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# On the direct and indirect real effects of credit supply shocks[☆]

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ABSTRACT

We explore the real effects of bank-lending shocks and how they permeate the economy through buyer-supplier linkages. We combine administrative data on all Spanish firms with a matched bank-firm-loan dataset of all corporate loans from 2003 to 2013 to estimate firm-specific credit supply shocks for each year. We compute firm-specific measures of exposure to bank lending shocks of customers (upstream propagation) and suppliers (downstream propagation). Our findings suggest that credit supply shocks have sizable direct and downstream propagation effects on employment, investment, and output, especially during the 2008–2009 crisis, but no significant impact on employment during the expansion. We provide evidence that both trade credit extended by suppliers and price adjustments in general equilibrium explain downstream propagation of credit shocks.



## 1. Introduction

Although there is by now extensive evidence on the relationship between the evolution of financial variables and the real economy, we still lack direct evidence on the particular mechanisms through which financial shocks propagate to the real economy. In this paper, we use detailed bank-firm-loan level data for Spain to examine the real effects of the bank lending channel and how bank-lending shocks permeate the economy through buyer-seller

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interactions. We show that credit supply shocks do affect firms' real outcomes, and indeed permeate the real economy through input-output relations, especially during the Global Financial Crisis. Our findings suggest that network effects substantially amplify the real effects of financial shocks typically estimated in the bank lending channel literature. Trade credit extended by suppliers and price adjustments in general equilibrium seem to be at the root of this propagation.

The exercise of quantifying the consequences of financial shocks on real variables and buyer-supplier (input-output) relations is very demanding. First, firm-level data linking credit information to outcome variables (such as employment, investment, output) is required, and, second, a plausibly exogenous source of variation in credit growth is also needed.[1] To address the second challenge, we exploit the universe of bank-firm loans in Spain over the 2003–2013 period, and we identify bank-year-specific credit supply shocks through differences in credit growth between banks lending to the same firm, as in Amiti and Weinstein (2018).

We validate the estimated bank-supply shocks in several ways. First, we divide the sample into healthy and weak banks, as in Bentolila et al. (2018).[2] We find that weak banks experienced stronger supply shocks until 2006 and weaker afterwards. We interpret this evolution as clear evidence favoring the plausibility of our estimated bank-supply shocks. Second, if our identified bank-specific credit shocks capture meaningful supply factors, a bank that experiences a larger shock should grant more loans to a given firm vis-a-vis a bank experiencing a lower shock. Using loan application data, available from the credit registry dataset, we show this to be the case.

Armed with the estimated bank-lending shocks, we turn to the first challenge and estimate their direct effects on firm outcomes as well as their propagation through input-output linkages (our so-called indirect effects) using balance-sheet data for the quasi-census of Spanish companies. For that purpose, we combine the Spanish input-output structure at the sector level with firm-specific measures of downstream and upstream exposure, following di Giovanni et al. (2018). In particular, we explore whether firms are indirectly affected by the fact that their suppliers are hit by the shocks (downstream propagation), and we also explore whether firms that sell goods to customers hit by the shocks are indirectly affected (upstream propagation).

We find both the direct and indirect effects of credit shocks on real variables to be sizable. Our estimates imply that an increase of one standard deviation in firms' credit supply generates increases of 0.30, 0.10, and 0.80 percentage points in the change of employment, output, and investment, respectively. In terms of the indirect effects, our estimates corroborate the importance of downstream propagation from suppliers to customers in quantifying the real effects of credit shocks. A one standard deviation increase in our downstream shock variable (how much firms buy inputs from suppliers in which credit supply expands) generates increases of 0.30, 0.35, and 0.69 percentage points in the change of employment, output, and investment. In contrast, we find mixed evidence on the importance of upstream propagation from customers to suppliers, in terms of both significance and size of the estimated effects. Finally, it is worth highlighting that our estimates point to significantly stronger effects during the Global Financial Crisis.

To rationalize downstream propagation of credit supply shocks, we explore the role of two possible mechanisms, namely, trade credit and price adjustments in general equilibrium. Trade credit provides a channel through which credit shocks can propagate downstream. Affected suppliers, for example, can reduce the trade credit offered to their customer firms which might then cut production if they are financially constrained (Kiyotaki and Moore, 1997). Indeed, Costello (2020) documents that U.S. firms that were more exposed to a large decline in bank lending during the Global Financial Crisis substantially reduced the trade credit extended to their customers. To explore this mechanism, we include in our regressions accounts payable (trade credit received from suppliers) and find that our downstream coefficient decreases in magnitude but remains significant and quantitatively relevant. We thus conclude that trade credit adjustment plays a significant role but it is not able to fully account for our estimated downstream propagation of credit shocks.

Another possible channel of propagation is through changes in relative prices. A negative credit shock to a particular supplier/industry may increase the price of its product, thus affecting customer decisions (Acemoglu et al., 2012). If a firm gets hit by a negative credit supply shock, its relative supply will fall, implying a higher price of the good produced by this firm in equilibrium. This also implies a higher production cost for this firm's customers, reducing their demand for the good produced by the affected firm and decreasing their total output. To check whether this channel is empirically plausible, we first construct changes in price indexes between 2007 and 2010 for several Spanish industries and correlate them with our estimated direct and downstream shocks. As predicted by the standard general equilibrium models with input-output linkages, we find that industries that were hit harder by negative direct and indirect shocks suffered higher increases in their price indexes.

To further evaluate the extent to which the Spanish production structure could have amplified the effects of our estimated financial shocks, we quantify the aggregate impact of the price adjustments channel by using a general equilibrium model with buyer-supplier relations under the

[1] An important concern in the literature has been identifying plausible exogenous shocks to disentangle the bank lending-channel (or bank-specific shock) from the firm borrowing-channel (i.e., a firm's ability, or lack thereof, to borrow from alternative sources). Firms may be able to undo a particular negative bank supply shock by resorting to another bank or other sources of funds. Kashyap et al. (1993) and Adrian et al. (2012) find that firms are able to substitute to other forms of credit in the presence of loan supply shocks. Klein et al. (2002) stress the difficulties of substituting loans from one bank with loans from another. Midrigan and Xu (2014) emphasize the role of self-financing; see Khwaja and Mian (2008), and Jimenez et al. (2020) for further discussion.

[2] Bentolila et al. (2018) define weak banks as those that were bailed out by the Spanish government as part of the restructuring process during the financial crisis.

presence of financial frictions, as in Bigio and La'o (2020). The model predicts, for instance, that during the financial crisis, around half of the fall in employment and almost the entire fall in output was due to propagation effects through the input-output network. The model also predicts that shocking some central sectors (sectors widely used by other sectors) like real state or wholesale alone would have generated big output losses, and that most of those losses would have been accounted for by the propagation channel.

*Related literature.* Our paper contributes to the research that identifies the economic effects of credit supply shocks by isolating the bank-lending channel. Papers in this strand include Khwaja and Mian (2008), Chodorow-Reich (2014), Jimenez et al. (2020), Greenstone et al. (2020), Cingano et al. (2016), and Bentolila et al. (2018). In relation to this literature, instead of observed supply shocks (e.g., liquidity in Khwaja and Mian (2008) or Huber (2018), securitization in Jimenez et al. (2020), or higher capital requirements in Blattner et al. (2017)), we estimate time-variant bank credit shocks and study their real effects on employment, output, and investment. Employment effects, for example, substantially differ during the expansion period and the financial crises.[3] We also contribute to this strand of the literature by considering the propagation of bank-lending shocks through input-output linkages.

Methodologically, our paper is closest to Amiti and Weinstein (2018). The authors estimate the direct effect of credit supply on firms' investment by exploiting a sample of around 150 banks and 1600 listed firms in Japan over a 20-year period (1990–2010). By using methods from the matched employer-employee literature, we are able to estimate year-by-year supply shocks for a broader sample, more than 200 banks, and demand shocks for more than 700,000 firms. As our data covers the quasi-population of Spanish firms, aggregation bias is less of a concern.[4]

In terms of literature on the importance of input-output linkages, Acemoglu et al. (2015) quantify the propagation effects of different types of supply and demand shocks, relying on instrumental variables for identification, showing their transmission effects to the aggregate economy as critically important. Our paper contributes to this literature by investigating the effects of a well-defined shock, that is, firm-level credit supply shocks, and quantifying the direct and indirect effects on other firms through connections in the production network.[5] Recent work also investigates the role of propagation in accounting for the effects of financial shocks. Dewachter et al. (2017), using mostly single bank-firm relations in Belgium and exploiting value added information, analyze the propagation effects of shocks. Demir et al. (2020) show that a negative shock to the cost of import financing of liquidity-constrained firms gets propagated to their customers. Giannetti and Saidi (2019) analyze the extent to which the propagation of credit market shocks depends on the structure of the banking system and the lenders' share of the loans outstanding in an industry.

Turning to the mechanisms explaining propagation of financial shocks through buyer-seller relations, Costello (2020) documents that firms with greater exposure to a large decline in bank lending reduced the trade credit extended to their customers resulting in negative effects on their real outcomes. Trade credit could also explain upstream propagation of financial shocks if debtor (customer) failure triggers suppliers' losses through both credit losses and demand shrinkage (see, e.g., Jacobson and Schedvin, 2015). While our evidence supports the downstream propagation mechanism (Costello, 2020), it does not explain the whole effect of our estimates.

Price and quantity adjustments in general equilibrium could also play a role, as shown in a series of recent papers that have investigated the aggregate effects of shocks that propagate through the economy's input-output network, such as Acemoglu et al. (2012). Our paper relates to recent work by Bigio and La'o (2020), who quantify the effects of financial shocks in a general equilibrium model in which industries are connected through the IO network. Instead of credit spreads, we use credit registry data to identify financial shocks at the firm level. We then aggregate these shocks at the industry-level to show that industries experiencing negative financial shocks suffered higher price increases, and use the model to quantify the implied aggregate effects over time.

The remainder of the paper is organized as follows. Section 2 describes the data, while Section 3 disentangles the banking-lending channel from the firm-borrowing channel and discusses the empirical specification. Section 4 presents the direct real effects of the bank lending shocks as well as our estimates for downstream and upstream propagation effects of the credit shocks. Section 5 explores the mechanisms rationalizing our main findings and quantifies the aggregate effects of the credit shocks. Finally, Section 6 offers some concluding remarks.

## 2. Data

We use three datasets: loan-level data on credit in the domestic banking sector from the Central Credit Registry (CIR) of Banco de España, administrative data on firm-level characteristics from the Spanish Commercial Registry, and IO tables provided by the Spanish National Statistical Institute (*Instituto Nacional de Estadística*).

*Credit Registry* The Central Credit Registry (CIR), maintained by the Bank of Spain in its role as primary banking supervisory agency, contains detailed monthly information on all outstanding loans exceeding 6000 euros granted to non-financial firms by all banks operating in Spain since

[3] Greenstone et al. (2020) and Gilchrist et al. (2018) find a small or no effect of credit supply shocks during the boom period in the United States. Our analysis, similarly to their work, expands the sample beyond the Global Financial Crisis to encompass all firms in the economy, including small and medium-size firms. Their identifications strategy exploits geographical differences in the origin of business-lending loans (Greenstone et al., 2020) or mortgages (Gilchrist et al., 2018).

[4] The Amiti and Weinstein (2018) methodology also accounts for general equilibrium constraints such that micro and macro features of the data are mutually consistent. In particular, the aggregation of their estimated bank- and firm-specific shocks exactly replicates the aggregate evolution of credit (even accounting for new lending relationships).

[5] A series of papers in the literature has exploited natural disasters as exogenous shocks, finding input-output propagation to account for sizable effects, see Carvalho et al. (2017), Barrot and Sauvagnat (2016) and Boehm et al. (2019).

1984. Given the low reporting threshold, virtually all firms with outstanding bank debt appear in the CIR.

The CIR identifies the parties involved in each loan, enabling us to match loan-level data from CIR with administrative data on firm-level characteristics. While the CIR data are available at the monthly frequency, firm-level characteristics are only available on a yearly basis. Therefore, we collapse the monthly loan-level data to annual frequency in order to merge the two datasets. At the monthly level, each bank-firm relationship is understood as a loan by aggregating all outstanding loans from each bank-firm-month pair. Annual bank-firm credit exposure is computed as the average value of monthly loans between bank $i$ and firm $j$. We end up with a bank-firm-year database covering 12 years from 2002 to 2013, 235 banks, 1,555,806 firms, and 18,346,144 bank-firm-year pairs (our so-called loans). Multibank firms represent nearly 75% of bank-firm-year relationships and 90% of total credit volume.

The CIR also contains loan application data. Banks receive borrower information (e.g. total indebtedness or defaults) from the CIR monthly. Because banks can obtain this information for any firm that makes a genuine attempt to secure credit, any requested information from a bank about a given firm can be interpreted as a loan application. Matching the monthly records on loan applications with the stock of credit enables us to infer whether a loan materialized. If not, either the bank denied it or the firm obtained funding elsewhere. We use this information in Section 3.1.2 to validate our estimated bank-specific credit shocks.

*Quasi-Census Administrative Data* For firm-level characteristics, we use administrative data from the Spanish Commercial Registry, which contains the balance sheets of the universe of Spanish companies which firms are legally obliged to report.[6] Among other variables, this includes information on: name, fiscal identifier; sector of activity (4-digit NACE Rev. 2 code); 5-digit zip code location; annual net operating revenue; material expenditures (cost of all raw materials and services purchased by the firm for the production process); number of employees, labor expenditures (total wage bill including social security contributions); and total fixed assets.

Our final sample includes balance sheet information for 1,801,955 firms, with an average of 993,876 firms per year. The firm-level database covers 85%–95% of firms in the non-financial market economy for all size categories in terms of both turnover and number of employees. Moreover, the correlation between micro-aggregated employment (and output) growth and the National Accounts counterparts is approximately 0.95 over the 2003–2013 period (see Fig. 1). Almunia et al. (2018) provide an in-depth analysis of this database.

*Input-Output Tables* We use the Input-Output tables provided by the *Instituto Nacional de Estadística* (INE) and constructed at the 64-industry-level of disaggregation (see Table I.3 for a list of industries). In order to use the most detailed IO that is available, and because prior year IO tables rely on an industry classification different from that used in our firm-level data, we use the IO table provided for the year 2010 throughout the paper.[7] Some examples of industries that are used intensively by many other industries (central sectors) are *Real Estate Services (44), Wholesale (29)* and *Electricity Services (24)*.

*Time Coverage* To explore whether the real effects of credit supply shocks might vary depending on the state of the economy, we divide the sample into three subperiods: 2003–2007 (*expansion*), 2008–2009 (*financial crisis*), and 2010–2013 (*recession*). This division is based on the FRED recession indicators. We think of 2003–2007 as a boom-expansion era of easy access to credit, 2008–2009 as a crisis period driven by the collapse of the banking sector during the Global Financial Crisis, and 2010–2013 as the post crisis period of sluggish recovery but still under recession of the Spanish economy. Financial crises tend to be characterized by deep recession and slow recovery (Reinhart and Rogoff, 2009). The evolution of the Spanish economy broadly fits this pattern.

## 3. Identificationstrategy and empirical models

In this section, we first estimate bank-specific credit supply shocks by exploiting the richness of our dataset. We also discuss various ways in which we validate the estimated shocks in Sections 3.1.1 and 3.1.2. Armed with the identified credit supply shocks, Section 3.2 presents the empirical model considered to estimate the effects of credit shocks on real outcomes, both directly and indirectly through input-output propagation. Note also that Appendices Appendix A and Appendix B quantify the impact of bank lending shocks on credit at the loan- and firm-level, respectively.

### 3.1. Estimating bank-specific credit supply shocks

Consider the following decomposition of credit growth between bank $i$ and firm $j$ in year $t$:

$$\Delta \ln c_{ijt} = \delta_{it} + \lambda_{jt} + \epsilon_{ijt} \tag{1}$$

where $c_{ijt}$ refers to the yearly average of outstanding credit of firm $j$ with bank $i$ in year $t$. $\delta_{it}$ and $\lambda_{jt}$ refer to a set of bank-year and firm-year fixed effects, respectively. Finally, $\epsilon_{ijt}$ captures other shocks to the bank-firm relationship assumed to be orthogonal to the bank-year and firm-year effects.

Following Amiti and Weinstein (2018), we interpret $\delta_{it}$ as a bank-year-specific credit supply shock identified through differences in credit growth between banks lending to the same firm. Intuitively, $\delta_{it}$ can be interpreted as supply-driven shocks because demand factors are held constant by the inclusion of firm-year-specific effects ($\lambda_{jt}$) as in Khwaja and Mian (2008).[8] In order to

[6] We combine two databases independently constructed from the Commercial Registry, Central de Balances Integrada (CBI) from the Banco de España and SABI (Spain and Portugal Business Registry). The resulting database, which includes approximately 1,000,000 firms in each year from 2000 to 2013, is available only to researchers undertaking projects for the Banco de España.

[7] Measured at a lower industry-level disaggregation, we can show that input-output tables in Spain have remained quite stable over time.

[8] Since the credit registry data has a monthly frequency, we could estimate Eq. (1) with quarterly or even monthly data. Using annual data

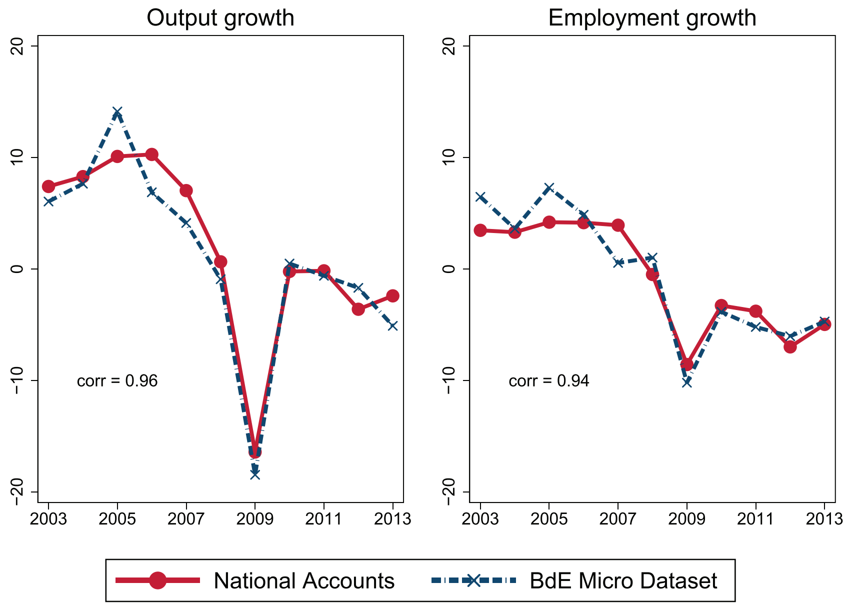


**Fig. 1.** Micro-aggregated nominal output and employment growth.

estimate the model in (1) and recover the estimated $\hat{\delta}_{it}$ and $\hat{\lambda}_{jt}$s, we resort to matched employer-employee techniques (see Abowd et al., 1999).[9] To be more concrete, we use the so-called "FEiLSDVj" approach described in Cornelissen (2008).

*3.1.1. Threats to Identification*

A concern when using Eq. (1) is that it does not allow for bank-firm-time interactions. As noted by Jimenez et al. (2020) and Paravisini et al. (2017), these interactions may be relevant in the context of bank-lending specialization. That is, an implicit assumption in this strategy is that firms' credit demand is the same for all lenders, and thus firm-time fixed effects ($\lambda_{jt}$) account for demand effects. However, in our case, three points alleviate this concern.

First, Amiti and Weinstein (2018) show that the bank-time fixed effects estimated from Eq. (1) are identical to those resulting from a specification accounting for bank-firm-time-specific factors (see Amiti and Weinstein, 2018 for a formal proof). As they explain, although bank-firm interactions enable us to understand a particular firm's demand, bank and firm shocks can be consistently estimated from Eq. (1). Intuitively, the effect of bank-firm-year factors is only identifiable if some component is orthogonal to the bank- and firm-year fixed effects, and this orthogonal variation is precisely the one identified in our bank-year fixed effects. In fact, our estimates remain broadly unaltered when accounting for idiosyncratic bank-firm-year factors such as lagged bank-firm credit in Eq. (1) (see Appendix A).

Second, specialization in housing by some banks may be a source of concern in the presence of firm attachment to those banks given the housing boom and bust cycle experienced by the Spanish economy. However, our findings are robust to the exclusion of construction and real estate firms from the sample (see Section 4.3).

Third, at the frequency of our analysis, the variation in maturity at the bank-firm level in our data is mostly explained by variation across firms for a given bank (59%), while the variation across banks for a given firm explains very little (7%) of the total variation. We interpret this pattern as an indication that firms' loans characteristics are similar across banks, at least in terms of maturity, so the assumption of firms' constant credit demand across banks is not sharply at odds with our data.

*3.1.2. Validating the bank-specific credit supply shocks*

We provide further validation of the estimated credit supply shocks. First, in order to assess the plausibility of the $\hat{\delta}_{it}$ estimates, we divide our sample into healthy and weak banks, as in Bentolila et al. (2018). Fig. 2 shows the time evolution of the average difference in credit supply shocks between healthy and weak banks as identified by the bank dummies ($\hat{\delta}_{it}$). Weak banks had higher supply shocks until 2006 and lower ones afterwards, which coincides with the narrative in Bentolila et al. (2018). We interpret this evolution as clear evidence in favor of the plausibility of our estimated bank supply shocks.

We also validate our estimates as follows. If our identified bank-specific credit shocks capture supply factors, a

allows us to have have more firms per bank and better estimate the bank effects. Using quarterly/monthly data allows us to better control for demand shocks because firm effects are allowed to vary within a year. With this trade-off in mind, we have finally decided to use annual data in order to merge the estimated effects with balance-sheet information at the firm-level available at a yearly frequency. Note also that this identification scheme implies reliance on multi-bank firms, which represent approximately 75% of the bank-firm-year relationships and 90% of total credit volume in our sample.

[9] Consistent with the matched employer-employee methods, banks and firms in our data correspond to firms and workers in typical matched employer-employee panels. Also, for each firm in our data we have the number of banks as the time dimension in standard matched employer employee datasets.

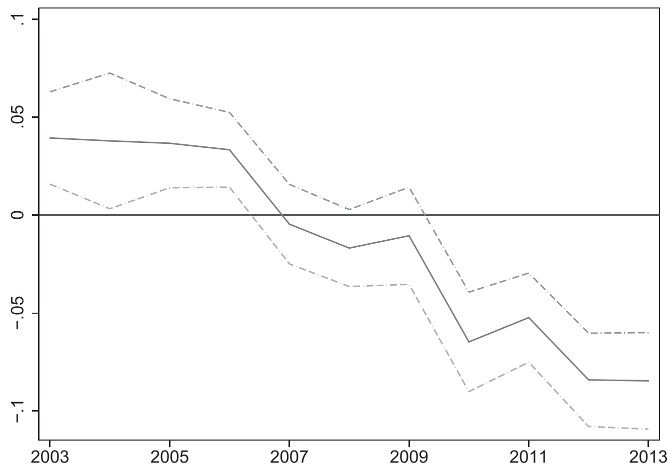


**Fig. 2.** Average difference in bank supply shocks (weak - healthy).
*Notes.* This plot is based on year-by-year regressions of the estimated bank-level shocks on a constant and a dummy that takes value of one if the bank is classified as "weak" in Bentolila et al. (2018). For each year, we plot the coefficient on the weak bank dummy, which estimates the average difference in supply shocks by type of bank (weak or healthy).

bank with a larger dummy ($\hat{\delta}_{it}$) should grant more loans to the same firm. Loan application data enables us to test this hypothesis. We regress a loan granting dummy on the estimated bank shocks and a set of firm fixed effects to account for demand factors. As mentioned above, the identification of our bank-year dummies relied on multi-bank firms. However, the firms used in this validation exercise cannot have any credit exposure to the banks in the regression used to estimate the bank-year shocks because otherwise they would not be observed in the loan application data. The bank-firm pairs exploited in this exercise are thus not used in the identification of the bank dummies in (1). In particular, for each year from 2003 to 2013, we run the following regression:

$$\text{Loangranted}_{ij} = \gamma\hat{\delta}_i + \lambda_j + \epsilon_{ij} \tag{2}$$

where $\text{Loangranted}_{ij}$ is a dummy variable taking the value 1 if firm $j$ has at least one loan granted by bank $i$ (conditional on having applied for a loan) and zero if no loans originated from loan applications from firm $j$ to bank $i$. $\hat{\delta}_i$ refers to our estimated bank supply shock for bank $i$, and $\lambda_j$ captures firm-specific effects to account for demand. The $\gamma$ parameter captures the effect of credit supply shocks on the probability of loan acceptance. A positive and significant estimate can be interpreted as evidence that our bank dummies capture credit supply. Intuitively, a firm applying to two different banks—with no previous credit relationship with the firm—has a higher probability of securing the loan from the bank with the larger bank dummy if $\gamma$ is positive. Fig. 3 plots the estimated $\gamma$ coefficient for each year. The effect of the bank-specific shocks is positive and significant in all years, which we interpret as further evidence of the validity of our identified bank supply shocks.

Following Amiti and Weinstein (2018), we further explore how well our predicted bank's credit growth explains the bank's actual credit growth. Specifically, we compute the R-squared of a regression of the banks' actual credit growth ($\Delta\ln c_{it}$) on the bank's credit growth predicted by our model ($\Delta\,\widehat{\ln}\,c_{it}$).[10] The $R^2$ for the entire 2003–2013 period is 52%, which indicates that the estimated bank- and firm-specific effects explains a significant fraction of the variation in bank lending as illustrated in Fig. 4. Note that Fig. 4 refers to the intensive margin without including new lending relationships from both credit growth variables, $\Delta\ln c_{it}$ and $\Delta\,\widehat{\ln}\,c_{it}$. Indeed, the R-squared drops to 30% when including the extensive margin in actual credit growth. All in all, the estimated $R^2$s are relatively large in both cases.

### 3.2. Empirical specifications

We now discuss the specification used to estimate the real effects of the identified credit supply shocks. To estimate the effects of the bank lending channel on real outcomes, we match the credit registry information with annual, firm-level administrative data on different firm characteristics. We consider the effects of credit supply on firms' annual employment and output growth as well as investment, as follows:

$$Y_{jt} = \theta\overline{\delta}_{jt} + \pi X_{jt} + \nu_{jt} \tag{3}$$

where $Y_{jt}$ refers to annual employment growth (in terms of log differences of number of employees), annual output growth (in terms of log differences of Euros), or investment (capital stock in year $t$ minus capital stock in year $t-1$ as a share of total capital stock in $t$) of firm $j$ in year $t$.[11] $X_{jt}$ represents a vector of firm-specific characteristics including the firm-specific credit demand shocks ($\hat{\lambda}_{jt}$) as

[10] We construct $\Delta\,\widehat{\ln}\,c_{it}$ as a weighted average of the change in credit at the bank-firm (loan) level, where weights are computed as the amount of credit extended to firm $j$ by bank $i$ as a fraction of total credit granted by bank $i$ (computed in $t-1$): $\Delta\,\widehat{\ln}\,c_{it} = \sum_j \frac{c_{ijt-1}}{\sum_j c_{ijt-1}}\Delta\,\widehat{\ln}\,c_{ijt}$ where $\Delta\,\widehat{\ln}\,c_{ijt} = \hat{\delta}_{it} + \hat{\lambda}_{jt}$.

[11] Results considering $\Delta\ln(1+E_j)$ and $(E_j - E_{j,-1})/(0.5\times(E_j + E_{j,-1}))$ as dependent variables remain unaltered. These alternative definitions are considered by Bentolila et al. (2018) and Chodorow-Reich (2014), respectively.

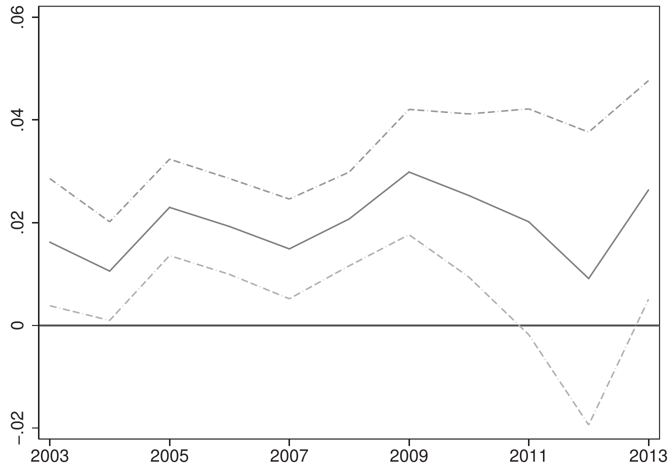


**Fig. 3.** Effect of the bank shocks on loan granting.
*Notes.* This plot is based on year-by-year regressions of the loan granted dummy on the bank-level dummies and a set of firm fixed effects. The $\gamma$ parameter plotted estimates the effect of the bank dummies on the probability of acceptance of a loan request. Standard errors are clustered at the bank level.

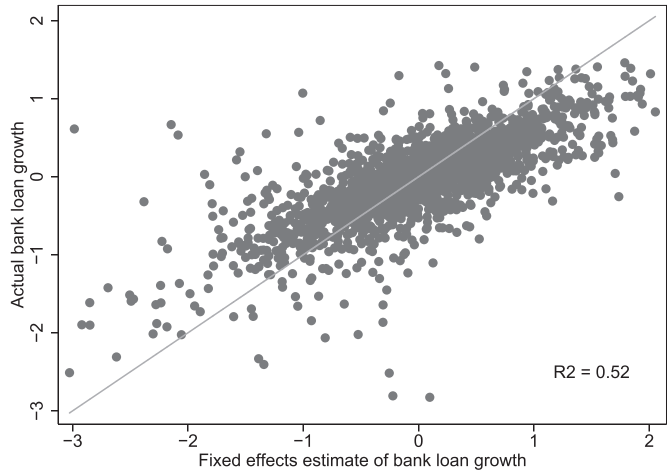


**Fig. 4.** Explanatory power of our estimated shocks.
*Notes.* This graph plots the relationship between the bank's actual credit growth ($\Delta \ln c_{it}$) (y-axis) and that predicted by our estimates ($\Delta \hat{\ln} c_{it}$) (x-axis). $\Delta \hat{\ln} c_{it}$ is constructed as a weighted average of the change in credit at the bank-firm (loan) level, where weights are computed as the amount of credit extended to firm $j$ by bank $i$ as a fraction of total credit granted by bank $i$ (computed in $t-1$): $\Delta \hat{\ln} c_{it} = \sum_j \frac{c_{ijt-1}}{\sum_j c_{ijt-1}} \Delta \hat{\ln} c_{ijt}$ where $\Delta \hat{\ln} c_{ijt} = \hat{\delta}_{it} + \hat{\lambda}_{jt}$.

well as size dummies, lagged loan-to-assets ratio, and lagged productivity. Moreover, we include a set of sector $\times$ year dummies. Finally, $\overline{\delta}_j$ represents a firm-specific credit supply shock constructed as a weighted average of the supply shocks estimated for all banks in a relationship with firm $j$. The weights are given by the share of credit of each bank with this firm in the previous period:

$$\overline{\delta}_{jt} = \sum_i \frac{c_{ij,t-1}}{\sum_i c_{ij,t-1}} \hat{\delta}_{it} \tag{4}$$

Crucially, firms not directly hit by a credit supply shock may be affected through buyer-supplier relations (indirect effects). For instance, if a supplier of firm $j$ is hit by a negative credit supply shock, the reaction of this supplier may also affect production of firm $j$. We exploit our firm-level information combined with input-output linkages to study the propagation effects of our identified bank-credit supply shocks. Specifically, following di Giovanni et al. (2018) we combine firm-specific measures of usage intensity of material inputs and domestic sales with the sector-level input-output matrix.[12] We use IO relations for Spain for both propagation downstream (i.e., shocks from suppliers) and upstream (i.e., shocks from customers). In practice, we include two additional regressors in the empirical model in (3) to capture the indirect effects of credit shocks through input-output relations. We use the variable $DOWN_{jt,s}$, which measures the indirect shock received by firm $j$ operating in sector $s$ from its suppliers, as proxy for the downstream propagation effect:

$$DOWN_{jt,s} = \omega_{jt}^{IN} \sum_p IO_{ps} \Delta_{jt,p} \tag{5}$$

We use the variable $UP_{jt,s}$, which measures the indirect shock received by firm $j$ operating in sector $s$ from its cus-

[12] di Giovanni et al. (2018) construct proxies for indirect linkages between French firms and foreign countries inspired by the propagation terms in Acemoglu et al. (2015).

**Table 1**
Direct and Indirect real effects of credit shocks.

| | Direct | | | Direct + Indirect | | |
|---|---|---|---|---|---|---|
| | Employment (1) | Output (2) | Investment (3) | Employment (4) | Output (5) | Investment (6) |
| Credit Shock | 0.292*** | 0.103*** | 0.802*** | 0.284*** | 0.107*** | 0.798*** |
| (s.e.) | (0.097) | (0.030) | (0.069) | (0.098) | (0.029) | (0.075) |
| DOWN | | | | 0.301** | 0.354*** | 0.690*** |
| (s.e.) | | | | (0.119) | (0.069) | (0.174) |
| UP | | | | 0.061 | 0.209*** | 0.174 |
| (s.e.) | | | | (0.120) | (0.077) | (0.209) |
| # obs | 4,064,376 | 3,873,003 | 3,938,238 | 3,827,042 | 3,744,353 | 3,737,540 |
| R2 | 0.050 | 0.057 | 0.028 | 0.053 | 0.067 | 0.030 |
| Sample firms | All | All | All | All | All | All |
| Fixed effects | sector × year | sector × year | sector × year | sector × year | sector × year | sector × year |

*Notes.* This table reports the effect of credit supply shocks on employment (columns (1) and (4)), output (columns (2) and (5)), and investment (columns (3) and (6)) estimated using Eq. (3) (direct effects, columns (1)-(3)) and Eq. (7) (indirect effects, columns (4)-(6)) for the 2003–2013 period. The dependent variables are employment growth in %, output growth in %, and investment as a share of capital stock. *Credit Shock* refers to the firm-specific credit supply shock estimated in Eq. (4), normalized to have zero mean and unit variance. DOWN and UP have been constructed according to Eqs. (5) and (6) respectively. All regressions include the following control variables: firm-specific credit demand shocks ($\hat{\lambda}_{jt}$), size dummies, lagged loan-to-assets ratio, and lagged productivity. Regressions include 216 banks, and 812,067 firms in (1) and (4), 779,500 in (2) and (5), and 782,872 in (3) and (6). We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. Standard errors clustered at the main bank level are reported in parentheses.

tomers, as a proxy for the upstream propagation effect.

$$UP_{jt,s} = \omega_{jt}^{DO} \sum_p IO_{sp} \Delta_{jt,p} \tag{6}$$

In terms of notation, both $s$ and $p$ index sectors, and firm $j$ belongs to sector $s$. $\Delta_{jt,p}$ is the credit supply shock hitting sector $p$ computed as a weighted average of firm-specific shocks ($\overline{\delta}_{jt}$) using as weights the firm credit shares within the sector. Notice that this shock is firm-specific because firm $j$ is excluded from the computation of sector-specific shocks in the case that $s = p$. $IO_{ps}$ is the domestic direct requirement coefficient of the 2010 Spanish Input-Output matrix, defined as the share of spending on domestically-produced sector $p$ inputs for production in sector $s$. $\omega_{jt}^{IN}$ refers to total input usage intensity of firm $j$ in year $t$, defined as the total material input spending divided by material input spending plus wage bill. Finally, $\omega_{jt}^{DO}$ measures domestic sales intensity, defined as the domestic market share of firm $j$'s sales, that is total sales minus exports divided by total sales.

Armed with these indirect credit supply shocks, we estimate the following empirical model:

$$Y_{jt} = \theta \overline{\delta}_{jt} + \theta_D DOWN_{jt,s} + \theta_U UP_{jt,s} + \pi X_{jt} + \nu_{jt} \tag{7}$$

where all elements are defined as in Eqs. (3), (5), and (6).[13]

## 4. Results

In this section, we first present the baseline results for direct and indirect real effects of credit shocks (Section 4.1). Then we show the estimated effects for different subperiods in Section 4.2 and discuss several robustness exercises in Section 4.3.

### 4.1. Baseline estimates

Table 1 presents our baseline estimates for the direct and indirect effects for the 2003–2013 period on employment growth, output growth, and investment.

*Direct Effects:* Table 1 (columns (1)-(3)) reports the results of estimating Eq. (3) for the 2003–2013 sample. Column (1) reports the results using employment changes of firm $j$ in year $t$ as the left hand side variable $Y_{jt}$. Columns (2) and (3) use output changes and investment. We find positive and statistically significant effects of credit supply shocks across all specifications, and all estimated coefficients are significant at 1%. Our estimated coefficients are also economically sizable. Let us focus first on discussing the magnitude of the estimated coefficients for employment.

Our estimates from columns (1) imply that a one standard deviation increase in the firm's credit supply shock is associated with an increase in firm employment growth of around 0.29 percentage points, which represents approximately 93% of the average firm-level annual employment growth rate (0.31%) over the 2003–2013 period.[14] With respect to output, the estimated coefficient reported in column (2) implies that one standard deviation increase in firm credit supply shock is associated with an average increase in firm output growth of around 0.10 pp., approximately 20% of the observed firm-level annual value added growth (0.5%) over the 2003–2013 period. When looking at investment, the estimated coefficient reported in column (3) implies that one standard deviation increase in firm credit supply shock is associated with an increase in firm investment of 0.80 pp. This number represents 10% of the average observed investment rate over the 2003–2013 period. Finally, it is worth highlighting that

[13] It is worth highlighting that our main conclusions are robust to (i) separately include sector and year fixed effects instead of sector × year fixed effects; (ii) include the shares of domestic sales (not interacted) as a control in specification (7).

[14] Average firm-level annual growth refers to the simple average of the change of a variable as measured in our final sample of firms for a particular period. These are the variables that we refer to when comparing the size of our estimates throughout this section.

**Table 2**
Direct real effects of credit shocks by period.

| | Employment | | | Output | | | Investment | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) 2003–07 | (2) 2008–09 | (3) 2010–13 | (4) 2003–07 | (5) 2008–09 | (6) 2010–13 | (7) 2003–07 | (8) 2008–09 | (9) 2010–13 |
| Credit Shock | 0.251 | 0.503*** | 0.243** | 0.060** | 0.152*** | 0.109*** | 0.821*** | 0.625*** | 0.711*** |
| (s.e.) | (0.153) | (0.149) | (0.111) | (0.028) | (0.032) | (0.024) | (0.173) | (0.087) | (0.080) |
| # obs | 1,823,859 | 810,335 | 1,430,182 | 1,765,665 | 764,699 | 1,342,639 | 1,763,184 | 783,316 | 1,391,738 |
| R2 | 0.042 | 0.055 | 0.035 | 0.040 | 0.075 | 0.042 | 0.034 | 0.016 | 0.011 |

*Notes.* This table reports the effect of credit supply on employment, output and investment for the 2003–2007 period (columns (1), (4), (7)), 2008–2009 (columns (2), (5), (8)), and 2010–2013 (columns (3), (6), (9)) estimated from Eq. (3). Dependent variable is employment growth in % in columns (1)-(3); output growth in columns (4)-(6); and investment in columns (7)-(9). *Credit Shock* refers to the firm-specific credit supply shock estimated in Eq. (4), normalized to have zero mean and unit variance. All regressions include a set of industry × year fixed effects as well as the following control variables: firm-specific credit demand shocks ($\hat{\lambda}_{jt}$), size dummies, lagged loan-to-assets ratio, and lagged productivity. We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. Standard errors clustered at the main bank level are reported in parentheses.

these effects are quantitatively and statistically significant for small- and medium-sized firms while effects for larger firms are not statistically significant.

*Indirect Effects:* We also find strong evidence on the propagation of real effects of firms' credit supply shocks (Table 1, columns (4)-(6)). In fact, depending on the specification, we find that the estimated coefficients associated with our measure of downstream propagation, $DOWN_{jt,s}$, are similar or larger in magnitude than the estimated coefficients for direct effects. We find mixed evidence for the case of upstream propagation, $UP_{jt,s}$, with our estimated coefficients having different size and significance depending on the left hand side variable considered. Regarding employment regressions, our estimates imply that an increase of one standard deviation in the *DOWN* variable is associated with an increase of approximately 0.30 pp. in the change in employment, which compares with the estimated 0.28 pp. for the direct effect. We find an insignificant effect for the indirect upstream propagation shock (*UP*). Turning to output regressions, the coefficients associated with the two indirect propagation shocks are significant at 1%. In fact, the indirect effects dominate the direct effects in terms of magnitude. The downstream (upstream) effect is 0.35 (0.21), which is significantly larger than the direct effect of 0.10 pp. Finally, in the case of investment regressions, the indirect downstream shock is significant at the 1% level. As in the employment case, the direct and indirect downstream effects are relatively similar in magnitude, 0.80 pp. and 0.69 pp. respectively.

### 4.2. Expansion, financial crisis, and recession

As mentioned above, an advantage of our methodology is that it enables us to estimate year-by-year supply shocks. We now investigate how the real direct and indirect effects of firms' credit supply shocks change with the state of the macroeconomy. To that end, we break down our sample into three periods. Tables 2 and 3 report our estimated direct and indirect effects for employment, output, and investment. We report the full set of year-by-year estimates in Appendix D.

*Employment:* The estimates in Table 2 suggest that aggregate economic conditions contribute to the understanding of the effects of credit supply shocks on employment. For example, the estimated effect is not significant in the regressions run for the *expansion* period of 2003–2007 in column (1), but it is positive and statistically significant in the regressions run for the *financial crisis* of 2008–2009 and the *recession* period (2010–2013) in columns (2) and (3). In terms of magnitude, the estimated effects represent 18% and 10% of the actual employment growth in 2008–2009 and 2010–2013, respectively.

Turning to the indirect effects in Table 3, both downstream and upstream effects are not significant when focusing during the *expansion* (2003–2007). For the *financial crisis* 2008–2009 period, we find the effect of the indirect downstream propagation shock (*DOWN*) to be particularly strong relative to the direct shock (see column (2) in Table 3) while the effect of the indirect upstream propagation shock remains insignificant. With respect to the 2010–2013 period in column (3), the estimated effect for the *DOWN* variable is insignificant and we find a negative and marginally significant effect of the upstream propagation shock (*UP*).[15]

*Output:* The direct effects of credit supply shocks on output are significant in all the three sub-periods (see Table 2). However, the effect is particularly strong during the *financial crisis* of 2008–2009 when it represents 9% of the actual change in output against the 3% that represents over the *expansion* period (2003–2007). Turning to the indirect effects in Table 3, we find that the effects of the downstream and upstream propagation shocks are only significant during the *financial crisis* 2008–2009 period. In particular, the estimated downstream and upstream effects represent around 36% and 26% of the observed average annual growth rate over the 2008–2009 period.

*Investment:* Turning to investment, we find that the estimated coefficients associated with the direct effect are significant at 1% across all specifications in Table 2. In terms of magnitude, the estimated effects represent approximately 6% of the actual average investment rate of 12% for the *expansion* period (2003–2007), around 12% of the average investment rate of 5.11% for the *financial crisis*, and more than double the average investment rate of 0.59% for the *financial crisis*. When focusing on the indirect

[15] Carvalho et al. (2017) show theoretically that negative upstream propagation effects are possible under low substitution elasticities between labor and intermediate inputs.

**Table 3**
Indirect real effects of credit shocks by period.

| | Employment | | | Output | | | Investment | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | 2003–07 | 2008–09 | 2010–13 | 2003–07 | 2008–09 | 2010–13 | 2003–07 | 2008–09 | 2010–13 |
| Credit Shock | 0.218 | 0.482*** | 0.255** | 0.069** | 0.155*** | 0.108*** | 0.845*** | 0.576*** | 0.708*** |
| (s.e.) | (0.151) | (0.156) | (0.111) | (0.027) | (0.031) | (0.020) | (0.177) | (0.101) | (0.085) |
| DOWN | -0.077 | 0.697*** | 0.129 | 0.204* | 0.646*** | 0.184 | 0.266 | 1.263*** | 0.110 |
| (s.e.) | (0.076) | (0.258) | (0.392) | (0.106) | (0.166) | (0.251) | (0.281) | (0.320) | (0.552) |
| UP | 0.062 | -0.187 | -0.233* | 0.086 | 0.459*** | -0.014 | 0.403** | 0.085 | -0.402 |
| (s.e.) | (0.078) | (0.291) | (0.123) | (0.086) | (0.141) | (0.125) | (0.172) | (0.352) | (0.401) |
| # obs | 1,727,803 | 759,170 | 1,340,069 | 1,704,934 | 739,238 | 1,300,181 | 1,687,930 | 739,729 | 1,309,881 |
| R2 | 0.040 | 0.059 | 0.036 | 0.051 | 0.086 | 0.049 | 0.036 | 0.018 | 0.012 |

*Notes.* This table reports the effect of credit supply on employment, output and investment for the 2003–2007 period (columns (1), (4), (7)), 2008–2009 (columns (2), (5), (8)), and 2010–2013 (columns (3), (6), (9)) estimated from Eq. (7). The dependent variable is employment growth in % in columns (1)-(3); output growth in columns (4)-(6); and investment in columns (7)-(9). *Credit Shock* refers to the firm-specific credit supply shock estimated in Eq. (4), normalized to have zero mean and unit variance. DOWN and UP have been constructed according to Eqs. (5) and (6) respectively. All regressions include a set of industry × year fixed effects as well as the following control variables: firm-specific credit demand shocks ($\hat{\lambda}_{jt}$), size dummies, lagged loan-to-assets ratio, and lagged productivity. We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. Standard errors clustered at the main bank level are reported in parentheses.

effects in Table 3, the downstream effect is only significant and stronger than the direct effect in the *financial crisis* period.

*Summary:* Over the entire sample period 2003–2013, indirect credit shocks through IO downstream propagation have a significant effect on the evolution of firm-level employment, output and investment. This finding is driven by the *financial crisis* period (2008–2009) when the downstream propagation effect is statistically and economically significant. Indeed, during the 2008–2009 Global Financial Crisis, the estimated downstream effects systematically dominate the direct effects of credit shocks in magnitude. Note also that the differences in the estimated downstream coefficients between the *expansion* (2003–2007) and the *financial crisis* (2008–2009) periods are statistically significant with p-values below 0.1 for employment, value added and investment.[16] In contrast, the differences between the estimates for the *financial crisis* (2008–2009) and the *recession* (2010–2013) are not statistically significant. Finally, evidence on the importance of the upstream propagation shock is weak and mixed in terms of both significance and size of the effect.

### 4.3. Robustness Checks

Appendix E reports a battery of exercises that confirm our main findings to be robust along several dimensions. As discussed in Section 3, Amiti and Weinstein (2018) show that the bank-time fixed effects estimated from Eq. (1) are identical to those resulting from a specification accounting for bank-firm-time-specific factors. In Table E.2, we show this to be the case. We first include in Eq. (1) the lagged exposure between bank *i* and firm *j* in order to account for bank-firm idiosyncratic factors (see Table E.1). As expected from the findings in Amiti and Weinstein (2018), the results are not affected by the inclusion of these bank-firm-specific factors (see Table E.2).

To further alleviate endogeneity concerns, we split our sample into two subsamples, one exploited for the estimation of bank shocks and the other used for the regressions of firm outcomes on bank shocks from the first subsample. Concretely, we randomly divide firms' fiscal IDs into two groups of equal size. Firms used in the identification of the bank credit shocks are thus not included in the subsequent regressions on real outcomes. The aim of this exercise is to ensure exogeneity of the bank shocks with respect to firms' decisions as relationship lending is fully absent in these results. This robustness exercise resembles the Bartik (1991) identification strategy popularized by Blanchard and Katz (1992) in which local employment growth is predicted by interacting local industry employment shares with national industry employment growth rates. Analogously, we combine bank fixed effects identified from a group of firms with the firm-bank shares of a different group of firms. Table E.3 in Appendix E shows that our baseline results remain unaltered when considering these exercises thereby corroborating the exogeneity of our baseline bank credit shocks.

As an additional robustness, we restrict our sample of multibank firms for bank shock identification to those with at least 5 banks per year, to ensure that results are not driven by firms whose fixed-effects estimates can be noisy due to being identified from too few observations. Table E.4 illustrates the main conclusions to be robust to this exercise.

In Table E.5 we exclude construction and real estate firms from our sample to ensure that the Spanish boom-bust housing cycle is not driving our baseline findings. In the presence of bank specialization in real estate, construction firms may turn to specific banks for credit (housing banks) during the boom and to non-housing banks during the bust. Then, credit demand would also determine our so-called bank supply shocks. The esti-

[16] In the case of direct effects, differences are also statistically significant for employment and value added but not for investment

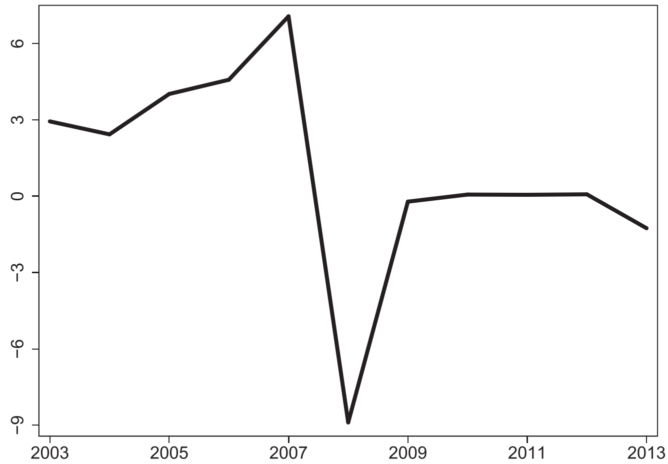


**Fig. 5.** Evolution of accounts payable growth (%).
*Notes.* This figure plots the evolution of average growth of accounts payable from our sample of Spanish firms.

mates in Table E.5 indicate that our findings hold when considering a sample of non-housing loans (i.e. excluding construction and real estate firms).

Finally, Appendix E.1 reports the real effects estimated for firms of different size. Overall the main patterns are quantitatively and statistically significant for small- and medium-size firms, while the estimated effects for larger firms are not statistically significant.[17] While these estimates point to potentially larger effects of credit shocks on real outcomes for SMEs, we acknowledge that the estimated standard errors render these differences barely significant.

## 5. Channels

The estimated direct and indirect effects of credit supply shocks on real variables are both statistically significant and economically sizable, particularly so during the financial crisis. Firms' real outcomes, conditional on their own credit supply shock, are also affected through buyer-supplier relations. To be more concrete, credit shocks affect not only the real outcomes of the firms directly, but also the real outcomes of their customers, i.e. downstream propagation.

In this section, we consider two different mechanisms that may rationalize these empirical findings. On the one hand, firms negatively affected by a credit shock may reduce the amount of trade credit that they extend to their customers. On the other hand, firms negatively affected by the shock may reduce their production, which induces an increase of the price of their products and thus affect their customers' optimal decisions in general equilibrium.

We first show that the trade credit channel explains part, but not the whole of the downstream propagation effect. We then show some evidence that is consistent with the price adjustment channel in general equilibrium and calibrate a model similar to Bigio and La'o (2020) that allows us to quantify the extent of that channel.

### 5.1. The role of trade credit

While bank lending generally represents the main source of firms' financing, trade credit is also important. In our sample, around 90% of the firms have positive accounts payable, and the average share of accounts payable over total credit is 47.6%. Costello (2020) documents that firms exposed to a decline in bank lending reduce the trade credit extended to their customers, resulting in negative effects on their real outcomes. This mechanism would thus rationalize our estimated downstream effects.[18] Fig. 5 shows a large drop in the average growth of accounts payable in 2008, which confirms that the financial crisis was indeed reflected in the form of a reduction in extended trade credit.

In order to explore the role of trade credit in explaining our findings on downstream propagation of credit supply shocks, we include an additional control variable (the growth of accounts payable by firm $j$) in our baseline specification from Eq. (7). The estimated coefficient on the $DOWN_{jt,s}$ regressor captures the effect of suppliers credit shock on firm $j$'s outcomes beyond the trade credit channel. Table 4 shows the results.[19] We find that the effect of accounts payable is always statistically significant and large, which corroborates the findings in Costello (2020) that trade credit shocks affect real outcomes of customer firms (buyers). We also find that, when controlling for the change in trade credit, the magnitude of the downstream propagation in 2008–2009 is considerably lower than in our baseline regression: 0.69 vs. 0.59 in the case of employment, 0.64 vs. 0.55 in the case of output, and 1.26 vs. 0.81 in the case of investment. However, our

[17] Note also that the cross-sector relationship between the share of large firms and downstreamnes is flat.

[18] Alternatively, trade credit may also explain upstream propagation of financial shocks if debtor (customer) failure triggers supplier's losses through both credit losses and demand shrinkage (see for instance Jacobson and Schedvin (2015)). However, we focus here on downstream propagation because our evidence for upstream effects is rather mixed.

[19] We focus in the 2008–2009 sub-period because accounts payable are only available for a small subsample of around 10,000 firms in 2003–2007. This is due to the fact that firms were not obliged to report this information to the Mercantile Registries before 2008.

**Table 4**
Indirect effects — the role of trade credit.

| | Employment | | Output | | Investment | |
|---|---|---|---|---|---|---|
| | (1) 2003–2013 | (2) 2008–2009 | (3) 2003–2013 | (4) 2008–2009 | (5) 2003–2013 | (6) 2008–2009 |
| Bank shock | 0.20** | 0.39*** | 0.08*** | 0.09*** | 0.61*** | 0.37*** |
| | (0.08) | (0.10) | (0.02) | (0.02) | (0.06) | (0.07) |
| DOWN | 0.47* | 0.59* | 0.41*** | 0.55*** | 0.66*** | 0.81*** |
| | (0.24) | (0.34) | (0.11) | (0.17) | (0.17) | (0.22) |
| UP | 0.28 | 0.28 | 0.14 | 0.27* | 0.14 | 0.32 |
| | (0.30) | (0.42) | (0.12) | (0.14) | (0.32) | (0.36) |
| Trade credit | 0.33*** | 0.37*** | 0.12*** | 0.22*** | 0.89*** | 0.75*** |
| | (0.05) | (0.07) | (0.04) | (0.08) | (0.18) | (0.24) |
| # obs | 1,175,489 | 225,549 | 1,149,871 | 221,186 | 1,152,278 | 221,140 |
| R2 | 0.04 | 0.04 | 0.06 | 0.09 | 0.01 | 0.01 |
| Fixed effects | sector × year | sector × year | sector × year | sector × year | sector × year | sector × year |

*Notes.* All regressions include the following control variables: firm-specific credit demand shocks ($\hat{\lambda}_{jt}$), lagged loan-to-assets ratio, and lagged productivity. We denote significance at 10%, 5% and 1% with *, ** and ***, respectively. Standard errors multi-clustered at the main bank and sector level are reported in parentheses. Trade credit refers to the growth of accounts payable of the firm, i.e., the growth of trade credit received from the firms' suppliers. All regressors are normalized to have zero mean and unit variance.

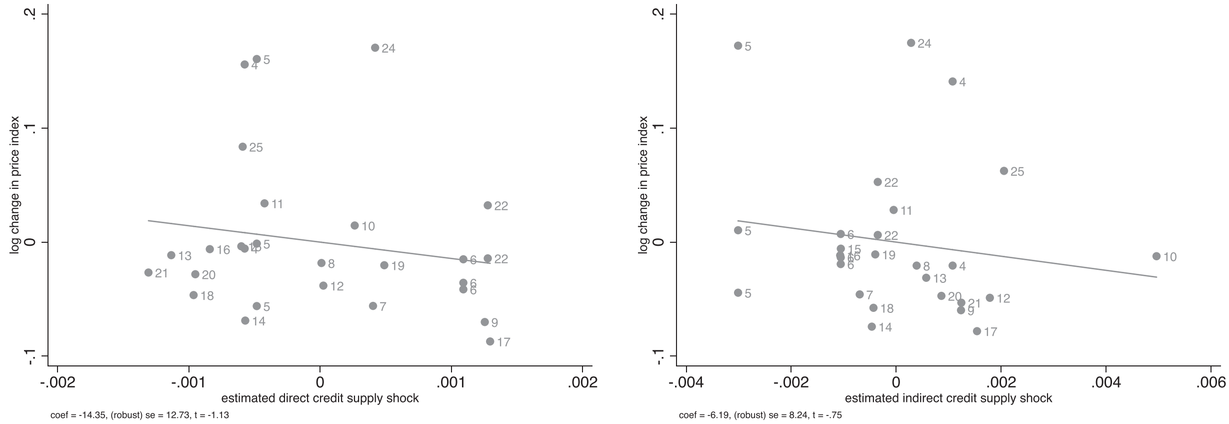


**Fig. 6.** Change in industrial price indexes and credit supply shocks.
*Notes.* This figure shows the partial correlation between the log change in industrial price indexes between 2007 and 2010 and our estimated direct and indirect credit supply shocks in 2007. The partial correlation has been computed from running a regression of the log change in prices against the two types of shocks. The source of the price indexes is *Indice the Precios Industriales, INE.* Price indexes are provided only for a limited number of industries. In particular, price indexes are not reported for service industries.

estimated effect of the suppliers credit shock ($DOWN_{jt,s}$) remains large and significant. Therefore, we conclude that some additional mechanism must be at work in order to explain the downstream propagation of credit shocks.

### 5.2. The role of price adjustments

The work by Acemoglu et al. (2012), Acemoglu et al. (2015), building on earlier work by Long and Plosser (1983), emphasize the role of input-output linkages in propagating sectoral shocks to the macroeconomy. The intuition is as follows: consider a negative supply shock that affects an industry producing good *i*. Its output decreases, which implies an increase in the price of the good *i*. Industries that use good *i* as an intermediate input now decrease their demand for that good, and as a consequence their production goes down and their price goes up. This affects industries that use their goods as inputs, and so on and so forth. The total effect on an economy is then a combination of the overall direct and indirect effects of the initial negative supply shock. Our estimates are consistent with this type of propagation.

In order to explore this mechanism, we construct changes in prices across different industries between 2007 and 2010 and correlate them with our estimated shocks for the year 2007. We construct credit supply shocks for each industry as a weighted average of the estimated firm level shocks as defined in Eq. (5), using firms' credit shares as weights. To compute changes in prices for each industry, we calculate the growth rate of industrial price indexes reported by the Spanish *Instituto Nacional de Estadística* over that period.

Fig. 6 shows the partial correlations deriving from a regression of the computed changes in prices against our direct and indirect (downstream) "credit shocks" at the industry level. The left panel shows the correlation between the log change in prices and the direct shock. The right panel shows the correlation with the indirect downstream shock. We find that both the direct and indirect shocks are negatively related to the change in prices. First, the price in a given industry increases when the industry faces a direct negative shock. Second, the price of that industry also increases when its suppliers face a negative shock. Note that these relationships confirm that the price mechanism is not sharply at odds with our data despite the fact that the statistical significance is admittedly weak.[20] In any event, it is worth noting that the lack of information on firm-level prices forces us to aggregate the firm-level credit shocks to the industry level, which substantially reduces the available variation in the data to identify statistically significant correlations.

### 5.2.1. Evidence from a general equilibrium model

To provide further evidence on the role of price adjustments, we calibrate a general equilibrium model that enables us to quantify the aggregate effects of our estimated credit supply shocks taking into account IO propagation. To this end, we use the model developed by Bigio and La'o (2020). This model is an otherwise standard general equilibrium model with input-output linkages extended to the presence of financial frictions and endogenous labor supply.

We start by describing the main features of the model, i.e., technology, financial constraints and preferences. We continue by presenting the firms' maximization problem. Finally, we describe the calibration strategy that allows us to quantify the aggregate effects of our reduced-form estimates through the channel of price adjustments in general equilibrium.

*Model's Fundamentals.* There are $n$ industries in the economy. In each of these industries $i = 1, \ldots, n$, there is a representative perfectly competitive firm that has access the following Cobb–Douglas production function:

$$y_i = z\left[ l_i^{\alpha_i} \left( \prod_{j=1}^{n} x_{ij}^{\omega_{ij}} \right)^{1-\alpha_i} \right]^{\eta_i} \tag{8}$$

where $y_i$ is the amount of units produced in industry $i$; $z$ is an aggregate productivity component that affects all industries equally; $x_{ij}$ is the amount of goods produced in industry $j$ used as inputs by industry $i$; $l_i$ is the amount of labor used by industry $i$; $\eta_i \in (0, 1)\ \forall i$ governs the fraction of revenue devoted to cover input expenditures, i.e., labor plus intermediate goods; $\alpha_i \in (0, 1)\ \forall i$ determines the share of labor in total input expenditures. Finally, $\omega_{ij}$ determines the share of intermediate good $j$ in total intermediate goods expenditure of industry $i$, with $\sum_{j=1}^{n} \omega_{ij} = 1$.

We assume the existence of working capital constraints, which implies that firms must pay wages and the cost of intermediate goods before production takes place. Firms must borrow for this purpose. Given some imperfections in financial markets, firms can only borrow up to a fraction $\chi_i$ of their revenue:

$$wl_i + \sum_{j=1}^{n} p_j x_{ij} \le \chi_i p_i y_i \tag{9}$$

The economy is populated by a representative household whose preferences are represented by the following utility function:

$$u(c, l) = \frac{c^{1-\gamma}}{1-\gamma} - \frac{l^{1+\epsilon}}{1+\epsilon} \tag{10}$$

where $c = \prod_{i=1}^{n} c_j^{\nu_j}$ with $\nu_j \in (0, 1)$ and $\sum_{j=1}^{n} \nu_j = 1$ is the composite consumption good and $l$ the amount of labor supplied by the household; $\gamma \ge 0$ captures the wealth effect on labor supply, whereas $\epsilon > 0$ captures the inverse of the substitution effect, i.e., the Frisch elasticity.

*Firms' maximization problem.* Taking all prices as given, a firm operating in industry $i$ solves the following maximization problem:

$$\max_{l_i, x_{ij}, \forall j} \left\{ p_i y_i - wl_i - \sum_{j=1}^{n} p_j x_{ij} \right\}$$

$$\text{subject to:} \quad y_i = z\left[ l_i^{\alpha_i} \left( \prod_{j=1}^{n} x_{ij}^{\omega_{ij}} \right)^{1-\alpha_i} \right]^{\eta_i}$$

$$wl_i + \sum_{j=1}^{n} p_j x_{ij} \le \chi_i p_i y_i$$

This problem can be solved in two stages. In the first stage, for a given level of expenditure $E_i = wl_i + \sum_{j=1}^{n} p_j x_{ij}$, the firm decides how to allocate this expenditure across the different production factors. The solution of this problem is given by:

$$wl_i = \alpha E_i \tag{11}$$

$$p_j x_{ij} = (1 - \alpha_i)\omega_{ij} E_i \tag{12}$$

In the second stage, the firm decides the level of expenditure $E_i$, which must satisfy:

$$E_i = \phi_i \eta_i R_i \qquad \text{where} \qquad \phi_i = min\left\{ \frac{\chi_i}{\eta_i}, 1 \right\} \tag{13}$$

Note that under decreasing returns to scale, the firm would always like to borrow an amount equal to $\eta_i p_i y_i = \eta_i R_i$. When $\eta_i \le \chi_i$, the firm will be able to borrow optimally. However, when $\eta_i > \chi_i$, the firm will borrow less than optimally and hence will be financially constrained. We provide further details on the definition of the household's maximization problem and equilibrium in Appendix G.

*Calibration.* In this section we describe our calibration strategy, which consists of the following steps. First, we calibrate the parameters of the model to the year 2003 by exploiting cross-industry variation in that year. Turning to subsequent years, we assume that all of the parameters remain constant except for those governing firms' financial constraints, i.e., the vector $\boldsymbol{\phi}$ that contains the industry-specific $\phi_i$'s, and the aggregate productivity component $z$.

[20] In a recent paper, Kim (2020) uses the ACNielsen Homescan data set to provide firm-level evidence that goes in the opposite direction. In particular, he finds that firms that were negatively affected by a credit supply shock decreased their output prices relative to unaffected firms in order to fire-sale their inventory and raise cash. Note however that the evidence in Kim (2020) comes from a different type of sample. While our evidence is based on national accounts information of industrial price indexes, his estimates are generated from a sample of large firms, which tend to rely more on inventory management practices.

Table H.1 in Appendix H summarizes our calibration strategy.

Year 2003: Our model economy is characterized by different sets of parameters: technological parameters, $\alpha_i$, $\eta_i$, $\omega_{ij}$; parameters related to preferences, $\gamma, \epsilon, \nu_i$; financial frictions, $\phi_i$; and the productivity shock $z$. We take some of them from outside the model by selecting conservative values similar to the ones used in the literature. We set the parameter governing decreasing returns to scale to $\eta_i = \eta = 0.90\ \forall i$. We set both $\epsilon$ and $\gamma$ to 1/2. The former implies a Frisch elasticity of 2. The latter implies little role for the wealth effect.

The rest of the parameters are chosen such that our model economy is consistent with some relevant cross-industry patterns that we observe in the Spanish economy in 2003. Our main source of information is the Input-Output table reported by the *Instituto Nacional de Estadística* (INE), which provides information at a 64-industry-level of disaggregation (this is the same source of information that we used in previous sections). From these tables, we can measure (i) the share of labor in industry $i$'s total input cost, which we use to identify $\alpha_i$ for all industries; (ii) the share of industry $i$'s in final consumption expenditure, which we use to identify $\nu_i$ for all industries; and the (iii) the expenditure on each industry $j$ as a fraction of total cost of intermediate goods by each industry $i$, which we use to identify the direct requirement coefficients $\omega_{ij}$. See Appendix H for further details.

To obtain initial values for $\phi_i$ in each industry, we exploit the fact that the cost-to-sales ratio in the model satisfies:

$$\frac{wl_i + \sum_{j=1}^{n} p_j x_{i,j}}{p_i y_i} = \phi_i \eta \quad \forall i$$

Given our assumed value of $\eta$ and data on sectoral gross output, labor and intermediate goods expenses, we can obtain a value of $\phi_i$ for each industry $i$ for the year 2003. Admittedly, attributing all the cross-industry variation in the cost-to-sales ratio to financial frictions is subject to some concerns. For example, different industries may have different degrees of decreasing returns to scale, or may be subject to distortions that are not necessarily related to financial frictions. This is why we use this strategy only to identify the initial level of financial frictions, which should not have first order implications when analyzing the propagation effects of their changes, which is our final goal in this section. Finally, we normalize the aggregate productivity component $z$ to be equal to one.

Subsequent years: As mentioned earlier, we assume that all the parameters remain constant over time except for the financial frictions $\boldsymbol{\phi}$ and the aggregate level of productivity $z$. For a given year $t > 2003$, we set the elements $\phi$ in vector $\boldsymbol{\phi}$ so that a version of the model without input-output linkages, i.e., a *horizontal economy*, perfectly matches the changes in employment across sectors predicted by reduced form estimates of the direct effect of credit supply shocks.[21] To obtain time-variant reduced form estimates of the direct effect of credit supply shocks at the industry level, we proceeded as follows:

a. We estimate the strength of the credit channel at the firm level by regressing firm's employment growth on credit growth instrumented by our firm-specific credit supply shocks $\bar{\delta}_j$:

$$\begin{aligned} \Delta \ln E_j &= \beta \Delta \ln c_j + \pi_{IV} X_j + u_j \\ \Delta \ln c_j &= \psi \bar{\delta}_j \qquad + \Phi_{IV} X_j + \nu_j \end{aligned} \tag{14}$$

where $\Delta \ln c_j$ refers to the credit growth of firm $j$, $\bar{\delta}_j$ is the bank supply shocks at the firm level defined in Eq. (4), and $X_j$ are firm level controls. The identification assumption is that bank credit supply ($\bar{\delta}_j$) affects firm's changes in employment only through its effect on credit. Note that the first stage captures the *bank lending channel* at the firm level. Moreover, the reduced form effect estimated in Eq. (3) of the main text is equal to this *bank lending channel* multiplied by the pass-through of credit to changes in employment: $\theta = \psi \times \beta$.

b. We estimate the year-by-year credit growth at the firm level predicted by credit supply shocks using the estimates from Eq. (14). More specifically, we compute:

$$\widetilde{\Delta \ln c_j} = \hat{\psi} \bar{\delta}_j \tag{15}$$

c. We compute the firm-level employment growth predicted by $\widetilde{\Delta \ln c_j}$:

$$\widetilde{\Delta \ln E_j} = \hat{\beta} \widetilde{\Delta \ln c_j} \tag{16}$$

d. We aggregate firm-level predicted employment growth to the sector level:

$$\widetilde{\Delta \ln E} = \sum_j \varphi_j \widetilde{\Delta \ln E_j} \tag{17}$$

where $\varphi_i$ refers to the employment weight of firm $i$ in a given sector in the previous year ($\varphi_i = \frac{E_{i(-1)}}{\sum_j E_{j(-1)}}$). Therefore, $\widetilde{\Delta \ln E}$ captures the direct effect of credit-supply shocks on changes in employment in a given sector at a given point in time.

Panel A of Fig. 7 shows the evolution of the predicted changes in employment for the aggregate economy, which is the result of aggregating $\widetilde{\Delta \ln E}$ using sectoral employment shares. Panel B shows the implied changes in $\boldsymbol{\phi}$, which we have computed by calculating the change in a weighted average of the implied $\phi_i$'s. The model predicts a relatively stable level of financial conditions over the 2003–2007 period, followed by a subsequent deterioration during the financial-crisis (2008–10) and a big collapse in the posterior recession. In Appendix F, we show the time evolution of aggregate credit shocks identified from an empirical strategy based on the identification of bank-specific time trends for credit supply. These estimates are reassuring as they provide a very similar picture to that of Panel B of Fig. 7.

Notice that our strategy to identify the evolution of financial frictions over time differs from the one used by Bigio and La'o (2020). The main reason is that we want to

[21] In particular, a *horizontal economy* is one in which we set $\alpha_i = \alpha = 1\ \ \forall i$ so that no intermediate inputs from other industries are used for production.

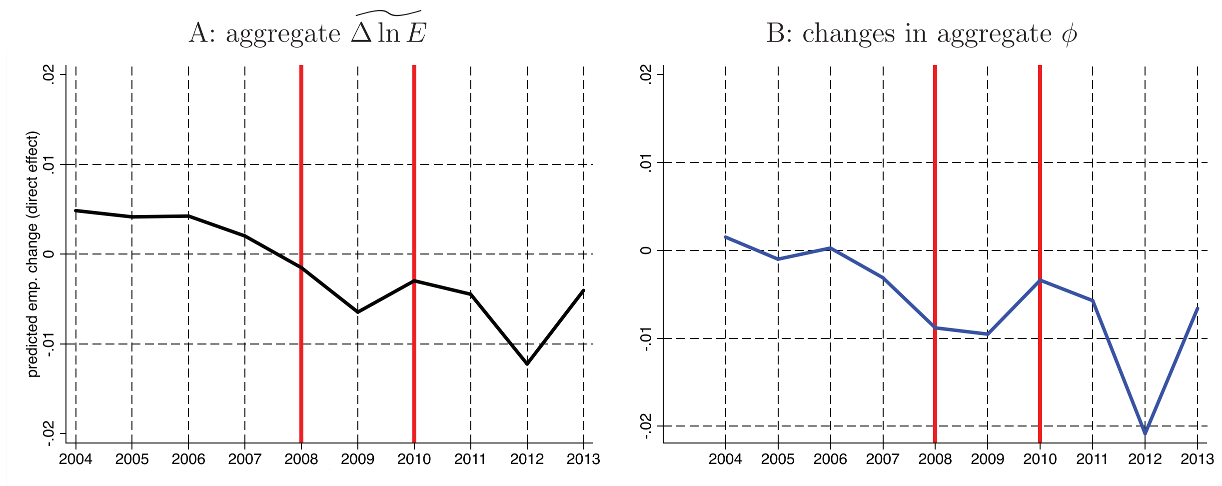


**Fig. 7.** Evolution of predicted sectorial direct effect on employment and implied.
*$\phi$Notes: Panel A in Fig. 7 shows the log changes in aggregate employment predicted by the direct effect according to the estimates from Eq. (14). Panel B shows the implied changes in the aggregate level of financial frictions, which have been computed as the change in the weighted average of the calibrated $\phi_i$'s.*

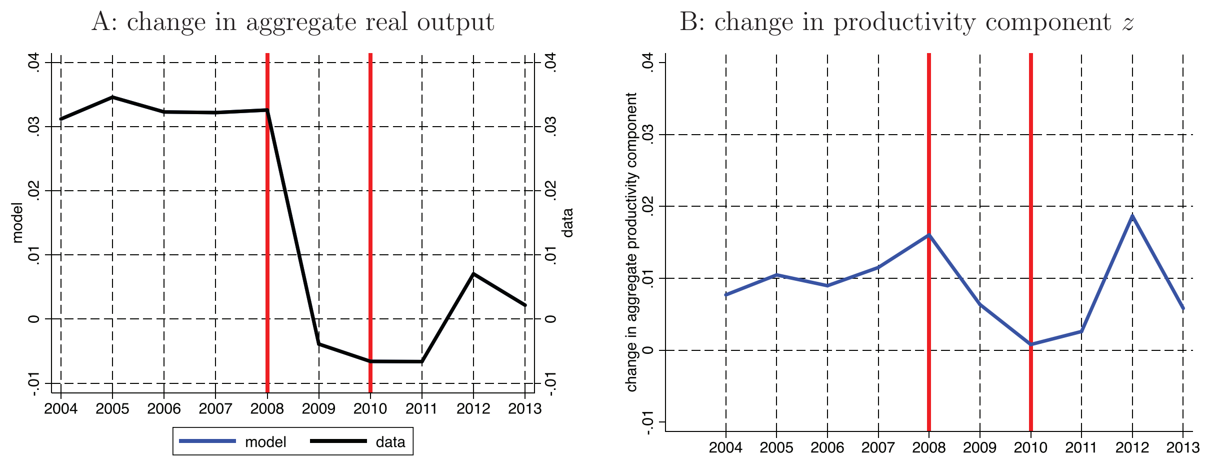


**Fig. 8.** Evolution of changes in aggregate real output and implied.
*$z$ Notes: Panel A in Fig. 8 shows the log changes in aggregate real output. Panel B shows the implied changes in the aggregate productivity component $z$.*

use a calibration that is tied to our reduced form estimates of the evolution of credit supply shocks. To the extent that we properly identify these effects in our regressions, the changes in the $\phi_i$'s that come out of our calibration should be credible. The idea behind this strategy is that we want our model to be consistent with the estimated direct effects of financial frictions, and allow the model to make predictions about the strength of the indirect effect. Alternatively, we could have used proxies for financial frictions over time to infer the evolution of the $\phi_i$'s. This strategy is the one used by Bigio and La'o (2020). In particular, they use measures of bond premia at the industry level constructed by Gilchrist and Zakrajsek (2012) to serve as a proxy for firms' financing costs. To the best of our knowledge, these industry-specific measures are not available for Spain.

To find the values of $z$ over time we proceed as follows. We first set $z = 1$ for the year 2003, as we mentioned earlier. We then set the $z$ in each period so that the full model matches perfectly the observed changes in aggregate real output. Panel A and Panel B of Fig. 8 show the evolution of changes in aggregate real output and the implied changes in the aggregate productivity component $z$. In Appendix H, we explain in detail the iterative procedure that we apply to jointly calibrate $\boldsymbol{\phi}$ and $z$ according to the strategy mentioned above.

Model fit: Fig. 9 shows the log changes in aggregate output predicted by the new calibrated model vs. those measured in the data data (panel A) and the log changes in aggregate employment predicted by the new calibrated model vs. those in the data (panel B). In terms of changes in aggregate real output, the fit of the model is perfect, which is achieved by construction given our calibration strategy. In terms of employment, changes in the data and in the model are highly correlated, moving together in all periods with the exception of the year 2012. However, the model tends to underestimate the size of the changes. For example, during the crisis, between the year 2008 and

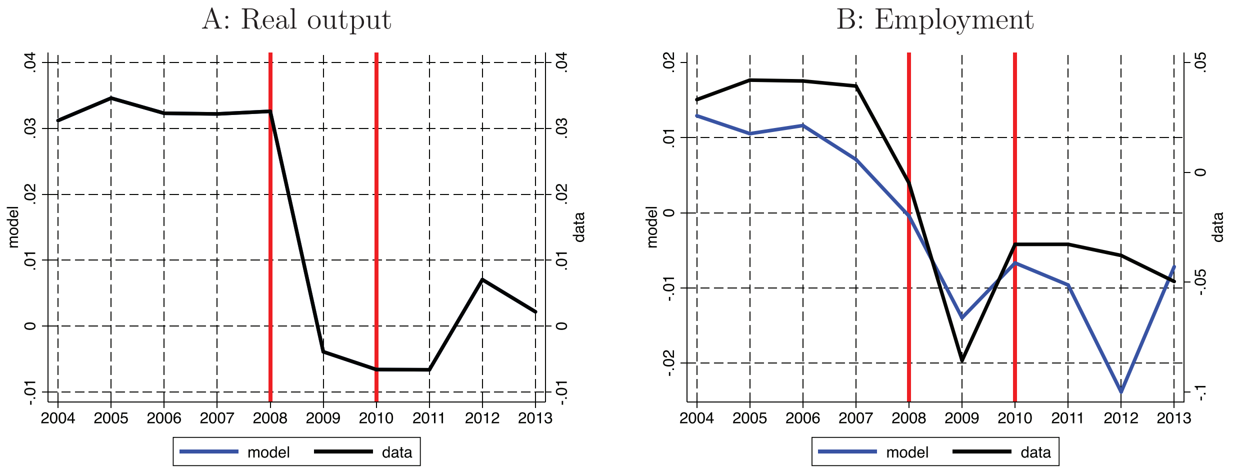


**Fig. 9.** Evolution of employment and output: model vs. data.
*Notes: Fig. 9 shows the log changes in aggregate output predicted by the calibrated model vs. data (panel A) and log changes in aggregate employment predicted by the calibrated model vs. data (panel B).*

**Table 5**
Counterfactuals.

| | Δ % Real Output | | Δ % Employment | |
|---|---|---|---|---|
| | (1) full economy | (2) horz. economy | (3) full economy | (4) horz. economy |
| 1. Benchmark | -0.39 | -0.01 | -1.40 | -0.64 |
| 2. Fixed $\phi_i$ $\forall i$ | 2.11 | 0.82 | 0.70 | 0.27 |
| 3. Change only $\phi_i$, $i =$ real state | -0.15 | 0.14 | -1.27 | -0.54 |
| 4. Change only $\phi_i$, $i =$ electricity | -0.23 | 0.01 | -1.30 | -0.61 |
| 5. Change only $\phi_i$, $i =$ construction | -0.24 | 0.00 | -1.30 | -0.62 |
| 6. Change only $\phi_i$, $i =$ wholesale | -0.25 | 0.03 | -1.26 | -0.58 |

*Notes. Table 5 shows the results of different counterfactual exercises. Columns (1) and (3) show the changes in real output and employment between 2008 and 2009 for the benchmark economy (row 1), a economy in which $\phi_i$ $\forall i$ are kept constant to their values in 2008 (row 2), and economies in which all $\phi_i$'s are kept constant to their values in 2008 except for one sector (rows 3, 4, 5, and 6). Columns (2) and (4) show the same changes but when shutting down the propagation effects in the model, i.e., imposing that $\alpha_i = 1$ $\forall i$.*

2009, our model predicts a fall in aggregate employment of 1.39%. Around half of this fall comes from the direct effect, which is our target, while the other half is generated by the input-output propagation. That is considerably lower than the 8% fall observed in the data.

*Counterfactuals.* We use the calibrated version of the model to run counterfactuals that allow us to quantify the role played by input-output propagation in accounting for the aggregate effects of credit supply shocks during the *financial crisis* (2008–2009).

Our starting point is the full calibrated model in the year 2008. In the first row of Table (5), we show the changes in output and employment (columns 1 and 3) between 2008 and 2009 implied by the model under the benchmark calibration, where the weighted average of $\phi$ decreases by around 1% and $z$ increases by around 0.75%. Columns 2 and 4 show the changes in output and employment under the same $\phi$ and $z$ as in the benchmark calibration, but when eliminating input-output linkages in the model, i.e., setting $\alpha_i = 1$ $\forall i$. The fall in aggregate real output predicted by the full model (-0.39%), i.e., with input-output propagation, is significantly higher than that predicted by the horizontal economy (-0.01%). Therefore, in the absence of input-output linkages and under the same growth in $z$ (0.75%), the same financial shocks would have generated almost no fall in output. In terms of changes in employment, the difference between the full economy and the horizontal economy is smaller but still sizeable: -1.40% vs. -0.64%.

In rows 2–6, we carry out a number of exercises to quantify the aggregate effects of financial friction shocks to particular sectors that are central in the Spanish economy, i.e., sectors that are intensively used (both directly and indirectly) by other sectors.[22] In row 2, we start by showing the counterfactual changes in output and employment under a scenario in which we keep the value of $\phi_i \, \forall i$ fixed over time. The model predicts that the Spanish economy would have grown 2.11% in terms of output and 0.70% in terms of employment between 2008 and 2009 in the absence of financial shocks – growth driven by the direct effect of the positive change in $z$ and its propagation. The counterfactual growth would have been lower in the absence of input-output propagation (0.82%). In row 3, we

[22] We compute this by taking the sum of the column associated to a given sector of the total requirement matrix. Carvalho and Tahbaz-Salehi (2029) show that, under certain assumptions on preferences, this measure coincides with the so-called *Bonacich centrality*.

solve for an economy in which we keep fixed $\phi_i \forall i$ except for that of the *"Real State"* sector. We find that the calibrated financial shock to this sector in isolation would have generated an output and employment loss of -0.15% and -1.27%, respectively. In the case of output, all of this fall is explained by the propagation effect; in the absence of input-output linkages real output would have grown 0.14% under the same financial shock. Rows 4–6 show the results from similar exercises but considering financial shocks in isolation to other central sectors in the Spanish economy: *"Electricity"*, *"Construction"*, and *"Wholesale"*. The results again show the importance of input-output linkages in explaining the aggregate effects of credit supply shocks to particular sectors. In the case of *"Wholesale"*, for example, its calibrated shock to $\phi$ in isolation would have generated an output loss of -0.25% under input-output propagation, and almost no change (0.03%) in the absence of it.

## 6. Concluding remarks

In this paper, we study the direct and indirect real effects of the bank lending channel. Using the quasi-census of firms' loans and economic activity for Spain and input-output linkages, we analyze the real effects of bank-lending shocks during the period of 2003–2013. This period allows us to study firms' responses to different shocks during times of boom (expansion) and contraction (financial crisis and recession).

We bring to this analysis methods from the matched employer-employee literature combined with a methodology that enables analyzing the evolution of credit shocks over time. Specifically, we construct firm-specific, exogenous credit supply shocks and estimate their direct effects on firm credit, employment, output, and investment over a decade. We find sizable effects of credit supply shocks on real outcomes, particularly during the Global Financial Crisis.

Combining the Spanish Input-Output structure and firm-specific measures of upstream and downstream exposure, we find the estimated bank credit supply shocks to have strong downstream propagation effects, especially during the the Global Financial Crisis. The massive reduction in trade credit extended by suppliers as well as price adjustments in general equilibrium seem to explain the downstream propagation of credit shocks.

Our results show that credit supply shocks affect the real economy through sizable direct and indirect effects that affect investment and output primarily. Loan, firm, direct, and indirect effects are quantitatively important during the financial crisis but the impact cannot be generalized to other episodes. Overall, our results corroborate the importance of network propagation in quantifying the real effects of credit shocks. In terms of mechanisms, we find evidence which is consistent with both trade credit and general equilibrium adjustments being quantitatively relevant.

## Appendix A. Bank lending channel at the loan-level

We estimate the magnitude of the so-called bank lending channel at the bank-firm (loan) level. In particular, quantifying the bank lending channel amounts to estimating the $\beta$ coefficient in the following model:

$$\Delta \ln c_{ijt} = \beta \hat{\delta}_{it} + \eta_{jt} + \nu_{ijt} \tag{18}$$

where $\Delta \ln c_{ij}$ refers to the credit growth between bank $i$ and firm $j$ in year $t$, $\hat{\delta}_{it}$ represents the estimated bank-specific supply shock,[23] and $\eta_{jt}$ accounts for firm-year demand shocks. The lending channel corresponds to the parameter $\beta$. Crucially, the availability of firms borrowing from different banks enables us to include in the regression time-varying firm-fixed effects ($\eta_{jt}$) to control for the demand side (see Khwaja and Mian, 2008). Bank supply shocks $\delta_{it}$ are proxied by exogenous changes in deposits in Khwaja and Mian (2008), or access to securitization in Jimenez et al. (2020). In our case, we exploit the bank supply shocks estimated above (see Section 3.1), standardized to have zero mean and unit variance. In contrast to previous literature, because we have estimated bank credit supply shocks for each year,[24] we can also estimate the evolution of the bank lending channel over time.

Note that Eq. (18) can only be estimated for the sample of multibank firms given the inclusion of firm-year fixed effects. However, the availability of time-varying firm fixed effects ($\hat{\lambda}_{jt}$) estimated in Section 3.1 enables us to estimate the bank lending channel parameter in the sample of all firms as follows:[25]

$$\Delta \ln c_{ijt} = \beta \hat{\delta}_{it} + \gamma \hat{\lambda}_{jt} + \nu_{ijt} \tag{19}$$

Table A.6 reports the estimates of the bank lending channel at the bank-firm (loan) level. Column (1) presents the results of estimating Eq. (18) using the entire period (2003–2013). We find a positive and significant effect: conditional on firm fixed effects, higher estimated bank shocks imply higher growth in credit at the bank-firm level. In terms of magnitude, our estimates imply that a one standard deviation increase in the credit supply shock of bank $i$ generates a 5.1 pp. increase in credit growth between bank $i$ and firm $j$. It is worth mentioning that when we re-estimate column (1) without firm-specific effects on the same sample of multibank firms, the bank lending channel is less important, the effect dropping from 5.1 pp. to 4.2 pp. This reduction indicates that banks' supply and firms' loan demand shocks are negatively correlated in the cross-section as also found by Khwaja and Mian (2008).

Column (2) of Table A.6 repeats the estimation of column (1) but substitutes our firm-year effects ($\hat{\lambda}_{jt}$)

[23] The shocks are standardized to have zero mean and unit variance in order to ease interpretation and enhance comparability across specifications (and time periods) of the estimated effects magnitudes. Note that without such standardization the estimated $\beta$ should be equal to 1.

[24] Since our regressor of interest is estimated in a first step, standard errors in Eq. (18) should be adjusted in order to account for the sampling error from the first step. However, the adjustment factor in linear models resembles the traditional sandwich formula that depends on the variance of the estimated parameters in the first step (see Murphy and Topel, 1985). Give the huge sample sizes we are using in the first step, the correction factor for the second step tends to have a negligible effect on our second-step inferences because the first-step variance is close to zero.

[25] Note that firm-specific shocks are recovered for firms without multiple bank relationships by subtracting the bank-specific component $\hat{\lambda}_{jt} = \Delta \ln c_{ijt} - \hat{\delta}_{it}$.

 

**Table A1**
Estimates of the bank lending channel at the loan-level.

| | 2003–2013 | | | 2003–2007 | 2008–2009 | 2010–2013 |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Credit Shock | 5.058*** | 5.218*** | 5.272*** | 5.401*** | 5.320*** | 5.181*** |
| (s.e.) | (0.088) | (0.037) | (0.025) | (0.021) | (0.062) | (0.063) |
| # obs | 12,216,375 | 12,216,375 | 17,954,745 | 7,624,590 | 3,682,414 | 5,124,886 |
| # banks | 221 | 221 | 221 | 209 | 192 | 192 |
| # firms | 700,722 | 700,722 | 1,511,767 | 1,183,558 | 1,049,208 | 1,019,567 |
| R2 | 0.350 | 0.349 | 0.522 | 0.543 | 0.503 | 0.484 |
| Fixed effects | firm × year | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ |
| Sample firms | Multibank | Multibank | All | All | All | All |

*Notes.* This table reports the estimates of the bank lending channel parameter at the loan level ($\beta$). Column (1) is based on Eq. (18) for a sample of multibank firms. Columns (2) are (3) are based on Eq. (19), controlling for the firm-year estimated fixed effects. The dependent variable is credit growth between firm $j$ and bank $i$. *Credit Shock* refers to the bank-specific credit supply shock ($\hat{\delta}_{it}$) estimated in Eq. (1), normalized to have zero mean and unit variance. We denote significance at 10%, 5%, and 1% by *, **, and ***, respectively. Standard errors clustered at the bank level are reported in parentheses.

estimated in Section 3.1 for the firm-year dummies. As expected, the estimates of the bank lending channel remain very similar as both approaches are equivalent (see Cingano et al., 2016). In column (3), we repeat the estimation for the sample including all firms, not only multibank firms, which is possible because of the availability of firm-specific effects ($\hat{\lambda}_{jt}$) for all firms in the sample. Finally, columns (4)-(6) show the magnitude of the bank lending channel at the loan-level to be stable over time. Fig. C.1 in Appendix C presents the year-by-year estimates of the loan-level effect. Finally, all the figures in Table A.6 remain very stable when controlling for bank-firm idiosyncratic factors in the identification of bank supply shocks (see discussion in Robustness Section 4.3 and Table E.1 in the Appendix).

## Appendix B. Bank lending channel at the firm-level

The bank lending channel appears to be quantitatively and statistically important given the loan-level estimates reported in Section Appendix A. Moreover, the magnitude of the effect is similar for multibank and single bank firms. However, firms may be able to undo a negative bank supply shock by resorting to other banks, especially in the case of multibank firms. If this is the case, a large drop in the credit of a client firm with a bank affected by a negative supply shock would not capture the actual effect of credit supply on annual credit growth. In order to obtain such an estimate, we consider the following regression at the firm level:

$$\Delta \ln c_{jt} = \beta^F \overline{\delta}_{jt} + \gamma^F \hat{\lambda}_{jt} + u_{jt} \tag{20}$$

where $\overline{\delta}_j$ represents a firm-specific credit supply shock constructed as a weighted average of the supply shocks estimated for all banks in a relationship with firm $j$. The weights are given by the share of credit of each bank with this firm in the previous period:

$$\overline{\delta}_{jt} = \sum_i \frac{c_{ij,t-1}}{\sum_i c_{ij,t-1}} \hat{\delta}_{it} \tag{21}$$

Given this specification, the bank lending channel at the firm-level can be estimated from $\beta^F$, as in Khwaja and Mian (2008) and Jimenez et al. (2020). As in the loan-level case, however, we can obtain time-varying estimates of the bank lending channel.

We also account for demand shocks at the firm-level. In the case of loan-level data, the inclusion of firm unobserved heterogeneity is possible due to the circumstance of firms borrowing from different banks. This approach is no longer possible when using firm-level data. Under these circumstances, Khwaja and Mian (2008) and Jimenez et al. (2020) take recourse to the correlation between supply and demand effects implied by differences between the OLS and FE estimates at the loan-level to correct the biased OLS estimate of $\beta^F$. In particular, they exploit the fact that differences between the OLS and FE estimates at the loan-level in Eq. (18) provide a quantification of the covariance between $\delta_{it}$ and $\eta_{jt}$ given the formula for omitted variable bias. In our case, we include, in the firm-level regression, the firm-level demand shocks ($\hat{\lambda}_{jt}$) estimated in Section 3.1 by means of matched employer-employee techniques. Both approaches are equivalent but including the estimated demand shocks enables us to easily compute appropriate standard errors (see Cingano et al., 2016).

Table B.1 reports the estimates of the bank lending channel at the firm-level. The effect is positive and significant. The magnitude is smaller than that estimated at the loan-level, which indicates that firms are able to partially offset bank supply shocks. Not surprisingly, multibank firms can better undo bank shocks: a one standard deviation increase in the credit supply of firm $j$ generates an overall increase of 3.2 pp. in credit growth (see column (2)), whereas the effect is 1.1 pp. in the case of multibank firms, as reported in column (1). Turning to the evolution of the bank lending channel at the firm-level, columns (3)-(5) illustrate that the effect of bank shocks on firm credit growth is significantly larger during the 2008–2009 financial crisis. In particular, a one standard deviation increase in credit supply generates a 4.8 pp. increase in credit growth during those years (average firm credit growth during 2008–2009 was -6.2%), which is significantly larger than the effect during 2003–2007 and 2010–2013. Fig. C.1 in Appendix C presents the year-by-year estimates of this effect. Note also that these estimates are robust to the inclusion of bank-firm controls as well as the exclusion of construction and real estate firms

**Table B1**
Estimates of the bank lending channel at the firm-level.

| | 2003–2013 | | 2003–2007 | 2008–2009 | 2010–2013 |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Credit Shock | 1.158** | 3.207*** | 3.414*** | 4.846*** | 2.162*** |
| (s.e.) | (0.515) | (0.278) | (0.197) | (0.483) | (0.564) |
| # obs | 4,424,519 | 8,743,459 | 4,122,017 | 1,920,723 | 2,700,719 |
| # banks | 220 | 220 | 208 | 191 | 193 |
| # firms | 924,441 | 1,481,377 | 1,183,558 | 1,049,208 | 1,019,567 |
| R2 | 0.330 | 0.501 | 0.525 | 0.521 | 0.412 |
| Sample firms | Multibank | All | All | All | All |

*Notes.* This table reports the estimates of the bank lending channel parameter at the firm level ($\beta^F$) estimated from Eq. (20). The dependent variable is the credit growth of firm $j$ in year $t$. *Credit Shock* refers to the firm-specific credit supply shock ($\hat{\delta}_{jt}$) estimated in Eq. (4), normalized to have zero mean and unit variance. All specifications include a set of firm-year effects ($\hat{\lambda}_{jt}$). We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. Standard errors clustered at the main bank level are reported in parentheses.

from the sample (see Section 4.3 for more details these robustness exercises in the case of real outcomes).

Interestingly enough, the magnitude of the bank lending channel at the firm-level varies significantly over the cycle (see Table B.1) while it does not vary at the loan-level (Table A.6). Since loan-level effects are very similar across the different subperiods, the larger effects at the firm-level during the financial crisis points to a more limited capacity of firms to substitute credit across banks during this period. This finding may also be at the root of the larger real effects of credit shocks during the global financial crisis discussed below.

Finally, it is worth mentioning that including firm-year demand shocks in the model has a crucial effect on the estimates. Re-estimating the model in (20) by OLS without firm-level effects ($\hat{\lambda}_{jt}$), the 2003–2013 estimate of $\beta^F$ drops from 3.2 pp. to 0.7 pp., indicating that banks' supply and firms' loan demand shocks are negatively correlated in the cross-section, as found in the loan-level case.

In terms of comparisons with the literature, although Jimenez et al. (2020) find credit supply shocks to have had no significant effects on credit growth at the firm-level between 2004 and 2007, both results are not strictly comparable given differences in the nature of the bank supply shocks and data sample. On one hand, they analyze supply shocks identified through greater access to securitization of real estate assets. The sample in Jimenez et al. (2020) covers loans in excess of € 60,000, mainly corresponding to larger multibank firms that may be better able to undo bank supply shocks by borrowing from other banks as our estimates suggest.

## Appendix C. Annual estimates of the bank lending channel

The left panel in Fig. C.1 plots year-by-year estimates of the bank lending channel at the loan-level. Despite including only multibank firms, our sample consists, on average of 1,632,249 loans in each year. Therefore, the coefficients are very precisely estimated (note that standard errors are multi-clustered at the bank and firm level—see Cameron et al. (2011)). The magnitude of the bank lending channel is sizable: an increase of one standard deviation in bank supply generates an average increase of 5.2 percentage points in the growth of each bank-firm credit ($\Delta \ln c_{ij}$). The highest average bank-firm credit growth is 6.25% in 2007. Moreover, Fig. C.1 also points to an increase in the relevance of the bank lending channel during the crisis.

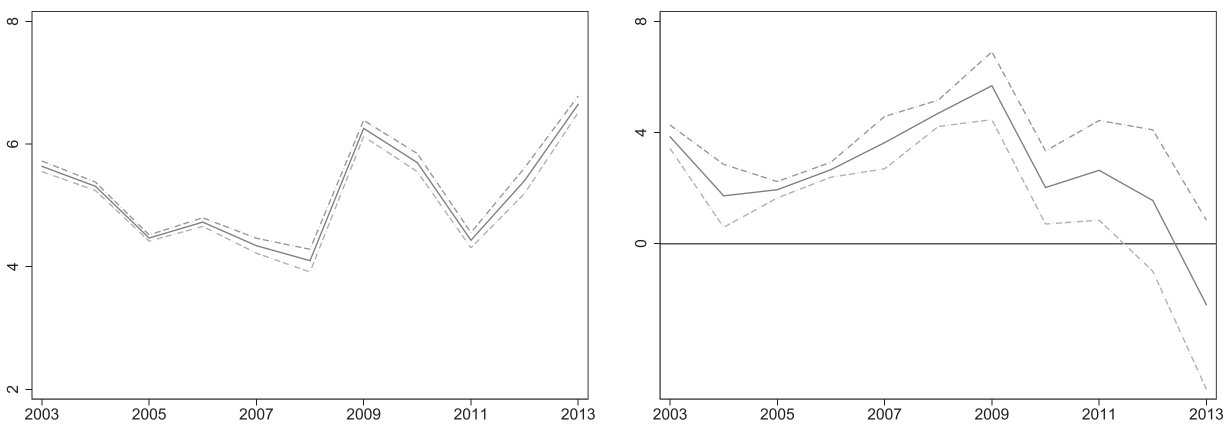


**Fig. C1.** Time-varying estimates of the bank lending channel at the loan- and firm-level.
*Notes.* The left panel plots the $\beta$ estimates from year-by-year regressions using Eq. (18). Standard errors used to construct the confidence bands are multi-clustered at the bank and firm level. The right panel plots the $\beta^F$ estimates from year-by-year regressions given by Eq. (20), which identify the bank lending channel at the firm level.

The right panel in Fig. C.1 plots time-varying estimates of the bank lending channel at the firm level. In this case, our sample comprises, on average, 870,734 firms per year. The magnitude of the bank lending channel is still sizable at the firm level: an increase of one standard deviation in bank supply generates an average increase of 2.6 percentage points in credit growth at the firm level ($\Delta \ln c_j$). The highest firm-level credit growth in our data is 5.9% in 2006, underscoring that the bank lending channel still operates at the firm level.

## Appendix D. Annual estimates of real effects

Fig. D.1 plots the estimated direct and indirect effects of credit supply shocks on firm growth in terms of employment (upper panel), output (middle panel), and investment (bottom panel). We find a positive and statistically significant direct effect of credit supply shocks on employment growth at the firm level for all years in our sample. However, note that the statistical significance is only marginal during the years 2004–2007. An increase of one standard deviation in bank supply generates an average increase of 0.3 percentage points in annual employment growth at the firm level while annual employment growth in our sample is, on average, 2.9%. These estimates confirm the larger real effects of the credit channel during the 2008–2009 credit collapse. Downstream effects are only positive and significant during 2008–2009 as reported in the main text while upstream effects are statistically indistinguishable from zero in all years. The magnitude of these propagation effects is larger than that of the direct effects.

The effects of firm-level credit supply shocks on output growth are positive and statistically significant on output growth for most years in the sample. A one standard deviation increase in the credit supply shock generates an average increase of 0.2 pp. in firm output growth, which accounts for 20% of the average output growth of 1.0% observed in the sample. Regarding propagation, there is a positive and significant downstream effect during 2007–2009. The effects are not significant before and after that period. In contrast to employment, there is a positive upstream effect during the global financial crisis.

The direct effects are larger and always significant in the case of investment, as reported in the main text. In line with the findings for employment and output, the magnitude of the indirect effects is also larger than that of

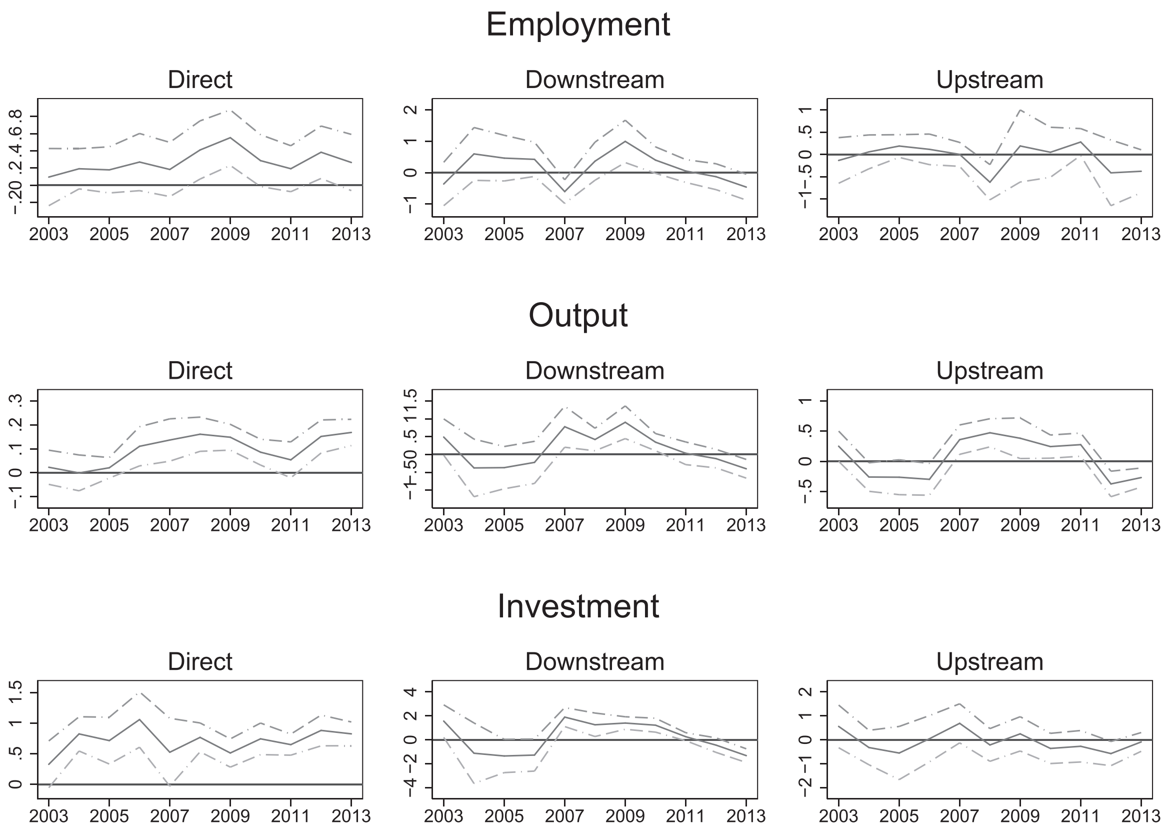


**Fig. D1.** Reduced-form effects of the bank lending channel on firm growth.
*Notes.* This figure plots the estimated direct and indirect effects of credit supply shocks from year-by-year regressions. Specifically the figure plots the effect of a one standard deviation increase in the credit supply shock on annual employment and output growth as well as investment in percentage points. The estimation samples includes, on average, 347,913, 340,396 and 339,776 firms in each year. Standard errors used to construct the confidence bands are multi-clustered at the main bank and industry level.

the direct effects, but insignificant in the case of upstream propagation. The estimated downstream effects are larger and more precisely estimated around the global financial crisis in 2008–2009.

## Appendix E. Robustness checks

The following tables summarize the estimated effects of a series of robustness to the main analysis considering different samples for identification of the shocks and for estimation of the real effect, additional controls at the bank-firm-year level, and excluding construction and real estate firms. The tables report estimates for the entire period (2003–2013) and the *financial crisis* (2008–2009).

### *E1. Results by Firm-Size*

The richness of our sample and identified shocks enables us to run our specification from Eq. (7) for three size bins: 0–10, 10–500, $\geq$ 500. Table E.6 reports the regression outcomes. Our main result from these regressions is that the largest firms do not seem to be affected either directly or indirectly by the estimated credit supply shocks. In particular, when we run the regression for firms with more

**Table E1**
Robustness I — Estimates of the bank lending channel controlling for bank-firm characteristics.

| | 2003–2013 | | | 2003–2007 | 2008–2009 | 2010–2013 |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Credit Shock | 5.341*** | 5.201*** | 5.163*** | 5.225*** | 5.329*** | 5.276*** |
| (s.e.) | (0.122) | (0.102) | (0.083) | (0.098) | (0.107) | (0.081) |
| # obs | 12,216,375 | 12,216,375 | 17,954,745 | 7,624,590 | 3,682,414 | 5,124,886 |
| # banks | 221 | 221 | 221 | 209 | 192 | 192 |
| # firms | 700,722 | 700,722 | 1,511,767 | 1,183,558 | 1,049,208 | 1,019,567 |
| R2 | 0.353 | 0.349 | 0.524 | 0.542 | 0.502 | 0.484 |
| Fixed effects | firm $\times$ year | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ | $\hat{\lambda}_{jt}$ |
| Sample firms | Multibank | Multibank | All | All | All | All |

*Notes.* This table reports the estimates of the bank lending channel parameter at the loan level ($\beta$) controlling for bank-firm idiosyncratic factors (lagged credit exposure) in the identification of the bank lending shocks. See notes to Table E.1 in the main text for more details.

**Table E2**
Robustness II — Shock identification including bank-firm controls in the regression.

| | Employment | | Output | | Investment | |
|---|---|---|---|---|---|---|
| | (1) 2003–2013 | (2) 2008–2009 | (3) 2003–2013 | (4) 2008–2009 | (5) 2003–2013 | (6) 2008–2009 |
| mean annual growth (%) | 0.312 | -2.764 | 0.508 | -1.755 | 7.572 | 5.111 |
| Credit Shock coefficient ($\theta$) | 0.299*** | 0.568*** | 0.106*** | 0.167*** | 0.786*** | 0.632*** |
| $\lvert\theta$/mean annual growth (%)$\rvert$ | 0.96 | 0.21 | 0.21 | 0.10 | 0.10 | 0.12 |
| *DOWN* coefficient ($\theta_D$) | 0.276** | 0.674** | 0.408*** | 0.627*** | 0.875*** | 1.239*** |
| $\lvert\theta_D$/ mean annual growth (%)$\rvert$ | 0.88 | 0.24 | 0.80 | 0.36 | 0.12 | 0.24 |
| *UP* coefficient ($\theta_U$) | 0.055 | -0.178 | 0.229*** | 0.447*** | 0.219 | 0.094 |
| $\lvert\theta_U$/mean annual growth (%)$\rvert$ | 0.18 | 0.06 | 0.45 | 0.25 | 0.03 | 0.02 |

*Notes.* This table summarizes the estimated effects when including bank-firm controls in the shock identification regression. We focus on the estimates for the entire period (2003–2013) and the *financial crisis* (2008–2009). *Mean annual growth (%)* refers to the average annual growth rate of the variable as measured in the sample of firms in a particular period. We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. $\lvert\theta$/mean annual growth (%)$\rvert$ is the absolute value of the estimated coefficient divided by the mean annual growth (%).

**Table E3**
Robustness III — Different subsamples for shock identification and real effects estimation.

| | Employment | | Output | | Investment | |
|---|---|---|---|---|---|---|
| | (1) 2003–2013 | (2) 2008–2009 | (3) 2003–2013 | (4) 2008–2009 | (5) 2003–2013 | (6) 2008–2009 |
| mean annual growth (%) | 0.312 | -2.764 | 0.508 | -1.755 | 7.572 | 5.111 |
| Credit Shock coefficient ($\theta$) | 0.277** | 0.594*** | 0.115*** | 0.175*** | 0.784*** | 0.617*** |
| $\lvert\theta$/mean annual growth (%)$\rvert$ | 0.89 | 0.21 | 0.23 | 0.10 | 0.10 | 0.12 |
| *DOWN* coefficient ($\theta_D$) | 0.316** | 0.663** | 0.344*** | 0.622*** | 0.662*** | 1.230*** |
| $\lvert\theta_D$/ mean annual growth (%)$\rvert$ | 1.01 | 0.24 | 0.68 | 0.35 | 0.09 | 0.24 |
| *UP* coefficient ($\theta_U$) | 0.065 | -0.186 | 0.200** | 0.458*** | 0.147 | 0.084 |
| $\lvert\theta_U$/mean annual growth (%)$\rvert$ | 0.21 | 0.07 | 0.39 | 0.26 | 0.02 | 0.02 |

*Notes.* This table summarizes the estimated effects when considering different samples for identification of the shocks and for estimation of the real effects. We focus on the estimates for the entire period (2003–2013) and the *financial crisis* (2008–2009). *Mean annual growth (%)* refers to the average annual growth rate of the variable as measured in the sample of firms in a particular period. We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. $\lvert\theta$/mean annual growth (%)$\rvert$ is the absolute value of the estimated coefficient divided by the mean annual growth (%).

**Table E4**
Robustness IV — Sample of firms working with at least 5 banks per year.

| | Employment | | Output | | Investment | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | 2003–2013 | 2008–2009 | 2003–2013 | 2008–2009 | 2003–2013 | 2008–2009 |
| mean annual growth (%) | 0.312 | -2.764 | 0.508 | -1.755 | 7.572 | 5.111 |
| Credit Shock coefficient ($\theta$) | 0.143 | 0.513*** | 0.124*** | 0.175*** | 0.649*** | 0.587*** |
| $\|\theta$/mean annual growth (%)$\|$ | 0.46 | 0.19 | 0.24 | 0.10 | 0.09 | 0.11 |
| *DOWN* coefficient ($\theta_D$) | 0.286*** | 0.770*** | 0.197*** | 0.709*** | 0.132 | 1.399*** |
| $\|\theta_D$/ mean annual growth (%)$\|$ | 0.92 | 0.28 | 0.39 | 0.40 | 0.02 | 0.27 |
| *UP* coefficient ($\theta_U$) | 0.059 | -0.191 | 0.097 | 0.514*** | 0.131 | 0.107 |
| $\|\theta_U$/mean annual growth (%)$\|$ | 0.19 | 0.07 | 0.19 | 0.29 | 0.02 | 0.02 |

*Notes.* This table summarizes the estimated effects when restricting the sample to those firms with at least five banks per year. We focus on the estimates for the entire period (2003–2013) and the *financial crisis* (2008–2009). *Mean annual growth (%)* refers to the average annual growth rate of the variable as measured in our sample of firms in a particular period. We denote significance at 10%, 5% and 1% with *, ** and ***, respectively. $|\theta$/mean annual growth (%)$|$ is the absolute value of the estimated coefficient divided by the mean annual growth (%).

**Table E5**
Robustness V — Excluding construction and real estate firms.

| | Employment | | Output | | Investment | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | 2003–2013 | 2008–2009 | 2003–2013 | 2008–2009 | 2003–2013 | 2008–2009 |
| mean annual growth (%) | 2.382 | 1.179 | 0.686 | -0.749 | 6.764 | 5.964 |
| Credit Shock coefficient ($\theta$) | 0.306*** | 0.589*** | 0.119*** | 0.207*** | 0.927*** | 0.811*** |
| $\|\theta$/mean annual growth (%)$\|$ | 0.13 | 0.50 | 0.17 | 0.28 | 0.14 | 0.14 |
| *DOWN* coefficient ($\theta_D$) | 0.308* | 0.659** | 0.284*** | 0.454*** | 0.459*** | 0.681*** |
| $\|\theta_D$/ mean annual growth (%)$\|$ | 0.13 | 0.56 | 0.41 | 0.61 | 0.07 | 0.11 |
| *UP* coefficient ($\theta_U$) | 0.019 | -0.309 | 0.151** | 0.319*** | 0.144 | 0.123 |
| $\|\theta_U$/mean annual growth (%)$\|$ | 0.01 | 0.26 | 0.22 | 0.43 | 0.02 | 0.02 |

*Notes.* This table summarizes the estimated effects when excluding construction and real estate firms both in the shock identification step and the network regressions. We focus on the estimates for the entire period (2003–2013) and the *financial crisis* (2008–2009). *Mean annual growth (%)* refers to the average annual growth rate of the variable as measured in the sample of firms in a particular period. We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. $|\theta$/mean annual growth (%)$|$ is the absolute value of the estimated coefficient divided by the mean annual growth (%).

**Table E6**
Real directs and indirect effects of credit shocks by firm size 2008–2009.

| | employment | | | output | | | investment | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | 0–10 | 10–500 | +500 | 0-10 | 10–500 | +500 | 0-10 | 10–500 | +500 |
| Credit Shock | 0.447*** | 0.638* | 1.063 | 0.065*** | 0.305*** | 0.268 | 0.460*** | 0.438*** | 3.106 |
| (s.e) | (0.133) | (0.319) | (0.894) | (0.013) | (0.049) | (1.247) | (0.098) | (0.148) | (2.807) |
| DOWN | 1.016*** | 0.480 | -1.028 | 0.515*** | 2.183*** | 4.407 | 1.497*** | 0.925** | 0.061 |
| (s.e) | (0.336) | (0.663) | (1.309) | (0.170) | (0.343) | (1.598) | (0.266) | (0.407) | (1.917) |
| UP | 0.312 | -0.219 | 1.455 | 0.328** | 0.246 | 1.834 | 0.242 | 0.134 | -0.212 |
| (s.e) | (0.392) | (0.609) | (0.838) | (0.153) | (0.224) | (1.218) | (0.348) | (0.402) | (1.215) |
| # obs | 289,327 | 98,522 | 1,036 | 279,098 | 97,389 | 1,015 | 280,285 | 97,939 | 1,050 |
| R2 | 0.042 | 0.051 | 0.058 | 0.116 | 0.096 | 0.10 | 0.012 | 0.015 | 0.013 |
| Sample firms | All | All | All | All | All | All | All | All | All |
| Fixed effects | sector × year | sector × year | sector × year | sector × year | sector × year | sector × year | sector × year | sector × year | sector × year |

*Notes.* This table reports the direct and indirect effects of credit supply on employment, output, and investment over the 2008–2009 period, estimated using Eq. (7), for firms of different size. Columns (1), (4), and (7) refer to firms with between 0 and 10 employees. Columns (2), (5), and (8) refer to firms with between 10 and 500 employees, and columns (3), (6), and (9) to firms with more than 500 employees. *Credit Shock* refers to the firm-specific credit supply shock estimated in Eq. (4), normalized to have zero mean and unit variance. $DOWN_{jt,s}$ measures the indirect shock received by firm $j$ operating in sector $s$ from its suppliers (downstream propagation). $UP_{jt,s}$ proxies for the indirect shock received by firm $j$ operating in sector $s$ from its customers (upstream propagation). All regressions include the following control variables: firm-specific credit demand shocks ($\hat{\lambda}_{jt}$), lagged loan-to-assets ratio, and lagged productivity. We denote significance at 10%, 5%, and 1% with *, **, and ***, respectively. Standard errors clustered at the main bank level are reported in parentheses.

than 500 employees, the coefficients associated to credit supply shocks and downstream and upstream propagation of these shocks are not statistically significant. This is the case for both employment growth, output growth, and investment when the direct shock is considered. Turning to downstream propagation (shock from suppliers), the effect is only significant in the case of output growth for large firms while it is not significant in the case of employment growth and investment. Note, however, that the sample for larger firms is substantially smaller.

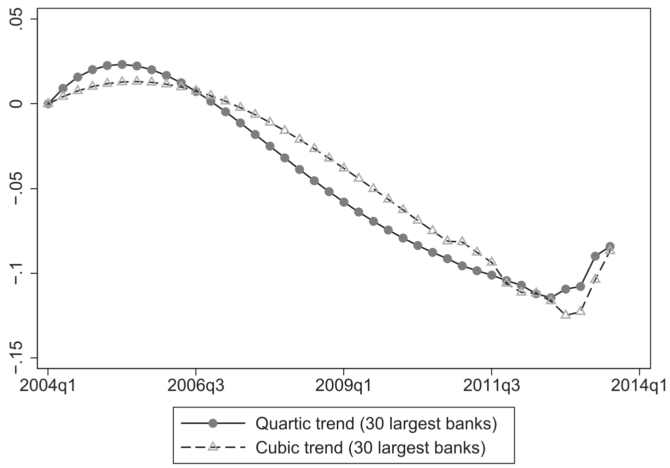


**Fig. F1.** Aggregate credit supply over time.
*Notes.* This figure plots the aggregate credit supply indicator that result from averaging the bank-specific trends given by $\hat{K}_i' \times T$. Quartic and cubic trend are plotted. The value in the first quarter is normalized to 0.

## Appendix F. A time-varying credit supply indicator

The aggregate evolution of credit shocks reported in Fig. 7 of Section 5.2.1 suggests a positive credit supply shock during the boom period (2004–2007) and a negative one afterwards. In this appendix, we present an alternative approach and estimate a time-varying indicator of credit supply that confirms this pattern. Intuitively, we use the loan-level data to estimate bank-specific time trends of credit supply after accounting for demand shocks (i.e., firm fixed effects). The resulting bank-specific time trends can then be aggregated to construct an aggregate indicator of credit supply over time.

Consider the following model:

$$\Delta \ln c_{ijt} = \mu_{jt} + \zeta_i + K_i' \times T + \xi_{ijq} \tag{22}$$

where $\Delta \ln c_{ijt}$ refers to credit growth between bank $i$ and firm $j$ in quarter $t$, $K_i' \times T$ captures a bank-specific time trend intended to identify the evolution of bank-specific credit supply. For our baseline quartic trend, we define $K_i = (\kappa_{1,i}, \kappa_{2,i}, \kappa_{3,i}, \kappa_{4,i})'$ and $T = (t, t^2, t^3, t^4)$. Bank-specific time trends in credit supply can be estimated as $\hat{K}_i' \times T$.

The identification of bank-specific credit supply time trends is based on the inclusion of firm-quarter effects ($\mu_{jt}$) that account for time-varying demand shocks as well as time invariant bank-specific effects ($\zeta_i$) that account for constant heterogeneity in supply factors at the bank level. Note that we use now quarterly data to get a better identification of the time trends that are now the focus of our analysis. Matched employer-employee techniques employed above enable to accommodate the firm-quarter ($\mu_{jt}$) and bank dummies ($\zeta_i$). However, the bank-specific time trends also represent a challenge from a computational perspective given the use of quarterly data, which multiplies by a factor of four the number of annual observations.[26] We therefore restrict the analysis to the 30 largest banks in the sample, which account for 88% of total credit.

Fig. F.1 plots the indicators of credit supply when considering cubic and quartic time trends. Interestingly, credit supply in both cases indicate an increase during 2004–2007, and a dramatic reduction starting in 2008. This pattern fully coincides with our aggregate quantification in Section 5.2.1. These exercises illustrate that the type of trend (cubic or quartic) does not alter the aggregate pattern of credit supply over time.

## Appendix G. Further details on the model

Taking prices as given, the representative household maximizes the following problem:

$$\max_{c_j, \forall j, l} \frac{c^{1-\gamma}}{1-\gamma} - \frac{l^{1+\epsilon}}{1+\epsilon}$$

$$\text{subject to:} \quad c = \prod_{i=1}^{n} c_j^{\nu_j}$$

$$\sum_j^n p_j c_j \leq wl + \sum_i^n \pi_i$$

where $wl = w\sum_{i=1}^n l_i$ measures the household's labor income and $\sum_i^n \pi_i$ the income coming from firms' profits. This problem can also be solved in two stages. In the first stage, given a total amount of consumption of the composite good, the household minimizes its associated expenditure across the different goods $j$. This stage implies an ideal price index for the composite good. Given this price index

[26] This is because each bank-specific time trend must be stored as an additional set of variables to be included in the regression. For instance, in the case of a quartic trend, quarterly loan-level data up to 2013 includes approximately 70 million bank-firm-quarter observations. The inclusion of a quartic trend for each bank in the sample implies that $180 \times 4 = 720$ variables must be included in the regression in addition to the firm-quarter and bank dummies handled by the FEiLSDVj approach. This estimation requires around 350 GB of memory which makes the problem computationally intractable.

and the wage, the household decides how much to spend on total consumption and how much to work. The solution of this problem is given by:

$$\frac{c_j p_j}{\bar{p}c} = \nu_j \tag{23}$$

$$\frac{c^{-\gamma}}{l^{\epsilon}} = \frac{\bar{p}}{w} \tag{24}$$

where $\bar{p} = \prod_{j=1}^{n} \left(\frac{p_j}{\nu_j}\right)^{\nu_j}$ is the ideal price index. Eq. (23) implies that the household's consumption expenditure share on a particular good $j$ is constant and given by the share parameter $j$. Eq. (24) implies that the marginal rate of substitution of consumption for leisure must be equal to the ratio of prices.

*Equilibrium* An equilibrium in this economy is defined as a set of prices $\{p_1, ..., p_n\}$ and allocations $\{l_1, ...; l_n\}$, $\{c_1, ..., c_n\}$ and $\{x_{i1}, ..., x_{in}\}$, $\forall i$, such that:

1. Firms solve their maximization problem, i.e., Eqs. (11), (12), and (13) are satisfied.
2. Households solve their optimization problem, i.e., Eqs. (23) and (24) are satisfied.
3. Markets clear:

$$y_i = \sum_{j=1}^{n} x_{ji} + c_i \quad \forall i \tag{25}$$

$$l = \sum_{i=1}^{n} l_i \quad \forall i \tag{26}$$

## Appendix H. Further details on the calibration

In this section of the appendix we provide further details on our calibration. We first present the particular moments we use to identify $\alpha_i$, $\nu_i$, and $\omega_{ij}$ for the year 2003.

We identify the labor share in each sector $i$ ($\alpha_i$) in the production function by exploiting the fact that in the model firms' expenditure in labor as a fraction of total expenses in inputs satisfies:

$$\alpha_i = \frac{wl_i}{wl_i + \sum_{j=1}^{n} p_j x_{i,j}} \quad \forall i \tag{27}$$

Finally, we identify the industry shares in the Cobb-Douglas consumption aggregator by matching the final consumption expenditure shares:

$$\nu_i = \frac{p_i c_i}{\sum_{j=1}^{n} p_j c_j} \quad \forall i \tag{28}$$

provided by the IO tables. The parameters governing the IO structure of the economy use the information provided by the Spanish *direct requirement matrix*. In particular, with the information provided in this matrix we can measure, in

**Table H1**
Summary of calibration.

| Param. | Definition | Value |
|---|---|---|
| (A) Parameters off the shelves | | |
| $\eta$ | Decreasing returns to scale | 0.90 |
| $\epsilon$ | Inverse of the Frisch elasticity | 0.50 |
| $\gamma$ | Wealth effect | 0.50 |
| (B) Parameters calibrated to the year 2003 | | |
| $\alpha_i$ | Share of labor in industry $i$'s total input cost | varies by sector |
| $\nu_i$ | Share of industry $i$'s in final consumption expenditure | varies by sector |
| $\omega_{ij}$ | Direct requirement coefficients | varies by sector pairs |
| $\phi_i^{2003}$ | Financial frictions | varies by sector |
| $z^{2003}$ | Aggregate productivity component | 1.00 |
| (C) Parameters calibrated to the years 2004–2013 | | |
| $\{\phi_i^t\}_{t=2004-2013}$ | Financial frictions | varies by sector-year |
| $\{z^t\}_{t=2004-2013}$ | Aggregate productivity component | vary by year |

Notes: Table H.1 presents a summary of our calibration strategy, which we explain in Section 5.2.1 of the main text.

**Table H2**
Counterfactuals ($\eta$ = 0.99).

| | Δ % Real Output | | Δ % Employment | |
|---|---|---|---|---|
| | (1) full economy | (2) horz. economy | (3) full economy | (4) horz. economy |
| 1. Benchmark | -0.39 | 0.33 | -1.62 | -0.64 |
| 2. Fixed $\phi_i$ $\forall i$ | 5.02 | 1.45 | 1.67 | 0.43 |
| 3. Change only $\phi_i$, $i =$ real state | 0.03 | 0.53 | -1.44 | -0.52 |
| 4. Change only $\phi_i$, $i =$ electricity | -0.01 | 0.36 | -1.45 | -0.61 |
| 5. Change only $\phi_i$, $i =$ construction | -0.07 | 0.34 | -1.46 | -0.62 |
| 6. Change only $\phi_i$, $i =$ wholesale | -0.10 | 0.38 | -1.43 | -0.58 |

*Notes. Table 5 shows the results of different counterfactual exercises. Columns (1) and (3) show the changes in real output and employment between 2008 and 2009 for the benchmark economy (row 1), a economy in which $\phi_i$ $\forall i$ are kept constant to their values in 2008 (row 2), and economies in which all $\phi_i$'s are kept constant to their values in 2008 except for one sector (rows 3, 4, 5, and 6). Columns (2) and (4) show the same changes but when shutting down the propagation effects in the model, i.e., imposing that $\alpha_i = 1$ $\forall i$.*

each industry $i$, the expenditure on each intermediate good $j$ as a fraction of total expenditure on intermediate goods:

$$w_{i,j} = \frac{p_j x_{ij}}{\sum_{j=1}^{n} p_j x_{ij}} \quad \forall i, j \tag{29}$$

We then explain how we apply our iterative procedure to jointly calibrate $\phi_i$ $\forall i$ and $z$ in the subsequent years.

1. We first calibrate all the parameters of the model for the year 2003 (see Section 5.2.1 in the main text). We normalize $z^{2003} = 1$.
2. For the year 2004, we proceed as follows:
   (a) Guess a value for $z^{2004}$
      (1) Given this value of $z^{2004}$, find the elements in vector $\boldsymbol{\phi}^{2004}$ that make a version of our model without input-output linkages to exactly reproduce the changes in employment across sectors predicted by our reduced form estimates of the direct effect. In particular, we do so by computing changes in employment between 2003 and 2004 in a model in which $\alpha_i = 1 \;\; \forall i$.
      (2) Given this value of $z^{2004}$ and this vector $\boldsymbol{\phi}^{2004}$, solve for the full model with input-output linkages and compute the aggregate change in real GDP between 2003 and 2004. Compare it with that from the data.
   (b) Iterate over $z^{2004}$ until the changes in real GDP between 2003 and 2004 are the same in the model and in the data.
3. For the year 2005, we start with our calibrated $z^{2004}$ and $\boldsymbol{\phi}^{2004}$ and apply the same procedure.
4. We do the same for all the subsequent years.

Finally, we provide a table that summarizes our calibration.

*Sensitivity analysis.* Table H.2 reports the counterfactual results under $\eta = 0.99$. That is, we apply the same calibration strategy as described in Section 5.2.1 but under a value of $\eta$ which puts the economy very close to a constant returns to scale. The results form this sensitivity analysis are as follows. First, we find that the propagation effect in the benchmark economy is bigger than in the baseline calibration: in the absence of input-output the economy would have grown 0.33%, which compares to -0.01% under the baseline calibration. Second, we find that keeping fixed all the financial frictions to their values calibrated for 2008 would have implied an output growth considerably bigger than in the case of the baseline calibration: 5.02% vs. 2.11%. The reason is the following: the negative propagation effects are stronger under $\eta = 0.99$ but the model still has to reproduce the same output growth (-0.39%). This means that the implied increase in the aggregate productivity component $z$ is bigger, which turns into a higher income growth in the absence of negative financial shocks. Finally, we find that shocking particular sectors in isolation would have implied lower output losses. This is driven by the fact that we are allowing $z$ to change over time and, as mentioned above, the calibrated increase is bigger than in baseline.

## Appendix I. List of industries

**Table I1**
List of industries.

| Number | Industry |
|---|---|
| 1 | Crop and animal production, hunting and related service activities |
| 2 | Forestry and logging |
| 3 | Fishing and aquaculture |
| 4 | Mining and quarrying |
| 5 | Manufacture of food products, beverages and tobacco products |
| 6 | Manufacture of textiles, wearing apparel and leather products |
| 7 | Manufacture of wood and of products of wood and cork, except furniture |
| 8 | Manufacture of paper and paper products |
| 9 | Printing and reproduction of recorded media |
| 10 | Manufacture of coke and refined petroleum products |
| 11 | Manufacture of chemicals and chemical products |
| 12 | Manufacture of basic pharmaceutical products and pharmaceutical preparations |
| 13 | Manufacture of rubber and plastic products |
| 14 | Manufacture of other non-metallic mineral products |
| 15 | Manufacture of basic metals |
| 16 | Manufacture of fabricated metal products, except machinery and equipment |
| 17 | Manufacture of computer, electronic and optical products |
| 18 | Manufacture of electrical equipment |
| 19 | Manufacture of machinery and equipment n.e.c. |
| 20 | Manufacture of motor vehicles, trailers and semi-trailers |
| 21 | Manufacture of other transport equipment |
| 22 | Manufacture of furniture; other manufacturing |
| 23 | Repair and installation of machinery and equipment |
| 24 | Electricity, gas, steam and air conditioning supply |
| 25 | Water collection, treatment and supply |
| 26 | Sewerage; waste collection, treatment and disposal activities; materials recovery; |
| 27 | Construction |
| 28 | Wholesale and retail trade and repair of motor vehicles and motorcycles |

(*continued on next page*)

**Table I1** (*continued*)

| Number | Industry |
|---|---|
| 29 | Wholesale trade, except of motor vehicles and motorcycles |
| 30 | Retail trade, except of motor vehicles and motorcycles |
| 31 | Land transport and transport via pipelines |
| 32 | Water transport |
| 33 | Air transport |
| 34 | Warehousing and support activities for transportation |
| 35 | Postal and courier activities |
| 36 | Accommodation; food and beverage service activities |
| 37 | Publishing activities |
| 38 | Motion picture, video and television programme production, sound recording and music publishing activities |
| 39 | Telecommunications |
| 40 | Computer programming, consultancy and related activities; information service activities |
| 41 | Financial service activities, except insurance and pension funding |
| 42 | Insurance, reinsurance and pension funding, except compulsory social security |
| 43 | Activities auxiliary to financial services and insurance activities |
| 44 | Real estate activities |
| 45 | Legal and accounting activities; activities of head offices; management consultancy activities |
| 46 | Architectural and engineering activities; technical testing and analysis |
| 47 | Scientific research and development |
| 48 | Advertising and market research |
| 49 | Other professional, scientific and technical activities; veterinary activities |
| 50 | Rental and leasing activities |
| 51 | Employment activities |
| 52 | Travel agency, tour operator reservation service and related activities |
| 53 | Security and investigation activities; services to buildings and landscape activities; business support activities |
| 54 | Public administration and defence; compulsory social security |
| 55 | Education |
| 56 | Human health activities |
| 57 | Social work activities |
| 58 | Creative, arts and entertainment activities; libraries, archives, museums and other cultural activities; gambling activities |
| 59 | Sports activities and amusement and recreation activities |
| 60 | Activities of membership organisations |
| 61 | Repair of computers and personal and household goods |
| 62 | Other personal service activities |

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