The Real Effects of Credit Supply: Review, Synthesis, and Future Directions

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Abstract

This paper reviews the rapidly growing literature on the real effects of bank credit supply fluctuations and identifies several worthwhile avenues for future research. In terms of the transmission of credit supply shocks into real effects, we suggest to further investigate the roles of (i) private borrower information, (ii) employment protection legislation, (iii) corporate governance, (iv) bank specialization, and (v) alternative financing sources. We also call for additional analyses of how these shocks affect (vi) investment efficiency, (vii) market structure, and (viii) the allocation of human capital, and emphasize the need for more evidence on (ix) the persistency, (x) asymmetry, and (xi) heterogeneity of their effects.

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1. Introduction

The question of how fluctuations in bank credit supply affect real economic activity has interested economists at least since the Great Depression and was prominently addressed in a number of early studies (e.g., Bernanke, 1983; Peek and Rosengren, 2000). Recently, due to the financial crisis of 2008 and the improved availability of granular loan-level data, this question has been revisited extensively (e.g., Gan, 2007; Khwaja and Mian, 2008; Chodorow-Reich, 2013; Cingano et al., 2016; Jiménez et al., 2017; Berg, 2018; Morais et al., 2019). Motivated by the rapid growth of this literature, we review and synthesize the extant body of work and derive recommendations for future research.

Reductions in credit supply have real effects for borrowers, when they are unable to compensate with alternative sources of funding and forced to adjust their investment and/or labour force (Bernanke and Gertler, 1989; Kiyotaki and Moore, 1997). Identifying the causal link from credit supply to firm behaviour empirically, however, is challenging for two main 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, e.g. because of reduced - and typically unobservable - investment opportunities and/or working capital needs (Bernanke and Gertler, 1995). Second, one needs to control for the endogenous matching between firms and banks, which arises when firms with a poor performance outlook prefer to borrow from banks that are less likely to monitor and discipline (Gan, 2007). Since the performance outlook is typically unobservable as well, researchers are frequently forced to rely on imperfect proxies.

While early research entirely relied on macro-level data and potentially biased time-series correlations (e.g., Bernanke, 1983), subsequent waves of papers focused on cross-sectional variation in macro- (e.g., Peek and Rosengren, 2000; Dell'Ariccia et al., 2008) or firm-level data (e.g., Campello et al., 2010; Duchin et al., 2010; Almeida et al., 2011; Benmelech et al., 2019). Most of these studies attempt to address the aforementioned endogeneity concerns in some way, but cannot rule them out altogether. More recently, 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) are the first to isolate credit supply by examining how banks with different exposure to adverse shocks change their lending towards the same borrower (the firm-time fixed effect approach). This approach enables causal inference and has subsequently been applied by many other studies (e.g., Chodorow-Reich, 2013; Bentolila et al.,

2017; Jiménez et al., 2017; Morais et al., 2019). Despite its advantages and popularity, however, it comes with two important limitations. It is unable to identify aggregate equilibrium outcomes and it is not applicable when firms borrow from only one lender; these limitations are addressed by, respectively, Amiti and Weinstein (2018) and Degryse et al. (2019).

Before documenting these methodological advances, we first extensively review the existing evidence. In terms of real outcomes, the literature focuses primarily on investment and employment. Extant research exploiting negative shocks to credit supply documents that reductions in credit supply translate into an overall decline of firms' investment (e.g., Cingano et al., 2016; Amiti and Weinstein, 2018) and employment (e.g., Chodorow-Reich, 2013; Bentolila et al., 2017). In addition, it has also been shown that negative effects of credit supply are also reflected in firms' valuation (e.g., Gan, 2007), firms' export growth (e.g., Paravisini et al., 2014) as well as sales growth (e.g., Acharya et al., 2018). These effects are generally stronger for small (e.g., Khwaja and Mian, 2008), young (e.g., Cingano et al., 2016), and single-bank firms (e.g., Degryse et al., 2019). Moreover, cash holdings (e.g., Berg, 2018) and access to other sources of funding (e.g., Campello et al., 2010) play an important role in the transmission of credit supply into the real economy.

Interestingly, the evidence on employment effects is sometimes moderate (e.g., Greenstone et al., 2014; Popov and Rocholl, 2018) or not significant (e.g., Berg, 2018), presumably because employment protection laws might induce companies to cut wages rather than employees (e.g., Popov and Rocholl, 2018). The role of employment protection is also consistent with the observation that labour force adjustments often concentrate on less educated (e.g., Hochfellner et al., 2015), shorter-tenured (e.g., Caggese et al., 2019), younger (e.g., Berton et al., 2018) and/or female employees (e.g., Berton et al., 2018), and in particularly those with temporary contracts (e.g., Caggese and Cuñat, 2008).

While most of the literature analyzes the effect of negative shocks to credit supply, there is also a small group of papers that examines positive shocks. For these, the results are more ambiguous. Some studies show that an increase in credit availability leads to zombie lending[1], without positive implications for the real sector (e.g., Acharya et al., 2019; Giannetti and Simonov, 2013). Other evidence, instead, suggests that positive credit supply shocks improve firm outcomes (e.g., Ferrando et al., 2019).

[1] Caballero et al. (2008) define zombie lending as the situation in which banks extend credit to insolvent firms in order to recover their non-performing loans and to avoid taking losses on their capital.

As a result of our comprehensive survey of the literature, we identify several worthwhile avenues for future research. Specifically, we suggest to further investigate (i) the role of banks' private borrower information, the roles of (ii) employment protection legislation and (iii) corporate governance in the transmission of credit supply fluctuations, the relevance of (iv) bank specialization and (v) firms' use of alternative sources of financing, (vi) the relation between credit supply and firms' investment efficiency, the effects on (vii) market structure and (viii) the allocation of human capital, as well as (ix) the persistency, (x) asymmetry, and (xi) heterogeneity of real outcomes. Finally, we also discuss shortcomings of the current methodological conventions to stimulate additional research on the identification of credit supply shocks.

The structure of the paper proceeds as follows. Section 2 briefly presents the theoretical literature linking credit supply to firm outcomes. Section 3 discusses the existing empirical evidence. Section 4 thoroughly reviews current identification strategies. Section 5 discusses avenues for future research, and Section 6 concludes.

2. How credit supply shocks affect firm outcomes

In principle, firms can compensate for reduced credit supply from banks either by tapping into other funding sources or by switching to different banks. Yet, other funding sources, such as bonds or equity, are not perfect substitutes and may not (always) be (easily) accessible (see James and Smith (2000) for a comprehensive review of the corresponding literature). At the same time, information asymmetries between lenders and borrowers and sticky bank-firm relationships, may also impair firms' ability to switch to a new bank. Long-term bank-firm relationships might serve as a signalling tool that alleviates moral hazard (Holmstrom and Tirole, 1997), or - when current lenders have private information about high-quality borrowers - result from adverse selection (Sharpe, 1990).[2]

These limitations collectively imply that firms may often not be able to smooth out fluctuations in bank credit supply by raising funding from alternative sources; in the presence of a credit crunch, they are therefore forced to adjust in other ways, which ultimately generates effects for the real economy. Like the empirical, the theoretical literature focuses primarily on

[2]In addition, Dell'Ariccia and Marquez (2004) theoretically shows the importance of private borrower information captured by lenders in credit allocation. However, Darmouni (2019) in his empirical work finds that the role that lenders' private information play in bank-firm relationships is rather moderate in the U.S. syndicated loan market; that is, there is common information that is observable to all lenders but not to the econometrician.

investment and employment outcomes, with Bernanke and Gertler (1989) and Kiyotaki and Moore (1997) prominently predicting that firms invest less when their net worth deteriorates.

Although these models suggest adverse effects on employment when credit is scarce, the effect is not necessarily straightforward and depends on the interaction between credit and labour market frictions (see, for example, the review of Boeri et al., 2013).

When there is a timing mismatch between payments to employees and the realization of cash flows, firms are forced to finance salaries through their working capital (Greenwald and Stiglitz, 1988). Accordingly, the deterioration in financing labour capital leads firms to cut employment.

Additionally, some studies stress the effect of firms' labour demand on the relation between credit supply and employment. Greenwald and Stiglitz (1987) argue that finance and labour complement each other in production so that financing constraints alter labour outcomes. They assert that firms are more likely to be risk-averse under capital market imperfections, which lowers the marginal product of labour and translates into reduced labour demand and thus employment. In addition, quasi-fixed labour costs (e.g. hiring or training costs) may reduce firms' demand for labour in imperfect capital markets (Oi, 1962).

Besides the aforementioned channels, regulation also plays an important role in the transmission of credit supply shocks to the real economy (albeit often indirectly). The existence of employment protection legislation, along with the availability of temporary and permanent contracts, tends to affect firms' employment decisions (e.g. hiring, firing, whom to hire, whom to fire). The theoretical literature, for example, shows that financially constrained firms prefer more temporary workers, who in turn contribute to a larger volatility of total employment (Caggese and Cuñat, 2008).

Our rudimentary discussion of the theoretical literature serves to put the empirical work that we review in the remainder of this paper into context, but also 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 evolution of the empirical literature on the real effects of credit supply fluctuations.

3. Real effects

This section reviews the empirical literature on the real implications of changes in bank credit supply (See Table 6 for a structured overview). Since studies in this strand of the literature have focused primarily on investment and employment consequences, we separately present the findings for these dependent variables in the first two subsections, before providing a summary of other real effects in the third subsection.

3.1. Investment

If firms cannot offset reductions in credit supply by switching to other lenders or by tapping into other financing sources, they may have to reduce investment (e.g., Peek and Rosengren, 2000; Gan, 2007; Almeida et al., 2011; Amiti and Weinstein, 2018; Cingano et al., 2016; Berg, 2018; Acharya et al., 2019).

Some papers linking credit supply to investment conduct an analysis at the aggregate-level. Peek and Rosengren (2000), for example, utilize state-level data and exploit the burst of the Japanese land market bubble as an exogenous shock to U.S. commercial real estate lending. The authors document that the reduction in Japanese bank lending translates into slower growth in new construction projects in the U.S. real estate sector. Dell'Ariccia et al. (2008) analyse industry-level data from 41 countries between 1980-2000. By following the approach of Rajan and Zingales (1998), who measure external financial dependence by the proportion of investment financed with external funds, they test whether industries more dependent on external finance experience slower investment growth than less dependent ones. They find around 4 percentage points more investment growth in less financially dependent sectors (the 25th percentile) vis-a-vis more financially dependent sectors (at the 75th percentile) during banking crises.

The vast majority of papers have investigated the relation between credit supply and investment at the firm level rather than at the macro level. Some of the papers examining this issue at the firm level do not have the necessary information to match banks to firms (Campello et al., 2010; Duchin et al., 2010; Lemmon and Roberts, 2010; Almeida et al., 2011). As one of them, Almeida et al. (2011) explore the investment outcomes of a sample of U.S. firms during the recent financial crisis. They use ex-ante variation in firms' long-term debt maturity to identify credit supply effects, since firms whose debt matures during the crisis will likely experience difficulties to re-finance their debt, and thus face a credit supply shock. Their study concludes that firms with large fractions of their long-term debt ma-

turing during the crisis reduce their quarterly investment by around 2.2 percentage points compared to those whose long-term debt did not mature. Duchin et al. (2010) also analyse a sample of U.S. firms, but differently than Almeida et al. (2011), they rely on firms' cash reserves and short-term debt positions as proxies for firms' exposure to the negative credit supply shock in the wake of the recent financial crisis. Their study reveals that, following a credit shrinkage, U.S. firms with low cash reserves and/or with high net short-term debt experience larger declines in investment, providing consistent results with Almeida et al. (2011). Lemmon and Roberts (2010) also utilize information from U.S. firms but focus on the period between 1986 and 1993. More specifically, they exploit a chain of events that induced an adverse credit shock to below-investment-grade firms.[3] In accordance with the preceding studies, they find that below-investment-grade firms reduce their net investment by 5% more than their unrated counterparts. Finally, Campello et al. (2010) use firm level data with a relatively more extensive coverage, including European and Asian firms besides U.S. firms. Unlike the rest of the papers, their study relies on survey data obtained from CFOs to identify the degree of financial constraints for each firm. They illustrate that the average constrained firm planned to cut back on investment by around 9 percentage points during the 2008 crisis, regardless of their origin.

Another set of papers in this strand of the literature have information on bank-firm relationships but not on the committed loans (Balduzzi et al., 2018; Ferrando et al., 2019).[4] Among them, Balduzzi et al. (2018) explore the effect of credit contraction on firm investment during the 2008-financial and sovereign debt crises for Italian firms. They exploit changes in banks' financial market valuations proxied by CDS spreads and Tobin's Q measures as the sources of variation in bank health. They find that, in line with the other studies, an average firm in the sample cuts investment when its bank sees an increase in CDS (or a decrease in equity valuation). Distinct from the aforementioned papers quantifying the impact of credit supply reductions, Ferrando et al. (2019) examine the subsequent investment outcome following the announcement of the Outright Monetary Transactions (OMT) program through which European banks with significant holdings of peripheral bonds experienced windfall gains.[5] They use data on small and medium-sized enterprises (SMEs), obtained mainly from the European Central Bank (ECB)'s "Survey on the Access to Finance of Enterprises".

[3]Those events are (i) the collapse of Drexel, (ii) the passage of the Financial Institutions Reform, Recovery and Enforcement Act of 1989, and (iii) regulatory changes in the insurance sector.

[4]See also Hosono et al. (2016) and Edgerton (2012) in this group of the literature.

[5]By means of the OMT program initiated in 2012, the ECB is theoretically permitted to purchase unlimited amount of sovereign bonds of 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 restabilize the European banking system.

They have information on bank-firm credit relationships but not on loan-level, and accordingly compare firms in country-sector-time clusters to control for credit demand. They find that SMEs borrowing from banks with higher exposure to sovereign bonds exhibited higher investment, in line with the firm balance sheet channel (Bernanke and Gertler, 1995).

A growing body of the recent empirical literature has utilized matched bank-firm loan data (often extracted from credit registers). Having access to such granular data sets has given those studies an edge in the identification of credit supply. More specifically, loan-level studies isolate credit supply by examining how banks with different exposure to adverse shocks change their lending towards the same borrower (i.e. the firm-time fixed effect approach (Gan, 2007; Khwaja and Mian, 2008)).[6] This approach allows to control for credit demand and addresses the potentially endogenous matching between firms and banks, which in turn enables causal inference.

Gan (2007) is one of the first studies that uses loan-level matched data. She exploits the Japanese land market collapse during the early 1990s, and uses the exposure of firms' top lender to the real estate sector as the transmission channel for the shock. She shows that Japanese industrial firms cut down on investment proportional to their exposure to the credit supply shock. Cingano et al. (2016) draw upon a sample of matched loan-level data extracted from the Italian credit registry. They exploit the liquidity shrinkage in interbank markets following the 2008 financial crisis, and find that a 10% increase in the exposure of the firm's lenders to the interbank market collapse reduces the firms' investment by 1.8 percentage points. Acharya et al. (2018) examine the resulting firm outcomes of the European sovereign debt crisis which induced a negative credit supply shock to banks holding sovereign debt, using syndicated loan-level matched data for the period from 2006 to 2012. Because their syndicated loan data does not contain information about changes in each loan over time, however, they do not use firm-time fixed effects. Instead, they compare firms in country-industry-credit rating clusters, similar to Ferrando et al. (2019), to attain identification. As a result, they reveal that one standard deviation increase in the exposure to the sovereign debt crisis resulted in 5.9 percentage points decline in investment.

[6]Broadly, loan-level studies estimate a loan growth equation with firm-time fixed effects at the loan level (i.e. within-firm comparison: comparison of changes in the same firm's loans from different banks) as well as without firm-time fixed effects at the firm level (i.e. cross-sectional comparison). Whereas a within-firm comparison rules out the identification concerns pertaining to unobservable firm characteristics, a cross-sectional comparison does not. Thus, by comparing these two estimations, studies that have access to loan-level data are able to draw causal inferences. Section 4 explains the identification strategies in detail.

Additionally, a number of loan-level studies examine the investment effects by exploiting positive shocks to credit supply. Morais et al. (2019) investigate the real effects of quantitative easing (QE) and low monetary policy rates in Europe and the U.S., on an emerging market, Mexico. Using Mexican loan-level matched data over the period 2001-2015, they find that Mexican firms with a higher share of credit from U.S. and European banks raised their net investment more than that of firms borrowing mainly from Mexican banks, especially when foreign monetary policy softened. Giannetti and Simonov (2013) examine the consequences of bank bailouts during the Japanese banking crisis of the 1990s using loan-level data on Japanese listed companies. They find that banks which remained under-capitalized following the government intervention mainly lent to "zombie" borrowers (Caballero et al., 2008) such that these borrowers could meet the payments on their outstanding loans. Hence, the banks themselves did not need to write off those outstanding loans, helping them to preserve their capital. As a result, Giannetti and Simonov (2013) show that the bank bailouts did not boost investment at large. Similar to Ferrando et al. (2019), Acharya et al. (2019) scrutinize firm outcomes following the announcement of the OMT. By using syndicated loan data between 2009-2014, the authors conclude that the program mainly induced zombie lending by weakly capitalized banks and subsequently did not reinforce investment overall. This is consistent with Giannetti and Simonov (2013), but contrasts to Ferrando et al. (2019), who find an increase in investment following the OMT announcement.

Empirical research utilizing loan-level matched data also contains examples of studies employing different identification strategies than the aforementioned ones. In their seminal work, Amiti and Weinstein (2018) develop a novel methodology, estimating the firm-time fixed effects model from loan-level data through the weighted least squares (WLS) procedure, to identify idiosyncratic credit supply effects which enable to draw aggregate inferences. They apply this methodology for a sample of listed Japanese firms between 1990-2010, and find that idiosyncratic granular credit supply shocks accounted for around 30-40% of the variation in aggregate investment in Japan. Amador and Nagengast (2016) utilize the same methodology using loan level data collected from the Portuguese credit register for the period between 2005-2013. They find that granular bank shocks explained about 20-40% of Portuguese investment, which is marginally lower than the effect found by Amiti and Weinstein (2018). As argued by Amador and Nagengast (2016), this indicates that firm-specific factors together with economy-wide shocks may have been a slightly bigger influence on aggregate investment in Portugal than in Japan for the given sample periods.

An estimation of the firm-time fixed effects regression model from loan-level data re-

quires to rely on firms that have credit relationships with multiple banks, *multi-bank firms*, instead of firms that borrow from a single bank, *single-bank firms*. In this regard, Degryse et al. (2019) have proposed a methodology that takes into account both single- and multi-bank firms in the identification of credit supply through loan-level matched data. Their methodology is based on clustering firms in industry-location-size-time bins. With this new methodology, they are able to conduct an analysis over the period from 2002 to 2012 for Belgian firms, the majority of which are single-bank firms. As a result, the authors, albeit using a different identification strategy, reach comparable conclusions. Specifically, they reveal that a one standard deviation decrease in credit supply corresponds to about 0.37 percentage points reduction in investment on average. This approach is also implemented by De Jonghe et al. (2019), who use a sample of Belgian firms during the recent financial crisis and find that an average firm cuts investment by 0.85 percentage when the firm's lender experienced an interbank funding shock of around -10%. This is lower than the result found by Cingano et al. (2016), who conduct an analysis for Italian firms using the same source of credit supply shock but implementing the firm-time fixed effects approach instead.

Additionally, and different from most other papers, Berg (2018) contributes to this strand of the literature by implementing a regression discontinuity design, in which he exploits a cutoff rule practised by a major German bank in the decision of whether or not to reject loan applications over the period 2009-2012. By comparing firms above and below the cutoff in a small bandwidth, he finds that firms whose loan applications were rejected cut their investment by 2.4% more than firms whose loan applications were accepted following the credit crunch of 2008.[7]

Overall, the literature finds that, whereas credit supply contractions result in declines in firm investment on average, an increase in credit supply does not always boost investment as it might lead to zombie lending. We next take a closer look at the heterogeneous impacts highlighted by the literature.

The heterogeneous effects of credit supply reductions on firm outcomes may stem from 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 ability of firms to compensate for reduced lending by other forms of financing. While the majority of the papers

[7]See also other loan-level studies investigating the impact of credit supply on investment: Jimenez et al. (2010), Daetz et al. (2017), Gropp et al. (2018), Acharya et al. (2019a), Chakraborty et al. (2019), Lin et al. (2017), Blattner et al. (2019), Fraisse et al. (2019), and De Marco (2019).

investigating heterogeneity in the real effects are unable to disentangle the two channels, an increasing set of studies that have access to loan-level information have been able to assess the strength of both the channels (e.g., Amador and Nagengast, 2016; Cingano et al., 2016; Berg, 2018; De Jonghe et al., 2019).

The empirical literature frequently looks at the effect of firm size in the relation between credit supply and investment. Although small firms are expected to be less able to replace reduced bank lending with alternative sources of finance (Gertler and Gilchrist, 1994), evidence is not always in this direction. On the one hand, a set of studies show that small firms experience a stronger decline in investment than their larger counterparts (Amador and Nagengast, 2016; Cingano et al., 2016; Balduzzi et al., 2018; Berg, 2018; De Jonghe et al., 2019). On the other hand, Campello et al. (2010) do not find any significant differential impact in their investment levels between small and large firms. In addition, some papers in this group have loan-level information, and examine whether banks disproportionally cut lending across size classes. While ample evidence indicates that banks cut lending to small firms more than large firms (Khwaja and Mian, 2008; Amador and Nagengast, 2016; Berg, 2018), there is also some evidence that banks do not selectively lower lending to small firms (Cingano et al., 2016; De Jonghe et al., 2019). Lastly, Morais et al. (2019) show that small Mexican firms benefited more from QE and low foreign monetary policy rates and raised their investment level, suggesting the risk-taking channel of monetary policy.

Besides firm size, some studies also investigate whether the effect of credit supply is unequal in firm age, given particularly that young firms tend to have lower ability to offset credit reductions. Firstly, evidence indicates that banks did not disproportionately cut back on lending to young firms during the global crisis of 2008 (Cingano et al., 2016; De Jonghe et al., 2019). However, evidence on the real effects is mixed. While De Jonghe et al. (2019) show that the investment of young firms in Belgium was not disproportionally affected by the credit supply shock, Cingano et al. (2016) and Balduzzi et al. (2018) do find a larger deteriorating effect on investment for younger Italian firms.

The number of banking relationships may also play an important role for firms, as the higher the number of banking relationships the firm has, the easier the firm switches across banks during a credit crunch. In this regard, Khwaja and Mian (2008) and Amador and Nagengast (2016) show that multi-bank firms face a lower credit shock from their lenders, suggesting that multi-bank firms might compensate for part of their lending by switching to banks that are less affected by adverse credit shocks. In line with this, the results of Degryse

et al. (2019) suggest that the subsequent real effects are smaller for multi-bank firms than for single-bank firms. Taken together, evidence underlines that the exclusion of single-bank firms (e.g. for the purpose of implementing the firm-time fixed effects model) may result in underestimation of the impact of credit supply on investment. Furthermore, given that small and/or young firms tend to have fewer banking relationships, this might be one of the reasons why such firms are affected more by declines in credit supply.

Another question that the literature has sought to answer is whether firms that have 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 considerably larger firms with access to public debt markets and find a negative effect, suggesting that credit reductions tend to affect all types of firms. Moreover, Amiti and Weinstein (2018) utilize a sample of listed firms (i.e. those that have access to equity financing), and show that even these companies are negatively affected by credit declines. These studies suggest that even firms with better access to other markets suffer from credit supply shocks. Nevertheless, evidence indicates that such firms tend to be affected less compared to those with no easy access to other funding sources; that is, investment decline is larger for unrated/unlisted firms (Acharya et al., 2018), those with lower credit rating (Campello et al., 2010; Lemmon and Roberts, 2010), and/or those in less financially developed areas (Cingano et al., 2016), since it is difficult for those firms to issue bonds as well as to raise additional equity. Particularly, firms seem to mainly substitute other forms of debt capital rather than equity financing (e.g., Acharya et al., 2018; Lemmon and Roberts, 2010).

Other ways of insulation for firms against reduced credit supply are to use cash holdings and/or to adjust dividend policy. Regarding the latter, there is some evidence that constrained firms cut dividends as credit rationing unfolds (Campello et al., 2010; Acharya et al., 2019a). With respect to the former, however, evidence is ambiguous. The results of Duchin et al. (2010) and Cingano et al. (2016) suggest that firms use cash reserves to cushion the subsequent negative outcomes of credit supply reductions. In addition, Campello et al. (2010) demonstrate that constrained firms are more likely to build cash stocks (based on the precautionary savings motive) during normal times, and they use some of their cash reserves to cope with a credit crunch. In contrast, Lemmon and Roberts (2010) show that below-investment grade firms were not able to issue long-term debt, although they did not tap into their cash balances. More specifically, Berg (2018) shows that firms with low liquidity raise their cash holding following loan rejections because rejected firms reduce their future prospects of financing availability. Those firms thus significantly cut investment. However,

firms with high liquidity use their cash holdings after a credit shock. This implies that real effects are smaller for such firms.

In sum, ample evidence indicates that small and/or young firms experience larger declines in investment during credit contractions, but evidence on whether the subsequent investment effect mainly emanates from the size of a shock or their inability to substitute for other sources than bank credit is mixed. There is also some evidence showing no significant differential impact between small and large firms during downturns. Multi-bank firms may find it easier to switch across banks during a credit crunch, and thus may more easily sustain investment. Firms with access to other markets also tend to experience more moderate negative outcomes. Moreover, there is evidence showing that, whereas firms cut payouts to buffer negative effects, they take into account a precautionary motive when deciding whether to draw on their cash holdings. Finally, the literature illustrates that, contrary to the negative impact of adverse credit shocks, credit expansions could lead to risk-taking, which in turn could serve small firms.

3.2. Employment

A large set of papers in the literature has sought to quantify the impact on the number of workers along with the heterogeneous effects across firms characteristics (e.g., Chodorow-Reich, 2013; Bentolila et al., 2017; Berton et al., 2018; Popov and Rocholl, 2018; Huber, 2018). Moreover, a growing body of the literature analyses the resulting changes in the composition of the labour force (e.g., Barbosa et al., 2019; Bentolila et al., 2017; Berton et al., 2018) as well as in wages (e.g., Hochfellner et al., 2015; Popov and Rocholl, 2018).

3.2.1. The effect on total employment

As in the case of the empirical literature linking credit supply to investment, some papers seek to pin down the effect of bank credit on employment through macro level data (at the industry or state level) or only through firm-level data, whereas a growing body of the literature has taken advantage of data sets that enable to match banks and firms at the loan level. In fact, some of the studies discussed in Section 3.1 also analyse the resulting employment outcomes (e.g., Peek and Rosengren, 2000; Cingano et al., 2016; Acharya et al., 2018; Berg, 2018).

Regarding evidence at the aggregate level, Dell'Ariccia et al. (2008) present slower employment growth in more financially dependent sectors than less dependent ones during

credit contractions. Peek and Rosengren (2000) show that the deterioration of credit supply abroad results in lower employment growth in the U.S real estate sector. Benmelech et al. (2011) also follow Peek and Rosengren (2000) and exploit the Japanese lending shock to U.S. markets. Different from Peek and Rosengren (2000), they test whether the shock propagates to the sectors other than real estate, and subsequently affects employment outcomes in those sectors. Indeed, the authors find a positive and significant link between the contraction in credit supply by Japanese banks and unemployment in U.S. firms, expanding the scope of the results presented by Peek and Rosengren (2000). In addition, Huber (2018) examines the effect of credit contraction on employment by constructing county-level data. Specifically, the author utilizes information about bank-firm relationships in Germany during the Great Recession, and exploits counties' Commerzbank dependence since Commerzbank cut lending mainly because of its exposure to the U.S. subprime mortgage crisis.[8] He finds that one standard deviation increase in dependence on Commerzbank reduced employment by around 0.8-1.3%.

Similarly, Greenstone et al. (2014) utilize county-level data from the U.S. to quantify the effect of the credit contraction on small firm employment following the 2008 crisis but differentiate themselves from the aforementioned papers in terms of identification. More specifically, they first estimate individual bank-level credit supply effects, and then, construct a county-level measure of credit supply.[9] Subsequently, they find that, even assuming that the entire decline in small business lending was mainly supply-driven, credit supply shocks explain only 5% of the overall drop in small (i.e. those with less than 20 employee) firm employment between 2008 and 2010, suggesting that credit supply has a modest impact on employment.

The literature also provides examples of micro-level studies that deal with the disadvantage of not having matched bank-firm data using alternative approaches (Benmelech et al., 2011, 2019; Hochfellner et al., 2015). In this strand of the literature, two papers use firm-level data, and implement the "long-term debt maturity" approach of Almeida et al. (2011). First, Benmelech et al. (2011) find that firms that needed to refinance their long-term debt during the 2008 crisis partially adjust to reduced credit supply through a contraction in the labour force. Specifically, firms with maturing long-term debt cut employment by on aver-

[8]To proxy for a county's Commerzbank dependence, Huber (2018) aggregates the ratio of firms' banking relationships with Commerzbank in a given county, and also constructs a "distance" instrument, i.e. a distance from a county to the nearest postwar head office of Commerzbank.

[9]This measure is obtained by the product of the banks' estimated individual credit shocks and their predetermined credit market share in a given county.

age 2% more than those without maturing long-term debt. Later, Benmelech et al. (2019) also implement the "long-term debt maturity" approach but focus exclusively on corporate bond maturity. They investigate how firms with corporate bonds maturing during the Great Depression adjust employment levels.[10] This follow-up study also finds a negative but more sizeable effect (a 4-5% larger employment cut by firms with maturing debt vis-à-vis without). As Benmelech et al. (2019) stress, the smaller effect of credit supply on employment during the 2008 crisis can likely be explained by more accurate interventions by policy makers who learned from the Great Depression.

Additionally, Hochfellner et al. (2015) quantify the employment effects of the financial crisis between 2007-2010 in Germany through exploiting the exposure of Landesbanken to the crisis. Their identification rests on two facts. First, Landesbanken are owned by a group of savings banks in their region, and savings banks had to provide capital for their respective Landesbank affected by the U.S. subprime mortgage crisis, thereby experiencing a negative shock (arguably exogenous to local economic conditions) to their capital. Second, German savings banks are only allowed to conduct business inside their geographic regions. For these reasons, the authors are able to compare affected and unaffected firms based on whether their respective regions are negatively influenced by their Landenbanken's trading losses. They find that average employment declined 1.4% more in firms in the affected states.

Some papers in this strand of the literature do not have loan-level data but they are able to match borrowers to their lenders using information about the existence of credit relationships (Popov and Rocholl, 2018; Huber, 2018). For example, by carrying the identification strategy of Hochfellner et al. (2015) one step further, Popov and Rocholl (2018) identify credit constrained firms based on their credit relationships with German savings banks whose Landesbanken were exposed to the mortgage-backed securities (i.e. affected banks). They find that firms affiliated with at least one affected bank reduce employment by 1.5% more than their unaffiliated counterparts, which is very similar to the effect (1.4%) found by Hochfellner et al. (2015). Given that the average annual net migration level was 3.2% between counties within the same state, this effect is rather small, and suggests that the migration of workers from more affected counties to less affected ones may have also contributed the employment fall. Furthermore, Popov and Rocholl (2018) investigate the

[10]The authors justify exclusive reliance on corporate bonds based on two arguments. First, the sample includes larger firms, and corporate bonds were the primary source of external finance for large firms in the 1930s. Second, public debt markets were completely closed during the Great Depression so firms whose corporate bonds matured at that time faced difficulties refinancing them, i.e. they could not easily substitute bonds for credit.

persistency of the effect, and show that the negative impact dissipated by 2011, three years after the initial shock.

In addition to Popov and Rocholl (2018), also Huber (2018) examines the resulting employment outcome at the firm level in Germany during the Great Recession through matched bank-firm data. He finds that, on average, firms fully dependent on Commerzbank cut employment 5.3% more than firms with no Commerzbank relationship. This effect is much larger than the one (1.5%) found by Popov and Rocholl (2018) for German firms during the same period. However, the sample of Popov and Rocholl (2018) is much larger and thus is more representative of the economy. Moreover, Huber (2018) investigates the persistency of the resulting employment effect and shows that there was a significant sizeable difference in employment between dependent- and not-dependent Commerzbank firms even two years after (by 2012) the lending had normalized (by 2010). This is in contrast with Popov and Rocholl (2018), who demonstrate that the effect on employment in Germany was not significant by 2011.

There is also ample evidence provided through loan-level matched bank-firm data. Most studies that have access to loan-level information estimate the effect of bank shocks on loan growth with and without firm-time fixed effects to fend off the endogeneity stories pertaining to unobserved firm characteristics (Chodorow-Reich, 2013; Giannetti and Simonov, 2013; Cingano et al., 2016; Bentolila et al., 2017; Jiménez et al., 2017; Morais et al., 2019). Chodorow-Reich (2013) is the first to use loan-level matched data to pin down the employment effect of the credit crunch during the 2008 crisis. Using U.S. syndicated loan-level data, he constructs a firm-level credit shock for U.S. firms, measured as the weighted average change in lending to all other borrowers by the firm's last pre-crisis syndicate during the crisis.[11] He finds that, after the collapse of Lehman Brothers, a one standard deviation decline in pre-crisis syndicate health translates into about 1-3.2% percentage points reduction in employment growth, which accounts for more than one-third of the employment fall in firms borrowing from less healthy banks from the crisis. This estimated large effect is in contrast with Greenstone et al. (2014), who find a small effect even using a sample of relatively smaller firms. Chodorow-Reich (2013) also looks at the persistency of effect and demonstrates that employment in the U.S. started to recover in 2010 following the 2007-08 crisis even though credit credit had normalized by 2009.

[11] In addition, Chodorow-Reich (2013) instruments this firm-level measure of credit supply using three indicators, namely exposure to Lehman Brothers, exposure to mortgage-backed securities, and a group of variables from banks' balance sheets and income statements.

Following the seminal contribution of Chodorow-Reich (2013), some other papers have also shed light on this issue using loan-level data. Utilizing Spanish loan-level data around the 2008 crisis, Bentolila et al. (2017) test whether firms that had pre-crisis relationships with unhealthy banks (i.e. banks that are bailed out by the Spanish government) experience different employment outcomes than firms that had relationships with healthy banks. The study finds that firms attached to unhealthy banks cut employment 2.8 percentage points more than those attached to healthy lenders. Further, by using syndicated loan data on European firms, Acharya et al. (2018) show that a one standard deviation increase in exposure to the sovereign debt crisis led to 4.7 percentage points lower employment. In addition, Berg (2018) analyses the results of loan applications to a major German bank following the 2008 crisis, and on average finds that the effect of loan rejection on employment is negative but not significant, corroborating the small deteriorating effect found by both Popov and Rocholl (2018) and Hochfellner et al. (2015) for German samples.

Berton et al. (2018) also use loan-level matched bank-firm data and apply the methodology of Greenstone et al. (2014), but rather construct credit supply shocks at the firm level rather than at the county level. They assess the employment outcomes of Italian firms in the Veneto region for the period of 2008 to 2012. They document that the average elasticity of employment to credit supply shock equals to around 0.36. This is about twice the elasticity found by Cingano et al. (2016) (around 0.18), who exploit the interbank market collapse using a sample from all over Italy rather than a sample from a specific region.

Another notable study in this area is conducted by Jiménez et al. (2017), who investigate the effect of time-varying macroprudential policy in credit cycles. Specifically, they study dynamic provisions (introduced in 2000) in Spain over the period 1998-2012 through loan-level data. Dynamic provisions require banks to reserve some of their retained profits as a buffer in good times to compensate for the realized losses in bad times. The authors show that, in good times, available credit to firms were declined immediately after the introduction of dynamic provisioning, but firms could easily switch to less affected lenders. Thus, there was no significant impact on firm employment in good times. However, firms borrowing from banks that had smaller dynamic provision funds when hit by a crisis could not easily replace reduced bank lending, thereby curtailing employment. This suggests that negative shocks to credit supply induce asymmetric real effects in good and bad times.

Additionally, regarding the effect on employment following positive shocks to credit sup-

ply, Morais et al. (2019) show that an increase in credit supply after accommodative foreign monetary policy raised employment in Mexican firms. In contrast, Giannetti and Simonov (2013) and Acharya et al. (2019) illustrate that, respectively, bank bailouts in Japan and the ECB's OMT program caused zombie-lending, and thus did not significantly affect employment.[12]

Taken together, the majority of the literature documents that credit contractions induce constrained firms to reduce employment. Nevertheless, some findings, especially those from U.S. and German samples, and particularly at the macro level, reveal a small and negligible effect. Moreover, as was the case with investment effects, also the findings with respect to the employment effects of positive credit supply shocks are mixed. Next, we focus more on sample aspects, and discuss the heterogeneous impacts on employment across firm characteristics.[13]

Empirical research linking credit supply to employment focuses principally on the effect across firm size. Most studies document that disruptions in credit supply cause higher reduction in employment in smaller firms (Chodorow-Reich, 2013; Duygan-Bump et al., 2015; Berton et al., 2018; Balduzzi et al., 2018; Siemer, 2019). Chodorow-Reich (2013) even finds no significant effect on large firms during the Great Recession. However, Benmelech et al. (2019) use a sample of large firms and find a strong negative effect during the Great Depression, providing evidence of a significant impact on large firms. Comparison of the results of Chodorow-Reich (2013) and Benmelech et al. (2019) implies that financial sector development from the Great Recession to the Great Depression has enhanced the ability of (especially large) firms to replace bank lending with other sources of financing. In contrast, Popov and Rocholl (2018) reveal that the employment effect is less noticeable among small firms than large ones in Germany. They attribute this finding to the lower flexibility of small firms in the firing decisions because of employment protection legislation and closer connections between employees and employers in small firms.

[12]See also other loan-level studies examining the employment effects of credit supply: Jimenez et al. (2010), Daetz et al. (2017) Blattner et al. (2019), Acharya et al. (2019a), and Fraisse et al. (2019).

[13]As discussed previously, two channels may give rise to heterogeneity, namely (i) the bank selectivity in the decision to cut lending to particular types of firms, and (ii) varying ability of firms to replace bank lending with other forms of financing. Regarding the first channel, we have already documented the findings of the literature in the section of credit supply-investment; for instance, the magnitude of the lending shock is on average larger for small firms (e.g., Khwaja and Mian, 2008; Amador and Nagengast, 2016; Berg, 2018), and single-bank firms (e.g., Khwaja and Mian, 2008; Amador and Nagengast, 2016), apart from Cingano et al. (2016) and De Jonghe et al. (2019), who find no differential effect in firm size and age. Therefore, here, we exclusively present evidence on the second channel.

Firm age is also frequently examined as a source of heterogeneity. Ample evidence indicates that credit supply reductions lead to a larger employment fall in younger firms (Berton et al., 2018; Balduzzi et al., 2018; Siemer, 2019). In fact, Siemer (2019) shows that the greater negative impact on small firms is largely driven by young firms, which are often small. This is because young firms do not tend to have established lending relationships, and thus are more likely to be affected by adverse credit conditions. In addition, 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 exploit the U.S. state banking deregulations, initiated between the 1970s and the early 1990s. They first show that the U.S. bank deregulation enabled banks to better detect firms with greater productivity through enhancing monitoring, and hence facilitated labour growth in small manufacturing firms. Then, they demonstrate that younger firms benefited the most from this reallocation effect, since they are more likely to borrow from new banks due to their lack of well-established banking relationships, in line with (Siemer, 2019).

The empirical literature also quantifies the importance of access to other forms of finance as well as cash holdings. Evidence illustrates that decline in employment is more noticeable among unlisted/unrated firms (Acharya et al., 2018), private firms (Hochfellner et al., 2015)[14], and firms without access to bond markets (Chodorow-Reich, 2013; Acharya et al., 2018). In addition, Berg (2018) shows that firms with low liquidity tend to hoard cash in anticipation of worse times following a credit crunch, and thus are more likely to reduce employment.

Overall, empirical research shows that the link between firm level employment and credit supply might be affected by firm characteristics (coupled with labour legislations). For example, higher firing costs imposed by the German legislation may have led to a lower level of layoffs in small firms than in large firms. Aside from this, small and/or young firms along with those with no access to public debt markets are more likely to be affected by adverse credit conditions. Some evidence also indicates that the role of firm size mainly stems from firm age. Finally, there is evidence that firms with low liquidity are more prone to cut employment.

[14]Public firms might have a better access to capital markets than privately owned firms do.

3.2.2. The effect on the labour force composition and wages

A growing body of research investigates how contractual dualism affects labour adjustments when firms experience an impairment in credit availability. Evidence indicates that, following credit contractions, employees with temporary contracts are more likely to be laid off than their counterparts with permanent contracts in Spain (Bentolila et al., 2017), Italy (Caggese and Cuñat, 2008; Berton et al., 2018), and Germany (Hochfellner et al., 2015). This asymmetric response to reduced credit supply could mainly be attributed to the non-renewal of temporary contracts and higher costs of firing employees with permanent contracts (Berton et al., 2018).

Additionally, evidence illustrates that less educated (Hochfellner et al., 2015; Berton et al., 2018), and/or younger (Berton et al., 2018) workers are more likely to be laid off when credit is scarce. Notwithstanding this, Berton et al. (2018) reveal that the negative effect on less educated and/or younger employees is mainly driven by the fact that they tend to have temporary contracts. There is also evidence that constrained firms dismiss foreign workers more than national ones (Berton et al., 2018).

Previous research has also shown that female workers are laid off more than male workers. Berton et al. (2018), for example, show that female workers account for 60% of the total decline in firm employment due to reduced bank lending in an Italian region over the period of 2008 to 2012. Popov and Zaharia (2019) complement this evidence by showing that increase in access to credit during the interstate branch deregulation of the U.S. banking sector conduced to a supply-driven rise in the female labour and diminished the gender gap by around 7.5-19 percentage points between 1970 and 2000.

The literature looks at the role that *skill* plays in the employment decisions. Hochfellner et al. (2015) show that unskilled workers are more likely to lose their jobs and to experience wage cuts than their skilled counterparts do in times of crises. Distinct from the aforementioned studies analysing the effect of credit supply reductions during bad times, Barbosa et al. (2019) exploit a negative shock to credit supply in non-crisis times, proxied by the changes in defined-benefit pension plans for Portuguese banks in 2005. They demonstrate that, for better opportunities in good times, skilled workers are more likely to leave and less likely to join constrained firms, suggesting that credit supply has important allocation effects on the labour composition.

Caggese et al. (2019) also examine the allocation effects of financing constraints through

utilizing employee-level data from Sweden over the period 1990-2010. They show that, because constrained firms attach more importance to current cash flows than to future ones, credit frictions induce them to sub-optimally lay off workers with shorter-tenure (i.e. those receiving less severance payments in the case of dismissal), but with higher expected future productivity. Their results thus suggest that credit constraints lead to inefficient labour allocation.

Firms also might seek to reduce labour costs in response to cuts in credit supply through reducing wages. Popov and Rocholl (2018), for example, show that German firms reacted to the global crisis mainly by wage cuts rather than layoffs, suggesting the higher firing costs imposed by the German labour legislation. Conversely, Hochfellner et al. (2015) show that income losses in private German firms in the affected states primarily emanated from employment spells as well as temporary exits, and retained employees did not experience a sizeable wage cut during the crisis. They demonstrate that constrained German firms de facto curtailed the number of their workforce during the recent financial crisis. In line with this, Fabiani et al. (2015) and Bentolila et al. (2017) utilize a broad survey of European firms and data from the Spanish credit registry, respectively, and show that the labour cost reduction strategy primarily works through reducing the number of employees rather than cutting down on wages. Just as such results suggest the existence of downward wage rigidity imposed by labour market regulations, it might also imply the higher share of temporary contracts used in financially constrained firms, which in turn makes it easier for them to dismiss workers.

Overall, a small group of studies show that firms' labour adjustment concentrates upon less educated, unskilled, shorter-tenured, younger, foreign and/or female employees as well as the ones with temporary contracts. There is also evidence highlighting a labour cost reduction strategy through adjusting wages instead of the number of employees. Nevertheless, existing evidence regarding the compositional effects is too little to draw inferences applicable to various conditions. Further analysis in these aspects may shed light on the bearers of the burden caused by credit contractions but depends crucially on the availability of employee-level data.

3.3. Other real indicators

As discussed earlier, empirical research examining the real effects of changes in credit supply focuses mainly on investment and employment outcomes. Nevertheless, the literature also

studies other firm outcomes such as market valuation, exports, default rate along with heterogeneity across firm characteristics such as size, age and ownership.

Empirical analysis reaches a consensus on the worsening valuation of firms facing less available credit. Gan (2007) shows that the liquidity shock to Japanese banks explains one-fourth of the decline in the stock market valuation of manufacturing firms borrowing from affected banks. In addition, Chava and Purnanandam (2011) exploit the Russian crisis of 1998 as an exogenous shock to the U.S. banking system, and find that bank-dependent firms suffer larger declines in their valuation following the reduction in credit supply.

The effect of reduced credit supply on export growth has also been of interest in the literature. Amiti and Weinstein (2011) exploit the Japanese financial crisis between 1990-2010 by matching exporters with the main financial institution supplying trade finance to them, and reveal that worsening bank health significantly reduces the export level of client firms as the trade finance channel deteriorates. Similarly, Paravisini et al. (2014) examine the sensitivity of exports to credit supply during the 2008 financial crisis. Utilizing loan-level matched data from Peru, they link contractions in credit supply to reductions in exports mainly through increasing the cost of production. In a later study, Paravisini et al. (2017) show that the effect of credit supply on Peruvian exports is much higher to destinations in which banks specialize. In addition, Zia (2008) examines the impact of reduced subsidized credit on firm-level export in Pakistan through loan-level data. The study reveals a negative subsequent impact, in line with Amiti and Weinstein (2011) and Paravisini et al. (2014). Furthermore, he shows that the negative effect on both working capital loans and exports is substantially larger when firms are privately owned, small, single-bank, and/or part of a group network.

Khwaja and Mian (2008) test whether changes in credit supply affect firms' default rate, and whether the resulting effect varies between small and large firms. The authors show that the default probability of a firm increases by 13.7 basis points on average in response to a one percent reduction in liquidity of its bank. Furthermore, large borrowers can totally offset the impact of credit shrinkage and thus exhibit no increase in their default rate, whereas small ones cannot; that is, they are significantly more prone to default. Additionally, Schnabl (2012) examines whether the 1998 Russian debt default had an impact on loan default and survival rates of firms in Peru. The author shows that higher levels of exposure to the Russian crisis raised loan default and decreased firm survival. He also finds that the real effects are larger for small and young firms.

Additionally, there is empirical evidence linking reduced credit supply to diminishing firm sales growth (Acharya et al., 2018; Gropp et al., 2018), profitability (e.g. change in operating income/revenue or return on assets) (Chava and Purnanandam, 2011; Ongena et al., 2015; Degryse et al., 2019), and more broadly total asset growth (Ongena et al., 2015; Berg, 2018; Gropp et al., 2018; Degryse et al., 2019; De Jonghe et al., 2019).

Finally, the literature documents evidence with respect to positive credit supply shocks. On the one hand, evidence indicates that an increase in credit supply might not result in positive firm outcomes. Giannetti and Simonov (2013) illustrate that the bank bailouts following the Japanese banking crisis did not yield a significant increase in the valuation of a firm if its lender was believed to remain under-capitalized. Similarly, Lin et al. (2017) show that the U.S. bailout program in the wake of the recent financial crisis, namely the Troubled Asset Relief Program, led to an adverse announcement effect for firms borrowing from the beneficiary banks, mainly because the program was accompanied with regulatory cost and thus caused negative effects. Acharya et al. (2019) find no significant effect on return on assets after the announcement of the OMT program. On the other hand, some evidence shows that capital injections might indeed help the real economy. Contrary to Acharya et al. (2019), Ferrando et al. (2019) indicate evidence that SMEs raised their profitability following the OMT program. In addition, Morais et al. (2019) show that low foreign monetary policy induced Mexican firms to increase their assets but also their loan default, and due to increasing bank risk-taking, their results are accordingly stronger for smaller firms.

In summary, empirical investigation confirms that credit contractions exacerbate various performance indicators besides employment and investment for the average firm, such as firm valuation, export growth, default, sales growth, profitability, and more widely total assets. There is also evidence for the disproportionate impact on small, young, privately owned, and single-bank firms. However, the effects appear to mainly relate to negative credit supply shocks and not to positive ones. Next, we focus on the recent methodological breakthroughs that allowed the literature to estimate the causal effect of credit supply on firm outcomes.

4. Identification strategies

Given that the aim is to gauge the real effects of credit supply, an ideal empirical strategy needs to follow two steps. It first needs to show how bank shocks affect lending in equilibrium. In other words, the first step should test whether banks, when hit by negative (or positive)

shocks, transmit those shocks to borrowers. After that, the second step pins down the resulting real firm outcomes such as investment and employment.

4.1. Identification of credit supply at the bank-firm level (Loan-growth equation)

The empirical literature utilizing loan-level data has employed a wide range of identification strategies in an attempt to isolate the effect of bank shocks on the equilibrium lending outcome. The reason for the variety of methodologies mainly stems from the endogeneity concerns that are not easy to address. To explain the main challenges to identification, assume that one runs the following loan-growth regression:

$$\Delta Lending_{bf} = \alpha + \beta \Delta Credit\ Supply_b + \Phi X_b + \Pi X_f + error\ term \tag{1}$$

In the equation above, the dependent variable, $\Delta Lending_{bf}$, denotes the change in credit committed from bank b to firm f. The independent variable, $\Delta CreditSupply_b$, is the change in available credit supplied by bank b (e.g. events that induce shocks to bank health). X_b and X_f are bank and firm controls, respectively. The coefficient of interest is β. Financing frictions may lead banks to pass on shocks to their borrowers, inducing $\beta \geqslant 0$. However, β could be biased for two main reasons. First, the events that affect bank credit supply are also likely to influence firm credit demand. During a financial crisis, for example, just as banks cut lending, firms may also demand less credit due to reducing working capital needs and worsening investment prospects (Bernanke and Gertler, 1995). Therefore, the demand for credit should be controlled for in an ideal empirical setting to isolate credit supply.

The second threat to identification is the non-random assignment of borrowing firms to banks. Such endogenous selection could arise between the firm's performance and lender's financial health. For instance, financially constrained firms (i.e. poorly performing firms) might prefer to borrow from weak banks (i.e. those that are more affected by adverse credit shocks) in order to avoid disciplining and monitoring (Gan, 2007). If this type of selection generates a positive correlation between the credit supply and demand shocks, the conventional OLS estimation would lead to overestimation of β.[15] For this reason, the selection issue should be addressed to attain identification.

[15]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 detail.

On the whole, an empirical strategy without addressing these identification concerns leads to biased estimations of bank credit supply. To ameliorate such concerns, one needs to deal with (both observable and unobservable) firm and bank characteristics. Just as firm-specific aspects could determine credit demand, both firm- and bank-specific factors could form the selection of bank relationships. We next present how the recent empirical literature tackles these identification challenges.

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

Gan (2007) and Khwaja and Mian (2008) identify credit supply shocks using firm fixed effects at the loan-level. They both exploit a natural experiment. Particularly, 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, since Japanese banks, prior to the collapse, had high-levels of exposure to the real estate market through both lending to the land market as well as through land holding. Similarly, Khwaja and Mian (2008) examine the effects of unforeseen nuclear tests of Pakistan in 1998. This induced a bank liquidity shortage, which reduced the available credit to firms in Pakistan.[16]

Gan (2007) and Khwaja and Mian (2008) mainly estimate the regression in Equation 2.[17] The dependent variable that they estimate, $\Delta Lending_{bf}$, 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, namely bank exposure to the real estate industry for Gan (2007), and change in

[16] In 1992, the Pakistani government introduced dollar deposit accounts, which had become very popular and accounted for 43.5% of total deposits by 1998. However, the banking reform in Pakistan did not allow banks to retain dollars, rather, obliged banks to hand over dollars to the central bank of Pakistan, in return for rupees at the prevailing exchange rate. When depositors claimed their dollars back, banks were only able to get dollars from the central bank in return for rupees at the initial exchange rate. This created an exchange rate risk, which ultimately kicked in when the International Monetary Fund stopped supporting exchange rate liquidity as an economic sanction after the nuclear tests of Pakistan in 1998. Then, the Pakistani government froze dollar deposit accounts, and thus rendered dollar withdrawal disadvantageous. Despite the disadvantageous exchange rate, dollar deposit holders demanded their money, and consequently caused a shock to bank liquidity (Khwaja and Mian, 2008).

[17] In fact, Gan (2007) conducts 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.

deposit growth for Khwaja and Mian (2008). Finally, α_f are firm fixed effects.[18]

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

The use of firm fixed effects in a first differenced data set at the loan level induces a within-firm comparison across banks, i.e. analysing how different banks with varying exposure to a given shock adjust their lending towards the *same* firm. This approach overcomes the challenges to identification. First, the within-firm comparison rules out the concern over the endogenous selection of bank relationships. Second, as firm fixed effects control for both observable and unobservable firm-specific factors, this approach enables to control for firm credit demand.

A within-firm comparison mitigates the identification concerns to a reasonable extent but nonetheless it comes with some limitations. First, this approach is only applicable to samples where firms borrow from multiple banks. Second, the within-firm specification fails to capture credit flows from new banking relationships, and hence might not reflect the aggregate lending outcomes in equilibrium. Finally, this methodology assumes that a firm has the same credit demand across its lenders. As an illustration, 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 be correlated with the Bank A's shock. In this regard, Gan (2007) argues that bank-specific credit demand shocks are not likely to exist in her study because her sample only contains manufacturing firms, and that Japanese banks are considerably large and thus are not expected to be specialized. Furthermore, Khwaja and Mian (2008) interacts loan types with firm fixed effects to ensure that loan type does not drive their results.

4.1.2. Further Methodological Advances

Amiti and Weinstein (2018) and Degryse et al. (2019) have recently proposed two alternative methodologies based on Khwaja and Mian (2008). To begin with, Amiti and Weinstein

[18] In a panel data set, firm-time fixed effects are used. As Gan (2007) and Khwaja and Mian (2008) collapse their data into single pre-and post- periods, they both use firm fixed effects instead of firm-time fixed effects in their main regressions. Khwaja and Mian (2008) also implement firm-time fixed effects in a panel data set without collapsing the data.

(2018) first show that empirical models based on Khwaja and Mian (2008) can be nested in the model expressed in Equation 3.

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

where α_{ft} are firm-time fixed effects that control for time-varying firm credit demand, and β_{bt} are bank-time fixed effects that capture time-varying credit supply shocks. As we previously indicated, the identifying assumption of this model is that a firm has the same credit demand shock across its lenders.

Amiti and Weinstein (2018) mainly question whether idiosyncratic credit supply shocks obtained by Equation 3 match the macro-level. Particularly, they investigate to what extent these individual shocks explain aggregate investment. They show that the WLS estimation of Equation 3 with percentage growth rates (i.e. $\Delta Lending = (L_{bft} - L_{fb,t-1})/L_{fb,t-1}$) substantially captures aggregate lending behaviour (through capturing new bank-firm relationships), whereas the WLS estimation with log growth rates (i.e. $\Delta Lending = ln(L_{bft}/L_{fb,t-1})$) produce estimates that poorly explain economy-wide lending.

Degryse et al. (2019) address another limitation of the model in Equation 3, i.e. within-firm comparison at the loan level is not applicable to samples in which firms have only one lender. This drawback is quite significant as in many advanced economies, the majority of firms borrow from only one bank. In this regard, they propose to use industry-location-size-time fixed effects (ILST), instead of firm-time fixed effects, to control for credit demand in Equation 3.[19] This approach assumes that firms with similar size in the same industry and location at a given time are equal in their credit demand. We also note that the regression models proposed by Amiti and Weinstein (2018) and Degryse et al. (2019) can be estimated not only in times of crisis but also in normal times when credit supply shocks can also occur.[20]

4.1.3. The impact of credit supply on firm outcomes

The change in available credit in the market tends to affect firms under capital imperfections. If, for instance, the bank-firm level analysis reveals a credit crunch, then firms that

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

[20] Both Amiti and Weinstein (2018) and Degryse et al. (2019) indicate a negative effect of credit contraction not only in times of crisis but also during normal times.

face difficulties raising their funds are expected to indicate a deterioration in their outcomes.

The vast majority of studies that use loan-level data, following the analysis at the bank-firm level (i.e. the estimation of the loan-growth equation), gauge the impact of credit supply shocks on firm outcomes mainly by estimating the following regression in Equation 4.

$$\Delta Y_f = \alpha + \rho \Delta Credit\ Supply_f + \Psi X_f + error\ term \tag{4}$$

where ΔY_f denotes the change in outcome variable Y of firm f, and $\Delta Credit\ Supply_f$ is the change in firm f's exposure to the credit supply shock aggregated by each of its lenders' exposure to the credit shock.

We should note that the papers construct the firm-level credit supply shock variable depending on how they identify credit supply at the bank-firm level analysis. 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 the time-varying credit supply shock at the firm-level by aggregating the fitted values of bank-time fixed effects (i.e. bank credit supply shocks) for each firm from estimation of the regression in Equation 3, weighted by each lender's share in the firm's borrowing portfolio.[21] Differently, Gan (2007) uses the real estate exposure of the firm's top lender as a proxy for firm-level credit shock.

On the one hand, the estimation is now conducted at firm-level (i.e. cross sectional) rather than at the loan-level. Thus, the specification above cannot include firm fixed effects, since the use of firm fixed effects would subsume $\Delta Credit\ Supply_f$. On the other hand, inability to use firm fixed effects leads this specification to suffer from endogeneity because firm credit demand stemming from firm characteristics tends to confound the results.

Given that estimation of the regression in Equation 4 might be biased, the empirical literature mainly posits the identification through showing the results from the estimations of the loan growth regressions in Equation 1 (the conventional OLS regression) and Equation 2 or 3 (fixed effects regressions) (e.g., Gan, 2007; Khwaja and Mian, 2008; Chodorow-Reich, 2013; Bentolila et al., 2017; Cingano et al., 2016; Jiménez et al., 2017). If the OLS and fixed effects regressions yield similar results in terms of magnitude and significance, the papers

[21] As we have previously mentioned, in Equation 3, Amiti and Weinstein (2018) use firm-time fixed effects to control for credit demand, whereas Degryse et al. (2019) implement industry-location-size-time fixed effects to control for demand effects.

argue that unobservable firm-specific factors are not important determinants of credit demand. Therefore, the estimation of the regression in Equation 4 is less likely to suffer from the correlation between credit demand and credit supply.

We also note, for multi-bank samples, that the loan-level analysis also helps correct the potential endogeneity at the firm-level analysis. This can be done in two ways. One of them is to correct for the potential bias in the cross sectional model in Equation 4 by including the estimates of firm(-time) fixed effects ($\hat{\alpha_f}$) from Equation 2 (e.g., Cingano et al., 2016). As the firm fixed effects, α_f, capture firm specific factors in Equation 2, the inclusion of $\hat{\alpha_f}$ in the cross sectional model (4) ensures that unobservable firm characteristics which might affect credit demand do not bias the estimates. The second approach by Jimenez et al. (2010) 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 4. However, this approach of Jimenez et al. (2010) can be applied only for firm-level loan outcomes.

In addition, a set of papers in this strand of the literature (e.g., Chodorow-Reich, 2013; Greenstone et al., 2014; Cingano et al., 2016; Bentolila et al., 2017; Berton et al., 2018) use IV to gauge the sensitivity of firm outcomes to the availability of credit. They both estimate the following two-stage least squares (2SLS) regression at the firm level:

$$\begin{aligned} \Delta Y_f &= \theta + \eta \Delta Lending_f + \Pi X_f + error\ term \\ \Delta Lending_f &= \alpha + \beta \Delta Credit\ Supply_f + \Phi X_f + error\ term \end{aligned} \tag{5}$$

In the model above, the change in bank credit supply acts as an instrument for committed credit. While β measures the impact of a credit supply shock on a firm's loan growth, η captures the effect of committed credit on firm outcomes. Thus, $\beta \times \eta$ equals ρ in Equation 4.

One might question the exogeneity of the instrument as the exclusion restriction (i.e. the events that induce a variation in bank credit supply alter firm outcomes only through credit) might not be satisfied. If, for example, banks that are more affected by negative shocks could charge higher interest rates, the exclusion restriction would not be valid and the results would be overestimated (Bentolila et al., 2017).[22] For this reason, results from

[22]The literature also aims to quantify the effect of bank shocks on the price of loans besides the quantity of loans. A set of studies show that banks that are more affected by adverse shocks charge higher interest

the IV model should be interpreted cautiously.

With the exception of this group of studies, Greenstone et al. (2014) do not use loan-level data, yet address the identification concerns in a similar way. Using bank-county level data, they exploit variation in county-level small firm lending in the United States. Their identification strategy thus focuses on within-county comparison through using county fixed effects (instead of firm-time fixed effects) in Equation 3 in order to control for local credit demand. They first estimate bank credit supply shocks, and then construct predicted county-level supply shocks, using interactions of the estimated credit supply shocks and banks' pre-crisis county market share (i.e. the extent of counties' exposure to banks).[23] After that, they regress county-level employment on predicted county-level credit supply shocks together with state-year fixed effects. In the same vein to Degryse et al. (2019) and Amiti and Weinstein (2018), the methodology of Greenstone et al. (2014) capture the credit supply effects in both good and bad times.[24]

In contrast to the vast majority of papers, Berg (2018) implements a regression discontinuity design and exploits a cutoff rule practised by a major German bank. According to this rule, the bank accepts a loan application if the firm's credit rating is above the cutoff. However, a loan application from a firm whose rating below the cutoff is thoroughly reviewed by the bank before the final decision. The (fuzzy) regression discontinuity design compares firms around the threshold in a small bandwidth. The rationale is that firms just above and just below the cutoff have similar characteristics (including credit demand) based on their close credit ratings, except their probability of getting a loan. Thus, comparison of firms around the rating threshold mitigates the endogeneity concerns. This method contains a two-stage regression, where firm credit ratings, which are regarded as exogenous, are instrumented for loan acceptance (or loan change) in the first stage, and employment is regressed on the fitted values of loan acceptance (or loan change) in the second stage. The empirical design of Berg (2018) constitutes an alternative to the previous methodologies in order to isolate causality.

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rates (e.g., Chodorow-Reich, 2013; Acharya et al., 2018; Santos, 2010), whereas some papers do not find a significant differential impact (e.g., Cingano et al., 2016; Bentolila et al., 2017; Khwaja and Mian, 2008) in line with Stiglitz and Weiss (1981), who argue that banks tend to adjust loan volumes rather than rates to ameliorate the adverse selection concerns.

[23]For each year, Greenstone et al. (2014) calculate $Credit\ supply_{county} = \sum_b bank\ base\ period\ market\ share_{county,b} \times \hat{\beta}_b$, where $\hat{\beta}_b$ are the estimated bank credit supply shocks from the regression equation: $\Delta Lending_{b,county} = \alpha_{county\times} + \beta_b + error\ term.$

[24]Contrary to Amiti and Weinstein (2018) and Degryse et al. (2019), Greenstone et al. (2014) find a significant differential impact of credit supply between crisis and tranquil times.

Overall, loan-level studies are better able to address the challenges in the identification of credit supply than those that do not use loan-level data. Nevertheless, the identification strategies implemented by the prior literature using only firm or aggregate level data can serve as models for future studies that do not employ loan-level matched data. For example, future papers using firm-level data could utilize the "long-term debt maturity" approach of Almeida et al. (2011). The rationale of using long-term debt maturity as a proxy for exposure to credit supply shock is that firms' long debt structures are formed in several years so long-term debt renegotiations do not hinge on firm performance right after a credit crunch. Alternatively, such future papers could also use lagged measures of external financial dependence such as cash reserves and excess cash, similar to Duchin et al. (2010), who measure these variables one to four year prior to the onset of the 2008 crisis in order to ameliorate the concern that those proxies are related to firm investment opportunities. Finally, further empirical studies with no loan-level information could conduct aggregate level analysis. For example, they could implement a methodology similar to that of Greenstone et al. (2014), who conduct within-county comparison instead of within-firm variation through disaggregating bank-level data. In line with Dell'Ariccia et al. (2008), those with industry-level data could also use Rajan and Zingales (1998)'s industry level measure of external financial dependence (i.e. the proportion of investment financed through external funds) to control for industry- and country-specific factors.

5. Future Directions

Although the vast majority of the studies we reviewed agree that changes in credit supply affect firm outcomes, a number of questions on the transmission channel(s), specific effects, and identification strategies remain. In this section, we first discuss general gaps that emerge from our synthesis of the empirical evidence, and then discuss methodological gaps that future research could aim to fill.

5.1. Gaps Emerging from the Existing Evidence

Do firms reduce investment optimally?: Empirical research generally indicates that borrowing-constrained firms cut investment. Given that the literature also finds adverse effects on numerous other firm outcomes, the overall suggestion is 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 (and when) the well-documented reduction in investment is welfare-reducing or part of a value-maximizing strategy.

The impact of employment protection legislations on labour outcomes: The empirical literature documents within- and between-country differences in labour cost adjustments. For example, evidence shows that layoffs were noticeable among Spanish firms following the 2007-08 crisis (Bentolila et al., 2017), whereas German firms reacted to the global crisis mainly by reducing wages, with the effect being driven primarily by small companies (Popov and 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. Although the literature often implicitly documents that legislation matters, the topic remains generally understudied.[25] Future research could therefore scrutinize the role of employment legislation for firms' response to credit supply shocks; ideally using employee-level data.

The effect of credit supply on labour allocation: Another interesting and open question is how firms' responses to credit supply shocks affects wage inequality and thus the (in)efficient allocation of human capital. Existing evidence on labour allocation is scarce (e.g., Berton et al., 2018; Bai et al., 2018; Barbosa et al., 2019; Moser et al., 2018), and leaves plenty of room for additional investigations.

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.

The role of corporate governance in the transmission of bank credit supply shocks to firms: The existing literature typically considers heterogeneous effects of credit supply across firm characteristics such as size, age, and rating. Instead, it does typically not pay much atten-

[25] One of the few exceptions is Laeven et al. (2018), although they examine how employment protection legislation in Spain affected firm growth rather than labour outcomes during the financial crisis.

tion 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 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 (Diamond, 1991; Bolton and Freixas, 2000). Second, better governed firms are more transparent, which alleviates information asymmetry between borrowers and lenders (see Shleifer and Vishny (1997) for a survey of this literature) and may help firms to switch across 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 do indeed perform better during the financial crisis. To comprehensively study whether good corporate governance protects firms against the negative effects of reduced credit supply, however, loan-level data would need to be matched with borrower-specific governance data.

Disentangling the firm's ability to absorb credit shocks from its exposure to the shock: Our survey of the literature has revealed that only some of the studies examining heterogeneity analyse 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 (Khwaja and Mian, 2008; Amador and Nagengast, 2016; Cingano et al., 2016; Berg, 2018; De Jonghe et al., 2019). The question (and corresponding empirical challenge) is amplified by mixed evidence on whether the level of a credit supply shock is higher for small firms during a credit crunch (Khwaja and Mian, 2008; Amador and Nagengast, 2016; Berg, 2018) or not (Cingano et al., 2016; De Jonghe et al., 2019). On the whole, future research could help us better to disentangle and better understand the two channels.

The role of other types of financing than bank loans: 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 et al., 2016), whereas others find that cash reserves play a non-linear 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) and sometimes not (e.g., Amiti and 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.

The effect of bank specialization: Empirical studies employing the firm-time fixed effects

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

The role of banks' private information about borrowers on lending: 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 (2019) has recently developed an empirical model to isolate private borrower information from common borrower information, and found 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 U.S. 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, e.g., by using granular data from credit registers. This would not only serve to corroborate and generalize the results of Darmouni (2019), it would also contribute to a more complete picture of the role of private information in bank-firm relationships.

The persistence of the real effects of credit supply: The question of whether the real effects of credit supply changes are persistent also remains largely unanswered. Chodorow-Reich (2013) demonstrates that employment in the U.S. started to recover in 2010 following the 2007-08 crisis even though credit had normalized by 2009. In addition, Popov and Rocholl (2018) illustrate that, in Germany, the resulting employment effect started to dissipate by 2011, i.e. three years after the credit crunch. In contrast, Huber (2018) shows that the subsequent effect on employment in affected German firms and counties persisted until 2012. Given this mixed evidence, future research could further explore the mechanisms that repress hiring.

More evidence on the real effects of credit supply during tranquil times: A large fraction of the literature focuses on crises times (i.e. on one-off exogenous events). The methodologies proposed by Amiti and Weinstein (2018), Degryse et al. (2019), and Greenstone et al. (2014), however, also enable researchers to estimate credit supply effects over the entire business cycle. Although Amiti and Weinstein (2018) as well as Degryse et al. (2019) find no significant difference between crisis and normal times, Greenstone et al. (2014) together with Jiménez

et al. (2017) show that the asymmetric effects of reduced credit supply do exist in some cases. 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 changes in credit supply than firms during normal times. Such analyses over longer periods would also provide a more suitable environment for policy recommendations.

5.2. Methodological Gaps

We document that the empirical literature has employed a variety of methodologies to identify the real effects of changes in credit supply. Starting from cross-sectional regressions using macro-level data (e.g., Peek and Rosengren, 2000; Dell'Ariccia et al., 2008), it has evolved to more advanced strategies using matched loan-level data. This more granular data has been used, for example, to estimate loan growth-equations with OLS and/or WLS and firm-time (Khwaja and Mian, 2008; Amiti and Weinstein, 2018) or firm-cluster fixed effects (Degryse et al., 2019), as well as in the context of a regression discontinuity design (Berg, 2018).

While these more advanced strategies improve on endogeneity concerns plaguing the earlier studies, a number of methodological shortcomings remain. Using firm-time fixed effects to control for credit demand, for example, as the majority of papers since Khwaja and Mian (2008) do, is not possible for single-bank firms. The approach also fails to capture aggregate consequences, and relies crucially on the assumption that a firm's demand is uniform across its lenders; which is questionable when banks specialize in loan types or invest in relationships with their borrowers. Although Amiti and Weinstein (2018) are able to identify aggregate lending outcomes using WLS and firm-time fixed effects, their approach remains constrained to single-bank firms as well. Degryse et al. (2019), instead, propose to use ILST fixed effects to identify credit supply for both multi- and single-bank firms, but require an arguably stronger identifying assumption: namely that firms of a similar size in the same industry and location are equal in their credit demand. What all these approaches have in common is that they identify credit supply shocks based on a numeraire. In contrast, a regression discontinuity design circumvents many of these shortcomings and provides high internal but (potentially) limited external validity.

Future research could thus (i) aim to improve the firm-time fixed effects specification in the spirit of Amiti and Weinstein (2018), (ii) build on Degryse et al. (2019) and test the use of ILST fixed effects outside of Belgium, and most ambitiously, (iii) propose novel ways of controlling for credit demand when firm-time fixed effects cannot be used, i.e. including for

single-bank firms.

6. 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.

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Appendix

Table A1: Selected papers on the real firm effects of credit supply

Real Outcome	Data	Sample	Papers
Investment	State Level	US	Peek and Rosengren (2000)
	Sector Level	Worldwide	Dell'Ariccia et al. (2008)
	Firm Level	US	Duchin et al. (2010), Lemmon and Roberts (2010), Almeida et al. (2011), Chava and Purnanandam (2011)
		Asia, EU, US	Campello et al. (2010)
	Matched Bank-Firm Level	Italy	Balduzzi et al. (2018)
		Japan	Hosono et al. (2016)
		US	Edgerton (2012)
		EU	Ferrando et al. (2019)
	Matched Bank-Firm Loan Level	Japan	Giannetti and Simonov (2013), Gan (2007), Amiti and Weinstein (2018)
		Italy	Cingano et al. (2016)
		Mexico	Morais et al. (2019)
		EU	Daetz et al. (2017), Gropp et al. (2018), Acharya et al. (2018), Acharya et al. (2019), Acharya et al. (2019a), De Marco (2019)
		Portugal	Amador and Nagengast (2016), Blattner et al. (2019)
		Belgium	Degryse et al. (2019), De Jonghe et al. (2019)
		Germany	Berg (2018)
		France	Fraisse et al. (2019)
		Spain	Jimenez et al. (2010)
		US	Lin et al. (2017), Chakraborty et al. (2019)
Total Employment	State Level	US	Peek and Rosengren (2000), Benmelech et al. (2011)
	Sector Level	Worldwide	Dell'Ariccia et al. (2008)
	County level	US	Greenstone et al. (2014)
		Germany	Huber (2018)
	Firm Level	US	Benmelech et al. (2011), Duygan-Bump et al. (2015), Bai et al. (2018), Benmelech et al. (2019), Siemer (2019)
		Germany	Hochfellner et al. (2015)
	Matched Bank-Firm Level	Italy	Balduzzi et al. (2018)
		Germany	Popov and Rocholl (2018), Huber (2018)
	Matched Bank-Firm Loan Level	Japan	Giannetti and Simonov (2013)
		Italy	Cingano et al. (2016), Berton et al. (2018)
		Mexico	Morais et al. (2019)
		EU	Daetz et al. (2017), Acharya et al. (2018), Acharya et al. (2019), Acharya et al. (2019a)
		Portugal	Barbosa et al. (2019), Blattner et al. (2019)
		Germany	Berg (2018)
		France	Fraisse et al. (2019)
		Spain	Jimenez et al. (2010), Bentolila et al. (2017), Jiménez et al. (2017)
		US	Chodorow-Reich (2013)

Table A1 continues...

Wages	Firm Level	EU	Fabiani et al. (2015)
	Matched Bank-Firm Level	Germany	Popov and Rocholl (2018)
	Matched Bank-Firm Loan Level	Spain	Bentolila et al. (2017)
	Firm and Employee Level	Germany	Hochfellner et al. (2015)
	Matched Bank-Firm Loan and Employee Level	Portugal	Barbosa et al. (2019)
Labour Composition	Firm Level	Italy	Caggese and Cuñat (2008)
		EU	Fabiani et al. (2015)
	Matched Bank-Firm Loan Level	Spain	Bentolila et al. (2017)
	Firm and Employee Level	US	Popov and Zaharia (2019)
		Germany	Hochfellner et al. (2015)
		Sweden	Caggese et al. (2019)
	Matched Bank-Firm Loan and Employee Level	Italy	Berton et al. (2018)
		Portugal	Barbosa et al. (2019)
Stock Market Valuation	Firm Level	US	Chava and Purnanandam (2011)
	Matched Bank-Firm Loan Level	US	Lin et al. (2017)
		Japan	Gan (2007), Giannetti and Simonov (2013)
Export Growth	Matched Bank-Firm Level	Japan	Amiti and Weinstein (2011)
	Matched Bank-Firm Loan Level	Pakistan	Zia (2008)
		Peru	Paravisini et al. (2014), Paravisini et al. (2017)
Default Rate	Matched Bank-Firm Loan Level	Pakistan	Khwaja and Mian (2008)
		Peru	Schnabl (2012)
Profitability	Firm Level	US	Chava and Purnanandam (2011)
	Matched Bank-Firm Level	Eastern EU,Turkey	Ongena et al. (2015)
		EU	Ferrando et al. (2019)
	Matched Bank-Firm Loan Level	Belgium	Degryse et al. (2019)
		EU	Acharya et al. (2019)
Sales Growth	Matched Bank-Firm Loan Level	EU	Acharya et al. (2018), Gropp et al. (2018)
Asset Growth	Matched Bank-Firm Level	Eastern EU,Turkey	Ongena et al. (2015)
	Matched Bank-Firm Loan Level	Germany	Berg (2018)
		EU	Gropp et al. (2018)
		Mexico	Morais et al. (2019)
		Belgium	Degryse et al. (2019), De Jonghe et al. (2019)