The Real Effects of Credit During the COVID-19 Pandemic*

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Abstract

We study the real effects of credit during the Covid-19 pandemic in Mexico. To this end, we merge administrative micro-level data on the universe of bank-firm matched loans to employer-employee fmatched data covering all formal employment. We construct a time-varying, firm-level credit supply shock by decomposing changes in credit into demand and supply components. Supply components are attributable to bank idiosyncratic shocks unrelated to firm characteristics. We find that credit supply shocks positively influence employment growth, while inversely affect the probability of exit during the pandemic. An increase of one standard deviation in a firm credit supply shock increases yearly employment growth by 1.5 pp and reduces the probability of exit by 0.25 pp The effects of credit were concentrated on smaller firms, startups, unincorporated businesses, and those in non-essential sectors, as determined by the government during the COVID-19 lockdown.

JEL Classification: E24, E44, G01, G20 **Keywords:** Covid-19, bank credit, economic activity

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1 Introduction

During the COVID-19 pandemic, firms across the globe experienced various combinations of negative supply and demand shocks. Government lock-downs and disruptions to global trade affected firms' ability to operate, while contagion risks and income shocks impacted demand. As a result, labor markets worldwide experienced major disruptions. In this challenging economic environment, the availability of bank credit likely played a role in shaping firms' responses to the pandemic. Specifically, access to bank credit may have influenced firms' ability to continue operating and hence their demand for labor.

In this paper, we study the effects of credit on employment, wages, and firm survival during the pandemic in Mexico. Understanding the real impact of credit during this recession provides valuable insights for designing effective public policies that aim to provide liquidity to banks and firms during crises. We focus on Mexico because of three particular features of this country. First, there was no substantial government support for businesses during this time, which allows us to better isolate the impact of credit. Second, like many developing countries, the banking sector is the main source of financing for most firms[1]. And lastly, but not least, the availability of both employer-employee and bank-firm data.

To study the real effects of credit during the pandemic, we employ administrative records to construct a panel on employment and credit for the universe of formal firms in Mexico. Identifying the effect of credit on real outcomes is challenging due to the endogenous connection between firm performance and changes in outstanding credit. To overcome this challenge, we first estimate bank-level supply shocks following the methodology of Degryse et al. (2019), a variation of that of Amiti and Weinstein (2018) that allows us to decompose changes in the outstanding credit of each firm into demand and supply components. The supply components are meant to capture changes in credit attributable to unique conditions within individual banks. Then, we aggregate these bank-level supply shocks, leveraging pre-existing bank-firm relationships, and create time-varying firm-level supply shocks. Finally, we use these firm-level supply shocks to

[1] According to the National Survey of Finances (ENAFIN), 67% of firms that asked for financing did it through Banks https://www.inegi.org.mx/contenidos/programas/enafin/2021/doc/Presentacion_ENAFIN.pdf

estimate their effects on real outcomes.

Our methodology to obtain credit supply shocks at the bank level consists of regressing changes in the outstanding credit of each firm to i) a rich set of dummies for the specific size, industry, and location in which the firm operates and ii) bank-fixed effects. The latter captures changes in credit associated with specific conditions of each bank, like their financial health and access to external funding. These bank fixed effects are the parameters of interest and we interpret them as capturing credit-supply shocks.

We then construct firm-level exposure metrics as (pre-shock) credit-weighted averages of the estimated bank fixed effects. The fact that we weight shocks based on the importance of the initial banking relationships implies, first, that we focus on the subsample of firms that already had credit before any shock happened. Moreover, this means that at the firm level, the importance of each credit-supply shock is modulated by the intensity of the pre-existing bank-firm relationships. Hence, these should be sticky; otherwise, firms would change banks when their main one experiences a negative shock or when other banks experience a positive one. We provide evidence that this is the case, consistent with a large body of literature regarding relationship banking (Petersen and Rajan (1994), Boot (2000), Chodorow-Reich (2013)). Thus, unsurprisingly, we find that these shocks strongly predict firm-credit growth.

Our main specification studies the effects of credit supply shocks on employment growth, mean wages, and firm exit. We regress these outcomes on firm fixed effects and a rich set of controls, along with our metric of credit supply shocks. Our analysis encompasses yearly changes for the periods 2018-2019, 2019-2020, and 2020-2021, each assessed from November to November. We find that during this period, credit-supply shocks had a positive effect on employment and a negative effect on firm exit. An increase of one standard deviation in a firm's credit supply shock (about 8.5 percentage points) increases employment growth by 1.5 percentage points (pp) and decreases the probability of exit by 0.25 pp. Importantly, credit had a positive effect on employment growth in surviving firms as well. Moreover, we find that for these continuing firms, an increase of 10 pp in a firm's credit supply shock decrease mean wages by 0.17 pp. This suggests that during this period, firms, when reducing their employment due to a negative credit supply shock, tend to lay off workers from the lower end of the wage distribution.

Our estimations also suggest that credit-supply shocks did not affect real outcomes in 2019. This is consistent with what Greenstone, Mas, and Nguyen (2020) find for the US during normal periods, but it contrasts with the findings of Gutierrez, Jaume, and Tobal (2021) for Mexico during 2010-2016. Instead, we find that in 2020 and 2021, credit-supply shocks positively affected employment. Moreover, these shocks decreased the likelihood of business closures in 2020, the year of extensive lockdowns, which suggests that firms with higher liquidity from available credit were less likely to close amid heightened uncertainty. Employment and overall activity saw an upturn in 2021, notably after the implementation of vaccines. Accordingly, we find that companies that experienced better-than-average credit supply shocks bounced back more quickly, potentially using credit to fund their growth. Credit did not significantly impact business exits during this recovery phase, as its effects were primarily focused on surviving firms.

Further, we examine whether credit shocks have heterogeneous effects across different types of firms. To do so, we interact credit-supply shocks with key firm characteristics like size, age, and incorporation status. We find that the positive effect of credit shocks on survival and employment growth is concentrated on small firms and start-ups. This aligns with research by Siemer (2019) and Chodorow-Reich (2013) that found that the real effects of credit during the Great Recession were focused on smaller, younger companies, underscoring the critical role of credit for this group. In particular, startups appear to be susceptible to credit disruptions. We also find that the influence of credit supply shocks on employment and the probability of exit is more concentrated in unincorporated firms. These types of firms, especially sole-proprietors, generally lack extensive access to capital markets compared to their incorporated counterparts because of their legal status. Consequently, they tend to depend more on credit to fund their operations. Lastly, we find that the effects of credit were more substantial for non-essential sectors. Due to regulations, these sectors faced a sharper drop in demand for their products, since they were unable to operate. Specifically, access to credit lowered the likelihood of business closure within non-essential sectors, an effect not observed within essential sectors[2].

In our baseline estimates of the bank-time fixed effects we control changes in credit demand

[2]In April 2020, the Mexican government declared a list of which sectors were considered as essential, and so, could continue to operate during lockdowns.

by using 3-digit industries and State as a location. However, our results are robust to different specifications for the estimation of credit supply shocks at the bank level and also to a more granular set of controls for economic sectors. We find similar results, for instance, if we compute the credit supply shocks using finer classification of industry or location. Moreover, we also show that these bank-time fixed effects are correlated with measures of bank performance, which further validates our credit-supply shocks.

1.1 Literature Review

In this paper, we study the real effects of credit on a developing country during the COVID-19 pandemic. Our research intersects with various strands of the literature. First, it is related to studies that focus on bank lending to firms during crises. Bank credit is pro-cyclical, which may be critical when the economy experiences an adverse shock. The global financial crisis led to several papers on the topic. Ivashina and Scharfstein (2010) show that following the Lehman Brothers collapse, US banks almost halved their lending to large corporations. De Haas and Van Horen (2013) show that banks decreased less their lending to geographically-close markets, where they were more experienced and where they had a subsidiary. Puri, Rocholl, and Steffen (2011) find that German savings banks affected by the US subprime mortgage crisis rejected significantly more loan applications than non-affected banks after 2007. Popov and Van Horen (2015) find that sovereign stress in peripheral countries negatively impacted bank credit to non-peripherals during the global financial crisis. Using matched firm-bank level data for Eastern Europe and Turkey, Ongena, Peydro, and Horen (2015) find that during the crisis internationally borrowing domestic and especially foreign owned banks contracted their credit more than locally funded domestic banks.

A number of studies explored the implications of the pandemic on bank lending behavior. Acharya, Engle, and Steffen (2021) show that banks with large ex-ante exposures to undrawn credit lines as well as large ex-post gross drawdowns experienced larger declines in equity prices and in term loan lending, even after policy measures were implemented. Li, Strahan, and Zhang (2020) document that in March 2020, banks faced the largest increase in liquidity demands ever observed, as firms drew funds on a massive scale from preexisting credit lines in anticipation of cash flow and financial disruptions, but coincident inflows of funds from both the Federal Reserve's liquidity injection programs and depositors, along with strong pre-shock bank capital, allowed banks to accommodate these liquidity demands. Fiordelisi et al. (2022) document that during the Covid-19 pandemic, euro-area banks using their own internal-rating based models to measure credit risk, decreased their on-balance sheet credit exposures, especially lending, to non-financial firms more than banks using standard fixed risk-weights models to the same borrower.

This paper also ties to studies on the impact of credit market shocks on employment. Utilizing a natural experiment or exogenous supply shock helps identify the effect of credit supply on real variables. Benmelech, Bergman, and Seru (2021) used 'quasi-experiments' to assess the causal effects of credit on employment, exploiting scenarios like maturing long-term debt, bank deregulation, and Japanese loan supply shocks. Boeri et al. (2012) considered how credit level and small business lending respectively influence employment during crises. Bentolila, Jansen, and Jimenez (2017) showed how Spanish firms with large exposure to weak banks cut more jobs during the global financial crisis. Chodorow-Reich (2013) used syndicated loan data to demonstrate how small firms borrowing from impaired banks reduced employment more. Duygan-Bump, Levkov, and Montoriol-Garriga (2015) and Popov and Rocholl (2018) revealed employment impacts due to high external financing needs and exogenous funding shocks. For Germany, Huber (2018) studied the effects of lending cuts on employment, using a large decline in lending by Commerzbank as an exogenous variation. Berton et al. (2018a) studied the effects of credit on labor in Italy, showing that financially constrained firms reduced employment. Greenstone, Mas, and Nguyen (2020) analyze a US county panel to gauge credit growth's impact on employment. They estimate an equation to isolate credit supply shocks, distinguishing between county and bank fixed effects. They find that small business lending doesn't significantly impact small businesses or overall US economic activity. A predicted decline in small business lending doesn't affect small firms or overall employment during crises or normal times.

For Mexico, in an analysis at the county level, Gutierrez, Jaume, and Tobal (2021) followed Greenstone, Mas, and Nguyen (2020) methodology to obtain an exogenous variation in the supply of credit. They found that credit supply shocks have an important effect on formal employment. Acosta and Cortes (2022), in an analysis at the firm level, exploit unexpected prepayment of loans by local governments to obtain exogenous variations in new credit available to firms. They found that an increase in new loans increases firms' employment and that smaller firms react to the expansion of credit earlier than larger firms.

2 Data

We use loan-level data from Mexico's National Banking and Securities Commission (*Comisión Nacional Bancaria y de Valores*, CNBV) and formal employment data from the Mexican Institute of Social Security (*Instituto Mexicano de Seguridad Social*, IMSS). Merging both datasets allows us to study the relation between credit and employment, average wages and exit at the firm level.

The credit data set contains information about the universe of business loans issued by commercial banks from 2003 to 2021. By law, all banks are required to submit monthly reports to the CNBV detailing each active business loan in their portfolio. For each loan in the database, we observe anonymized IDs of the issuing bank and the borrower, along with a comprehensive set of credit and firm characteristics. Since our unit of interest is the firm, we use borrowers IDs which correspond to their anonymized tax IDs (RFC) to aggregate observations at the firm-month level. This allows us to obtain each firms outstanding debt level , total credit line, and other relevant variables for our analysis.

We also create an employer-employee monthly panel data set using social security payroll tax records from IMSS. This data set contains information about the universe of formal workers that receive a wage from a formal private firm. We refer to formal firms as those that are properly registered with the tax authority and to formal workers as those that are registered in the social security system by their employer. All private employers are obligated by law to report the daily wages of each one of their employees to IMSS and to pay social security taxes based on those reports. The data set starts in November 2004 and the unit of observation is a worker. Each observation contains anonymized worker IDs, the daily wage reported on the last day of each month, and some worker characteristics, including their age, gender, industry of employment, and the location in which they work. Anonymized tax IDs at the firm level are also available in the data set starting in November 2018.

Since both datasets include firm tax IDs that have been anonymized using the same algorithm, we are able to merge them. The resulting dataset is a panel with monthly frequency, containing firm-level information on outstanding credit, the industry and size of the firm, as well as its employment. The industry variable is defined according to the NAICS classification, and the size variable takes one of four values: micro, small, medium, or large. Each size category is defined based on the firm's employment and revenue. Both of these variables come from the CNBV dataset. Additionally, we determine the age of each firm using employment information from the IMSS dataset.[3]

In our baseline analysis, we study the yearly changes in credit, employment, average wage, and probability of exit for 2018-2019, 2019-2020, and 2020-2021, each assessed from November to November. Table 1 presents the summary statistics for each variable and period of interest. Given that we are interested in the effects of credit on real outcomes, we restrict our sample to firms that had both positive credit and employment in November 2018. That month, the total number of firms with positive employment was 1,028,863, and only 168,472 (16%) had outstanding credit. While the subset of firms that do not have credit is interesting, our identification strategy relies on relationship banking, and hence we focus on the subset of firms with pre-crisis access to credit.

A drawback of our analysis is that we can only observe formal employment while it has been documented that formal firms have both formal and informal employees (Busso, Fazio, and Algazi (2012), Samaniego de la Parra and Fernández Bujanda). It is plausible that firms would adjust employment via informal workers first, as it is less costly for them to do so. In this sense, our results present a lower bound for the real effects of credit.

[3]We can also construct the variables industry and size using the IMSS dataset and we test the robustness of our results by using them rather than the definition from the CNBV. See A.2.2 for an explanation of how to construct them along with the age of a firm.

Table 1: Summary Statistics

			N	Mean	Std	p25	p50	p75
2019m11	Δ Credit	All	194,015	-0.14	1.21	-2.00	-0.21	2.00
		Our Sample	168,472	-0.42	0.98	-2.00	-0.31	0.80
	Δ Employment	All	1,028,863	0.04	1.05	-2.00	0.00	2.00
		Our Sample	168,472	-0.07	0.74	-0.75	0.00	0.51
	Δ Mean Wages	All	1,028,863	0.12	1.00	-2.00	0.09	2.00
		Our Sample	168,472	0.03	0.66	-0.18	0.07	0.37
2020m11	Δ Credit	All	182,909	-0.24	1.15	-2.00	-0.22	1.86
		Our Sample	139,725	-0.46	0.95	-2.00	-0.29	0.61
	Δ Employment	All	1,032,976	-0.03	1.04	-2.00	0.00	2.00
		Our Sample	139,725	-0.11	0.74	-0.89	0.00	0.44
	Δ Mean Wages	All	1,032,976	0.08	0.98	-2.00	0.12	2.00
		Our Sample	139,725	0.03	0.65	-0.20	0.08	0.28
2021m11	Δ Credit	All	182,290	-0.16	1.23	-2.00	-0.23	2.00
		Our Sample	120,852	-0.45	0.99	-2.00	-0.31	0.77
	Δ Employment	All	1,084,824	0.10	1.11	-2.00	0.00	2.00
		Our Sample	120,852	0.05	0.83	-0.67	0.00	1.00
	Δ Mean Wages	All	1,084,824	0.16	1.04	-2.00	0.14	2.00
		Our Sample	120,852	0.09	0.72	-0.18	0.10	0.38

Notes: Changes in credit, employment, mean wages and establishments are computed using the definition of Davis, Haltiwanger, and Schuh (1996). See Equation (4) for additional details.

3 Stylized Facts of the COVID-19 Recession in Mexico

Before we move to study the real effects of credit, in this section we give an overview of the features that characterized the COVID-19 recession in Mexico. We present aggregate facts of employment, firm, and credit dynamics.

3.1 Firm Survival

The number of formal firms decreased by .89% (8.1 thousand firms) from March 2020 to May 2020 and it remained well below its pre-pandemic level by the end of 2020. The main driver behind the fall in formal firms was a sharp and persistent decrease in the entry rate (Figure 1)

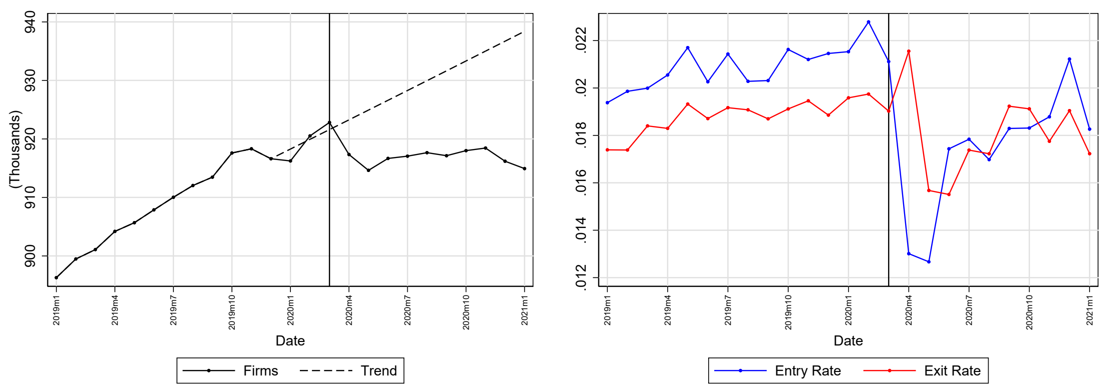

Figure 1: Number of formal firms (left). Entry and exit rates (right)

3.2 Employment dynamics

Formal employment decreased 5.34% (1.1 million workers) between March 2020 and July 2020. It did not recover its pre-pandemic levels until November 2021. Inflows and outflows were both responsible for the decline in formal employment. Most of the increase in job destruction was due to surviving firms downsizing rather than firms exiting the market (Figure 2).

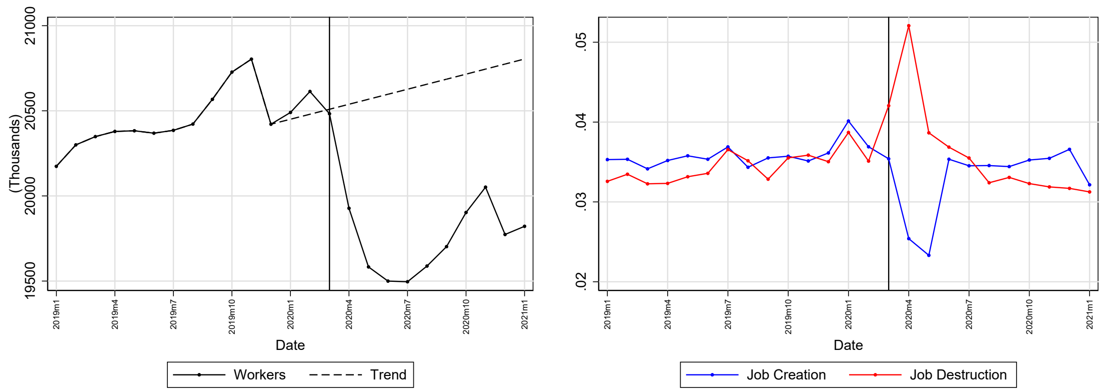

Figure 2: Number of formal firms (left). Entry and exit rates (right)

3.3 Heterogeneity in firm and employment dynamics

Firm and employment dynamics were heterogeneous by geographical location, economic sector and firm size. For example, between March and July 2020: employment decreased by 6% in the Service sector and 3% in Manufacturing, while the number of firms decreased by 1.5% and .8%, respectively. Employment in the South decreased by 7.7% and 3.0% in the North, while the number of firms decreased by 1.0% and .5%, respectively. Given that banks pre-Covid portfolio was heterogeneous in its sectoral and geographic composition, the effects of Covid-19 on each bank's balance sheet were also different.

3.4 Banking Credit Dynamics

Total credit from commercial banks to non-financial private firms increased by 10% between January 2020 and April 2020. This represented the largest quarterly increase in the last in 10 years (Figure 3). A similar phenomena was documented in the USA by Li, Strahan, and Zhang (2020). This large increase in credit contrasts with the behavior of credit during the Great Recession (see A.1).

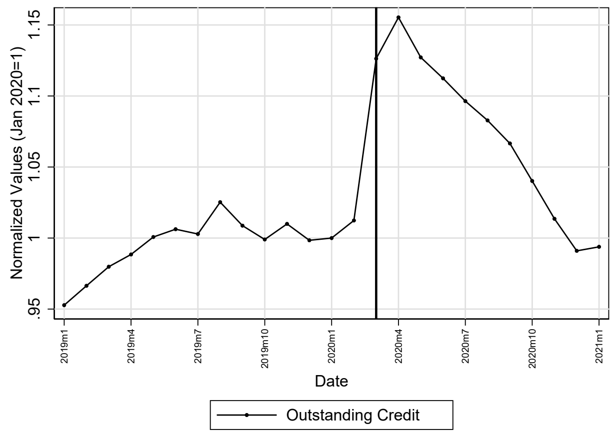

Figure 3: Outstanding Banking Credit

4 Empirical Strategy

Our objective is to measure the effects of credit on real outcomes during the pandemic. This is a challenging task due to the endogenous nature of credit. For instance, firms that faced substantial drops in revenue during the pandemic may have increased their demand for credit to meet their working capital requirements. To address the issue of endogeneity and credibly identify the causal effects of credit on real outcomes, we need to abstract from variations in credit that are related to a firm's demand. Thus, we estimate credit supply shocks at the bank level by isolating changes in credit explained by demand factors. Then, we compute credit supply shocks at the firm level based on each firm's pre-existing banking relationships.

4.1 Credit-supply shocks

To estimate credit supply shocks, we adopt a strategy closely related to the one proposed by Amiti and Weinstein (2018). This approach involves attributing changes in outstanding credit to both demand and supply factors. To do so, it heavily relies on multi-bank firms, since by observing the same firm with different banks, one can distinguish demand (firm) from supply (bank) variations. Nonetheless, given that multi-bank firms are only 30% of our sample, we slightly depart from their approach and instead implement the industry-location-size-time (ILST) estimator proposed by Degryse et al. (2019). In particular, we estimate,

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

where our dependent variable is the change in outstanding credit between period t-j to t granted by bank b to firm i, where firm i operates in industry k, location l, and is in size category s. Thus, $\gamma_{kls(i),t}$ are a set of time-varying industry-location-size fixed effects that capture changes in credit attributable to factors related to the specific size, industry and location in which the firm operates. While $\delta_{b,t}$ are bank-time fixed effects meant to capture changes in credit associated with specific conditions of each bank, like their financial health and access to external funding. These bank-time fixed effects are the parameters of interest as they represent credit supply shocks. We consider yearly changes (so $j = 12$), and our analysis covers November 2018 to November 2021, which means we estimate equation (1) for three different periods[4].

The implicit identification assumption behind (1) is that credit demand shocks are common across firms in the same industry-location-size group and thus, the set of fixed effects $\gamma_{kls(i),t}$ can absorb changes in credit caused by variation in firm's demand. In that sense, we try to narrow these fixed effects as much as possible, but without losing many observations. Hence, in our baseline estimates, we consider the firm's industry to be the 3-digit NAICS classification, the location to be a state in which it operates, and the firm size to be classified in four brackets according to the number of employees and sales. To put an example, we assume that the demand for credit of small firms (<10 workers) operating in the food manufacturing industry (311), in the northern State of Nuevo Leon, is comparable. In appendix A.4 we compare our bank fixed effects to the ones obtained by using Amiti and Weinstein (2018) multi-firm approach, and find a strong correlation among them.

An important point of Amiti and Weinstein (2018) methodology, that we adopt when implementing the ILST estimator, is that each observation is weighted by its share in the economy-wide outstanding credit in the base period $(t - j)$. Hence, by construction, the estimates match aggregate credit changes. In that sense, abstracting from new bank-relations, equation (1) can

[4]We consider yearly changes considering November as a base to avoid capturing seasonal variations from both credit and employment as firms tend to lay off workers in December and then rehire them in January

be estimated using Weighted Least Squares (WLS) as shown by Tielens and Van Hove (2017). This is a superior strategy as opposed to estimate (1) via Ordinary Least Squares (OLS), given that it accounts for general equilibrium constraints. In this case, the bank 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. These bank fixed effects can be either normalized to a reference value at each point in time or taken as given. In our baseline estimates, we follow the aforementioned work and normalize the bank fixed effects relative to the median in each period.

Our estimation of bank fixed effects differs from the geographical approach pioneered by Greenstone, Mas, and Nguyen (2020) and implemented by Gutierrez, Jaume, and Tobal (2021) for the context of Mexico. They aggregate credit at the location-bank level and incorporate location and bank fixed effects to separate supply from demand factors. In that case, their implicit assumption is that demand at a locality, either a county or a local labor market, is homogeneous for firms, regardless of their industry and size.[5]. Given that the impact of the pandemic varied across industries and firm size, we posit a more flexible specification for demand shocks that allow for heterogeneity across firms within a location based on these additional characteristics. Berton et al. (2018b) also consider industry, location, and size controls rather than location alone.

By construction, our estimated credit supply shocks are meant to capture changes in credit attributable to unique conditions within individual banks, such as shifts in their internal cost of funding. An example of one the potential sources of credit supply shocks is documented by Morais et al. (2019) who demonstrated that foreign banks in Mexico tend to increase their credit provision when the monetary policy in their home countries eases. Our approach is data-driven and hence deliberately agnostic regarding the sources of credit supply shocks. We see this as a strength, given the multitude of factors that impacted banks concurrently during the pandemic.

While our methodology does not allow us to pinpoint the exact causes of credit supply shocks, we study whether they correlate with changes in the bank's funding sources or their profits as

[5]This is a strong assumption as states in Mexico are heterogeneous in their industry and firm size composition. Especially as industries differ substantially in their financial needs (Rajan and Zingales (1998)) and credit demand is strongly correlated with size (Chodorow-Reich et al. (2022))

a form of an external validity check. In particular, we consider three variables that affect the funding availability of banks: deposits, equity, and interbank liabilities. In terms of profitability, we use two measures: return on assets (ROA) and return on equity (ROE). All these bank-specific variables are obtained at a yearly frequency from the banking regulator, the CNBV. The year-over-year change in deposits, equity, and interbank liabilities are expressed as percentage changes relative to each base year's assets. Meanwhile, ROA and ROE correspond to the earnings of the preceding 12 months.

Table 2 presents the results of a set of regressions in which the independent variable is the credit supply shock, and the explanatory variables are the aforementioned bank-specific factors. We include four years in our sample (2018-2021), and our reference month each year is November to be consistent with the estimation of Equation (1). Column one presents the results for the set of financial intermediaries that lend to firms and receive deposits from the public while column two presents the results for all financial intermediaries that lend to firms, irrespective of whether they receive deposits or not. We find that our credit supply shocks are positively related to growth in interbank funding for all financial intermediaries and negatively related to equity growth for financial intermediaries that receive deposits. This suggests that banks that receive deposits inject equity following a negative supply shock.[6] We also find a positive relationship between credit supply shocks and both metrics of banks' profitability. These findings show that our estimated credit supply shocks correlate not only with growth in outstanding credit but also with a broader range of banking metrics.

[6] Degryse et al. (2019) also finds a positive relationship between credit supply shocks and interbank lending using monthly data for Belgium. Furthermore, they also find a negative relationship between credit supply shocks and equity growth during 2009-2012.

Table 2: Bank Credit Supply Shocks and Bank Metrics.

	Bank Fixed Effects			
	(1)	(2)	(3)	(4)
Deposit growth	0.0338			
	(0.1396)			
Equity growth	-1.172**	-.3845		
	(0.5540)	(0.4154)		
Interbank liabilities growth	0.6116*	0.2406***		
	(0.3625)	(0.0910)		
ROE			0.0474**	
			(0.0020)	
ROA				.0247*
				(0.0150)
Observations	111	163	163	163
Time FE	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes

Notes: Column (1) includes only those financial intermediaries that receive deposits from the public. Conversely, columns (2), (3), and (4) encompass all financial intermediaries that lend to firms, irrespective of whether they receive deposits or not. Standard errors are clustered at the bank level.

4.2 Firm-level credit supply shocks

To estimate the effects of credit supply shocks on real outcomes, we construct firm-level exposure metrics as weighted averages of the estimated bank fixed effects from (1), namely $\widehat{\delta_{b,t}}$. In particular, we use as weights the initial-period outstanding credit shares that each firm has with a particular bank,

$$Z_{j,t} = \sum_b w_{i,b,t_0} \times \hat{\delta_{b,t}} \tag{2}$$

So, w_{i,b,t_0} is the share of credit of firm i with bank b at $t = 0$. Hence, by definition, our sample only includes firms that had positive credit in the baseline period (November 2018). This allows us to construct Bartik-type shocks in which the shares are fixed, and variation comes solely from exogenous credit-supply changes.[7] Thus, by following the cohort of firms that were operating in November 2018, we rely on both the fact that switching lenders is costly for firms, and the implicit assumption that firms did not select banks strategically by anticipating which ones will be more affected during the recession.[8] The data supports the idea that switching lenders is costly and hence uncommon. In November 2019, 88% of firms maintained the same main bank they had in November 2018, while 81% did in November 2020.

In Table 3, we investigate whether our estimates of credit supply shocks correlate with a set of observable firm characteristics. We categorize firms into quartiles based on their credit supply shocks between November 2018 and November 2019. We then compare the composition of firms in each quartile in terms of their employment, outstanding credit, size, age, legal status, industry, and the region in which they operated in November 2018. Our findings indicate no systematic differences in the composition of firms across different quartiles. Furthermore, our baseline estimation incorporates firm fixed effects to account for potential selection based on non-observable characteristics.

Table 3: Balance Test

		Quartile of exposure to credit supply shock			
		(1)	(2)	(3)	(4)
Mean Employment		2.57	2.86	2.63	2.89
Mean Credit		13.30	13.92	13.29	13.93
	Small	0.88	0.82	0.88	0.81
Size	Medium	0.09	0.12	0.09	0.13
	Large	0.03	0.06	0.03	0.06
	Start-up	0.30	0.22	0.21	0.24
Age	Young	0.22	0.24	0.24	0.22
	Old	0.48	0.54	0.55	0.54
Firm type	Unincorporated	0.51	0.34	0.43	0.36
	Incorporated	0.49	0.66	0.57	0.64
	Agriculture	0.02	0.02	0.02	0.04
	Construction	0.07	0.06	0.06	0.10
Sector	Manufacture	0.11	0.12	0.15	0.16
	Commerce	0.49	0.35	0.39	0.30
	Services	0.31	0.46	0.39	0.41
	Norte	0.25	0.27	0.19	0.39
Region	Centro Norte	0.26	0.27	0.25	0.24
	Centro	0.36	0.33	0.43	0.28
	Sur	0.13	0.14	0.13	0.09
	Observations	47,436	47,104	62,758	31,782

Notes: We classified firms into quartiles according to their credit supply shock between November 2018 and November 2019. Before calculating the means of employment and credi we took logarithms.

[7] Our estimates of firm-level credit supply shocks display substantial heterogeneity each year in our sample (see A.3).

[8] An alternative strategy is to fix the weights at November 2019, just months before the pandemic started. We decided to not do so to contrast our estimates in "normal times" vs a recession.

With our firm credit-supply exposure metrics at hand, we estimate their corresponding real effects on different outcomes by estimating the following regression:

$$Y_{j,t} = \beta_0 + \beta_1 Z_{j,t} + \omega_j + \psi_{j,t} + \psi_{k,t} + \psi_{l,t} + \epsilon_j \tag{3}$$

Where $Y_{j,t}$ is the outcome of interest, $Z_{i,t}$ is the exposure metric described above, ω_j are firm-fixed effects that account for time-invariant unobservables at the firm level, and $\psi_{j,t}$, $\psi_{k,t}$, $\psi_{l,t}$ are time fixed-effects interacted with size, industry dummies, respectively. We include the latter to account for potential time-varying heterogeneous effects that the COVID recession may have had on different groups of firms. In this case, the coefficient of interest from (1) is β_1, which can be interpreted as the percentage change in a firm's credit supply conditions. Since the treatment effect originates at the financial institution, we clustered standard errors at the (main) bank level.[9]

The fact that the unit of observation is the firm, along with our panel data structure, allows us to control for unobserved firm characteristics that could be driving changes in either employment or other outcomes. We achieve this control by introducing firm fixed effects. This is one of the advantages of working with firm-level data, as opposed to aggregate information at a particular geographical level, which may include confounding locality factors. Furthermore, the nature of our data also allows us to directly explore how different firm characteristics interact with our credit supply exposure shocks. In this sense, the richness of the data enables us to examine a variety of heterogeneous effects, as shown in Section 5.2.

5 Results

5.1 Main Results

We estimate equation (3) using changes in employment, average wage, and the number of establishments as outcome variables. To compute these changes, we follow the firm-dynamics

[9]We define a firm's main-bank as the one with whom the firm has the largest outstanding credit at the reference period (November-2018). Chodorow-Reich (2013), Berton et al. (2018a), Degryse et al. (2019), Chodorow-Reich et al. (2022) also cluster at the main bank level.

literature (see. e.g., Davis, Haltiwanger, and Schuh (1996)) and calculate them as:

$$\Delta Y_{i,t,t-s} = \frac{Y_{i,t} - Y_{i,t-s}}{0.5(Y_{i,t} + Y_{i,t-s})} \tag{4}$$

which has the advantage to account for both entry and exit, and to mitigate the influence of outliers, given that this metric is bounded between -2 and 2.

Our analysis encompasses two sets of firms. First, we consider our entire sample, which includes both continuing firms and those that have exited (report zero employment). Then, we narrow our focus to include only the firms that have continued operations. We also study the probability of exit as another outcome variable. Table 4 present our main results. The coefficients associated with credit supply shocks are positively related to employment and negatively related to the probability of exit. The effects on employment appear both on the extensive and intensive margin as credit supply shocks are also positively related to employment when we restrict our estimation to firms that did not exit. Mean wage is negatively related to credit supply shocks indicating that firms that decrease their employment as a consequence of a negative credit supply shock lay off workers on the lower end of the firm's wage distribution.

Table 4: The Real Effects of Credit Supply Shocks.

	Δ Employment		Δ Mean Wage	Exit
	All	Continuers		Continuers + Exiters
	(1)	(2)	(3)	(4)
Credit Supply Shock	0.1855***	0.0442*	-0.0172***	-0.0278***
	(0.0686)	(0.0238)	(0.0057)	(0.0102)
Observations	398589	358177	358177	382134
Firm FE	Yes	Yes	Yes	Yes
Industry x time FEs	Yes	Yes	Yes	Yes
State x time FEs	Yes	Yes	Yes	Yes
Size x time FEs	Yes	Yes	Yes	Yes

Notes: Changes in Employment, Mean Wage and Establishment are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.

Table 5 shows the results of estimating equation 3 using changes in credit at the firm level as an outcome variable. The coefficients associated to credit supply shocks are positive and statistically different than zero. This result provides evidence for credit supply shocks affecting real outcomes via changes in credit at the firm level. Furthermore, it implies that firms are not able to change banks when their main bank experiences a negative shock or when other banks experience a positive one. This result is in line with what Greenstone, Mas, and Nguyen (2020) finds for US and Gutierrez, Jaume, and Tobal (2021) find for Mexico.

Table 5: Credit Supply Shocks and Firms Credit

	Δ Credit		
	All	Continuers	Continuers + Exiters
	(1)	(2)	(3)
Credit Supply Shock	0.5879**	0.5789**	0.5861**
	(0.2515)	(0.2646)	(0.2522)
Observations	398589	358177	382134
Firm FE	Yes	Yes	Yes
Industry x time FEs	Yes	Yes	Yes
State x time FEs	Yes	Yes	Yes
Size x time FEs	Yes	Yes	Yes

Notes: Changes in Firms Credit are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.

5.2 Heterogenous effects

5.2.1 Results by Year

Our baseline estimation incorporates data from the years 2019, 2020, and 2021. By including 2019, a period unaffected by the pandemic, we are able to estimate firm fixed effects for a larger pool of firms expanding our sample size. However, given that our primary focus in this paper is to study the effects of credit during the pandemic we also estimated equation 3 interacting our metric of credit supply shocks with year dummies. The result of this estimation are presented in Table 6. According to our estimations, credit supply shocks had no effect on real outcomes during 2019. This is a result that is in line with what Greenstone, Mas, and Nguyen (2020) finds for the US in normal times but differs from the what Gutierrez, Jaume, and Tobal (2021) find for Mexico for the period 2010-2016. It is, however, important to consider that since equation 3 includes firm fixed effects then firms that permanently exited in 2019 are, by construction, not in our sample which makes the comparison between 2019 and subsequent years less precise.

Credit supply shocks had a positive effect on employment during 2020 and 2021. Furthermore, they reduced the probability of exit during 2020 which was the year of widespread lock-downs. This outcome aligns with the notion that firms with more liquidity, in the form of available credit, were less likely to close during the year of peak uncertainty and with the largest fall in aggregate demand. Employment and general economic activity experienced a significant rebound in 2021 following the announcement and subsequent rollout of COVID-19 vaccines. Our estimates suggest that firms experiencing better-than-average credit supply shocks recovered more swiftly, likely using credit to finance their expansion. Credit did not seem to have played a role on exit during the recovery period as its effects were concentrated on the firms that survived.

5.2.2 Results by Size and Age

Tables 7 and 8 present the results of interacting our measure of credit supply shocks with the variables Size and Age respectively. We find that the positive relationship between credit supply shocks on employment and probability of survival is concentrated in firms categorized as *small* and in firms categorized as *start-ups*. This result is consistent with what is found by Siemer (2019) and Chodorow-Reich (2013) who find that the effects of credit during the Great Recession were concentrated in small, younger firms. Our results confirm the importance of credit for this group of firms. The effect is particularly large for startups which indicates that disruptions in credit availability for new firms is highly important.

Table 6: The Real Effects of Credit Supply Shocks by Year

		Δ Employment		Δ Mean Wage	Exit
		All	Continuers		Continuers + Exiters
		(1)	(2)	(3)	(4)
Credit Supply Shock	Pre-COVID (2019)	0.0848	0.0396	-0.0175	-0.0347
		(0.0953)	(0.0427)	(0.0177)	(0.0247)
	Early COVID (2020)	0.1572**	0.0112	-0.0126**	-0.0247**
		(0.0594)	(0.0245)	(0.0058)	(0.0114)
	Late COVID (2021)	0.2303**	0.0667	-0.0200**	-0.0278
		(0.1100)	(0.0417)	(0.0078)	(0.0197)
Observations		398589	358177	358177	382134
Firm FE		Yes	Yes	Yes	Yes
Industry x time FEs		Yes	Yes	Yes	Yes
State x time FEs		Yes	Yes	Yes	Yes
Size x time FEs		Yes	Yes	Yes	Yes

Notes: Changes in Employment, Mean Wage and Establishment are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.

Table 7: The Real Effects of Credit Supply Shocks by Size

		Δ Employment		Δ Mean Wage	Exit
		All	Continuers		Continuers + Exiters
		(1)	(2)	(3)	(4)
Credit Supply Shock	Small	0.2004***	0.0384	-0.0178**	-0.0320***
		(0.0732)	(0.0256)	(0.0071)	(0.0104)
	Medium	0.1285**	0.0764	-0.0073	-0.0091
		(0.0609)	(0.0585)	(0.0194)	(0.0142)
	Large	0.0127	0.0797	-0.0256	0.0133
		(0.0860)	(0.0808)	(0.0243)	(0.0164)
Observations		398589	358177	358177	382134
Firm FE		Yes	Yes	Yes	Yes
Industry x time FEs		Yes	Yes	Yes	Yes
State x time FEs		Yes	Yes	Yes	Yes
Size x time FEs		Yes	Yes	Yes	Yes

Notes: Changes in Employment, Mean Wage and Establishment are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.

Table 8: The Real Effects of Credit Supply Shocks by Age

		Δ Employment		Δ Mean Wage	Exit
		All	Continuers		Continuers + Exiters
		(1)	(2)	(3)	(4)
Credit Supply Shock	Start-up	0.8656***	0.1537***	-0.0094	-0.1190***
		(0.1601)	(0.0411)	(0.0094)	(0.0251)
	Young	0.0566	0.0297	-0.0349***	-0.0343**
		(0.0579)	(0.0414)	(0.0123)	(0.0170)
	Old	-0.1120	0.0021	-0.0132***	0.0178
		(0.0942)	(0.0244)	(0.0048)	(0.0192)
Observations		398589	358177	358177	382134
Firm FE		Yes	Yes	Yes	Yes
Industry x time FEs		Yes	Yes	Yes	Yes
State x time FEs		Yes	Yes	Yes	Yes
Size x time FEs		Yes	Yes	Yes	Yes

Notes: Changes in Employment, Mean Wage and Establishment are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.

5.2.3 Results by Legal Status

We also explore the heterogeneity of our baseline results by interacting our metric of credit supply shocks with a dummy variable that indicated whether a firm is incorporated or unincorporated. In Table 9 we can see that the effects of credit supply shocks on employment and probability of exit are concentrated on unincorporated firms. Unincorporated firms, which often include sole proprietorship's and partnerships, are typically smaller and have less access to capital markets than incorporated firms. As a result, they may rely more heavily on credit for financing their operations and investment needs.

Table 9: The Real Effects of Credit Supply Shocks by Legal Status

		Δ Employment		Δ Mean Wage	Exit
		All	Continuers		Continuers + Exiters
		(1)	(2)	(3)	(4)
Credit Supply Shock	Unincorporated	0.4798***	0.1786***	-0.0232***	-0.0730***
		(0.0972)	(0.0369)	(0.0076)	(0.0148)
	Incorporated	-0.0569	-0.0600**	-0.0126**	0.0084
		(0.0565)	(0.0294)	(0.0052)	(0.0142)
Observations		398535	358130	358130	382080
Firm FE		Yes	Yes	Yes	Yes
Industry x time FEs		Yes	Yes	Yes	Yes
State x time FEs		Yes	Yes	Yes	Yes
Size x time FEs		Yes	Yes	Yes	Yes

Notes: Changes in Employment, Mean Wage and Establishment are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.

5.2.4 Results by Essential Sectors

Table 10 shows the results of credit supply shocks for sectors that were deemed as *essential* by the government and compares it with the ones that were not. As expected, the effects of credit supply shocks on both sectors were positive but were larger for non-essential sectors as these sectors faced a deeper decreased in demand for their products and some of them could not operate due to regulations. Furthermore, credit decrease the probability of exit for non-essential sectors while it did not for essential ones.

Table 10: The Real Effects of Credit Supply Shocks by Essential and Non-Essential Sectors

		Δ Employment	Δ Mean Wage		Exit
		All	Continuers		Continuers + Exiters
		(1)	(2)	(3)	(4)
Credit Supply Shock	Non-essential	0.1946***	0.0248	-0.0145	-0.0360***
		(0.0717)	(0.0226)	(0.0090)	(0.0101)
	Essential	0.1743*	0.0678*	-0.0204***	-0.0175
		(0.0924)	(0.0394)	(0.0062)	(0.0132)
Observations		398589	358177	358177	382134
Firm FE		Yes	Yes	Yes	Yes
Industry x time FEs		Yes	Yes	Yes	Yes
State x time FEs		Yes	Yes	Yes	Yes
Size x time FEs		Yes	Yes	Yes	Yes

Notes: Changes in Employment, Mean Wage and Establishment are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.

6 Conclusions

To the best of our knowledge, this is the first paper to document the real effects of credit during COVID-19 for a developing country. Our empirical approach consists on, first, constructing credit supply shocks at the bank level and, second, use these estimations to construct credit supply shocks at the firm level based on the exposure of each firm to different banks. Our results suggest a quantitatively important effect of credit on employment during 2020, the year of the lock-downs and the one of the highest uncertainty, and also in 2021 as firms with relationships with banks with credit supply shocks above average grew faster. We identify effects of credit during 2020 at both intensive and extensive margin as credit supply shocks also reduced the probability of exit.

We also present evidence that the credit supply shocks were highly heterogeneous between different types of firms. Specifically, its effects were concentrated on firms categorized as small, startups and unincorporated. Our results are robust to different specifications for the bank level credit supply shocks and also to different levels of granularity in terms of economic sector and location. Our findings are consistent with the positive effects of credit shocks during crises, see for example, Chodorow-Reich (2013), Bentolila, Jansen, and Jimenez (2017) and Berton et al. (2018a) among others. They also suggest that targeted measures to alleviate the financial stress among vulnerable firms during a severe crisis in developing countries could also contribute to mitigating adverse real effects on the economy.

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A Appendix

A.1 Banking Credit During the Great Recession

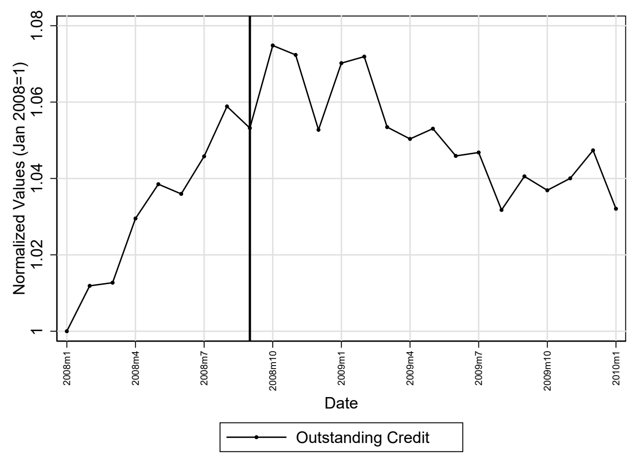

Figure 4: Bank Credit during the Great Recession

A.2 Data Appendix

In this section we describe the cleaning process applied to the credit and employment data.

A.2.1 Treatment of Credit Data

Before estimating the credit supply shocks at the bank level, we made several adjustments to the original data set. We only keep loans provided to firms with a fiscal address in Mexico. This is because we can only merge employment data for these firms. Additionally, we only keep loans provided to incorporated and non-incorporated firms, excluding those made to the government (at the federal, state, or municipal level) or to companies partially owned by the government. Banks might face different incentive lending to the government and hence including them would change the interpretation of of estimation of credit supply shocks.

We also remove loans issued by development banks and other financial institutions from our data set, as we are specifically interested in examining the impact of credit from private banks on employment. Another adjustment involves considering only credits that were issued in Mexican pesos. Otherwise, we would have to adjust for exchange rates movements which were dramatic during the first months of the pandemic. Our analysis focuses exclusively on performing loans. This is because once a loan passes from performing to non-performing status, it does not leave our sample homogeneously. This means that a loan from bank j that has 8 months past due might disappear from the data set while another equivalent loan from a different bank might stay in the data set and disappear once it reaches 10 months. Keeping non performing loans would add noise to our estimates. We also removed banks that had less than 100 active loans on average during the months November 2018, November 2019, November 2020 and November 2021. Banks with an 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 described in Section 4.

Furthermore, in some cases, the location or the industry in which a firm operates are not constant over time. In those cases, we make those variables time invariant assigning the most common value over the period of interest. For example, if the industry of a firm is manufacturing in the majority of credits and services in a minority of them then their industry is set to manufacturing for the whole sample.

Lastly, we also adjust by merges and acquisitions. In particular, we join financial entities, for the whole period of study, that at some point during 2017-2021 where either absorbed or purchased. Moreover, we join institutions that belong to the same financial group, but, for different circumstances, have split their banking business in several branches[10].

A.2.2 Treatment of Employment Data

Age is an important variable to consider as it is an important factor determining growth both in normal and crisis times. Given that this variable is not included in the credit data set, we construct it using the employment data set. Specifically, we classify firms into age categories based on their age at the reference period, $t-k$. To do this, we determine the date of birth of a firm by taking the earlier of two dates: the date of first appearance in the Social Security

[10]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.

data set, or the date the firm registered for tax purposes (in the case of incorporated firms). For unincorporated firms, we use the date of first appearance in the Social Security data set as their date of birth, as the date of register for tax purposes coincides with the date of birth of the entrepreneur. We determine the incorporated or unincorporated status of the firm based on the tax ID. We use the following age categories: 1) startup: 0-4 years; 2) young: 5-9 years; 3) old: 10 years or more.

A.3 Distribution of Credit Supply Shocks

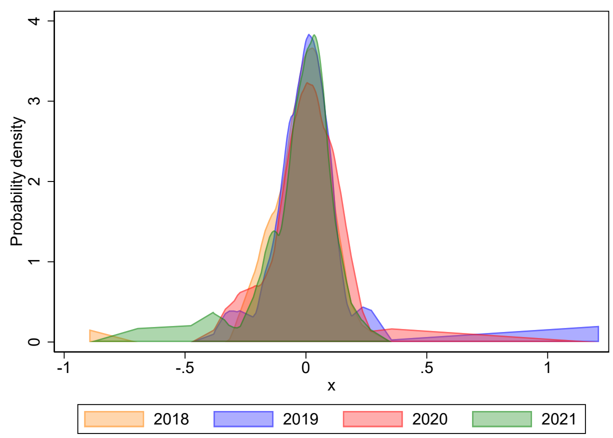

Figure 5: Kernell Density Approximation for Credit Supply Shocks

A.4 Robustness Checks

A.4.1 Comparison Between Our Baseline Credit Supply Shocks (ILST) and Amiti-Weinstein's Specification (FT)

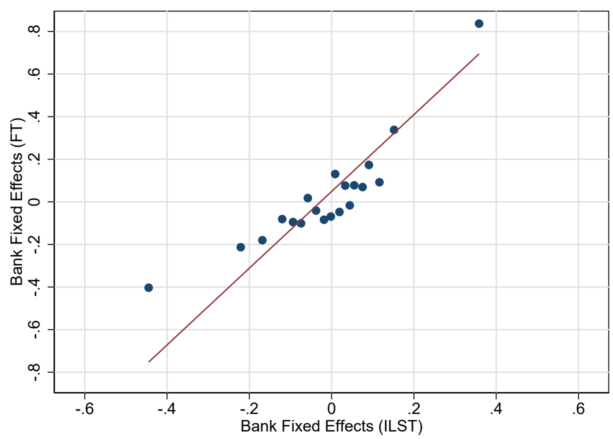

Figure 6: Bank FE: ILST vs FT

A.4.2 Demand controls when estimating Bank Fixed Effects

Table 12 replicates the results of Table 4, but when we consider more dissagregated industries (k) when estimating 1. In particular, we control for 5-digit industries (the maximum level of industry codes we observe in our dataset), as opposed to the baseline 3-digit ones. As can be seen, the results remain unchanged.

Table 12: The Real Effects of Credit Supply Shocks (controling at the 5D NAICS level).

	Δ Employment		Δ Mean Wage	Δ Establishment	Exit
	All	Continuers			Continuers + Exiters
	(1)	(2)	(3)	(4)	(5)
Credit Supply Shock	0.1733***	0.0434**	-0.0184***	-0.0032	-0.0282***
	(0.0555)	(0.0215)	(0.0059)	(0.0047)	(0.0085)
Observations	391823	351972	351972	351972	375584
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry x time FEs	Yes	Yes	Yes	Yes	Yes
State x time FEs	Yes	Yes	Yes	Yes	Yes
Size x time FEs	Yes	Yes	Yes	Yes	Yes

Notes: Changes in Employment, Mean Wage and Establishment are computed as in Equation (4). Credit Supply Shocks are defined in Equation (2). See text for additional details.