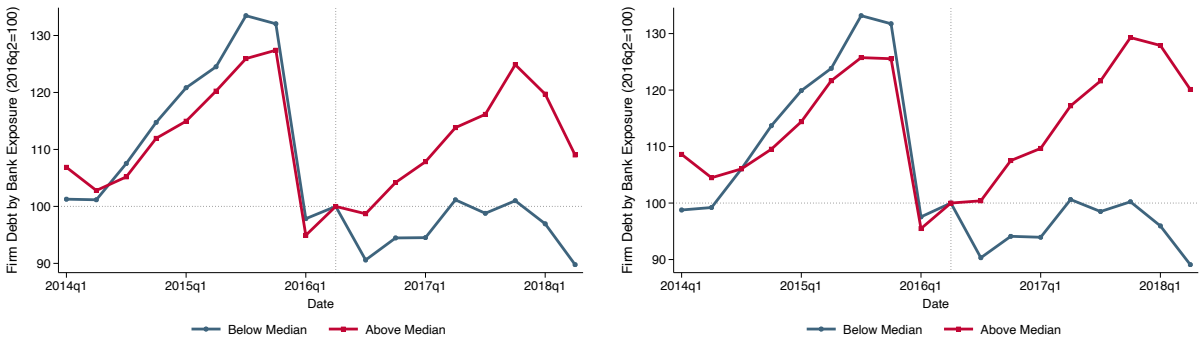


We report 95% confidence bands. Bank characteristics are averages of the year before the tax amnesty. All variables were standardized with the exception of the participation of each bank in aggregate deposits (our *proxy* for bank size). Regressions are weighted by the share of each bank in total private sector lending. *Public Bank*: = 1 if bank is publicly owned. *Share of Business Lending*: weight of lending to firms in total lending. *Leverage*: assets over equity. *Liquidity*: ratio of liquid assets over total assets. *ROE*: return on equity. *Average Loan Rate*: average interest rate on local currency loans. *Average Deposit Rate*: average interest rate on local currency deposits. *ROA*: return on assets. *Assets*: total assets. *Share of Aggregate Deposits*: participation of each bank in aggregate deposits.

More importantly, Figure 3 illustrates the relationship between bank exposure and credit supply using raw data. Panel (a) aggregates firm-level debt within each exposure quartile, while Panel (b) restricts the sample to firms borrowing from both low- and high-exposure banks, allowing for within-firm comparisons across banks. In both panels, lending to firms follows parallel trends across exposure groups in the two years prior to the policy, indicating the absence of pretrends or anticipation effects. Following the policy, even without controlling for fixed effects, banks with higher exposure exhibit a clear increase in total lending.

Intuitively, the policy was introduced as a one-off fiscal measure in a context unrelated to banking sector distress, which reduces the likelihood of observing pretrends or anticipation effects.

Figure 3: Above *vs* Below Median Bank Exposure
Total Credit



(a) Across Banks (b) Within Firms & Across Banks

In Panel (a) firm level debt is added up for each exposure quantile. Panel (b) restricts the sample to firms that had debt with banks both above and below median exposure in $2016Q2$. We construct the evolution of their real debt from each of these group of banks. In other words, firms appear in both the red and blue lines. In both cases the base period is $2016q2$ and total credit is expressed in current US dollars.

Firm-level Exposure We measure firm-level exposure as:

$$S_i = \sum_b \omega_{ib}^0 S_b$$

where S_i is firm's i exposure to the tax amnesty shock and ω_{ib}^0 is the weight of bank b on firm's i total borrowing in the quarter preceding the tax amnesty. The exposure of firm i is a weighted average of the exposure of the banks from which firm i was borrowing from at the onset of the tax amnesty. We argue that the fact that different firms are connected to banks with different degrees of exposure provides a source of plausibly exogenous variation in the credit supply faced by individual firms.

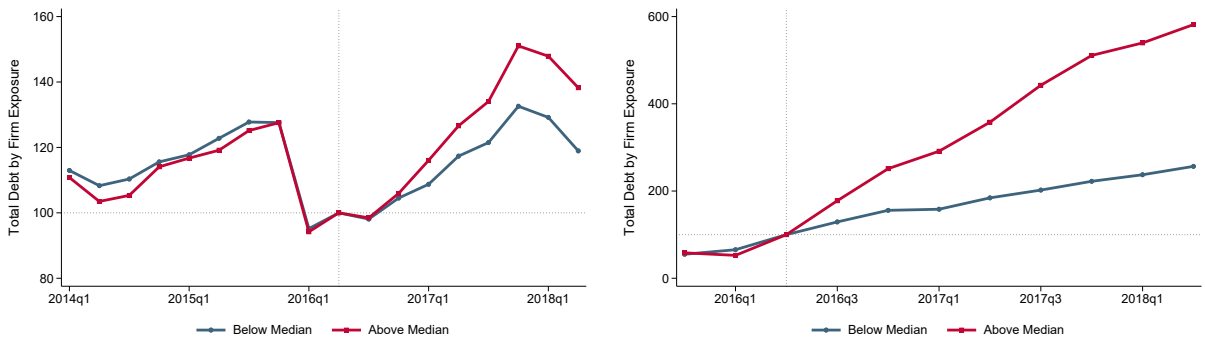
As an initial overview of the results— which we will explore in greater detail in Section 4.3—Figure 4 provides insights into the debt dynamics of firms with different levels of exposure over time, as observed in the raw data. The left panel illustrates the evolution of total credit for firms with below-

and above-median exposure, while the right panel depicts the evolution of dollar-denominated debt. Both measures are expressed in current US dollars and relative to $2019q2$ to facilitate comparison.

First, the raw data shows no evidence of differential pre-trends across firms with heterogeneous exposure to the shock. Second, following the tax amnesty, firms with higher exposure experienced stronger total debt growth—particularly in dollar-denominated debt—relative to their lower-exposure counterparts.

We will revisit these results in Section 4.3 when we formally analyze the impact of the tax amnesty on firm outcomes.

Figure 4: Below *vs.* Above Median Firm Exposure



(a) Total Debt (in US Dollars) (b) Dollar Debt (in US Dollars)

Panel (a) shows the evolution of total debt, expressed in current US dollars, for firms below and above median exposure. The period covered is $2014Q1 - 2018Q2$. Panel (b) plots the evolution of dollar debt, expressed in current US dollars, for each group of firms. Panel (b) covers the period $2015Q4 - 2018Q2$ for which dis-aggregated data by firm-currency is available.

4.2 Bank Lending Channel

We begin by testing the presence of a bank lending channel in response to the funding shock generated by the tax amnesty. Specifically, we examine whether banks that were more exposed to the inflow of tax amnesty funds extended more credit than less-exposed banks. To isolate the effects of credit supply from potential demand-side confounders, we follow the within-firm estimator proposed by Khwaja and Mian (2008). This estimator exploits the fact that many firms borrow simultaneously from multiple banks and compares lending changes by differently exposed banks to the same firm, effectively controlling for any time-varying firm-specific shocks to credit demand.

Assume that there are only two time periods $t = 0, 1$, before and after the tax amnesty shock. Then, we can express the level of debt of firm i with bank b in period t as:

$$Y_{ib0} = \alpha_{i0} + \nu_{ib0} + \Gamma \mathbb{X}_{b0} + \epsilon_{ib0} \tag{2}$$

$$Y_{ib1} = \alpha_{i1} + \nu_{ib1} + \Gamma \mathbb{X}_{b1} + \beta S_b + \epsilon_{ib1} \tag{3}$$

The realization of Y_{ibt} depends on a number of objects. First, α_{it} captures firm-time credit demand shocks that a firm spreads homogeneously between all its lenders. Second, ν_{ibt} are credit demand shocks specific to a certain bank-firm relationship. For example, a firm needs a type of credit line that is offered only by a specific bank. $\mathbb{X}_{bt}$ captures all other bank-specific liquidity shocks except for the tax amnesty shock. Finally, S_b is our measure of bank-level exposure to the tax amnesty shock and ϵ_{ibt} is an idiosyncratic shock. Taking first differences yields:

$$\Delta Y_{ib} = \Delta\alpha_i + \Delta\nu_{ib} + \Gamma\Delta\mathbb{X}_b + \beta S_b + \tilde{e}_{ib} \tag{4}$$

We are interested in estimating β, which captures the marginal effect of an increase in the exposure of bank b on its lending to firm i. β has a causal interpretation under the condition that bank exposure is not correlated with other factors that affect changes in credit demand and bank liquidity. We take equation 4 to the data as follows:

$$\log L_{ibt} = \alpha_{it} + \nu_{ib} + \beta\ S_b \times Post + \Gamma\mathbb{X}_{b,pre} \times Post + e_{ibt} \tag{5}$$

In order to make *within-firm* comparisons we restrict the analysis to multi-bank firms. This allows us to incorporate firm-time fixed effects, α_{it}, which absorb time-varying firm-specific credit demand shocks that firms spread equally across all their lenders. We also include *firm-bank* fixed effects to control for aspects specific to each firm-bank match. *Post* is a dummy equal to one after the start of the tax amnesty. Lastly, we incorporate as bank-level controls a set of bank characteristics, measured at the quarter prior to the shock, and we interact them with our *Post* dummy. Based on the balance tests from Section 4.1, we include bank ownership status (private vs. public) and bank size as controls in our preferred specification.

Results Table 1 presents our results. Point estimates measure the percent increase in lending to firm i by bank b for every percentage point (p.p.) increase in bank exposure. The results in column (1) include only firm-time fixed effects. Column (2) adds bank-firm fixed effects and bank ownership controls. Our preferred specification, in column (3), also adds bank size controls. Comparing between banks in the 75th *versus* 25th percentile of exposure[9], we find an increase in lending of 10.7% .

In column (4) we allow for the effect of exposure to differ across exporters and non-exporters, where the exporting status of a firm is defined at baseline. Exporters are the main recipients of dollar bank credit[10] thus we would expect a higher impact on credit supplied to them. The coefficient on the interaction term is positive and statistically significant. If we compare between banks in the

[9]The difference in exposure between the 75th and 25th percentiles of exposure is 10 percentage points.

[10]In Appendix C we provide details on the distribution of dollar-denominated bank credit to firms at the start of the tax amnesty.

75th *versus* 25th percentile of exposure, we find an increase in lending to the *same* exporting firm of 18.4%. The same comparison for non-exporting firms yields a 7% increase in lending. In summary, we find evidence of a bank lending channel in response to the tax amnesty shock. Banks that were more exposed to the tax amnesty granted more credit relative to their less exposed counterparts. The effect was heterogeneous between lending to different types of firms. The impact on exporters was greater and more than double that of non-exporters.

Table 1: Bank Lending Channel

	(1)	(2)	(3)	(4)
Bank Exposure × Post	0.21**	1.17***	1.07***	0.71***
	(0.097)	(0.125)	(0.130)	(0.141)
... × Exporter				1.13***
				(0.395)
N	1655983	1655981	1655981	1655981
Firm-Time FE	Yes	Yes	Yes	Yes
Bank-Firm FE	No	Yes	Yes	Yes
Public Bank-Time FE	No	Yes	Yes	Yes
Bank Size - Time FE	No	No	Yes	Yes
R-squared	0.61	0.87	0.87	0.87

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered at the bank-firm level. Point estimates measure the percent increase in L_{ibt} for every $1p.p.$ increase in bank exposure, S_b. *Post* is equal to one between $(2016Q3, 2018Q2)$. The sample consists of all firms that have debt with two or more banks. We winsorize the top and bottom 1% of observations to limit the influence of outliers. Bank size is measured as the participation of each bank in total deposits at baseline and public bank is an indicator for a bank being publicly owned.

Identification Assumptions and Robustness Our empirical strategy for the bank lending channel exploits variation in the intensity of the liquidity shock across banks lending to the same firm, following Khwaja and Mian (2008).

The key identification assumption is that, conditional on our comprehensive set of fixed effects and controls, banks that received more amnesty funds were not differentially exposed to other unobserved shocks that affected either (i) their lending capacity (ii) or the credit demand they faced. Under this assumption, differences in post-amnesty lending behavior across banks capture a causal supply-side response to the inflow of funds.

Importantly, this identification strategy does not require that deposits have been randomly allocated between banks. Instead, it relies on a conditional exogeneity assumption typical of difference-in-differences approaches: absent the amnesty shock, lending patterns would have evolved similarly between banks with higher and lower exposure to the shock. In addition, the inclusion of firm-time fixed effects allows one to compare credit outcomes within a firm across banks. This attenuates

concerns related to firm-specific credit demand shocks.

To structure our identification discussion clearly, we distinguish two main potential threats, one arising from unobserved bank-level supply shocks and the other from firm-level credit demand shocks.

On the bank supply side, a potential threat to identification would arise if unobserved funding shocks or shifts in banks' lending capacities were correlated with the inflow of amnesty deposits. We address this concern by including controls for observable bank characteristics (such as bank size and ownership status) interacted with post-amnesty indicators.

On the credit demand side, our identification assumption further requires that changes in credit demand faced by banks are not systematically correlated with their exposure to the shock. To mitigate concerns about firm-level demand shocks, we exploit within-firm variation, following the methodology developed by Khwaja and Mian (2008). Specifically, by restricting our analysis to firms borrowing simultaneously from multiple banks with varying levels of exposure, we effectively absorb any firm-level shocks affecting their overall demand for credit. Panel (b) of Figure 3 illustrates this clearly, showing the differential credit growth for highly exposed versus less exposed banks using only firms that borrowed from both groups prior to the amnesty. Because each firm contributes to both high and low exposure bank groups, any observed differences post-amnesty cannot be attributed to aggregate firm-level demand shocks.

A remaining threat to our identification arises if there are bank-firm specific credit demand shocks, that is, shocks affecting certain bank-firm pairs disproportionately due to factors such as bank specialization or differential exposure to other simultaneous economic shocks. A particularly relevant potential shock relates to foreign trade financing. The contemporaneous trade liberalization that occurred in Argentina between 2016 and 2017 could have led international trade-oriented firms (exporters or importers) to redirect their credit demand specifically towards banks specialized in trade finance. If these banks were systematically more or less exposed to the amnesty-induced inflow, our estimates would be biased.

In Appendix E, we show results for a number of alternative specifications. First, we present results controlling for different measures of bank specialization and show that our findings remain largely unchanged. Second, we show that our results remain unchanged when adding, one at a time, additional bank-level controls to our preferred specification. Each of these robustness exercises is designed to address the identification threats identified above.

4.3 Firm-level Outcomes

In the previous section, we established that firms borrowed relatively more from banks that were more exposed to the tax amnesty shock. But did this relative shift towards more exposed banks result in more total borrowing by firms? Or did firms mostly reallocate their debt portfolio from less exposed towards more exposed banks? This section tackles this question, which is in turn key

to understanding the potential effects of the tax amnesty shock on firms' real performance.

We begin by describing our estimation strategy. For the remainder of this section, we will be interested in measuring the effect that firm-level exposure, S_i, had on a series of firm-level outcomes Y_{it} - including total credit, access to dollar credit markets, trade performance and employment growth. For simplicity, suppose that there were only two periods $t = 0, 1$ that correspond to before and after the tax amnesty, respectively. We can think of outcome Y_{it} as follows:

$$Y_{i0} = \alpha_i + \eta_{i0} + \Gamma \mathbb{X}_{b(i)0} + \epsilon_{i0}$$
$$Y_{i1} = \alpha_i + \eta_{i1} + \Gamma \mathbb{X}_{b(i)1} + \beta_2 S_i + \epsilon_{i1}$$

The realization of outcome Y_i in period $t = 0$ is the result of (i) a time-invariant firm shifter, α_i; (ii) a time-varying firm-level shifter, η_{it}; (iii) a credit supply shifter, $\mathbb{X}_{b(i)0}$, coming from the banks from which firm i sources its debt (other than the tax amnesty); plus, (iv) an idiosyncratic component ϵ_{i0}. In period $t = 1$, we add the credit supply shock resulting from the tax amnesty, S_i. Taking first differences yields:

$$\Delta Y_i = \Delta \eta_i + \beta_2 S_i + \Gamma \Delta \mathbb{X}_{b(i)} + \tilde{e} \tag{6}$$

Our main object of interest is β_2. It captures the causal effect of firm exposure on outcome Y_i. To identify β_2, two conditions must be satisfied. First, there should be no systematic correlation between changes in firm-level shifters and firm exposure to the shock. Formally, we need $E[\Delta \eta_i S_i] = 0$. For example, if Y_{it} is firm-level credit, then credit demand shocks hitting specific firms over time should not be related to their exposure to the shock. The second condition speaks to the correlation between firm exposure and, what can be thought of as, all other liquidity shocks hitting the banks from which firm i borrows. Formally, we need $E[\mathbb{X}_{b(i),t} S_i] = 0$. This would be challenged if more exposed banks were also more likely to be hit by other funding shocks than less exposed banks over time.

We take equation 6 to the data using the following difference-in-difference specification:

$$\log Y_{it} = \beta_2 \; S_i \times Post + \Gamma \mathbb{X}_{b(i)} \times Post + \gamma_i + \gamma_{jt} + e_t \tag{7}$$

where the dependent variable is the logarithm of the outcome of interest (unless otherwise noted) in period t, $Post$ is a dummy equal to 1 after the start of the tax amnesty, $\mathbb{X}_{b(i)}$ is a vector of firm-level weighted average bank controls, γ_i are firm fixed effects and γ_{jt} capture industry-time fixed effects. Firm-level bank controls are computed as weighted averages of baseline bank characteristics, where the weight is the share of each bank b in firms i debt at baseline. We study the time period between 2014-2018. The frequency of analysis is either quarterly or annual, depending on the outcome variable.

We address the concern that firm exposure could potentially be correlated with time-changing

shifters at the industry level, such as credit lines for specific industries, by adding time-industry fixed effects. These take care of any trends in firm-level shifters that are shared within narrowly defined industries (6-digit level). We take a conservative stance and include the same two bank controls as in our *within-firm* regressions and interact them with the *Post* dummy. These bank controls are ownership status (privately or publicly owned bank) and bank size, which we measure as a bank's participation in total deposits.

Lastly, we estimate the event-study analogue of Equation 7 to check for pretrends and to study the dynamic impact of the shock. Our baseline dynamic difference-in-difference specification is:

$$\log Y_{it} = \sum_{s \in (\pm h)} \left(\beta_s S_i + \Gamma_s \mathbb{X}_{b(i)} \right) \times \mathbb{1}(t = s) + \gamma_i + \gamma_{jt} + e_{it} \tag{8}$$

where $\mathbb{1}(t = s)$ are quarterly or yearly time dummies and h is the horizon of analysis (e.g., eight quarters before and after the shock). The coefficient β_s measures the s-period cumulative effect of firm exposure on outcome Y. The absence of pre-trends requires $\hat{\beta}_s = 0 \ \forall \ s \leq 0$.

In the following, we present our main findings that characterize how the tax amnesty affected firms through the financial channel.

Identification Assumptions Our identification assumption is that, conditional on a rich set of fixed effects, firms more exposed to the tax amnesty shock were not differentially affected by unobserved shocks to our outcomes of interest. This assumption does not require random assignment of firms to banks, nor does it require firms with different levels of exposure to have similar characteristics in levels. Instead, we rely on the standard parallel trends assumption: that in the absence of the tax amnesty, outcomes for treated and control firms would have followed similar trends.

Reassuringly, as the figures in this section will illustrate, none of the outcome variables display significant pretrends. Moreover, our firm-level loan data at quarterly frequency allow us to test whether the amnesty began affecting firms precisely in mid-2016. This timing test helps mitigate concerns that firms connected to more affected banks were also systematically exposed to other contemporaneous shocks.

We also control for firms operating in the same market, defined as industry-year, and include a comprehensive set of firm characteristics, such as export (import) destination countries, traded products, and bank types, interacted with time-fixed effects. These controls absorb the impact of time-varying unobserved shocks correlated with those firm or bank traits.

4.3.1 Firm Borrowing

In this section we explore the impact of the tax amnesty shock on credit growth and credit access through three different avenues. First, we study whether the positive effects identified in the within-firm regression extend to the overall borrowing behavior of firms. This aims to determine if firms

that were more exposed to the shock experienced higher credit growth - a mechanism often referred to as the *firm borrowing channel*. Second, we evaluate the performance of the dollar credit market, as the tax amnesty shock significantly enhanced banks' capacity to issue dollar-denominated credit. The well-functioning of foreign-currency, in particular dollars, credit markets has been pointed out as an important driver of firm performance, specially in emerging economies. Thus, we evaluate the tax amnesty's effectiveness in broadening and deepening the domestic market for dollar credit. Finally, we examine how the tax amnesty influenced the currency composition of firms' borrowing, shifting our focus from aggregate credit growth to the currency choices made by borrowing firms. By leveraging the currency-specific nature of the shock, we estimate the elasticity of substitution between peso- and dollar-denominated debt at the firm level. To our knowledge, this paper provides the first causal estimate of this parameter.

Total Credit We begin by analyzing the effect on firms' total borrowing. Table 2 reports the estimates from the two-way fixed effects specification in Equation (7) and Figure 5 shows the results of the dynamic panel.

We employ quarterly data for single and multi-bank firms[11], winsorize the dependent variable at the 1% level and cluster standard errors at the firm level. Reported coefficients measure the percentage increase in total credit for every additional percentage point in firm exposure.

Table 2 shows the results of running specification (7) with total firm credit as dependent variable. Column (1) only includes firm fixed effects and column (2) adds (6-digit) industry-time fixed effects. Both specifications show positive and statistically significant results. Column (3) shows the results from our preferred specification which includes firm fixed effects, (6-digit) industry-time fixed effects and firm-level bank controls. For the average firm, the effect of an additional percentage point in exposure is .97%. When comparing between firms in the 75th *versus* 25th percentiles of exposure[12], our estimates imply an increase of 9.7% in total credit. The magnitude of this effect is almost the same as that found for the *within-firm* exercise. We interpret this finding as evidence that exposure to the tax amnesty shock primarily influenced total firm credit, rather than firms reallocating existing borrowing across differentially exposed banks. In column (4) we add an interaction term with a firm's exporting status. We find that the effect of exposure was stronger among exporting firms, which is consistent with our *within-firm* findings.

[11]The *bank-lending* regressions restrict the sample to multi-bank firms and pre-existing bank relations. Firm-level regressions consider both single and multibank firms, plus all credit available to the firm at each point of time. Thus, total credit incorporates both preexisting bank relations and newly formed ones.

[12]The difference in exposure between a firm in the 75h percentile of exposure and one in the 25th percentile is 10 percentage points.

Table 2: Firm Borrowing - Total Credit

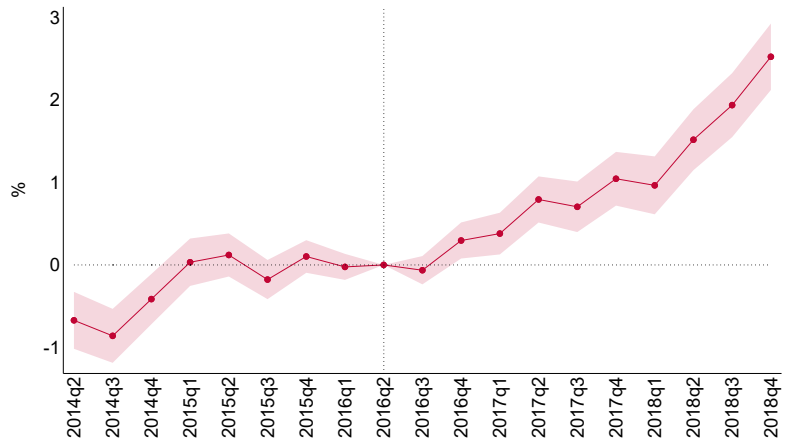
	(1)	(2)	(3)	(4)
Firm Exposure × Post	1.39***	1.21***	0.97***	0.86***
	(0.093)	(0.105)	(0.141)	(0.144)
... × Exporter				2.17***
				(0.613)
N	1362654	1355733	1355733	1358139
Firm FE	Yes	Yes	Yes	Yes
Industry-Time FE	No	Yes	Yes	Yes
Public Bank-Time FE	No	No	Yes	Yes
Bank Size - Time FE	No	No	Yes	Yes
Number of Firms	75879	75494	75494	75628
R-squared	0.85	0.85	0.85	0.85

Standard errors in parentheses and clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (6-digit) Industry-time fixed effects are allowed to differ across exporters and non-exporters. Bank size and bank ownership controls are firm-level averages. Nominal variables are winsorized at the top and bottom 1% to limit the influence of outliers. The time period spans between $2014Q2 - 2018Q2$ and the frequency of the data is quarterly. *Post* indicates the period $(2016q3, 2018q2)$. Point estimates measure the percent increase in total credit for every percentage point of firm exposure. The standard deviation of firm exposure is 10 percentage points. In column 4, the exporting status of a firm is defined at baseline.

We complement the above results by estimating the dynamic difference-in-difference specification outlined in equation (8). We set the base period to 2016q2 which corresponds to the quarter preceding the launch of the tax amnesty. The event study plot in Figure 5 shows no significant evidence of pretrends for at least a year and a half prior to the shock. The plot shows that the positive effects of firm exposure gradually build up over time with a peak effect of around 2% at the 2-year horizon.

The above results establish that firms that were more exposed to the tax amnesty shock, due to their pre-existing bank relations, experienced higher credit growth.

Figure 5: Total Credit



We report 95% confidence intervals. Standard errors are clustered at the firm level. We include firm fixed effects, (6-digit) industry-time fixed effects and firm level averages of bank size and bank ownership interacted with time dummies as additional controls. The base period corresponds to 2016q2. Nominal variables are winsorized at the top and bottom 1% to limit the influence of outliers. The plot shows the estimated coefficients on firm exposure for each quarter. Point estimates measure the percentage increase in total debt for every 1 percentage point increase in firm exposure relative to baseline. The standard deviation of firm exposure is 10 percentage points.

Access to Dollar Credit Markets The tax amnesty led to an increase in the foreign currency liquidity of banks, thus potentially allowing them to grant more dollar credit to domestic borrowers. The role of access to foreign currency credit markets for firm performance has received significant attention in corporate finance and international economics literature, particularly following the freezing of international markets during the Great Recession. Fluctuations in dollar credit availability are linked to reduced firm investment (e.g. Kalemli-Ozcan et al. (2016),Elias (2021)), diminished export dynamism (e.g. Paravisini et al. (2015c)), and weaker employment growth performance (e.g. Hardy (2023)). The tax amnesty shock resembles the opposite of a reversal in international capital flows, providing a unique opportunity to study how changes in foreign currency funding affect firms' participation in foreign-currency credit markets.

To examine the impact of the tax amnesty on firms' access to domestic dollar-denominated credit, we leverage that our dataset includes the loan's currency denomination from 2015q4 onward. The currency denomination of a bank liquidity shock theoretically increases loanable funds in both foreign and domestic currencies. However, due to currency mismatch regulations, domestic banks are restricted in how they can use dollar-denominated liabilities. Specifically, they must match dollar liabilities with dollar assets. This requirement creates a direct link between the currency denomination of the shock and the supply of dollar credit. In addition, access to domestic dollar credit is restricted to firms whose revenues are, at least partially, linked to the evolution of the dollar exchange rate. These firms are primarily exporters, suppliers of exporters, or producers of

highly tradable goods whose prices are set internationally. At the beginning of 2016, 27% of the firms in our sample had some amount of dollar-denominated bank credit. This percentage drops to 13% when considering only firms with an outstanding balance of at least 500[13] and increases to 50% when conditioned on the firm being an exporter and/or importer. Regarding the distribution of dollar credit, exporting firms held around 80% of total dollar credit at the start of the sample, while exporters and importers combined accounted for almost 90%. Appendix C provides further details on the distribution of domestic dollar credit before and after the tax amnesty.

As pointed out above, a small number of firms had dollar debt before the tax amnesty which restricts the power of intensive margin analysis. For this reason, we focus our analysis on the extensive margin effects of the tax amnesty. This allows us to study changes in access to dollar credit for a larger sample of firms. We define two outcomes related to the ability of firms to borrow in dollars. First, we construct an indicator variable that is equal to one if the firm i has positive dollar credit in period t, and zero otherwise. This variable allows us to estimate the effect of the tax amnesty on the probability of having access to dollar credit for the average firm. Second, we construct an indicator equal to one in every quarter in which the firm i experienced an increase in dollar credit, and zero otherwise. This outcome variable allows us to study a combination of intensive and extensive margin results without dropping firms with zero outstanding balances, giving a more general picture of the response of dollar credit growth for the average firm.

Table 3 summarizes our results. Columns (1) and (2) use the probability of having dollar credit as the dependent variable. Our results indicate that firm exposure had a positive and significant effect on this probability. Our preferred specification in column (2) finds that an additional percentage point of exposure increased it by .3 percentage points. Comparing between firms in the 75th *versus* 25th percentiles of exposure, we find an increase of 3 percentage points in the probability of borrowing in dollars. This represents a 10% increase relative to the unconditional probability of holding dollar debt at the start of the sample. In columns (3) and (4) we show results for our dollar credit growth indicator. We estimate that an additional percentage point of exposure increased the probability of dollar credit growth by .22 percentage points. When comparing between firms in the 75th *versus* 25th percentiles of exposure, this is an increase of 2.2 percentage points in our outcome variable.

Firm-level Currency Choices Next, we study how the tax amnesty shock affected the currency choices made by borrowing firms. Specifically, we exploit the currency-specific nature of the shock plus institutional features of Argentina's financial system to provide a causal estimate of firms' elasticity of substitution between pesos and dollar-denominated loans. Let κ_i denote this firm-level elasticity:

$$\kappa_i = \frac{\partial \ln \ell_i^{Pesos}}{\partial \ln \ell_i^*} \tag{9}$$

[13]This restriction excludes firms that use, for example, credit cards abroad, etc.

Table 3: Access to Dollar Credit Market

	(1) Dollar Debt	(2) Dollar Debt	(3) Dollar Debt Growth	(4) Dollar Debt Growth
Firm Exposure × Post	0.13***	0.31***	0.09***	0.22***
	(0.021)	(0.028)	(0.017)	(0.023)
N	832009	832009	832009	832009
Firm FE	Yes	Yes	Yes	Yes
Industry-Time FE	Yes	Yes	Yes	Yes
Public Bank-Time FE	No	Yes	No	Yes
Bank Size - Time FE	No	Yes	No	Yes
Number of Firms	75649	75649	75649	75649
R-squared	0.65	0.65	0.34	0.34

Standard errors in parentheses and clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (6-digit) Industry-time fixed effects are allowed to differ across exporters and non-exporters. Bank size and bank ownership controls are firm-level averages. The time period spans between $2015Q4 - 2018Q2$. *Post* indicates the period $(2016q3, 2018q2)$. Point estimates measure the *p.p.* increase in the dependent variable for every 1 *p.p.* of firm exposure.

where the subscript i identifies a firm and a variable with the superscript $*$ denotes a dollar-denominated quantity.

Emerging markets are recurrently subject to fluctuations in the availability and access to foreign-currency credit markets. For example, the global financial cycle is well documented in papers such as Obstfeld and Zhou (2022), Bruno and Shin (2023), Rey (2014), Miranda-Agrippino and Rey (2022), Boyarchenko and Elias (2024), among others. The real and aggregate effects of relative fluctuations in domestic versus foreign currency funding will partly depend on firms' ability to substitute one source of funds for the other. If both types of financing serve the same purposes or can be used to finance the same types of activities, then we would expect peso and dollar credit to act as substitutes for each other. However, different forms of financing may be better suited for different productive tasks based, for example, on the maturity of the project or the source of revenues. This can lead to complementarities between debt in different currencies, thereby limiting the ability of firms to hedge against fluctuations in dollar credit by turning to peso markets. Although there is little evidence on how these dynamic portfolio choices take place, knowledge of the substitution parameter is crucial for understanding the aggregate effects of fluctuations in the availability of foreign currency debt.

To identify κ_i in expression 9, we need an exogenous shift in the supply schedule of dollar-denominated loans that leaves the supply schedule of peso-denominated loans unchanged. We think that the tax amnesty shock provides a good quasi-experimental setting for this type of supply shifter. Concretely, this is equivalent to saying that domestic banks responded to the inflow of dollar funding mainly by increasing their willingness to supply dollar loans. The regulatory requirements that were in place at the time of the shock tightly limited domestic banks' on- and off-balance sheet currency exposure. In practice, this imposes a restriction on the type of positions that could be funded via the inflow of tax amnesty dollars. Therefore, the regulatory framework rules out a scenario in which domestic banks could directly use the increase in dollar liquidity to grant more

peso-denominated credit to firms and households. This brings us closer to the type of supply shifter needed to identify κ. However, there is still the possibility that the increase in dollar funding leads to an *indirect* change in the bank-level supply of pesos credit through other channels. For example, when firms move towards dollar credit they may free-up pesos-denominated lending capacity, which could cause banks to increase the supply of pesos loans and crowd-in firms that cannot access dollar-denominated loans. First, for this mechanism to pose a threat to our identification strategy, it should be the case that firms reduce their peso borrowing with the same bank with which they now borrow more in dollars. Otherwise, if the firm reduces its demand for peso loans uniformly across its lenders, then this would result in an increase in pesos-denominated loan supply at the system-wide level, but is not correlated with individual bank-level exposure. Second, this mechanism has the testable implication that more exposed banks should decrease their pesos lending rates by *relatively* more in response to the the shock to accommodate an increase in pesos lending. Additionally, other mechanisms, such as portfolio rebalancing, that indirectly affect peso lending decisions at the bank level would generally also manifest in differential price dynamics. In Appendix F, we show that bank-level exposure has no detectable effect on the evolution of the average pesos lending rate[14]. In light of this, we argue that it is plausible to assume that the tax amnesty shock operated mainly as a shifter to the supply schedule of dollar loans and we propose firm-level exposure, S_i, as an instrument for firm-level dollar credit growth.

Formally, consider the following reduced-form equation linking the evolution of peso-denominated loans to the evolution of dollar-denominated loans:

$$\Delta \ell^{Pesos}_{i,t,t+h} = \alpha_h + \kappa \Delta \ell^{*}_{i,t,t+h} + \epsilon_{i,t+h} \tag{10}$$

where $\Delta \ell^{Pesos}_{i,t,t+h}$ is the growth rate of peso debt of firm i between t and $t+h$, and similarly for dollars loans which are denoted by $\ell^{*}_{i,t,t+h}$. The error term, $\epsilon_{i,t+h}$ condenses all other supply and demand factors that affect firm-level demand for pesos credit. We estimate the parameter κ in equation 10 through two-stage least squares using firm exposure, S_i, as an instrument for the growth rate of dollar credit between the start of the tax amnesty, $t = 0$ and the horizon h. In addition to requiring S_i to capture solely a shift in the supply of dollar credit, we require it to be uncorrelated with firm-specific credit demand shocks, just as in the previous exercise. Therefore, the exclusion restriction for this specific exercise is stronger than before.

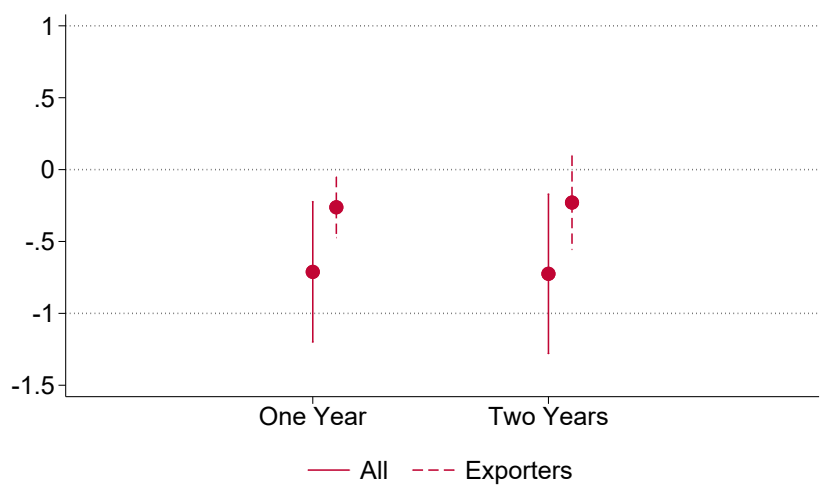
We take the panel-local projection in 10 to the data by including: i) (6-digit) industry fixed effects, ii) firm-weighted bank controls, and (iii) a one-period lag of the dependent variable. Industry fixed effects are a useful proxy for firm-specific credit demand shocks to the extent that this are explained by industry-wide trends. We include the same two firm-weighted bank variables to control for other potential changes in bank-level funding conditions, and we lag-augment the local projection (Montiel Olea and Plagborg-Møller (2021), Dube et al. (2023)). All standard errors are clustered

[14]We lack information on loan-level interest rates and average bank-level dollar lending rates which is why we have to limit ourselves to studying the evolution of bank-level pesos rates.

at the firm level.

Figure 6 presents our estimation results. We consider two sets of firms: i) any firm with dollar debt in $t = 0$, ii) exporters with dollar debt in $t = 0$. We present estimates for the elasticity of substitution at the one- and two-year horizons for each type of firm. We find evidence in favor of both types of debt being at least weak substitutes. When considering the broader set of companies, our point estimates are close to $-.7$, regardless of the horizon, and we cannot reject $\kappa = -1$ at the 10% level. Interestingly, we find evidence of weaker substitution among exporting firms, with point estimates that are half of those for all firms. In fact, at the two-year horizon, we cannot reject $\kappa = 0$ at the 10% level. Understanding the factors driving this heterogeneity is beyond the scope of this paper. However, we think that it could suggest that the nature of the productive process influences how easily firms can substitute between funding in different currencies.

Figure 6: Elasticity of Substitution between Pesos and Dollar Debt



We report 90% confidence bands. Standard errors are clustered at the firm level. The solid line corresponds to the sample of all firms holding dollar debt at baseline while the dotted line restricts the sample to exporting firms with dollar debt at baseline. First-stage F-statistics (Kleibergen-Paap rk Wald F statistic) are 8.92 (17.65) and 8.94 (11.19) at the one and two year horizons for all firms (exporters), respectively.

4.3.2 Imports

We study how the tax amnesty shock affected the importing behavior of firms. Our hypothesis is that improved access to credit, particularly dollar-denominated bank loans, helps credit-constrained firms overcome the sunk costs of discovering foreign suppliers, as well as finance international purchases.

We restrict the analysis to firms that were already importing before the amnesty and focus on annual imports. We then examine two margins of adjustment: (i) the intensive margin, which focuses on import volumes, and (ii) the extensive margin, focusing on the number of products and the number of origins.

The results of our static difference-in-difference are presented in Table 4. Just as in the analysis of firm credit outcomes, we include firm fixed effects, industry-time fixed effects and the same set of firm-level weighted average bank controls. We measure the number of imported products as the number of distinct products imported by a firm during each year. The number of origins is computed analogously.

Table 4: Effect on Imports

	(1) Total Imports	(2) Intermediates	(3) Capital Goods	(4) Origins	(5) Products
Firm Exposure × Post	1.27***	1.23***	0.44*	0.10	0.11
	(0.315)	(0.357)	(0.248)	(0.088)	(0.142)
N	57079	48875	33023	57079	57079
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Public Bank-Year FE	Yes	Yes	Yes	Yes	Yes
Bank Size-Year FE	Yes	Yes	Yes	Yes	Yes
Number of Firms	13489	11850	8660	13489	13489
R-squared	0.81	0.84	0.73	0.88	0.85

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered at the firm level. Point estimates in columns $1-3$ measure the percent increase in the outcome of interest for every 1 *p.p.* increase in firm exposure. Point estimates in columns $4-5$ are expressed in *p.p.*. The sample consists of all firms that were importing at the start of the tax amnesty. Nominal variables are winsorized the top and bottom 1% to limit the influence of outliers.

We find that higher firm exposure resulted in an increase in their total imports relative to less exposed firms. If we compare between firms in the 75th *versus* 25th percentile of exposure, we find an increase of 12.7% in total imports. We then look at different types of imported products and find evidence of an increase in both intermediate and capital goods. This is suggestive that firms were using the newly funds to invest. Lastly, we find no evidence that greater exposure to the tax amnesty affected the extensive margin, either through the introduction of new products or sourcing from new origins.

4.3.3 Exports

Next, we examine how increased access to credit affects export performance. Since exporting is a finance-intensive activity, looser dollar-credit conditions can influence exporting by easing borrowing constraints. An increase in exports can also be indicative of a firm increasing its productivity. Table 5 reports the main results. We restrict the sample to firms that were exporting before the tax amnesty. As in previous exercises, we include firm fixed effects, year fixed effects, and the same set of firm-level weighted average bank controls. We replace industry-time fixed effects with main exported product (6-digit HS) by time fixed effects to better account for time-varying demand shocks specific to the good being sold. Columns (1)–(3) sequentially add fixed effects and controls.

Throughout the specifications, we observe a positive effect on exports. Our preferred specification in column (3) shows a positive and significant effect of exposure on total exports. A comparison between firms at the 75th and 25th percentiles of exposure implies a 13.7 percent increase in exports.

Table 5: Effect on Exports

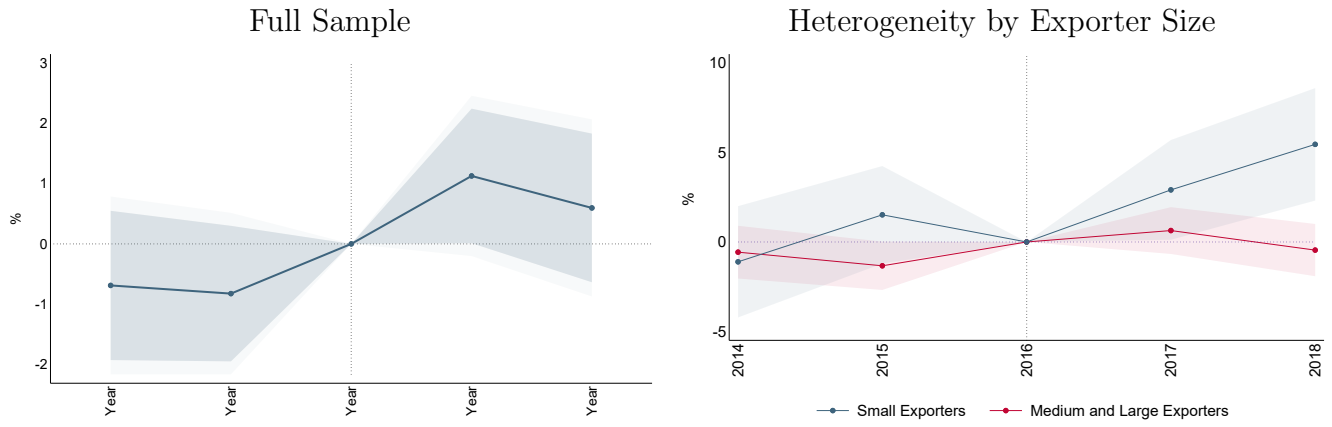
	(1)	(2)	(3)	(4)
Firm Exposure × Post	0.99**	1.10**	1.37**	
	(0.41)	(0.43)	(0.58)	
... × Exports Quantile = 1				4.04***
				(1.19)
... × Exports Quantile = 2				1.02
				(0.95)
... × Exports Quantile = 3				1.02
				(0.92)
... × Exports Quantile = 4				-0.09
				(0.87)
N	19075	19075	19075	19075
Firm FE	Yes	Yes	Yes	Yes
Product×Year FE	Yes	No	No	No
Product×Year×Destination FE	No	Yes	Yes	Yes
Bank Controls	No	No	Yes	Yes
Number of Firms	4,925	4,925	4,925	4,925
R-squared	0.92	0.92	0.93	0.93

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered at the firm level. Point estimates measure the percent increase in export values for every 1 p.p. increase in firm exposure. We restrict the sample to all firms that were exporting at the start of the tax amnesty. We winsorize the top and bottom 1% of observations to limit the influence of outliers.

We then study whether the effect of firm exposure was homogeneous between exporters of different sizes. If the increase in credit supply operates through a relaxation in firm's borrowing constraints, then we should see a greater impact for firms closer to this constraint or with less access to credit markets. In general, those are small- and medium-sized firms. To evaluate whether this is the case, we estimate the impact of firm exposure for each quartile of the exporting distribution. We report our findings in column (4). In fact, we find strong evidence that the increase in exports was driven mainly by smaller exporters (in terms of their exported values). If we compare between a highly exposed exporter and a low exposed exporter, both in the lower quartile of the export distribution, we find an increase of 40% in total exports. The estimated coefficients are positive and smaller for the two middle quartiles but statistically insignificant. Finally, the impact on the top quartile of exporters is null. These results are consistent with the fact that smaller firms tend to be more financially constrained. In addition,

Our dynamic difference and difference plots in Figure 7 complement these findings.

Figure 7: Exports



We report 95% confidence intervals. Standard errors are clustered at the firm level. The plot shows the estimated coefficients on firm exposure for each year. Point estimates measure the percentage increase in export values for every 1 p.p. increase in firm exposure. We restrict the sample to all firms that were exporting at the start of the tax amnesty and winsorize the top and bottom 1% of observations to limit the influence of outliers.

4.3.4 Labor Outcomes

Tax amnesties and inflows of capital have been studied for their potential effects on government revenue and capital flows, but a lesser-known potential outcome is the indirect benefits they may offer to employers and workers of firms connected with banks exposed to these capitals. By overcoming liquidity constraints, firms can increase investment and productivity, which can lead to an increase in the number of employees or the wages paid. In other words, the benefits of liquidity shocks stemming from tax amnesties can trickle down to workers. Although this effect is often overlooked, we shed light on this potential outcome and provide an analysis of the impact of tax amnesties on labor outcomes.

Table 6: Employment and wages

	(1) Employment	(2) Employment	(3) Employment	(4) Wages	(5) Wages	(6) Wages
Firm Exposure × Post	0.20***	0.17***	0.17***	0.01	0.02	0.02
	(0.03)	(0.04)	(0.05)	(0.02)	(0.03)	(0.03)
... × Post			0.08			0.06
			(0.15)			(0.09)
N	1258952	1258952	1258952	1258260	1258260	1258260
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Public Bank-Time FE	No	Yes	Yes	No	Yes	Yes
Bank Size - Time FE	No	Yes	Yes	No	Yes	Yes
Number of Firms	69828	69828	69828	69812	69812	69812
R-squared	0.96	0.96	0.96	0.85	0.85	0.85

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parentheses and clustered at the firm level. Point estimates measure the percent increase in the outcome of interest for every 1 p.p. increase in firm exposure. Employment refers to the total number of formal employees in each quarter t. Wages corresponds to the average nominal wage paid by each firm in quarter t. Bank controls include bank size and bank ownership status interacted with the *Post* indicator as in previous regressions.

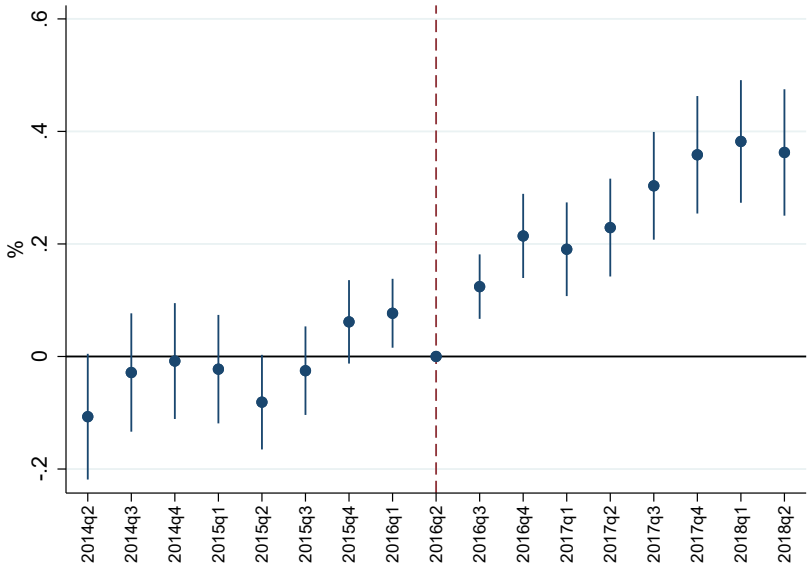
Table 6 reports the results for our difference-in-difference specification. The first two columns show results on employment without and with bank controls, respectively. The third column incorporates an interaction term for the export status. Columns (4)-(5) show results for firm-level average wages without and with bank controls, respectively, and column (6) adds an interaction term for export status. We find positive and significant effects of firm exposure on employment. The export status term is also positive, but statistically insignificant. Our findings imply that an average firm in the 75th percentile of exposure experienced an average increase of 2.1% in employment relative to the average firm in the 25th percentile, during the aftermath of the tax amnesty. In contrast, the impact of firm exposure to the tax amnesty on average wages is not statistically significant. In a context of rising employment, any increase or decrease in the wages of workers who remain with the firm could have been offset by changes in the composition of the workforce.

Lastly, Figure 8 shows the event study for firms' employment. Reassuringly, we observe no pretrends for these firms in the years before the event and a substantial and persistence increase in employment after the tax amnesty.

5 Conclusion

This paper identifies a new channel through which tax amnesties affect the economy: the financial channel. When designed to encourage asset repatriation, these policies can expand the financial sector, especially in underdeveloped credit markets.

Figure 8: Effect on Employees



We report 95% confidence intervals. Standard errors are clustered at the firm level. The plot shows the estimated coefficients on firm exposure for each quarter. Point estimates measure the percentage increase in employees for every 1 p.p. increase in firm exposure

We study Argentina's 2016 Tax Amnesty, one of the largest in history, and show that the inflow of hidden assets into domestic banks increased credit supply. Banks more exposed to these inflows expanded lending, which in turn improved firms' access to credit. This facilitated firm growth, as more exposed firms raised imports, exports, and employment.

Taken together, our findings reveal that tax amnesty policies can stimulate economic growth by expanding the financial sector, demonstrating effects beyond their direct fiscal impact. These results are particularly relevant for countries with underdeveloped financial systems, where the potential for growth through improved access to capital is significant.

The financial channel of tax amnesties has two main policy implications. First, it highlights an additional benefit that should be considered when evaluating such programs. Second, it suggests that policymakers should design incentives not only to encourage disclosure of hidden assets but also to promote their repatriation into domestic banks. On this point, Gil et al. (2024) provide evidence on mechanisms that increase disclosure. Future research can examine what mechanisms are most effective in fostering asset repatriation.

References

Alfaro, L., García-Santana, M., and Moral-Benito, E. (2021). On the direct and indirect real effects of credit supply shocks. *Journal of Financial Economics*, 139(3):895–921.

Allayannis, G., Brown, G. W., and Klapper, L. F. (2003). Capital structure and financial risk: Evidence from foreign debt use in east asia. *The Journal of Finance*, 58(6):2667–2710.

Alstadsæter, A., Johannesen, N., and Zucman, G. (2018). Who owns the wealth in tax havens? macro evidence and implications for global inequality. *Journal of Public Economics*, 162:89–100.

Amiti, M. and Weinstein, D. E. (2011). Exports and financial shocks. *The Quarterly Journal of Economics*, 126(4):1841–1877.

Amiti, M. and Weinstein, D. E. (2018). How much do idiosyncratic bank shocks affect investment? evidence from matched bank-firm loan data. *Journal of Political Economy*, 126(2):525–587.

Boyarchenko, N. and Elias, L. (2024). The global credit cycle. *FRB of New York Staff Report*, (1094).

Brown, M., Ongena, S., and Yeşin, P. (2011). Foreign currency borrowing by small firms in the transition economies. *Journal of Financial Intermediation*, 20(3):285–302.

Brown, M., Ongena, S., and Yeşin, P. (2014). Information asymmetry and foreign currency borrowing by small firms. *Comparative Economic Studies*, 56(1):110–131.

Bruno, V. and Shin, H. S. (2023). Dollar and exports. *The Review of Financial Studies*, 36(8):2963–2996.

Caggese, A. and Cuñat, V. (2013). Financing constraints, firm dynamics, export decisions, and aggregate productivity. *Review of Economic Dynamics*, 16(1):177–193.

Calvo, G. A. (2002). On dollarization. *Economics of transition*, 10(2):393–403.

Chodorow-Reich, G. (2014). The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *The Quarterly Journal of Economics*, 129(1):1–59.

Chor, D. and Manova, K. (2012). Off the cliff and back? credit conditions and international trade during the global financial crisis. *Journal of international economics*, 87(1):117–133.

Degryse, H., Havrylchyk, O., Jurzyk, E., and Kozak, S. (2012). Foreign bank entry, credit allocation and lending rates in emerging markets: Empirical evidence from poland. *Journal of Banking & Finance*, 36(11):2949–2959.

Dube, A., Girardi, D., Jorda, O., and Taylor, A. M. (2023). A local projections approach to difference-in-differences event studies. Technical report, National Bureau of Economic Research.

Elias, L. (2021). Capital flows and the real effects of corporate rollover risk. Technical report, Working paper, Federal Reserve Bank of New York.

Federico, S., Hassan, F., and Rappoport, V. (2023). Trade shocks and credit reallocation. Technical report, National Bureau of Economic Research.

Galindo, A., Panizza, U., and Schiantarelli, F. (2003). Debt composition and balance sheet effects of currency depreciation: a summary of the micro evidence. *Emerging Markets Review*, 4(4):330–339.

Gil, P., Holz, J., List, J. A., Simon, A., and Zentner, A. (2024). Toward an understanding of tax amnesty take-up: Evidence from a natural field experiment. *Journal of Public Economics*, 239:105245.

Hardy, B. (2018). Foreign currency borrowing, balance sheet shocks and real outcomes. *Balance Sheet Shocks and Real Outcomes (November 22, 2018). BIS Working Paper*, 1(758).

Hardy, B. (2023). Foreign currency borrowing, balance sheet shocks, and real outcomes. *Journal of International Money and Finance*, 139:102969.

Herreño, J. (2020). *Essays in Empirical Macroeconomics*. Columbia University.

Jiménez, G., Mian, A., Peydró, J.-L., and Saurina, J. (2020). The real effects of the bank lending channel. *Journal of Monetary Economics*, 115:162–179.

Kalemli-Ozcan, S., Kamil, H., and Villegas-Sanchez, C. (2016). What hinders investment in the aftermath of financial crises: Insolvent firms or illiquid banks? *Review of Economics and Statistics*, 98(4):756–769.

Kamil, H. (2012). *How do exchange rate regimes affect firms' incentives to hedge currency risk? Micro evidence for Latin America*. International Monetary Fund.

Kashyap, A. K. and Stein, J. C. (2000). What do a million observations on banks say about the transmission of monetary policy? *American Economic Review*, 90(3):407–428.

Khwaja, A. I. and Mian, A. (2008). Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review*, 98(4):1413–42.

Kohn, D., Leibovici, F., and Szkup, M. (2016). Financial frictions and new exporter dynamics. *International economic review*, 57(2):453–486.

Kohn, D., Leibovici, F., and Szkup, M. (2023). No credit, no gain: Trade liberalization dynamics, production inputs, and financial development. *International Economic Review*, 64(2):809–836.

Langenmayr, D. (2017). Voluntary disclosure of evaded taxes—increasing revenue, or increasing incentives to evade? *Journal of Public Economics*, 151:110–125.

Le Borgne, M. E. and Baer, M. K. (2008). *Tax amnesties: Theory, trends, and some alternatives*. International Monetary Fund.

Leibovici, F. (2021). Financial development and international trade. *Journal of Political Economy*, 129(12):3405–3446.

Lejour, A., Leenders, W., Rabate, S., and van't Riet, M. (2022). Offshore tax evasion and wealth inequality: Evidence from a tax amnesty in the netherlands. *Journal of Public Economics*.

Levchenko, A. A., Lewis, L. T., and Tesar, L. L. (2010). The collapse of international trade during the 2008–09 crisis: in search of the smoking gun. *IMF Economic review*, 58(2):214–253.

Londoño-Vélez, J. and Tortarolo, D. (2022). Revealing 21% of gdp in hidden assets: Evidence from argentina's tax amnesties. Technical report, UNU WIDER Working Paper 103.

Londoño-Vélez, J., Tortarolo, D., et al. (2022). Revealing 21 per cent of gdp in hidden assets: Evidence from argentina's tax amnesties. Technical report, World Institute for Development Economic Research (UNU-WIDER).

Manova, K. (2008). Credit constraints, equity market liberalizations and international trade. *Journal of International Economics*, 76(1):33–47.

Manova, K. (2012). Credit Constraints, Heterogeneous Firms, and International Trade. *The Review of Economic Studies*, 80(2):711–744.

Miranda-Agrippino, S. and Rey, H. (2022). The global financial cycle. In *Handbook of international economics*, volume 6, pages 1–43. Elsevier.

Montiel Olea, J. L. and Plagborg-Møller, M. (2021). Local projection inference is simpler and more robust than you think. *Econometrica*, 89(4):1789–1823.

Mora, N. (2013). The bank lending channel in a partially dollarized economy. *Journal of Applied Economics*, 16(1):121–151.

Muûls, M. (2015). Exporters, importers and credit constraints. *Journal of International Economics*, 95(2):333–343.

Obstfeld, M. and Zhou, H. (2022). The global dollar cycle. *Brookings Papers on Economic Activity*, 2022(2):361–447.

Paravisini, D. (2008). Local bank financial constraints and firm access to external finance. *The Journal of Finance*, 63(5):2161–2193.

Paravisini, D., Rappoport, V., and Schnabl, P. (2015a). Specialization in bank lending: Evidence from exporting firms. Technical report, National Bureau of Economic Research.

Paravisini, D., Rappoport, V., Schnabl, P., and Wolfenzon, D. (2015b). Dissecting the effect of credit supply on trade: Evidence from matched credit-export data. *The review of economic studies*, 82(1):333–359.

Paravisini, D., Rappoport, V., Schnabl, P., and Wolfenzon, D. (2015c). Dissecting the effect of credit supply on trade: Evidence from matched credit-export data. *The Review of Economic Studies*, 82(1):333–359.

Rey, H. (2014). The international credit channel and monetary autonomy. *Mundell-Fleming Lecture, International Monetary Fund*, 13.

Schnabl, P. (2012). The international transmission of bank liquidity shocks: Evidence from an emerging market. *The Journal of Finance*, 67(3):897–932.

Villacorta, A., Villacorta, L., and Gutierrez, B. (2023). (unobserved) heterogeneity in the bank lending channel: Accounting for bank-firm interactions and specialization. *Available at SSRN 4608656*.

A Data Cleaning

A.1 Bank Balance Sheet Data

Our initial dataset consisted of raw data from 64 banks operating domestically To ensure the reliability and relevance of our analysis, we applied a series of filters which narrowed down the dataset to a final sample of 46 banks.

We exclude: i) seven banks that were not operational in 2016; ii) two banks with no lending activity according to the credit registry data in certain years of our sample; iii) four banks that were not actively operating in all years of our sample; and, iv) five banks that reported zero deposits in all years. These filters leave us with a balanced panel of banks that actively participate in the domestic financial system. Our sample accounts for 97% of total private sector deposits and 96% of total credit granted to the private sector.

A.2 Data Quality

We combine two sources of micro data to construct bank credit variables. Both are collected by the Credit Registry of the Central Bank of Argentina. The first one is the core information collected at the firm-bank level with monthly frequency. This dataset contains outstanding end-of-month total credit levels for every bank-firm pair between 2013-2019. We supplement this data with an information Annex that started being recorded in 2015Q4. We source firm level data dis-aggregated by currency[15] from this Annex. Note that this information is not available at the firm-bank level, unlike the first dataset. Figure 9 compares aggregate bank credit from our two sources of micro data with the aggregate data published by the Central BankOur aggregate series, constructed with firm-bank data, accurately tracks the macro series. The aggregate series built from the Annex data performs relatively well in terms of tracking the slope of the macro data but misses its level. Importantly, Figure **??** shows that total dollar debt from the Annex closely tracks the evolution of dollar debt in the macro data. In Figure 13 we plot both aggregate dollar debt series in first differences against each other. Most data points lie at the 45 degree line[16].

The aggregate series for Peso debt that we construct out of the Annex data has a number of caveats. Figure 11 plots our results. First, discrepancies in the level of Peso debt account for the bulk of the difference between our total debt series and that of the macro data. Second, and most importantly, the Peso debt series from the Annex crosses the macro data during our period of analysis.

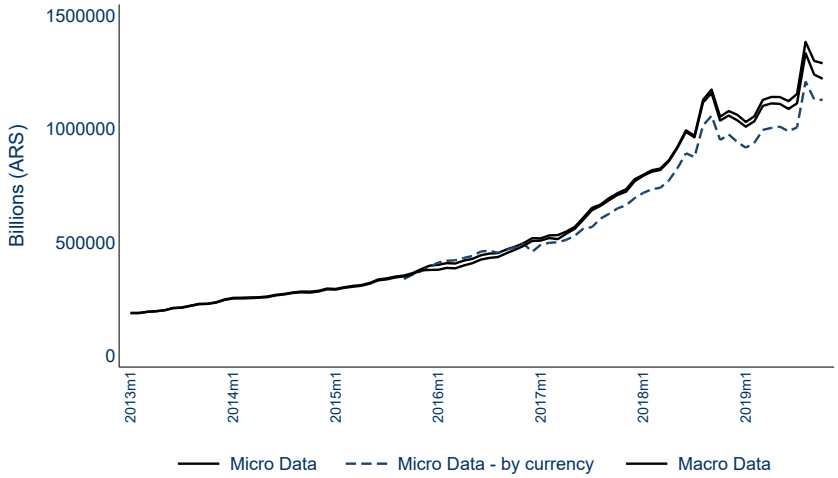
We choose a conservative approach and avoid using the pesos debt data from the Annex. Instead, we take total firm-level debt from the core dataset, firm-level dollar debt from the Annex and

[15] Banks separately inform the outstanding end-of-month level of debt in domestic and foreign currency. The latter is informed in US dollars.

[16] Since our empirical approach differences out the level of debt, this is the relevant statistic to assess the quality of our data.

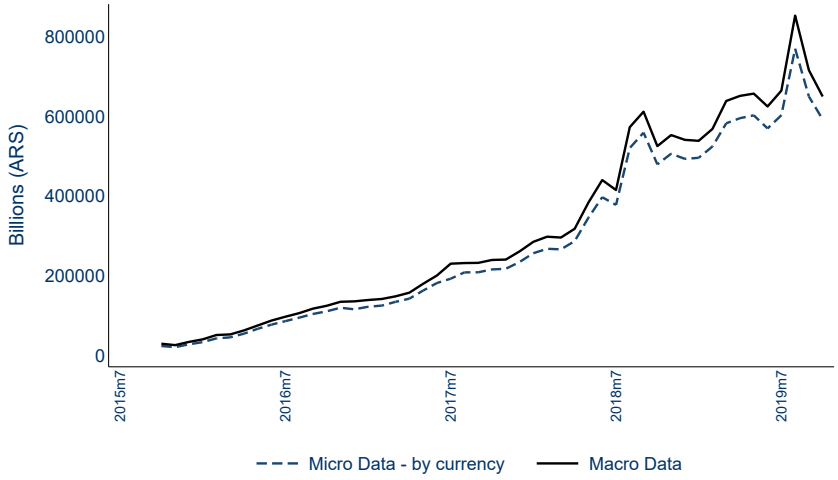
combine them to get an implicit measure of firm-level pesos debt. Figure 15 shows that, despite level differences, our implicit measure of pesos debt is well-aligned with the macro data once first differenced.

Figure 9: Total Bank Credit



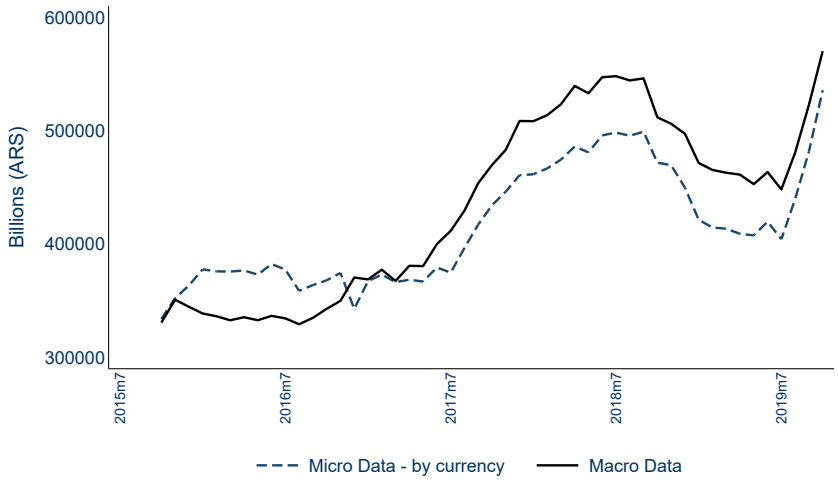
Data are expressed in billions of Argentine Pesos. Macro data corresponds to the end-of-period total credit granted to private non-financial firms published by the Central Bank of Argentina. The aggregated micro data excludes firms in the public and financial sectors to maximize comparison.

Figure 10: Dollar Bank Credit



Data are expressed in billions of Argentine Pesos. Macro data corresponds to the end-of-period total credit in foreign currency granted to private non-financial firms published by the Central Bank of Argentina. The aggregated micro data excludes firms in the public and financial sectors to maximized comparison.

Figure 11: Pesos Bank Credit (Raw)



Data are expressed in billions of Argentine Pesos. Macro data corresponds to the end-of-period total credit in domestic currency granted to private non-financial firms published by the Central Bank of Argentina. The aggregated micro data excludes firms in the public and financial sectors to maximized comparison.

Figure 12: Pesos Bank Credit (Implicit)

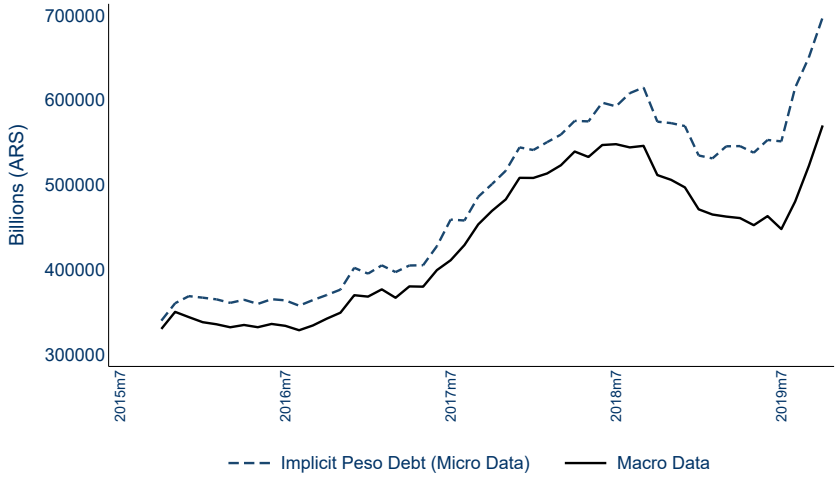


Figure 13: Dollar Bank Credit - First Differences

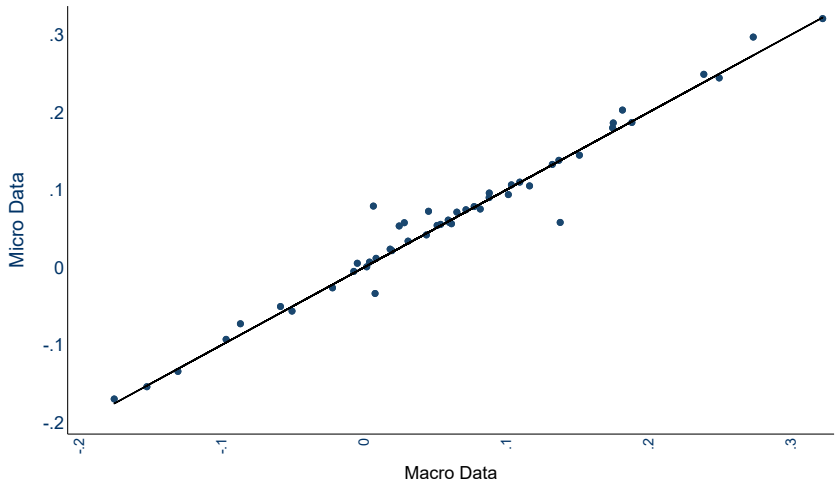
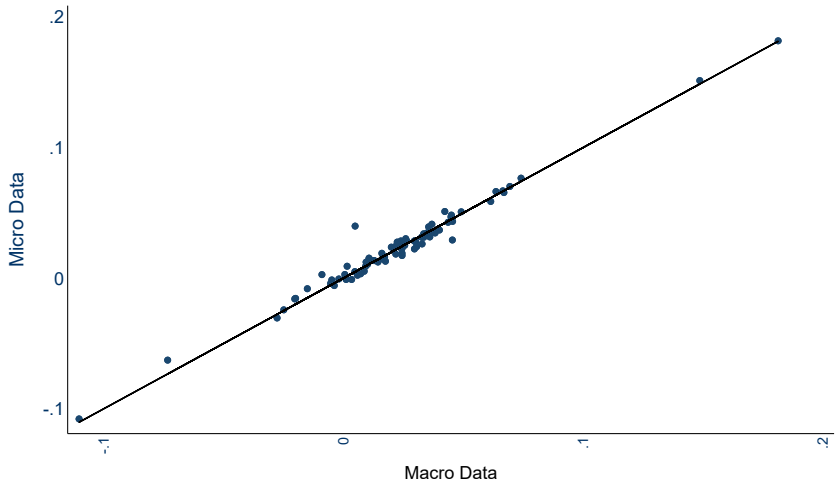
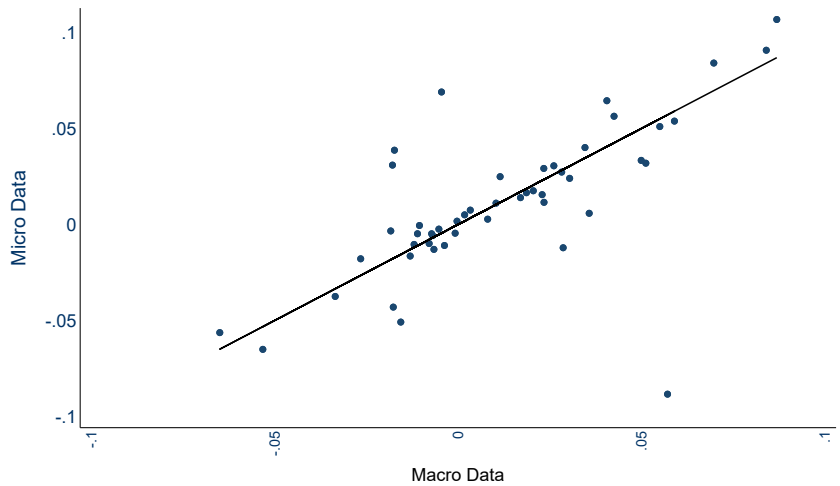


Figure 14: Total Bank Credit - First Differences



Original data was log transformed and first differenced. Micro data refers to the dollar series constructed using the Annex. Macro data corresponds to the aggregate debt series published by the Central Bank of Argentina

Figure 15: Pesos (Implicit) Bank Credit - First Differences



Original data was log transformed and first differenced. Micro data refers to the dollar series constructed using the Annex. Macro data corresponds to the aggregate dollar debt series published by the Central Bank of Argentina

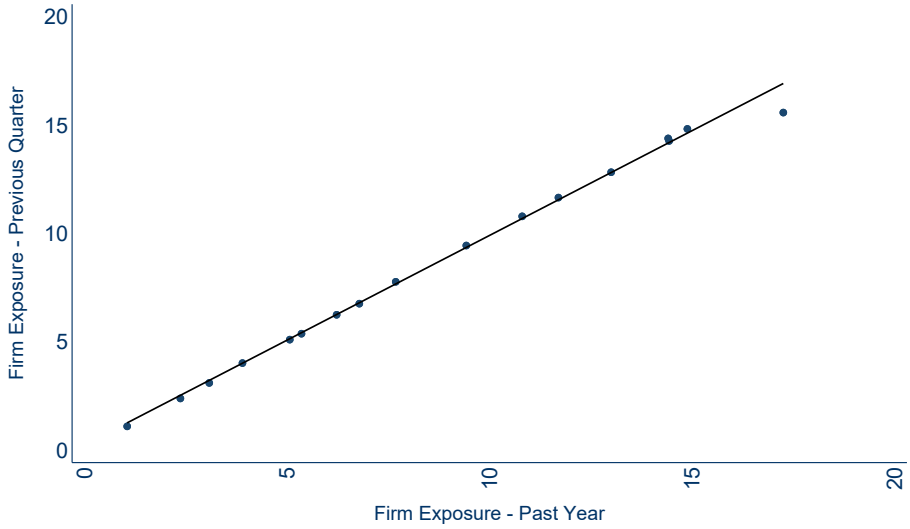
A.3 Alternative Measure of Firm Exposure

Our baseline measure of firm exposure is:

$$S_i = \sum_b \omega_{ib}^{Start} S_b \tag{11}$$

where the weights, ω_{ib}^{Start}, correspond to the participation of bank b in firm's i total debt in the quarter before the implementation of the tax amnesty (i.e. 2016Q2). The binscatter plot below shows that our measure of firm exposure is invariant to considering the average weight during the year prior to the tax amnesty. The correlation coefficient between these two alternatives is $\approx .97$.

Figure 16: Firm Exposure
Alternative Weights



The *x-axis* computes firm exposure using year-to-date weights while the *y-axis* uses last quarter's weights. The correlation coefficient between these two alternative ways of measuring firm exposure is $\approx .97$.

B Banking System in Argentina

When the tax amnesty started in Argentina, the banking system moved from an intervened ideology to a progressively free-market one. Several rules defined how loans, deposits, and reserves were managed in this context.

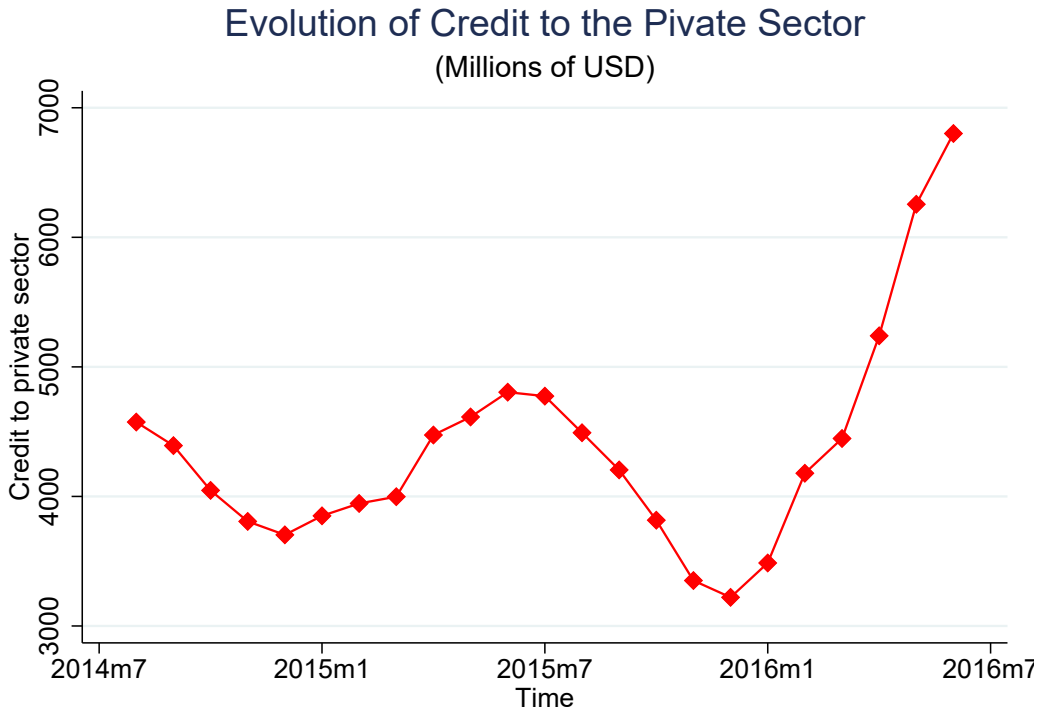
To begin with, strict rules defined what percentage of the banks' money had to be stored in reserves. This is called *encaje* and was differentiated between pesos and foreign currencies. The pesos' accounts and deposits were required to keep at least 20% in reserves, while US dollars' had to keep at least 25%[17]. On top of that, banks had to keep their net global position of foreign currency holdings below 15%. This could be relaxed if the long-term peso loans to the private sector increased or if regulatory capital increased[18].

Also, US dollar loans were not available for every firm. The only firms that had access to loans in foreign currencies were those related to exporting activities. Exporting activities, however, included firms that exported, their suppliers, indirect exporters, and services exporters[19]. These firms had authorization to give credit to their buyers for up to five years [20]. Also, firms could get dollar loans if they were going to invest abroad or in the energy sector and had contracts in dollars[21].

[17]Communications A 5980, A 5074, and A6007 from the BCRA
[18]Communication A 5997 from the BCRA
[19]Communications A 59095, and A 5916 from the BCRA
[20]Resolution number 242 from Secretaría de Comercio del Ministerio de la Producción
[21]Communication A 6031 from the BCRA

Related to the amnesty, in July, the central bank created the mandatory special accounts destined to store the incoming dollars. These accounts couldn't be accessed for six months or before March 31, 2017 (whatever was longer) except for buying government bonds or paying the amnesty taxes. The money stored in these accounts wasn't subject to interest rates, nor could it be retired in cash. Finally, the accounts were to be automatically closed once they were emptied, and every movement in them had to be reported to the AFIP (Argentina tax authority)[22].

Banks had other restrictions on their loans. One example of this was the Línea de Financiamiento para la Producción y la Inclusión Financiera (LFPIF). The LFPIF was a credit program established by the Banco Central de la República Argentina (BCRA) to support productive investments and financial inclusion, particularly for Micro, Small, and Medium Enterprises (MiPyMEs). For this program, big banks had to grant at least 15.5% of their loans to the private sector in pesos, while small banks had to keep the 9%[23].



C Composition of Dollar Debt

We group firms with positive dollar debt between 2015q4-2016q4 into six mutually exclusive groups: i(ii)) Exporters with dollar debt above (below) one thousand, iii(iv): Importers that do not export with dollar debt above (below) one thousand, v(vi): Firms that neither export nor import with dollar debt above (below) one thousand. Table 7 summarizes the characteristics of each group.

[22]Communication A 6022 form the BCRA

[23]Communications A 5917, A 5975, and A 6032 from the BCRA

Around two thousand exporting firmss take on the bulk of dollar debt with an average participation of 87%. They also have larger levels of average and median dollar debt per firm. Importers that do not export represent 5.5% of total dollar debt at baseline while the remainder of firms that neither export nor import take up 6.8%. The average and median dollar debt levels of both types of firms are orders of magnitudes lower than that of exporters. The industrial composition differs substantially between exporters and the other two groups of firms (regardless of their dollar debt level (above/below 1K). Among exporters, 70% belong to the industrial or agricultural sector. This share drops to 30% for non-exporting importers and to only 10% among neither exporting nor importing firms.

Table 7: Composition of Dollar Debt - At Baseline

	Average Debt	Median Debt	Participation(%)	Manufacturing(%)	Agricultural(%)	Number of Firms
Exporters (+1k)	1243541	34892	87.4	0.6	0.1	1893
Exporters(-1k)	272	171	0.0	0.7	0.0	2076
Importers(+1k)	115554	2335	5.6	0.3	0.0	1586
Importers(-1k)	260	162	0.0	0.3	0.0	3044
Non Exp./Imp.(+1k)	54666	2202	6.8	0.1	0.1	14659
Non Exp.Imp.(-1k)	190	80	0.1	0.1	0.1	17190

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered at the bank-firm level. Point estimates measure the percent increase in L_{ibt} for every $1p.p.$ increase in bank exposure, S_b. *Post* is equal to one between $(2016Q3, 2018Q2)$. The sample consists of all firms that have debt with two or more banks. We winsorize the top and bottom 1% of observations to limit the influence of outliers. Bank size is measured as the participation of each bank in total deposits at baseline and public bank is an indicator for a bank being publicly owned.

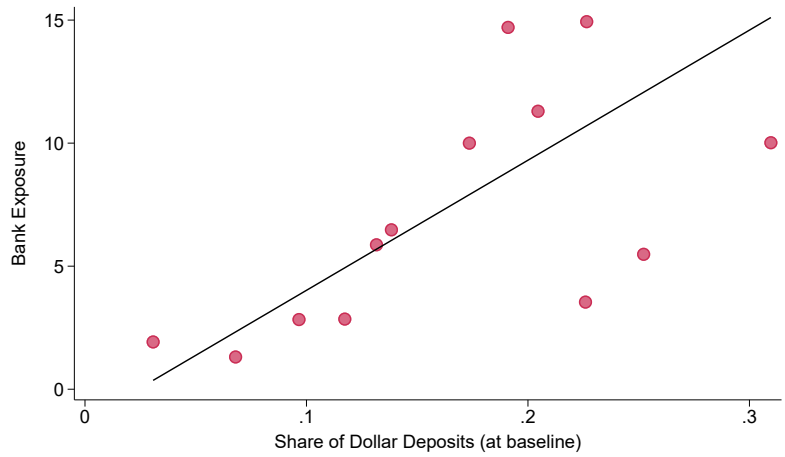
D Empirical analysis

D.1 Characteristics of Banks Receiveing Tax Amnesty Funds

Figure 18 shows a binscatter plot of bank exposure against the share (%) of dollar deposits before the shock.

We also leverage the mandatory six-month holding period for tax amnesty special accounts to track the movement of these funds once they become eligible for reallocation. If households have other accounts in the bank, it is expected that they will move their dollars within the same bank. As shown in Figure 1b, the balances in tax amnesty accounts declined sharply across banks following the end of this period. At the same time, overall dollar deposits within each bank remained largely stable. This stability suggests that individuals primarily transferred funds from special accounts to their existing standard dollar accounts within the same bank. To see this, Figure 18 presents a histogram of the percentage change in total dollar deposits between the first quarter of 2017—when most funds were required to be held in special accounts—and the second quarter of 2017, after the holding period expired. The median change is 3.8%, and 85% of banks in the sample experienced

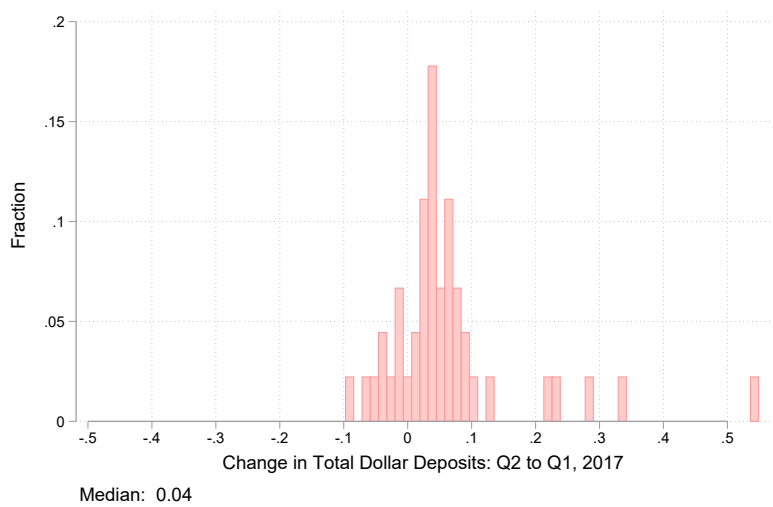
Figure 17: Bank Exposure and Dollar Deposit Share



The share of dollar deposits in the $x - axis$ is the average share for $2016Q2$.

fluctuations of less than 10%.

Figure 18: Total Dollar deposits by bank: Changes between Q2 2017 and Q1 2017



The x-axis represents the change in total dollar deposits between Q2 2017 (period in which the mandatory special accounts were not binding) and Q1 2017 (period in which the bandatory special accounts were binding).

E Robustness Checks

E.1 Bank Lending Channel

This section presents robustness results for the bank lending exercise. Table 8 shows results when we flexibly control for bank specialization at the industry and international trade financing level.

Table 9 presents additional results where we control for a battery of bank level characteristics interacted with a the post treatment dummy. Overall, our results remains remarkably stable across specifications.

Table 8: Bank Lending Channel - Bank Specialization Controls

	Baseline	Industry$_A$	Exporter$_A$	Importer$_A$	Industry$_B$	Exporter$_B$	Importer$_B$
Bank Exposure × Post	1.10***	1.11***	0.99***	0.84***	1.10***	1.09***	1.11***
	(0.132)	(0.132)	(0.132)	(0.131)	(0.132)	(0.132)	(0.132)
N	1635729	1635496	1635729	1635729	1635496	1635729	1635729
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Public Bank-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Size - Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.87	0.87	0.87	0.87	0.87	0.87	0.87

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered at the bank-firm level. The subscript A is used to denote the specialization measure computed as the share of credit from bank b going to firm type X, where X is either industry, exporting or importing status. The subscript B denotes the specialization measure computed as the share of credit to firm type X supplied by bank b. Point estimates measure the percent increase in L_{ibt} for every $1p.p.$ increase in bank exposure, S_b. $Post$ is equal to one between $(2016Q3, 2018Q2)$. The sample consists of all firms that have debt with two or more banks. We winsorize the top and bottom 1% of observations to limit the influence of outliers. Bank size is measured as the participation of each bank in total deposits at baseline and public bank is an indicator for a bank being publicly owned.

Table 9: Bank Lending Channel - Bank-level Time Varying Controls

	Baseline	Leverage	Dollar Assets	Default(%)	ROE	CB Bonds
Bank Exposure × Post	1.10***	1.00***	1.10***	1.20***	1.03***	1.34***
	(0.132)	(0.131)	(0.132)	(0.135)	(0.133)	(0.141)
N	1635729	1635729	1635729	1635729	1635729	1635729
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Public Bank-Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank Size - Time FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.87	0.87	0.87	0.87	0.87	0.87

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered at the bank-firm level. Each column incorporates an additional interaction between the post indicator and a bank characteristic. Leverage measures bank leverage, dollar assets measures the participation of dollar securities in total dollar assets, default is the share of private sector loans in default, ROE measures return on equity and CB Bonds measures the participation of bonds issued by the central bank on total assets. Point estimates measure the percent increase in L_{ibt} for every $1p.p.$ increase in bank exposure, S_b. *Post* is equal to one between $(2016Q3, 2018Q2)$. The sample consists of all firms that have debt with two or more banks. We winsorize the top and bottom 1% of observations to limit the influence of outliers. Bank size is measured as the participation of each bank in total deposits at baseline and public bank is an indicator for a bank being publicly owned.

F Elasticity of Substitution Between Peso and Dollar Debt

F.1 Pesos Lending Rate and Bank Exposure

We test whether bank exposure had an impact on the average pesos lending rate set by individual banks. A differential response of the peso-lending rate across banks with different exposure would pose a threat to our identification assumption. Unfortunately, the only information that we have on interest rates is the average lending rate for the whole portfolio of peso-denominated loans granted to the private sector. This series includes both loans to households and private non-financial firms. It also reflects the specific composition of credit lines in each bank portfolio which are likely to be heterogeneous. In the exercise below, we include several controls in an attempt to reduce the influence of heterogeneity in the loan portfolio composition on our estimates.

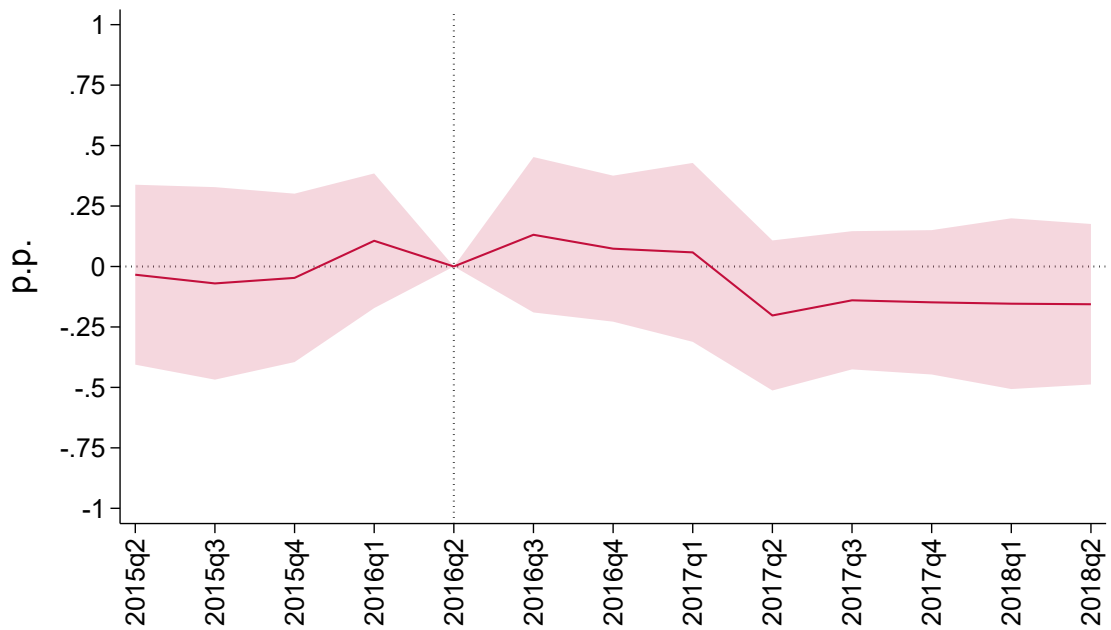
We run the following lag-augmented panel local projection:

$$\Delta i^{Pesos}_{bt+h} = \beta_h S_b + \Gamma_h \mathbb{X}_b + \sum_{s=1}^{2} \delta_{hs} i^{Pesos}_{bt-s} + e_{bt+h} \tag{12}$$

where i^{Pesos}_{t+h} is bank's b average pesos lending rate h periods after the start of the tax amnesty and

S_b is bank exposure. β_h measures the differential effect of bank exposure on the pesos lending rate at horizon h. We let $h = \{-4, 8\}$ which allows us to check for pre-trends. $\mathbb{X}_b$ is a vector of bank level controls. On top of the bank controls included in the main text (i.e. bank size and bank ownership status) we add the share of commercial loans at baseline. This additional control helps us account for differences in the composition of loan portfolios. We incorporate two lags of the dependent variable and cluster standard errors at the bank level. Figure 19 plots the estimated coefficients. We do not find significant evidence of a differential response of pesos lending rates across banks with differential exposure to the tax amnesty shock. For reference, the average lending rate was 32.3% during this time period.

Figure 19: Bank Exposure and Pesos Lending Rates



We report 95% confidence intervals. Standard errors are clustered at the firm level.Point estimates measure the percentage point increase in the pesos lending rate for every 1p.p. increase in bank exposure, relative to baseline. The time period corresponds to 2015Q2 2018Q2 and the base period is set to 2016q2. Standard errors are clustered at the bank level.

F.2 General Expression for κ

Consider a setting where firm-level demand for pesos and dollar credit satisfy:

$$L_{it}^{Pesos} = f(Y_{it}, r_{it}^{Pesos}, r_{it}^{USD}) \tag{13}$$

$$L_{it}^{USD} = h(Y_{it}, r_{it}^{Pesos}, r_{it}^{USD}) \tag{14}$$

where $f(.)$ and $h(.)$ are arbitrary functions mapping firm-level output, Y_{it}, peso and dollar lending rates,$r_{it}^{Pesos}, r_{it}^{USD}$, to credit demand in each currency. Totally differencing both expressions

with respect to the dollar interest rate yields

$$\mathbf{d}L_{it}^{Pesos} = \frac{\partial f}{\partial Y_{it}} \frac{\partial Y_{it}}{\partial r_{it}^{USD}} \mathbf{d}r_{it}^{USD} + \frac{\partial f}{\partial r_{it}^{Pesos}} \frac{\partial r_{it}^{Pesos}}{\partial r_{it}^{USD}} \mathbf{d}r_{it}^{USD} + \frac{\partial f}{\partial r_{it}^{USD}} \mathbf{d}r_{it}^{USD} \tag{15}$$

$$\mathbf{d}L_{it}^{USD} = \frac{\partial h}{\partial Y_{it}} \frac{\partial Y_{it}}{\partial r_{it}^{USD}} \mathbf{d}r_{it}^{USD} + \frac{\partial h}{\partial r_{it}^{Pesos}} \frac{\partial r_{it}^{Pesos}}{\partial r_{it}^{USD}} \mathbf{d}r_{it}^{USD} + \frac{\partial h}{\partial r_{it}^{USD}} \mathbf{d}r_{it}^{USD} \tag{16}$$

Our identifying assumption implies that we have at hand a shifter to the dollar interest rate, $\mathbf{d}r_{it}^{USD}$, that leaves the supply of pesos credit unchanged, such that, $\frac{\partial r_{it}^{Pesos}}{\partial r_{it}^{USD}} |_{\mathbf{d}r_{it}^{USD}} = 0$. Re-arraing yields:

$$\frac{\mathbf{d}L_{it}^{Pesos}}{\mathbf{d}L_{it}^{USD}} = \left[\frac{\frac{\partial f}{\partial Y_{it}} \frac{\partial Y_{it}}{\partial r_{it}^{USD}}}{\frac{\partial h}{\partial Y_{it}} \frac{\partial Y_{it}}{\partial r_{it}^{USD}} + \frac{\partial h}{\partial r_{it}^{USD}}} + \frac{\frac{\partial f}{\partial r_{it}^{USD}}}{\frac{\partial h}{\partial Y_{it}} \frac{\partial Y_{it}}{\partial r_{it}^{USD}} + \frac{\partial h}{\partial r_{it}^{USD}}} \right] \tag{17}$$

G Quantification: Effect on GDP and employment

G.1 System of Equilibrium Conditions

The solution to this system yields a full set of equilibrium prices and quantities that are mutually consistent across all sectors of the economy, conditional on the size and distribution of the initial deposit shock.

G.1.1 Household

Consumption bundle:

$$\widehat{c} = \left[\sum_i \Lambda_i \widehat{c_i}^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}} \tag{18}$$

The household's labor supply. We assume the aggregate price level is the numeraire ($\widehat{p} = 1$).

$$(\widehat{h})^{\psi} = \frac{\widehat{w}}{\widehat{p}} \tag{19}$$

Consumption for each good i:

$$\widehat{c_i} = \left(\frac{\widehat{p_i}}{\widehat{p}} \right)^{-\sigma} \widehat{c} \tag{20}$$

G.1.2 Firms

Each firm i produces output $\hat{y}_i$ using a linear technology with labor $\hat{h}_i$ and productivity $\hat{A}$.

$$\widehat{y_i} = \widehat{A}\widehat{h_i} \tag{21}$$

Firms demand labor where the marginal cost (including financing costs from the working capital constraint, θ) equals the marginal revenue product.

$$\widehat{w}(1+\theta\widehat{r_i^L}) = \widehat{A}\widehat{p_i} \tag{22}$$

The firm's demand for loans is determined by its working capital needs.

$$\widehat{L}_i = \widehat{w}\widehat{h_i} \tag{23}$$

G.1.3 Financial Sector: Loan Aggregators and Banks

A competitive aggregator bundles loans from different banks $\{\widehat{L_{ib}}\}$ into a single loan bundle $\widehat{L}_i$ for each firm.

$$\widehat{L}_i = \left[\sum_b \gamma_{ib}(\widehat{L_{ib}})^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\epsilon}{\epsilon-1}} \tag{24}$$

The demand for a loan from a specific bank b by firm i depends on the bank's relative interest rate.

$$\widehat{L_{ib}} = \left(\frac{1+\widehat{r_b^L}}{1+\widehat{r_i^L}}\right)^{-\epsilon}\widehat{L}_i \tag{25}$$

Each bank sets its lending rate based on its funding cost, which is decreasing in its deposit level $\widehat{D_b}$.

$$1+\widehat{r_b^L} = 1+\widehat{r}-\phi\ln(\widehat{D_b}) \tag{26}$$

The level of deposits is determined exogenously by the amnesty shock S_b, which is the entry point of the shock into the model.

$$\widehat{D_b} = 1+S_b \tag{27}$$

The bank's balance sheet must hold, where total loans are funded by deposits, equity, and other liabilities.

$$\widehat{L_b} = \Omega_b^D\widehat{D_b} + \Omega_b^K\widehat{K_b} + \Omega_b^N\widehat{N_b} + \Omega_b^O \tag{28}$$

Total loans supplied by a bank is the sum of its loans to all firms.

$$\widehat{L_b} = \sum_i \omega_{ib}^L\widehat{L_{ib}} \tag{29}$$

G.1.4 Market Clearing and GDP

The labor market must clear.

$$\widehat{h} = \sum_i \Lambda_i^h\widehat{h_i} \tag{30}$$

The aggregate market for good must clear.

$$\widehat{c} = \widehat{y} \tag{31}$$

The market for each good i must also clear.

$$\widehat{c_i} = \widehat{y_i} \tag{32}$$

Finally, Real GDP is defined as total final consumption.

$$\widehat{GDP} = \widehat{c} \tag{33}$$

G.2 Computational Model

This appendix details the structure of the computational model used to obtain the simulation results. The model is specified as a system of simultaneous equations representing the economy's general equilibrium. All variables are expressed as log-deviations from an initial steady state, a method commonly referred to as "hat algebra."

The solution method involves defining a function that represents the full set of equilibrium conditions. This function takes two main inputs:

1. A vector containing the current values of all endogenous variables in the model.

2. A set of structural parameters and the amnesty bank deposit shock (S_b).

The function returns a vector where each element corresponds to an equilibrium condition written in the form $LHS - RHS = 0$. A numerical root-finding algorithm is then used to find an equilibrium vector of variables that makes all elements of this returned vector simultaneously equal to zero. The initial guess for the value of endogenous variables is a vector of ones.

The model is solved under a specific scenario: the only exogenous disturbance is a shock to bank deposits, S_b, which is heterogeneous across banks. This isolates the transmission mechanism of the financial shock through the bank lending channel. The model is solved for $B = 4$ and $I = 100$. We calibrate the exposure of each bank as follows. In the data, we rank banks by exposure and bin them into four groups each accounting one fourth of total lending. This leaves us with four bank types. We then rank firms by exposure and bin them into 100 firm types each accounting for one percent of total lending. For each firm bin we compute the share of lending from each bank type at baseline and compute bin-level exposure as in the empirical section.

An equilibrium of the model is defined as a set of prices, wages, interest rates, and allocations. This set includes prices for each intermediate good $\{\hat{p}_i\}_i$, the aggregate wage $\hat{w}$, and interest rates at both the firm $\{\hat{r}_i^L\}_i$ and bank $\{\hat{r}_b^L\}_b$ levels. It also consists of the corresponding allocations for aggregate consumption $\hat{c}$ and labor $\hat{h}$; firm-level output $\{\hat{y}_i\}_i$, labor $\{\hat{h}_i\}_i$, and loans $\{\hat{L}_i\}_i$; and

bank-level variables including total loans $\{\hat{L}_b\}_b$, deposits $\{\hat{D}_b\}_b$, foreign funding $\{\hat{N}_b\}_b$, and the full matrix of firm-bank specific loans $\{\hat{L}_{ib}\}_{i,b}$. These variables, along with the resulting real $\widehat{GDP}$, are such that the final consumption good, loan, and labor markets clear, and all bank balance sheets hold.