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Abstract: We study the real effects of credit supply shocks during the COVID-19 pandemic in Mexico. To this end, we merge administrative micro-level data on the universe of bank loans to firms with matched employer-employee social security records. For each firm, we measure its exposure to time-varying credit supply shocks. We find that a negative credit shock of one standard deviation would have increased a firm's exit probability by 0.15 percentage points (pp) and decreased its annual employment growth by 1 pp. These effects were most pronounced among unincorporated businesses, small and young firms, and those in non-essential sectors. Negative credit supply shocks led to higher separation rates for workers with low layoff costs, like those with low tenure or temporary contracts.
Keywords: Banks, credit supply shocks, employment
JEL Classification: D22, E24, E44, E51, G21

Resumen: Estudiamos los efectos reales de los choques de oferta de crédito durante la pandemia de COVID-19 en Mexico. Para ello, fusionamos microdatos administrativos sobre el universo de préstamos bancarios a empresas con registros de seguridad social que vinculan a empresas y trabajadores. Para cada empresa, medimos su exposición a choques de oferta de crédito que varían en el tiempo. Encontramos que un choque negativo de oferta de crédito de una desviación estándar habría incrementado la probabilidad de que una empresa saliera del mercado en 0.15 puntos porcentuales (pp) y su crecimiento en el empleo habría disminuido en 1 pp. Estos efectos fueron más pronunciados en empresas registradas como personas físicas con actividad empresarial, empresas jóvenes y pequeñas, y aquellas en sectores categorizados como no esenciales durante el confinamiento por la pandemia. Los choques negativos de oferta de crédito llevaron a tasas de separación más altas para los trabajadores con bajos costos de despido, como aquellos con poca antigüedad o contratos temporales.
Palabras Clave: Bancos, choques de oferta de crédito, empleo

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1 Introduction

During the COVID-19 pandemic, firms worldwide experienced a variety of adverse supply and demand shocks. Government-imposed lockdowns and disruptions in global trade hampered firms' operations, while contagion risks and income shocks dampened demand. As a result, markets globally experienced significant distress, with businesses facing increased exit risks and workers a surge in furloughs and layoffs. In this challenging economic environment, credit supply, particularly from the banking sector, may have shaped firms' ability to survive, hire, or retain workers, making studying their effects a crucial topic for research.

Analyzing the real effects of bank credit supply during a turbulent episode such as the pandemic can prove valuable in supporting a more comprehensive understanding of recessions and the design of effective public policies during such times. However, in contrast to what we know about other recessions, we have scant evidence on the real effects of credit supply during the pandemic, as both data limitations and the concurrence of multiple shocks 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 pandemic.

Mexico is an ideal setting for studying this question for several reasons. First, as in many developing countries, the banking sector is one of the primary sources of firms' funding.[1] Additionally, Mexico's response to the pandemic in terms of direct support to firms was modest compared to most economies, amounting to less than 1% of GDP (see A.1). 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 loan-level bank–firm matched data that, in turn, can be merged with administrative employer–employee records. These records allow us to build a panel on employment and credit covering the universe of formal firms between 2018-2021.

Our empirical analysis is multistep. We begin by estimating and validating time-varying bank credit supply shocks. Next, we use prepandemic bank relationships to mea-

[1] According 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.

sure firms' exposure to these shocks across time. We then study these shocks' effects on firm survival and employment. We compare these effects across different stages of the pandemic crisis and among firms of varying age, size, incorporation status, and operating conditions during the lockdowns. Additionally, we provide novel insights on how firms' exposure to credit supply shocks differentially affected employees according to their gender, tenure profile, and contract type. Finally, we use our reduced-form estimates and a partial equilibrium framework to assess the aggregate employment impact of credit supply shocks.

We first estimate bank-level credit supply shocks following the methodology of Degryse et al. (2019), a variation on the one developed by Amiti and Weinstein (2018) that allows us to decompose overall changes in bank credit into demand and supply components. This approach is well suited to settings where few firms borrow from more than one bank, such as Mexico. Specifically, we group firms into categories defined by their industry, location, and size. Then, we regress bank–firm credit changes on firm group–time and bank–time fixed effects. The former controls for demand, while the latter captures changes in credit associated with each bank's specific conditions, such as shifts in its risk tolerance and access to external funding. These bank fixed effects are the parameters of interest, as they capture credit supply factors. To validate our estimated credit supply shocks, we show that they correlate positively with banks' profitability measures, interbank funding, and deposits and negatively with equity growth. The estimated shocks also track bank lending standards across time from Mexico's Senior Loan Officer Opinion Survey. Last, 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. Most firms were exposed to negative credit supply shocks. Consistent with these metrics' capturing exposure to unexpected changes in supply-side credit conditions, we show that they are unrelated to firm-level characteristics before the recession but predict firms' credit growth and other loan market outcomes after it.

Equipped with this metric of firm-level exposure to credit supply shocks, we examine its effects on yearly firm exit and formal employment growth from 2018 to 2021. We find that firms exposed to a negative credit shock decreased employment growth by more and had a higher exit probability than those exposed to positive ones. A firm facing a negative one-standard-deviation bank credit supply shock increased its probability of exit by 0.15 percentage points (pp) and decreased its formal employment growth by 1 pp. Notably, the negative effect on employment growth is not driven solely by the reduction in firm survival. 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.

To understand the heterogeneity in the impact of the credit supply shocks during different stages of the pandemic, we split our sample and estimate effects across time. We find that the effects of the credit supply shocks on firm survival and employment growth were more pronounced during the first year of the pandemic (2020), a period marked by high uncertainty and strict lockdown measures. However, these effects were muted afterward. This initially sizable effect of the credit supply shocks during the recession, followed by the reduced impact during the recovery, is consistent with credit supply shocks' being more critical in times of tighter liquidity constraints and higher uncertainty (Alfaro, García-Santana, and Moral-Benito, 2021).

We also provide detailed analyses of the heterogeneity in the impacts of credit supply shocks by firm type. To do so, we interact our exposure metrics with relevant firm characteristics. We find that the effects were concentrated mainly among small and young firms, as in previous recessions (Chodorow-Reich, 2013; Siemer, 2019).[2] Moreover, we show a more pronounced effect of the credit supply shocks on survival and employment for unincorporated businesses and firms operating in sectors classified as nonessential by the government.[3] As the latter could not operate during the lockdowns, differential exposure to credit supply shocks played a critical role in shaping firms' decisions to preserve and hoard labor or destroy employment matches.

Furthermore, we examine the effects of credit supply shocks on firms' different groups of workers. By leveraging our dataset's matched employer–employee structure, we esti-

[2]Small firms are defined as those with fewer than 100 employees, while young firms are those with fewer than 10 years in operation.

[3]Unincorporated businesses are those registered under the Mexican tax code as "*persona física con actividad empresarial*"

mate the impact of an employer's credit supply shock exposure on worker inflows, outflows, and wage growth for different types of workers. Our findings reveal that exposure to negative credit supply shocks increased outflows of female workers, workers with short tenure, and workers 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 of credit supply shocks 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 growth during the pandemic's most critical year: 2020. Since most firms were exposed to negative credit supply shocks, we use our reduced-form estimates to calculate the counterfactual employment firms would have reached if their exposure to credit supply shocks had been at or above the level of the 20% least affected firms. Based on this partial equilibrium exercise, negative bank credit supply shocks can explain between 14% and 28% of the employment decrease among small firms in our sample between 2019 and 2020. To the extent that even the most capitalized banks also contracted their available credit, our results are conservative.

Our paper contributes mainly to the literature that examines the real effects of credit supply shocks during a recession. Prior research has focused on the global financial crisis (GFC), exploiting the heterogeneity in banks' exposure to mortgage back securities (Chodorow-Reich, 2013; Bentolila, Jansen, and Jimenez, 2017; Popov and Rocholl, 2018; Huber, 2018). Instead, our paper examines the COVID-19 recession, a critical period not extensively studied in this literature, primarily because of the complexity of identifying credit supply shocks amid the simultaneous economic shocks and numerous government interventions of the period, which, as mentioned above, were modest in Mexico.[4]

Moreover, we differentiate from the literature by studying the implications of credit shocks amidst a recession that did not originate in the financial sector. As banks' balance sheets were not the main source of the crisis, the behavior of bank credit to firms during

[4]One of the few studies covering the pandemic, albeit for a developed economy (the US), is Greenwald, Krainer, and Paul (2020), which focuses on how credit line drawdowns by large firms crowded out term lending for small ones, thus impacting the latter's investment. In another study, Granja et al. (2022) study the Paycheck Protection Program (PPP) in the US. However, as the PPP offered guaranteed, forgivable loans, its impact differs from that of traditional bank credit.

the pandemic differed from the substantial drop observed, for instance, during the GFC (Ivashina and Scharfstein, 2010). In contrast, we document that bank credit increased during the initial months of the pandemic, but only for large firms, as in the US (Li, Strahan, and Zhang, 2020), also particularly driven by draws on preexisting credit lines (Greenwald, Krainer, and Paul (2020), Chodorow-Reich et al. (2022)). Thus, despite the large contraction in economic activity, banks potentially had a larger margin in helping firms cushion the negative effects of the recession.

The richness of our data also allows us to analyze in detail the heterogeneity in the impacts of credit supply shocks. While other works have focused separately on the effects of such shocks on 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 matched employer–employee records. Moreover, this type of data allows us to decompose the changes in employment into inflows and outflows and study both the intensive and extensive margins of adjustment, whereas most papers in the literature focus only on a subset of these outcomes.

This work further sheds light on the real effects of bank credit shocks in a large developing country. In this same vein, Gutierrez, Jaume, and Tobal (2023) apply the methodology of Greenstone, Mas, and Nguyen (2020) to Mexico for the period 2010–2016 and find that credit supply shocks had an important effect on employment. However, while their analysis focuses on regular times, our study specifically examines the effect of credit supply shocks during the COVID-19 recession, a lesser-studied period overall. Moreover, our approach differs in several ways despite using a data-driven method to estimate credit supply shocks. For instance, we compute these shocks by controlling for variations in demand by firm industry and size rather than only by geography, as we argue that these dimensions are critical for firms' credit demand. In addition, our observation unit is the firm instead of the locality, allowing us to better control for unobserved factors and study the heterogeneity in the effect of credit supply shocks in depth. Thus, we construct measures of firm-level exposure to these shocks as in Bentolila, Jansen, and Jimenez (2017) and Alfaro, García-Santana, and Moral-Benito (2021).[5]

[5]In a related study, Morais et al. (2019) also use firm-level data for Mexico to study the in-

The paper is organized as follows: Section 2 describes the administrative data we use. Section 3 overviews firm, employment, and bank credit dynamics during the pandemic in Mexico. Section 4 outlines our empirical strategy. Section 5 presents the results, and Section 6 concludes.

2 Data

Our work 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). The following subsections explain these data sources and how we use them for our analysis.[6]

2.1 Mexico's Bank Commercial and Corporate Lending Regulatory Reports

Banking law in Mexico requires all credit issuing institutions, both traditional banks and nonbanking intermediaries, to provide detailed monthly information on the corporate and commercial loans they issue. In the rest of the paper, we refer to both types of institutions as *banks* for practical purposes. Mexico's CNBV collects and reviews this information and afterward compiles it into the R04c report (Comision Nacional Bancaria y de Valores, 2018-2021). Thus, this monthly dataset follows the universe of businesses' loans over time. We observe the IDs of the issuing bank and the borrowing firm, along with a comprehensive set of credit characteristics, including issuance date, interest rate, delinquency

ternational transmission of monetary policy. Furthermore, there is a long-standing literature on the banking channel that employs firm-level analyses, including Khwaja and Mian (2008), Berton et al. (2018), Bai, Carvalho, and Phillips (2018), Acharya et al. (2018), Chodorow-Reich (2013), Benmelech, Bergman, and Seru (2021), and Greenwald, Krainer, and Paul (2020), among others.

[6]The datasets used in this paper are confidential and were accessed through the EconLab at Banco de México (Banxico). EconLab collected and processed the data as part of its effort to promote evidence-based research and foster ties between Banxico's research staff and the academic community. Inquiries regarding the terms under which the data can be accessed should be directed to econlab@banxico.org.mx.

and default status, size of the line of credit, and outstanding amount, among others. The data also include information about the firm, such as the industry (5-digit NAICS), size category, and location. While this dataset is available from 2007, we use the data only from 2017 to 2021 since they are comparable over this period because of their common format.

Using the R04c reports, we construct a firm–bank–month panel aggregating each firm's outstanding loans with each bank at the end of each calendar month. We remove from our sample 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.2.1 offers a detailed description of this dataset, including the cleaning process we applied for our estimations.

2.2 Social Security Matched Employer–Employee Records

Mexico's social security law requires that private-sector employers register their wage-earning employees with the IMSS. IMSS registration provides employees access to several social benefits, including 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 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).[7]

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

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

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.2.2.[8]

2.3 Sample Selection

We define a *firm* by its tax ID, so we merge both 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. After a simple cleaning process (see A.2.2), in November 2018, 181,003 firms had a positive credit amount from one of 42 banks operating in the banking sector, which represents around 16% of the universe of formal firms in the Social Security dataset, but close to 30% of IMSS total employment.[9] While the subset of firms that do not have credit is interesting in itself, our identification strategy leverages relationship banking, and hence, we focus on the subset of firms with precrisis access to credit.

[8]IMSS 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.

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

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 Suppy 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				
		#				
Firms		181,003				
Banks		42				
Firm-Date		494,932				

Notes: Changes in credit and employment are computed using equation (3). Firm's exposure to credit supply shocks is defined in section 4.2. 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 withe less (more) than 10 years. See Appendix A.2.2 for additional details. Source: authors' calculations using IMSS and R04-C datasets.

3 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, emphasizing the small size of fiscal support programs to assist firms relative to the size of such programs in other countries. We also examine firm, employment, and credit dynamics throughout this challenging period, noting that these were heterogeneous across different firm age and size categories.

3.1 Government Interventions

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 former 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.[10]

Economic Support Measures

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

[10] For 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.

December 2020, direct economic support measures for individuals and firms amounted to 0.63% of Mexico's GDP (see A.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.[11]

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.[12]

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 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. Critically, we capture the overall impact of this measure in our metric of credit supply shocks.

3.2 Firm, Employment, and Credit Dynamics

Firm and Employment Dynamics

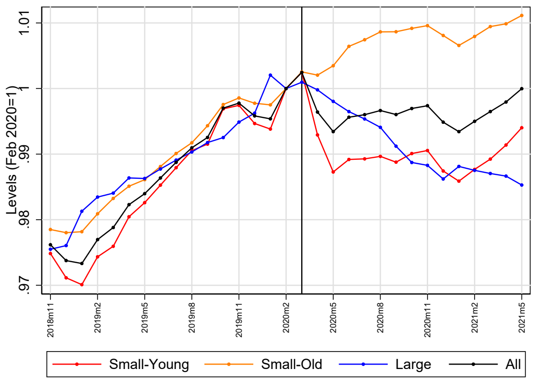
Figure 1a shows the dynamics of formal firms in Mexico. From March to May 2020, the initial months of the pandemic, 8,300 firms (0.9%) exited the formal market. However,

[11] This 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).

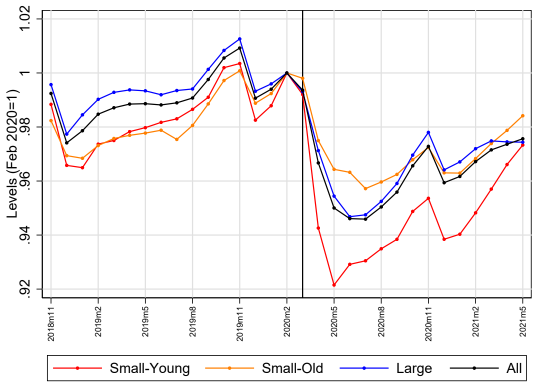
[12] Among these measures, the two with the most extensive reach were the repurchase window for government securities, established with the aim of ensuring liquidity access for financial institutions with government debt holdings, and the 15% reduction in commercial and development banks' central bank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

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 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.[13]

Figure 1: Firm and Employment Dynamics



(a) Number of Firms by Firm Size and Age



(b) Number of Workers by Firm Size and Age

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 1b 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 August 2020, particularly increasing during the first months of 2021.[14]

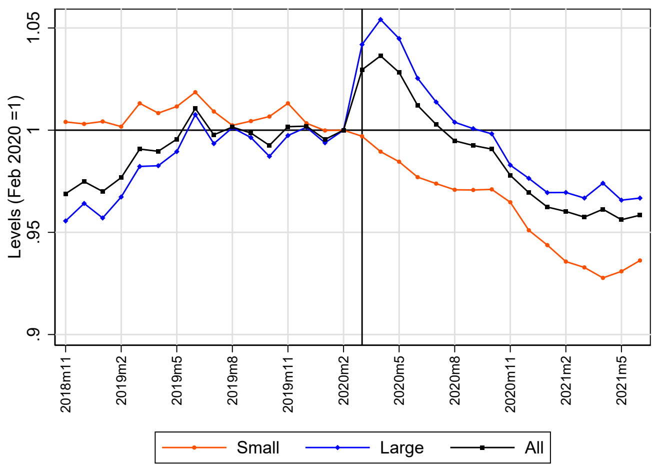
[13] In April 2021, Mexico's congress passed legislation regulating outsourcing, restricting subcontracting of core activities, and requiring registration of specialized service providers. This reform, promulgated in April and enacted in August, led many firms to reconfigure their business structures, 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.

[14] The rapid recovery in employment at small firms during 2021 can be partly explained by the

Credit Dynamics

Figure 2 illustrates the evolution of credit from commercial banks to nonfinancial private firms by firm size.[15] 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.

Figure 2: 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.2.1 for the exact definition. Source: authors' calculations using the R04-C dataset.

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outsourcing reform. Workers previously registered at large outsourcing firms were reallocated to small young firms.

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

4 Empirical Strategy

We are interested in providing causal estimates of the effect of credit supply on real outcomes, a challenging task given the endogenous nature of this variable. Nonetheless, instead of estimating the effects of credit supply *per se*, we take advantage of insights from the novel credit literature and focus on the effects of credit supply shocks arising from the banking sector, using a data-driven approach. We proceed in three steps. First, we compute and validate our bank credit supply shocks variable. Second, we build a metric of firm-level exposure to these shocks, provide evidence about its exogenous character, and show its relatedness to firms' credit utilization along several dimensions. Last, we use this exposure metric to study its real effects on firm outcomes. The following sections describe this approach.

4.1 Identifying Credit Supply Shocks

To estimate credit supply shocks, we adopt a strategy closely related to that proposed by Amiti and Weinstein (2018), which involves decomposing changes in credit into both their demand and supply components. This approach relies on our observing firms with multiple lenders (multibank firms), which allows us to distinguish variations in credit arising from the firm (demand) from those stemming from the bank (supply). Nonetheless, given that multibank 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 is more suitable for application with our data as it allows us to capture information from the entire set of firms rather than only from multibank ones.[16] In particular, we estimate

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

[16]Degryse et al. (2019) shows, for Belgium, that the credit supply shocks estimated using the Amiti and Weinstein (2018) multibank approach and proposed ILST estimators are highly correlated when both are estimated in the multibank sample. This implies that the ILST estimator properly controls for demand and allows us to include a larger set of firms, hence capturing valuable information that would otherwise be lost if we considered only multibank firms.

where we regress firm j's outstanding yearly credit growth rate with bank b between periods t and $t-1$ ($\Delta Credit_{j,b,t}$) on a set of industry–location–size–time fixed effects $\left(\gamma_{ils(j),t}\right)$ 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 shifts in risk tolerance, access to external funding, or deposit levels, among other factors. These bank-time fixed effects are the parameters of interest, as they capture idiosyncratic credit supply factors.

shifts in its risk tolerance, access to external funding, supply of deposits, among other factors."

Following Amiti and Weinstein (2018), we weight each observation by its economy-wide credit share and estimate (1) using weighted least squares (WLS) (Tielens and Hove, 2017). Moreover, we compute the growth rate using the midpoint definition (see equation (3)), common in the firm dynamics literature (Davis, Haltiwanger, and Schuh, 1998), which is symmetric to expansions and contractions but, critically, accommodates entry and exit, thus allowing us to fully match aggregate growth. Under this estimation approach, the bank–time fixed effects possess a straightforward interpretation as the corresponding total percentage change in credit attributable to the specific conditions of each bank, net of demand factors. Since we consider yearly changes from 2018 to 2021, with November as a base, our panel encompasses three periods.[17] 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. To achieve this, we consider a granular set of demand controls, grouping firms by 3-digit industry (92 categories), state (32 entities), and size (firms with more than 100 employees and all others). As a practical

[17]In this case, 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 and account for *new* bank–firm relationships arising from banks or firms that enter into the bank system, which cannot be included when the standard growth rate is used. We consider yearly changes because, as shown in Figures 1b and 2, both formal employment and credit have a strong seasonal component. In particular, we use November as base when computing annual changes to avoid capturing idiosyncratic fluctuations in employment caused by end-of-year layoffs and rehires, as firms tend to lay off workers in December and rehire them in January, but our results are robust to altering this choice (see A.8.4). In 2022, the format of the R04c credit report changed, which prevents us from extending the panel beyond 2021.

example, we assume that firms with more than 100 employees in the food manufacturing industry (NAICS code 311) located in the state of Nuevo León have similar credit demand in a given period. This assumption is reasonable to the extent that credit demand is driven by, for example, productivity or product demand shocks common to firms in these industry–location–size groups. Moreover, in Appendix A.6, we show these bank–time fixed effects strongly correlate with those obtained by means of the Amiti and Weinstein (2018) multifirm approach.[18]

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. They 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 this approach.[19]

The estimated credit supply shocks capture variations in credit attributable to changes in each bank, such as shifts in their internal cost of funding. However, our data-driven approach is deliberately agnostic regarding the specific causes of the variation. We consider this an advantage in a context such as the COVID-19 pandemic period, characterized as it was by various concurrent shocks. The downside, of course, is that we cannot pinpoint the precise mechanisms generating these credit supply shocks. For instance, the considerable variation observed in our estimated credit supply shocks partly reflects the pandemic's differential effect on bank portfolios, as specific sectors and locations were affected particularly acutely during the recession, significantly elevating their credit risks, and on banks' liquidity cushion in the months preceding the pandemic, with its corre-

[18] Having more disaggregated ILST controls is not necessarily better. If the groups are defined too narrowly, fewer firms may be in each category, reducing the sample size and thus weakening statistical power.

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

sponding effect on their funding sources (Banco de México, 2020). Nonetheless, despite our methodology's limitation regarding the causes of these shocks, we formally provide evidence that they capture supply-side factors.

Table 2: Bank Performance and Credit Supply Shocks

	Credit Supply Shocks			
	(1)	(2)	(3)	(4)
ROE	0.3498			
	(0.3064)			
ROA		1.8573		
		(2.4543)		
Deposit growth			0.05371***	
			(0.1747)	
Equity growth			-1.3461*	-.1423
			(0.7262)	(0.2631)
Interbank liabilities growth			0.8655	0.3937**
			(0.7331)	(0.1841)
Observations	167	167	111	167
Time FE	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.

First, as the pandemic largely disrupted the entire banking system, affecting banks' profitability and funding sources (see Appendix A.3), we use public banks' balance sheet information from the CNBV to examine whether our estimated credit supply shocks correlate with either changes in the banks' funding sources or performance. Regarding banks' funding, we consider three variables affecting its availability: 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 using standard measures such as return on assets (ROA) and return on equity (ROE) over the prior 12 months.[20]

Table 2 presents the results of a set of regressions in which the dependent variable is our estimated credit supply shocks variable ($\hat{\delta_{b,t}}$) and the explanatory variables are the aforementioned bank-specific funding and performance variables, with bank and time

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

fixed effects. 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 among nondepository 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 both deposit and interbank liability growth is consistent with supply-driven expansions in credit occurring when banks have higher availability of funds. Conversely, the negative correlation with equity growth suggests that equity injections concur with periods of negative credit supply shocks.[21]

Second, we build an aggregate index using our estimated credit supply shocks, which we refer to as *bank lending policies* (Berton et al., 2018), by computing a weighted average of them (the weights are equal to each bank's credit market share in the previous year). We then compare this index to both the change in aggregate credit and a diffusion index that captures the evolution of self-reported bank lending standards. The latter uses data from the Bank Lending Survey (Encuesta sobre Condiciones Generales y Estándares en el Mercado de Crédito Bancario or EnBan) conducted by Banxico. This survey collects qualitative information from executives responsible for credit-granting policies in commercial banks operating in Mexico about the evolution of credit approval standards in the market. We follow Banxico's methodology to create an aggregate measure of quarterly changes in lending standards using a weighted average of bank's responses to this question: "Over the past three months, how have your bank's credit standards, as applied to the approval of loans or credit lines to firms, changed?" We then calculate cumulative changes in the aggregate diffusion index over the prior four quarters.[22]

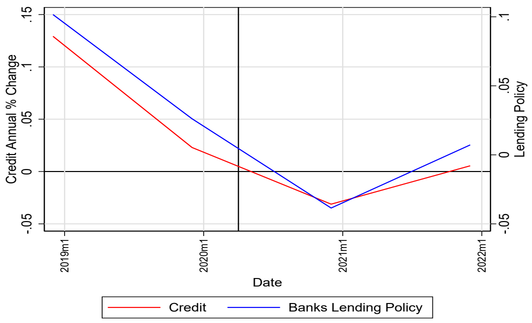
As shown in Figure 3a, there is a strong correlation between our bank lending policy

[21]Degryse et al. (2019) observe, using monthly data for Belgium from 2009 to 2012, similar correlations, exhibiting the same sign, between credit supply shocks, equity growth, and interbank lending.

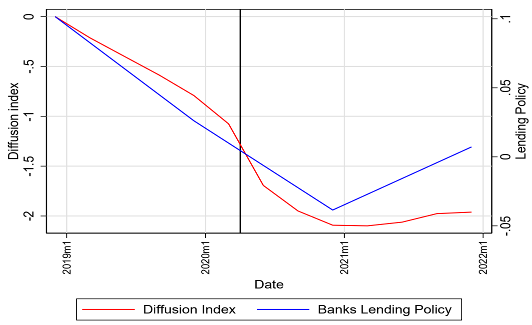
[22]Banxico calculates this "diffusion index" as a weighted average of the responses of each bank executive. There are five potential answers to this question, each coded with a numerical value ranging from -1 to 1 as follows: "tightened considerably" (-1), "tightened moderately" (-.5), "unchanged" (0), "eased moderately" (.5), and "eased considerably" (1). The weights correspond to the previous year's market share.

index and the annual growth rate of aggregate credit, which suggests that supply factors played a significant role in explaining credit behavior during our study period. Similarly, Figure 3b shows that the estimated credit supply shocks and the diffusion index from EnBan follow similar trends. This finding is consistent with the interpretation that our estimated credit supply shocks accurately capture the dynamics of banks' credit approval standards. Interestingly, credit conditions began to deteriorate before the onset of the pandemic but tightened during its peak and started to ease again in 2021.

Figure 3: 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 ENBAN datasets.

4.2 Firm's Exposure to Credit Supply Shocks

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 allows to study the heterogeneous impact of these shocks across firms with different characteristics. To estimate the impact of bank credit supply shocks on firm-level outcomes, we construct a metric of firm exposure to these shocks as

$$Z_{j,t} = \sum_{b} w_{j,b,2018} \times \widehat{\delta_{b,t}} \qquad t > 2018 \tag{2}$$

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.[23] As expected, most firms were exposed to negative credit supply shocks (see Table1).

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.

[23]By 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.

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 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.[24]

Table 3 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

[24]Although 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 atomistic nature of firms, as shown by 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.

defined in equation (3). Table 4 presents the results of this exercise.

An exposure to a negative credit supply shock decreases 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 3: 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 (2). *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. Source: authors' calculations using IMSS and R04-C datasets.

Table 4: 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*	0.3876*	0.4380*	0.2223*	0.0068
	(0.2683)	(0.2238)	(0.2624)	(0.1167)	(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 (2). Changes in credit, credit lines and loans are computed as in equation (3). "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 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.

4.3 Real Effects of Credit Supply Shocks

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 (3).

$$\Delta Y_{j,t} = \frac{(Y_{j,t} - Y_{j,t-1})}{0.5(Y_{j,t} + Y_{j,t-1})} = \frac{(Inflows_{j,t,t-1} - Outflows_{j,t,t-1})}{0.5(Y_{j,t} + Y_{j,t-1})} \tag{3}$$

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.[25]

[25] We cannot distinguish between layoffs and voluntary separations; thus, when examining job

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 (4). 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 5.1.[26]

$$Y_{j,t} = \beta_0 + \beta_1 Z_{j,t} + \omega_j + \psi_{ilsa(j),t} + \epsilon_{j,t} \tag{4}$$

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, 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).[27]

outflows, we focus on all terminated matches.

[26]We 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).

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

5 Results

Table 5 presents our main results. During the COVID-19 recession, credit supply shocks affected firms' annual formal employment growth (column (1)) and their yearly 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.

Table 5: 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***	0.0281	0.0074	-0.0207***	-0.0152**
	(0.0341)	(0.0190)	(0.0190)	(0.0065)	(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 (3). "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 (2). 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. These results suggest that, during the COVID-19 recession, firms exposed to negative credit shocks were less likely to survive and had higher employment outflows, albeit without decreasing 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 (4) for each of them.[28]

Panel A of Table 6 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.

[28]Incorporating 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 6: 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)
			Panel A: 2019-2020		
Credit Supply Shock	0.1359***	0.0303*	-0.0104	-0.0407***	-0.0343***
	(0.0467)	(0.0165)	(0.0067)	(0.0124)	(0.0118)
Mean of $Y_{j,t}$	-0.0679	-0.0361	0.2477	0.2838	0.0386
Observations	316406	281578	281578	281578	316406
			Panel B: 2020-2021		
Credit Supply Shock	0.0901	0.0348	0.0128	-0.0221**	-0.0088
	(0.0574)	(0.0258)	(0.0248)	(0.0109)	(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 (3). “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 (2). 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 6). 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.[29] Alfaro, García-Santana, and Moral-Benito (2021) also document sizable real effects of credit supply shocks for Spanish firms 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

[29]The 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).

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.

5.1 Effect Heterogeneity

Prior literature documents heterogeneous effects of credit supply shocks across firms by sector, size, or age (Chodorow-Reich, 2013; Duygan-Bump, Levkov, and Montoriol-Garriga, 2015; Siemer, 2019). An important driver of this heterogeneity in the effects of credit supply shocks is the availability of other sources of financing to firms (Berg, 2018). In this section, we examine the cross-sectional heterogeneity in the effects of credit supply shocks during the COVID-19 recession. To do so, 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, incorporation status, and sector designation as essential or nonessential.[30] Namely, for each $H \in \{Age \times Size, \dots\}$, we estimate the following equation:

$$Y_{j,t} = \beta_0 + \sum_{h \in H} \beta_h Z_{j,t} \times D_h + \omega_j + \psi_{ilsah(j),t} + \epsilon_{j,t} \tag{5}$$

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 ($\psi_{ilsah(j),t}$) to control for any time-varying effect at that level (ILSAHT fixed effects).

Age and Size

Table 7 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. These groups broadly proxy the degree of firms' financial constraints.[31] Columns

[30] In Section A.7, we further explore heterogeneity along other dimensions, such as firms' degree of financial dependence (Rajan and Zingales, 1998) and industry of operation.

[31] We do not distinguish large firms by age given the scant number of large young firms.

(1) and (2) show that credit supply shocks affected employment at 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 in recessions, we find that the effects of credit supply shocks were largest among this set of firms. This finding is also in line with the findings of prior work focusing on the great recession (Chodorow-Reich, 2013; Siemer, 2019).

Table 7: 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×					
Small×$Young$	0.1581***	0.0405	0.0126	-0.0279***	-0.0246***
	(0.0521)	(0.0302)	(0.0264)	(0.0062)	(0.0070)
Small×Old	0.0309	0.0147	0.0005	-0.0142	-0.0038
	(0.0238)	(0.0125)	(0.0126)	(0.0106)	(0.0067)
Large	-0.0415	-0.0050	0.0130	0.0179	0.0180
	(0.0758)	(0.0579)	(0.0198)	(0.0468)	(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 (3). "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 (2). Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those withe less (more) than 10 years. See Appendix A.2.2 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.

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 8 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.

Table 8: 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×					
Unincorporated	0.0891***	0.0302**	0.0124*	-0.0178***	-0.0068
	(0.0184)	(0.0120)	(0.0074)	(0.0069)	(0.0060)
Incorporated	0.0247	0.0066	-0.0156	-0.0222**	-0.0027
	(0.0254)	(0.0225)	(0.0191)	(0.0108)	(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 (3). 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 (2). 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.

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 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 pandic credit supply shocks differently affected sectors deemed essential and nonessential by the government.[32] 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 9 shows the results.

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.

5.2 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, such as severance expenses, 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.

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

Table 9: 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×					
Non-essential	0.1651***	0.0423**	-0.0087	-0.0510***	-0.0384**
	(0.0605)	(0.0196)	(0.0097)	(0.0134)	(0.0177)
Essential	0.0730	0.0078	-0.0144	-0.0222**	-0.0227***
	(0.0493)	(0.0213)	(0.0140)	(0.0113)	(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 (3). "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 (2). 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.

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 gender, 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.[33]

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

[33]Using 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 underreport wages to partially evade payroll taxes. Nonetheless, if credit supply shocks do not differentially affect the incentive to underreport, the analysis is informative about the impact on wage growth.

entrant firms hire from particular categories (low-tenure workers). Thus, for the sample of continuing firms, we estimate equation (4) but define the dependent variable $\Delta Y^g_{j,t}$ as either employment or wage bill growth for worker group g, where $g \in \{$*male*, *female*, *high tenure*, *low tenure*, *permanent contracts*, *temporary contracts*$\}$. Since the dependent variables are growth rates, the coefficients are also interpreted as elasticities to credit supply shocks, though for different categories of workers.[34]

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 10 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 she-cession in Mexico (Hoehn-Velasco et al., 2022; Viollaz et al., 2023; Rangel, Llamosas, and Hutchinson, 2024), 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

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

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 10: 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)
		Men		
Credit Supply Shock×				
Small×$Young$	0.0269	0.0062	-0.0207	-0.0117*
	(0.0415)	(0.0268)	(0.0175)	(0.0067)
Small×Old	0.0170	0.0014	-0.0156	-0.0163***
	(0.0166)	(0.0193)	(0.0126)	(0.0050)
Large	0.0098	0.0161	0.0063	-0.0164
	(0.0571)	(0.0181)	(0.0465)	(0.0103)
Mean of $Y_{j,t}$	-0.0228	0.2669	0.2898	0.0859
Observations	421711	421711	421711	421711
		Women		
Credit Supply Shock×				
Small×$Young$	0.0625***	0.0237	-0.0388***	-0.0052
	(0.0228)	(0.0228)	(0.0139)	(0.0102)
Small×Old	0.0275	0.0022	-0.0253**	-0.0046
	(0.0183)	(0.0146)	(0.0112)	(0.0067)
Large	-0.0870	-0.0177	0.0693	0.0116
	(0.0701)	(0.0296)	(0.0561)	(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 (3). "Stayers" refers to workers who continued in the firm between t and $t-1$. "Credit Supply Shock" is defined in equation (2). Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those withe less (more) than 10 years. See Appendix A.2.2 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.

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 (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 11 and 12 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.[35] Table 11 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 12). 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. While this does not imply that these firms did not engage in this behavior, their large alternative financing sources mute

[35] We 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 3.

the impact of bank credit supply shocks.

Consistent with both low-tenure workers and those with temporary contracts absorbing a larger portion of the employment volatility 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 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 worker type. 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 11: Real Effects of Credit Supply Shocks on Employees by Firm Tenure (2019–2021)

	Continuers			
	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Δ Wages (Stayers)
	(1)	(2)	(3)	(4)
	High tenure			
Credit Supply Shock×				
Small×$Young$	-0.0007	0.0017	0.0025	-0.0079***
	(0.0095)	(0.0042)	(0.0071)	(0.0022)
Small×Old	0.0141	0.0051	-0.0090	-0.0164**
	(0.0096)	(0.0052)	(0.0099)	(0.0064)
Large	-0.0382	0.0109	0.0491	-0.0198**
	(0.0632)	(0.0132)	(0.0547)	(0.0095)
Mean of $Y_{j,t}$	-0.0988	0.0215	0.1203	0.0705
Observations	421711	421711	421711	421711
	Low tenure			
Credit Supply Shock×				
Small×$Young$	0.0637*	0.0226	-0.0411***	-0.0085
	(0.0354)	(0.0312)	(0.0100)	(0.0101)
Small×Old	0.0250	0.0201	-0.0049	-0.0128***
	(0.0225)	(0.0206)	(0.0151)	(0.0034)
Large	0.0022	0.0221	0.0199	-0.0091
	(0.0632)	(0.0245)	(0.0465)	(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 (3). "Stayers" refers to workers who continue in the firm between t and $t-1$. "Credit Supply Shock" is defined in equation (2). "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 withe less (more) than 10 years. See Appendix A.2.2 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 12: 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)
		Permanent contracts		
Credit Supply Shock×				
Small×$Young$	0.0314	0.0215	-0.0099	-0.0088
	(0.0381)	(0.0273)	(0.0144)	(0.0066)
Small×Old	0.0068	-0.0015	-0.0083	-0.0172***
	(0.0154)	(0.0136)	(0.0076)	(0.0046)
Large	0.0263	0.0026	-0.0237	-0.0184
	(0.0678)	(0.0205)	(0.0599)	(0.0185)
Mean of $Y_{j,t}$	-0.0550	0.1167	0.1718	0.0567
Observations	421711	421711	421711	421711
		Temporary contracts		
Credit Supply Shock×				
Small×$Young$	0.0603**	0.0317	-0.0285***	-0.0072
	(0.0242)	(0.0250)	(0.0061)	(0.0067)
Small×Old	0.0334**	0.0126	-0.0207	-0.0082
	(0.0161)	(0.0169)	(0.0140)	(0.0081)
Large	-0.1320	-0.0779**	0.0540	-0.0367**
	(0.0812)	(0.0321)	(0.0558)	(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 (3). "Stayers" refers to workers who continue in the firm between t and $t-1$. "Credit Supply Shock" is defined in equation (2). Small (Large) firms are those with less (more) than 100 workers in the previous 12 months. Young (Old) firms are those withe less (more) than 10 years. See Appendix A.2.2 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.

5.3 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 achieve this, we follow Chodorow-Reich (2013) and compare the employment losses observed across the firms in our sample with a counterfactual where all firms were exposed to banks in the top percentiles of the credit supply shock distribution during the aforementioned period. Specifically, we compute a counterfactual employment growth rate assuming that firms had credit supply exposure at or above the 90th, 85th, or 80th percentiles for this variable as follows:[36]

$$\Delta\hat{E}_{j,t}(\tau) = \Delta\hat{E}_{j,t} + \hat{\beta}\,[\max\{0, Z(\tau) - Z_{j,t}\}] \tag{6}$$

In equation (6), $Z(\tau)$ denotes the τ percentile for the variable $Z_{j,t}$, $\Delta\hat{E}_{j,t}$ the predicted value of employment growth, 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 (4). Specifically, $\hat{\beta}$ corresponds to 0.19 for firms categorized as small and young and 0.08 for those categorized as small and old.[37] Computing equation (7) allows us to estimate the fraction of employment losses that could be attributed to having an exposure below the percentile τ:

$$\frac{\sum(\hat{E}_{j,t} - \hat{E_{j,t}(\tau)})}{\sum(E_{j,t} - E_{j,t-1})} \tag{7}$$

Table 13 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 14.2% and 27.6% of the employment drop in small and medium firms and between 4.5% and 8.7% of the total decrease in employment. The

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

[37] We obtain these values by estimating equation (4) for the period 2019–2020.

exact values depend on which τ we pick to define the reference level of exposure to credit supply shocks. To the extent that even the banks least affected by the pandemic tightened their credit availability, our results are likely 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 might have hired workers laid off by firms tied to banks that severely tightened credit standards.

Table 13: Share of Employment Losses Attributable to Tightening of Credit Standards (2019–2020)

τ	Small Firms	All Firms
90^{th}percentile	27.56%	8.70%
85^{th}percentile	18.40%	5.81%
80^{th}percentile	14.18%	4.48%

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.

6 Conclusion

We document the real effects of credit supply shocks during the COVID-19 recession in Mexico. Our strategy consists of first identifying time-varying credit supply shocks at the bank level and then constructing metrics of exposure to these shocks for each firm. We use the credit registry, which includes the universe of business loans, to achieve this. To study the real effects of credit supply shocks, we match our exposure metrics with the employment data of each firm, utilizing the universe of formal private sector workers.

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 small young firms and unincorporated businesses; moreover, the credit supply shocks more markedly shaped the employment responses of firms that could not operate during the lockdowns. A partial equilibrium exercise indicates that negative credit supply shocks accounted for approximately a quarter 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 the significant impact of credit supply shocks on employment predominantly affected low-tenure workers and those on temporary contracts. Female workers were also notably impacted, likely due to their over-representation in these types of contracts and tenure levels. Our 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.

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. We also show that firms' exposure to credit supply shocks is ex ante systematically uncorrelated with the firms' characteristics. Moreover, our findings are robust across various specifications and when we consider economic sectors at different levels of granularity.

Looking ahead, an important area of investigation is the medium-term effects of heterogeneous exposure to credit supply shocks on firm productivity. This task should include an examination of the impacts on firms that faced negative credit supply shocks, which may have been unable to retain potentially valuable matches, in comparison to that on those with better access to credit, which could retain workers or hire those laid off. This aspect is relevant for understanding the broader implications of credit during economic crises and holds the potential to shape future policy decisions and research directions.

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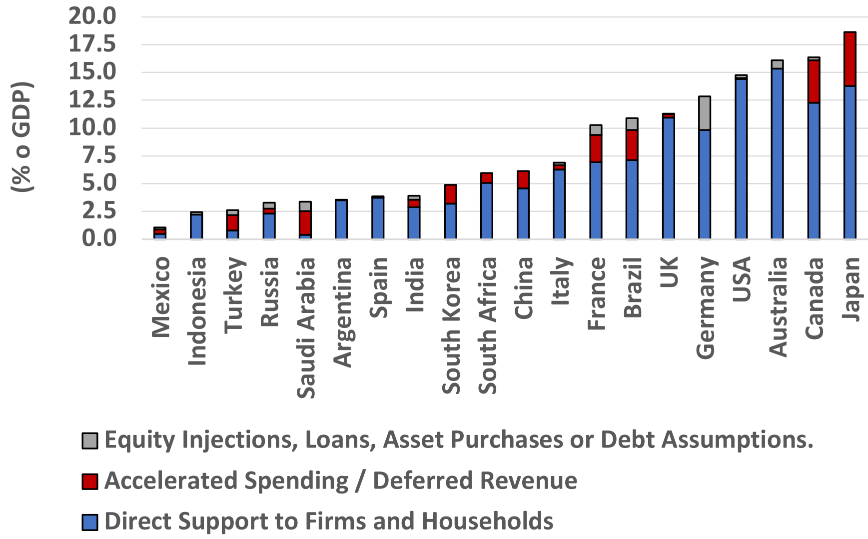
A Appendix

A.1 Policy Responses to COVID-19

Figure 4 displays the key measures announced or enacted by G20 economies in response to the COVID-19 pandemic as of December 31, 2020, excluding health expenditures. Mexico's relatively modest support to firms and individuals stands out as an outlier.

Figure 4: 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.

A.2 Data Appendix

A.2.1 Treatment of Credit Data

Cleaning Process. 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.[38]. Finally, we remove banks with less than 100 active loans (typically investment institutions) during the studied period. Banks with a small 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 14 summarize them.[39]

[38] This 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.

[39] If 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

Table 14: Firm Size Definitions Used in the Credit Registry

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

Source: R04-C report instruction manual.

A.2.2 Treatment of the Employment Dataset

Cleaning Process. 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).

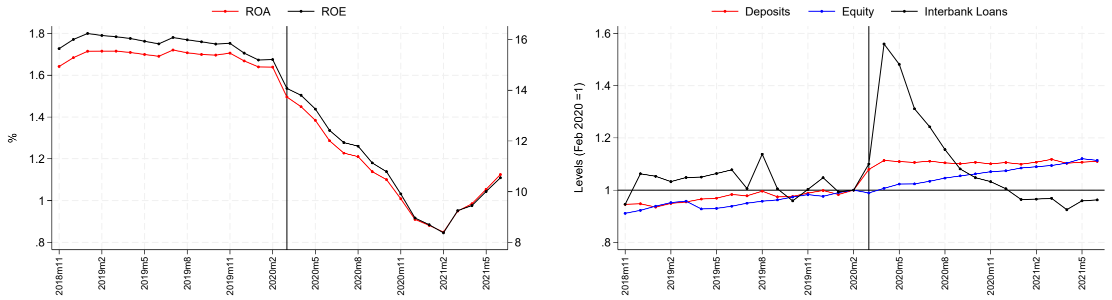
Firm Characteristics. Consistent with the firm dynamics literature, we define firm size in the IMSS dataset by classifying firms according to their average employment of the previous 12 months. We use this value to divide them into "large" (more than 100 employees) and "small" (less than 100 employees). 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.3 Bank 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 5 was constructed using that public information and covers the entire sector.

to the maximum scores listed in the 'Max Score' column.

Figure 5: Bank Performance and Funding Sources.



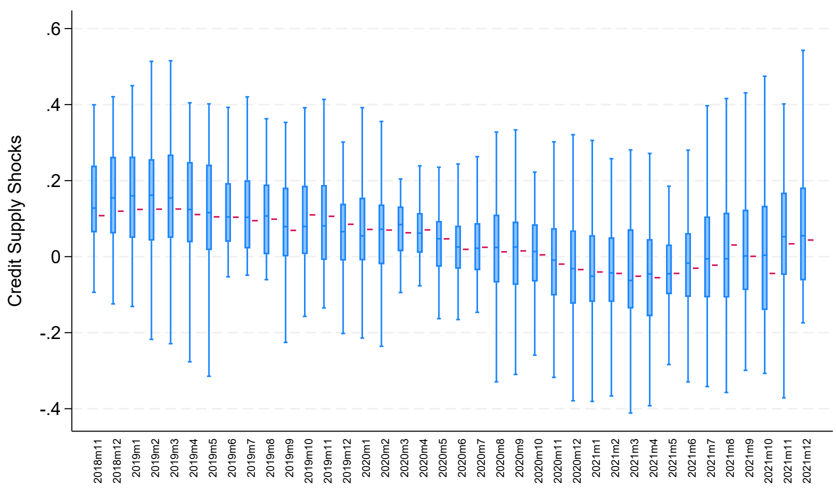
(a) Bank Performance: Return on Assets (left axis) and Return on Equity (right axis). (b) Bank Funding Sources: Deposits, Equity, and Inter-bank Loans.

Source: authors' calculations using CNBV public reports.

A.4 Distribution of Credit Supply Shocks at the Bank Level

Figure 6 presents the distribution of credit supply shocks at the bank level, estimated using Equation 1. While our main estimates use yearly changes, considering the month of November, here we present credit supply shocks considering year-to-year changes for every month between November 2018 and December 2021.

Figure 6: 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). Source: authors' calculations using the R04-C dataset.

A.5 Decomposition of Annual Changes in Aggregate Credit

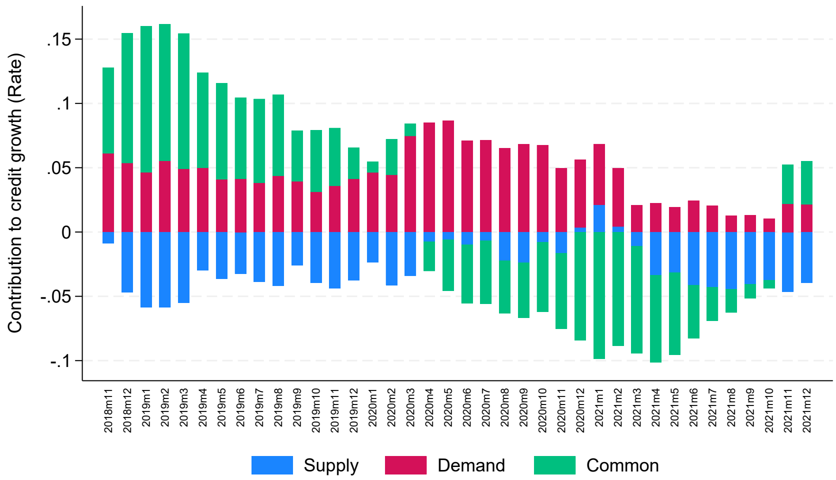
We can use our estimators from equation (1), namely $\gamma_{il\hat{s(j)},t}$ and $\hat{\delta_{b,t}}$, with appropriate weights, to see the contribution of both supply and demand factors to *aggregate* credit growth. Yet, because of collinearity issues, one cannot recover all the fixed effects of the estimation, so a normalization is needed. Thus, following Amiti and Weinstein (2018), we normalize the estimated coefficients to their corresponding median, $\gamma_{il\bar{s(j)},t}$ and $\bar{\delta_{b,t}}$, such that $\gamma_{il\tilde{s(j)},t} = \gamma_{il\hat{s(j)},t} - \gamma_{il\bar{s(j)},t}$ and $\tilde{\delta_{b,t}} = \hat{\delta_{b,t}}$ -$\bar{\delta_{b,t}}$, and aggregate the elements of the normalized version of (1) using $\omega_{j,b,t}$ weights as follows,[40]

$$\sum_{j,b,t} \omega_{j,b,t} \Delta_k Credit_{j,b,t} = \sum_{j,b,t} \omega_{j,b,t} \gamma_{il\tilde{s(j)},t} + \sum_{j,b,t} \omega_{j,b,t} \tilde{\delta_{b,t}} + (\gamma_{il\bar{s(j)},t} + \bar{\delta_{b,t}}) \qquad (8)$$

where the first and second elements correspond to the demand and supply components, respectively, whereas the last term, $(\gamma_{il\bar{s(j)},t} + \bar{\delta_{b,t}})$ refers to the "common" component, that is, aggregate shocks that cannot be separately attributted no neither supply nor demand factors. Figure 7 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.

[40]These 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 7: Annual Credit Changes: Supply, Demand, and Common Factors

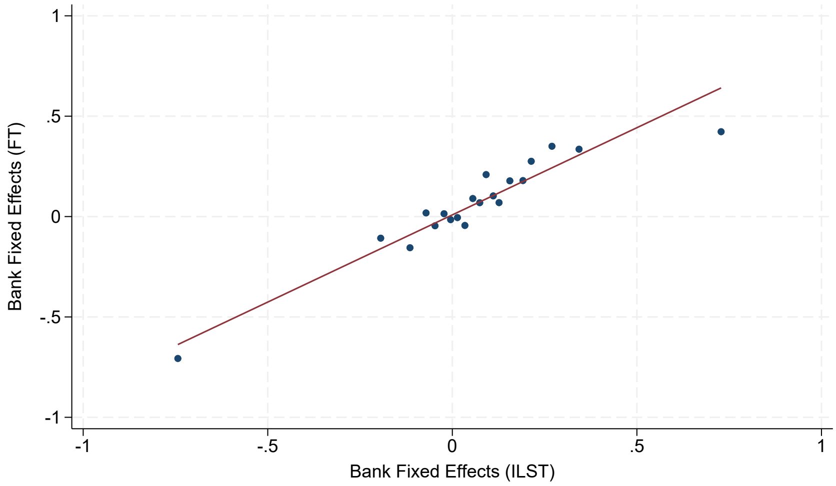


Notes: The decomposition exercise is based on estimating equation (8). Source: authors' calculations using the R04-C dataset.

A.6 Comparison Between Our Baseline Credit Supply Shocks (ILST) and Amiti-Weinstein's Specification (FT)

Figure 8 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 8: 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.7 Additional Results: Heterogeneous Effects of Credit Supply Shocks

Financial Dependence

Table 15 presents the impacts of credit supply shocks on firms segmented by their reliance on external finance as defined in Rajan and Zingales (1998). Using the sector classification from Duygan-Bump, Levkov, and Montoriol-Garriga (2015) at the SIC level and a dictionary that maps SIC with NAICS codes, we identify firms in sectors with above-median levels of external financial dependence as ”High Financial Dependence” ones, whereas those below it are classified oppositely. Our findings indicate that firms in sectors with a greater dependence on external finance exhibit a more pronounced response to credit supply shocks. This observation aligns with theoretical expectations, given these sectors' heightened reliance on external funding sources.

Sector

Table 16 reveals that the impact of credit supply shocks on employment is larger for the Service and Commerce sectors, with both economically and statistically significant

Table 15: 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×					
Low Fin. Dependence	0.0716**	0.0146	-0.0044	-0.0190	-0.0121
	(0.0291)	(0.0239)	(0.0241)	(0.0116)	(0.0076)
High Fin. Dependence	0.1368***	0.0424***	0.0196	-0.0227***	-0.0193***
	(0.0418)	(0.0156)	(0.0148)	(0.0056)	(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 (3). 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 (2). 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.

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.

Table 16: 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×					
Construction	0.1345	0.0198	-0.0038	-0.0236	-0.0299*
	(0.1112)	(0.0391)	(0.0219)	(0.0269)	(0.0169)
Manufacture	0.0274	-0.0165	-0.0115	0.0050	-0.0011
	(0.0639)	(0.0517)	(0.0347)	(0.0284)	(0.0173)
Commerce	0.1434***	0.0458***	0.0107	-0.0351***	-0.0271**
	(0.0516)	(0.0171)	(0.0144)	(0.0110)	(0.0134)
Services	0.0896**	0.0276	0.0104	-0.0172	-0.0096
	(0.0440)	(0.0311)	(0.0293)	(0.0142)	(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 (3). 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 (2). 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 17 replicates the results of Table 5, but when we controls for 5 as opposed to 3-digit industries in equation (4). As can be seen, this specification yields similar results to the main ones, with slightly different magnitudes.

Table 17: The Real Effects of Credit Supply Shocks (5D NAICS classification)

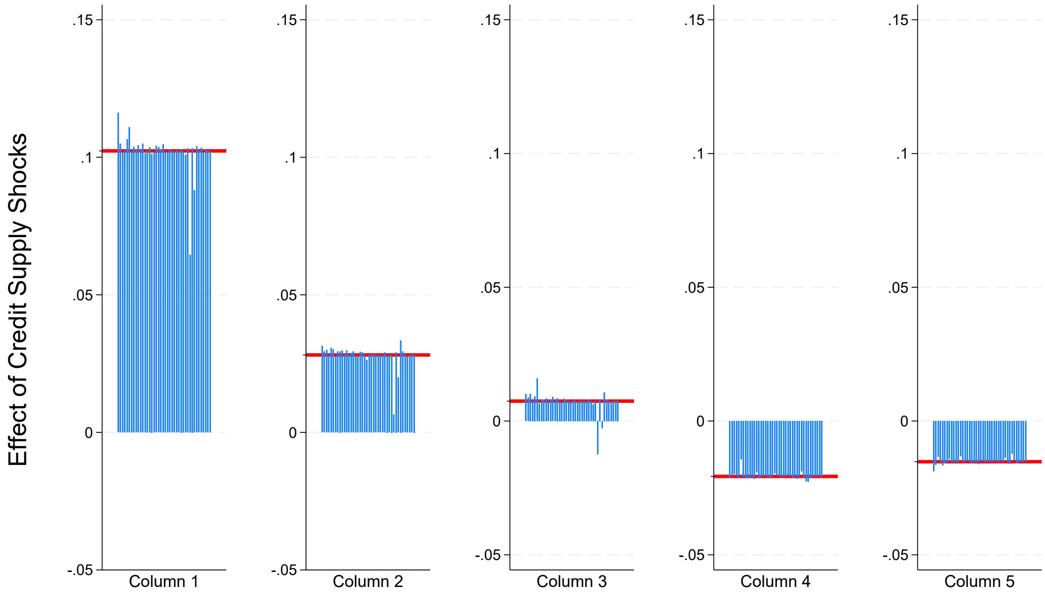
	All	Continuers			All
	Δ Employment	Δ Employment	Δ Employment (Inflows)	Δ Employment (Outflows)	Exit
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock	0.0946***	0.0371**	0.0127	-0.0244***	-0.0115
	(0.0273)	(0.0159)	(0.0173)	(0.0069)	(0.0073)
Mean of $Y_{j,t}$	-0.0565	-0.0145	0.2626	0.2771	0.0508
Observations	459577	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 (3). 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 (2). 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 9 replicates the estimates in Table 5, 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 9: Sensitivity of Main Results to Removing Banks



Notes: The bar in each graph represents the estimates of the corresponding column in Table 5 when one bank is removed. The red horizontal line is the estimate presented in Table 5. 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 Banxico (2021).[41] While our baseline sample already removes potential outsourcing providers (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 18, removing these firms delivers results similar to the baseline ones, although with higher statistical precision, particularly for the recovery period.

[41] If 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 outsourcing provider, and all transferred workers are considered outsourced.

Table 18: 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)
			Panel A: 2019-2020		
Credit Supply Shock	0.1281***	0.0359**	-0.0094	-0.0452***	-0.0278***
	(0.0422)	(0.0174)	(0.0059)	(0.0129)	(0.0093)
Mean of $Y_{j,t}$	-0.0781	-0.0413	0.2371	0.2784	0.0415
Observations	287876	254434	254434	254434	287876
			Panel B: 2020-2021		
Credit Supply Shock	0.0900**	0.0294*	0.0118**	-0.0176	-0.0105
	(0.0414)	(0.0155)	(0.0053)	(0.0115)	(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 (3). 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 (2). 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.8.4 Reference Month

Table 19 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 19: 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***	0.0321	0.0137	-0.0184**	-0.0103
	(0.0356)	(0.0235)	(0.0193)	(0.0094)	(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 (3). 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 (2). 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.