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# The real effects of banks' corporate credit supply: A literature review

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**Abstract**

In this article, we review the rapidly growing literature on the real effects of banks' corporate credit supply. We cover recent methodological advances and provide an in-depth survey of the existing evidence. The literature consistently shows that credit supply contractions lead to adverse real outcomes, but economic magnitudes vary across samples and identification strategies. This variation has become smaller in more recent work, using highly granular data. We further document heterogeneity in firm outcomes and show that the evidence is more ambiguous for expansionary shocks. Our analysis allows us to identify current knowledge gaps and worthwhile avenues for future research.

**KEYWORDS**

bank health, corporate credit, credit supply, real effects

**JEL CLASSIFICATION**

E22; E24; E50; G21

## 1 | INTRODUCTION

The question of how fluctuations in banks' credit supply affect real economic activity has interested economists at least since the Great Depression. It was prominently addressed in a number of early studies (e.g., Bernanke, 1983; Peek & Rosengren, 2000) and has recently been revisited extensively (e.g., Berg, 2018; Chodorow-Reich, 2013; Cingano, Manaresi, & Sette, 2016; Gan, 2007; Jiménez et al., 2017; Khwaja & Mian, 2008; Morais et al., 2019). The rapid recent growth of the literature can be attributed to the global financial crisis, but also to the improved availability of granular loan-level data. Motivated by this wealth of new data, methodological advances, and novel results, we review and synthesize the extant body of work and derive recommendations for future research.

Banks' credit supply contractions have adverse real effects if borrowers are unable to replace their borrowing with alternative funding. Conversely, credit expansions have real implications if they relax firms' borrowing constraints or fuel risk-taking.[1] Identifying the effects of credit supply on firm behavior empirically, however, is challenging for at least two

**Abbreviations:** 2SLS, two-stage least squares; DB, defined-benefit pension; EBA, European Banking Authority; ECB, European Central Bank; EPL, employment protection legislation; ILST, industry-location-size-time fixed effects; OMT, Outright Monetary Transactions; QE, quantitative easing; SME, small and medium-sized enterprises.

reasons. First, it requires the disentangling of credit supply from demand. When banks are more likely to cut the supply of credit, firms' demand is likely to be low as well; for example, because of reduced—and typically unobservable—investment opportunities or working capital needs (Bernanke & Gertler, 1995). Second, one also needs to control for the endogenous matching between firms and banks. Such matching arises, for example, when firms with a poor performance outlook prefer to borrow from banks that are less likely to monitor and discipline them (Gan, 2007).

While early research entirely relied on macroeconomic data and potentially biased time-series correlations (e.g., Bernanke & Blinder, 1992), subsequent papers also conducted cross-sectional or panel data analysis using macro- (e.g., Dell'Ariccia et al., 2008; Peek & Rosengren, 2000) or firm-level data (e.g., Almeida et al., 2011; Duchin et al., 2010). Most of these studies attempt to address the aforementioned endogeneity concerns in some way, but cannot rule them out altogether. With improved access to matched loan-level data, however, a rapidly growing body of research has been able to address the challenges more comprehensively. In this strand of the literature, Gan (2007) and Khwaja and Mian (2008) were the first to isolate credit supply by examining how banks with different exposure to adverse shocks change their lending toward the same borrower (the firm-time fixed effects approach). Their approach enables causal inference and has subsequently been applied by many other studies (e.g., Chodorow-Reich, 2013; Jiménez et al., 2017; Morais et al., 2019). Yet, despite its advantages and popularity, it comes with two important limitations. First, it does not consider general equilibrium constraints, which has led to Amiti and Weinstein (2018)'s approach of accounting for firms' relevance in banks' lending portfolios as well as for banks' relevance in firms' total borrowing. Second, it is not applicable to firms borrowing from only one bank, which has prompted Degryse et al. (2019) to consider firm-cluster fixed effects. Additionally, studies with access to loan applications can also account for sample selection bias (e.g., Berg, 2018; Jiménez et al., 2014), and thus further improve identification.

We discuss these methodological advances in detail in Section 4, but first provide an overview of the most commonly used data sources in this literature in Section 3. This is important because many of the methodological innovations cannot be implemented without access, for example, to syndicated loan data or national credit registers. Data availability therefore continues to shape the evolution of the literature.

In Section 5, we review the empirical evidence showing that banks transmit their liquidity or capital shocks to firms by adjusting their credit supply (e.g., Bernanke & Blinder, 1992; Ivashina & Scharfstein, 2010; Jiménez et al., 2014). Although banks pass on both positive and negative shocks, the effect is not necessarily symmetric (e.g., Becker & Ivashina, 2014; Kashyap et al., 1993; Leary, 2009). Nonetheless, the literature generally provides strong evidence that credit supply shocks have a significant impact on the funding of firms.

We then review the literature on whether the impact of credit supply on funding translates into real effects in Section 6. We focus first on effects for the average firm, and—consistent with the majority of the literature—on investment and employment outcomes. Regardless of the origin of the shock (e.g., crisis, monetary policy), credit supply contractions seem to generally result in reduced firm investment (e.g., Cingano, Manaresi, & Sette, 2016) and employment (e.g., Chodorow-Reich, 2013; Huber, 2018). Contrary to the effect on investment, however, the impact on employment is often economically moderate and occasionally insignificant. This is because firms tend to reduce investment before cutting employment, but also have the option to reduce their employment cost by cutting wages (e.g., Popov & Rocholl, 2018). This points to an important role for employment protection legislations and is consistent with the observation in the literature that labor force adjustments after credit supply shocks often concentrate on less educated (e.g., Hochfellner et al., 2015), younger (e.g., Berton et al., 2018), female (e.g., Berton et al., 2018), or unskilled employees (e.g., Hochfellner et al., 2015), and on employees with temporary contracts (e.g., Bentolila et al., 2017).

Additionally, negative effects of reduced credit supply are also reflected in other real outcomes, such as valuations (e.g., Gan, 2007), exports (e.g., Paravisini et al., 2014), and productivity (e.g., Duval et al., 2020). In response to positive credit supply shocks, instead, the evidence is rather ambiguous. Some studies show that an increase in credit availability leads to zombie lending, without positive implications for the real sector (e.g., Acharya et al., 2019; Giannetti & Simonov, 2013). Other papers suggest that positive credit supply shocks improve firm outcomes (e.g., Ferrando et al., 2019).

Next, we proceed to discuss heterogeneity across firms. The literature shows that adverse effects after credit supply reductions are weaker for firms that have more banking relationships (e.g., Degryse et al., 2019), that are part of a group network (e.g., Khwaja & Mian, 2008), and/or that have access to other forms of finance (e.g., Acharya et al., 2018). The evidence on heterogeneity across firm size and age tends to show stronger effects for smaller and younger firms. However, it remains inconclusive whether this comes from larger credit supply reductions to these firms or from their inability to replace bank credit with alternative funding sources (e.g., Cingano, Manaresi, & Sette, 2016; De Jonghe et al., 2020; Khwaja & Mian, 2008). Similarly, the existing evidence is also not clear cut with respect to the role of cash holdings (Berg, 2018), and identifies mixed results regarding the asymmetry (e.g., Alfaro et al., 2021; Amiti &

Weinstein, 2018) and persistence (e.g., Huber, 2018; Popov & Rocholl, 2018) of real outcomes. In addition, there is some evidence showing sizeable indirect effects of credit supply contractions through the supply chain (e.g., Costello, 2020).

We furthermore observe that employment protection, economic fragility, and sample composition can account for considerable cross-country differences. Focusing on papers analyzing the same or similar economies, it also seems to be the case that identification strategies matter for the magnitude of the identified effects; especially when the analysis relies on firm- or macro-level instead of matched bank-firm loan level data. Recent methodological innovations and improved data availability, in other words, have helped not only to eliminate biases, but also to provide a clearer overall picture.

Having taken a critical look at the literature, we then identify several worthwhile avenues for future research in Section 7. To this end, we first discuss knowledge gaps that emerge from our synthesis of the existing evidence, and point to understudied issues or issues with conflicting results. Next, we also identify the main shortcomings of current methodological conventions on the identification of credit supply shocks. Finally, Section 8 concludes.

## 2 | THEORY

In a frictionless world, decentralized markets for contingent claims allocate capital efficiently toward its most productive use. In the presence of real-world frictions, however, theory suggests that (some) capital is best allocated as credit and by financial intermediaries. The insufficient or excessive supply of credit by banks can thus lead to an inefficient allocation of capital, which consequently affects firm decisions.

Brunnermeier and Krishnamurthy (2020), for instance, identify three principal corporate finance theories. In the "entrepreneurial owner-manager model" and the "agency model," debt contracts help to overcome information frictions between lenders and borrowers (e.g., Hart & Moore, 1994; Holmström & Tirole, 1998) or mitigate agency conflicts between owners and managers (e.g., Hart & Moore, 1995; Jensen, 1986; Zwiebel, 1996). In the "model of shareholder-debtholder conflicts," instead, similar frictions between investors create costs of excess debt; for example, through asset substitution (Jensen & Meckling, 1976) or underinvestment (Myers, 1977).

At the macroeconomic level, Brunnermeier and Krishnamurthy (2020) identify the "financial accelerator model" of Bernanke et al. (1999) as the main framework to understand the role of financing frictions. The model draws on Kiyotaki and Moore (1997), Bernanke and Gertler (1989), and Townsend (1979)'s "entrepreneurial owner-manager model" but does not actually feature any intermediaries. Instead, information frictions between corporate borrowers and household lenders imply that firms' ability to raise external financing becomes a function of corporate net worth in the following way: A negative shock to net worth reduces firms' demand for capital, which lowers the price of capital and feeds back into the net worth of capital-owning firms. Even without banks explicitly in the model, it is easy to see how this feedback can amplify exogenous credit supply shocks.[2]

The information frictions in these models generate nonconvexities in transaction costs and thus the potential for economies of scale and scope in lending. This potential provides an important rationale for the existence of financial intermediaries (e.g., Freixas & Rochet, 2008). Banks, as "information-sharing coalitions" (e.g., Diamond, 1984; Ramakrishnan & Thakor, 1984) and "delegated monitors" (e.g., Calomiris & Kahn, 1991; Diamond, 1984; Holmstrom & Tirole, 1997), are particularly well-equipped to supply the types of contracts that firms—plagued by information asymmetries—require. To the extent that they also have the incentives and resources to actually provide these contracts, banks' credit supply therefore supports the efficient allocation of capital in the economy.

At the same time, when banks are unable or unwilling to lend, firms can be affected in two ways. If banks straightforwardly ration otherwise borrowing-constrained firms, these firms will be forced to forego viable investment opportunities, experience financial distress, or seek to cut expenses—including the wage bill. Amplifying the aggregate effect, they might also extend less trade credit to downstream firms. Even without rationing, however, changes in credit supply can cause firms to adjust their investment and hiring through the effect on equilibrium interest rates. Adrian et al. (2013), for example, provide a model of direct and intermediated credit in which real effects primarily depend on the cost and composition of corporate credit, not the overall quantity.

Whether they are indeed rationed or bank credit simply becomes more expensive, firms are generally more affected if they cannot rely on reputation, collateral, technology, or ratings to overcome information frictions. The role of banks in overcoming information frictions further implies that reduced credit supply has a stronger effect on firms if bank-firm relationships are more important. For instance, when current lenders have private information about borrowers (Dell'Ariccia & Marquez, 2004; Sharpe, 1990). In such circumstances, firms are less able to mitigate the effects of changing credit supply from a single bank by resorting to direct funding via bond or equity markets or by borrowing from a

different bank. This distorts the allocation of capital and can ultimately impair its efficient use by firms (for a comprehensive review of the corresponding literature, see James & Smith, 2000).

While the existing theory typically ties investment outcomes very closely to credit availability, the effects of changing credit supply on employment often depend on the interaction between credit and labor market frictions (see, e.g., the review of Boeri et al., 2013) or institutional factors. When there is a timing mismatch between payments to employees and the realization of cash flows, for instance, firms are forced to finance salaries through their working capital (Greenwald & Stiglitz, 1988). Impaired access to external financing then leads firms to cut employment. At the same time, the existence of employment protection legislation, along with the availability of temporary and permanent contracts, implies that financially constrained firms might prefer more temporary workers, who contribute to a larger volatility of total employment (Caggese & Cuñat, 2008).

As for the reasons why banks may be unwilling or unable to lend, Calomiris et al. (2017) explain that early theories attribute credit rationing to exogenous interest rate rigidities (permanent or transitory), imperfect competition (Scott, 1957), or to bankruptcy costs that limit borrowers' abilities to repay (Hodgman, 1960; Miller, 1962). The predominant theoretical frameworks today instead—again—emphasize information frictions between borrowers and lenders. Both, adverse selection and moral hazard can cause credit markets to freeze entirely (Akerlof, 1970) or for particularly risky borrowers (Jaffee & Russell, 1976; Stiglitz & Weiss, 1981).[3] Equilibrium prices under adverse selection may further imply that safe borrowers voluntarily borrow less (Jaffee & Russell, 1976; Stiglitz & Weiss, 1981). At the same time, banks themselves may be subject to agency conflicts and informational asymmetries vis-à-vis their own lenders (e.g., Gertler & Kiyotaki, 2010). They may thus need to be incentivized to perform their role, for example, as delegated monitor, and weakening these incentives can cause them to cut credit to some borrower segments or to direct capital toward riskier borrowers.

This brief discussion of the theoretical literature puts our review of the empirical work into context and informs our suggestions for future research. It predicts, for example, an important role for informational asymmetries in the transmission of credit supply shocks and a heterogeneous range of responses—for both investment and employment—by firms operating under different legal regimes, in different industries, or in environments with differently developed markets for alternative sources of funding. Against this background, we now proceed to review the empirical literature. We begin with surveying the most commonly used data sources.

## 3 | DATA SOURCES

The literature examining the real effects of bank credit supply shocks can be broadly divided into two groups: Studies that use matched bank-firm data (more recent studies) and studies that do not (mostly earlier studies). The majority of the latter group relies on firm balance sheet data and typically uses exogenous one-off events for identification. When examining the 2007–2008 financial crisis, for example, these papers often use firm-level data from Compustat for the United States (e.g., Almeida et al., 2011; Duchin et al., 2010), from Orbis for the EU (e.g., Duval et al., 2020), or from proprietary surveys (e.g., Campello et al., 2010). These sources provide information on relevant real outcomes, such as investment in tangible assets and firm-level employment. Additionally, some papers in this group exploit exogenous events by aggregating bank data (e.g., to the state level as in Peek & Rosengren, 2000) and studying aggregate real outcomes. The main challenge these studies face is to disentangle credit supply from credit demand.[4]

More recent studies often combine bank and firm data using information on bank-firm relationships, which generally provides them with an edge in the identification of credit supply. Some papers in this group, obtain information on bank-firm relationships from Kompass (e.g., Ferrando et al., 2019; Ongena et al., 2015), Dafne (e.g., Popov & Rocholl, 2018), or surveys (e.g., Balduzzi et al., 2018) and do not have information on committed loans. A growing body of empirical research, however, has information on bank-firm relationships at the loan level. Access to this loan-level data improves identification further and has enabled researchers to examine the real effects of credit supply fluctuations over the entire business cycle.

Numerous papers with such loan-level information use data on syndicated loans, often provided by DealScan (e.g., Acharya et al., 2018, 2019; Chodorow-Reich, 2013; Gropp et al., 2018). Syndicated loans are large loans that are typically funded by a consortium of banks and managed by a lead arranger. More granularly, researchers frequently employ matched bank-firm loan data from credit registers. Most of these credit registers are maintained by central banks and principally require a credit institution to report each single loan above a certain amount.[5] Depending on the jurisdiction, credit registers can cover even very small loans and therefore provide a better representation of the entire

**TABLE 1** Data sources for loan level data

| Country | Loan level data source | Selected papers |
|---|---|---|
| Belgium | Credit Register | Degryse et al. (2019) |
| EU | Dealscan (Syndicated Loans) | Acharya et al. (2019) |
| France | Credit Register | Fraisse et al. (2020) |
| Germany | A Major German Bank | Berg (2018) |
| Italy | Credit Register | Cingano, Manaresi, and Sette (2016) |
| | Dealogic (Syndicated Loans) | Doerr et al. (2018) |
| Japan | Nikkei NEEDS | Amiti and Weinstein (2018) |
| | Development Bank of Japan | Gan (2007) |
| Mexico | Credit Register | Morais et al. (2019) |
| Pakistan | Credit Register | Khwaja and Mian (2008) |
| Peru | Peruvian Bank Regulator (SBS) | Schnabl (2012) |
| Portugal | Credit Register | Iyer et al. (2014) |
| Spain | Credit Register | Jiménez et al. (2017) |
| Uganda | Credit Register | Abuka et al. (2019) |
| United States | Dealscan (Syndicated Loans) | Chodorow-Reich (2013) |

*Notes:* This table shows the most important sources for loan level data along with a number of selected papers that use them to generate matched bank-firm loan level information. See Appendix Tables A1–A6 for the main data sources of all the discussed studies in this article.

universe of loans than DealScan.[6] Moreover, some credit registers allow researchers to observe loan applications, and thus to address selection bias. Because of the granularity of the information, confidentiality concerns have been an important obstacle to accessing loan-level data in the past. Recently, however, different central banks have started to provide access to credit registers to internal and external researchers.

Among the most frequently used credit registers are the Spanish (e.g., Jiménez et al., 2017), the Italian (e.g., Cingano, Manaresi, & Sette, 2016), the Belgian (e.g., De Jonghe et al., 2020), and the Portugese (e.g., Iyer et al., 2014). In Table 1, we provide an overview of the most important sources for loan level data along with a number of selected papers that use them to generate matched bank-firm loan level information (see Appendix Tables A1–A6 for the main data sources of all reviewed papers examining the real effects of credit supply shocks).

Next, we review identification strategies employed in the literature and elaborate on how more granularity of the data enables better identification.

## 4 | IDENTIFICATION STRATEGIES

To identify the real effects of credit supply, it is typically first determined how shocks to banks affect lending in equilibrium. After that, it is analyzed how firms respond to these lending shocks in terms of real outcomes, such as investment and employment. Below, we discuss these steps and the corresponding econometric strategies in more detail.

### 4.1 | Identifying credit supply

The empirical literature has employed a wide range of identification strategies attempting to isolate the effect of bank shocks on lending in equilibrium. The reason for this multiplicity of approaches stems from endogeneity concerns that are not easy to address. To explain the main challenges to identification, assume that one runs the following loan-growth regression[7]:

$$\Delta Lending_{bf} = \alpha + \beta \Delta Credit\ Supply_b + \Phi X_b + \Pi X_f + \Theta X_{bf} + error\ term \quad (1)$$

In the equation above, the independent variable, $\Delta CreditSupply_b$, is the change in credit supply by bank $b$ typically caused by a shock to the bank. The dependent variable, $\Delta Lending_{bf}$, denotes the change in committed credit from bank $b$ to firm $f$ in the post-shock period relative to the pre-shock period, and is usually expressed as a logarithmic change to capture growth rates. $X_b$, $X_f$, and $X_{bf}$ are bank, firm, and bank-firm controls, respectively, ideally measured during the pre-shock period to avoid the bad control problem. The coefficient of interest is $\beta$. Financing frictions may lead banks to pass on shocks to their borrowers, inducing $\beta \neq 0$.

However, if $\Delta Credit\ Supply_b$ would be correlated with unobserved firm characteristics, $\beta$ could be biased for two main reasons. First, the events that affect bank credit supply might also influence credit demand. During a financial crisis, for example, as banks cut lending, firms may also demand less credit due to reduced working capital needs or worsening investment prospects (Bernanke & Gertler, 1995). Therefore, the demand for credit should be controlled for in an ideal empirical setting to isolate credit supply. The second threat to the identification of $\beta$ is the nonrandom assignment of borrowing firms to banks. Such endogenous selection could arise between the firm's performance and the lender's financial health. For instance, poorly performing firms might prefer to borrow from weaker banks that are less capable or willing to monitor and discipline (Gan, 2007). If this type of selection generates a positive correlation between credit supply and demand, the conventional OLS estimation would lead to an overestimation of $\beta$.[8]

Next, we present how the empirical literature tackles these identification challenges. First, we briefly present the methodologies used in early empirical research. This group of studies generally relies on (macro- or micro-level) data on banks, but does not have information on individual loans. Then, we discuss methodological advances in the more recent literature. These more recent studies mainly employ loan-level data which matches banks to borrowing firms, and often contains bank and firm balance sheet information.[9]

### 4.1.1 | Identification through bank-level data

In early work, Bernanke and Blinder (1992) study time-series correlations between changes in the federal funds rate and changes in aggregate bank lending. Given that such correlations might be biased by economy-wide demand shocks, Kashyap et al. (1993) conduct the same analysis but consider bank credit relative to commercial paper. As this is another important source of external funding that should be equally affected by demand, they can assess whether changes in bank loans in response to fluctuations in the federal funds rate are demand or supply driven. However, as argued by Kashyap and Stein (1995), commercial paper is substituted for bank loans mostly by large firms, implying that such macro-level analysis ultimately neglects idiosyncratic credit demand. To further investigate this issue, Kashyap and Stein (1995) also use disaggregated bank-level data. Specifically, they examine how banks in different size classes respond to interest rate changes in terms of loan growth and security holdings. Yet, their analysis does not account for potentially endogenous bank-firm matching. For example, for the fact that large banks might mainly lend to large firms, which might experience a more moderate drop in loan demand during downturns than small firms.

Later, Kashyap and Stein (2000) use micro-level panel data on banks and examine the effect of monetary policy on the volume of credit supplied by banks with heterogeneous bank balance sheet liquidity. By exploiting cross-sectional as well as time-series variation in bank characteristics, the authors seek to determine the direction of the bias in their analysis. They then proceed to address this bias by conducting various regressions, but lack the necessary information on bank-firm relationships to eliminate it fully.

Alternatively, a group of studies exploits events that produce credit supply shocks that are more plausibly exogenous to demand conditions. Peek and Rosengren (1997) investigate the lending by branches of Japanese banks in the United States following the burst of the Japanese stock market bubble, which is arguably exogenous to credit demand in the United States. They use risk-based capital ratios of Japanese parent banks as a proxy for credit shocks of their US branches. Later, Peek and Rosengren (2000) implement the same setup to quantify the effect of the Japanese banking crisis on credit supply to the US construction industry. Additionally, Ashcraft (2005) analyzes the change in aggregate bank lending following bank failures that were not related to local economic conditions.[10]

Finally, we observe the use of instrumental variables to tackle endogeneity concerns. As an example, Paravisini (2008) exploits an Argentinian government program that allocates funds across banks. He argues that part of the allocation formula is independent of credit demand and uses the corresponding allocations to instrument bank financing in a two-stage least squares (2SLS) regression.

All in all, whereas the earliest studies could not (or only partially) address the aforementioned identification concerns, others have focused on exogenous—and very specific—events or instruments. Next, we present the more recent empirical literature which mainly employs matched bank-firm loan data.

### 4.1.2 | Identification through matched bank-firm loan level data

*Firm (-time) fixed effects to control for credit demand*

One of the most important contributions in this stream of research comes from Gan (2007) and Khwaja and Mian (2008), both of whom identify credit supply shocks using firm fixed effects, loan-level data, and a natural experiment. Gan (2007) explores the consequences of the Japanese land market crash during the early 1990s (which created an exogenous shock to credit supply) on lending to Japanese manufacturing firms. Japanese banks had high levels of exposure to the land market crash through both lending to the real estate market as well as through their own land ownership. Khwaja and Mian (2008), instead, examine the effects of unforeseen nuclear tests in Pakistan in 1998. These tests induced a bank liquidity shortage, which reduced the available credit to firms.[11] Gan (2007) and Khwaja and Mian (2008) mainly estimate the regression in Equation (2).[12]

$$\Delta Lending_{bf} = \alpha_f + \beta \Delta Credit\ Supply_b + \Phi X_b + \Theta X_{bf} + error\ term \tag{2}$$

The dependent variable, $\Delta Lending_{bf}$, is the change in lending from bank $b$ to firm $f$. The independent variable, $\Delta Credit\ Supply_b$, here denotes the shock faced by bank $b$; that is, exposure to the real estate industry for Gan (2007), and the change in deposit growth for Khwaja and Mian (2008). Finally, $\alpha_f$ are firm fixed effects.[13]

The use of firm fixed effects in a first differenced data set at the loan level implies a within-firm comparison, analyzing how multiple banks with varying exposure to a given shock adjust their lending toward the *same* firm. This approach overcomes the challenges to identification. First, the within-firm comparison rules out concerns about the endogenous selection of bank-firm relationships. Second, firm fixed effects control for both observable and unobservable firm-specific factors, including importantly firms' credit demand.

While this approach significantly reduces the main identification concerns, it also has limitations. First, it is only applicable to samples where firms borrow from multiple banks, which might represent only a minority of firms in a country (Degryse et al., 2019). Second, the within-firm specification fails to capture credit flows from new banking relationships, and hence might not reflect aggregate lending outcomes (Amiti & Weinstein, 2018). Third, the methodology assumes that a firm has the same credit demand across its lenders. Gan (2007) gives the following example which violates this assumption. Suppose a firm has two lenders, namely Bank A and Bank B, each providing the firm with different types of loans. Bank A provides real-estate loans, whereas Bank B gives loans for the firm's capital expenditures. During a land market crisis, the firm may demand fewer real estate loans from Bank A due to decreasing real estate related business, but still the same amount of capital expenditure loans from Bank B. In this case, the firm's loan demand would be bank-specific and correlated with Bank A's shock. However, as has been argued in the literature, this issue might be a minor concern when bank affectedness is unlikely to be correlated with the provision of a special type of loan (which is likely for large banks as mentioned by Gan, 2007); furthermore, it can be mitigated by interacting firm fixed effects with loan types (as done by Khwaja & Mian, 2008). Next, we discuss how the literature has addressed some of these limitations.

*The estimator of Amiti and Weinstein (2018)*

To begin with, Amiti and Weinstein (2018) show that empirical models based on Khwaja and Mian (2008) can be nested in the panel data model expressed in Equation (3).

$$\Delta Lending_{bft} = \alpha_{ft} + \beta_{bt} + error\ term \tag{3}$$

where $\alpha_{ft}$ are firm-time fixed effects that control for time-varying firm credit demand, and $\beta_{bt}$ are bank-time fixed effects that capture time-varying credit supply shocks.[14]

The aim of Amiti and Weinstein (2018) is to measure the effect of idiosyncratic bank credit supply shocks on aggregate investment. They start from the observation that the model in Equation (3) does not take into account general equilibrium constraints. That is, banks can only make an additional loan if there are firms that want to borrow, and firms can only obtain a new loan if there are banks willing to lend. To capture the implied equilibrium linkages, they then include the weight of each firm in the bank's lending portfolio ($\phi_{fb,t-1} \equiv \frac{L_{fbt-1}}{\sum_f L_{fb,t-1}}$) as well as the weight of each bank in the firm's borrowing portfolio ($\theta_{fb,t-1} \equiv \frac{L_{fbt-1}}{\sum_b L_{fb,t-1}}$). This approach results in the following system of equations.

$$D^B_{bt} = \beta_{bt} + \sum_f \phi_{fb,t-1}\alpha_{ft} + \sum_f \phi_{fb,t-1}\varepsilon_{fbt} \tag{4}$$

$$D^F_{ft} = \alpha_{ft} + \sum_b \theta_{fb,t-1}\beta_{bt} + \sum_b \theta_{fb,t-1}\varepsilon_{fbt} \tag{5}$$

where $D^B_{bt}$ denotes the growth rate of total lending by bank $b$ to all its borrowers, and $D^F_{ft}$ equals the growth rate of total borrowing of firm $f$ from all its lenders. Equation (4) represents the lender side and shows that a bank's total credit growth in period $t$ relies on its credit supply shock in that period ($\beta_{bt}$) and the sum of the credit demand shocks from all its borrowers, weighted by the share they have in the bank's lending portfolio in $t-1$ ($\sum_f \phi_{fb,t-1}\alpha_{ft}$). In a similar way, Equation (5) stands for the borrower side and demonstrates that the growth rate of the firm's total borrowing in period $t$ is explained by its credit demand shock ($\alpha_{ft}$) and the sum of the lenders' credit supply shocks, weighted by the share they have in the firm's total borrowing in $t-1$ ($\sum_b \theta_{fb,t-1}\beta_{bt}$). Because $\phi_{fb,t-1}$ and $\theta_{fb,t-1}$ are pre-determined, one can further impose the following moment conditions: $\phi_{fb,t-1}E[\varepsilon_{fbt}] = 0$ and $\theta_{fb,t-1}E[\varepsilon_{fbt}] = 0$. This yields the following system of equations to estimate firms' credit demand and banks' credit supply shocks:

$$D^B_{bt} = \beta_{bt} + \sum_f \phi_{fb,t-1}\alpha_{ft} \tag{6}$$

$$D^F_{ft} = \alpha_{bt} + \sum_b \theta_{fb,t-1}\beta_{bt} \tag{7}$$

In practical terms, Tielens and Van Hove (2017) show that Amiti and Weinstein (2018)'s estimator is equivalent to the estimation of the regression model in Equation (3) through a weighted least square procedure, with lagged weights capturing the importance of each loan in aggregate credit.

The methodology of Amiti and Weinstein (2018) thus improves the firm-time fixed effects approach initially applied by Gan (2007) and Khwaja and Mian (2008) by accounting for general equilibrium constraints that Equation (3)-type models do not take into account. Their weighting approach is useful particularly in samples where firms substantially vary in their loan exposures; for instance, when the average firm differs significantly from the median firm. Moreover, the framework saturated with firm-time and bank-time fixed effects does not require a singular exogenous shock to bank health to identify credit supply shocks. This makes it possible to estimate credit supply shocks, as well as their real effects, over the full business cycle.

That said, since Amiti and Weinstein (2018)'s estimator includes firm-time fixed effects in the bank-firm level credit growth regression, their methodology also relies on firms that have credit relationships with multiple banks (as in Gan, 2007 and Khwaja & Mian, 2008). This is an important drawback in samples with many single-bank firms. In addition, the within-firm-time approach also relies on the assumption that a firm has the same credit demand across its lenders in each period, which might not be the case in some samples.

*Firm-cluster fixed effects to estimate credit supply shocks also for single-bank firms*

How does empirical research identify credit supply shocks for single-bank firms to which the firm-time fixed effects approach is not applicable? This is of significance as single-bank firms might constitute a large percentage of firms in many economies. For instance, using a very granular dataset which combines various European credit registers, Altavilla et al. (2020) show that single-bank firms account for 54–90% of firms, depending on the country. Furthermore, the share that these single-bank firms have in the total credit volume ranges from 13% to 59%, suggesting that the

exclusion of single-bank firms from the analysis might leave a significant amount of the lending portfolio unaccounted for. Considering that single-bank firms also have different characteristics than multibank firms, the reliance on multibank firms alone might therefore lead to inaccurate conclusions on the effects of credit shocks (Degryse et al., 2019).

Degryse et al. (2019) seek to tackle this issue using data from the Belgian credit register. In Belgium, only 16% of the firm-time observations have lending relationships with multiple banks, accounting for around 50% of the entire loan volume. Their sample reveals important differences between single- and multibank firms. For instance, single-bank firms are on average younger and smaller, and borrow smaller amounts. If credit shocks affect single-bank firms more than multibank firms, the exclusion of single-bank firms from the analysis would underestimate credit shocks and their real effects.

$$\Delta Lending_{bft} = \alpha_{ILST} + \beta_{bt} + error\ term \tag{8}$$

To incorporate single-bank and multibank firms into the analysis, Degryse et al. (2019) propose to use narrow industry-location-size-time fixed effects (ILST), instead of firm-time fixed effects, to control for credit demand.[15] They estimate the regression model in Equation (8) and are able to include more than 97% of the borrowing firms in the analysis.[16] For comparison, they also estimate the regression model in Equation (3), but then can only include multibank firms (i.e., 16% of the borrowing firms). After providing evidence that the ILST fixed effects are indeed capable of controlling for credit demand, Degryse et al. (2019) show that the monthly correlation between estimated credit supply shocks from Equation (8) and those from Equation (3) varies significantly over time (from 0.23 to 0.94). This suggests that the exclusion of single-bank firms from the analysis might result in biased estimates of credit supply shocks and thus ultimately the resulting real effects. Indeed, the authors also find that the estimated real outcomes obtained using ILTS fixed effects are more sizeable than those obtained using firm-time fixed effects.

Overall, in comparison with Gan (2007), Khwaja and Mian (2008), and Amiti and Weinstein (2018), the approach of Degryse et al. (2019) has the advantage that it can include single-bank firms in the identification of credit supply (shocks). Like Amiti and Weinstein (2018)'s estimator, it also has the advantage that it can identify credit supply over the full business cycle.[17,18] Finally, while the approach of Degryse et al. (2019) relies to a lesser extent on the assumption that a firm's credit demand is equal across its lenders (as single-bank firms have only one lender), it requires a similar and perhaps even stricter assumption, namely: firms of similar size in the same industry and location are equal in their credit demand at a given time. Stated differently, firms in the same narrowly defined cluster have the same credit demand.

Which methodology is preferable is sample-specific as it depends on how important single-bank firms are, how different they are from multibank firms, and whether or not banks behave differently toward them (irrespective of their financial characteristics). Therefore, one should carefully examine the sample characteristics to decide whether to use ILST or firm-time fixed effects. Indeed, Degryse et al. (2019) point out that the use of firm-time fixed effects might still be preferable in samples where the majority of firms have multiple lending relationships or where single-bank firms do not significantly differ from multibank firms.

### *Identification with loan applications*

The aforementioned firm-time and firm-cluster fixed effects models rely only on granted loans. However, firms can also apply for a loan and have their application rejected. Leaving rejected applications out of the analysis might lead to sample selection bias.[19] Researchers are often not able to address this type of sample selection bias because unsuccessful loan applications are generally not observed. One exception is Jiménez et al. (2014), who investigate the effect of monetary policy on the composition of credit supply over the business cycle by employing loan data from the Spanish credit register, where both accepted and rejected loan applications are observed.[20] To address the selection bias, they implement a two-stage model in the spirit of Heckman (1979). In the first stage, they estimate the probability that the loan application is approved (based on the interaction of the overnight interest rate, firms' ex ante credit risk, and bank capital); in the second stage, they analyze the committed credit amount conditional on the loan application being granted. Furthermore, the authors estimate the regressions in each stage with firm-time fixed effects to control for both observable and unobservable time-varying firm characteristics.[21]

The results of Jiménez et al. (2014) reveal that the selection bias could be sizeable. When they estimate their main equation without accounting for sample selection, the magnitude of their estimates is considerably smaller. This suggests that an econometric analysis should ideally also account for loan applications to obtain unbiased results. Although datasets that contain information on loan applications are currently rare, in the future this type of information might become more available to researchers, for instance through online lending platforms.

## 4.2 | Identifying the real effects of credit supply

Most studies that use loan-level data, follow the analysis at the bank-firm level (i.e., the estimation of the loan-growth equation) with an attempt to gauge the impact of credit supply shocks on firm outcomes by estimating the following regression[22]:

$$\Delta Y_f = \alpha + \rho \Delta Credit\ Supply_f + \Pi X_f + error\ term \tag{9}$$

where $\Delta Y_f$ denotes the change in outcome variable $Y$ (e.g., investment, employment, or exports) of firm $f$, and $\Delta Credit\ Supply_f$ is the change in firm $f$'s exposure to the credit supply shock aggregated across each of its lenders' exposure to the event causing the credit supply shock.

Importantly, different papers construct the firm-level credit supply shock differently, depending on how they identify credit supply at the bank-firm level. Khwaja and Mian (2008), for instance, calculate for each firm the average deposit growth of the firm's pre-shock lenders, weighted by the share of each bank in the firm's total borrowing. Similarly, Amiti and Weinstein (2018) and Degryse et al. (2019) create firm-level time-varying credit supply shocks by aggregating the fitted values of bank-time fixed effects (i.e., the credit supply shocks) for each firm, weighted by each lender's share in the firm's total borrowing. Differently, Gan (2007) uses the real estate exposure of the firm's top lender to proxy firm-level credit shocks.

Notice, that this implies that this type of estimation is conducted at the firm-level and not at the loan-level. The specification above can thus not include firm fixed effects, as these would subsume $\Delta Credit\ Supply_f$. The inability to use firm fixed effects, however, leads this specification to suffer from endogeneity, because credit demand being driven by firm characteristics potentially confounds the results.

Given that the estimation of the regression in Equation (9) might be biased, the empirical literature typically corroborates the identification strategy by reporting the results from the loan growth regressions in Equation (1) (the conventional OLS regression) *and* from Equation (3)-type models (the fixed effects regressions) (e.g., Bentolila et al., 2017; Chodorow-Reich, 2013; Cingano, Manaresi, & Sette, 2016; Gan, 2007; Jiménez et al., 2017; Khwaja & Mian, 2008). If the OLS and fixed effects regressions yield similar results in terms of magnitude and significance, the papers argue that unobservable firm-specific factors are not important determinants of credit demand. Therefore, the estimation of the regression in Equation (9) is less likely to suffer from any correlation between credit demand and credit supply.

For samples with multibank firms, the outcome of the loan-level analysis can help to correct the potential endogeneity in the firm-level analysis (9). This can be done in two ways. The first is to correct for the potential bias in the cross sectional model in Equation (9) by including the estimates of firm(-time) fixed effects ($\hat{\alpha}_f$) from Equation (2) (e.g., Cingano, Manaresi, & Sette, 2016). As the firm fixed effects, $\alpha_f$, capture firm specific factors in Equation (2), the inclusion of $\hat{\alpha}_f$ in the cross sectional model (9) ensures that unobservable firm characteristics which might affect credit demand do not bias the estimates. The second approach by Jiménez et al. (2019) is based on obtaining the otherwise unobservable covariance between demand and supply shocks from the estimation of the loan-growth regression in Equation (2), and using it to numerically correct the bias on the estimated effect at the firm-level, $\hat{\rho}$, from Equation (9). This approach, however, can only be applied for firm-level loan outcomes.

In addition, a set of papers in this strand of the literature (e.g., Bentolila et al., 2017; Berton et al., 2018; Chodorow-Reich, 2013; Cingano, Manaresi, & Sette, 2016) uses IV to gauge the sensitivity of firm outcomes to the availability of credit. They estimate the following 2SLS regression at the firm level:

$$\begin{aligned} \Delta Y_f &= \theta + \eta \Delta Lending_f + \Pi X_f + error\ term \\ \Delta Lending_f &= \alpha + \beta \Delta Credit\ Supply_f + \Phi X_f + error\ term \end{aligned} \tag{10}$$

In the model above, the change in bank credit supply acts as an instrument for committed credit. While $\beta$ measures the impact of a credit supply shock on a firm's loan growth, $\eta$ captures the effect of committed credit on firm outcomes. Thus, $\beta \times \eta$ equals $\rho$ in Equation (9).

One might question the exogeneity of the instrument as the exclusion restriction (that events driving banks' credit supply alter firm outcomes only through credit) might not be satisfied. If banks that are more affected by negative shocks

could charge higher interest rates, for example, the exclusion restriction would not be valid and the results would be overestimated (Bentolila et al., 2017).[23] For this reason, results from the IV model should be interpreted cautiously.

## 4.3 | Alternative approaches

### 4.3.1 | Identification by aggregating micro-level data to macro-level data

Greenstone et al. (2020) investigate how reduced credit following the 2008 crisis affected US employment. They do not use information on individual loans but attain identification similar to the aforementioned fixed effect approaches. They do so by focusing on bank-county level lending rather than bank-firm level lending. More specifically, they analyze how banks change their lending toward a particular US county (i.e., within-county comparison), rather than analyzing how banks change their lending toward the same borrower (i.e., within-firm comparison). They regress change in bank-county level lending on bank and county fixed effects for each year. In the spirit of Amiti and Weinstein (2018), their sample is weighted by each bank's pre-crisis lending in a given county. In this approach, credit demand is controlled for by county fixed effects. They then construct county-level estimated credit supply shocks, by taking the sum of the estimated credit supply shocks for each county, weighted by each bank's pre-crisis county market share (i.e., the extent of counties' exposure to banks).[24] After that, they quantify the effect of their estimated county-level credit supply shocks on county-level employment.

By using micro data, Greenstone et al. (2020) construct credit shocks and employment at the aggregate (i.e., county) level. On the one hand, this enables them to gauge the real effects of credit shocks without using matched bank-firm data.[25] On the other hand, this approach makes it difficult to account for local general equilibrium effects. For example, penetration of counties by banks might be the result of endogenous managerial decisions and a shock that influences these decisions might also affect county-level employment. For further discussion of this topic along with the economic meaning of estimates from such aggregate level regressions, we refer to Nakamura and Steinsson (2018) and Guren et al. (2020).

### 4.3.2 | Regression discontinuity design

Berg (2018) obtains data on loan applications, granted credit amounts, and firms' credit ratings from a major German bank. Different from the previously discussed papers, he implements a regression discontinuity design, where he exploits a cutoff rule for firms' credit ratings that determines the bank's lending decision. The regression discontinuity design compares firms in a small bandwidth around this cutoff. It is implemented as a two-stage regression, in which firms' credit ratings, which are regarded as exogenous, are used as instruments for loan acceptance (or loan change) to explain the resulting firm outcomes in the second stage. The rationale of the model is that borrowers just above and just below the cutoff are similar in their credit quality, but one group of firms obtains credit whereas the other group does not. The comparison of such similar firms mitigates the endogeneity concerns from (un)observable firm-specific factors. Given also that all sample firms applied for a loan, this setup further controls for credit demand and eliminates sample selection bias. On the one hand, the regression discontinuity design therefore overcomes most of the identification problems and provides high internal validity; on the other hand, the exclusion of the observations far from the cutoff might limit external validity.

### 4.3.3 | Others

Loan-level studies are generally better able to address the challenges in the identification of credit supply. Yet, the identification strategies from the literature that do not use loan-level data can nonetheless serve as a reference for future work using firm-level data. For example, future papers could employ the "long-term debt maturity" approach of Almeida et al. (2011) which relies on the fact that firms with maturing debt will likely experience difficulties when trying to re-finance during a financial crisis. The rationale of using long-term debt maturity as a proxy for exposure to credit supply shocks is that firms' debt structures are formed over multiple years so that long-term debt renegotiations do not hinge on firm performance right after a credit crunch. Alternatively, firm-level research could also rely on Rajan

and Zingales (1998)'s industry-level measure of external finance dependence (i.e., the proportion of investment financed through external funds) to alleviate the concern that idiosyncratic credit demand is correlated with credit supply.

### 4.4 | Is there a best approach?

We have demonstrated that the empirical methodologies using matched bank-firm loan level data are able to tackle the most important challenges to identification. As a result, loan-level data has become the gold standard in this line of research. Which empirical strategy should best be applied to loan-level data, however, depends on the sample characteristics. First of all, firm(-time) fixed effects should be favored to control for credit demand when the sample primarily contains multibank firms that have uniform loan demand across their lenders and when there is a specific event under study that impacts banks' lending capacity. In the absence of such an event, however, the researcher should implement an Equation (3)-type model. If, in addition, there is also significant heterogeneity with respect to firms' exposure, the estimation strategy of Amiti and Weinstein (2018) works best.

On the other hand, if the sample largely consists of single-bank firms which also exhibit different characteristics than multibank firms, one should use firm-cluster fixed effects to control for credit demand (and implement an Equation (8)-type model). That is, firm-cluster fixed effects can be used to estimate the loan-growth equation either with a measure of bank health or over the business cycle. However, using firm-cluster fixed effects might require a stronger identifying assumption.

Finally, if one has access to information on loan applications, it becomes possible to also address sample selection bias (e.g., Jiménez et al., 2014). Alternatively, with access to more specific information, for example, on a bank's lending cutoff, a regression discontinuity design becomes feasible (e.g., Berg, 2018).

Having explained how the literature identifies the financial and real effects of credit supply shocks, we summarize the empirical evidence on the financial effects of credit supply shocks in the next section, and for real effects in the section after that.

## 5 | EVIDENCE ON THE FINANCIAL EFFECTS OF CREDIT SUPPLY SHOCKS

Credit supply shocks could, in principle, result from unprompted changes in banks' lending standards (because management or strategies change, because the bank adopts a new risk-management tool, etc.). In reality, however, they are mostly prompted by systemic events or policy changes that are exogenous to any individual bank; they are also primarily negative. Real effects then occur if borrowers cannot easily resort to alternative funding.[26] To set the scene for the discussion of these real effects in Section 6, we therefore briefly review evidence showing (i) that banks do indeed pass on exogenous shocks to borrowers, and (ii) that borrowers try to overcome these shocks (but are not always successful).

Using aggregate data, the seminal paper of Bernanke and Blinder (1992) finds that US banks make fewer new loans and do not renew old ones in response to a strong monetary-policy-induced drop in deposit funding. More recent papers improve the identification of credit supply by exploiting heterogeneity in banks' ability to absorb exogenous shocks. Kishan and Opiela (2000), for instance, show that small and undercapitalized banks in the US struggle to raise alternative funding after contractionary monetary policy and thus, to maintain pre-contraction lending. Similarly, lending is reduced most by small banks with the least liquid balance sheets in Kashyap and Stein (2000) and by weakly capitalized banks or banks with lower liquidity ratios in Jiménez et al. (2012). In response to monetary easing, on the other hand, banks tend to increase risk-taking (Borio & Zhu, 2012) and expand lending toward all types of borrowers (Ferrando et al., 2019; Luck & Zimmermann, 2020).

Ivashina and Scharfstein (2010), instead, exploit cross-sectional variation in US banks' exposure to the interbank funding dry-up and simultaneous credit line take-up during the financial crisis of 2008.[27] While they find all types of new lending to drop during the crisis, they show that the effect is strongest among banks that saw their liquidity position deteriorate the most. Similarly, Iyer et al. (2014) find that more interbank market-dependent banks in Portugal cut lending more following a shock to the European interbank market.

Alternatively, changes in banks' credit supply may also stem from shocks to banks' capital. Peek and Rosengren (2000), for instance, show that Japanese banks cut lending to the real estate market in the United States, following the drop in Japanese real estate prices in the 1990s. Gropp et al. (2018), instead, focus on large European banks' response to higher capital requirements and show that these banks reduce lending to corporate borrowers in order to

reduce risk-weighted assets. While they specifically investigate the European Banking Authority (EBA)'s 2011 capital exercise, similar effects on credit supply have also been identified following the implementation of Basel II (Fraisse et al., 2020). On the other hand, Jiménez et al. (2017) show that a positive shock to banks' capital, generated by dynamic provisioning, can lead to more credit supply towards all firms during bad times.

The general observation that banks pass on adverse shocks to their borrowers by reducing credit supply (and that there is cross-bank variation in this), has been widely confirmed. Huang (2003), for example, provide evidence for the United Kingdom, Gambacorta (2005) for Italy, Gunji and Yuan (2010) for China, Cetorelli and Goldberg (2011) for emerging markets, Puri et al. (2011) for Germany, Fungáčová et al. (2014) for the Euro area. Matousek et al. (2019) and Morais et al. (2019), instead, observe that banks also pass on positive shocks to their borrowers by increasing credit supply in Japan and Mexico, respectively.

Additionally, there is also a large set of papers studying within-bank variation, i.e., which borrowers are most likely to be affected when a bank cuts its lending. Several of these papers have identified borrower risk as an important criterion when banks (are forced to) shift their credit supply. DeYoung et al. (2015), for instance, study the 2008 crisis and reveal that banks cut lending less toward safer US firms, following a negative funding shock. Using micro-level data from a single multinational bank, Liberti and Sturgess (2018) also show that large and less risky firms as well as firms with strong lending relationships and high revenues experience less credit rationing following a negative exogenous shock. Similarly, Ongena et al. (2018) show that German banks with exposure to the US real estate market cut lending to all firms during the financial crisis, but less to firms in industry-region clusters with higher solvency ratios. De Jonghe et al. (2020) use matched bank-firm loan level data to investigate the lending behavior of Belgian banks following a negative funding shock and confirm that firms with less debt, lower default risk, and more collateral face a smaller-than-average contraction in bank credit. In response to an expansionary monetary policy shock, Jiménez et al. (2014) find that weakly capitalized banks lend more to ex ante risky Spanish firms and that they impose fewer collateral requirements.

In addition, the literature has also identified a "flight home" effect. Giannetti and Laeven (2012) study the syndicated loan market and find that banks reduce lending more towards foreign borrowers during the crisis. Consistently, Cetorelli and Goldberg (2012) examine US-based foreign bank branches and show that lending is cut in the United States when the parent experiences an adverse shock.

For reductions in banks' credit supply to have real effects at the firm level, firms should not be able to fully compensate for the reduction in bank lending with nonbank sources of funding.[28] Several papers have contributed to this strand of literature and show that firms, indeed, take actions and try to overcome reductions in banks' credit supply. We discuss these papers next.

Kashyap et al. (1993) and Becker and Ivashina (2014) respectively show that commercial paper and bond issuances increase after monetary-policy-induced contractions in credit supply. In addition, there is ample evidence that firms experiencing reductions in bank credit supply rely more on (more costly) supplier credit (Blasio, 2005; Fukuda et al., 2006; Garcia-Appendini & Montoriol-Garriga, 2013; Love et al., 2007; Nilsen, 2002).[29] Alternatively, firms have also been shown to rely on equity issuances (Leary, 2009; Levine et al., 2016) and—less frequently—on alternative funding, such as informal loans (Casey & O'Toole, 2014).

By and large, the literature suggests that the different efforts to substitute for reduced credit supply from banks, can ultimately not fully offset the shock. This is true, in particular for small firms that often have no access to public debt markets (Iwaki, 2019; Leary, 2009) and for low-rated and highly leveraged firms for which public debt issuance is prohibitively costly (Becker & Ivashina, 2014). Stronger bank-firm relationships, instead, can sometimes help to offset contractions in bank lending (Iyer et al., 2014), but also seem to limit the utilization of alternative sources of funding (Leary, 2009).

In summary, the evidence strongly suggests that negative shocks to banks' lending capacity are not (fully) absorbed at the bank level and indeed affect the supply of credit to firms. Both bank and firm characteristics matter for the extent to which bank shocks are transmitted to firms and most firms find it difficult to fully compensate for reduced credit supply. Similarly, positive shocks to the lending capacity indeed increase the supply of credit to corporate borrowers. As a result, the existing evidence implies the potential for real effects at the firm level and we now proceed to review the corresponding literature in detail.

## 6 | EVIDENCE ON THE REAL EFFECTS OF CREDIT SUPPLY SHOCKS

After documenting how banks transmit shocks to firms, we now present the evidence on how shocks translate into real effects for borrowers (see Appendix Tables A1–A6 for a structured overview). We first discuss average effects (in terms

of investment, employment, exports, etc.) and then illustrate the heterogeneity, asymmetry, persistence, and propagation of real outcomes, as well as existing cross-country evidence.

## 6.1 | Average effects

Because the empirical literature has focused primarily on investment and employment consequences of credit supply shocks, we first separately present the findings for these two dependent variables. Afterwards, we provide a summary of other real effects.

### 6.1.1 | Investment

*Crisis-induced credit supply shocks*

If firms cannot offset reductions in banks' credit supply, they may have to reduce investment. This is indeed corroborated by the existing literature, including by a large number of papers that examine the real effects of crisis-induced credit supply shocks. The majority of these papers analyze the global financial crisis of 2007–2008 and document negative, bank-lending induced effects on investment. Duchin et al. (2010), Almeida et al. (2011), and Campello et al. (2010), for example, employ firm-level data and provide evidence mainly for US firms. Duchin et al. (2010) rely on firms' cash reserves as a proxy for the exposure to credit supply shocks and find that a decrease in cash reserves from the 75th to the 25th percentile is associated with an average decline in investment of 69%. A shortcoming of their proxy is that firms might build cash reserves in good times to use in crisis times, which would lead to reverse causality and spuriously amplify the positive relation between cash and investment. Almeida et al. (2011), instead, proxy firms' exposure to credit supply shocks with their long-term debt maturity. They show that US firms with large fractions of their long-term debt maturing during the crisis reduce their quarterly investment by approximately 27% more than firms with long-term debt maturing after the crisis. The large differences in the magnitude of the effects found by Duchin et al. (2010) and Almeida et al. (2011) could thus be a result of Duchin et al. (2010)'s proxy leading to overestimation. At the same time, and although the approach of Almeida et al. (2011) has since been used by various other studies (e.g., Costello, 2020; De Ridder, 2019; Duval et al., 2020), it could be argued that high levels of maturing long-term debt might also capture poor managerial performance (De Ridder, 2019). Finally, Campello et al. (2010) rely on CFO survey data to identify firms' financial constraints and illustrate that the average constrained US firm planned to cut investment by around 8.5% during the 2008 crisis.[30] Although we believe that the survey measure of credit constraints is better suited to disentangle the effect of credit supply from firms' credit demand, it should be pointed out that Campello et al. (2010) analyze expected investment, whereas Duchin et al. (2010) and Almeida et al. (2011) analyze realized investment. The differences in magnitude of the identified effects are therefore not readily comparable.

Additionally, a set of papers examines the real effects of credit tightening in the wake of the 2008-financial crisis using loan-level data. Among them are the studies of Cingano, Manaresi, and Sette (2016) and De Jonghe et al. (2020), which both exploit the crisis-induced liquidity shrinkage in the interbank market and which both find comparable results. They respectively show that a one standard deviation decrease in credit supply reduces investment on average by 4.7% in Italy and by 6.7% in Belgium. Different from De Jonghe et al. (2020), Cingano, Manaresi, and Sette (2016) control for credit demand by also including the estimates of firm fixed effects from their loan-growth regression in their firm-level regression. This, however, does not seem to significantly alter the results. Finally, using confidential data from a major German bank, Berg (2018) shows that micro and small German firms just below a lending cutoff reduce investment by 2.4% relative to similar firms just above the cutoff.

Beyond the crisis of 2008, a group of studies have also investigated investment outcomes during the subsequent European sovereign debt crisis. Using data on syndicated loans, Acharya et al. (2018) show that banks exposed to risky sovereign debt, which lost value and became riskier, cut lending. This lending cut induced an investment decline for affected European firms. Doerr et al. (2018) also study data on syndicated loans but focus only on the case of Italy. They show that Italian banks that were highly exposed to foreign borrowers in distressed countries reduced lending to Italian firms, which in turn reduced investment. Lastly, Bottero et al. (2020) investigate Italian firms and the sovereign debt crisis as well but identify credit supply using loan data from the Italian credit register. They find that banks exposed to

sovereign securities curtailed lending, but no effect on investment for the average firm. They do, however, show that small firms significantly cut investment.[31]

*International transmission*

A number of papers focus particularly on the real effects of internationally transmitted credit shocks. These shocks are arguably exogenous to local demand and therefore help researchers to disentangle credit supply from credit demand. The previously mentioned paper by Peek and Rosengren (2000), for example, uses state-level data and exploits the burst of the Japanese land market bubble as an exogenous shock to commercial real estate lending in the United States. The authors find that the reduction in Japanese bank lending translated into lower growth of new construction projects. Chava and Purnanandam (2011), instead, exploit the Russian crisis of 1998 as an exogenous shock to the US banking system, and find that bank-dependent firms (i.e., firms without a credit rating) suffer a significant investment decline following the reduction in credit supply.

*Credit supply shocks stemming from shocks to bank capital*

A set of papers investigates negative bank capital shocks stemming from an increase in capital requirements and their resulting effects. The already mentioned paper by Gropp et al. (2018) uses syndicated loan data and the capital enhancement exercise (from 5% to 9% core tier 1 capital ratio) of the EBA. They show that affected banks responded primarily by reducing their corporate lending, and that this translated into lower investment growth for their borrowers compared to borrowers of non-treated banks. Analyzing the effects of Basel II capital requirements, Fraisse et al. (2020) corroborate not only the effect on bank lending, using data from the French credit register, but also the effect on investment by affected firms.

Giannetti and Simonov (2013), instead, analyze positive shocks to bank capital and assess the consequences of bank bailouts during the Japanese banking crisis of the 1990s. Using loan-level data for listed Japanese companies, they show that banks that remained undercapitalized following a government intervention mainly lent to zombie borrowers; presumably to help these borrowers make payments on their outstanding loans. As a result, the banks themselves did not need to write off outstanding loans and were thus able to preserve their capital. The bank bailouts, therefore, failed to boost investment at large.

*Credit supply shocks stemming from monetary policy*

Abuka et al. (2019) investigate the real effects of a monetary contraction in Uganda between 2010 and 2014. They group firms into district bins and categorize districts based on bank capitalization, to then compare the number of applications for commercial building permits in low and high capital districts. By this metric, a one standard deviation increase in the policy rate reduces investment in the following quarter by about 12% more in low capital districts.

More recently, the literature also exploits positive shocks to bank credit supply resulting from unconventional monetary policy. Using syndicated loan data from the United States, Chakraborty et al. (2020) document that quantitative easing (QE) through the purchase of mortgage-backed securities on the one hand incentivized US banks to increase mortgage lending. On the other hand, however, it reduced commercial and industrial loans, which in turn led exposed banks' borrowers to invest less. Studying syndicated loan data as well, Acharya et al. (2019) examine the investment outcome following the announcement of the European Central Bank (ECB)'s Outright Monetary Transactions (OMT) program.[32] Reminiscent of Giannetti and Simonov (2013), they also conclude that the program mainly induced zombie lending by weakly capitalized banks and thus failed to promote overall investment. Their conclusion differs from Ferrando et al. (2019), who also analyze firm investment following the announcement of the OMT. Ferrando et al. (2019) use data on small and medium-sized enterprises (SMEs), obtained mainly from the ECB's "Survey on the Access to Finance of Enterprises." They have information on bank-firm credit relationships but not at the loan level, and find that SMEs borrowing from banks with higher exposure to sovereign bonds invest more. Unlike the results of Acharya et al. (2019), who study large firms, the result on SMEs is consistent with the firm balance sheet channel in (Bernanke & Gertler, 1995). The evidence on the positive real effects of credit expansions is further corroborated by Morais et al. (2019), who investigate the spillover of QE and low monetary policy rates in Europe and the United States to Mexican firms. Using matched loan-level data for 2001–2015, they find that Mexican firms with a higher share of credit from US and European banks increased their net investment more in response to expansionary monetary policy abroad than firms borrowing mainly from Mexican banks.[33]

Indicating a potential avenue for reconciling some of this mixed evidence, Ottonello and Winberry (2020) focus on firm heterogeneity in the response to monetary policy shocks. They document that investment by firms with low default

risk reacts more to expansionary monetary policy than that of high-risk firms, and attribute this to borrowing costs decreasing more for low-risk firms.

*Business cycle*

Finally, a group of loan-level studies estimate time-varying credit supply shocks, and are thus able to investigate the relationship between credit supply and investment over the entire business cycle. Amiti and Weinstein (2018), for instance, analyze a sample of listed Japanese firms between 1990 and 2010. They find that the average firm's investment decreases by around 2.1% when credit supply is reduced by one standard deviation. In addition, idiosyncratic credit supply shocks seem to explain around 30–40% of the variation in aggregate investment in Japan. This is consistent with evidence from Portugal: Amador and Nagengast (2016) apply the same methodology as Amiti and Weinstein (2018) and show that credit supply accounts for about 20–40% of the variation in investment over the period from 2005 to 2013.

Alfaro et al. (2021) and Degryse et al. (2019) examine the link between credit supply and investment in Spain and Belgium, respectively, but—unlike Amiti and Weinstein (2018)—do not account for general equilibrium constraints. Alfaro et al. (2021) show that a one standard deviation drop in credit supply leads to approximately 10% less investment by the average Spanish firm over the period 2003–2013. Degryse et al. (2019), instead, document that a one standard deviation decrease in credit supply leads to an average reduction in investment of around 3.6% between 2002–2012. That the effects in Alfaro et al. (2021) and Degryse et al. (2019) are larger than in Amiti and Weinstein (2018) (2.1%) is not surprising, as they both employ credit registry data. This allows them to cover a larger fraction of the universe of loans and especially smaller loans. The sample of Amiti and Weinstein (2018), on the other hand, only includes listed firms which are larger and tend to be less affected by credit shocks. Yet, there are also differences between the Spanish and the Belgium data. The sample of Degryse et al. (2019) consists of both single-bank and multibank firms, for instance, while Alfaro et al. (2021) only consider multibank firms.

### 6.1.2 | Employment

A large set of papers has further sought to quantify the impact of credit supply fluctuations on employment, and more specifically on the number of workers (e.g., Berton et al., 2018; Chodorow-Reich, 2013; Huber, 2018; Popov & Rocholl, 2018), the composition of the labor force (e.g., Barbosa et al., 2020; Berton et al., 2018), and wages (e.g., Hochfellner et al., 2015; Popov & Rocholl, 2018).

*The effect on total employment*

*Crisis-induced credit supply shocks.* A number of papers in the investment literature also examine the employment effects of crisis-induced credit supply shocks, particularly following the 2007–2008 financial crisis. The survey data analysis of Campello et al. (2010), for example, also reveals that constrained firms planned to reduce employment during the crisis by 8.4% on average.

In addition, Greenstone et al. (2020) quantify the effect of the crisis specifically for small US firms. They find that a one standard deviation decrease in county-level credit supply during 2009 corresponds to an average decline in small business employment growth of 5%. Chodorow-Reich (2013) was among the first to use matched loan-level data to study the employment effect of the 2008 credit crunch. Using data from the syndicated loan market in the United States, he constructs firm-level credit shocks from the weighted average crisis-induced change in lending to all other borrowers by firms' last pre-crisis syndicates.[34] He finds that a one standard deviation drop in pre-crisis syndicate health, following the collapse of Lehman Brothers, corresponds to an 18% reduction in average employment growth. That the magnitude of the effect is considerably larger than in Greenstone et al. (2020), could be because firms that tend to be affected more by credit shocks might also have demanded more credit during the crisis, which would not be controlled for by Greenstone et al. (2020). This explanation would be consistent with Schwert (2018), who provides evidence that bank-dependent firms in the US syndicated loan market mainly borrow from well-capitalized lenders. Firms with access to public debt markets, instead, borrow from poorly-capitalized banks, which ultimately leads to a negative correlation between credit supply and demand.

While these papers focus on US firms, other authors have focused on Europe. Among them, Popov and Rocholl (2018) quantify the crisis effect in Germany by identifying credit-constrained firms based on their credit relationships with savings banks whose Landesbanken were particularly exposed to mortgage-backed securities.[35] They find that firms affiliated with at least one affected savings bank reduced employment by 1.5% more than firms that were

not linked to an exposed Landesbank. Huber (2018) uses matched bank-firm data as well, but exploits German firms' dependence on Commerzbank, which cut lending during the financial crisis because of its high US exposure. He finds that firms that are fully dependent on Commerzbank cut employment by 5.3% more than firms that have no relationship with Commerzbank.[36] This effect is considerably larger than the one found by Popov and Rocholl (2018) during the same period. The latter sample, however, is much larger and thus more representative of the economy. In addition, Berg (2018) analyzes a major German bank's lending cutoff utilizing the bank's data on loan applications. He finds that firms just below the cutoff reduce employment by 1.5% relative to similar firms just above the cutoff. Even though identification in Berg (2018)'s approach is arguably better suited to control for demand and selection bias, his result is largely in line with Popov and Rocholl (2018).

The negative effect of the 2008 crisis on employment is further documented for firms in Spain and Italy. Using data from the Spanish credit register, Bentolila et al. (2017) show that firms attached to unhealthy banks (those that were bailed out by the Spanish government) cut employment significantly more than firms attached to healthy lenders. Berton et al. (2018) employ loan-level data from the Italian region Veneto and apply the methodology of Greenstone et al. (2020), although with credit supply shocks constructed at the firm—rather than the county-level. They find a sizeable negative impact on employment, that is in line with Cingano, Manaresi, and Sette (2016), who study the interbank market collapse and a sample covering all of Italy.

Studying the European sovereign debt crisis and data on syndicated loans, Acharya et al. (2018) show that banks exposed to risky sovereign debt cut lending, which in turn reduced employment among affected European firms. Doerr et al. (2018) also use data on syndicated loans but focus exclusively on Italy. Although they show that lenders that are highly exposed to distressed foreign borrowers reduced lending to Italian borrowers, they do not find a significant impact on these firms' employment. They attribute this lack of an effect to the rigidity of the Italian labor market. In line with this and using loan data from the Italian credit register, Bottero et al. (2020) also do not find a significant impact for the average Italian firm following a lending cut from banks exposed to sovereign securities. That said, Bottero et al. (2020) are in fact able to document a significant employment reduction among small Italian firms.[37]

*International transmission.* In the previously mentioned paper by Peek and Rosengren (2000), the deterioration of credit supply from Japanese banks not only affects investment, but also results in lower employment growth in the US real estate sector.

*Credit supply shocks stemming from shocks to bank capital.* Regarding negative shocks to bank capital, the paper by Fraisse et al. (2020) finds that the Basel II-induced tightening of credit supply reduced employment among affected French firms. The authors, specifically, show that a one standard deviation increase in the capital requirements of a firm's lender implied a drop of 1.4% in average employment. Considering that investment dropped by 4.6% in the same paper, it seems that the effect of reduced credit supply is stronger for investment than for employment. In addition, Barbosa et al. (2020) investigate an increase in the value of defined-benefit pension (DB) plans for Portuguese banks in 2005 following the adoption of new accounting standards (IAS 19). The adoption obliged banks to make cash contributions to their DB plans, so that affected banks experienced a negative shock to their capital. As a result, these banks benefited less from capital inflows during good times and reduced lending, relative to less affected banks. Borrowers of affected lenders, in turn, reduced employment.

Regarding positive capital shocks, the zombie lending in Giannetti and Simonov (2013) did not seem to translate into a significant impact on employment growth. Similarly, Jiménez et al. (2019) use data from the Spanish credit register and analyze the real estate boom from 2004 to 2007. They show that banks initially exposed to real estate assets increased lending to non-real estate firms, but that this increase in credit supply was offset by the firms' other banking relationships (i.e., through general equilibrium adjustments) and did not ultimately have an effect on employment.

*Credit supply shocks stemming from monetary policy.* Acharya et al. (2019) find no impact on employment growth from positive credit supply shocks induced by loose monetary policy, which they also attribute to zombie lending. Morais et al. (2019), on the other hand, do find a positive and sizeable effect. In their analysis, a one standard deviation loosening of foreign monetary policy, raised average employment by 0.3% for firms borrowing mainly from foreign lenders. The magnitude of this effect is—again—smaller than the effect that the authors find for investment (0.5%).

*Business cycle.* In papers analyzing the effects of credit shocks over the business cycle, the evidence on employment implications is somewhat ambiguous. Greenstone et al. (2020) examine the link between credit supply and small firm

employment in the United States between 1997 and 2010, and do not find a significant effect on employment. On the other hand, Alfaro et al. (2021) find that a one standard deviation increase in credit supply leads to a more than 90% increase in employment growth for the average Spanish firm over the period 2003–2013.

In summary, the literature overall illustrates that credit supply contractions result in reduced investment and employment for the average firm. Yet, the economic magnitude of the effect on employment appears to be more moderate (and sometimes even insignificant), in part because firms tend to reduce investment first. In addition, the impact of the financial crisis on employment seems more sizeable for US firms than for European firms, likely reflecting differences in employment protection. At the same time, the results following an increase in credit supply (as well as over the business cycle for employment) are mixed. That is, more credit supply sometimes boosts investment and employment (in line with the firm balance sheet channel) and sometimes not (due to zombie lending). More specifically, it seems that firm risk plays an important role in the transmission, at least of expansionary monetary policy shocks. We also observe that identification strategies appear to play an important role. Specifically, the differences in the magnitude of the effects are larger among papers analyzing the same or a similar economy without loan-level data. For example, the size of the investment impact for the United States during the 2007–2008 financial crisis appears to vary considerably in studies relying on firm-level data, whereas this variation is smaller in papers using (European) loan-level data.

### *The effect on labor force composition and wages*

In addition to the headlining effect on total employment, there is also a growing body of research studying heterogeneity across employees and the effect on wages. Evidence for Spain (Bentolila et al., 2017) and Italy (Berton et al., 2018), for example, indicates that employees with temporary contracts are more likely to be laid off following credit contractions than employees with permanent contracts. According to Berton et al. (2018), this asymmetric response results mainly from the nonrenewal of temporary contracts and can be attributed to differences in firing costs.

It also seems to be the case that less educated (Berton et al., 2018; Hochfellner et al., 2015), and/or younger (Berton et al., 2018) workers are more likely to be laid off when credit is scarce, and that constrained firms are more likely to dismiss foreign workers (Berton et al., 2018). Interestingly, Berton et al. (2018) find that the negative effect on less educated and/or younger employees is mainly driven by the fact that they often have temporary contracts.

Additionally, existing research shows that female workers are more likely to be laid off than male workers. According to Berton et al. (2018), female workers account for 60% of the total drop in firm-level employment following reduced bank lending in Italy's Veneto region over the period from 2008 to 2012. Interestingly, there is also evidence that female hiring is more responsive to positive credit supply shocks: Popov and Zaharia (2019) show that better access to credit during the interstate branch deregulation in the United States contributed to a supply-driven increase in female labor. This contributed to a reduction of the gender gap by 7.5–19% between 1970 and 2000.

Furthermore, the literature has also explicitly considered the role that skills play for employment decisions. Hochfellner et al. (2015) show that unskilled workers are more likely to lose their jobs during a crisis. Barbosa et al. (2020), instead, study a negative shock to credit supply during noncrisis times and find that skilled (and/or more educated) workers are more likely to join and less likely to leave unconstrained firms.

Finally, firms might also seek to reduce labor costs by cutting their wage bill in response to reduced credit supply. Popov and Rocholl (2018), for example, suggest that small German firms, facing high firing costs, primarily reacted to the 2008 crisis by cutting wages and not employment. Hochfellner et al. (2015), instead, demonstrate that crisis-induced reductions in wage income in Germany can be attributed to shorter employment spells and temporary exits, but that retained employees did not experience sizeable wage cuts. Similarly, evidence from European survey data (Fabiani et al., 2015) and the Spanish credit register (Bentolila et al., 2017) also shows that labor costs are primarily reduced via the number of employees and not through wages. While these results may reflect downward wage rigidity imposed by labor market regulations, they are also consistent with financially constrained firms using more temporary contracts.

Overall, it appears that credit-constrained firms, concentrate their labor force adjustment among less educated, unskilled, younger, foreign and/or female employees, as well as among those with temporary contracts. Instead, it remains less clear whether firms respond to a decrease in credit supply by (also) cutting wages and which role employment legislation plays in this context. The entire literature, however, is still relatively small and very recent, and further analysis is required to identify the relevant trade-offs and institutional factors. This analysis depends crucially on the availability of employee-level data.

### 6.1.3 | Other real outcomes

Empirical research also examines the effect of credit supply on real outcomes other than investment and employment. Firm export, for instance, has been of particular interest in the literature, which generally finds that firms export less following a drop in credit supply. Zia (2008), for example, documents that the removal of subsidized credit for the export of cotton yarn reduced exports of Pakistani yarn firms relative to non-yarn firms in 2001. Amiti and Weinstein (2011) study Japan and show that the worsening health of trade credit providers causes a decrease in the exports of downstream manufacturing firms between 1990 and 2010. Similarly, Paravisini et al. (2014) compare firms in the same product-destination clusters and link the credit contraction during the 2008 crisis to reduced exports by Peruvian firms. In a later study, Paravisini et al. (2017) show that the effect of credit supply on Peruvian exports is significantly higher for destinations in which banks specialize.[38] At the same time, Doerr and Schaz (2021) find that investment and employment of firms borrowing from internationally diversified lenders are affected less in times of distress, as such lenders are better able to raise funding than less diversified lenders. Lastly, Abuka et al. (2019) apply the methodology of Paravisini et al. (2014) and Paravisini et al. (2017) to Ugandan firms and compare exports in the same product-destination category. Following contractionary monetary policy, they document a significant difference in export volumes between districts with high- and low-capital banks.

Additionally, existing evidence also indicates that adverse shocks to credit supply lead to a decline in firm assets (Berg, 2018; De Jonghe et al., 2020; De Marco, 2019; Fraisse et al., 2020; Gropp et al., 2018; Ongena et al., 2015), sales (Acharya et al., 2018; Gropp et al., 2018), profitability (Chava & Purnanandam, 2011; Ongena et al., 2015), value added (Cingano, Manaresi, & Sette, 2016; Dell'Ariccia et al., 2008), stock market valuation (Chava & Purnanandam, 2011; Gan, 2007), as well as an increase in firm default rates (Khwaja & Mian, 2008; Schnabl, 2012). Credit reductions might also reduce firm productivity by distorting firms' optimal capital-to-labor ratio (Doerr et al., 2018; Duval et al., 2020; Huber, 2018). During credit contractions intangible investments might be cut as well, contributing further to reduced productivity (De Ridder, 2019; Duval et al., 2020).

Finally, the positive relationship between credit supply and firm assets, profitability (Degryse et al., 2019), value added (Alfaro et al., 2021), and productivity (Manaresi & Pierri, 2019) has also been corroborated by studies estimating credit supply shocks over the entire business cycle. At the same time, the literature is less clear on the response to positive credit supply shocks, with some papers indicating that an increase in credit supply might not result in positive firm outcomes. Giannetti and Simonov (2013), for example, illustrate that bank bailouts following the Japanese banking crisis did not yield a significant increase in firm valuation if lenders were believed to remain undercapitalized. Similarly, Acharya et al. (2019) find no significant effect on the return on assets after the announcement of the OMT program and Jiménez et al. (2019) show that Spanish firms during 2001–2009 were unable to benefit from relationships with banks that were highly exposed to the real estate boom (in terms of larger total sales or lower default rates). On the other hand, there is some evidence showing that capital injections might actually help the real economy. Ferrando et al. (2019), for instance, provide evidence that SMEs became more profitable following the OMT program and Morais et al. (2019) show that monetary policy-induced credit supply and bank risk-taking allowed Mexican firms to grow but also led to more defaults.

In summary, the empirical literature confirms that credit supply has a broader impact on firms than only on investment and employment. In general, these effects are more clearly identified for negative credit supply shocks.

## 6.2 | Heterogeneous effects across firm characteristics

As discussed in Section 5, credit supply shocks may have heterogeneous effects on firm outcomes through two different channels. First, banks may selectively reduce lending to some particular types of firms and not to others. Second, heterogeneity may arise from the (in)ability of firms to compensate for reduced lending through switching to other lenders or through switching to other forms of financing.

### 6.2.1 | Firm size

Empirical research frequently considers heterogeneous effects across firm size. Regarding the question of whether banks disproportionally cut lending across size categories, the results are mixed. While a set of studies finds that banks

cut lending to small firms more than to large firms (Berg, 2018; Khwaja & Mian, 2008), there is also some evidence that banks do not selectively cut lending to smaller firms (Bottero et al., 2020; Cingano, Manaresi, & Sette, 2016; De Jonghe et al., 2020; Schnabl, 2012).

The literature also documents mixed results on whether firms in different size categories vary in their ability to substitute for bank credit and accordingly experience differential real outcomes. Small firms are typically expected to be less able to replace reduced bank lending (Gertler & Gilchrist, 1994), and ample evidence is in line with this. A large group of papers shows that small firms, relative to their larger counterparts, have more difficulties to overcome reduced credit supply and thus experience a stronger decline in assets (Ongena et al., 2015), investment (Amador & Nagengast, 2016; Balduzzi et al., 2018; Bottero et al., 2020; Cingano, Manaresi, & Sette, 2016; De Jonghe et al., 2020; De Marco, 2019), employment (Balduzzi et al., 2018; Berton et al., 2018; Bottero et al., 2020; Chodorow-Reich, 2013; Costello, 2020; Duygan-Bump et al., 2015; Siemer, 2019), productivity (Manaresi and Pierri, 2019), profitability (Ongena et al., 2015), exports (Zia, 2008), and default rates (Khwaja & Mian, 2008). In addition, some studies find no significant real effects of credit supply shocks for large firms, suggesting that the observed effects are entirely driven by smaller firms (Bottero et al., 2020; Chodorow-Reich, 2013; Khwaja & Mian, 2008). Finally, greater effects on smaller firms are also prevalent during credit expansions. Morais et al. (2019), for example, show that expansionary foreign monetary policy induced lending to smaller Mexican firms, which in turn grew and invested more. At the same time, these firms were also more likely to default due to increased bank risk-taking.

Contrary to evidence of larger effects for smaller firms, Campello et al. (2010) do not find any differential impact on the investments of small and large US firms from their survey data analysis. Interestingly, Popov and Rocholl (2018) also observe that their negative effect on employment is less noticeable among smaller German firms. They attribute this in part to differences in employment protection legislation, but also to closer connections between employees and employers in smaller firms.

### 6.2.2 | Firm age

Firm age is another frequently examined source of heterogeneity, with the evidence generally indicating that banks do not disproportionately cut lending to young firms (Cingano, Manaresi, & Sette, 2016; De Jonghe et al., 2020; Schnabl, 2012). When it comes to real effects, however, the evidence is less clear cut. Ample evidence indicates, for example, that reduced credit supply leads to a larger reduction in investment (Balduzzi et al., 2018; Cingano, Manaresi, & Sette, 2016; De Marco, 2019) and employment (Balduzzi et al., 2018; Berton et al., 2018; Siemer, 2019) among younger firms. In fact, Siemer (2019) shows that the greater negative employment impact on small firms is largely driven by young firms. This is because young firms do not yet have established lending relationships and are thus more likely to be affected by adverse credit conditions. Bai et al. (2018) complement this evidence by showing that young firms drive the effect on small firms also during positive shocks to credit supply. They study the state banking deregulation in the United States, between the 1970s and the early 1990s, and show that it enabled banks to better detect firms with greater productivity. This in turn facilitated employment growth particularly among small manufacturing firms. Like Siemer (2019), Bai et al. (2018) then demonstrate that the effect is driven by younger firms which had fewer well-established banking relationships and were thus more likely to borrow from new banks.

Conversely, De Jonghe et al. (2020) show that the investment of young firms in Belgium was not disproportionally affected after the Lehman Brothers-induced interbank funding crunch. Similarly, Duval et al. (2020) and Manaresi and Pierri (2019) also do not find any age-dependent effect of credit supply on productivity growth.

### 6.2.3 | The number of banking relationships

The number of banking relationships may also play an important role for firms, as firms with more relationships should find it easier to switch banks during a credit crunch. In line with this, Degryse et al. (2019) find that the real effects (total assets, investment, operating margin) of credit shocks are often smaller for multibank firms than for single-bank firms in Belgium. Additionally, Zia (2008) shows that reduced credit supply has more detrimental effects on exports of Pakistani single-bank firms. Manaresi and Pierri (2019) also provide evidence that the productivity of Italian firms with fewer lenders is affected more by credit shocks. Finally, Ongena et al. (2015) investigate the real effects of internationally transmitted bank shocks in Eastern European countries and Turkey, and show that these shocks reduce firm assets

and profitability, especially among single-bank firms. Given that small and/or young firms tend to have fewer banking relationships, this might be one of the reasons why such firms are found to be more affected by reduced credit supply in some studies, but not in others.[39]

### 6.2.4 | Being part of a group network

The literature also examines whether firms that are a part of a conglomerate are better hedged against adverse credit supply shocks. Khwaja and Mian (2008) and Zia (2008) show that Pakistani firms that are a part of a larger network exhibit fewer defaults and better export performance, respectively. Greenstone et al. (2020) also provide evidence from the 2007–2008 financial crisis that small stand-alone firms in the United States tend to cut employment more than multi-unit firms.

### 6.2.5 | Access to other forms of finance

Another question that the literature has sought to answer is whether firms with access to other means of financing are better able to mitigate the consequences of a credit crunch. Lemmon and Roberts (2010) use a sample of large firms with access to public debt markets and still find a negative effect on firm investment when credit is contracted. Moreover, Amiti and Weinstein (2018) use a sample of listed firms that have access to both public debt and equity markets and show that even these companies reduce investment when they experience a decrease in credit supply. Although these results imply that firms with access to alternative sources of funding can be negatively affected from credit tightening, most evidence suggests that they are less affected than firms without such access. Specifically, the literature shows that when credit supply is reduced, subsequent real effects are more severe for unrated/unlisted firms (Acharya et al., 2018; Chodorow-Reich, 2013; Gropp et al., 2018; Hochfellner et al., 2015), firms with lower credit ratings (Campello et al., 2010; Lemmon & Roberts, 2010), and/or firms in less financially developed areas (Cingano, Manaresi, & Sette, 2016).

### 6.2.6 | Cash holdings

A specific form of alternative financing is internal funding with cash. The literature therefore also explores whether firms tap into their cash holdings to offset credit contractions, but the current evidence remains unclear. Duchin et al. (2010), Almeida et al. (2011), and Cingano, Manaresi, and Sette (2016) illustrate that firms use cash reserves to cushion the negative consequences of reduced credit supply. In addition, Campello et al. (2010) demonstrate that constrained firms are more likely to build cash stocks during normal times (presumably for precautionary reasons), and that they draw on their cash reserves during a credit crunch. In contrast, Lemmon and Roberts (2010) show that below-investment grade firms do not tap into their cash balances when they face negative shocks from their lenders—even if they are unable to issue long-term debt. Finally, Berg (2018) shows that firms with low liquidity increase their cash holdings while cutting noncash assets, investment, and employment following loan rejections. He attributes this to firms expecting to be more credit constrained in the future, that is, to a precautionary saving motive. Firms with high liquidity, instead, use their cash holdings to mitigate the effect of a loan rejection.

## 6.3 | Asymmetric effects

We have documented that negative credit supply shocks often lead to adverse real effects, while positive shocks only sometimes boost firm outcomes. To better understand whether the effect is indeed asymmetric for positive and negative credit supply shocks as well as between crisis and noncrisis times, we first turn to the group of papers that conduct their analysis over the entire business cycle covering both normal and crisis times. Amiti and Weinstein (2018), Degryse et al. (2019), Greenstone et al. (2020), have all made methodological contributions to recover time-varying credit supply without having to rely on measures of bank health. Yet, none of them finds strong evidence of asymmetric effects. Specifically, Amiti and Weinstein (2018) find no significantly different impact of credit supply shocks on investment in normal and crisis times in Japan between 1990 and 2010. Similarly, studying all corporate Belgian borrowers between 2002

and 2012, Degryse et al. (2019) show that the ability of firms to compensate for reduced bank lending is not different between crisis and tranquil times; accordingly, they find no differential impact on firm assets, investment, and profitability. Greenstone et al. (2020), instead, show that credit supply affects the employment of small stand-alone firms in the United States more during the 2008 crisis than during noncrisis times. Despite the magnitude of the crisis, however, the difference in the effect seems to be economically small.

In contrast, another group of papers appears to find that credit shocks do have asymmetric effects. Alfaro et al. (2021), for example, study real outcomes for an expansion period (2003–2007), a crisis period (2008–2009), and a recession period (2010–2013). They show that the estimated impacts of credit shocks on Spanish employment and output growth are highest during the financial crisis, followed by the effects during the recession period, and lowest during the expansion. Finally, Jiménez et al. (2017) provide additional evidence from Spain, when they investigate the effect of time-varying macroprudential policies. More specifically, they study dynamic provisions, which were introduced in 2000 in Spain, over the period 1998–2012. Dynamic provisions require banks to reserve some of their retained profits as a buffer in good times to compensate for realized losses in bad times. The authors show that the availability of credit to firms declined immediately after the introduction of dynamic provisioning. Because firms were able to switch to less affected lenders, however, there was no significant impact on firm employment during good times. Yet, firms borrowing from banks with relatively lower provisions could not easily replace reduced bank lending during bad times and responded by reducing employment. This suggests that negative shocks to credit supply matter more in bad times than in good times. Finally, rather than analyzing real effects by distinguishing between good and bad times, Manaresi and Pierri (2019) directly examine the shape of the relation between credit supply and productivity for the period of 1997–2013. They show that negative credit supply shocks reduce Italian firms' productivity, whereas positive shocks do not increase productivity to the same degree.

In summary, the evidence on asymmetric effects of credit supply shocks is ambiguous even though the relevant papers are able to rely on matched loan-level data and reliable identification strategies. It is thus likely that the diverging results are due to the different underlying samples. It is conceivable, for example, that Spanish and Italian firms are more vulnerable to reductions in banks' credit supply during crises times than firms in Japan and Belgium.

## 6.4 | Persistence of effects

The question of how persistent real effects of credit supply shocks are, is also of interest in the literature, although the evidence to date is relatively scarce and inconclusive. For example, Popov and Rocholl (2018) show that the negative employment impact of the 2008 credit crunch had dissipated 3 years after the initial shock. Huber (2018), on the other hand, demonstrates that the effect on employment remained significant by 2012; that is, 2 years after lending had normalized in 2010. Contrary to Popov and Rocholl (2018), this suggests that the 2008 credit crunch led to persistent effects in Germany, which is attributed by Huber (2018) to decreases in innovation and productivity. As when we discussed employment effects in Section 6.1.2, it is worth pointing out, however, that the sample of Popov and Rocholl (2018) is much larger and thus likely more representative. Falling somewhere between the two papers on Germany, Chodorow-Reich (2013) demonstrates that employment in the United States started to recover only in 2010, even though credit had normalized by 2009. Adding to the difficulties in determining the persistency of credit supply-induced real effects, it is well-documented that adverse credit shocks persistently affect firm productivity mainly due to reductions in intangible investment (De Ridder, 2019; Duval et al., 2020; Manaresi & Pierri, 2019).

## 6.5 | Spillover effects

So far, we have reviewed the literature examining the effects of credit supply shocks on borrowing firms only. Beyond these direct effects, a few studies show that credit shocks might also have indirect effects through the production network. Using data on supplier–buyer linkages, Costello (2020) investigates the spillover effects of credit supply shocks in the United States during the 2007–2008 crisis. She shows that credit constraints induce suppliers to cut trade credit to their downstream customers. She further illustrates that, when such credit rationing unfolds, constrained suppliers cut sales because the increased cost of capital forces them to reduce output and/or increase prices. As a result, she finds reduced trade credit and sales to be associated with a reduction in employment of about 11% for the average small downstream firm. In her sample, this is more than 60% of the direct effect (17.8%).

Alfaro et al. (2021) explore similar spillover effects of credit supply shocks in Spain over the period of 2003–2013. They do not have data on interfirm relationships, but use input–output information at the industry level. Like Costello (2020), they demonstrate that suppliers respond to credit shocks by increasing prices and more importantly by reducing trade credit. On average, a one standard deviation decrease in downstream credit supply seems to reduce investment by 9% (compared to a direct effect of 10%), employment growth by 96% (compared to a direct effect of 93%), and value added growth by 70% (compared to a direct effect of 20%). All in all, Costello (2020) and Alfaro et al. (2021) document sizeable externalities occurring primarily through the downstream transmission of credit supply shocks.

### 6.6 | Cross-country evidence

A small set of studies in this stream of the literature examines whether and how real effects of credit supply vary across countries. In their analysis of 41 countries worldwide, Dell'Ariccia et al. (2008) show that banking crises have greater real effects in the credit-dependent sectors of developing countries where equity and public debt markets are presumably less developed and government support for distressed banks might be less readily available. In a similar vein, they also document larger effects for countries with less access to foreign finance. Additionally, Ongena et al. (2015) provide cross-country evidence using data containing bank-firm relationships from Eastern European countries and Turkey. They show that the international transmission of credit supply shocks during the 2008 crisis was higher to countries with lower pre-crisis GDP growth, a lower pre-crisis ratio of credit to GDP (i.e., less financially developed), a higher pre-crisis ratio of foreign credit to GDP (i.e., higher reliance on foreign funding), and slower contract enforcement (because slow legal enforcement can make it difficult for banks to seize the borrower's collateral in case of default). Finally, Duval et al. (2020) demonstrate that firms in stressed European economies (Greece, Ireland, Italy, Portugal, and Spain) experienced a higher decline in their productivity growth than firms in non-stressed European countries following the crisis of 2008.

## 7 | FUTURE DIRECTIONS

Although the vast majority of the studies we have reviewed agree that credit supply shocks affect firm outcomes, a number of questions on the transmission channels, specific effects, and identification strategies remain. In this section, we first discuss general gaps that emerge from our synthesis of the empirical evidence. That is, we identify gaps that have not been studied in depth as well as gaps arising from mixed evidence. Then, we discuss methodological gaps that future research could aim to fill.

### 7.1 | Gaps that have not been studied in depth

#### 7.1.1 | The role of banks' private information about borrowers

The extant literature principally attributes sticky bank-firm relationships to private borrower information that is captured by firms' current lenders but not by others, which in turn makes it difficult for firms to switch across lenders. However, Darmouni (2020) has recently developed an empirical model to isolate private borrower information from common borrower information and provides an estimate of the information gap by taking into account the dynamic elements of lending relationships. By analyzing the United States syndicated loan market during the 2007–2008 crisis, he finds that private information plays only a small role in explaining sticky bank-firm relationships and thus the 2007 collapse of the syndicated loan market. This suggests that there is common borrower information that is observable to all lenders but not to the econometrician. Given that the United States syndicated loan market mainly contains large and transparent firms, future research could investigate whether the role that lenders' private information plays in bank-firm relationships is similar for smaller firms; for example, by using granular data from credit registers. This would not only serve to corroborate and generalize the results of Darmouni (2020), but it would also provide a more complete picture of the role of private information in bank-firm relationships.

### 7.1.2 | Do firms reduce investment optimally?

Empirical research generally indicates that borrowing-constrained firms cut investment, suggesting that investment is reduced suboptimally. Yet, there is also evidence implying that firms which lower their investment during a credit crunch are mainly those that previously overinvested. Lemmon and Roberts (2010), for example, show that adverse credit conditions resulted in improvements in the performance (e.g., operating income and return on equity) of below-investment-grade firms, suggesting that those firms may have invested in negative net present value projects when credit was loose. Evidence on the question of optimality is scarce, in part because it is difficult to measure firms' investment opportunities. Future research could thus aim to identify whether, when, and to what extent, the well-documented reduction in investment is welfare-reducing or part of a value-maximizing strategy.

### 7.1.3 | The effect of different loan types

Empirical studies employing the firm-time fixed effects approach mainly control for loan-types which otherwise might confound the results. Yet, the question of how the resulting real effects vary among different types of loans (capital expenditure, real estate, export, working capital) has been treated negligently in the literature, and could be addressed in future studies.

### 7.1.4 | The joint impact of employment protection and credit supply shocks

The empirical literature documents within- and between-country differences in labor cost adjustments. For example, evidence shows that layoffs were noticeable among Spanish firms following the 2007–2008 crisis (Bentolila et al., 2017), whereas German firms reacted to the same crisis mainly by reducing wages, with the effect being driven primarily by small companies (Popov & Rocholl, 2018). One important reason of such within- and between-country heterogeneity is likely to be employment protection legislation that affects both hiring and firing policies. For example, when we plot the average employment effects from different loan-level studies against the OECD's employment protection index in Figure 1, the effect on total employment tends to be smaller in countries with stricter employment legislation.[40] Although the literature often implicitly documents that legislation matters, its effect along with credit supply shocks on firms' investment and employment policies remains generally understudied.[41] Future research could therefore scrutinize the role of employment legislation for firms' response to credit supply shocks; ideally combining loan-level data with employee-level data.

### 7.1.5 | The effect of credit supply on labor allocation

Another interesting and open question is how firms' responses to credit supply shocks affect wage inequality and thus the (in)efficient allocation of human capital. Existing evidence on labor allocation is scarce (e.g., Bai et al., 2018; Barbosa et al., 2020; Berton et al., 2018; Moser et al., 2020), and leaves plenty of room for additional investigations.

### 7.1.6 | The effect of credit supply on market structure

Given that empirical research finds an adverse effect of reduced credit supply on sales (e.g., Acharya et al., 2018), the question of how changes in credit supply alter product market structures, constitutes another gap in the literature. Future studies could therefore examine the effects of credit supply on competition and pricing policies.

### 7.1.7 | The role of corporate governance in the transmission of bank credit supply shocks to firms

The existing literature has not paid much attention to the role of governance-related borrower characteristics and their role for the effect of changing credit supply on firm outcomes. The theoretical literature indicates a number of reasons

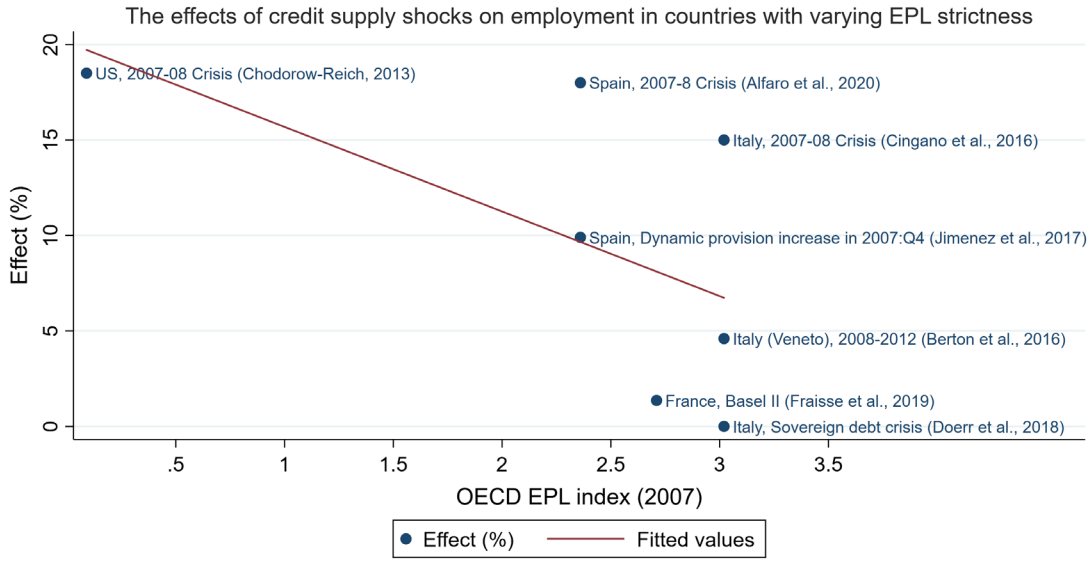


**FIGURE 1** This figure shows the effects of credit supply shocks on employment documented in the literature for countries with varying strictness of EPL. The y-axis corresponds to the documented average effect (%) on employment when credit supply increases by one standard deviation. The solid line illustrates the linear fit. For comparability, we only present the studies that use matched bank-firm loan level data. The OECD EPL index is based on individual and collective dismissals

for why corporate governance may matter. First, better governed firms have fewer agency problems, which reduces the cost of capital, enhances operational efficiency, and ultimately implies reduced reliance on external financing (Bolton & Freixas, 2000; Diamond, 1991). Second, better governed firms are more transparent, alleviating information asymmetries between borrowers and lenders (see Shleifer and Vishny (1997) for a survey) and may help firms to switch banks during a credit contraction. Although the channels linking corporate governance to bank lending are theoretically well-understood, evidence on the topic is limited. Nguyen et al. (2015), for instance, investigate this issue with unmatched firm-level data, and find that better governed firms performed better during the financial crisis of 2007–2008. To comprehensively study whether good governance protects firms against the negative effects of reduced credit supply, loan-level data would need to be matched with borrower-specific governance data.

### 7.1.8 | The spillover effects of credit shocks

The vast majority of the literature assesses the real impact of bank health only on direct borrowers. However, credit shocks might have important spillover effects through the production network, especially when credit constraints induce suppliers to cut trade credit to their customers. Only a few studies show that credit supply shocks can indeed be propagated through buyer–seller relations, and affect downstream firms' investment, employment, and output (Alfaro et al., 2021; Costello, 2020). This existing evidence further illustrates that such indirect effects might be as large as direct effects (Alfaro et al., 2021).

### 7.1.9 | Cross-country heterogeneity of the resulting outcomes

Our literature review suggests important cross-country variation in the real effects of credit shocks. This can be seen from Figure 2, which suggests that the effect on investment is larger in countries with lower GDP per capita. Nevertheless, extant research provides little direct evidence on this topic (Dell'Ariccia et al., 2008; Ongena et al., 2015). This issue can be better examined using datasets that combine loan-level data from multiple countries. As also emphasized by Jakovljević et al. (2020), current research has already started to employ such datasets. Altavilla et al. (2020), for example, employ a dataset that combines loan data from 15 European credit registers, collected within the scope of the

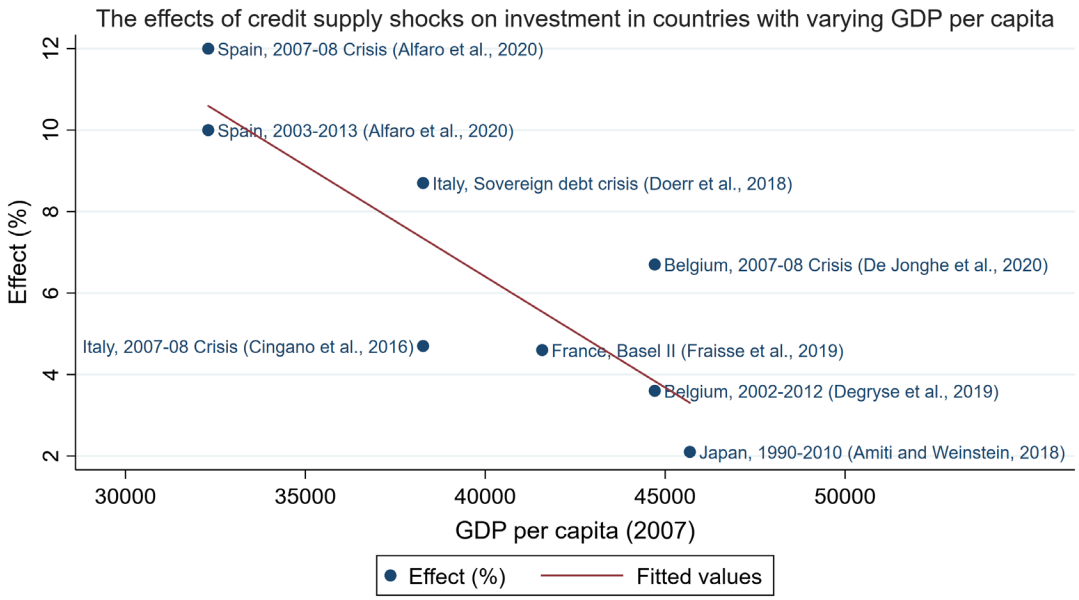


**FIGURE 2** This figure shows the effects of credit supply shocks on investment for countries with varying GDP/capita (in $). The y-axis corresponds to the average effect (%) on investment when credit supply increases by one standard deviation. The solid line illustrates the linear fit. For comparability, we only present studies that use matched bank-firm loan level data. Data on GDP/capita is obtained from World Bank Development Indicators

AnaCredit project by the ECB. Future studies that have access to such granular combined data can thoroughly exploit cross-country variation and document the mechanisms behind heterogeneous real outcomes.

## 7.2 | Gaps arising from mixed evidence

### 7.2.1 | The role of other types of financing

The existing literature provides mixed evidence on the use of other means of financing during a credit crunch. Some studies document that firms use their cash holdings to offset the reduction in credit supply (e.g., Cingano, Manaresi, & Sette, 2016), whereas others find that cash reserves play a nonlinear role (e.g., Berg, 2018). Similarly, evidence shows that firms with better access to other markets are sometimes better protected from credit reductions (e.g., Acharya et al., 2018; Chodorow-Reich, 2013) and sometimes not (e.g., Amiti & Weinstein, 2018). Taken together, we do not know much about the factors shaping firms' incentives to use other sources of financing when credit is scarce.

### 7.2.2 | Disentangling firms' ability to absorb credit shocks from the direct exposure to the shock

Only some of the studies examining heterogeneity in the real effects of credit supply shocks analyze whether the resulting real effects stem from firm-type-specific cuts in credit supply or from firm-type-specific inability to substitute for bank credit. Among them, Khwaja and Mian (2008) show that the resulting adverse effects for small firms result from both a larger credit cut that they experience from their lenders and their inability to compensate for reduced bank lending. In contrast, Cingano, Manaresi, and Sette (2016), De Jonghe et al. (2020), and Bottero et al. (2020) illustrate that small firms experience more severe real effects, not because their lenders reduce credit more to them, but because they cannot easily offset credit contractions. Given this mixed evidence, future research could help to better disentangle and understand these two channels of heterogeneity across firm characteristics.

### 7.2.3 | The persistence of the real effects of credit supply

The question of whether the real effects of credit supply shocks are persistent also remains largely inconclusive. For example, while Popov and Rocholl (2018) find that the negative employment effect of the 2008 crisis was not persistent in Germany, Huber (2018) demonstrates that the 2008 credit crunch had persistent employment effects for German firms even long after credit supply had normalized. Given this mixed evidence, future research could further explore the mechanisms that repress investment and hiring after a credit crunch.

### 7.2.4 | More evidence on the asymmetric effects of credit supply

A large fraction of the literature investigates the real effects of credit supply shocks only during crises times (i.e., after one-off exogenous events). A small set of studies, however, examines the resulting effects over the entire business cycle and looks at whether the impact varies between crisis and normal times, particularly after the methodological contributions that enable the estimation of time-varying credit supply shocks without relying on a measure of bank health (Amiti & Weinstein, 2018; Degryse et al., 2019; Greenstone et al., 2020). These studies generate mixed evidence. Some papers show that credit supply might have differential outcomes between crisis and good times (Alfaro et al., 2021; Jiménez et al., 2017), others find no asymmetric effects (Amiti & Weinstein, 2018; Degryse et al., 2019; Greenstone et al., 2020). This suggests that future research is needed and indicates that longer sample periods may be necessary to fully understand whether firms in crises times are differently affected by credit supply shocks than firms during normal times. Such analyses over longer periods would also provide a more suitable environment for policy recommendations.

## 7.3 | Methodological gaps

### 7.3.1 | More strategies to control for credit demand also for single-bank firms

We have documented that loan-level studies estimate a loan-growth equation mainly either using firm-time fixed effects for multibank firms or using firm-cluster fixed effects for single- and multibank firms to control for credit demand. While firm-time fixed effects control for idiosyncratic loan demand by controlling for both observed and unobserved time-varying firm-specific factors, firm-cluster fixed effects do not. They assume that firms in the same cluster have equal credit demand. The validity of the firm-cluster fixed effects approach depends on its assumption which might arguably be strong and ultimately may not always hold. Although Degryse et al. (2019) show that the ILST fixed effects approach is valid for a Belgian sample, it may not be feasible to use ILST fixed effects for samples that have different characteristics than those of the Belgian sample. In this regard, future research could test the validity of ILST fixed effects outside Belgium by building on Degryse et al. (2019), and more ambitiously propose novel ways of controlling for credit demand for samples where single-bank firms are abundant and are different than multibank firms.

### 7.3.2 | Incorporating loan applications into the analysis to address sample selection bias

Given that data on loan applications are often not available to researchers, the vast majority of the studies are not able to incorporate rejected loans into their analysis, and thus their methodology might suffer from sample selection bias. As an exception, Jiménez et al. (2014) utilize loan applications and show that sample selection bias indeed matters for their loan-level regressions. In the future, data on loan applications may become more available to researchers; for example from online lending platforms.

## 8 | CONCLUSION

Improved access to granular loan-level data has recently enabled a wealth of new insights on the real effects of bank credit supply fluctuations. We therefore review the evolution of the corresponding literature, including these recent contributions, and identify a number of critical questions that require further analysis. In doing so, we hope not

only to provide a comprehensive overview of the current state of knowledge, but also to foster and guide future research.

At the same time, our in-depth review also provides value for policymakers. For example, by emphasizing the role of employment protection legislation or by summarizing how regulatory or monetary policy interventions affect credit supply and ultimately lead to real effects at the firm level. We also draw attention to the importance of bank capitalization in curbing zombie lending and to the effectiveness of banking supervision frameworks in restraining excessive risk taking. Finally, our survey also points to the potentially stabilizing effect of promoting market-based financing.

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## ENDNOTES

[1] See Section 2 for a brief review of the main underlying frictions.

[2] Brunnermeier and Krishnamurthy (2020) discuss extensions of the financial accelerator model, featuring feedback from asset prices (Brunnermeier & Sannikov, 2014; He & Krishnamurthy, 2019) and point out shortcomings of the framework of Bernanke et al. (1999). This discussion is beyond the scope of our article and we refer to the original paper for additional details.

[3] See Calomiris et al. (2017) for a more comprehensive review of the evolution of credit rationing theory.

[4] In Section 4, we discuss the endogeneity concerns pertinent to earlier research in more detail and illustrate how matched bank-firm loan level data has enabled researchers to improve identification.

[5] Central banks also often have data on firm and bank balance sheets. For papers that employ matched data but not from credit registers, Bankscope is the most commonly used source of bank balance sheet information, whereas Compustat and Orbis serve as the main sources of firm balance sheet data.

[6] The minimum loan amount that requires registration depends on the regulations of each country. In most countries, this amount is fairly low (e.g., EUR 6000 in Spain) and hence almost all loans are observed. One exception, however, is the German credit register where the threshold is 1 million euro.

[7] The reason why we do not present Equation (1) as a panel-data model is because the majority of the empirical literature uses two periods (i.e., a before-and-after), and then takes first-differences of the data.

[8] The selection could also occur in the opposite direction. In the study of Khwaja and Mian (2008), for example, more affected banks tend to be selected by better performing firms, which in turn leads to a negative correlation between credit supply and credit demand. Section 4.1.1 explains the study of Khwaja and Mian (2008) in more detail.

[9] Note that our aim here is to provide an overview of methodologies used to estimate how banks transmit shocks to their borrowers. The papers presented in Section 4.1 therefore do not necessarily consider real effects. Afterwards, we illustrate how these identification strategies are used to estimate the real effects of bank lending shocks.

[10] The cross-guarantee provisions that were exercised by the Federal Deposit Insurance Corporation led to the failure of healthy bank subsidiaries between 1988 and 1992, when the unhealthy parent of these subsidiaries failed. Thus, the failure of those subsidiaries was independent of local economic factors and did not affect demand.

[11] In 1992, the Pakistani government introduced dollar deposit accounts, which had become very popular and accounted for 43.5% of total deposits by 1998. Banks, however, were not actually permitted to hold dollars themselves, and had to exchange them for rupees at the central bank. When depositors claimed their dollars back in response to the nuclear tests, banks were only able to withdraw them from the central bank at the initial exchange rate. With the International Monetary Fund no longer supporting the exchange rate as an economic sanction after the tests, exchange rate risk materialized and caused a shock to bank liquidity (Khwaja & Mian, 2008).

[12] In fact, Gan (2007) employs the two-stage model of Heckman (1979) to address the potential survivorship bias because 35% of the bank-firm relationships in her sample did not survive after the Japanese land market crash. In the first stage, she estimates a probit regression on whether the lending relationship survived. Then, the second stage estimates the loan growth regression demonstrated in Equation (2).

[13] In a panel data set, firm-time fixed effects need to be used. As Gan (2007) and Khwaja and Mian (2008) collapse their data into single pre- and post- periods and then take the first difference of the committed credit, their regression sample is only cross-sectional. Hence, they use firm fixed effects instead of firm-time fixed effects in their main regressions.

[14] The right hand side of Equation 3 can also include bank-firm fixed effects to control for endogenous bank-firm relationships, as in Jiménez et al. (2017). Amiti and Weinstein (2018) formally demonstrate that as long as regressions in their model are estimated based on the same numeraire, the exclusion of bank-firm fixed effects does not affect the consistency of the estimates of bank and firm shocks.

[15] In fact, some other papers compare firms in similar clusters (e.g., Acharya et al., 2018; Berton et al., 2018; Edgerton, 2012; Ferrando et al., 2019; Morais et al., 2019). However, those studies do not examine the validity of their approaches as detailed as Degryse et al. (2019).

[16] The reason why this is not 100% is because Equation (8) requires that firms in ILST clusters borrow from at least two different banks. Otherwise, all firms in that ILST cluster borrow from the same lender, and then that cluster cannot contribute to the identification of $\beta_{bt}$.

[17] Degryse et al. (2019) also apply the methodology of Amiti and Weinstein (2018) through a weighted-least-square procedure suggested by Tielens and Van Hove (2017), except that they replace firm-time fixed effects with ILTS fixed effects allowing them to also account for single-bank relationships.

[18] Alternatively, De Jonghe et al. (2020) run an Equation (2)-type model and replace firm fixed effects with industry-location-size fixed effects to incorporate both single- and multibank firms into their analysis.

[19] By using survey data, a small set of studies also demonstrates that discouraged firms (i.e., those that need external finance but do not apply for a bank loan in anticipation of being rejected) matter in bank lending as well as for the resulting real effects. Not incorporating them into the analysis can thus also cause biased results (Ferrando & Mulier, 2015; Popov, 2016; Popov & Udell, 2012).

[20] Other exceptions are Jiménez et al. (2012), Abuka et al. (2019), and Agarwal et al. (2020), all of whom also consider data on loan applications from credit registers but do not tackle the selection bias.

[21] Jiménez et al. (2014) estimate the selection equation through a linear probability model. Then, to estimate the second-stage regression, they follow Kyriazidou (1997), who proposes a method to recover the estimates of the main equation in the presence of the nonlinear sample selection effect coming from the first-stage regression. For further details on the main estimation procedure, we refer to Jiménez et al. (2014).

[22] In early research, a set of studies without loan-level data also investigates the effect of credit shocks on real activity (e.g., Ashcraft, 2005; Bernanke, 1983; Peek & Rosengren, 2000). However, our primary focus is on the recent methodologies that mainly employ loan-level data.

[23] The literature also attempts to quantify the effect of bank shocks on the price of loans. A set of studies show that banks that are more affected by adverse shocks charge higher interest rates (e.g., Acharya et al., 2018; Chodorow-Reich, 2013; Santos, 2010), whereas some papers do not find a significant differential impact (e.g., Bentolila et al., 2017; Cingano, Manaresi, & Sette, 2016; Khwaja & Mian, 2008). The latter is in line with Stiglitz and Weiss (1981), who argue that banks tend to adjust loan volumes rather than rates to ameliorate concerns related to adverse selection.

[24] For each year, Greenstone et al. (2020) calculate $\textit{Credit supply}_{county} = \sum_b \textit{bank base period market share}_{county,b} \times \hat{\beta}_b$, where $\hat{\beta}_b$ are the estimated bank credit supply shocks from the regression equation: $\Delta \textit{Lending}_{b,county} = \alpha_{county} + \beta_b + \textit{error term}$.

[25] Similar to Amiti and Weinstein (2018) and Degryse et al. (2019), the approach of Greenstone et al. (2020) also allows the researcher to estimate bank credit supply shocks over the business cycle.

[26] See Section 2 for more detail on the origins of real effects.

[27] As in the previously mentioned papers, the strength of a bank's deposit base also plays an important role in this article, as banks with a weaker deposit base rely more on the interbank market.

[28] The primary nonbank source of external funding for firms is typically the issuance of bonds. Indeed, Adrian et al. (2013) decompose the total credit to the nonfinancial, nonfarm corporations in the United States between 1990 and 2010, and show that firms rely on corporate bonds (40%), nonbank loans and advances (25%), bank loans (15%), mortgages (15%), and commercial paper (5%) for funding.

[29] In fact, theories of intermediated debt imply that bank lending -compared to other forms of debt- is more attractive to borrowers in times of financial distress because of its potential flexibility through renegotiations, and superior monitoring. For the detailed theoretical arguments, see Diamond (1991), Rajan (1992), Chemmanur and Fulghieri (1994), and Bolton and Freixas (2000).

[30] The survey data of Campello et al. (2010) also comprises European and Asian firms. The authors demonstrate that those firms planned to reduce investment as well.

[31] Apart from the bulk of the literature analyzing the 2008 and sovereign debt crises, see also the following studies linking crisis-induced credit supply contractions to reduced investment: Gan (2007), who is one of the first to conduct a loan-level analysis and exploits the Japanese land market collapse during the 1990s, and both Dell'Ariccia et al. (2008) and Doerr and Schaz (2021), who exploit historical banking crises.

[32] The OMT program was initiated in 2012, and theoretically permits the ECB to purchase unlimited amounts of sovereign bonds from financially distressed countries in secondary markets. The aim of the program was to recapitalize European banks with significant holdings of peripheral bonds and thus to stabilize the European banking system.

[33] Notice that the study of Morais et al. (2019) also examines internationally-transmitted credit supply shocks and could thus have also been categorized alongside Peek and Rosengren (2000) and Chava and Purnanandam (2011).

[34] In addition, Chodorow-Reich (2013) instruments this firm-level measure of credit supply using the firm's lenders' exposure to Lehman Brothers, exposure to mortgage-backed securities, trading account losses, real estate charge-offs, and deposits-to-liabilities ratio.

[35] Landesbanken are owned by a group of savings banks in their region, and savings banks had to provide capital for their Landesbank when the US subprime mortgage crisis imposed a negative shock (arguably exogenous to local economic conditions) on their capital.

[36] Huber (2018) also assesses the employment outcome following the 2008 crisis at the county-level. His results from the county-level regressions are more sizeable than his results from the firm-level regressions. He attributes this to the indirect effects captured only by the county-level estimates.

[37] In this stream of the literature, see also Dell'Ariccia et al. (2008) and Doerr and Schaz (2021), both of whom link credit supply contractions to reduced employment by exploiting historical banking crises.

[38] More specifically, Paravisini et al. (2017) show that a reduction in credit supply by a bank results in a stronger decline in exports to countries in which the bank specializes. This is, presumably, because lending from a specialized bank is more difficult to replace.

[39] In fact, Amador and Nagengast (2016) illustrate that the number of banking relationships, rather than firm size, determines the firm's ability to replace reduced bank lending.

[40] Because of comparability concerns, Figure 1 should be read as indicative. Although we improve comparability by focusing only on studies using loan-level data, the underlying papers are not identical in terms of sample characteristics, identification strategies, or events analyzed.

[41] Although some papers examine the joint impact of labor and financial market frictions, they do so without using loan-level data (Bai et al., 2020; Calcagnini et al., 2009, 2014; Cingano, Leonardi, et al., 2016). Coming closest to what we suggest is Laeven et al. (2018), which examines how employment protection legislation in Spain affected firm growth during the financial crisis.

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Additional supporting information may be found online in the Supporting Information section at the end of this article.

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