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Credit Supply Shocks During a Non-Financial Recession

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Abstract

We study the drivers and real effects of credit supply shocks during a major non-financial recession, the COVID-19 crisis. Using data on the universe of bank loans in Mexico, we isolate the supply-driven component of credit variations. Credit supply conditions deteriorated in this period, driven by banks' heightened risk aversion. Using matched employer-employee records, we find that negative credit supply shocks reduced firms' employment and increased their exit probability. These effects are larger among financially constrained firms and workers with lower separation costs. In the aggregate, negative credit shocks account for one-third of the total employment decline for small firms.

JEL Classification: G21, E44, E51, J23

Keywords: Recession, banking, credit supply, risk tolerance, firm dynamics, job destruction

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1 Introduction

The literature has documented the effect of credit availability—namely, the bank lending channel—particularly during financial recessions. However, it is unclear whether the impact or drivers of credit supply shocks are similar in non-financial crises. In financial crises, banks’ balance sheets are directly compromised, thus limiting their lending capacity and causing sizable effects on employment and investment (Chodorow-Reich (2013), Cingano, Manaresi, and Sette (2016), Bentalila, Jansen, and Jimenez (2017), Acharya et al. (2018)).¹ Instead, in non-financial recessions, well-capitalized banks could help firms smooth an aggregate shock. However, precautionary motives amid significant uncertainty could make banks cut lending even when their fundamentals do not deteriorate, thus potentially causing significant effects in the real sector. This paper contributes to bridging this gap in the literature by examining the drivers and real effects of credit supply shocks across different stages of a major non-financial recession.

Analyzing bank credit supply during a non-financial crisis can provide a more comprehensive understanding of the role of credit in different recessions and help design effective public policies. However, in contrast to what we know about other recessions, we have scant evidence on the drivers and real effects of credit supply during non-financial ones, as both data limitations and the concurrence of multiple shocks and policy interventions make identifying the causal effects a complex task. In this paper, we overcome these challenges and study the real effects of credit supply shocks on firms in Mexico during the COVID-19 episode, the most significant non-financial recession in the last 100 years.

While this recession was unusual in that a health crisis triggered it, it offers valuable insights beyond this specific episode. The pandemic shock generated conditions that may arise in future crises, such as in recessions caused by natural disasters or large trade disruptions, although not studied exhaustively. For instance, as highlighted by Goldstein, Koijen, and Mueller (2021), the COVID-19 health emergency significantly disrupted the supply and demand of goods. As a

¹See also Berton et al. (2018), Alfaro, García-Santana, and Moral-Benito (2021), among others.

result, it was a period when firms and households experienced extraordinary liquidity needs and widespread uncertainty, while the banking sector was ex-ante well-capitalized and highly liquid. Nevertheless, banks mostly contracted credit, particularly to Small and Medium Enterprises (SME) (Li, Strahan, and Zhang (2020), Greenwald, Krainer, and Paul (2020), and Chodorow-Reich et al. (2022)). This suggests, first, that bank fundamentals may not be the primary driver of credit supply in non-financial recessions and, second, that in the presence of a large share of SMEs, credit supply could have significant real effects even when the crisis is not of financial origins.

We focus on Mexico because it is an ideal setting for studying the role of credit supply during the pandemic for several reasons. First, there is a large share of SMEs for which the banking sector is the primary source of funding.² Besides, unlike most other economies, Mexico did not offer substantial government support or large-scale public funding programs to businesses during the pandemic. Thus, firms' reliance on private banks for financing, coupled with the absence of confounders in terms of governmental assistance, allows us to better isolate the impact of credit supply shocks. Moreover, we can identify these shocks by leveraging the availability of supervisory loan-level bank–firm–matched data that, in turn, can be merged with confidential administrative employer–employee records. These records allow us to build a panel on employment and credit that covers the universe of formal firms between 2018 and 2021.

As various concurrent shocks characterized the COVID-19 pandemic, we opt for an approach that does not require specifying a particular source driving credit supply but still captures the overall changes in banks' conditions. Thus, we estimate bank-level credit supply shocks following the methodology of Degryse et al. (2019), a variation of the one developed by Amiti and Weinstein (2018) that allows us to decompose overall changes in bank credit into demand and supply components. Specifically, we group firms into categories defined by their industry, location, and size, and regress bank–firm credit changes on firm–group–time and bank–time fixed effects.

²According to the National Business Financing Survey (*Encuesta Nacional de Financiamiento de las Empresas, ENAFIN*), 67% of firms seeking funding approached commercial banks for a loan.

The former controls for demand, while the latter captures changes in credit associated with each bank’s specific conditions. These bank fixed effects are the parameters of interest, as they capture credit supply factors. We document that credit supply shocks deteriorated, i.e., turned on average more negative as the recession advanced, suggesting that, on average, banks restricted their supply of credit during this period, despite the large demand for it.

Given our data-driven approach, we use balance sheet and confidential microdata from Mexico’s Senior Loan Officer Opinion Survey (SLOOS) to validate and study the drivers of our estimated shocks. We document that changes in credit supply were driven primarily by variation in risk tolerance towards large firms, rather than by bank fundamentals such as portfolio quality, funding availability, or liquidity position—which was strong at the onset of the pandemic and actually improved afterwards. This novel finding suggests that even in periods of high liquidity and capitalization, banks’ precautionary motives and changes in risk aversion can constrain credit growth. In such contexts, attempts to inject liquidity are less effective (and have lower take-up rates). Consistent with this interpretation, banks did not use the large credit lines offered by the Central Bank during this period.

Despite the non-financial origin of the recession, banks’ credit supply contraction due to this precautionary behavior transmitted to the real sector. To quantify this impact, we measure each firm’s exposure to credit supply shocks as the weighted average of the estimated bank–time fixed effects, using as weights the share of credit a firm had with different banks before the recession. Given this shift-share research design, we follow state-of-the-art practices as recommended by [Borusyak, Hull, and Jaravel \(2021\)](#), for instance, when computing standard errors, being the first paper to bring these novel checks to the credit supply shocks literature. We find that a firm facing a negative one-standard-deviation bank credit supply shock increased its exit probability by 0.15 percentage points (pp) and decreased its formal employment growth by 1 pp. Moreover, the granularity of our data allows us to distinguish job inflows and outflows when we analyze employment changes, and we find that negative credit supply shocks impacted employment

growth mainly through an increase in job outflows rather than a decrease in inflows among surviving firms.

The richness of our data also allows us to analyze in detail the heterogeneity in the impacts of credit supply shocks. While prior work has focused separately on the effects of such shocks on sub-samples of either different types of firms (e.g., [Cingano, Manaresi, and Sette, 2016](#); [Balduzzi, Brancati, and Schiantarelli, 2018](#); [Bottero, Lenzu, and Mezzanotti, 2020](#); [Costello, 2020](#)) or workers ([Berton et al., 2018](#); [Hochfellner et al., 2015](#)), we can analyze both jointly thanks to our access to the universe of matched employer–employee records. We find larger effects among likely financially constrained firms (e.g., smaller and younger firms) and among firms with high external financial dependence—consistent with prior suggestive evidence ([Chodorow-Reich, 2013](#); [Duygan-Bump, Levkov, and Montoriol-Garriga, 2015](#); [Siemer, 2019](#))—though by fully accounting for the entire firm distribution we mitigate sample selection concerns and bring more precise estimates. Furthermore, we examine the heterogeneous effects of credit supply shocks on the composition of firms’ workforce. Our findings reveal that exposure to negative credit shocks increased outflows of workers with short tenure and those on temporary contracts, particularly in small, young firms. These findings are consistent with financially constrained firms’ laying off workers with lower dismissal costs first in the face of a negative shock ([Caggese, Cuñat, and Metzger, 2019](#)). We also find a negative, yet quantitatively small, effect on wage growth, which is more pronounced for workers with high tenure and permanent contracts.

Finally, we assess the aggregate impact of credit supply shocks on employment during 2020 (the onset of the pandemic). We use our reduced-form estimates to compute counterfactual employment levels under the hypothetical scenario where each firm borrowed from banks that contracted their credit supply the least. Based on this partial equilibrium exercise, bank credit supply shocks can account for 28% to 33% of the employment decrease among small firms in our sample between 2019 and 2020. Hence, despite being a non-financial recession, the aggregate employment impact of credit supply shocks is similar to what was observed during the Great

Recession ([Chodorow-Reich, 2013](#)) and the European Debt Crisis ([Acharya et al., 2018](#)).

Our paper mainly contributes to the literature that studies credit supply shocks and their real effects. Previous work in this area focuses primarily on recessionary episodes of financial sector origins in which banks’ balance sheets were directly compromised.³ Such balance sheet shocks unambiguously lead to a decrease in credit supply, and prior work has extensively documented the resulting, economically meaningful, real effects. In contrast, we study a period in which banks’ balance sheets were not the main source of the crisis. Instead, the economy faced a large shock that increased the demand for liquidity at a time when banks were well-positioned to lend. In this context, banks may increase lending helping firms smooth the shock, or amplify it by contracting credit. We show that most firms were exposed to negative credit supply shocks despite the non-financial origin of the crisis. We posit that the negative direction of credit supply shocks, despite banks’ ex-ante strong balance sheets, could be attributed to the large magnitude of the recession and accompanying uncertainty that, as we show, resulted in a decrease in banks’ risk tolerance. We thus provide insights into the underlying mechanisms behind credit shocks, which is particularly relevant during recessions and less obvious during non-financial ones. Moreover, our finding that credit supply shocks responded more to changes in risk aversion than to fundamentals indicates that policies that are frequently applied during financial crises could be less effective during a non-financial episode.

The few papers that have studied the effects of credit supply in the US during a non-financial recession—the dot-com bubble—do not find real effects ([Siemer \(2019\)](#), [Greenstone, Mas, and Nguyen \(2020\)](#)).⁴ Studies that analyze “normal times” in the US similarly find no real effects (e.g., [Greenstone, Mas, and Nguyen \(2020\)](#)). Arguably, the effects of the bank lending channel are amplified in contexts with both the prevalence of small firms and high reliance on banking credit,

³For example, the GFC when banks’ balance sheets were differentially affected based on their exposure to mortgage-backed securities ([Ivashina and Scharfstein \(2010\)](#), [Chodorow-Reich \(2013\)](#)), or the European debt crisis when shocks stemmed from banks’ exposure to government bonds ([Popov and Van Horen \(2014\)](#), [Acharya et al. \(2018\)](#)).

⁴Without being their main focus of study, both papers document, using different methodologies, that credit had little to no effect on employment during the 2000 recession.

as a developing country like Mexico. In line with this idea, we find substantial effects on firm survival and employment growth of a comparable magnitude to those observed during financial episodes in developed countries. Whereas prior work has studied the credit channel in Mexico and found real effects—focusing on the transmission of foreign financial shocks (Morais et al. (2019)) or using municipality-level employment during non-crisis periods (Gutierrez, Jaume, and Tobal (2023))—we are the first to use firm-level data to study the real effects of credit supply shocks during a non-financial recession in a context with a large share of SMEs and high reliance on bank lending. Interestingly, prior work finds that even in such contexts, the effect of credit supply shocks is no different in recessions relative to normal times (Degryse et al. (2019)). Yet, we find that the real effects of credit were concentrated during the first year of the pandemic (i.e., 2020), consistent with credit supply shocks being more critical in times of tighter liquidity constraints and higher uncertainty (as in Alfaro, García-Santana, and Moral-Benito (2021)). We find muted effects in 2021, the recovery period, in contrast to the positive effects after the Great Recession found in Gutierrez, Jaume, and Tobal (2023), providing novel evidence on the variability of effects in so-called “normal times,” and highlighting the importance of using firm-level data to account for potential unobserved factors when analyzing real effects.

Moreover, few papers have explored the real effects of credit during the pandemic. Among these are Greenwald, Krainer, and Paul (2020), who analyzed the effects of credit on investment during the first months of the pandemic using loan-level data for the U.S., and Granja et al. (2022), who studied the Paycheck Protection Program’s (PPP) effect on employment in the U.S. A key difference between our paper and these studies is that the relationship between bank credit and economic outcomes in the U.S. was heavily influenced by the numerous support programs that the government implemented. In contrast, we study the effects of credit supply shocks in a context where bank credit is important and there were no large-scale support programs. Moreover, we rely on the universe of credit and employment records, which allows us to offer a more complete picture of the real effects of credit supply shocks during the COVID-19 recession.

The paper is organized as follows: the next Section overviews firm, employment, and bank credit dynamics during the pandemic in Mexico. Section 3 describes the administrative data we use. In section 4, we outline our strategy to estimate credit supply shocks. We examine the distribution of the estimated shocks and discuss their drivers. Section 5 describes our empirical strategy to measure firms’ exposure to credit supply shocks and their real effects. Section 6 presents the results. Section 7 concludes.

2 The COVID-19 Recession in Mexico

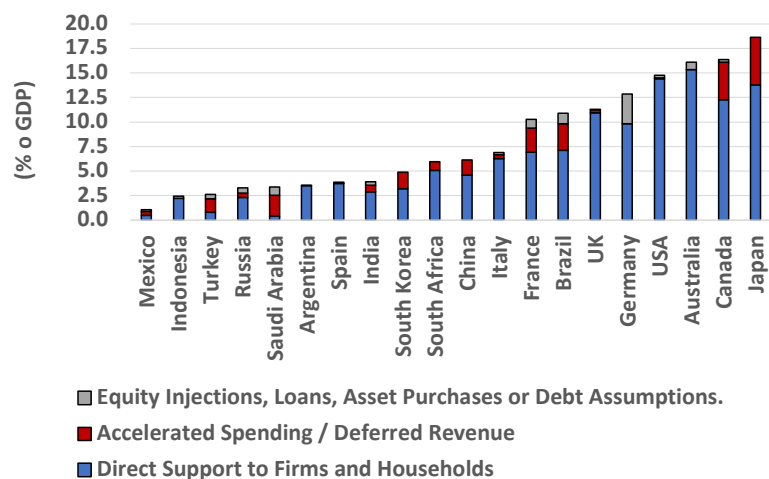
The COVID-19 pandemic severely affected the Mexican economy, with real GDP contracting by 8.5% from Q4-2019 to Q4-2020, marking the most substantial decline over three decades. This section outlines the most relevant governmental interventions during the pandemic, which were very limited in Mexico relative to the size of such programs in other countries. We also examine firm, employment, and credit dynamics throughout this challenging period, highlighting heterogeneity across firms of different age and size.

2.1 Government Economic Interventions

Compared to that of other G20 members and emerging economies, the Mexican government’s pandemic response, in terms of direct support programs for households and firms, was notably limited in both size and reach. The country’s fiscal framework focused on maintaining a balanced budget and restricting new debt issuance. Consequently, by December 2020, direct economic support measures for individuals and firms amounted to 0.63% of Mexico’s GDP (see Figure 1). In contrast, G20 advanced economies averaged 10.2%, and G20 emerging economies 3.3%, making Mexico a clear outlier (IMF (2020), Hannan, Honjo, and Raissi (2022)). Furthermore, out of these fiscal measures, business support programs represented only 0.2% of GDP, and they targeted mainly informal businesses, which have limited or no access to traditional banking

services and thus fall outside the scope of our analysis.⁵

Figure 1: Economic Measures Announced or Enacted by G20 Economies in Response to COVID-19



Notes: This figure presents the discretionary fiscal measures implemented by G20 countries as of December 31, 2020, excluding health-related expenditures, guarantees, and quasi-fiscal operations. It captures government efforts that supplemented existing automatic stabilizers. All variables are expressed as a percentage of GDP by the end of 2021. Direct support to firms and individuals includes cash transfers, unemployment benefits, subsidies, grants, and forgivable loans, among others. Source: Authors' calculations based on the *Fiscal Policies Database* published by the IMF.

In terms of monetary policy, Banxico reduced its policy rate from 7.25% at the beginning of 2020 to 4.0% in February 2021. The central bank also implemented several measures to provide liquidity and improve domestic markets' functioning. Although these measures acted as a critical backstop for banks, their utilization amounted to just 0.8% of GDP.⁶

A final intervention worth discussing is the CNBV's issuance of temporary modifications to banks' accounting criteria, allowing borrowers to defer loan payments. Borrowers could opt into

⁵This assistance mainly took the form of optional repayment loans. The government offered forgivable loans to domestic workers, the self-employed, and micro, small, and medium-sized enterprises (MSMEs) that kept employees on their payrolls. Family businesses registered in the welfare census were also eligible for these loans. Each loan could be as much as MXN 25,000 (approximately USD 1,250).

⁶Among these measures, the two with the most extensive reach were the repurchase window for government securities, established with the aim of ensuring liquidity access for financial institutions with government debt holdings, and the 15% reduction in commercial and development banks' central bank reserve requirement, which aimed to increase their available liquidity. See [Banco de México \(2020a\)](#) for a comprehensive description of the measures implemented by Banxico and [IMF \(2020\)](#) for details on their utilization as of October 2020.

this program, and if approval was granted, financial institutions were not required to designate loans as past due during the deferral period. This, in turn, implied that banks were not required to adjust their reserves, thereby mitigating the intervention’s potential liquidity impacts. As we will see in Section 4, the overall impact of the measures describe above is captured in our metric of credit supply shocks.

2.2 Public Health Measures to Curb Contagion

Mexico’s federal government implemented a multifaceted strategy to reduce contagion of the virus throughout the pandemic by issuing widely publicized sanitary guidelines, such as social distancing and stay-at-home recommendations. However, instead of imposing blanket, mandatory lockdowns, it developed a system of targeted restrictions that varied across economic activities and regions. For instance, as early as March 31, economic sectors began to be classified as “essential” (e.g., healthcare and food production) or “nonessential” (e.g., entertainment and tourism). While the essential sectors could operate during the pandemic as long as they followed social distancing practices, permission to operate for nonessential sectors varied locally based on contagion risk, which was determined on the basis of factors such as new deaths and infection rates. Hence, restrictions on establishments’ activities varied widely across the country.⁷

2.3 Firm, Employment, and Credit Dynamics

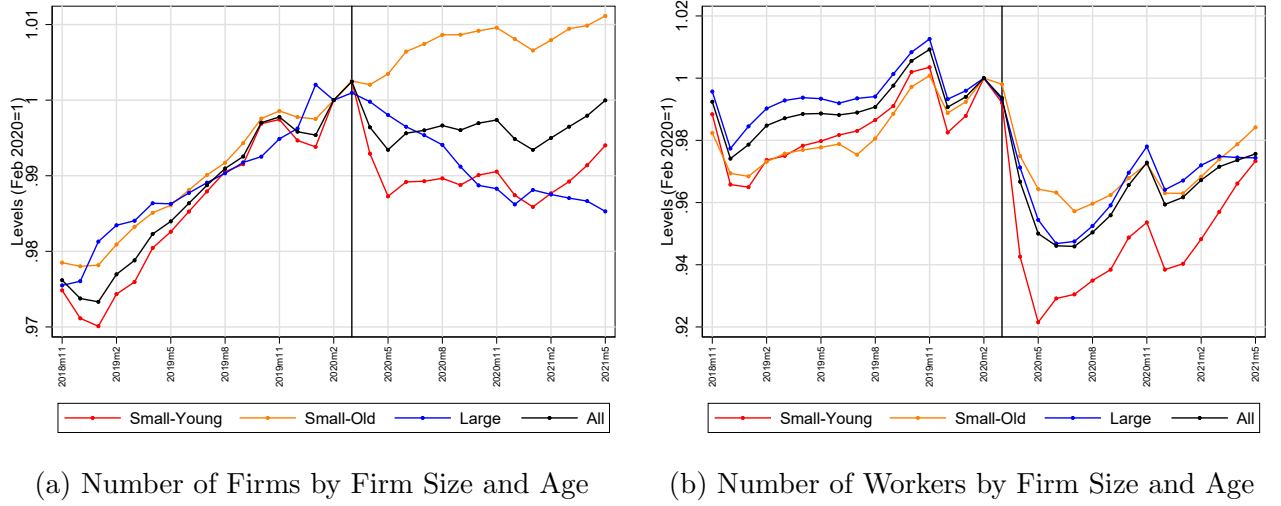
Firm and Employment Dynamics

Figure 2a shows the dynamics of formal firms in Mexico. From March to May 2020, the initial months of the pandemic, 8,300 firms (0.9%) exited the formal market. However, this drop was heterogeneous across groups. For instance, small (<100 workers) old (>10 years) firms experienced a slight drop at the beginning of the pandemic but then continued to grow, albeit

⁷For example, during the second half of October 2020, twenty states were categorized in the low-contagion-risk category, eleven in the medium-low category, and one state as medium risk.

below their pre-pandemic trend. In contrast, small young firms faced the most significant drop (1.1%), followed by a similar, albeit more gradual, decline among large firms. While, by mid-2021, the overall number of firms had returned to prepandemic levels, small young and large firms failed to fully recover.⁸

Figure 2: Firm and Employment Dynamics



Notes: Small firms are those with less than 100 workers, while young firms are those with less than 10 years of operation. Source: authors' calculations using the IMSS dataset.

Figure 2b presents the behavior of formal employment in Mexico. From February 2020 to July 2020, the total number of formal workers decreased by 5.4%, representing a loss of 1.1 million jobs. The employment trends at small old firms and large firms closely mirrored the performance of the economy as a whole, as these firms employ close to 70% of all formal workers, whereas employment at small young firms fell by 8% between February and May 2020. This result was driven both by firms in this category exiting the market, as described above, and by firms downsizing during the critical months of the pandemic. Employment began its recovery in

⁸In April 2021, Mexico's congress passed legislation regulating outsourcing, restricting subcontracting of core activities, and requiring registration of specialized service providers. This reform, promulgated in April and enacted in August, led many firms to reconfigure their business structures, inducing them to create new entities under separate tax IDs, thus contributing to the increase in the total number of firms observed after April 2021.

August 2020, particularly increasing during the first months of 2021.⁹

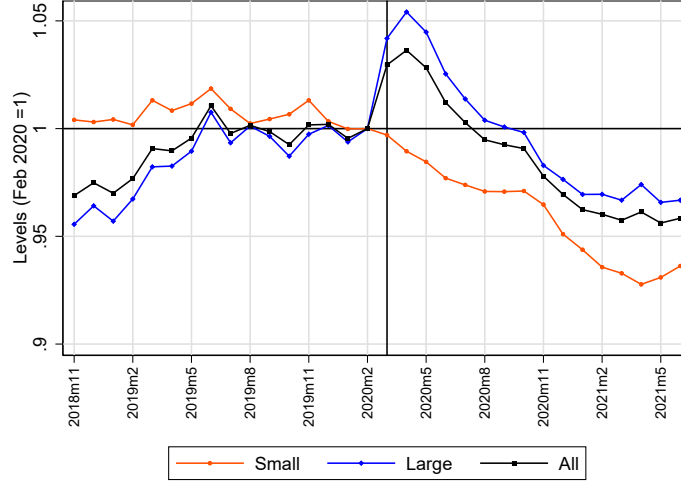
Credit Dynamics

Figure 3 illustrates the evolution of credit from commercial banks to nonfinancial private firms by firm size.¹⁰ Credit to small firms was stagnated prior to the outbreak of the pandemic and decreased during the most critical months of the crisis. An uptick in lending to these firms emerged in the second half of 2021 during the economic recovery phase. In contrast, credit to large firms initially surged between February and April of 2020, driven by increased demand for funding in anticipation of potential disruptions stemming from the pandemic. After this initial increase, a significant contraction followed until August 2021, as lending standards tightened and demand for credit dried up. As of November 2021, total credit allocations to both categories of firms remained below prepandemic levels. Interestingly, [Greenwald, Krainer, and Paul \(2020\)](#) and [Chodorow-Reich et al. \(2022\)](#) document similar findings for the US: an increase in overall credit during Q1-2020, concentrated among large firms drawing on preexisting credit lines, followed by an overall contraction in credit.

⁹The rapid recovery in employment at small firms during 2021 can be partly explained by the outsourcing reform. Workers previously registered at large outsourcing firms were reallocated to small young firms.

¹⁰We do not document credit dynamics by age, as this variable is available only for the subset of firms matched to the IMSS records.

Figure 3: Credit Dynamics



Notes: The *size* variable in the credit registry is defined as a combination of sales and the number of employees. See Appendix A.1.1 for the exact definition. Source: authors' calculations using the R04-C dataset.

3 Data

Our work mainly relies on data from two administrative sources: matched bank–firm loan-level data for the universe of commercial and corporate loans from the banking sector from the National Banking and Securities Commission (Comision Nacional Bancaria y de Valores or CNBV) and employer–employee matched records for the universe of formal employment provided by the Mexican Institute of Social Security (Instituto Mexicano de Seguridad Social or IMSS). Moreover, we use Balance Sheet and Survey data from the Senior Loan Officer Opinion Survey on Bank Lending Practices to validate and study the drivers of credit supply shocks. The following subsections explain these data sources and how we use them for our analysis.

3.1 Bank Commercial and Corporate Lending Regulatory Reports

Banking law in Mexico requires all regulated financial institutions to provide detailed information on all the corporate and commercial loans they issue each month.¹¹ CNBV compiles this information into a monthly loan-level dataset that keeps track of lending conditions and loan performance, known as the R04c report ([Comision Nacional Bancaria y de Valores, 2018-2021](#)). For each loan, the R04 includes unique (anonymized) identifiers for the issuing bank and the borrowing firm, along with a comprehensive set of loan characteristics, including issuance date, interest rate, delinquency and default status, outstanding amount, credit lines, among others. We can also observe the borrowing firm’s industry code (5-digit NAICS), size category (based on number of employees), and location.

From the R04c, we construct a firm–bank–month panel by aggregating each firm’s outstanding loans with each bank at the end of each calendar month. We exclude firms that do not operate in Mexico and government-related entities, loans issued in foreign currency, and government-sponsored banks (*Banca de Desarrollo*), which leaves us with 42 banks for the studied period. Appendix [A.1.1](#) offers a detailed description of this dataset, including the cleaning process we applied for our estimations.

3.2 Social Security Matched Employer–Employee Records

Mexico’s social security law requires that private-sector employers register their wage-earning employees with the IMSS. Registration provides employees access to several social benefits, including government-provided healthcare, disability and retirement funds, and severance payments, among others. As a result of this mandate, IMSS records offer a panel covering the universe of formal employer–employee matches ([Instituto Mexicano del Seguro Social, 2018-2021](#)). Since we can access these data from November 2004, we can follow matches over time and track their

¹¹This includes traditional banks and non-banking intermediaries. In the rest of the paper, we refer to both types of institutions as *banks* for brevity.

corresponding starting and ending dates. In addition, this dataset includes information on workers’ gender, date of birth, workplace location and industry, and type of contract (temporary or permanent).¹²

Using IMSS records, we construct a firm-level monthly panel dataset by aggregating matches to the tax ID level. Thus, we can track the stock of formally employed workers, their wages, and the number of new hires and separations. Furthermore, the granular nature of our data enables us to differentiate these inflows and outflows by various worker groups. For instance, we can compute job creation and destruction by gender or type of contract in each firm. We also build from this data an indicator variable for whether the firm exits the formal sector, where we define the latter as the firm’s having no formal employees. Additionally, we create a measure of firm age based on the date the firm registers its first formal employee. We provide further details of the variables in this dataset in Appendix A.1.2.¹³

3.3 Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOS)

In an effort to better understand the evolution of credit and banks’ lending standards, in 2015 Mexico’s Central Bank (Banxico) began the Senior Loan Officer Opinion Survey.¹⁴ This quarterly, panel survey asks senior loan officers from all commercial banks operating in Mexico a set of questions regarding how their bank’s credit standards, lending behavior, and overall credit conditions have changed over the previous quarter. The survey also includes questions on the officer’s perceptions on the economy, the credit market, and demand from core loan categories.

¹²Following International Labour Organization guidelines, we refer to employer–employee matches registered with the IMSS as *formal* employment relationships.

¹³IMSS records identify distinct employers using an ID known as the *registro patronal*. A tax ID can have more than one registro patronal, and a registro patronal can be associated with more than one tax ID. Since it is tax IDs that are used to identify lenders in the R04c loans data, we use the tax IDs to identify firms.

¹⁴In Mexico, the survey is named Encuesta sobre Condiciones Generales y Estándares en el Mercado de Crédito Bancario (EnBan) which directly translates to Survey on General Conditions and Standards in the Banking Credit Market. The survey is analogous to the US Federal Reserve Board’s Senior Loan Office Opinion Survey on Bank Lending Practices (SLOOS).

The SLOOS asks loan officers about several demand and supply “factors” that could affect credit for the bank. Example of supply factors included in the survey are the bank’s capitalization, liquidity, funding costs, exposure to regulatory standards, risk tolerance, credit portfolio quality, etc.¹⁵ Loan officers report whether the bank has “tightened considerably” (-1), “tightened somewhat” (-0.5), “unchanged” (0), “eased somewhat” (0.5), or “eased considerably” (1) for each credit factor.¹⁶ Respondents are also asked about their expectations for movements in these factors over the following quarter. For a subset of factors (e.g., portfolio quality and risk tolerance), questions in the SLOOS distinguish between small/medium sized and large borrowers.

Banxico provided access to the confidential, bank-level responses. As we detail in section 4, we use these responses to validate our measures of credit supply shocks, and study their drivers.

3.4 Banks’ Balance Sheets

All banks operating in Mexico must provide monthly financial reports to CNBV, as well as audited balance sheet and end-of-year statements. CNBV monitors capital and liquidity requirements across the banking sector, and ensures default rates and reserves align with regulation. In the next section, we rely on these monthly reports to validate that our estimates of credit supply shocks correlate with bank performance.

3.5 Sample Selection

We define a *firm* by its tax ID, so we merge datasets using this identifier. While borrowers’ IDs in the R04c reports are defined on this basis, in the IMSS records, tax IDs are available only from November 2018. Moreover, because of the requirements of our identification strategy (see below), we focus our analysis on the set of firms with at least one active loan from a bank in November 2018. Table 1 presents the summary statistics for each variable and period of interest.

¹⁵Demand factors include investment in physical capital and work capital requirements.

¹⁶The questions in the survey are phrased as: ‘Over the past three months, how have your bank’s [factor], changed?’.

After a simple cleaning process (see A.1.2), in November 2018, 181,003 firms had a positive credit amount from one of 42 banks operating, which represents around 16% of the universe of formal firms in the Social Security dataset, but close to 30% of IMSS total employment.¹⁷

Table 1: Summary Statistics (2019-2021)

		Mean	Std Deviation	p10	p50	p90
Δ Employment		-0.06	0.79	-0.86	0.00	0.67
Δ Credit		-0.47	1.00	-2.00	-0.32	0.70
Exposure to Credit Supply Shocks		-0.05	0.10	-0.15	-0.03	0.03
Size	Small Young	0.51				
	Small Old	0.44				
	Large	0.04				
Firm type	Incorporated	0.41				
Sector	Construction	0.07				
	Manufacture	0.14				
	Retail & Wholesale Trade	0.40				
	Services	0.39				
Region	North	0.26				
	Center-North	0.26				
	Center	0.36				
	South	0.13				
No. of Firms		181,003				
No. of Banks		42				
No. of Firm-Date		494,932				

Notes: Changes in credit and employment are computed using equation (4). Firm's exposure to credit supply shocks is defined in section 5. Firms in our sample are those that had positive outstanding credit in November 2018. Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those with the less (more) than 10 years. See Appendix A.1.2 for additional details. Source: authors' calculations using IMSS and R04-C datasets.

¹⁷According to the ENAFIN, around 13% of firms have a credit with a bank at a given month, so our estimates are slightly larger.

4 Credit Supply Shocks: Identification, Validation, and Drivers

4.1 Identifying Credit Supply Shocks

We follow a strategy closely related to [Amiti and Weinstein \(2018\)](#) to decompose changes in credit into their demand and supply components. This approach relies on observing firms with multiple lenders (multi-bank firms) to distinguish variation in credit arising from the firm (demand) from those stemming from the banks (supply). Given that multi-bank firms account for only 30% of our sample, we implement a variation of their approach, as proposed by [Degryse et al. \(2019\)](#), and isolate demand factors by grouping firms by industry–location–size–time (ILST). This methodology does not require focusing on banks with multiple institutions, but instead assumes that demand-side idiosyncratic fluctuations in credit are common across firms in the same industry–location–size bins.¹⁸

We estimate equation (1) regressing firm j 's annual outstanding credit growth rate with bank b ($\Delta Credit_{j,b,t}$) on a set of industry–location–size–time fixed effects ($\gamma_{ils(j),t}$) and a set of bank–time fixed effects ($\delta_{b,t}$). The former control for demand, while the latter capture changes in credit associated with each bank's specific conditions, such as its financial health, access to external funding, or risk tolerance, to name a few potential drivers of supply shocks. These bank–time fixed effects are the parameters of interest, as they capture idiosyncratic changes in credit driven by supply-side factors. Nonetheless, they have a straightforward interpretation as the total percent change in credit attributable to supply from each bank, net of demand factors.

¹⁸For Belgium, [Degryse et al. \(2019\)](#) shows that the credit supply shocks estimated using the [Amiti and Weinstein \(2018\)](#) multi-bank and the ILST approaches are highly correlated when both are estimated in the multi-bank sample. This is consistent with the ILST estimator being equally suitable for controlling for demand-side factors as the firm-time fixed effects methodology, with the advantage of allowing a larger set of firms to be included in the estimation sample.

$$\Delta Credit_{j,b,t} = \gamma_{ils(j),t} + \delta_{b,t} + \epsilon_{j,b,t} \quad (1)$$

Following [Amiti and Weinstein \(2018\)](#), we weight each bank-firm observation by its economy-wide credit share and estimate (1) using weighted least squares (WLS) ([Tielens and Hove, 2017](#)). We compute growth rates using the midpoint definition (see equation (4)), common in the firm dynamics literature ([Davis, Haltiwanger, and Schuh, 1998](#)).¹⁹ This measure is symmetric to expansions and contractions and, critically, accommodates entry and exit (i.e., accounts for new bank-firm matches and their termination). Using WLS and accounting for firm entry and exit allow us to match observed aggregate growth and decompose it into demand- and supply-side factors (see Appendix A.3).²⁰

The identifying assumption behind equation (1) is that credit demand shocks, while time-varying, are homogeneous across firms in the same industry–location–size group. If this assumption holds, the firm–group–time fixed effects properly control for demand-side shocks, allowing the bank–time fixed effects ($\delta_{b,t}$) to be consistently estimated. Thus, to implement the ILST approach, we consider a granular set of demand controls, grouping firms by 3-digit industry (92 categories), location (32 states), and size (firms with more than 100 employees and all others).²¹ To the extent that idiosyncratic fluctuations in credit demand are driven by, for example, productivity or product demand shocks common to firms within an industry–location–size groups then our specification would appropriately control for these fluctuations. In Appendix Figure A.4, we show that the set of bank–time fixed effects estimated using the firm-group-time specification from Equation (1) strongly correlate with those obtained using firm-time fixed effects (as in [Amiti and Weinstein \(2018\)](#)) instead, consistent with both methods being similarly effective

¹⁹Using this growth rate definition, the weights correspond to $Credit_{j,b,t} + Credit_{j,b,t-1}$. This approach, as in [Barbieri et al. \(2022\)](#), also helps moderate the influence of outliers.

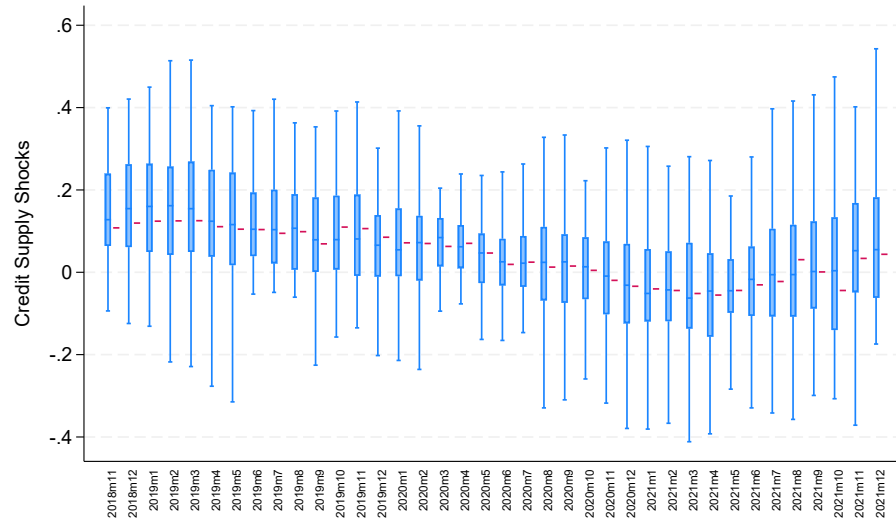
²⁰Changes to the R04c credit report that occurred in 2022 prevents us from extending the panel beyond 2021.

²¹For example, all firms with more than 100 employees, in the food manufacturing (NAICS code 311), located in the state of Nuevo León are grouped into one ILST category and we are therefore assuming that they face similar credit demand shocks in a given period.

in capturing demand-side factors.²²

Our method of estimating bank fixed effects differs from the geographical approach pioneered by [Greenstone, Mas, and Nguyen \(2020\)](#) and implemented by [Gutierrez, Jaume, and Tobal \(2023\)](#) in the context of Mexico. The latter aggregate credit to the location–bank level and incorporate location and bank fixed effects to separate supply from demand factors. Thus, their implicit assumption is that demand at the locality level is homogeneous across firms, regardless of industry and size. Since industries differ substantially in their financial needs ([Rajan and Zingales, 1998](#)) and credit demand depends on size ([Chodorow-Reich et al., 2022](#)), accounting for these factors is critical. Moreover, the heterogeneity in the impact of the pandemic across industries and size groups, as documented above, justifies our approach.²³

Figure 4: Distribution of Bank Level Credit Supply Shocks



Notes: Credit supply shocks at the bank level are derived from the bank fixed effects estimated in Equation (1). The red dashed line is the average of the shocks. Outliers are omitted from the box plots. Source: authors' calculations using the R04-C dataset.

²²Having more disaggregated ILST controls is not necessarily better. If the groups are defined too narrowly, sample size in each bin decreases and so does statistical power.

²³Indeed, recent papers depart from the practice of accounting solely for regional variation and, instead, take into account demand differences arising from firm size and industry composition across localities as well ([Berton et al., 2018](#)).

Figure 4 presents the distribution of these credit supply shocks in different time periods, estimated using Equation (1) with monthly year-to-year changes between November 2018 and December 2021. The shocks followed a downward trend since March 2020, becoming, on average, negative in November 2020 and beginning to recover during the second half of 2021. This fact indicates that during the recession, banks’ supply conditions deteriorated, a topic we explore further in the next section.

4.2 Validating the Estimated Fixed Effects as Supply Shocks

We next provide evidence that the estimated shocks capture supply-side factors. We do so by confirming that the estimated shocks are meaningfully correlated with several bank-specific variables that feasibly co-move with credit supply.

We use banks’ balance sheet information from the National Banking and Securities Commission to examine whether our estimated credit supply shocks correlate with changes in banks’ funding sources or performance. For funding availability, we consider three variables: deposits, equity, and interbank liabilities. We calculate the year-over-year changes in these funding variables and express them as percentage changes relative to each base year’s assets. We measure banks’ performance return on assets (ROA) and return on equity (ROE) over the prior 12 months.^{24,25}

Table 2 presents the results of a set of regressions in which the dependent variable is our estimated credit supply shocks and the explanatory variables are the aforementioned bank-specific funding and performance variables, with bank and time fixed effects, namely,

$$\hat{\delta}_{b,t} = X_{b,t}^k + \eta_b + \xi_t + \epsilon_{b,t} \quad (2)$$

²⁴We use November as a reference for all annual changes to be consistent with the estimation of equation (1).

²⁵Degryse et al. (2019) uses equivalent liquidity measures to validate their estimated shocks. Alfaro, García-Santana, and Moral-Benito (2021) split the same of banks into “healthy” and “unhealthy” banks and show the estimated shocks are good predictors of bank health. Amiti and Weinstein (2018) use capital ratios and market-to-book values to confirm that their estimated shocks positively correlate with banks’ performance.

Table 2: Bank Performance and Credit Supply Shocks

	Credit Supply Shocks			
	(1)	(2)	(3)	(4)
ROE	0.3499 (0.3064)			
ROA		1.8573 (2.4543)		
Deposit Growth			0.5372*** (0.1747)	
Equity Growth			-1.3461* (0.7263)	-0.1424 (0.2632)
Interbank Liabilities Growth			0.8656 (0.7331)	0.3937** (0.1842)
Observations	167	167	111	167
Number of Banks	42	42	28	42
Adjusted R2	0.3307	0.3247	0.4028	0.3478
Time FEs	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes

Notes: Credit supply shocks at the bank level are the bank fixed effects estimated from Equation (1). Columns (1), (2), and (4) include all credit issuing institutions. Column (3) includes only those that receive deposits from the public. Standard errors are clustered at the bank level. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using the R04-C dataset and CNBV public reports.

where $\hat{\delta}_{b,t}$ are the bank-time fixed effects estimated from equation (1), $X_{b,t}^k$ are different metrics for bank b in period t , and η_b and ξ_t are bank and time fixed effects, respectively. Columns (1) and (2) show that the credit supply shocks positively correlate with ROE and ROA, although the relationships are not statistically significant. Columns (3) and (4) show the results for funding availability metrics for all financial intermediaries and deposit-taking institutions, respectively. The estimated credit supply shocks correlate positively with interbank liabilities growth when including non-deposit taking institutions, and negatively with equity growth for both groups. There is also a positive correlation between credit supply shocks and deposit growth for deposit-taking institutions. The positive correlation with deposit and interbank liability growth suggests that part of the heterogeneity in banks' credit supply shocks reflected differences in funding availability. Conversely, the negative correlation with equity growth is consistent with banks that faced negative credit supply shocks responding through equity injections.²⁶

²⁶Using monthly data for Belgium from 2009 to 2012, Degryse et al. (2019) observe similar correlations between

4.3 Drivers of Credit Supply Shocks

While on average banks’ credit supply conditions deteriorated during the pandemic recession, the estimated shocks exhibit considerable heterogeneity between banks (see Figure 4). Our data-driven approach is deliberately agnostic regarding the underlying drivers of this heterogeneity. As noted by [Amiti and Weinstein \(2018\)](#), a different approach that relies on a specific proxy for the source of the supply shock likely “captures only a small part of the total variation in bank supply shocks.” We posit that in a context such as the COVID-19 pandemic period, characterized by various concurrent shocks, an approach that does not require specifying a particular source for the shocks can be useful to capture the overall changes in banks’ conditions. The downside, of course, is that we cannot pinpoint the precise mechanisms that drive the credit supply shocks. To address this gap, we leverage confidential data from Mexico’s Senior Loan Officer Opinion Survey (SLOOS) on Bank Lending Practices to examine potential drivers behind banks’ credit supply shocks.²⁷

We compute aggregate indices for different factors listed in the survey, as the net percentages of banks that responded favorably to the corresponding question.²⁸ This aggregation is simply the difference between the share of banks reporting that a given factor contributed to an easing (considerably or somewhat) and the share of banks reporting that the factor contributed to a tightening (considerably or somewhat). Thus, negative (positive) values indicate a supply restriction (easing).

Figure 5a displays the quarterly changes in the indices for overall demand and supply conditions for commercial loans for small and large firms.²⁹ Reassuringly, and consistent with the answers in the SLOOS containing relevant information about changes in credit supply, the credit supply shocks, equity growth, and interbank lending.

²⁷For the US, [Cavallo et al. \(2024\)](#) similarly argue that the SLOOS contains information that allows independently identifying shifts in the supply conditions of bank credit.

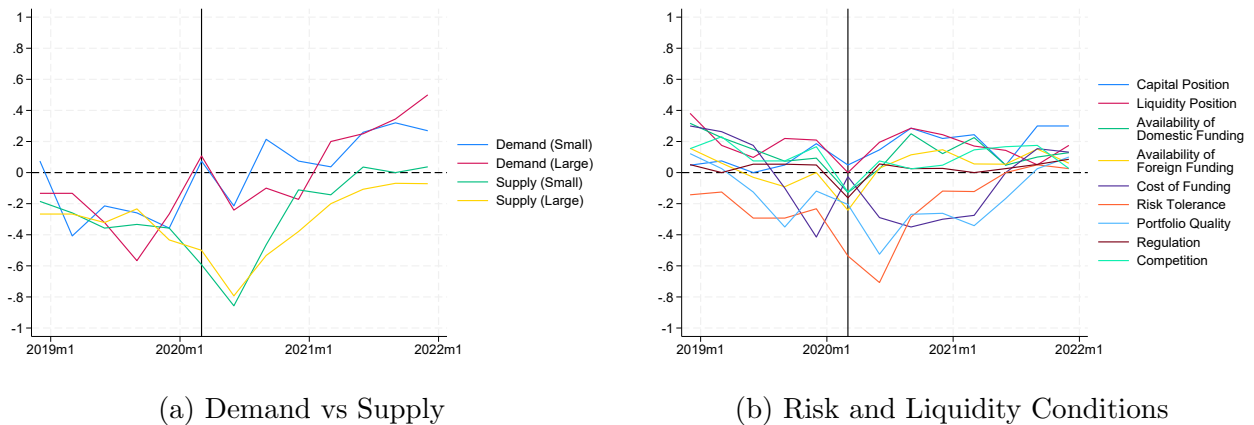
²⁸Constructing such indices is standard in the literature. See, for example, [Bassett et al. \(2014\)](#) and [Cavallo et al. \(2024\)](#).

²⁹Mexico’s SLOOS survey defines “large” borrowers as those firms with 100 employees or more.

index tracks the drop and recovery of aggregate credit in the economy.³⁰ This figure indicates a tightening in supply conditions for both small and large borrowers in the early stages of the pandemic. There was a concurrent increase in the demand for commercial loans, suggesting that the observed early expansion in credit (Figure 3) responded more to demand conditions than to supply ones. In fact, our decomposition of aggregate credit (Appendix A.3) shows the same pattern, where *aggregate* credit growth at the beginning of the pandemic stemmed from idiosyncratic demand shocks.

Figure 5b shows indices for the nine supply-related factors listed in the survey. The drops in risk tolerance and loan portfolio quality are the most pronounced, and are concurrent with the fall and eventual recovery in the aggregate supply index, suggesting these two factors as important drivers of idiosyncratic variation in credit supply.

Figure 5: Evolution of Credit Demand, Lending Standards, and Supply-Side Determinants According to the Bank Lending Survey



Notes: In the Mexican Bank Lending Survey, bank executives report how different conditions have evolved over the past four months. A positive value reflects an improvement in a given condition, while a negative value indicates a deterioration. Panel (a) shows the net percentage of banks reporting improved demand and supply conditions (i.e., the fraction reporting an improvement minus the fraction reporting a deterioration) by firm's size. The *size* variable in the SLOOS survey is defined in the same way as in the credit registry—as a combination of sales and number of employees. See Appendix A.1.1 for the exact definition. Panel (b) focuses on the underlying factors affecting credit supply during the pandemic. The vertical solid line marks March 2020. Source: authors' calculations using the Mexican SLOOS.

³⁰Figure A.2 in the Appendix further shows that the time-series behavior of the SLOOS's credit supply factors index replicates that of a weighted average of the estimated credit supply shocks.

Having verified that supply factors in the SLOOS are feasible measures of supply-based movements in aggregate credit, we seek to decompose the estimated credit supply shocks into the contributions made by the various explanatory factors as reported in the survey. For this, we use a regression analysis as shown in Equation 2.³¹ As supply factors we include changes in risk tolerance, loan portfolio quality, capital and liquidity position, cost and availability of domestic funding.³² While the results are not meant to be interpreted as casual, they do shed light on the differences across banks that could drive the heterogeneity in credit supply shocks.

Table 3 presents the results. We begin by focusing on fund-related metrics. During financial crises, liquidity and cost of funding concerns are commonly cited factors underlying variation in credit supply. Column (1) instead shows that none of the funding, liquidity, or capital position factors significantly correlate with our estimated shocks. This finding underlines the need to better understand the differences in drivers of credit supply shocks during non-financial recessions. Unlike crises characterized by substantial bank liquidity concerns, Mexican banks during the COVID-19 pandemic had a strong liquidity position before the recession. On December 2019, all banks had a liquidity coverage ratio well above the regulatory standard (Banco de México, 2020b) and their coverage actually increased for most banks during 2020 (Appendix A.6). Indeed, the survey shows that liquidity or capital considerations did not change dramatically during this period and remained almost at their pre-pandemic levels, except for a small drop at the beginning of the pandemic.

In Column (2) we include as regressors two additional supply-side listed factors in SLOOS.³³ Interestingly, we see that neither *overall* loan portfolio quality nor risk tolerance correlate with our estimated shocks. However, in column (3) we consider a specification that allows for separately

³¹To be consistent with our estimated shocks, we add each bank's responses in the SLOOS survey over the prior four quarters to calculate yearly changes.

³²This analysis excludes non-deposit taking institutions since the SLOOS survey focuses on loan officers at deposit taking institutions only.

³³We omit regulation and competition factors from the regression as they did experience limited variation during the sample period. We also exclude foreign funding since we exclude foreign-currency loans from our sample.

modeling the effect of portfolio quality and risk tolerance by firm size. We find that risk tolerance towards large firms is the only statistically significant driver behind credit supply shocks.

Table 3: Credit Supply Shocks and Bank-Reported Supply-Side Factors

	Credit Supply Shocks		
	(1)	(2)	(3)
Cost of Funding	0.0023 (0.0139)	0.0027 (0.0142)	0.0057 (0.0150)
Capital Position	-0.0039 (0.0194)	-0.0037 (0.0194)	-0.0010 (0.0239)
Domestic Funding	0.0122 (0.0195)	0.0120 (0.0196)	0.0110 (0.0206)
Liquidity Position	-0.0301 (0.0234)	-0.0308 (0.0242)	-0.0311 (0.0252)
Portfolio Quality		-0.0052 (0.0213)	
Portfolio Quality (Small)			-0.0148 (0.0225)
Portfolio Quality (Large)			0.0048 (0.0289)
Risk Tolerance		-0.0038 (0.0322)	
Risk Tolerance (Small)			-0.0220 (0.0292)
Risk Tolerance (Large)			0.0849* (0.0492)
Observations	90	90	90
Number of Banks	23	23	23
Adjusted R2	0.3383	0.3268	0.3845
Time FEs	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes

Notes: Credit supply shocks at the bank level correspond to the bank fixed effects estimated from Equation (1). Each explanatory variable represents the sum of banks’ responses over the past four quarters to the question: “Over the past three months, how has condition X changed?”, where X refers to each of the variables listed in the table. Responses take five options, each mapped to a numerical value: “tightened considerably” (-1), “tightened somewhat” (-0.5), “unchanged” (0), “eased somewhat” (0.5), and “eased considerably” (1). Standard errors are clustered at the bank level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Source: authors’ calculations using the R04-C dataset and public reports from the CNBV.

Prior literature has documented the increase in credit line use by large firms (e.g., [Chodorow-Reich et al. \(2022\)](#), [Greenwald, Krainer, and Paul \(2020\)](#)) during the COVID-19 pandemic. Such an increase could cause banks to adjust their risk tolerance towards this market segment and, in turn, modify their credit supply across all firms. In this sense, our findings are consistent

with [Acharya et al. \(2024\)](#) who posit that exposure to undrawn credit lines drove banks’ stock performance, and with [Kapan and Minoiu \(2021\)](#) who argue that banks exposed to higher risks of credit line drawdowns (which are concentrated among large firms) tightened lending conditions.

Overall, the evidence suggests that changes in banks’ credit responded primarily to variation in risk tolerance. It is important to highlight that during this conjuncture banks were well capitalized and did not face liquidity constraints (as shown in Figure [A.6](#)). Moreover, the observed correlation between changes in risk tolerance and credit supply shocks controls for such changes in liquidity and funding costs, as well as bank and time fixed effects. In other words, (i) changes in risk aversion correlate with credit supply growth, and (ii) it is not differences in funding, liquidity, portfolio quality, or time-invariant bank behavior that drive banks’ changed risk tolerance during the sample period.

This further suggests that liquidity provision policies may not be as effective when credit supply shocks are not primarily driven by fundamentals, and are instead related to changes in risk aversion. Consistent with this being the case in Mexico, banks did not use the large credit lines offered by the Central Bank during this period ([Banco de México \(2020a\)](#)). Instead, policies that foster guarantees to back loans, such as the PPP program in the US ([Granja et al. \(2022\)](#)), are likely more effective in non-financial recessions.

5 Real Effects of Credit Supply Shocks

5.1 Firm Exposure

Having shown that credit supply conditions deteriorated during this recession, likely due to changes in banks’ risk tolerance, we now want to understand its real effects. Thus, we follow a large strand of the literature and study the impact of credit supply shocks on firm outcomes. Defining the unit of observation as a firm, as opposed to a locality, allows us not only to study the heterogenous impact of these shocks across firms with different characteristics but also to avoid

capturing unobservable time-varying local conditions (e.g., industry shocks) or compositional effects within those localities. To estimate the impact of bank credit supply shocks on firm-level outcomes, we construct a metric of firm exposure to these shocks as

$$Z_{j,t} = \sum_b w_{j,b,2018} \times \widehat{\delta}_{b,t} \quad t > 2018 \quad (3)$$

where $w_{j,b,2018}$ is firm j 's credit stock on November 2018 issued by bank b as a share of firm j 's total credit in that period and $\widehat{\delta}_{b,t}$ is the corresponding estimated bank-time fixed effect from (1). The intuition behind this measure is that firms' preexisting relationships with a subset of banks and the relative importance of each of those banks for the firm's financing determines the impact of any *future* bank supply shock on the firm's outcomes. We opt to fix the shares in a given period instead of changing them period-by-period to further strengthen the case that the exclusion restriction holds. In this regard, note that by fixing the shares to their 2018 levels, the cross-time variation in each firm's exposure arises solely because of credit supply shocks.³⁴

We posit that using fixed-composition firm loan portfolios is also consistent with the high costs of switching lenders documented by prior literature on relationship lending and bank competition (e.g., Petersen and Rajan, 1994; Elyasiani and Goldberg, 2004; Barone, Felici, and Pagnini, 2011). The persistence in the set of banks from which Mexican firms borrow suggests that significant costs are associated with a firm's switching to a different lender. In November 2020, 81% of firms with positive credit in both November 2018 and November 2020 had the same main lender as in November 2018. Moreover, even in the absence of switching costs, the allocation of loans across banks in 2018 arguably is not correlated with banks' credit supply shocks in future periods since firms could not anticipate the distribution of these shocks during the pandemic.

Our estimation framework falls within the shift-share design with estimated shocks setting described in Borusyak, Hull, and Jaravel (2021) and thus does not require that the credit shares

³⁴By construction, firms with no outstanding loans from the banking sector in November 2018 had zero exposure to credit supply shocks throughout the studied period. We exclude these firms from the sample.

be exogenous to yield consistent estimates—only that the assignment of shocks be conditionally quasi-random. We argue that this assumption is satisfied given the discussion in Section 4.1 about both the unexpected nature of the shocks and their identification. Nonetheless, we further confirm that firms’ exposure to credit supply shocks is uncorrelated with relevant firm characteristics. To do so, we regress these characteristics at their 2018 values on our measure of exposure to bank credit supply shocks in November 2019 ($Z_{j,2019}$). Moreover, we take advantage of the equivalence result in the aforementioned work and compute exposure-robust standard errors from a bank-level weighted regression.³⁵

Table 4 shows the results of this balance test. Our findings indicate no systematic differences of either economic or statistical significance in the composition of firms across the distribution of exposure to bank credit supply shocks. While this check further supports the exogeneity of our exposure metrics regarding observables, firms could have sorted on other dimensions (e.g., productivity, management, expectations). Thus, when estimating the real effects of firms’ exposure to credit supply shocks, we include firm fixed effects in all specifications to account for unobserved, time-invariant firm characteristics.

We also study the effects of our measure of exposure to credit supply shocks on various loan market outcomes at the firm level. In particular, we consider changes in outstanding credit, credit lines, and number of loans, as well as the probability of a firm’s having a new credit line and the firm’s average interest rate. We regress each of these variables on the firm’s exposure to credit supply shocks and a set of firm and industry–location–size–time fixed effects analogous to those used in equation (1), while we compute growth rates as defined in equation (4). Table 5

³⁵Although this bank-level weighted regression delivers a coefficient identical to that from the full-sample regression, it facilitates the computation of standard errors that account for the likely autocorrelation of units treated with the same shock. Thus, we cluster standard errors by bank in the bank-level weighted regression to account for any potential serial correlation. In addition, [Borusyak, Hull, and Jaravel \(2021\)](#) show that both a large effective sample size and mutually uncorrelated shocks are needed to guarantee consistency. We argue that these conditions are satisfied since our estimation strategy isolates specific bank variations net of demand factors. They also recommend computing the shift-share instrument for each firm by employing the leave-one-out procedure. Given the large number of firms per bank, we do not do this. For instance, in November 2018, each bank had, on average, around 10,000 firms as clients, ranging from more than 100 for the smallest bank up to more than 100,000 for the largest one.

presents the results of this exercise.

An exposure to a positive credit supply shock increases the growth of outstanding credit, credit lines, and the number of loans, as well as the probability of acquiring a new credit line. However, it has a (statistically) null effect on a firm’s average interest rate. The direction of these results is consistent with what the theory would predict. Moreover, these findings align with the interpretation that firms face frictions in obtaining alternative funding sources by switching banks and provide evidence that exposure to credit supply shocks affects firms’ credit availability through various channels. For this reason, we do not use our exposure metric as an instrument for credit growth, as the exclusion restriction for this instrument would likely be violated (Güler et al., 2021). Instead, we directly study the effects of our exposure metric on real outcomes.

Table 4: Balance Test

		Coefficient	Std Errors
Ln(Employment)		-1.07	1.05
Ln(Credit)		-1.26	1.71
Size	Small Young	0.16	0.23
	Small Old	-0.15	0.23
	Large	-0.01	0.09
Firm type	Incorporated	0.41	0.50
Sector	Construction	0.04	0.12
	Manufacture	0.01	0.19
	Retail & Wholesale Trade	-1.45**	0.67
	Services	1.40*	0.75
Region	North	0.41	0.63
	Center-North	-0.14	0.16
	Center	-0.09	0.53
	South	-0.18	0.15

Notes: This table presents the results of regressing each variable in column 1 on our measure of exposure to credit supply shocks, $Z_{j,t}$, defined in equation (3). *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

Table 5: Impact of Credit Supply Shocks on Loan Market Outcomes

	Δ Credit	Δ Credit Line	Δ Loans	New loans	$\ln(i)$
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock	0.5242* (0.2683)	0.3876* (0.2238)	0.4380* (0.2624)	0.2223* (0.1167)	0.0068 (0.0223)
Mean of $Y_{j,t}$	-0.3904	-0.2383	-0.2372	0.2736	0.1773
Observations	404799	404799	404799	404799	337718
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Firm level exposure to credit Supply Shocks is defined in equation (3). Changes in credit, credit lines and loans are computed as in equation (4). “New Loans” takes the value of 1 if the firm was granted any new loan between t and $t - 1$ and 0 otherwise. “ $\ln(i)$ ” is the average interest rate on all the firm’s active loans. Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using the R04-C dataset.

5.2 Estimation of Real Effects

In this section, we explain how we estimate the effect of firms’ exposure to bank credit supply shocks on employment growth and firm exit. We capture the latter with an indicator variable that takes the value of one if a firm has zero employment in period t but had positive employment in $t - 1$. Regarding growth rates, we follow a long tradition in the firm dynamics literature (Davis, Haltiwanger, and Schuh, 1998) and compute them as in equation (4).

$$\Delta Y_{j,t} = \frac{(Y_{j,t} - Y_{j,t-1})}{0.5(Y_{j,t} + Y_{j,t-1})} = \frac{(Inflows_{j,t,t-1} - Outflows_{j,t,t-1})}{0.5(Y_{j,t} + Y_{j,t-1})} \quad (4)$$

This definition has the advantage of accommodating entry and exit, reducing the influence of outliers, and being symmetric for expansions and contractions. Moreover, given the granularity of our data, this definition allows us to decompose firms’ employment growth into inflows (new hires) and outflows (separations) and compute their corresponding contributions parsimoniously.

By doing so, we can further understand whether credit supply shocks operate either by expanding employment or by mitigating its reduction.³⁶ Since we consider yearly changes from 2018 to 2021, with November as a base, our panel encompasses three periods.³⁷

The fact that our unit of observation is the firm, along with the panel structure of our data, allows us to control for unobserved firm characteristics that could drive outcome changes. This is one of the advantages of working with firm-level data instead of information aggregated to a particular geographical level. Thus, to study the real effects of credit supply shocks on firms, we regress our outcomes of interest, $Y_{j,t}$, on our exposure metric, $Z_{j,t}$, including firm fixed effects, ω_j , and a set of industry–location–size–age–time fixed effects, $\psi_{ilsa(j),t}$, as shown in equation (5). The former captures firm-invariant characteristics, while the latter captures common time-varying shocks across firms in the same industry, location, and size–age group, consistent with how we estimate the credit supply shocks, namely, equation (1). Furthermore, performing the analysis at the firm level also allows us to explore how different firm characteristics such as size and age interact with exposure to credit supply shocks, as shown in Section 6.1.³⁸

$$Y_{j,t} = \beta_0 + \beta_1 Z_{j,t} + \omega_j + \psi_{ilsa(j),t} + \epsilon_{j,t} \quad (5)$$

The coefficient of interest is β_1 , which we argue captures the causal effect of credit supply shocks in light of our discussion in the previous sections. Since both our bank credit supply shocks and outcomes of interest are in terms of rates (employment growth, exit), β_1 is actually an elasticity, as it represents the percentage-point change in Y in response to a 1 pp change in expected credit solely due to supply factors given preexisting banking relationships. For convenience,

³⁶We cannot distinguish between layoffs and voluntary separations; thus, when examining job outflows, we focus on all terminated matches.

³⁷We consider yearly changes because, as shown in Figures 2b and 3, both formal employment and credit have a strong seasonal component. We use November as the base month when computing annual changes to avoid capturing idiosyncratic fluctuations from end-of-year layoffs and rehires, as firms tend to lay off workers in December and rehire them in January. Our results are robust changes in the base month (see A.8.4)

³⁸We also distinguish across age groups, as previous works show the importance of firm age for job dynamics (Haltiwanger, Jarmin, and Miranda, 2013) and for the dynamics of financial constraints during recessions (Siemer, 2019).

however, we analyze its magnitude in terms of standard deviations. Concerning inference, while many papers cluster standard errors at the (main) bank level (Chodorow-Reich, 2013; Berton et al., 2018; Degryse et al., 2019; Chodorow-Reich et al., 2022) given that the treatment effect occurs at this level (Abadie et al., 2022), we take advantage of the latest advances in the shift-share literature and, as mentioned above, compute exposure-robust standard errors from a bank-level weighted regression with clustered standard errors (Borusyak, Hull, and Jaravel, 2021).³⁹

6 Results

Table 6 presents our main results. During the COVID-19 recession, credit supply shocks affected firms' annual formal employment growth (column (1)) and their exit probability (column (5)). A firm facing a negative credit shock of one-standard-deviation (10 pp) decreased its formal employment growth by 1 pp and increased its exit probability by 0.15 pp. In this regard, these effects, in addition to being statistically significant, are of economic importance: a move from a firm at the 90th (0.03) to one at the 10th (-0.15) percentile of the credit supply shock distribution corresponds to an additional decline in employment growth of 1.8 pp and an increase in exit probability of 0.27 pp.

³⁹The standard errors from clustering at the main bank level are actually smaller, so our approach is more conservative.

Table 6: Real Effects of Credit Supply Shocks (2019–2021)

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock	0.1023*** (0.0341)	0.0281 (0.0190)	0.0074 (0.0190)	-0.0207*** (0.0065)	-0.0152** (0.0059)
Mean of $Y_{j,t}$	-0.0568	-0.0149	0.2629	0.2778	0.0504
Observations	475437	421711	421711	421711	475437
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in employment are computed as in equation (4). “Exit” is a dummy variable that takes the value of 1 if a firm does not have employment in period t but did have employment 12 months before. “Credit Supply Shock” is defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

The employment effect comes from both the extensive and intensive margins. In columns (2) through (4), we examine the effects on the intensive margin by restricting the sample to firms that neither entered nor exited the formal sector during the analysis period (i.e., continuing firms) and decompose the corresponding contributions of inflows and outflows. We find that conditional on a firm’s continuing to operate, the overall effect of positive shocks on employment (column (2)) is positive, albeit not statistically significant, yet the negative effect on separations (column (4)) is significant. These results suggest that, during the COVID-19 recession, firms exposed to positive credit shocks were more likely to survive and had lower outflows, albeit without increasing their job creation. Since we observe only formal employment but formal firms may have both formal and informal employees (Busso, Fazio, and Algazi, 2012; Samaniego de la Parra and Fernández Bujanda, 2024), it is plausible that firms may have adjusted employment of informal workers first, as doing so is less costly, such that our results represent a lower bound on the employment effects.

Prior literature has found that the effects of credit supply shocks vary across the business cycle. Specifically, the volatility and effects of credit supply shocks on aggregate variables are larger during recessions (Becker and Ivashina, 2014; Gambetti and Musso, 2017; Colombo and Paccagnini, 2020; Alfaro, García-Santana, and Moral-Benito, 2021; Barnichon, Matthes, and Ziegenbein, 2022). With these findings in mind, we study the effects of credit supply shocks during the different stages of the pandemic. To do so, we split our sample into two periods, 2019–2020 and 2020–2021, and separately estimate equation (5) for each of them.⁴⁰

Panel A of Table 7 presents the results for the 2019–2020 period, which are in general larger than those observed when we pool periods. We find that during this initial phase of the pandemic, marked as it was by high uncertainty and strict lockdown measures, credit supply shocks affected employment at both the intensive and extensive margins. Firms that faced a negative credit shock of one-standard-deviation decreased their formal employment growth by 1.4 pp, which represents 20% of the mean decline during this period. In line with our main results, the estimates indicate that the effect of the credit supply shocks operated mainly through their effect on employment outflows and exit probability. Moreover, the extensive margin was of particular importance during this period. These results are consistent with those in Chodorow-Reich (2013), Bentolila, Jansen, and Jimenez (2017), Berton et al. (2018) and Popov and Rocholl (2018), who report large effects of credit supply shocks at the onset of the Great Recession.

⁴⁰Incorporating firm fixed effects into our baseline specification requires that we have data for each firm across at least two periods. Moreover, since our specification uses growth rates as the dependent variable, we need at least three time periods to calculate year-on-year changes for at least two time periods.

Table 7: Real Effects of Credit Supply Shocks by Period

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
<u>Panel A: 2019-2020</u>					
Credit Supply Shock	0.1359*** (0.0467)	0.0303* (0.0165)	-0.0104 (0.0067)	-0.0407*** (0.0124)	-0.0343*** (0.0118)
Mean of $Y_{j,t}$	-0.0679	-0.0361	0.2477	0.2838	0.0386
Observations	316406	281578	281578	281578	316406
<u>Panel B: 2020-2021</u>					
Credit Supply Shock	0.0901 (0.0574)	0.0348 (0.0258)	0.0128 (0.0248)	-0.0221** (0.0109)	-0.0088 (0.0100)
Mean of $Y_{j,t}$	-0.0496	-0.0102	0.2627	0.2729	0.0417
Observations	306678	271316	271316	271316	306678
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in employment are computed as in equation (4). “Exit” is a dummy variable that takes the value of 1 if a firm does not have employment in period t but did have employment 12 months before. “Credit Supply Shock” is defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

While the credit supply shocks had economically and statistically significant effects on real outcomes at the onset of the pandemic, a year later, during the recovery phase, their effects on employment and exit probability were smaller and statistically nonsignificant, yet their effect on outflows remained (Panel B of Table 7). The strong effects of credit early on, followed by the lack of a meaningful impact, are consistent with credit availability being more critical during episodes with tight liquidity constraints and high uncertainty.⁴¹ Alfaro, García-Santana, and Moral-Benito (2021) also document sizable real effects of credit supply shocks for Spanish firms

⁴¹The muted effect during the second phase could be a result of Mexico’s outsourcing reform, as the estimates are more precisely estimated when we exclude firms likely affected by it (see A.8.3).

during the years of the Great Recession but a small effect during the recovery.

In summary, the results in this section suggest that credit supply shocks had quantitatively relevant real effects on firms' employment and survival during the pandemic. To further validate these results, in Appendix A.8, we show that they are robust to our including more detailed industry controls, excluding firms likely influenced by the 2021 outsourcing reform, and changing the reference month. We also verify that our results are not driven by a particular bank.

6.1 Financial Constraints

In this section, we study how the real effects of credit supply shocks vary across firms depending on their degree of financial constraints. We focus on this factor because theory predicts that financially constrained firms should be more sensitive to changes in credit availability (Bernanke, Gertler, and Gilchrist, 1996), and previous empirical work confirms that the effects of credit supply shocks vary by firm characteristics (Chodorow-Reich, 2013; Duygan-Bump, Levkov, and Montoriol-Garriga, 2015; Siemer, 2019). Since a firm's degree of financial constraint is not directly observable, we categorize firms into groups based on proxies for financial constraint, specifically, firm age, size, and the degree of external financial dependence of the sector they operate in (Rajan and Zingales, 1998). To test for heterogeneous effects along these proxies, we modify our baseline specification and interact the firm's exposure to credit supply shocks (Z_{jt}) with indicator variables that categorize firms by age and size, and by their sector's degree of external financial dependence.⁴² Namely, for each $H \in \{\text{Age} \times \text{Size}, \text{Financial Dependence category}\}$, we estimate the following equation:

$$Y_{j,t} = \beta_0 + \sum_{h \in H} \beta_h Z_{j,t} \times D_h + \omega_j + \psi_{ilsah(j),t} + \epsilon_{j,t} \quad (6)$$

⁴²In Appendix A.7.1, we present heterogeneity between incorporated and unincorporated firms, as incorporation status is another proxy for financial constraints. We also explore other dimensions more relevant to the pandemic, such as designation as an essential or non-essential sector and industry of operation.

where D_h is a dummy variable that takes the value of 1 if firm j belongs to category h while everything else is as above. Note, however, that, if applicable, we also interact the heterogeneous time trends with the corresponding group H of analysis ($\psi_{ilсах(j),t}$) to control for any time-varying effect at that level (ILSAHT fixed effects).

Table 8 presents the results derived when we interact our measure of credit supply shocks with dummy variables indicating whether the firm belongs to the following groups: small young, small old, and large. Small (large) firms are those with fewer (more) than 100 workers, whereas young (old) firms are those that have operated for less (more) than ten years.⁴³ These groups broadly proxy the degree of firms’ financial constraints, as small and young firms tend to have less collateral, weaker relationships with lenders, shorter credit histories, and other characteristics that lead them to face tighter borrowing limits imposed by banks. Columns (1) and (2) show that credit supply shocks affected employment at both the intensive and extensive margins for small firms but not for large ones. In particular, the effects were largest among small young firms. Consistent with small young firms having fewer alternative sources of financing, lower cash reserves, and higher revenue volatility during recessions, we find that the effects of credit supply shocks were strongest among this group. This finding is also in line with prior work focusing on the Great Recession (Chodorow-Reich, 2013; Siemer, 2019).

Table 9 presents the impacts of credit supply shocks on firms segmented by their reliance on external finance, defined as the share of investment not financed by internal cash flows, following Rajan and Zingales (1998). Using the sector classification from Duygan-Bump, Levkov, and Montoriol-Garriga (2015), we classify firms in sectors with above-median levels of external financial dependence as “high financial dependence” firms, and those below the median as “low financial dependence” firms. Our findings indicate that firms in sectors with greater dependence on external finance exhibit a more pronounced response to credit supply shocks. This result is intuitive, as firms in these sectors rely more on bank lending than others.

⁴³We do not distinguish large firms by age given the scant number of large young firms.

Table 8: Real Effects of Credit Supply Shocks by Age and Size (2019–2021)

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock $\times$					
Small $\times$ Young	0.1581*** (0.0521)	0.0405 (0.0302)	0.0126 (0.0264)	-0.0279*** (0.0062)	-0.0246*** (0.0070)
Small $\times$ Old	0.0309 (0.0238)	0.0147 (0.0125)	0.0005 (0.0126)	-0.0142 (0.0106)	-0.0038 (0.0067)
Large	-0.0415 (0.0758)	-0.0050 (0.0579)	0.0130 (0.0198)	0.0179 (0.0468)	0.0180 (0.0210)
Mean of $Y_{j,t}$	-0.0568	-0.0149	0.2629	0.2778	0.0504
Observations	475437	421711	421711	421711	475437
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in employment are computed as in equation (4). “Exit” is a dummy variable that takes the value of 1 if a firm does not have employment in period t but did have employment 12 months before. “Credit Supply Shock” is defined in equation (3). Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those with less (more) than 10 years. See Appendix A.1.2 for additional details. Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

Table 9: The Real Effects of Credit Supply Shocks by High and Low Financial Dependence

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock $\times$					
Low Fin. Dependence	0.0716** (0.0291)	0.0146 (0.0239)	-0.0044 (0.0241)	-0.0190 (0.0116)	-0.0121 (0.0076)
High Fin. Dependence	0.1368*** (0.0418)	0.0424*** (0.0156)	0.0196 (0.0148)	-0.0227*** (0.0056)	-0.0193*** (0.0074)
Mean of $Y_{j,t}$	-0.0569	-0.0150	0.2629	0.2779	0.0504
Observations	473651	420133	420133	420133	473651
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in Employment are computed as in equation (4). Exit is a dummy variable that takes the value of 1 if a firm does not have employment in period t , but did have employment 12 months before. Credit Supply Shocks are defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using IMSS and R04-C datasets.

6.2 Worker Heterogeneity

This section studies how the pandemic credit supply shocks affected firms' labor force across different types of workers and contracts. Labor costs and match values differ within a firm, so the incentives to terminate a match vary across employment groups. For instance, temporary contracts and younger matches (i.e., workers with shorter tenure) have lower adjustment costs and arguably less match-specific human capital and, thus, are more likely to be terminated when a firm needs to downsize. Firms may also face different incentives to terminate matches for workers with different demographic characteristics, such as people of a certain age or gender, based on these groups' attachment to the labor force.

To examine whether firms differentially terminate matches across groups when exposed to credit supply shocks, we first categorize matches in each firm by the worker's firm-level tenure and

contract duration. We then study how firms' exposure to credit supply shocks affects employment growth across these worker and contract categories. Given our prior findings showing that the impact of credit supply shocks concentrates among small young firms, we allow the effects to vary by firm size and age. We also analyze the effects on wage growth across the various worker categories and contract types, focusing on the set of workers who remained at the firm.⁴⁴

We restrict our analysis to continuing firms first because we also decompose employment growth into inflows and outflows as above but, more importantly, because firms that exit have, by construction, homogeneous job outflows across all types of matches (i.e., all matches terminate regardless of worker or contract type once the firm ceases to exist) and entrant firms, by definition, have a workforce composed entirely of low-tenure workers. Thus, for the sample of continuing firms, we estimate equation (5) but define the dependent variable $\Delta Y_{j,t}^g$ as either employment or wage bill growth for worker group g , where $g \in \{high\ tenure, low\ tenure, permanent\ contracts, temporary\ contracts\}$.⁴⁵ Since the dependent variables are growth rates, the coefficients are also interpreted as elasticities to credit supply shocks, though for different categories of workers.⁴⁶

Tenure and Contract Type

In the presence of financial constraints, firms may find it easier to separate workers with lower dismissal costs, such as short-tenure workers and workers with temporary contracts, despite this not being optimal. For instance, in the face of a shock, entry-level workers with promising career paths may be dismissed instead of long-tenured ones simply because of liquidity concerns

⁴⁴Using our employed-employee individual records, we first identify workers who stayed in the firm between t and $t - 1$ and then compute the wage bill growth of these staying workers as for other variables, namely, $\frac{(Wage_t^{Stayers} - Wage_{t-1}^{Stayers})}{0.5(Wage_t^{Stayers} + Wage_{t-1}^{Stayers})}$. A limitation of studying wages using the social security data is that they are top-coded to 25 minimum wages, though in practice fewer than 2% of our observations are coded in this top category. Moreover, firms may under report wages to partially evade payroll taxes. Nonetheless, if credit supply shocks do not differentially affect the incentive to under report, the analysis is informative about the impact on wage growth.

⁴⁵In Appendix A.7.2, we also study the heterogeneous effects by gender, finding that the sensitivity of women's employment to credit supply shocks was higher than that of men's.

⁴⁶By definition, the overall elasticity is a weighted average of the elasticities of the corresponding groups, with the weights defined as $\frac{Y_{j,t}^g + Y_{j,t-1}^g}{Y_{j,t} + Y_{j,t-1}}$.

(Caggese, Cuñat, and Metzger, 2019) since financial frictions lead firms to place more weight on current than on future cashflow. Similarly, firms with financial constraints may demand a larger share of temporary workers in regular times, despite this being less efficient than employing workers on permanent contracts, as a tool to absorb temporary volatility shocks during a downturn (Caggese and Cuñat, 2008). To test whether these mechanisms were at play during the pandemic recession, we study the impact of firms’ exposure to credit supply shocks among workers with different tenure profiles or contract duration, as these groups have heterogeneous dismissal costs. Furthermore, as above, we study these effects by firm size, considering that large firms tend to be less financially constrained than small ones.

Tables 10 and 11 show the effects of exposure to credit supply shocks on employment growth and flows by worker tenure and contract type, respectively. We segment workers into the low- and high-tenure categories by whether they have exceeded five years in the company, whereas contract type refers to the contract duration: temporary or permanent.⁴⁷ Table 10 shows that among small young firms (which are arguably more financially constrained), a one-standard-deviation (10 pp) difference in exposure to credit supply shocks is associated with a 0.6 pp difference in employment growth among low-tenure workers. We find a similar result for temporary workers in small young firms (Table 11). In contrast, consistent with our previous results, the estimates for small old and large firms are not statistically significant for either group of workers, suggesting that less financially constrained firms did not differentiate their labor hoarding among workers in the presence of positive supply shocks during the COVID-19 recession.

Consistent with both low-tenure workers and those with temporary contracts being more likely to separate from their employer when a firm experiences a negative shock, we find that differential job outflow rates drive the effect on employment growth. This behavior also aligns with the idea that a worker’s value to a firm tends to increase with experience, making it more

⁴⁷We group workers into mutually exclusive categories based on their tenure in $t - 1$. Specifically, we define the high-tenure (low-tenure) group as the set of workers who in period t had been at firm j for more (less) than 5 years. Then, for each firm j , we calculate the year-on-year change in the stock of workers for each tenure group as in equation 4.

likely for companies facing liquidity shocks to adjust their workforce at the lower end of the tenure scale, where temporary contracts are more prevalent. In fact, [Osuna-Gomez \(2023\)](#) finds that shorter-tenured workers had a higher probability of job loss during the pandemic in Mexico, and our findings suggest that exposure to negative credit supply shocks amplified this phenomenon.

The effect of credit supply shocks on the wage growth of remaining workers is generally negative, regardless of the type of worker. However, the impact is more pronounced, both in economic magnitude and statistical significance, for workers with high tenure and permanent contracts. This is consistent with firms reducing the number of workers with low dismissal costs, while adjusting the wages of those with high dismissal costs, not only to retain them, but also to compensate for a likely increase in their workload ([ILO \(2022\)](#)).

Table 10: Real Effects of Credit Supply Shocks on Employees by Tenure at Firm (2019–2021)

	Continuers			
	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Δ Wages (Stayers)
	(1)	(2)	(3)	(4)
<u>High tenure</u>				
Credit Supply Shock $\times$				
Small $\times$ Young	-0.0007 (0.0095)	0.0017 (0.0042)	0.0025 (0.0071)	-0.0079*** (0.0022)
Small $\times$ Old	0.0141 (0.0096)	0.0051 (0.0052)	-0.0090 (0.0099)	-0.0164** (0.0064)
Large	-0.0382 (0.0632)	0.0109 (0.0132)	0.0491 (0.0547)	-0.0198** (0.0095)
Mean of $Y_{j,t}$	-0.0988	0.0215	0.1203	0.0705
Observations	421711	421711	421711	421711
<u>Low tenure</u>				
Credit Supply Shock $\times$				
Small $\times$ Young	0.0637* (0.0354)	0.0226 (0.0312)	-0.0411*** (0.0100)	-0.0085 (0.0101)
Small $\times$ Old	0.0250 (0.0225)	0.0201 (0.0206)	-0.0049 (0.0151)	-0.0128*** (0.0034)
Large	0.0022 (0.0632)	0.0221 (0.0245)	0.0199 (0.0465)	-0.0091 (0.0111)
Mean of $Y_{j,t}$	0.0360	0.3714	0.3354	0.0872
Observations	421711	421711	421711	421711
Firm FE	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes

Notes: The sample includes only continuing firms. Employment growth, the contributions of inflows and outflows, and wage growth are computed as in (4). “Stayers” refers to workers who continue in the firm between t and $t - 1$. “Credit Supply Shock” is defined in equation (3). “High tenure” (“Low tenure”) refers to workers with more (less) than 5 years in the firm. Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those with the less (more) than 10 years. See Appendix A.1.2 for additional details. Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

Table 11: Real Effects of Credit Supply Shocks on Employees by Contract Type (2019–2021)

	Continuers			
	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Δ Wages (Stayers)
	(1)	(2)	(3)	(4)
<u>Permanent contracts</u>				
Credit Supply Shock $\times$				
Small $\times$ Young	0.0314 (0.0381)	0.0215 (0.0273)	-0.0099 (0.0144)	-0.0088 (0.0066)
Small $\times$ Old	0.0068 (0.0154)	-0.0015 (0.0136)	-0.0083 (0.0076)	-0.0172*** (0.0046)
Large	0.0263 (0.0678)	0.0026 (0.0205)	-0.0237 (0.0599)	-0.0184 (0.0185)
Mean of $Y_{j,t}$	-0.0550	0.1167	0.1718	0.0567
Observations	421711	421711	421711	421711
<u>Temporary contracts</u>				
Credit Supply Shock $\times$				
Small $\times$ Young	0.0603** (0.0242)	0.0317 (0.0250)	-0.0285*** (0.0061)	-0.0072 (0.0067)
Small $\times$ Old	0.0334** (0.0161)	0.0126 (0.0169)	-0.0207 (0.0140)	-0.0082 (0.0081)
Large	-0.1320 (0.0812)	-0.0779** (0.0321)	0.0540 (0.0558)	-0.0367** (0.0162)
Mean of $Y_{j,t}$	0.0293	0.3064	0.2771	0.0785
Observations	421711	421711	421711	421711
Firm FE	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes

Notes: The sample includes only continuing firms. Employment growth, contributions of inflows and outflows, and wage growth are computed as in (4). “Stayers” refers to workers who continue in the firm between t and $t - 1$. “Credit Supply Shock” is defined in equation (3). Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those with less (more) than 10 years. See Appendix A.1.2 for additional details. Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

6.3 Aggregate Effects

In this section, we quantify the aggregate impact of negative credit supply shocks on the observed decline in employment during the most critical pandemic year (2020). To do so, we follow Chodorow-Reich (2013) and compare the employment losses observed across firms in our sample with a counterfactual in which all firms were exposed to banks in the top percentiles of the credit supply shock distribution during the same period. Specifically, we compute a counterfactual employment growth rate assuming that firms had credit supply exposure at or above the 95th or 90th percentiles for this variable as follows:⁴⁸

$$\Delta \hat{E}_{j,t}(\tau) = \Delta \hat{E}_{j,t} + \hat{\beta} [\max\{0, Z(\tau) - Z_{j,t}\}] \quad (7)$$

In equation (7), $Z(\tau)$ denotes the τ th percentile for the variable $Z_{j,t}$ which represents the exposure of firm j to credit supply shocks, $\Delta \hat{E}_{j,t}$ the predicted value of employment growth according with our baseline regression, and $\Delta E_{j,t}(\tau)$ its counterfactual value assuming the exposure of firm j to credit supply shocks was $Z(\tau)$ or larger. Given that we showed credit supply shocks had a statistically significant effect only in the period 2019–2020 and that this effect was concentrated in small firms, we perform this exercise considering changes in total employment from November 2019 to November 2020 and assume that the effect of credit supply shocks on large firms is zero. For firms categorized as small, we consider the values of $\hat{\beta}$ estimated for this group of firms according to equation (5). Specifically, $\hat{\beta}$ corresponds to 0.19 for firms categorized as small and young and 0.08 for those categorized as small and old.⁴⁹ Computing equation (8) allows us to estimate the fraction of employment losses that could be attributed to having an exposure below the percentile τ :

⁴⁸Acharya et al. (2018) also follow this approach to calculate the aggregate effects of the contraction of credit supply during the European sovereign debt crisis.

⁴⁹We obtain these values by estimating equation (5) for the period 2019–2020.

$$\frac{\sum(\hat{E}_{j,t} - \hat{E}_{j,t}(\tau))}{\sum(E_{j,t} - E_{j,t-1})} \quad (8)$$

Table 12 shows our results. Employment among firms in our sample dropped by 4.2% between November 2019 and November 2020. Of this decrease, negative credit supply shocks account for between 27.6% and 33.3% of the employment drop in small and medium firms and between 8.7% and 10.5% of the total decrease in employment if we attribute firms the 90th and 95th percentile exposure to credit supply shocks, respectively. To the extent that even banks that contracted their credit supply the least still reduced lending, our results are conservative. However, we abstract from general equilibrium effects, which might have played a role that we cannot capture in this exercise, given its nature. For instance, firms with relatively more access to credit lines might have hired workers laid off by firms tied to banks that severely tightened credit standards.

Table 12: Share of Employment Losses Attributable to Tightening of Credit Standards (2019–2020)

τ	Small Firms	All Firms
95 th percentile	33.26%	10.5%
90 th percentile	27.56%	8.70%

Notes: This table presents the estimated additional employment that firms would have retained if exposed to credit supply shocks at or above the τ percentile. These numbers are computed for different values of τ and are expressed as a percentage of the total employment losses observed in our sample from November 2019 to November 2020. Source: authors’ calculations using IMSS and R04-C datasets.

It is informative to compare our findings with those from previous studies analyzing the real effects of credit supply shocks during financial crises. [Chodorow-Reich \(2013\)](#) constructs credit supply shocks based on banks’ exposure to mortgage-backed securities during the Great Recession, while [Acharya et al. \(2018\)](#) focus on banks’ holdings of sovereign bonds from Greece, Italy, Ireland, Portugal, and Spain (GIIPS countries) during the European Debt Crisis. In both in-

stances, unexpected declines in the value of these assets led to significant losses on banks’ balance sheets, resulting in a contraction of credit supply. To assess the aggregate impact of these shocks, both studies employ a counterfactual analysis similar to ours: estimating what employment levels would have been if firms had been exposed to the least-affected banks. [Chodorow-Reich \(2013\)](#) finds that negative credit supply shocks account for between one-third and one-half of the employment decline among small and medium-sized firms in the year following the Lehman Brothers bankruptcy. Similarly, [Acharya et al. \(2018\)](#) estimate that such shocks account for 49% of the employment drop for European non-GIIPS countries and 66% for GIIPS countries.⁵⁰

Although there are caveats in comparing our results to those from the Great Recession and the European Debt Crisis—mainly related to the composition of shocks and the economic context—our estimates are of similar magnitude. This is despite the pandemic being a non-financial recession and banks entering it with solid liquidity positions, which improved for most banks during the crisis. These results suggest that, even when liquidity is not a constraint, bank behavior can amplify downturns—likely through increased risk aversion, as suggested by the lending survey and bank balance sheet data (see Section 4.3). This points to a different amplification mechanism than in financial crises: not driven by capital or funding constraints, but by changes in the willingness to lend. Taken together, the results imply that liquidity injections, while useful as a backstop, may not be enough to sustain credit supply during non-financial recessions. In these cases, other tools—such as government-backed loan guarantees—may be more effective.

7 Conclusion

We examine the drivers and real effects of credit supply shocks during a non-financial recession in a developing country. Our strategy consists of first identifying time-varying credit supply shocks

⁵⁰[Chodorow-Reich \(2013\)](#) uses the 95th and 90th percentiles of firm exposure to credit supply shocks to obtain lower and upper bounds for the employment effects, and we follow this approach. In contrast, [Acharya et al. \(2018\)](#) consider the 95th percentile for their main estimates.

at the bank level and then constructing metrics of exposure to these shocks for each firm. To achieve this, we use the credit registry, which includes the universe of business loans. Further, we leverage banks' balance sheet data and confidential, bank-level responses to a SLOOS survey to first, validate the shocks as supply-driven, and second, to analyze their drivers.

We find substantial economic effects of credit supply shocks on employment and the probability of firm survival, particularly during the height of the pandemic (2020). These effects were highly heterogeneous across firms, with significant impacts on financially constrained firms (small and young firms). A partial equilibrium exercise indicates that negative credit supply shocks accounted for approximately a third of the drop in employment for small firms. We also extend our analysis to study the effects of credit supply shocks on different types of workers and find that the significant impact of credit supply shocks on employment predominantly affected female workers, low-tenure workers, and those on temporary contracts.

Despite our data-driven approach being agnostic about the specific shocks that banks faced during this period, our measure of bank-level credit supply shocks correlates with bank performance and funding metrics and tracks a credit supply index from Mexico's bank lending survey. Moreover, we show that changes in risk tolerance, rather than in funding availability, deteriorated credit supply conditions. These results suggest that targeted measures to alleviate financial stress among vulnerable firms during a severe crisis in developing countries could help mitigate adverse real economic effects.

Looking ahead, an important area for future work is incorporating endogenous bank risk aversion in models that seek to understand the bank lending channel. In particular, allowing for heterogeneity in risk aversion across time, and across market segments, are necessary to rationalize the heterogeneity that we document. Improving our understanding of non-fundamental driven shocks is important to shape future policy decisions during non-financial recessions.

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A Appendix

A.1 Data Appendix

A.1.1 Credit Report Data (R04c)

Data Cleaning. We make several adjustments to the loan raw data before estimating the credit supply shocks. First, we only keep loans provided to firms with a fiscal address in Mexico, as those are the ones for which we have employment data. Additionally, we exclude loans issued to the government (at the federal, state, or municipal level) or companies partially owned by the government since banks might face different incentives for lending to the latter. Including them would change the interpretation of the credit supply shocks. We also remove loans issued by development banks, as we are specifically interested in examining the impact of credit from private banks on employment. Additionally, we remove credits issued in other currencies rather than Mexican pesos. Otherwise, we would have to adjust for exchange rate movements, which were dramatic during the first months of the pandemic. Moreover, we exclude nonperforming loans as these do not leave our sample homogeneously. For instance, an eight-month past due loan from bank B might disappear from the dataset in the appropriate period, while an equivalent one from a different bank might stay in the dataset for longer. However, our estimates capture these movements, as they are relationships that pass from positive to zero. Lastly, we exclude loans from firms with access to the bond markets since these have alternative funding sources.

As is common in the literature, we also adjust by mergers and acquisitions. In particular, we join financial entities either absorbed or purchased by others during 2017-2021. Moreover, we join institutions that belong to the same financial group but, for different circumstances, have split their banking business into several branches.⁵¹ Finally, we remove banks with less than 100 active loans (typically investment institutions) during the studied period. Banks with a small

⁵¹This is a particular feature of the Mexican Banking System due to the existence of SOFOMES (*Sociedades Financieras de Objeto Múltiple*), financial entities that are allowed to extend credits, but not to receive deposits. For instance, certain banks have their own SOFOME for credit cards or car loans.

number of loans tend to exhibit large fluctuations in their outstanding level of credit and hence can bias the estimation of bank fixed effects.

Firm Characteristics in the Credit Dataset As mentioned in the main text, firm characteristics in the dataset are collected once a loan is issued. However, in some cases, these characteristics, like the location or the industry, may vary across periods or time. We make those variables time-invariant in those cases, assigning the most common value throughout 2017-2021. Regarding size, firms in the credit dataset are classified as micro, small, medium, or large based on a combination of their number of workers and revenues, according to standard national rules of classification. Table A.1 summarize them.⁵²

Table A.1: Firm Size Definitions Used in the Credit Registry

Size	Sector	Number of Workers	Net Sales or Net Income	Max Score
Small	Commerce	11-30	\$4.01-\$100	93
	Industry and Services	11-50	\$4.01-\$100	95
Medium	Commerce	31-100	\$100.01-\$250	235
	Services	51-100		235
	Industry	51-250		250
Large	When max score exceeds 235 for firms operating in Commerce and Services and 250 for Industry.			

Source: R04-C report instruction manual.

A.1.2 Administrative Employer-Employee Matched Dataset

Data Cleaning. We remove from our sample firms in the following NAICS sectors: Agriculture (11), Finance (52), Management (55), Foreign Agencies (93), and Administrative and Support Services (561), as the latter likely includes employment outsourcing providers (see A.8.3).

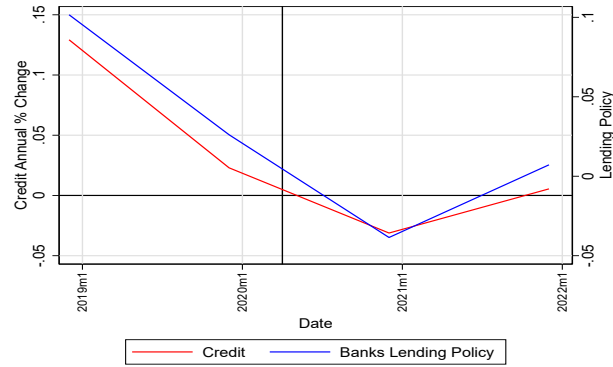
Firm Characteristics. Consistent with the firm dynamics literature, we define firm size in the IMSS dataset by classifying firms according to their average employment of the previous 12 months. We use this value to divide them into "large" (more than 100 employees) and "small"

⁵²If a firm falls outside the standard categories, it is evaluated using a scoring system. For example, a commercial firm with 20 employees and \$150 million in net sales would have its score calculated by adding 10% of its employee count to 90% of its net sales. This score is then compared to the maximum scores listed in the 'Max Score' column.

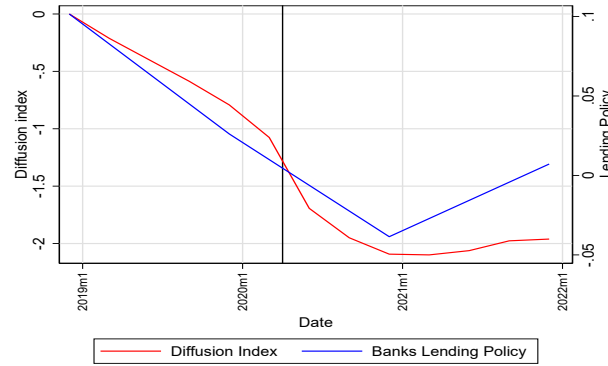
(less than 100 employees). To avoid reclassifying firms into different size categories, we consider their size the first time they appear during our study period. To construct age at period t , we compute the difference between that date and a firm's first appearance in this dataset, which we consider its birth date. Our analysis categorizes firms into two age groups: less than ten years of operating and more than ten years of operating.

A.2 Bank Lending Policies

Figure A.1: Bank Lending Policies' Relationship with Overall Credit Growth (a) and Lending Standards Measured by a Diffusion Index (b).



(a) Bank Lending Policies and Credit Growth



(b) Bank Lending Policies and Credit Standards According to a Diffusion Index Constructed with the Bank Lending Survey

Notes: Our bank lending policy metric is constructed as a weighted average of bank fixed effects, aimed at capturing overall credit standards according to our estimations. Panel (b)'s diffusion index uses data from Banxico's lending survey. Each quarter, banks answer the following item: "Over the past three months, how have your bank's credit standards for loans to firms changed?" Responses, paired with numerical values, are as follows: "tightened considerably" (-1), "tightened somewhat" (-0.5), "unchanged" (0), "eased somewhat" (0.5), "eased considerably" (1). Each bank's score is weighted by its portfolio significance. Quarterly results, starting from December 2018, cumulatively form a diffusion index tracking the evolution of credit standards since then. Weights are the total outstanding credit in the previous period. Source: authors' calculations using the R04-C and SLOOS data.

A.3 Decomposition of Annual Changes in Aggregate Credit

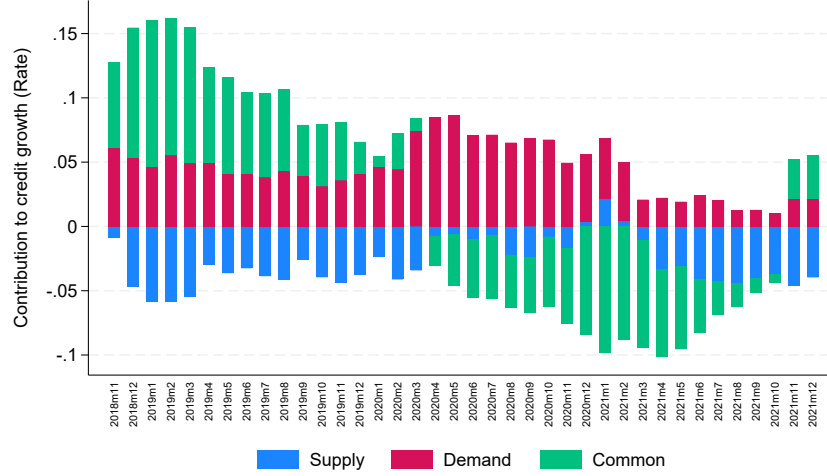
We can use our estimators from equation (1), namely $\gamma_{ils(j),t}$ and $\delta_{b,t}$, with appropriate weights, to see the contribution of both supply and demand factors to *aggregate* credit growth. Yet, because of collinearity issues, one cannot recover all the fixed effects of the estimation, so a normalization is needed. Thus, following [Amiti and Weinstein \(2018\)](#), we normalize the estimated coefficients to their corresponding median, $\gamma_{ils(j),t}$ and $\delta_{b,t}$, such that $\gamma_{ils(j),t} = \gamma_{ils(j),t} - \gamma_{ils(j),t}$ and $\tilde{\delta}_{b,t} = \delta_{b,t} - \delta_{b,t}$, and aggregate the elements of the normalized version of (1) using $\omega_{j,b,t}$ weights as follows,⁵³

$$\sum_{j,b,t} \omega_{j,b,t} \Delta_k Credit_{j,b,t} = \sum_{j,b,t} \omega_{j,b,t} \gamma_{ils(j),t} + \sum_{j,b,t} \omega_{j,b,t} \tilde{\delta}_{b,t} + (\gamma_{ils(j),t} + \delta_{b,t}) \quad (9)$$

where the first and second elements correspond to the demand and supply components, respectively, whereas the last term, $(\gamma_{ils(j),t} + \delta_{b,t})$ refers to the “common” component, that is, aggregate shocks that cannot be separately attributed to neither supply nor demand factors. Figure [A.2](#) presents the results of this decomposition. As can be seen, during the studied period, supply factors generally contributed negatively to aggregate credit growth, with the “common” component doing it mostly during the most enduring months of the pandemic.

⁵³These weights are consistent with how we compute growth rates, namely $\frac{(Credit_{j,b,t} + Credit_{j,b,t-1})}{\sum_{j,b,t} (Credit_{j,b,t} + Credit_{j,b,t-1})}$.

Figure A.2: Annual Credit Changes: Supply, Demand, and Common Factors

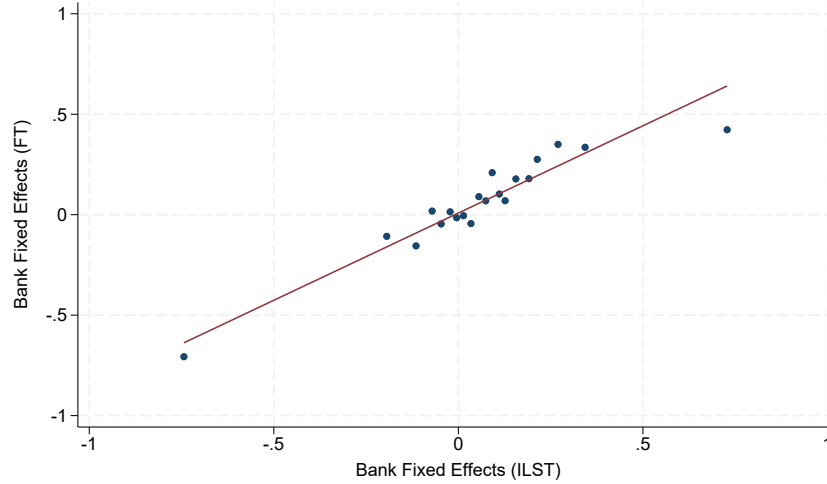


Notes: The decomposition exercise is based on estimating equation (9). Source: authors' calculations using the R04-C dataset.

A.4 Comparison Between Our Baseline Credit Supply Shocks (ILST) and Amiti-Weinstein's Specification (FT)

Figure A.3 presents the Bank FE estimated using our baseline methodology described in equation (1) (ILST) and contrast them with the ones obtained by applying the methodology proposed by Amiti and Weinstein (2018), which estimates Bank FE considering only multi-bank firms (FT). The later set of Bank FE are obtained by estimating the following equation: $\Delta_k Credit_{j,b,t} = \gamma_{j,t} + \delta_{b,t} + \epsilon_{j,b,t}$, where $\gamma_{j,t}$ are firm FE and $\delta_{b,t}$ are Bank FE. The figure presents a bin scatter plot and shows that both sets of Bank FE are highly correlated, but there are some differences between them. These differences, as noted by Degryse et al. (2019), capture the additional information provided by single-bank firms, which constitute the majority of the sample (70%) in the case of Mexico.

Figure A.3: Bank FE: ILST vs FT

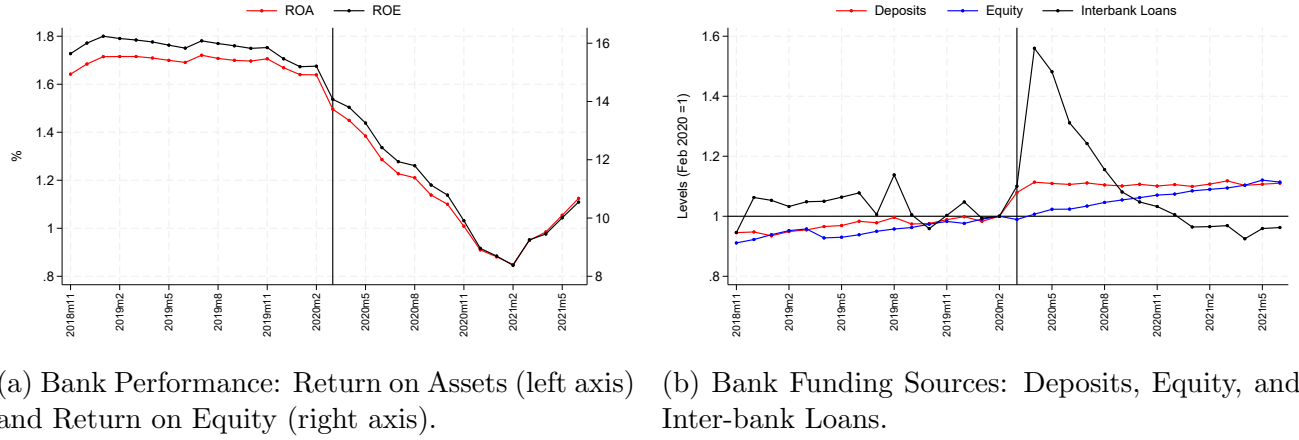


Notes: Our baseline credit supply shocks at the bank level are derived from the bank fixed effects estimated in Equation (1). In this figure, they are contrasted using a bin scatter plot with their counterparts estimated using the methodology of [Amiti and Weinstein \(2018\)](#). Source: authors' calculations using the R04-C dataset.

A.5 Banks Performance and Funding Sources

The bank regulator, CNBV, publishes monthly reports on the balance sheets and financial indicators of individual banks, as well as for the entire banking sector. Figure A.4 was constructed using that public information and covers the entire sector.

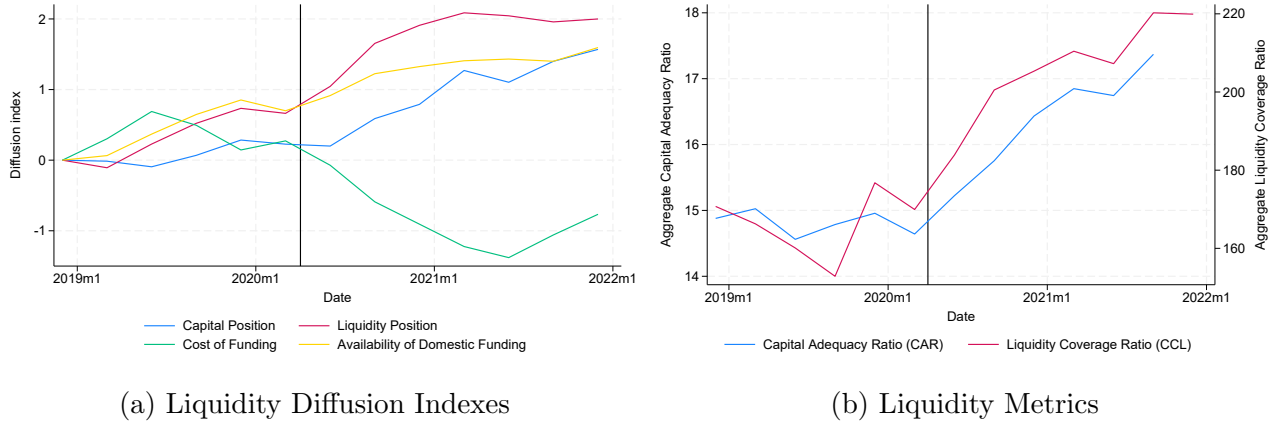
Figure A.4: Bank Performance (a) and Funding Sources (b)



Source: Authors' calculations using CNBV public reports.

A.6 Evolution of Bank Liquidity

Figure A.5: Risk and Liquidity Conditions



Notes: In the Mexican Bank Lending Survey, bank executives report how different conditions have evolved over the past four months. A positive value reflects an improvement in a given condition, while a negative value indicates a deterioration. Responses, paired with numerical values, are as follows: “tightened considerably” (-1), “tightened somewhat” (-0.5), “unchanged” (0), “eased somewhat” (0.5), “eased considerably” (1). Each bank’s score is weighted by its portfolio significance and that weighted scored is accumulated starting in December 2018 to form a diffusion index. Panel a) present the evolution of those diffusion indexes for different liquidity measures. In Panel b) the Capital Adequacy Ratio (CAR) and the Liquidity Coverage Ratio of the banking system as a whole are presented. Source: authors’ calculations using Mexico’s SLOOS, Banks’ Balance Sheets, and Banxico’s public reports.

A.7 Additional Results: Heterogeneous Effects of Credit Supply Shocks

A.7.1 Firm Level Results

Incorporation Status

In our sample, fifty-nine percent of employers are unincorporated firms. These type of firms generally sole proprietors and family firms, are typically younger, smaller, and abundant in developing countries ([Rivadeneira \(2023\)](#)). In addition, they have less access to capital markets. As a result, they rely more heavily on credit to finance their operations and investment needs. Moreover, they depend on the owner's credit history and wealth for credit access, so collateral constraints are more likely to bind. Consistent with these firms' higher reliance on bank credit, [Table A.2](#) shows that credit supply shocks have larger effects on unincorporated firms at both the intensive and extensive margins. Across all dimensions (except exit), the effects for this group of firms are sizable and statistically significant. These results confirm that a high prevalence of unincorporated firms is critical to a country's firm dynamics, especially in times of crisis.

Sector

[Table A.3](#) reveals that the impact of credit supply shocks on employment is larger for the Service and Commerce sectors, with both economically and statistically significant effects, although the effect on exit is only statistically significant in the latter. This result intuitively aligns with the observation that the demand for those industries declined more sharply compared to other sectors during the pandemic, making access to liquidity for firms in those industries more critical.

Essential Sectors

A unique feature of the COVID-19 recession was that the government impeded some firms from operating due to the health emergency. Moreover, as mentioned earlier, Mexico stood out for its lack of government business support programs. This critical situation posed a dilemma for

Table A.2: Real Effects of Credit Supply Shocks by Incorporation Status (2019–2021)

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock $\times$					
Unincorporated	0.0891*** (0.0184)	0.0302** (0.0120)	0.0124* (0.0074)	-0.0178*** (0.0069)	-0.0068 (0.0060)
Incorporated	0.0247 (0.0254)	0.0066 (0.0225)	-0.0156 (0.0191)	-0.0222** (0.0108)	-0.0027 (0.0112)
Mean of $Y_{j,t}$	-0.0565	-0.0148	0.2630	0.2778	0.0504
Observations	473040	419265	419265	419265	473040
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in Employment are computed as in equation (4). Exit is a dummy variable that takes the value of 1 if a firm does not have employment in period t , but did have employment 12 months before. Credit Supply Shocks are defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using IMSS and R04-C datasets.

firms, as they may have wanted to preserve good matches until the health emergency was over yet lacked the means to do so. Thus, positive credit supply shocks may have helped firms by improving credit availability, boosting their resources to cover wage bills. With this in mind, we test whether the pandemic credit supply shocks differently affected sectors deemed essential and nonessential by the government.⁵⁴ Firms in essential sectors were allowed to operate during the pandemic, in contrast to those in nonessential sectors, which had to shutter their operations during this period and thus faced significantly different financial needs. Since these operational restrictions were in force mainly at the beginning of the pandemic, we conduct this analysis only for 2019–2020. Table A.4 shows the results.

⁵⁴INEGI mapped the essential/nonessential sectors declared by the government into 6-digit NAICS codes. Details about the methodology can be found in https://inegi.org.mx/contenidos/temas/directorio/doc/nota_metodologica_scian.pdf

Table A.3: The Real Effects of Credit Supply Shocks by Sector

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock $\times$					
Construction	0.1345 (0.1112)	0.0198 (0.0391)	-0.0038 (0.0219)	-0.0236 (0.0269)	-0.0299* (0.0169)
Manufacture	0.0274 (0.0639)	-0.0165 (0.0517)	-0.0115 (0.0347)	0.0050 (0.0284)	-0.0011 (0.0173)
Commerce	0.1434*** (0.0516)	0.0458*** (0.0171)	0.0107 (0.0144)	-0.0351*** (0.0110)	-0.0271** (0.0134)
Services	0.0896** (0.0440)	0.0276 (0.0311)	0.0104 (0.0293)	-0.0172 (0.0142)	-0.0096 (0.0074)
Mean of $Y_{j,t}$	-0.0568	-0.0149	0.2629	0.2778	0.0504
Observations	475437	421711	421711	421711	475437
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in Employment are computed as in equation (4). Exit is a dummy variable that takes the value of 1 if a firm does not have employment in period t , but did have employment 12 months before. Credit Supply Shocks are defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using IMSS and R04-C datasets.

The effect of credit supply shocks on employment is statistically significant only for the nonessential sectors. Notably, while the credit supply shocks had a negative and statistically significant effect on outflows among continuing firms of both types, the effect on nonessential ones was larger, meaning that positive credit supply shocks helped continuing firms in nonessential industries avoid a larger reduction in employment. In fact, all the effects concentrate on this margin. This result is consistent with firms' being able to hoard labor during the lockdowns if faced with positive credit conditions. In addition, positive credit shocks from the banking sector helped firms in both groups survive (column (5)), although the effects on the latter are also larger

for firms in nonessential sectors. This last result is consistent with some firms in this group not being able to operate because of government regulations and thus facing a larger decrease in demand than firms in essential sectors.

Table A.4: Real Effects of Credit Supply Shocks by Essential Sector Status (2019–2020)

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock $\times$					
Non-essential	0.1651*** (0.0605)	0.0423** (0.0196)	-0.0087 (0.0097)	-0.0510*** (0.0134)	-0.0384** (0.0177)
Essential	0.0730 (0.0493)	0.0078 (0.0213)	-0.0144 (0.0140)	-0.0222** (0.0113)	-0.0227*** (0.0077)
Mean of $Y_{j,t}$	-0.0678	-0.0361	0.2477	0.2837	0.0386
Observations	315532	280696	280696	280696	315532
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in employment are computed as in equation (4). “Exit” is a dummy variable that takes the value of 1 if a firm does not have employment in period t but did have employment 12 months before. “Credit Supply Shock” is defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

A.7.2 Worker Level Results

Gender

The literature has characterized the pandemic contraction, in contrast to previous recessions, as a *she-cession* (Alon et al., 2022), in the sense that women’s labor market outcomes deteriorated more than men’s. This phenomenon was observed not only in the US (Albanesi and Kim, 2021) but in a wide range of countries (Bluedorn et al., 2023). These studies suggest that the factors behind this disproportionate impact include the greater childcare burden shouldered by women,

their larger representation in the contact-intensive sectors severely affected during the lockdowns, and their higher presence in temporary and part-time jobs, which are more vulnerable during economic downturns.

Table A.5 shows the effects of the credit supply shocks on firms' employment growth by worker gender. No statistically significant effect exists for men, regardless of firm size or age. Meanwhile, a positive one-standard-deviation (10 pp) exposure to credit supply shocks increases female employment growth by 0.6 pp at small young firms. Importantly, this effect is driven by a decline in separations of female workers in these firms, with no significant effect on job creation. These results are consistent with small young firms being more responsive to credit supply shocks and female employment being more prone to adjustments, likely due to its tenure and contract characteristics (see below). While the literature is inconclusive regarding the existence of a shecession in Mexico (Viollaz et al., 2023; Hoehn-Velasco et al., 2022), our results suggest that the mostly negative credit supply shocks during this period contributed to a more pronounced negative effect on labor market outcomes for women.

On the other hand, the effect of credit supply shocks on female wages in small firms is not statistically significant, yet this is not the case for men. Column (4) shows a negative and statistically significant effect of credit supply shocks on men's wage growth. However, the magnitude of this effect is quantitatively small, as an increase of one standard deviation (10 pp) reduces wage growth by 0.11 pp for small young firms and 0.16 pp for small old firms.

Table A.5: Real Effects of Credit Supply Shocks on Employees by Gender (2019–2021)

	Continuers			
	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Δ Wages (Stayers)
	(1)	(2)	(3)	(4)
<u>Men</u>				
Credit Supply Shock $\times$				
Small $\times$ Young	0.0269 (0.0415)	0.0062 (0.0268)	-0.0207 (0.0175)	-0.0117* (0.0067)
Small $\times$ Old	0.0170 (0.0166)	0.0014 (0.0193)	-0.0156 (0.0126)	-0.0163*** (0.0050)
Large	0.0098 (0.0571)	0.0161 (0.0181)	0.0063 (0.0465)	-0.0164 (0.0103)
Mean of $Y_{j,t}$	-0.0228	0.2669	0.2898	0.0859
Observations	421711	421711	421711	421711
<u>Women</u>				
Credit Supply Shock $\times$				
Small $\times$ Young	0.0625*** (0.0228)	0.0237 (0.0228)	-0.0388*** (0.0139)	-0.0052 (0.0102)
Small $\times$ Old	0.0275 (0.0183)	0.0022 (0.0146)	-0.0253** (0.0112)	-0.0046 (0.0067)
Large	-0.0870 (0.0701)	-0.0177 (0.0296)	0.0693 (0.0561)	0.0116 (0.0112)
Mean of $Y_{j,t}$	0.0055	0.2437	0.2381	0.0778
Observations	421711	421711	421711	421711
Firm FE	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes

Notes: The sample includes only continuing firms. Employment growth, the contributions of inflows and outflows, and wage growth are computed as in (4). “Stayers” refers to workers who continued in the firm between t and $t - 1$. “Credit Supply Shock” is defined in equation (3). Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those with the less (more) than 10 years. See Appendix A.1.2 for additional details. Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors’ calculations using IMSS and R04-C datasets.

A.8 Robustness Checks

A.8.1 Demand controls when estimating Bank Fixed Effects

Table A.6 replicates the results of Table 6, but when we controls for 5 as opposed to 3-digit industries in equation (5). As can be seen, this specification yields similar results to the main ones, with slightly different magnitudes.

Table A.6: The Real Effects of Credit Supply Shocks (5D NAICS classification)

	All	Continuers			All
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock	0.0946*** (0.0273)	0.0371** (0.0159)	0.0127 (0.0173)	-0.0244*** (0.0069)	-0.0115 (0.0073)
Mean of $Y_{j,t}$	-0.0565	-0.0145	0.2626	0.2771	0.0508
Observations	459577	405990	405990	405990	459577
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

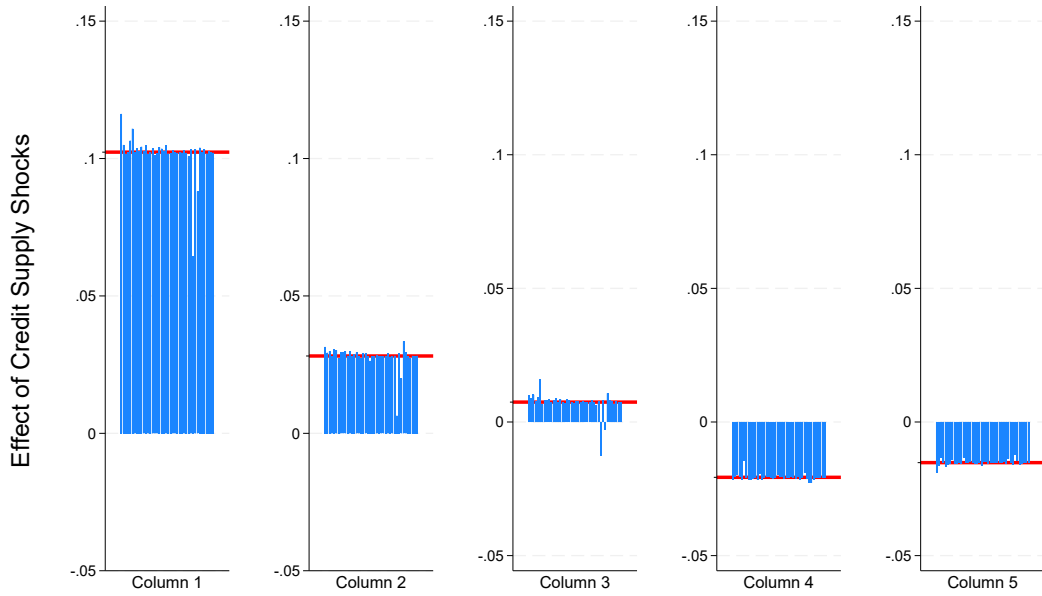
Notes: Changes in Employment are computed as in equation (4). Exit is a dummy variable that takes the value of 1 if a firm does not have employment in period t , but did have employment 12 months before. Credit Supply Shocks are defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using IMSS and R04-C datasets.

A.8.2 Sensitivity of Results to Individual Banks

Figure A.6 replicates the estimates in Table 6, but when each of our 42 banks is removed iteratively from the analysis. The idea is to test whether a single bank drives the overall findings. To do so, we take advantage of the equivalence result in Borusyak, Hull, and Jaravel (2021) and estimate a bank-level weighted regression without each bank and with weights re-normalized according to the absence of each bank. Each blue bar represents the results without a single bank, while the red bar represents our baseline results. As can be seen, this exercise delivers,

in general, similar results as our baseline ones for all outcomes, with some differences when the dependent variable is inflows, yet recall that this effect is already not statistically significant in our main analysis.

Figure A.6: Sensitivity of Main Results to Removing Banks



Notes: The bar in each graph represents the estimates of the corresponding column in Table 6 when one bank is removed. The red horizontal line is the estimate presented in Table 6. Source: authors' calculations using the IMSS and R04-C datasets.

A.8.3 Outsourcing Reform.

In April 2021, Mexico's Congress passed a law regulating employment outsourcing, requiring workers registered under outsourcing providers to reallocate and register with the firms they actually work for. As the law contemplated a period for firms to comply with these changes, most employment transitions occurred between June and September 2021. We identify potential outsourcing providers and firms using that scheme following the methodology proposed by [Banco de México \(2021\)](#).⁵⁵ While our baseline sample already removes potential outsourcing providers

⁵⁵If a firm either transferred a worker from the "Professional and Technical Services" (Code 233 in the IMSS dataset) or more than 20 workers to another firm between June and September 2021, it is categorized as an

(NACIS 561), we further remove additional ones using this methodology. We take the most conservative approach and remove all firms that received at least one outsourced worker. As seen in Table A.7, removing these firms delivers results similar to the baseline ones, although with higher statistical precision, particularly for the recovery period.

Table A.7: The Real Effects of Credit Supply Shocks
(Removing potential users and providers of outsourcing)

	All	Continuers			All
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
<u>Panel A: 2019-2020</u>					
Credit Supply Shock	0.1281*** (0.0422)	0.0359** (0.0174)	-0.0094 (0.0059)	-0.0452*** (0.0129)	-0.0278*** (0.0093)
Mean of $Y_{j,t}$	-0.0781	-0.0413	0.2371	0.2784	0.0415
Observations	287876	254434	254434	254434	287876
<u>Panel B: 2020-2021</u>					
Credit Supply Shock	0.0900** (0.0414)	0.0294* (0.0155)	0.0118** (0.0053)	-0.0176 (0.0115)	-0.0105 (0.0082)
Mean of $Y_{j,t}$	-0.0729	-0.0277	0.2382	0.2659	0.0441
Observations	277054	244064	244064	244064	277054
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in Employment are computed as in equation (4). Exit is a dummy variable that takes the value of 1 if a firm does not have employment in period t , but did have employment 12 months before. Credit Supply Shocks are defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using IMSS and R04-C datasets.

outsourcing provider, and all transferred workers are considered outsourced.

A.8.4 Reference Month

Table A.8 presents our main results estimated using December rather than November as the reference month. Overall, the results are similar to our main specification in magnitude and statistical significance.

Table A.8: The Real Effects of Credit Supply Shocks
(December as reference month)

	All	Continuers		All	
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock	0.1041*** (0.0356)	0.0321 (0.0235)	0.0137 (0.0193)	-0.0184** (0.0094)	-0.0103 (0.0082)
Mean of $Y_{j,t}$	-0.0547	-0.0135	0.2624	0.2759	0.0505
Observations	469818	416354	416354	416354	469818
Firm FE	Yes	Yes	Yes	Yes	Yes
ILST FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in Employment are computed as in equation (4). Exit is a dummy variable that takes the value of 1 if a firm does not have employment in period t , but did have employment 12 months before. Credit Supply Shocks are defined in equation (3). Exposure-robust standard errors clustered at the bank level in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using IMSS and R04-C datasets.