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Loans and employment: Evidence from bank-specific liquidity shocks

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journal homepage: www.elsevier.com/locate/latchbLoans and employment: Evidence from bank-specific liquidity shocks[☆]Román Acosta^{a,1,*}, Josué Cortés^b^a Northwestern University, United States^b Banco de México, Mexico

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ABSTRACT

This paper investigates the relationship between expansionary credit events and firms' employment decisions. To overcome the endogeneity coming from the supply side of credit we exploited the legal and political framework in Mexico to examine the effects of local governments' prepayment of loans, a situation that leads banks to channel newfound liquidity in firms. Analysis of a novel data set covering a 10-year period showed that a 1-standard-deviation increase in the issuance of new loans increases firms' employment by 2.57 percentage points. Timing of the boost in employment varies, with smaller firms reacting immediately and larger firms reacting four months later. The effects are driven by firms in the manufacturing sector. Our results highlight the importance of the bank lending channel to stimulate employment in the short term, especially for smaller firms. Further, our estimates suggest that the effect of credit on employment could be amplified with policies that promote a more competitive corporate loan market.

1. Introduction

The 2008-2009 global financial crisis showed the importance of understanding the relationship between firms' access to credit and employment. Far-reaching economic consequences can arise due to credit-constrained firms reducing staffing levels despite having sound fundamentals. The literature on this topic has focused on how financial frictions can reduce firms' access to credit and impair job growth (Benmelech et al., 2011; Chodorow-Reich, 2013). However, to complete the picture, we must understand how firms respond to an increased access to credit. Assuming that this relationship is symmetrical to one of employment and credit crunches can lead to costly policy decisions if this assumption does not hold. This becomes especially important for policymakers trying to stimulate job creation through credit expansions (Yellen, 2013). Ideally, we would like to know how these credit-expanding policies affect different types of firms (Bernanke et al., 2010). Unfortunately, studies in this literature are often limited to specific groups of firms for which financial and employment data is available. We provide evidence for a causal relationship between an increase in firms' access to credit and their labor outcomes.²

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² Studying labor outcomes is important because economic policy often targets employment. Some central banks explicitly include maximum sustainable employment as part of their mandate.

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In this paper, we study how employment responds to an increase in firms' access to credit. Our quasi-experimental design exploits institutional characteristics of Mexican credit markets. We identified exogenous bank-specific liquidity shocks originated with the prepayment of local government loans, that increased a bank's supply of credit. To study the effects of this credit increase, we constructed a novel data set that follows the employment and borrowing decisions of firms nationwide from 2009 to 2018. We estimate that a 1-standard-deviation in new credit increases a firm's number of employees by 2.57 percent. The richness of the data allows us to look at firms of different sizes. We found that response times vary significantly across firms. For example, firms with a handful of employees increase employment within the first month of receiving more credit, and the boost in employment continues after 12 months. In contrast, larger firms can take up to four months to hire more staff, and the stimulus fades away in eight months. Finally, we tested whether differences in capital intensity of investment among firms and competition in the corporate credit market explain the mechanisms behind these results.

The first part of this paper develops the identification strategy. Our main challenge was separating banks' loan offerings from firms' demand for credit. To do this, we found variations in the banks' liquidity originating in the prepayment of local government loans. This variation is substantial because, unlike local governments in the US, Mexican local governments have a negligible capacity to issue bonds. Virtually all of their borrowing comes from bank loans. The key aspect of prepayments is that they are unrelated to firms' demand for credit; their main drivers are local political considerations (Benton and Smith, 2017; Kinto, 2014).³ We tested this piece of common knowledge, and, as expected, we found no relation between prepayment events and the economic situation at the local or national levels. Our identification strategy has two features that are unusual in this literature. First, it allows us to study credit expansions instead of the more common credit crunches. Second, it does not rely on a systemic shock to the economy or a structural policy change. Indeed, local government loans are highly concentrated in 32 states and some large municipalities, which means that a single loan can represent a large percentage of a bank's portfolio. At the same time, a single loan is not large enough to disturb the whole financial system. This gives our estimations the advantage of being derived in "normal times," which can be useful for policymakers considering credit expansions to stimulate employment outside of a crisis.

To connect bank-specific shocks to firms, we relied on an important characteristic of the Mexican banking system: its strong relationship banking. This phenomenon occurs when banks find it more profitable to offer new loans to customers with which they have developed a relationship than to look for new customers. More than 53 percent of firms in our sample dealt with at most one bank, and fewer than 7 percent dealt with more than three. This means that firms that have dealt with banks receiving liquidity shocks in the past are more exposed to those shocks. Our first-stage results confirm this. A firm connected to a bank that experiences an increase in liquidity of 1 percent will receive 0.46 percent more liquidity from that bank.

We constructed a new data set with two main sources of information to conduct our analysis. Our first source was a monthly report of corporate loans that banks are required to file to Mexican regulators. This allowed us to follow firms' borrowing behavior for the past 10 years as well as to keep track of their banking relationships. Our second source of information comes from the Mexican Department of Social Security, which keeps monthly records of wages for all workers employed by private firms. This gave us the opportunity to study firms of all sizes, including the smallest firms in the economy. This is important because small firms are an important driver of gross job creation (Bernanke et al., 2010), and studies are often limited in their ability to analyze them because of data restrictions.

To estimate the effect on employment, we used exogenous liquidity shocks to banks as an instrumental variable for the supply of new credit to firms. Our estimates confirm a positive relationship between greater access to credit and increased payroll. With no significant increase in the average or median wages among firms, this effect is mainly driven by new hires: A 1-standard-deviation increase in new credit increases the number of employees by 2.57 percentage points, or around 1.7 employees, for the average firm. To continue with the analysis, we separated firms into four groups according to their size measured in employees. The magnitude of the effects was similar across different firm sizes, but reaction times varied significantly. Firms in the first quartile of the number of employees saw an almost immediate increase in staffing levels after receiving more credit, with the effect persisting over the next 12 months. Conversely, firms in the second and third quartiles took around four months to respond to the credit increase, and this effect fades away within the first 12 months. For firms in the fourth quartile, we did not observe increases in employment after the firms accessed more credit. The largest response of these firms occurred before they actually received the credit.

To understand the heterogeneity in the hiring responses across firms, we explored two mechanisms. First, to understand why smaller firms react faster, we tested the relationship between capital needs and employment growth. The objective was to understand whether labor is growing because of its complementarity with capital or firms are directly hiring new workers. Unfortunately, we did not observe capital expenditures in our data, so we took a different approach. We separated firms into manufacturing and non-manufacturing sectors under the premise that firms in the manufacturing sector have greater capital requirements. Hence, if labor grows because it complements capital, we should observe that manufacturing firms present a slower response to increased credit. Our results for firms in the first quartile show that this is not the case; firms in the manufacturing sector displayed a faster response. This suggests that for these firms, the direct effect of credit on labor is more important. For firms in the second and third quartile, responses in both sectors were more similar. These results suggest that as firms increase in size, their employment growth is more closely related to their capital investments.

The second mechanism we explored helps us understand the anticipated reaction in employment for firms in the fourth quartile. We posit that this effect reflects the banks' ability to choose the firms they prefer to lend to. This is unlikely to be the case for the smaller

³ To make sure that firms' expectations on local elections were not a source of bias in our results, we excluded firms based in the same state in which an election occurred from the prepayment sample.

Table 1
Local government liabilities 2019Q1.

Source	Amount*	%
Bank Loans	526,881	94.8
Bonds	26,944	4.8
Other	2013	0.4

*Millions of Mexican pesos

This table shows the distribution of local government liabilities by source. The data is provided by the Secretaría de Hacienda y Crédito Público, the Mexican equivalent to the Department of the Treasury. By law, all states and municipalities in Mexico must report their financial obligations to the federal government each quarter.

firms in our sample, due to the cost of placing small corporate loans individually. However, as firms grow, it may become profitable for banks to take the time to screen and pick from among their better-performing clients to offer new loans. If this performance correlates with past employment growth, this would explain the anticipated effect of new credit on employment. This explanation would only be plausible in a context of very low competition in the Mexican corporate loan market. To test this hypothesis, we studied the changes in interest rates offered by banks after they experienced liquidity shocks. Interest rate reduction would suggest that banks need to lower their prices to place more credit in the market. We found no evidence of a decrease in interest rates. On the contrary, we found a modest increase, although it is still statistically significant. This increase in the interest rate was greatest for the smallest firms in our sample, which is consistent with previous diagnoses of the Mexican corporate loan market (Banxico, 2015).

Our findings paint a rich picture of the effects of credit expansions for firms in normal times. Our estimations suggest moderate effects on employment, with a rapid response from the smallest firms in the economy. These results suggest that policymakers trying to use the bank lending channel to stimulate job growth are better off focusing on expanding credit to small firms. It is worth underscoring that some firms in our sample took additional credit at an extra cost; this suggests that improving competition in the corporate loan market for small firms could further amplify the stimulating effects of credit.

The rest of the paper proceeds as follows: Section 2 describes the Mexican institutional context in detail and provides an overview of the literature. Section 3 presents the data we used. Section 4 explains the natural experiment setup. Section 5 presents our main results. Section 6 shows the changes in loan conditions that we used to draw inferences about the competitiveness of the Mexican corporate loan market. Section 7 concludes the paper.

2. Mexican institutional context and literature review

Mexican firms' access to bank financing is limited. For example, estimates suggest that approximately half of Mexican firms lacked access to bank financing in 2010 (Banxico, 2015). The Mexican banking system is also highly concentrated. Using the corporate loan census by the Comisión Nacional Bancaria y de Valores (CNBV), we observed that the 10 largest banks made 90 percent of the loans to Mexican firms.

For these reasons, relationship banking is strong in the Mexican economy. In Mexico, only 17 percent of firms with bank loans from 2009 to 2018 had outstanding balances with more than two banks (see Table 3). This suggests that firms in Mexico may be exposed to bank-specific economic shocks. To find these bank-specific shocks, we turn, to another peculiar characteristic of the Mexican financial system: the deep dependence of local governments on bank loans to finance their projects.

Unlike states and municipalities in the United States, local governments in Mexico rarely issue bonds to finance infrastructure projects. According to the local government debt registry of the Mexican Ministry of Finance, only 4.8 percent of the total debt held by local governments was in the form of bonds. In contrast, almost 90 percent of total debt held by states and municipalities in Mexico is in the form of bank loans.⁴

As a result of this setup, banks are the actors in the financial system that are most exposed to changes in local governments' management of their debt portfolios. The Mexican literature has documented the dependence of debt management decisions on political variables from an empirical standpoint. Studies show how Mexican local governments' debt-related decisions strongly correlate to the local political context in their states (Kinto, 2014) and municipalities (Benton and Smith, 2017). These results are unsurprising given the Mexican institutional framework. For example, all local governments planning to refinance debt need the approval of the local congress (for states) or the town hall (for municipalities). Hence, any decision regarding the government's debt passes through the legislative process. Institutional features such as this allow us to use local government debt prepayment decisions as a source of variation in the available liquidity of banks that is plausibly uncorrelated with the firms' demand for credit, especially out-of-state firms.

The political economy literature has also explored the strong relationship between debt management and politics from a theoretical standpoint (Alesina and Passalacqua, 2016). For example, political budget cycle models explain how governments are induced to cut taxes and/or increase spending before an election, potentially acquiring more debt (Drazen and Eslava, 2006; Rogoff, 1990; Rogoff

⁴ Available online at <http://disciplinafinanciera.hacienda.gob.mx>.

and Sibert, 1988). Other models argue that public debt can be used as a strategic variable by politicians uncertain about their future tenure in office (Aghion and Bolton, 1990; Alesina and Tabellini, 1990; Lizzeri, 1999). Finally, this literature has also documented the driving force of the common pool problem in fiscal policy, in which the agents in charge of deciding the level of spending do not fully internalize its costs, leading to excessive spending (Baron and Ferejohn, 1989; Battaglini and Coate, 2008; Weingast et al., 1981).

From the perspective of a bank, a debt prepayment means that the bank is at risk of losing a customer. Indeed, when a state or municipality decides to prepay its debt, the process usually involves refinancing by acquiring debt with different banks.⁵ Unlike the relationship banking observed at the firm level, the financing needs of states and municipalities are so great that most of them contract debt with multiple banks. This gives us the opportunity to trace the events in which a bank loan is prepaid unexpectedly before its contractual maturity. This in turn generates an unexpected excess cash flow in the bank's balance sheet. If the bank wants to keep making money, it needs to use this cash to purchase an asset that will compensate for the lost revenue from the local government.

Then, because corporate loans are among the assets that banks can purchase, these prepayments may increase the bank's willingness to lend, effectively relaxing the bank-specific credit supply. In this sense, the prepayments from the local governments act as a positive liquidity shock to firms. In Section 5, we document that the sensitivity of banks' lending to firms is around 0.46 percentage points for each percentage point increase in local government debt prepayment.

Having established the relationship between local government prepayments and a bank's supply of credit, we analyze their effects on firms' employment decisions. To do this, we took advantage of the Mexican regulatory setup. All private firms in Mexico are required to register their employees in the Mexican Institute of Social Security (Instituto Mexicano del Seguro Social, known by its Spanish acronym, IMSS). This institution is responsible for the administration of public health care and other worker benefits such as childcare, pensions, and disability insurance. Hence, IMSS data contains demographic information and employment status for all people employed in private firms. By linking the CNBV corporate loan data set and the IMSS's monthly employee records, we studied how changes in the firms' access to finance affected their employment outcomes.

The corporate finance literature has documented the effects of credit crunches and institutional changes on employment. For example, Berton et al. (2018) documented the effect of credit supply-side shocks on job contracts. Benmelech et al. (2011) documented how financial frictions and access to credit affect firm-level employment decisions, as well as aggregate unemployment outcomes. Chodorow-Reich (2013) used dispersion in a lender's health after the Lehman Brothers bankruptcy to track the effect of access to finance on a firm's employment decisions. Similarly, Bentolila et al. (2013) exploited solvency problems from Spanish banks in the Great Recession. Finally, Fonseca and Van Doornik (2022) found evidence that a credit expansion following a bankruptcy reform in Brazil led to reallocation of skilled labor from financially unconstrained to financially constrained firms. These results are related to ours in the sense that they provide empirical evidence in favor of the hypothesis that firms' access to finance affects their labor decisions.

The magnitude of our estimates is smaller than others found in the literature. Berton et al. (2018) estimated an employment elasticity of 0.36 in response to a credit shock of 0.36. The caveat is that in their study, the shock to credit was negative. Benmelech et al. (2011) reported estimates of 0.017. This was closer to our estimates in magnitude; however, in their setup, the variation came specifically from firms having debt of at least 5% of their total assets approaching maturity. Finally, even though Fonseca and Van Doornik (2022)'s results are not comparable to ours, it is notable that they also found a positive relationship between more access to finance and employment decisions, at least concerning skilled workers.

These studies largely exploited significant financial or economic events or institutional changes to analyze firms' responses to changes in access to finance. Most of these events involved negative shocks to financing access and occurred in periods of overall economic and financial distress. In contrast, we examine positive shocks in firms' access to credit, looking at their responses to changes in access to finance in crisis-free times. We analyze the period from the second semester of 2009 to the end of 2018, an economically stable period in Mexico with modest economic growth. Our work uses these lenses to shed light on the importance and effects of access to finance for firms.

3. Data

We used two sources that provide loan- and firm-level data spanning a 10-year period (2009–2018). The first source is a monthly report of all loans issued to firms by financial institutions operating in Mexico. This report is mandatory and is enforced by the CNBV, the Mexican equivalent of the U.S. *Securities and Exchange Commission*.

This report tracks two aspects of the loans. First, we capture the terms and conditions of all new loans, including the issuing financial institution; the receiving firm; and the agreed-upon financial terms such as authorized funds, interest rate, and maturity of the loan. Second, we track the monthly status of each loan issued to the same firms in the past. Regulations require financial institutions to report whether a loan was paid before it reached the maturity date, and whether the prepayment was due to a restructure of the loan, a refinancing, or other motives.

This last condition allowed us to track prepayments of local government loans and differentiate them from restructures or loans that were terminated for other reasons before reaching maturity. Note that this database includes data for both firms and local

⁵ From the perspective of the local government, this process can be a refinance (if it uses funds from a different bank), a prepayment (if it uses its own funds to pay for the loan), or a restructure (if it stays with the same bank but improves on the terms of the credit contract). From the perspective of the bank, the first two situations constitute a prepayment, which is the case we are interested in examining.

governments, which allows us to track all prepayments of local governments. Also, the loan-level data allows us to construct a measure of proximity between firms and banks: We say that a firm is connected to bank i at time t if the firm had any loans with bank i prior to period t .

The second dataset is the administrative records of employees working for Mexican firms. These records contain monthly information about Mexican workers' employment, including their gender and daily salary and their employer's name, tax identification number and other information, including geographical location. To link these records with the loan census data, we signed a collaboration agreement between the IMSS and the CNBV to share the tax identification numbers of firms with the Bank of Mexico. In this way, we gained unprecedented access to the merged data sets for the firms operating in Mexico in the last months of 2018. This allowed us to analyze a subpopulation of more than 118,000 Mexican firms. It is important to note that there are also potential issues with using this administrative data. First, we depend on the agencies' record-keeping diligence. One potential issue is that currently operating firms are more likely to appear in our data if agencies have improved their record keeping in recent years. For example, it could be the case that the agencies did not know the tax ID of a firm in 2010 but updated their records in 2018. If the firm survived until 2018, we had access to their tax ID; if it did not survive, we would be unable to match it in the data. In our case, the sign of the bias depends on what we think about surviving firms. If we believe that financially healthy firms are more likely to survive and less likely to respond to greater access to finance, our results may underestimate the effect of credit on employment.

4. Methodology and natural experiment setup

We exploited the characteristics of Mexican capital markets, the banking system in Mexico, the political factors governing public debt decisions at the local government level, and the strong presence of relationship banking between firms and banks. In this section, we review each of these dimensions and explain how they build up to the main argument in our identification strategy.

Before describing each dimension in detail, we discuss our methodology in the next subsection.

4.1. Methodology

Our empirical strategy used liquidity shocks as an instrumental variable for new loans. We did this because an OLS estimation of the effect of new loans on firm outcomes has two potential biases from the demand side. First, growing firms may demand more credit to continue their growth. Second, troubled firms may demand more credit to overcome hardships. Therefore, we used bank liquidity shocks to instrument for the new loans received by firms.

To test the first stage, we used the following model:

$$Prepayment_{it} = \beta_0 + \beta X_{it} + \gamma_i + \eta_{m(t)} + \epsilon_{it}$$

where $Prepayment_{it}$ takes the value of 1 when state i makes a prepayment in period t . X_{it} is the annual percentage changes on different economic indicators. All specifications include monthly fixed effects to control for seasonality effects on the corporate debt market. Errors are clustered at the year level. Table 7 presents the results of this exercise.

The second stage of our model comes from regressing different staffing outcomes on the instrumented amount of money coming from new loans. The exact model is:

$$Y_{it} = \beta_0 + \beta_1 NewLoans_{it} + \gamma_i + \eta_t + \epsilon_{it}$$

where Y_{it} is the daily payroll, the number of employees, the average wage, the median wage, and the average gender in firm i in period t . $NewLoans_{it}$ refers to the amount of money that firm i received in period t from new loans. γ_i and η_t are firm- and time-fixed effects, respectively.

We instrumented the New Loans variable using the local government prepayments and estimated the model using a two-stage least square method. There are possible biases that we wish to address with this strategy. First, firms that are doing well and growing might demand more loans to keep growing. Second, firms that are not doing well might demand more loans to stay afloat. In the former scenario, new loans would be correlated with a growth in staffing outcomes, while in the latter new loans would be correlated with lower growth or even a decrease in staffing outcomes.

4.2. Mexican capital markets and local governments

Unlike local governments in the United States, states and municipalities in Mexico finance their budgets mainly via bank loans, as shown in Table 1. In the first quarter of 2019, almost 95 percent of local government liabilities were held by banks in the form of loans. Fewer than 5 percent of total liabilities were held by the public. In contrast, according to the Organisation for Economic Co-operation and Development, almost 60 percent of the liabilities held by US local governments in 2014 were in the form of debt securities.

These aspects of the Mexican market are important for two reasons. First, the large market for local government loans means that there is an elevated chance that prepayments will be large enough to relax a bank's supply of credit. Second, the data shows that local government lending is an important business for banks. According to public CNBV data, local government lending represents around 20 percent of Mexican banks' commercial lending portfolios.

4.3. Politics as a driver of local fiscal and financial decisions

The main motivation behind using local government debt prepayment, instead of any other type of debt prepayment, is that debt management decisions are strongly driven by political factors: The party composition of local congresses, changes in the party in power, and electoral timing are among the most cited drivers in both the empirical and theoretical literature.⁶

The following two excerpts of news articles exemplify the findings of the Mexican political economy literature. In the first, the governor of the State of Mexico argues that a debt restructure (which he refers to as a “refinance”) should take place immediately because of a looming shift of power to the opposing party: “Before losing majority in local Congress, the PRI governor Alfredo del Mazo asks legislators for the authorization to refinance the state’s public debt for more than 43 billion pesos” (Mendoza, 2018). In the second article, the new governor of the state of Quintana Roo argues for restructuring (again, using the term “refinance”) state debt based on bad budgetary management by his predecessors in the opposing party: “Governor Carlos Joaquín González, informed that he has approached financial institutions to refinance the state’s debt of 19 billion pesos... the transition team informed that the previous administration left the state in bankruptcy with a deficit of 2.7 billion pesos... ” (Vázquez, 2016).

We used the prepayment episodes in our sample to test the idea that decisions to prepay a local government loan are not related to a firm’s demand for credit at the national level we use the prepayment episodes in our sample. We constructed a panel of the 32 states in Mexico across 114 months from July 2009 through December 2018, with economic indicators at the national and state levels. We expected that none of the national-level economic variables would be good predictors of a local government loan prepayment.

The model we uses was:

$$Prepayment_{it} = \beta_0 + \beta X_{it} + \gamma_i + \eta_{m(t)} + \epsilon_{it}$$

where $Prepayment_{it}$ takes the value of 1 when state i makes a prepayment in period t . X_{it} is the annual percentage changes on different economic indicators. All specifications include month-fixed effects to control for seasonality effects on the corporate debt market, and errors are clustered at the year level. Table 7 presents the results of this exercise.^{7,8}

Column 1 shows the results of the first specification using the unemployment rate. The estimated coefficient was 0.00005, and was not statically or economically significant. Column 2 adds the state unemployment rates and state-level fixed effects, but the results are similar: we observed no apparent relation between unemployment at the national or state level and prepayment episodes. Column 3 repeats the same exercise using economic indicators as proxies for monthly GDP behaviour. Again, none of these variables was statistically or economically significant enough to explain the prepayment episodes.

The next specification built upon the previous one by adding the reference interest rate and the price of Mexican oil. We included these two variables for different reasons. The reference interest rate is an important financial determinant of a loan’s total cost and it is especially important in a debt refinancing process. On the other hand, oil revenues make up a significant portion of government funds, including state and municipal governments. Column 4 presents the estimations for this specification, in which none of these variables is statistically or economically significant. Finally, Column 5 presents the results of the same specification as Column 4, but with the addition of the unemployment variables. The results did not change.

The findings in the literature and the evidence we provide support our identifying assumptions: Local government prepayments increase the issuance of new bank loans, and the cause of these prepayments is orthogonal to firms’ demand for credit. That is, the reasons that lead a local government to prepay its debt do not include the business conditions faced by firms around the country. Further, to eliminate any concerns about how local political factors may correlate with firms’ demand for credit, we tracked the geographical source of shocks and excluded the firms in the same regions from the analysis of the impact of a given shock. Certain characteristics of the Mexican banking system helped us do that (as explained in Section 4.4).

Having presented the arguments in favor of our identification strategy we also want to point out that our strategy has potential issues that, although we attempted our best to address, should be considered before interpreting our results. Large states can have spillover effects on neighboring states. This means that even though we have removed firms in the state in which each shock originated, we might still find them to be affected by neighboring states.

4.4. Banking system in Mexico

Two facts about the Mexican banking system provide reassurance that our identifying assumption holds. First, lending to local governments is highly concentrated in a few banks. This is not exclusive to local government lending. The Mexican banking system is highly concentrated, and most business is conducted by the seven largest banks.⁹ Also, because these banks tend to be large, they have operations throughout the country, as shown in Table 2.

For the purposes of our strategy, the national presence of banks is the second important characteristic of banking and local government loans in Mexico. The nationwide networks mean that the sources of the prepayments are scattered across the country,

⁶ For more on this literature, see Section 2.

⁷ Our results are robust to using expectations for economic activity. Unfortunately, we do not have access to indicators of expectations at the local level.

⁸ Our results are robust to probit and logit specifications.

⁹ According to publicly available data of the CNBV, in 2019Q1 the 7 largest banks held more than 75% of the total commercial debt portfolio (corporate, local government, residential, and consumption) in the country.

Table 2
Geographical presence of banks in Mexico.

Bank States (32)	A	B	C	D	E	F	G	H	I	J	K	K	M	N	O
	32	32	32	32	30	25	9	20	32	9	21	32	31	22	22

This table shows the number of states in which Mexican banks operate. We considered only banks that did business with local governments. These banks were also in business with more than 97% of the firms in the sample. There are 32 states in Mexico, and we say that a bank has presence in a state if we observed it conducting business with at least one firm at any point in our sample (July 2009 through December 2018).

Table 3
Firm-bank business relationships in Mexico.

Total Banks	N	%	Cumulative
0	3272	2.8	2.8
1	66,936	56.6	59.4
2	28,415	24.0	83.4
3	11,902	10.1	93.4
4	4867	4.11	97.5
5+	2908	2.5	100.0

This table shows the distribution of the number of banks with which a firm has dealt in our sample. We say that firm *i* deals with *n* banks if throughout our sample, we observed loans from *n* different banks being issued to firm *i*. Firms with a value of zero dealt only with banks that do not issue local government loans.

making it possible for us to limit the analysis to the variation of liquidity at the bank level coming from prepayments that originated out of state. Fig. 2 shows the states of origin and the prepayment used in our analysis.¹⁰

4.5. Relationship banking

Using the source of variation in a bank’s liquidity as a means to analyze firms’ responses to increased access to finance requires firms to be heterogeneously exposed to bank-specific liquidity shocks. We use relationship banking as a source of this heterogeneity.

The CNBV’s loan census data shows that 56.6 percent of Mexican firms dealt with at most one bank, and that more than 93 percent have dealt with at most three banks throughout the 10 years of our sample. Access to more banks is highly correlated with firm’s size, meaning that only very large firms in Mexico have access to several banks. Table 3 shows the distribution of the number of banks with which a firm has dealt in the 10 years of our sample. However, we used a slightly different definition to link firms with banks: We defined firm *i* as being linked to bank *j* at time *t* if firm *i* was a client of bank *j* at any point on or before period *t*.¹¹

5. Results

We have organized the results into three subsections. First, we show how sensitive the issuance of new loans is to the prepayment of local government loans. We found an elasticity of 0.46, and that issuance respond quickly. Second, we estimated the effect of the instrumented measure of new loans on staffing. We found a positive but heterogeneous effect that depends on the firm’s size. Finally, we present an exercise to estimate how long firms take to respond to this new source of finance, in terms of employment. Again, we saw a heterogeneous effect depending on the firm’s size. This ranged from a quick and sustained positive effect on small firms to a small and shorter-term effect on larger firms.

5.1. Local government prepayments and the issuance of new loans

Table 6 shows the result of regressing the monthly amount of firms’ finances that comes from new loans made possible by a local government loan being prepaid to a bank. The exact model is as follows:

$$NewLoans_{it} = \beta_0 + \beta_1 I_{it} * Prepayment_t + \gamma_i + \eta_t + \epsilon_{it}$$

¹⁰ Note that the process of making a loan offer of this size is considerably more time consuming from the banks’ point of view compared to receiving prepayment. This means that any effects that we capture in the long term come at the risk of a crowding-out effect at the bank level. For short-term effects, this is less of a concern. Given that our estimates appear in the first months after we observed prepayments, we are less concerned about the crowding-out effect.

¹¹ Note that condensing our definition of banking relationships would potentially exclude many firms from the treatment group, thus inflating our estimates. We believe this definition, including the sample time span, is more conservative.

Table 4
Geographical presence of banks in Mexico per quarter.

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O
2009Q3	32	0	32	32	32	31	12	27	20	14	16	32	18	10	1
2009Q4	32	1	32	32	30	32	7	26	22	11	15	32	28	11	1
2010Q1	32	32	32	32	32	32	6	24	18	9	17	32	28	10	1
2010Q2	32	16	32	32	31	32	7	26	22	11	17	32	28	12	2
2010Q3	32	32	32	32	31	31	4	28	20	10	15	32	25	10	1
2010Q4	32	32	32	32	31	32	6	28	19	13	17	32	23	13	1
2011Q1	32	32	32	32	30	32	2	28	21	10	16	32	30	11	21
2011Q2	32	32	32	32	29	31	8	28	18	10	17	32	29	14	17
2011Q3	32	32	32	32	28	32	7	28	21	13	19	32	27	15	24
2011Q4	32	32	32	32	29	31	8	26	22	10	20	32	27	17	18
2012Q1	32	32	32	32	30	31	4	27	21	12	17	32	31	15	17
2012Q2	32	32	32	32	30	32	11	29	19	8	21	32	25	17	22
2012Q3	32	32	32	32	31	32	7	26	21	12	24	32	30	17	23
2012Q4	32	32	32	32	30	29	4	26	20	14	22	32	27	18	22
2013Q1	32	32	32	32	31	32	4	26	22	10	26	32	27	18	26
2013Q2	32	32	32	32	30	32	7	28	24	10	26	32	27	19	22
2013Q3	32	32	32	32	30	30	6	26	25	12	28	32	25	18	22
2013Q4	32	32	32	32	31	32	3	27	26	6	27	32	29	19	24
2014Q1	32	32	32	32	30	32	3	16	24	11	28	32	22	14	27
2014Q2	32	32	32	32	29	32	6	14	28	7	29	32	28	18	13
2014Q3	32	32	32	32	32	32	5	12	26	13	30	32	25	15	14
2014Q4	32	32	32	32	29	32	5	12	23	10	30	32	24	18	15
2015Q1	32	32	32	32	30	30	2	11	25	11	30	32	23	16	20
2015Q2	32	32	32	32	29	31	4	10	24	12	27	32	25	19	20
2015Q3	32	32	32	32	31	21	3	11	25	14	30	32	24	20	20
2015Q4	32	32	32	32	32	29	8	9	24	12	28	32	28	24	19
2016Q1	32	32	32	32	30	19	1	8	23	10	28	32	24	17	18
2016Q2	32	32	32	32	30	21	3	8	22	10	26	32	27	24	21
2016Q3	32	32	32	32	32	16	5	10	27	11	29	32	26	20	19
2016Q4	32	32	32	32	32	19	4	9	29	10	28	32	26	22	23
2017Q1	32	32	32	31	29	18	5	10	30	14	25	32	24	16	25
2017Q2	32	32	32	31	29	11	3	12	32	8	24	32	24	19	25
2017Q3	32	32	32	31	29	8	3	10	30	9	23	32	25	17	22
2017Q4	32	32	32	31	32	14	5	10	29	11	24	32	26	16	24
2018Q1	32	32	32	31	31	11	4	13	31	15	24	32	23	17	19
2018Q2	32	32	32	31	29	13	2	15	32	16	23	32	24	12	23
2018Q3	32	32	32	31	29	14	0	11	31	13	28	32	25	17	23
2018Q4	32	32	32	31	31	13	0	11	31	15	26	32	25	20	22
Average	32	30	32	32	30	26	5	19	24	11	24	32	26	16	18

This table shows the number of states in which each Mexican bank issued at least one loan per quarter from July 2009 to December 2018. We considered only banks that did business with local governments. There are 32 states in Mexico.

where $NewLoans_{it}$ refers to the total amount of money that firm i received in month t from new loans.¹² I_{it} is a vector, in which the j 'th entry indicates with a 1 that firm i has done business with bank j before or on period t and is 0 otherwise. $Prepayment_t$ is a vector in which the j 'th entry has the total amount of prepayments coming from bank j 's local government loans. Notably, that in the construction of the $Prepayment_t$ variable, we did not include the prepayments from local governments in the same state or municipality in which the firm was located, nor did we include prepayments that were too small or too large.¹³ So, for example, if firm i is in Tijuana, any prepayments coming from local governments in Baja California were not considered for the $Prepayment_t$ values corresponding to firm i . Finally, γ_i and η_t are firm-and time-fixed effects, respectively.

The estimated coefficient for the log value of the prepayment is 0.4552, which can be interpreted as an increase of 0.4552 percent in the amount of new loans received by firms, with a 1 percent increase in prepayments coming from local governments. To check how long it takes the prepayment to affect the supply of loans, we included a specification with lags of the prepayment variable in Column 2. We observe statistically significant and positive effects of the lags but with limited economic significance: a 0.032 percent increase for every 1 percent increase in the prepayments after one month.

We were also interested in verifying if any reaction in the issuance of new loans suggested that banks could anticipate the prepayments. To see this, we included a specification with three leads of the prepayment variable. Column 3 shows the estimated

¹² Note that we are trying to measure the effect of access to more funds, rather than the actual drawdown of loans. We use increases in total access to credit, regardless of whether it is a new credit line or not.

¹³ Small prepayments are defined as those for which the prepaid amount is less than 1 percent of the bank's yearly issuance of loans in that year. Big loans are defined as those for which the prepaid amount is more than 100 percent of the bank's yearly issuance of loans in that year. The results do not change substantially if we use the whole sample of prepayments.

Table 5
Relationship banking per quarter in Mexico.

	1	2	3	4	5	6	7 or more
2009Q3	10,755	1270	214	45	11	4	3
2009Q4	9770	1103	189	49	9	8	4
2010Q1	9312	1022	171	40	12	2	4
2010Q2	10,781	1256	205	46	19	5	3
2010Q3	11,515	1486	229	69	16	5	3
2010Q4	12,128	1461	240	58	22	7	7
2011Q1	11,982	1320	222	51	21	3	3
2011Q2	11,539	1426	258	52	31	7	3
2011Q3	11,453	1301	248	75	26	4	4
2011Q4	13,499	1595	277	78	24	9	3
2012Q1	14,578	1783	302	77	28	12	3
2012Q2	14,010	1638	323	69	19	6	5
2012Q3	15,737	1848	307	84	31	5	5
2012Q4	14,802	1806	368	66	28	6	5
2013Q1	13,713	1591	310	83	24	11	3
2013Q2	14,687	1824	343	83	27	15	-
2013Q3	15,252	1907	364	85	27	12	4
2013Q4	16,312	1913	378	100	33	17	4
2014Q1	15,996	1798	313	79	25	4	1
2014Q2	16,525	1966	372	105	29	11	2
2014Q3	16,371	1920	342	123	24	2	6
2014Q4	20,767	2572	475	122	33	13	1
2015Q1	17,387	2009	389	116	17	8	5
2015Q2	18,571	2039	406	117	44	6	3
2015Q3	19,339	2140	413	121	26	9	5
2015Q4	19,378	2272	462	115	35	11	4
2016Q1	22,122	2552	449	114	27	12	7
2016Q2	18,559	2096	401	144	37	6	2
2016Q3	18,821	2229	385	125	46	11	7
2016Q4	19,302	2159	402	112	23	14	7
2017Q1	15,397	1454	237	35	13	1	1
2017Q2	15,444	1537	258	51	10	4	-
2017Q3	15,077	1443	240	52	10	1	-
2017Q4	14,497	1366	249	31	10	5	2
2018Q1	14,699	1337	213	39	8	-	2
2018Q2	15,475	1486	225	45	12	3	2
2018Q3	15,176	1300	233	45	12	2	-
2018Q4	14,767	1330	222	34	7	3	-

This table shows the distribution of firms that received loans from different numbers of banks per quarter from July 2009 to December 2018.

effects. Similar to the analysis with the prepayment lags, we observed a statistically significant but economically small effect for the leads. Finally, Column 4 includes both sets of lags and leads and confirms the previous results.

To ensure that we did not miss a relevant time period, we analyzed a window of 12 months before and 12 months after the prepayment. Fig. 1 shows that the largest effect of prepayments took place in the same period when prepayment occurred. This is consistent with what we know about the timing of when prepaid loans are reported: Banks only report a loan as prepaid after the local government has transferred the funds. This means that by the time we observe the prepayment in the monthly CNBV report, the bank has known for some time that the loan was in the process of being prepaid.

In general, it is possible that local governments are cash constrained after prepaying their debt. This would raise validity concerns if public investment were affected after a prepayment, for example. We diminished the risk of endogeneity by excluding firms in the same state in which the prepayment occurred from the sample for that period. Further, both our data and anecdotal evidence suggest that local governments use new loans to prepay old loans, i.e., most prepayment events are refinancing events. This leaves local governments' cash holdings the same or better (usually better due to lower interest rates or lower capital payments, due to having a longer term to maturity).

Finally, it is important to discuss the role of government-owned banks in Mexico. According to the "Secretaría de Hacienda y Crédito Público," the most important government-owned lender is BANOBRAS.¹⁴ This bank is in charge of financing development projects in Mexico, and states and municipalities often turn to this institution for funds to refinance their loans. Clearly, the business model of this publicly owned bank is different from the business model of private institutions. Furthermore, BANOBRAS does not finance firms in Mexico, so it is not included in the set of banks that received liquidity shocks in our panel.

¹⁴ Please see hacienda.gob.mx for more on BANOBRAS role in financing local governments in Mexico.

Table 6
Loan issuance sensitivity to prepayments.

Variable	Log(New Loans + 1)			
	(1)	(2)	(3)	(4)
Log(Prepayment + 1)	0.4552a (0.0007)	0.4478a (0.0017)	0.3957a (0.0022)	0.4416a (0.0019)
Lag 1		0.0368 (0.0017)		0.0320 (0.0010)
Lag 2		0.0316 (0.0011)		0.02618 (0.0010)
Lag 3		0.028 (0.0010)		0.0244 (0.0010)
Lead 1			-0.0020 (0.0013)	0.0396 (0.0011)
Lead 2			-0.0109 (0.0014)	0.0347 (0.0011)
Lead 3			0.0023 (0.00125)	0.0287 (0.0010)
Time Fixed Effects	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES
N	4,598,255	4,598,255	4,598,255	4,598,255
Firms	118,300	118,300	118,300	118,300

Parentheses indicate standard errors clustered at the firm level

This table shows the result of regressing the monthly amount of finance received by a firm coming from new loans against the prepayment of a local government loan to a bank. The exact model takes the following form:

where $NewLoans_{it}$ refers to the total amount of money that firm i received in period t from new loans. I_{it} is a vector in which the j 'th entry indicates with a 1 that firm i has done business with bank j before or during period t and 0 otherwise. $Prepayment_t$ is a vector in which the j 'th entry has the total amount of prepayments coming from bank j 's local government loans. The data goes from July 2009 to December 2018. The letters a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7
Prepayment decisions and national economic indicators.

Variable	Prepayment Episode at State Level				
	(1)	(2)	(3)	(4)	(5)
National Unemployment	0.05 (0.94)	0.10 (0.93)			-0.91 (0.91)
State Unemployment		-0.08 (0.25)			-0.09 (0.28)
National GDP Index			3.77 (2.18)	4.42 (3.48)	4.11 (3.29)
State GDP Index			6.41 (6.28)	6.95 (7.28)	6.68 (7.56)
Reference Interest Rate				-0.35 (0.22)	-0.52 (0.031)
Oil Price				-0.07 (0.14)	0.00 (0.14)
Month Fixed Effects	YES	YES	YES	YES	YES
State Fixed Effects	NO	YES	YES	YES	YES
N	3,264	3,264	3,648	3,648	3,264
R-squared	0.0129	0.0632	0.0613	0.0688	0.0695

Parentheses indicate robust standard errors clustered at the year level

This table shows the estimated effects of different national and local economic indicators on the probability of observing a prepayment. We included state and month fixed effects and cluster standard errors at the year level. The letters a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

5.2. Case-by-Case study

The results in the previous subsection suggest that banks respond quickly to shocks in liquidity, with the greatest effect occurring in the same month when the prepayment is reported. In this section, we provide supplementary evidence of this finding. To this end, we adapted the methodology used by [Khwaja and Mian \(2008\)](#).

Table 8
Descriptive statistics.

	N	Mean	Std. Dev.	Q1	Median	Q3
Firms	118,300					
Periods	114					
Employees	4,598,321	66.15	638.36	6	15	38
Payroll (daily)	4,598,321	985.96	317,389.40	2,598.12	2598.12	7366.2
Average wage (daily)	4,598,321	218.15	188.96	113.04	164.76	255.79
Median wage (daily)	4,598,321	188.23	181.36	99.29	139.36	212.93
Gender (1 = male, 2 = female)	4,598,321	1.34	0.25	1.14	1.3	1.5
Loan issued (Mexican pesos)	4,598,321	1,261,217	31,200,000	0	0	0

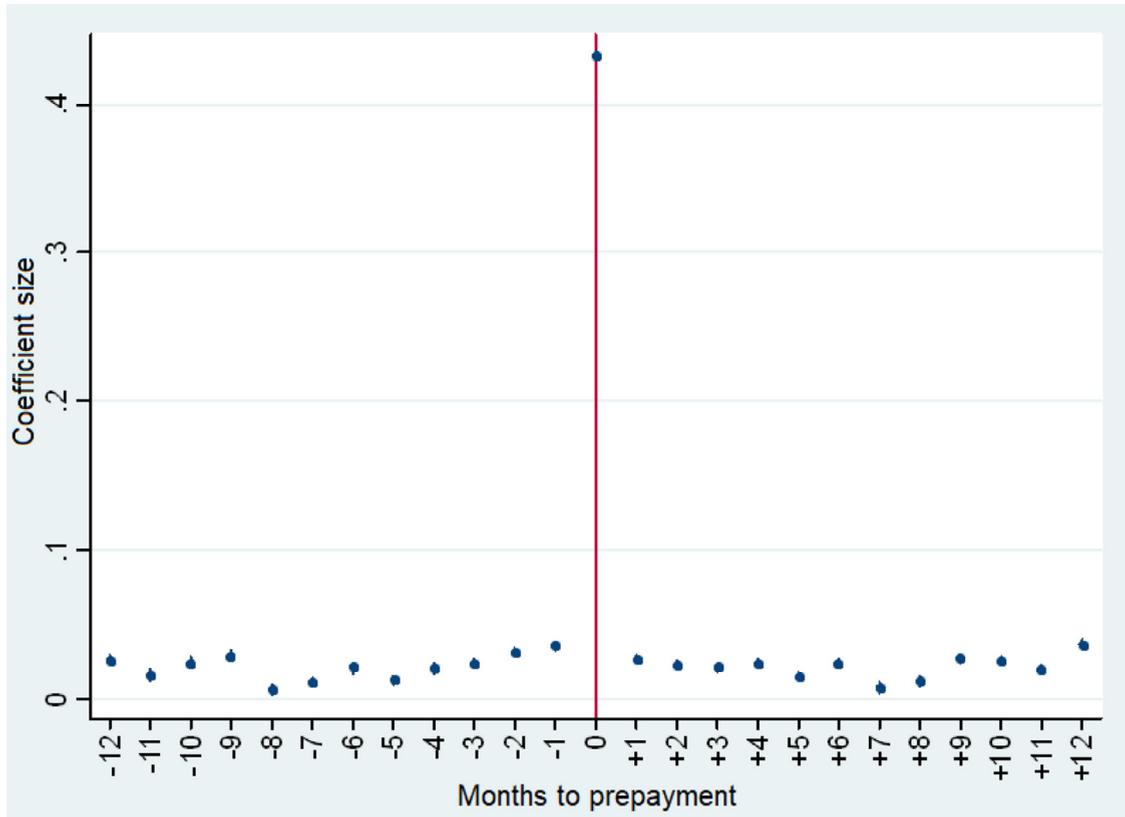


Fig. 1. New loan issuance after prepayment. The first summation includes 12 lags of the *Prepayment* variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-months window, and we include firm and period fixed effects. $NewLoans_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d Prepayment_{i,t+d} + \beta_0 Prepayment_{i,t} + \sum_{d=1}^{d=12} \beta_d Prepayment_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$ where $NewLoans_{it}$ refers to the total amount of money that firm i received in period t from new loans. $Prepayment_{it}$ is the total amount of prepayments that banks in businesses with firm i at period t received from local governments. The data are from the period of July 2009 to December 2018. All variables are in logs.

The approach consisted of taking firms with multiple banking relationships and calculating the growth in their loans from each bank after they received the liquidity shock. By doing so, we could test whether the same firm showed a larger increase in lending from the bank facing a relatively greater increase in their liquidity.

In our case, however, banks received a liquidity shock at different time points. Hence, to implement this design, we analyzed separate time windows. Each window has the characteristic that no bank received a liquidity shock immediately before the beginning or immediately after the end window.¹⁵ The ideal window size was two months: one month before and one after the liquidity shock. In this way, we alleviated the concern that liquidity shocks at different periods affected the estimation. The exact specification is as follows:

$$Y_{ij} = \beta_0 + \beta_1 Shock_j + \gamma_i + \epsilon_{ij}$$

¹⁵ We consider only months immediately before or after the limits of the window because our estimates in the previous subsection suggest that most of the response to the liquidity shock happens in the same month.

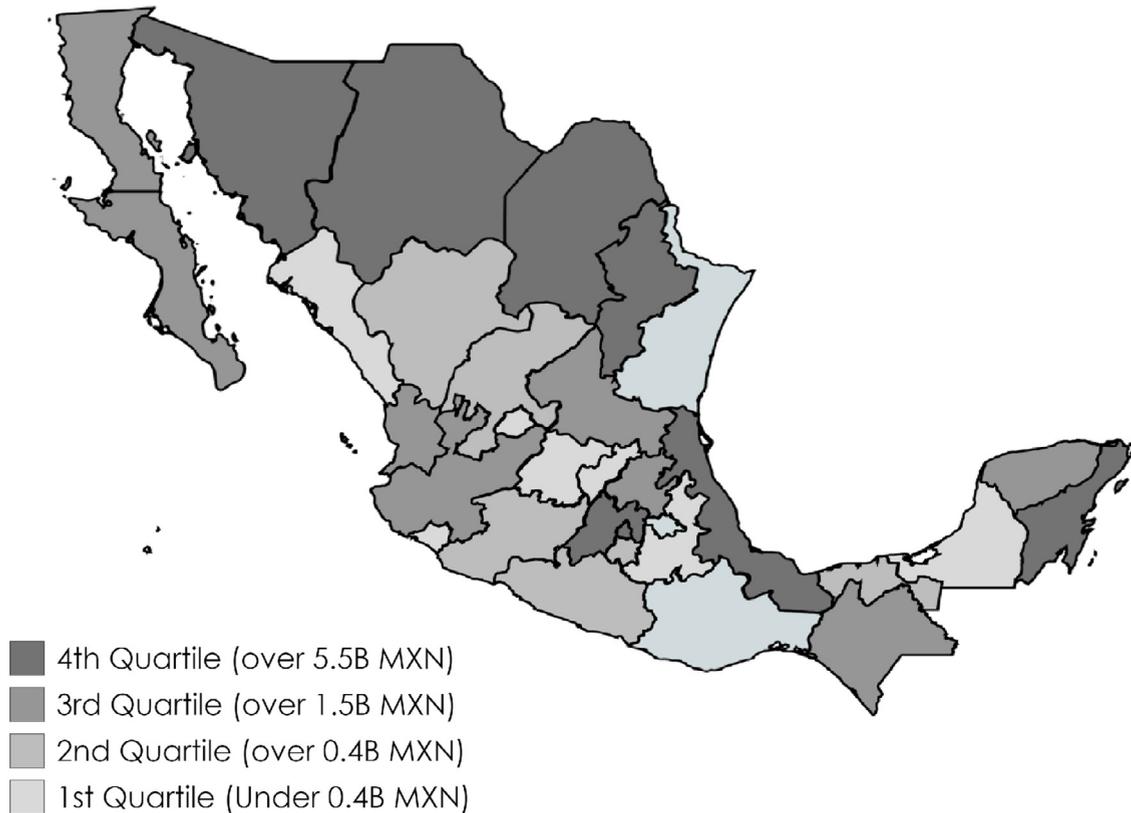


Fig. 2. Geographical distribution of prepayments in Mexico.

Table 9
Loan growth after liquidity shock.

Variable	Windows		
	Aug16 - Sep16 (1)	Oct17 - Nov17 (2)	Oct18 - Nov18 (3)
Log(Prepayment + 1)	0.263a (0.039)	0.105a (0.014)	0.093a (0.017)
Constant	-0.022 (0.039)	0.138a (0.033)	2.04a (0.17)
Firm Fixed Effects	YES	YES	YES
N	27,604	20,790	5,542
R-squared	0.503	0.493	0.431

Parentheses indicate robust standard errors clustered at the firm level

This table shows the results of regressing liquidity shocks to banks (i.e., the local government loan prepayments) on the new loan growth at the firm level for firms that dealt with two banks. The exact model is as follows: where Y_{ij} is the log difference between new loans received by firm i from bank j before the shock and those received after the shock. $Shock_j$ is the liquidity shock received by bank j coming from the prepayment of local government liquidity shocks. γ_i is a firm-fixed effect. We clustered the standard errors at the firm level, and all variables are in logarithms. The letters a, b, and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

where Y_{ij} is the log difference of new loans received by firm i from bank j from before and after the shock. $Shock_j$ is the liquidity shock received by bank j coming from the prepayment of local government liquidity shocks, while γ_i is a firm-fixed effect.

Table 9 presents the results of this exercise for the ideal window size of two months. The sample was restricted to firms having a relationship with a bank that was shocked. Also, we restricted the sample to firms related to exactly two banks.¹⁶ For the three time

¹⁶ The results did not change significantly when including firms with a relationship with more than two banks. We are interested in firms that had relationships with only two banks because they tend to be smaller firms, which will be the focus of our analysis later on.

Table 10
Employment response to new loans.

Variable	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	Log(Payroll + 1)				Log(Employees + 1)				Log(Mean Wage + 1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\log(NewLoans + 1)$	0.964a (0.096)	1.198a (0.02235)			0.802a (0.086)	1.039a (0.200)			0.138a (0.036)	0.070 (0.089)		
$\log(NewLoans + 1) * I_{1Q}$			1.342a (0.180)	0.821 (0.563)			0.949a (0.111)	0.547 (0.340)			0.124 (0.111)	0.226 (0.333)
$\log(NewLoans + 1) * I_{2Q}$			0.383b (0.168)	0.917b (0.409)			0.288a (0.133)	0.782a (0.325)			0.141b (0.072)	0.085 (0.190)
$\log(NewLoans + 1) * I_{3Q}$			0.578 a (0.184)	1.100 a (0.391)			0.462 a (0.159)	0.909 a (0.354)			0.138 b (0.067)	0.102 (0.169)
$\log(NewLoans + 1) * I_{4Q}$			1.446a (0.194)	1.589a (0.436)			1.318a (0.190)	1.484a (0.424)			0.143b (0.058)	0.026 (0.122)
Time-Fixed Effects			YES				YES				YES	
Firm-Fixed Effects			YES				YES				YES	
N			4,598,255				4,598,255				4,598,255	
Firms			118,300				118,300				118,300	

Parentheses indicate robust standard errors clustered at the firm

This table shows the results of regressing the new loans on different employment outcomes in a firm.

where Y_{it} is the daily payroll, the number of employees, and mean wage in firm i in period t . $NewLoans_{it}$ refers to the amount of money that firm i received in period t from new loans. We instrumented $NewLoans_{it}$ with the local government prepayments and present the results for both the IV and the non-IV regression. γ_i and η_t are firm-and time-fixed effects respectively. We clustered the standard errors at the firm level, and all variables are in logarithms. The letters a, b, and c denote statistical significance at the 1%, 5%, and 10% levels, respectively. All coefficients are multiplied by 1000 to facilitate reading.

Table 11
Employment response to new loans (continued).

Variable	OLS		IV		OLS		IV	
	Log(Median Wage + 1)		Log(Gender + 1)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(<i>NewLoans</i> + 1)	0.141a (0.038)	0.035 (0.0943)			-0.023a (0.007)	-0.037b (0.018)		
log(<i>NewLoans</i> + 1) * I_{1Q}			0.009 (0.117)	0.194 (0.343)			-0.046c (0.026)	-0.073 (0.081)
log(<i>NewLoans</i> + 1) * I_{2Q}			0.210a (0.074)	0.167 (0.199)			-0.007 (0.015)	-0.061 (-0.038)
log(<i>NewLoans</i> + 1) * I_{3Q}			0.116 c (0.069)	0.081 (0.182)			-0.035a (0.011)	-0.018 (0.029)
log(<i>NewLoans</i> + 1) * I_{4Q}			0.177 a (0.064)	-0.144 (0.135)			-0.014c (0.008)	-0.022 (0.020)
Time Fixed-Effects		YES					YES	
Firm Fixed-Effects		YES					YES	
N		4,598,255					4,598,255	
Firms		118,300					118,300	

Parentheses indicate robust standard errors clustered at the firm level

This table shows the results of regressing the new loans on different employment outcomes in a firm. where Y_{it} is the median wage and the average gender in firm i in period t . $NewLoans_{it}$ refers to the amount of money that firm i received in period t from new loans. We instrumented New Loans with the local government prepayments and presented the results for both the IV and the non-IV regression. γ_i and η_t are firm-and time-fixed effects, respectively. We clustered the standard errors at the firm level and all variables are in logarithms. The letters a, b, and c denote statistical significance at the 1%, 5%, and 10% levels, respectively. All coefficients are multiplied by 1000 to facilitate reading.

Table 12
Changes in credit conditions.

Variable	Log(Interest Rate + 1)		Log(Time to Maturity + 1)	
	(1)	(2)	(3)	(4)
log(Prepayment + 1)	1.262a (0.09)		1.428a (0.167)	
log(Prepayment + 1) * I_{1Q}		2.460a (0.23)		4.471a (0.421)
log(Prepayment + 1) * I_{2Q}		1.648a (0.178)		1.504a (0.323)
log(Prepayment + 1) * I_{3Q}		1.054a (0.169)		0.803 b (0.320)
log(Prepayment + 1) * I_{4Q}		0.759a (0.143)		0.694a (0.267)
Time-Fixed Effects		YES		YES
Firm-Fixed Effects		YES		YES
N		4,598,255		4,598,255
Firms		118,300		118,300

Parentheses indicate robust standard errors clustered at the firm level

This table shows the results of regressing the local government prepayments on a firm's credit conditions for new loans, the average interest rate spread, and the time to maturity. where Y_{it} is the average spread above the reference interest rate and the time to maturity at the time of issuance for firm i in period t . $Prepayment_{it}$ refers to the amount of money that banks in business with firm i received in period t from local governments prepaying their loans. γ_i and η_t are firm-and time-fixed effects, respectively. We clustered the standard errors at the firm level and all variables are in logarithms. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively. All coefficients are multiplied by 1000 to facilitate reading.

windows, we estimated a positive and statistically significant effect of the liquidity shock. Column 1 shows the estimated coefficient for the window starting in August 2016 and ending in September 2016. It shows a coefficient of 0.263, which translates to an increase of 0.26 percentage points in the firm's borrowing from a bank that had a liquidity shock 1 percentage point higher than the shock received by the other bank.

Columns 2 and 3 show the estimated coefficients for the time windows of October 2017 to November 2017 and October 2018 to November 2018. These coefficients translate to an increase of 0.11 and 0.09 percentage points in the growth rate of loans, respectively, for a 1 percentage point increase in liquidity. The results of this analysis confirm that a positive relationship exists between new loans received by firms and the additional liquidity of the banks with which they do business.

5.3. Staffing effects

Table 10 shows the results of regressing different staffing outcomes on the instrumented amount of money coming from new loans. The exact model is as follows:

$$Y_{it} = \beta_0 + \beta_1 \text{NewLoans}_{it} + \gamma_i + \eta_t + \epsilon_{it}$$

where Y_{it} is the daily payroll, the number of employees, the average wage, the median wage, and the average gender in firm i in period t . NewLoans_{it} refers to the amount of money that firm i received in period t from new loans, while γ_i and η_t are firm- and time-fixed effects, respectively.

We instrumented the new loans variable using local government prepayments, and we estimated the model using a two-stage least-square method. We wished to address with several possible bias this strategy. First, firms that are doing well and growing might demand more loans to keep growing. Second, firms that are not doing well might demand more loans to stay afloat. In the former scenario, new loans would be correlated with growth in staffing outcomes, while in the latter scenario, new loans would be correlated with lower growth or even a decrease in staffing outcomes.

We started by analyzing the total amount of money spent daily by the firm using the payroll variable. Columns 1 and 2 in Table 10 show a positive relationship between new loans and payroll. The estimated coefficient for the IV is 24.2 percent larger, which suggests a downward bias in the non-IV estimation. The coefficient of 0.00198 translates into an increase of 2.96 percent for a 1-standard-deviation increase in new loans. This 2.96 percent represents around USD 30 for a firm with the same average daily payroll as found in the sample.

To analyze whether new loans had any heterogeneous effects on firms, we interacted this variable with the firm's size, as measured by the average of workers across time. This provides a time-invariant classification of firms. We conducted the same interaction using two other time-invariant definitions of firm size: one that allows firms to change classifications each month depending on their relative place in the distribution, and a second using the number of workers during the first time at which the firm appeared in the sample. The outcomes do not change significantly with any of these definitions.¹⁷ The exact model is as follows:

$$Y_{it} = \beta_0 + \beta_1 \text{NewLoans}_{it} * I_{1Qi} + \beta_2 \text{NewLoans}_{it} * I_{2Qi} \\ + \beta_3 \text{NewLoans}_{it} * I_{3Qi} + \beta_4 \text{NewLoans}_{it} * I_{4Qi} + \gamma_i + \eta_t + \epsilon_{it}$$

where the variables are the same as above and I_{nQi} is a dummy variable that takes the value of 1 if firm i is in the n th quartile of the size distribution. Because we used a time-invariant definition, the classification of firms does not change through time.

Columns 3 and 4 in Table 10 show the estimated results for both the IV and the non-IV models. Unlike the aggregated results, for small firms, the estimated coefficient of new loans on payroll showed an upward bias, with the IV-estimated coefficient being 38.8 percent lower than the non-IV coefficient. This suggests that the impact of financing on small firms might be overestimated when looking solely at the simple correlation. Furthermore, the estimated coefficient was not statistically significant in this sample ($p = 0.108$). However, the economic size of the coefficient is not small: it translates into a 3.41 percent increase in payroll after a 1-standard-deviation increase in the amount of new loans received by small firms (see Tables 13 and 14).

For firms in the second, third, and fourth quartiles, the non-IV coefficient underestimated the effect of new loans on payrolls. The largest bias was in the second quartile firms, for which the IV coefficient were 139.43 percent larger than the non-IV coefficient. Firms in the second quartile are also the most sensitive to new loans, with a 1-standard-deviation increase in new loans being followed by a payroll increase of 2.89 percent, as compared to 1.06 percent for firms in the third quartile and 2.28 percent for firms in the fourth quartile.

These results show that smaller firms responded the most to a 1-standard-deviation increase in loans, relative to their average payroll. Furthermore, the fact that the OLS estimate was larger than the IV estimate suggests that, for these firms, the bias came from a higher demand for credit among growing firms. This is contrary to the other groups, for which the direction of the bias suggests that struggling firms are demanding more loans, which leads non-IV coefficients to underestimate the effect of new loans on employment.

Due to the detail of the data available, we were able to investigate what was driving the increase in the firms' payroll. The two most straightforward options were new hires or salary increases. To test these two possibilities, we looked into the number of employees and average salary in each firm.

Columns 6 and 10 in Table 10 show that the increase in the number of employees was significant, both statistically and economically. The IV coefficient on employment implies a 2.57 percent increase in the number of employees, for a firm with the mean number of employees, after a 1-standard-deviation increase in new loans. The result is statistically significant at the 1 percent level. By contrast, a 1-standard-deviation increase in the amount of new loans only increases the average wage by 0.17 percent for a firm with the mean average wage; this result is not statistically significant at any of the conventional levels. We take this as evidence that firms that increased their labor expenditures as a result of the availability of new loans did so by hiring new workers.¹⁸

For the number of employees, we estimated a similar model to the previous one with payroll, by interacting new loans with the firm's size. The pattern was similar to the one observed by payroll: The OLS coefficients overestimated the effect of new loans on

¹⁷ The Mexican Census Bureau (Instituto Nacional de Estadística y Geografía) defines four sizes of firms: micro (0–10 employees), small (11–50 employees), medium (51–100 employees), and large (100+ employees). We prefer our definition because it allows us to capture variations in size among micros and small firms, for which we expect the effect credit to have a larger effect.

¹⁸ Note that the size of the OLS estimates are very different from the IV estimates for firms in the first quartile. We interpreted this to mean that the main source of endogeneity in the OLS approach was the smallest firms in our sample.

Table 13
Descriptive statistics by firm size.

Variable	N	Mean	Std. Dev.	Min.	Max.
Employees	4,598,321	66.15	638.36	1	141,220
Firms in the 1Q	961,772	3.49	2.17	1	97
Firms in the 2Q	1,281,211	9.94	4.60	1	220
Firms in the 3Q	1,192,219	24.09	11.29	1	726
Firms in the 4Q	1,163,119	223.01	1,256.08	1	141,220
Payroll	4,598,321	19,928.05	31,789.40	0	45,700,000.00
Firms in the 1Q	961,772	746.80	867.61	0	41,306.08
Firms in the 2Q	1,281,211	1,938.00	1,699.60	54.30	94,976.69
Firms in the 3Q	1,192,219	4,927.74	4,175.15	20.92	244,861.80
Firms in the 4Q	1,163,119	70,981.03	628,279.10	0	45,700,000.00
Average wage	4,598,321	218.15	188.96	0	2,015.00
Firms in the 1Q	961,772	236.34	285.30	0	2,015.00
Firms in the 2Q	1,281,211	197.31	147.76	54.30	2,015.00
Firms in the 3Q	1,192,219	206.19	141.56	20.92	2,015.00
Firms in the 4Q	1,163,119	238.33	167.19	0	2,015.00
Median wage	4,598,321	188.23	181.36	0	2,015.00
Firms in the 1Q	961,772	223.15	286.58	0	2,015.00
Firms in the 2Q	1,281,211	169.12	136.58	54.30	2,015.00
Firms in the 3Q	1,192,219	172.19	126.91	20.92	2,015.00
Firms in the 4Q	1,163,119	196.86	152.70	2.01	2,015.00
Proportion females (=2) to males (=1)	4,598,321	1.34	0.25	1	2.00
Firms in the 1Q	961,772	1.39	0.34	1	2
Firms in the 2Q	1,281,211	1.34	0.23	1	2
Firms in the 3Q	1,192,219	1.32	0.22	1	2
Firms in the 4Q	1,163,119	1.32	0.22	1	2

Table 14
Descriptive statistics by firm size.

Variable	N	Mean	Std. Dev.	Min.	Max.
New loans (millions of pesos)	4,598,321	1,261,217	31,200,000	0	22,900,000,000
Firms in the 1Q	961,772	979,696	40,700,000	0	8,090,000,000
Firms in the 2Q	1,281,211	329,837	10,400,000	0	2,780,000,000
Firms in the 3Q	1,192,219	439,985	4,251,259	0	845,000,000
Firms in the 4Q	1,163,119	3,361,728	48,200,000	0	22,900,000,000
Local government prepayments (millions of pesos)	4,598,321	31,300,000	352,000,000	0	9,700,000,000
Firms in the 1Q	961,772	20,400,000	281,000,000	0	9,700,000,000
Firms in the 2Q	1,281,211	22,600,000	296,000,000	0	9,510,000,000
Firms in the 3Q	1,192,219	30,200,000	346,000,000	0	9,510,000,000
Firms in the 4Q	1,163,119	51,100,000	453,000,000	0	9,510,000,000
Average spread above interest rate (pct points)	4,598,321	8.14	5.66	0	120.92
Firms in the 1Q	961,772	8.78	6.02	0	61.92
Firms in the 2Q	1,281,211	8.97	5.62	0	61.59
Firms in the 3Q	1,192,219	8.39	5.42	0	62.19
Firms in the 4Q	1,163,119	6.45	5.27	0	120.92
Average time to maturity (months)	4,598,321	26.70	25.45	0	492.10
Firms in the 1Q	961,772	26.93	26.04	0	492.10
Firms in the 2Q	1,281,211	28.67	25.32	0	492.10
Firms in the 3Q	1,192,219	27.80	25.08	0	492.10
Firms in the 4Q	1,163,119	23.20	25.48	0	492.10

employment on small firms; for the rest of the firms, the bias was downward. Firms in the second quartile of the size distribution also showed the largest bias, with the IV coefficient being 171.53 percent larger than the OLS estimated coefficient. The effects on firms with the average number of employees were 2.27 percent (first quartile), 2.47 percent (second quartile), 0.88 percent (third quartile), and 2.13 percent (fourth quartile).

This last set of results suggests that firms hire workers after receiving new loans. We then analyzed the effect of new loans on the median salary in each firm and the average gender composition, to understand which types of workers each firm hired after receiving new loans.

Table 11 shows the results of estimating a similar model as before but with the median salary and gender as the dependent variables. Column 2 shows that new loans did not have a statistically or economically significant effect on the median wage. The coefficient for the IV estimate in Column 2 translates into a 0.09 percent increase in the median salary for 1-standard-deviation increase in new loans. A similar situation emerged when we broke down the effect by the firm's size. Column 4 shows that none of the IV-estimated coefficients were statistically significant. The smallest firms had the greatest economically significant effect among the four groups, with a 1-standard-deviation increase in new loans translating into a 0.81 percent increase in the median wage.

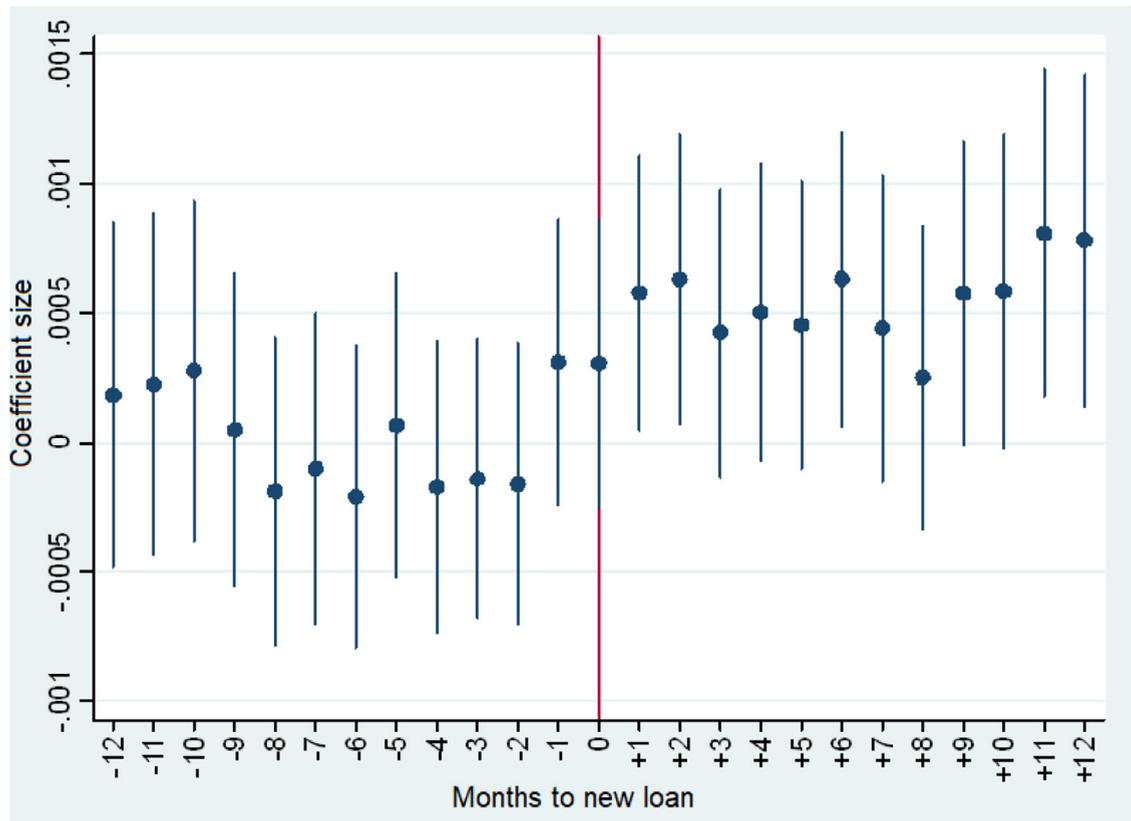


Fig. 3. Firms with Averages of 5 or Fewer Employees This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for firms in the first quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d \widehat{NewLoans}_{i,t+d} + \beta_0 \widehat{NewLoans}_{i,t} + \sum_{d=1}^{d=12} \beta_d \widehat{NewLoans}_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$. The first summation includes 12 lags of the $\widehat{NewLoans}_{i,t}$ variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-months window, and we include firm- and period fixed-effects. We instrumented new loans with the local government prepayments and include firm-and time fixed-effects.

Interestingly, for the largest firms, the effect on the median wage was negative, albeit not very large economically: A 1-standard-deviation increase in new loans translated into a 0.21 percent decrease in the median wage.

We interpret these results regarding the median wage to mean that firms with access to new finance hire workers who earn similar wages to those of their current workers, with the new employees earning a slightly higher wage than the median worker. Next, we analyze the gender composition of the workforce in Columns 6 and 8. None of the estimated coefficients in Columns 10 and 12 were statistically significant, and the largest effect on gender composition took place among the smallest firms, with 0.30 percent decrease in the average female composition of the workforce.

Our evidence suggests that financing does have a positive effect on a firm’s labor expenditures. This effect is mainly driven by the number of employees, with slight wage increases that are not statistically significant. The composition of the workforce, in terms of the median distribution and gender composition did not change significantly with the new incorporation of employees.

5.4. Employment in time

In this subsection, we explore the time it takes firms to respond to new funding, in terms of the number of employees. We used an event-study-like test that includes the lags and leads of the new loans variable around the date when the firm received the loan. The exact specification is as follows:

$$\begin{aligned}
 Employment_{it} = & \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d \widehat{NewLoans}_{i,t+d} + \\
 & + \beta_0 \widehat{NewLoans}_{i,t} + \sum_{d=1}^{d=12} \beta_d \widehat{NewLoans}_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}
 \end{aligned}$$

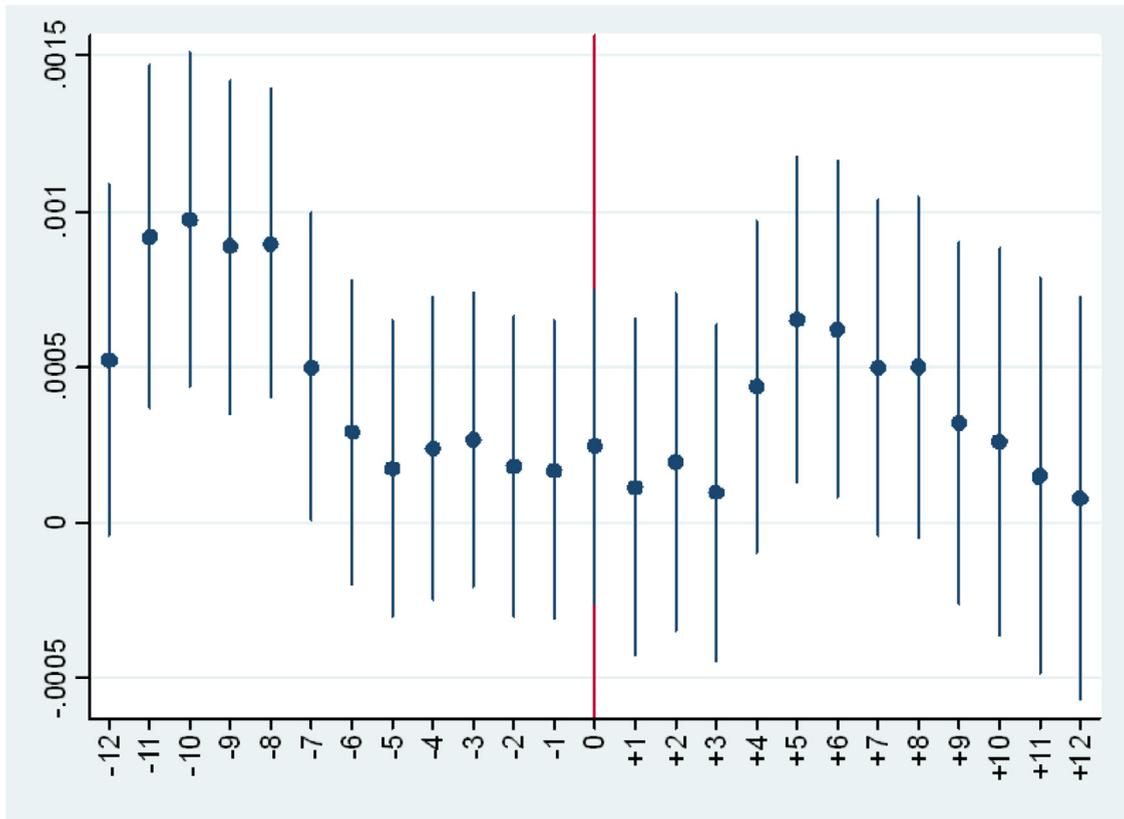


Fig. 4. Firms with Averages of 6 to 14 Employees This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for firms in the second quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{-1} \beta_d NewLoans_{i,t+d} + \beta_0 NewLoans_{i,t} + \sum_{d=1}^{12} \beta_d NewLoans_{i,t+d} + \gamma_t + \eta_t + \epsilon_{it}$. The first summation includes 12 lags of the $NewLoans_{i,t}$ variable, while the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25 months window and we included firm-and period-fixed effects. We instrumented new loans with the local government prepayments and include firm-and time-fixed effects.

The first summation includes 12 lags of the new loans variables, while the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-month window, and we included firm-and period-fixed effects. Finally, just as we did in the previous section, we instrumented new loans using the local government prepayments.

In the previous section, we showed that firms’ employment reactions differed depending on the size of the firms. For this reason, we analyzed firms by sorting them into four bins by size. Fig. 3 presents the effect of loans, on employment using the instrumented variable for new loans for firms in the first quartile of the size distribution. The horizontal axis represents the months before and after the loan was issued. The baseline was the effect that the sum of all leads and lags outside the 25-month window has on employment. The vertical axis represents the estimated coefficients in the equation described above.

We observed an almost immediate response in employment one and two months after each firm received the loan. This positive effect picked up on the sixth month and again on the twelfth month after the loan. On tests that we performed with larger time windows we observed that the effect dissipated after the twelfth month and became slightly negative.

Fig. 4 shows that firms in the second quartile of the firm-size distribution did not show an immediate reaction to new loans. The first effect on employment surfaced at four months, and it reached its maximum effect in the fifth month. After that, the effect dropped consistently until practically reaching zero.

Unlike their smaller counterparts, firms in the second quartile showed an anticipated effect of employment in months 11 through eight prior to the new loan. We conjectured that this might be a selection effect coming from banks: As firms grow larger and become more important clients, it might be easier for banks to pick firms with strong past growth and offer them loans. This story is plausible but unlikely to be the main driver behind the positive effect of new loans on employment among firms in the second quartile. If the only driver of employment was the bank picking growing firms, then we would probably observe a positive effect on months seven through one prior to the new loan and no effect on the months after the new loan.

Fig. 5 shows a similar exercise for firms in the third quartile of the size distribution. For these firms, the effect on employment started on the month in which the new loan was reported to the CNBV, month zero. The effect grew for the first months, and reached its maximum effect in the fourth month after the new loan was issued. After that, the effect dropped consistently until practically reaching zero. Similar to firms in the second quartile, we observed a positive effect in the months before the loan.

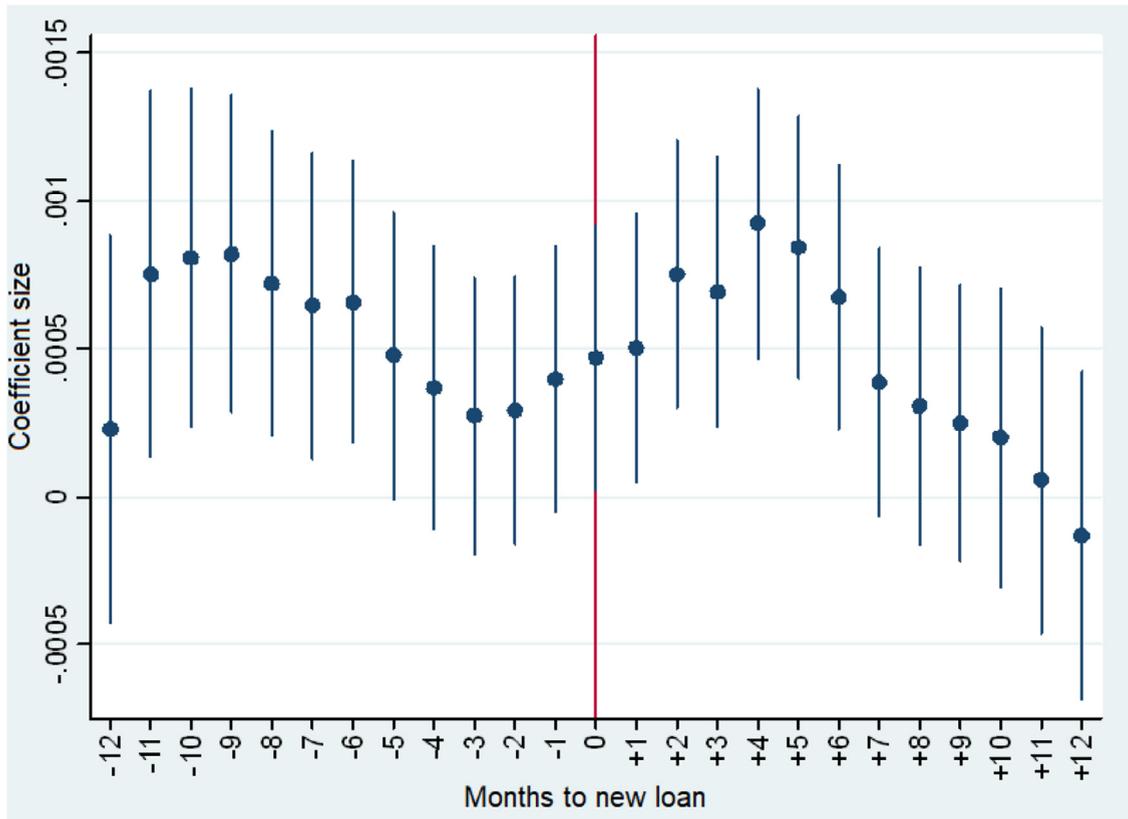


Fig. 5. Firms with Averages of 15 to 37 Employees This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for firms in the third quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d \widehat{NewLoans}_{i,t+d} + \beta_0 \widehat{NewLoans}_{i,t} + \sum_{d=1}^{d=12} \beta_d \widehat{NewLoans}_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$ The first summation includes 12 lags of the $\widehat{NewLoans}_{i,t}$ variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-month window, and we included firm-and period fixed-effects. We instrumented new loans with the local government prepayments and included firm-and time fixed-effects.

Fig. 6 shows the estimated coefficients for the same model but only for the firms in the fourth quartile. For this group of firms, the largest effects on employment occurred prior to the issuance of the new loan, 11 and 10 months before the issuance. The anticipated effect on loans decreased consistently until the eighth month after the loan and then picked up again slightly.

The pattern observed for firms in the highest quartile suggests that banks have a strong selection effect in this group of firms. This means that banks may be choosing to lend to firms that have demonstrated good past performance. This makes us cautious about interpreting our results as average treatment effects, especially for larger firms. Rather, this pattern suggests that the estimated effects of new loans on employment are better understood as the average treatment effect on the treated firms.

The results in this subsection confirm the importance of studying different-sized firms separately. The employment response of the smallest firms in our sample was quicker than that of the larger firms was, and the positive effect was more persistent. Furthermore, for smaller firms, the timing of the response suggests that increased employment was in fact driven by the additional sources of finance.

Finally, it is important to point out that large firms in Mexico potentially face very different financial constraints as compared to smaller firms. First, they are more likely to have more than one banking relationship, meaning that they are less exposed to their banks' liquidity shocks. Second, larger firms have access to credit in international markets, which makes them less sensitive to the financial health of their local banks. Finally, the largest firms in our sample had access to capital markets, which made leverage only one more option among their sources of access to funds.

5.5. Direct and indirect effects of credit on employment

There are two potential mechanisms through which more access to credit might lead firms to increase their workforce. The first one is the indirect effect of an increase in the firm's capital. Adding capital goods to grow a firm is likely to increase labor productivity and entice that firm to hire more workers. The second one is the direct effect of the newfound liquidity to cover the initial costs of

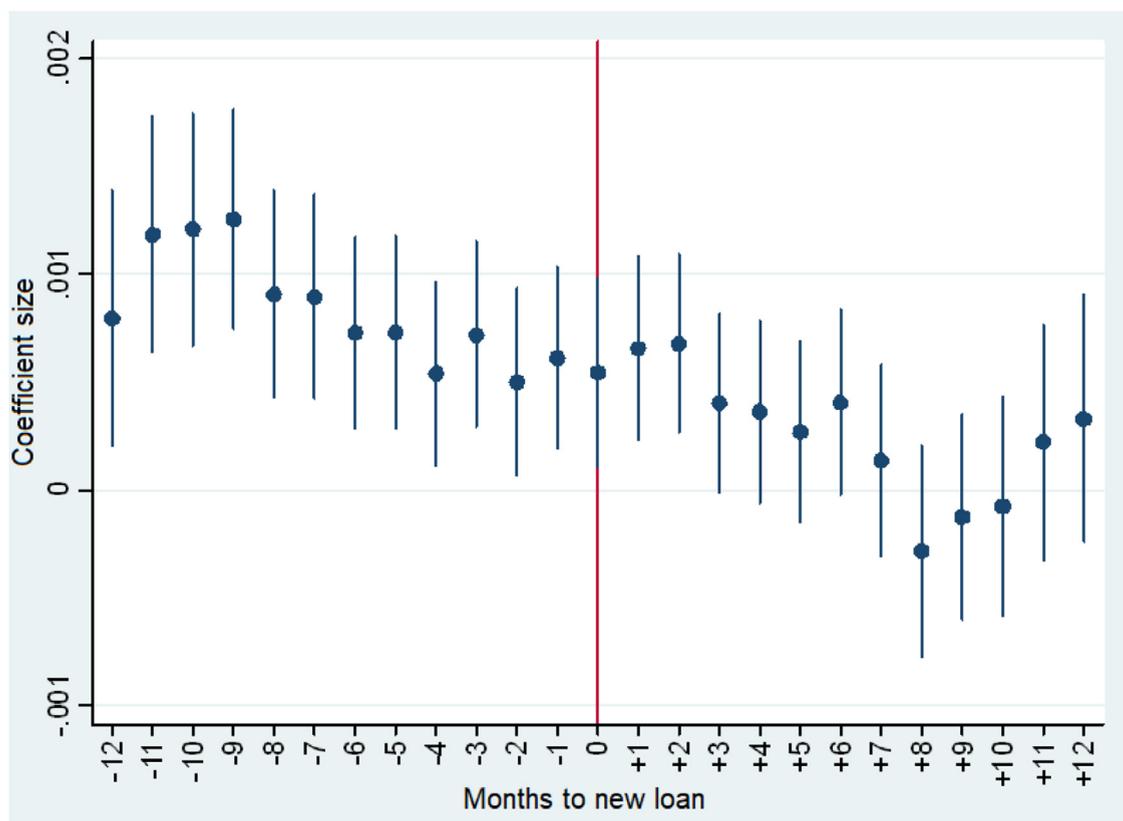


Fig. 6. Firms with Averages of 38 or More Employees This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for firms in the fourth quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d \widehat{NewLoans}_{i,t+d} + \beta_0 \widehat{NewLoans}_{i,t} + \sum_{d=1}^{d=12} \beta_d \widehat{NewLoans}_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$ The first summation includes 12 lags of the $\widehat{NewLoans}_{i,t}$ variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-month window and we included firm-and period fixed-effects. We instrumented new loans with the local government prepayments and include firm-and time-fixed effects.

hiring.¹⁹ Understand which mechanism play, we would ideally observe the variation in the firms’ purchase of capital goods and other investments. Unfortunately, we do not have this information in our data. Hence, we took a different approach.

We separated the firms in our sample into manufacturing and non-manufacturing firms to compare their employment’s reaction times. We focused on the smaller firms. If the indirect effect was driving the increase in employment, we would expect it to be larger for firms in the manufacturing sector which have greater capital requirements. Furthermore, since the indirect effect requires a previous investment to increase the firm’s capital, we would expect employment among firms in the manufacturing sector (where the indirect effect should be higher) to react more slowly. Similarly, if the indirect effect is the important mechanism at play, we would expect employment among non-manufacturing firms to respond more quickly to additional credit.

Fig. 7 presents the results for both types of firms in the first quartile of the size distribution. In this group, manufacturing firms presented the largest response in employment at the third and eleventh month after the new loan was received. In contrast, non-manufacturing firms exhibited a more modest response in the first months after the loan, and their largest response was around the sixth month. The fact that manufacturing firms responded faster than non-manufacturing firms did suggests that the direct effect drove the increase in employment for the smallest firms in our sample.

Fig. 8 shows the results for firms in the second quartile. Both types of firms had similar response patterns during the first nine months. After this, manufacturing firms had a significantly stronger response, especially on the tenth month after receiving the new loan. These patterns suggest that the indirect effect was more important for firms in the second quartile. As firms grow in size their employment growth is more closely related to their investment opportunities.

Finally, Fig. 9 presents the results for firms in the third quartile. The patterns for both types of firms were similar except for the persistence of the effect, which appears to have been larger for non-manufacturing firms. We omitted the analysis of the firms in the fourth quartile because, as we discussed previously, we believe that the effect on employment cannot be interpreted as a treatment effect. Nevertheless, Fig. 10 presents the results for this group of firms. Note that the largest firms in our sample faced very

¹⁹ See Benmelech et al. (2011) for more on how labor can have fixed cost components, such as training costs.

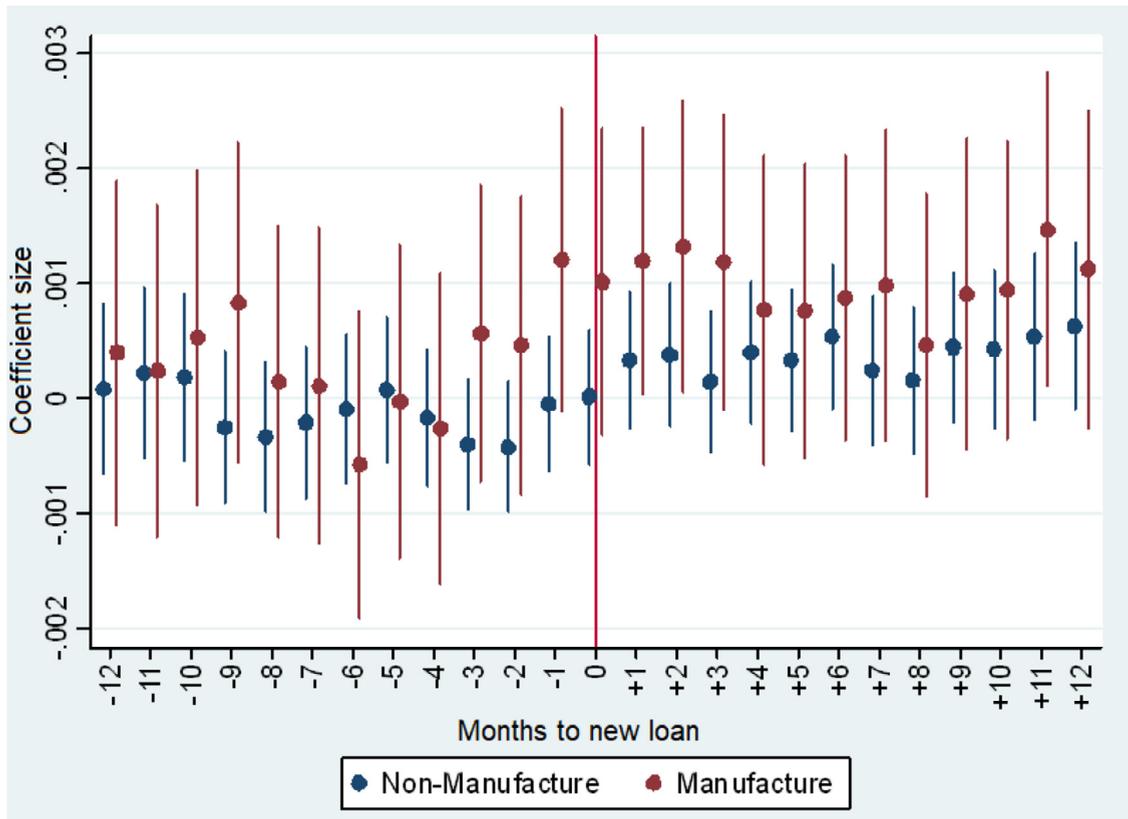


Fig. 7. Manufacturing and non-manufacturing firms with averages of 5 or fewer employees. This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for manufacturing and non-manufacturing firms in the first quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d \widehat{NewLoans}_{i,t+d} + \beta_0 \widehat{NewLoans}_{i,t} + \sum_{d=1}^{d=12} \beta_d \widehat{NewLoans}_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$. The first summation includes 12 lags of the $\widehat{NewLoans}_{i,t}$ variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-month window, and we included firm-and period fixed-effects. We instrumented new loans with the local government prepayments and include firm-and time fixed-effects.

different financial constraints. Indeed, they were more likely to have multiple banking relations, they probably had access to credit in international markets, and many of them could access capital markets, which makes them less dependent on leverage.

6. Loan conditions after prepayment

In this section, we explore how the terms of the newly issued loans changed after a local government prepayment. We focused on two variables: the interest rate spread and the time to maturity of new loans. The interest rate spread is defined as the difference between the nominal interest rate of the new loan and the reference interest rate at the time of the new loan’s issuance. The reference interest rate in Mexico is called the Tasa de Interés Interbancaria de Equilibrio (TIIE). Most of the variable-rate loans in our sample were referenced to this reference interest rate, which is the one we used to calculate the spread. The time to maturity was defined as the number of months until the date of the loan’s maturity, which was set upon issuance of the loan.

The intuition behind the interest-rate spread is straightforward: This is the price of the loan. We looked into the loan’s price because it helped us understand the situation underpinning the loan market. If firms are in a situation in which all their financing needs are all satisfied, then the only way banks can make loans more attractive is by lowering the price of the loan. However, if firms are credit constrained, then banks may be able to get away with offering firms a similar or slightly higher price. The evidence we found for changes in firms’ credit conditions suggests that the latter scenario was happening.

Table 12 shows the results of estimating the effect of a local government prepayment on the conditions of the loans issued by banks. The exact model being estimated is as follows:

$$Y_{it} = \beta_0 + \beta_1 Prepayment_{it} + \gamma_i + \eta_t + \epsilon_{it}$$

where Y_{it} is the average spread above the reference interest rate and the time to maturity at the time of issuance for firm i in period t . Prepayment refers to the monetary amount that banks in business with firm i received in period t from local governments prepaying their loans, while γ_i and η_t are firm-and time fixed-effects, respectively.

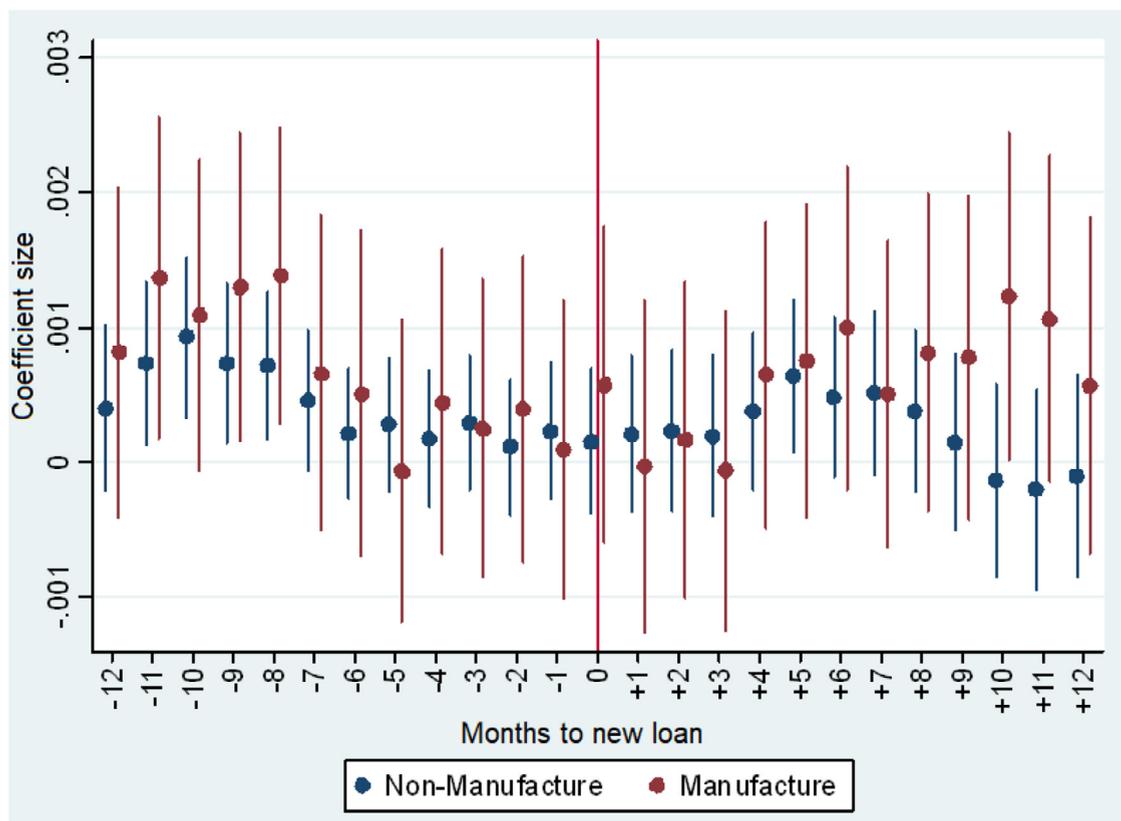


Fig. 8. Manufacturing and non-manufacturing firms with averages of 6 to 14 employees. This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for manufacturing and non-manufacturing firms in the second quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d \hat{NewLoans}_{i,t+d} + \beta_0 \hat{NewLoans}_{i,t} + \sum_{d=1}^{d=12} \beta_d \hat{NewLoans}_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$. The first summation includes 12 lags of the $\hat{NewLoans}_{i,t}$ variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-month window, and we included firm-and period fixed-effects. We instrumented new loans with the local government prepayments and include firm-and time fixed-effects.

Columns 1 and 2 present the results for the overall change in the interest rate and the breakdown of the effect by firm size. The coefficient in Column 1 translates into a 1.42 percent increase in the average interest rate spread for each 1-standard-deviation increase in local government prepayments; this represents around 12 basis points for the firm with the average spread. Column 2 shows that the effect was largest for small firms, with the coefficient representing a 3.39 percent increase in the average interest rate spread for each 1-standard-deviation change in prepayments; this translates into an increase of 30 basis points. The effect decreases as the size of the firm grows, with the coefficients representing changes of 2.16 (second quartile), 1.21 (third quartile), and 0.67 percent (fourth quartile), respectively.

These results suggest that banks are not being forced to lower their prices to place more loans. Furthermore, the rise in the interest rate spread, particularly for smaller firms, could be due to the bank lending money to its riskier clients. However, as Columns 3 and 4 show, banks are also increasing the time to maturity of loans.²⁰ The fact that banks were offering longer times to maturity to their clients made it difficult for us to conclude that firms were financially undeserved, or that banks were lending to riskier clients because it is possible that banks were offering the longer time to maturity as a way to convince firms to take new loans. Furthermore, a longer time to maturity implies more risk for a bank. In other words, it makes the costs of lending higher, which would also explain the increase in the interest rate spread.

The coefficient in Column 3 translates into a 1.61 percent increase in the average time to maturity for each 1-standard-deviation increase in local government prepayments; this represents around 0.43 months for the firm with the average time to maturity. Column 4 shows that the effect was largest for small firms, with the coefficient representing a 6.16 percent increase in the average time to maturity for a 1-standard-deviation change in prepayments; this translates into an increase of 1.66 months. The effect decreases as

²⁰ Notably, this increase is relative to the time to maturity of the last loan acquired by the firm. That is, the increase is not a change with respect to the current time to maturity. Otherwise, time to maturity would always be mechanically higher for similar loans.

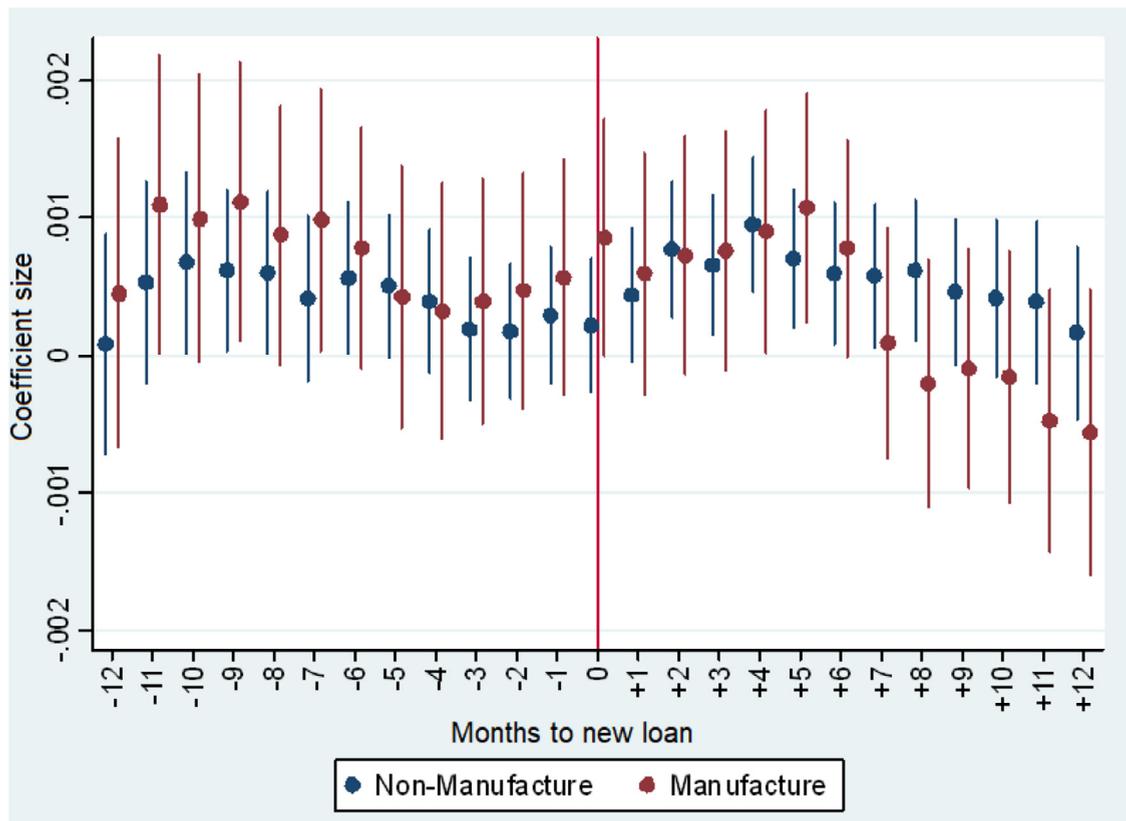


Fig. 9. Manufacturing and non-manufacturing firms with averages of 15 to 37 employees. This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for manufacturing and non-manufacturing firms in the third quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d NewLoans_{i,t+d} + \beta_0 NewLoans_{i,t} + \sum_{d=1}^{d=12} \beta_d NewLoans_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$. The first summation includes 12 lags of the $NewLoans_{i,t}$ variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-month window, and we included firm-and period fixed-effects. We instrumented new loans with the local government prepayments and included firm-and time fixed-effects.

the size of the firm grows, with the coefficients representing changes of 1.97 percent (second quartile), 0.92 percent (third quartile), and 0.62 percent (fourth quartile), respectively.

7. Conclusions

Trying to establish a causal relationship between a firm’s access to credit and its performance is a complicated empirical task. one main problem is the difficulty in directly observing a firm’s access to finance. For instance, it is difficult to know exactly how many loans a firm has and with which financial institutions, the amounts, and the terms, among other characteristics. Moreover, even when one can observe the details of a firm’s debt, it is difficult to break the endogeneity between a firm’s performance and its financial access. Indeed, a growing firm might need funds to keep growing, and a struggling firm might seek access to more credit to slow down the downsizing of its operations.

A novel data set linking Mexican firms’ loan-level information with administrative records on their employees allowed us to surpass these difficulties. We tracked firms’ monthly borrowing history and payroll information. Further, we designed an identification strategy that allowed us to address the concern that the effect of loans on employment is coming from the credit demand side of the equation. Our identification strategy relied on the fact that local governments in Mexico use bank loans as their primary source of funding. This means that every time a state or a municipality refinances a loan, a bank likely experiences an unexpected excess of liquidity.

Mexican firms do not deal with many banks; more than 83 percent of firms deal with at two or fewer banks, and more than 93 percent deal with at most three. This suggests that relationship banking is strong in Mexico and that firms with established relationships with banks that receives local government prepayments are more exposed to variations in their access to finance than other firms.

We used this variation in liquidity coming from local government prepayments to instrument for the issuance of new loans received by firms. This allowed us to estimate the causal effect of new loans on different employment outcomes. We found statistically and

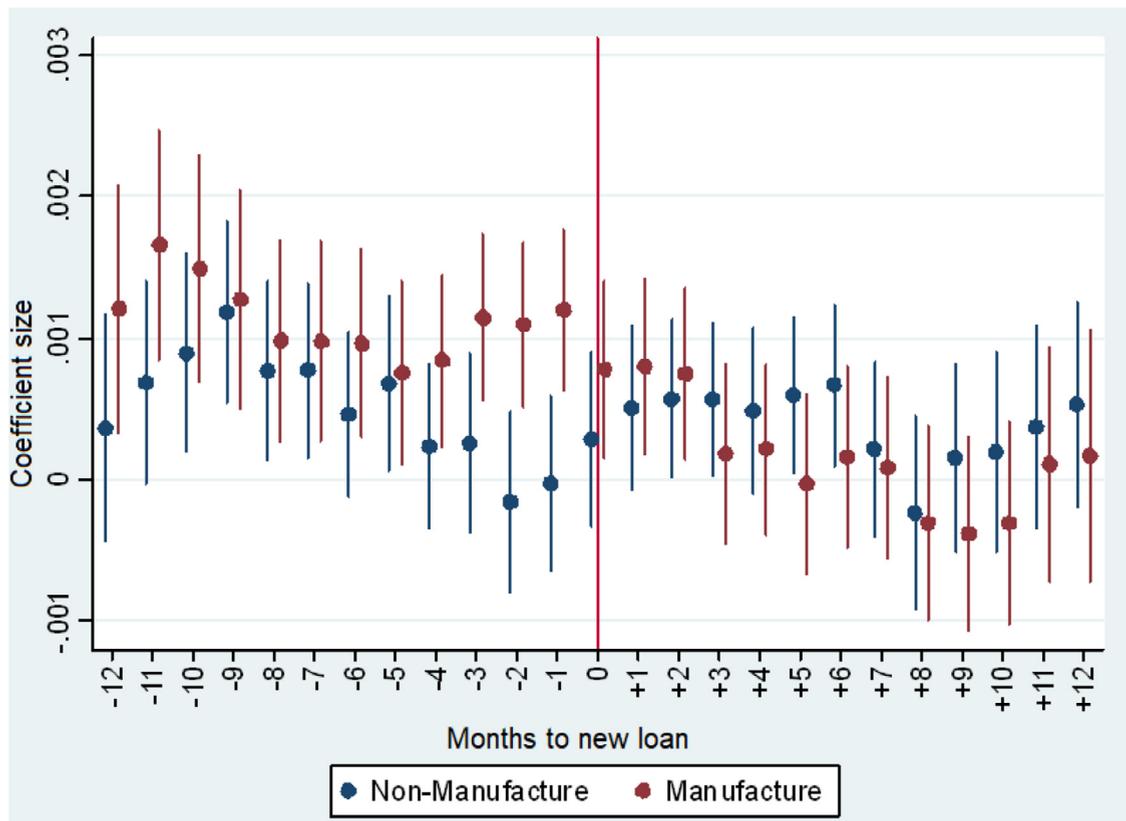


Fig. 10. Manufacturing and non-manufacturing firms with averages of 38 or more employees. This figure shows the estimated coefficients of the effects of different lags and leads of new loans on employment for manufacturing and non-manufacturing firms in the fourth quartile of the size distribution: $Employment_{it} = \alpha_0 + \sum_{d=-12}^{d=-1} \beta_d \widehat{NewLoans}_{i,t+d} + \beta_0 \widehat{NewLoans}_{i,t} + \sum_{d=1}^{d=12} \beta_d \widehat{NewLoans}_{i,t+d} + \gamma_i + \eta_t + \epsilon_{it}$. The first summation includes 12 lags of the $\widehat{NewLoans}_{i,t}$ variable, whereas the second summation includes 12 leads of the same variable. The omitted category is the sum of all leads and lags beyond this 25-month window, and we included firm-and period fixed-effects. We instrumented new loans with the local government prepayments and included firm-and time-fixed effects.

economically significant effects on the firms’ labor expenditure. The level of detail of our data allowed us to investigate the drivers of this change. We found that the main causes of this increase in payroll were the number of employees and, to lesser extent, an increase in the average wage. We also found moderate effects on the firm employees’ median wage, as well as an increase in the male proportion of the workforce.

The impact of the access to new loans on employment differs depending on the firms’ size. For the smallest firms, employment responded quickly, and the effect persisted through the first 12 months after the issuance of the new loan. For firms in the second and third quartiles of the size distribution, the effect took approximately four months to reach its maximal impact on employment; after that, the effect quickly diminished to zero. Finally, for the largest firms, the effect on employment occurred in the months before the loan was issued. We interpreted this as a selection effect of banks on this group of firms, which made us cautious about interpreting this result as an average treatment effect; rather, we saw it as the effect of being selected by the bank on employment.

Our final set of results reflected how credit conditions change after the prepayment of local government loans. We observed an increase in the average interest rate spread, and an increase in the time to maturity for the new loans. The increase in the average interest rate spread can be interpreted as indicating that banks are not forced to lower prices to place more loans. This is consistent with a story of firms being underserved financially-especially smaller firms, which are the main drivers of the results showing an increase in the interest rate spread. However, the fact that we observed an increase in the new loans’ time to maturity cast doubt on the previous story. Increasing the time to maturity increases the costs of lending, which might drive up the interest rate spread. This could be interpreted as revealing banks’ efforts to convince their clients to take on more debt.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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