DEUTSCHE BUNDESBANK
EUROSYSTEM

Discussion Paper

Deutsche Bundesbank
No 54/2021

Markups and financial shocks

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Internet http://www.bundesbank.de

ISBN 978–3–95729–864–5
ISSN 2749–2958

Non-technical summary

Research Question

Recent research shows that firm heterogeneity plays a crucial role in understanding the dynamics of aggregate market power. Several factors may influence markup adjustments at the firm level in different ways. In this paper, we analyse the impact of financial shocks on firms' markups, and, in particular, the ability of these shocks to generate heterogeneous markup dynamics across firms.

Contribution

We do so by combining two state-of-the-art approaches and detailed German micro data from the Bundesbank for the years 2007-2013, which allow us to obtain estimates of both markups and financial shocks that vary across firms and time. By using matched bank-firm-level data, we can estimate bank-loan supply shifters that are exogenous from the perspective of the firm. Moreover, we follow a widely used approach, and corresponding applications and extensions, in order to estimate firm-level markups using data on firms' financial accounts. We can thus study the role of financial shocks in driving firm-level markups. Moreover, we investigate the economic mechanisms behind our results and gauge their implications at the aggregate level.

Results

We find that markup responses to bank-credit supply shocks are heterogeneous across firms. In particular, firms more exposed to liquidity risks tend to raise markups in response to negative bank-loan supply shocks, while less exposed firms generally reduce them. The effects we find turn out to occur only contemporaneously, to be driven by more negative financial shocks, and to take place mainly during the financial crisis period. Further empirical analyses suggest that our findings are mostly consistent with models featuring a sticky customer base, where financially constrained firms have an incentive to raise markups in order to sustain liquidity. Finally, we present evidence that financial frictions can introduce a countercyclical dimension in the aggregate markup.

Nichttechnische Zusammenfassung

Fragestellung

Jüngere Forschungsarbeiten zeigen, dass die Heterogenität von Unternehmen eine wichtige Rolle für die Entwicklung der durchschnittlichen Preisaufschläge in einer Volkswirtschaft spielt. Dabei können unterschiedliche Faktoren die Margen von Unternehmen auf verschiedene Weisen beeinflussen. In diesem Papier wird der Einfluss von Finanzmarktfriktionen auf die Preisaufschläge von Firmen analysiert. Dabei wird insbesondere untersucht, inwieweit Finanzmarktschocks heterogene Margenanpassungen bei Unternehmen verursachen.

Beitrag

Dazu werden auf Basis moderner empirischer Ansätze und detaillierter deutscher Mikrodaten der Bundesbank für die Jahre 2007 bis 2013 Finanzmarktschocks und Preisaufschläge geschätzt, die über Unternehmen und Jahre variieren. Insbesondere können anhand von Einzeldaten zur Kreditvergabe von Banken an Unternehmen Finanzmarktstörungen geschätzt werden, die Bankkreditangebotsschocks entsprechen und daher aus Sicht der Unternehmen exogen bedingt sind. Zudem werden auf Basis einer weitverbreiteten Methode Preisaufschläge von Firmen anhand von Bilanzinformationen von Unternehmen ermittelt. Dies ermöglicht nicht nur die Analyse des Einflusses von Finanzmarktschocks auf Unternehmensmargen, sondern auch eine Untersuchung des Kanals, der zu der Anpassung von Preisaufschlägen bei Firmen führt und eine Abschätzung der Auswirkungen im Aggregat.

Ergebnisse

Die Ergebnisse zeigen, dass Unternehmen, die Liquiditätsrisiken ausgesetzt sind, ihre Margen in Reaktion auf einen negativen Bankkreditangebotsschock erhöhen, während Unternehmen, die weniger anfällig gegenüber diesen Schocks sind, die Preisaufschläge eher senken. Diese heterogenen Effekte zeigen sich insbesondere im Jahr der Schockrealisation, werden durch relativ negative Schocks verursacht und traten vornehmlich während der Zeit der Finanzkrise auf. Weitere empirische Analysen legen nahe, dass die Ergebnisse im Einklang mit Modellen stehen, in denen Unternehmen, beispielsweise aufgrund von Umstellungskosten, eine zumindest in der kurzen Frist beständige Kundenbasis haben. In einem solchen Marktumfeld haben kreditrestringierte Unternehmen einen Anreiz, ihre Margen kurzfristig auszuweiten, um Liquidität zu sichern. Schließlich zeigen weitergehende Schätzungen, dass Finanzmarktfriktionen über diesen Kanal die durchschnittlichen Preisaufschläge im Unternehmenssektor antizyklisch beeinflussen können.

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November 2021

Abstract

This paper analyses the impact of financial frictions on markup adjustments at the firm level. We use a rich panel data set that matches information on banking relationships with firm-level data. By relying on insights from recent contributions in the literature, we obtain exogenous credit supply shifters and markups that are both firm specific and time varying. We uncover new findings at this level. In particular, firms more exposed to liquidity risks tend to raise markups in response to negative bank-loan supply shocks, while less exposed firms generally reduce them. Further empirical analyses suggest that our findings are mostly consistent with models featuring a sticky customer base, where financially constrained firms have an incentive to raise markups in order to sustain liquidity. Our results have important economic implications regarding the cyclicality of the aggregate markup.

Keywords: Financial Shocks, Markups, Firm-level data

JEL classification: L22, L11, D22, G10, G01

*The views expressed in this paper are those of the authors and do not necessarily coincide with the views of the Deutsche Bundesbank, the Banco de Portugal or the Eurosystem.

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The authors would like to thank Sven Blank, Roman Duval, Peter Egger, Falko Fecht, Arne Nagengast, Friederike Niepmann, José-Luis Peydró, Steven Ongena, Davide Sala as well as participants at the economics seminar at the University of Passau, Deutsche Bundesbank, European Central Bank, and Aarhus University, at the workshop on competitiveness and current account imbalances hosted by the Banque de France, at the ETSG 2017, at the 7th CompNet annual conference in Halle in 2018, and at the 2018 meeting of the EEA for their helpful comments and suggestions.

Additional information with respect to the analysis conducted in this paper is available in a Supplementary Appendix, which can be found here.

1 Introduction

There is a renewed interest in understanding the behavior of aggregate market power, its causes and consequences, as recently illustrated by Basu (2019) and Syverson (2019). While this interest is long-standing among economists, until recently, the macroeconomic debate largely dismissed factors related to firm heterogeneity. Such a setting is in sharp contradiction with a wide body of microeconomic evidence, which emphasizes that markups vary widely across firms, even within narrowly defined industries. This apparent contradiction has been lately revisited, uncovering that firm heterogeneity plays a crucial role in understanding the dynamics of aggregate market power (De Loecker, Eeckhout, and Unger, 2020).

This article contributes to this line of research by investigating the role of financial shocks in driving firm-level markups, and, in particular, by analysing the ability of these shocks to generate heterogeneous markup dynamics across firms. Moreover, we aim to understand the economic mechanisms behind our results and gauge their implications at the aggregate level. To this end, we empirically analyze how firms change their markups in response to financial shocks in light of two conflicting theoretical predictions.

On the one hand, Gilchrist, Schoenle, Sim, and Zakrajšek (2017) point out that the interplay between customer markets and financial frictions induces financially constrained firms to raise prices (and markups) in the short run in order to sustain liquidity. They provide evidence of this mechanism by showing that during the 2008/09 financial crisis, low-liquidity firms in the US raised their output prices, while their liquid counterparts decided to move prices in the opposite direction. On the other hand, Kim (2021) argues that financially constrained firms may have an incentive to engage in fire sales, which implies that they lower output prices (and markups) in order to quickly sell off their inventories. He provides empirical evidence suggesting that firms in the US adopted such a strategy over this same period of time. These opposing views imply that the direction in which firms change their markups in the presence of financial shocks is ultimately an empirical question.

We address this question by combining two state-of-the-art approaches that allow us to obtain estimates of both markups and financial shocks that vary across firms and time. By using matched bank-firm-level data, we can estimate bank-loan supply shifters that are exogenous from the perspective of the firm, exploiting recent methodological contributions proposed by Amiti and Weinstein (2018) and Degryse, De Jonghe, Jakovljević, Mulier, and Schepens (2019). Moreover, we follow the widely used approach of De Loecker and Warzynski (2012), and corresponding applications and extensions (in particular, Brandt, Van Biesebroeck, Wang, and Zhang, 2017), in order to estimate firm-level markups. In this regard, it is important to note that we are not interested in the level of markups, but rather their change in the presence of bank-loan supply shocks, comparing firms within narrowly defined industries. For this reason, we can mostly sidestep the recent debate on the challenges regarding the identification of the level of markups (Bond, Hashemi, Kaplan, and Zoch, 2021; De Loecker, 2021).

We find that markup responses to bank-credit supply shocks are heterogeneous across firms. Specifically, firms with a large share of short-term bank loans in total assets tend to raise markups when facing negative bank-loan supply shocks, while firms less reliant on short-term loans generally reduce them. A high share of short-term bank loans in total assets makes firms more vulnerable to bank-loan supply shocks by exposing them to liquidity risks due to, for example, problems in rolling over loans when their banks unexpectedly cut lending (see e.g. Custódio, Ferreira, and Laureano, 2013; Duchin, Ozbas, and Sensoy, 2010; He and Xiong, 2012). In this sense, our results broadly corroborate the mechanism modelled by Gilchrist et al. (2017), since they imply that financially constrained firms (i.e. firms for which the liquidity risks materialize) raise their markups relative to their unconstrained peers. In addition, we show that the effects on markups occur only contemporaneously, that they are driven by more negative (i.e., below median) financial shocks, and take place mainly during the financial crisis period. Our findings are robust to a wide set of controls for potentially confounding effects and alternative markup proxies.

One of the key features of the theoretical framework proposed by Gilchrist et al. (2017) is the presence of customer markets. In this setting, the customer base of the firm is sticky in the short run, implying that firms face a trade-off between current profits and future market shares. Specifically, by setting lower prices in the current period, firms invest in a future customer base, at the cost of foregoing higher short-term profits. However, financial frictions can alter this trade-off. In particular, financially constrained firms may have an incentive to raise current prices (and markups) and thus sacrifice future market shares for higher short-run cash flow in order to avoid costly external financing. By contrast, their less exposed peers may have an incentive to lower prices, since current demand relative to future demand makes it less attractive to price high in times of crisis.

We therefore investigate the role of customer market features in driving our results, by also assessing whether our finding of heterogeneous markup responses to financial shocks is strengthened in industries that are relatively more prone to customer market characteristics.[1] To identify these industries, we use proxy variables for switching costs (in the spirit of Secchi, Tamagni, and Tomasi, 2016). In the presence of such costs, consumers cannot easily switch producers, which is a way of capturing indirectly demand stickiness associated with customer market features. As a proxy for switching costs, we consider: i) a measure of the elasticity of substitution across varieties of differentiated goods (Broda and Weinstein, 2006; Gehrig and Stenbacka, 2004); ii) research and development (R&D) intensity (Kugler and Verhoogen, 2012); and iii) a measure of the persistence of firms' market shares (Shcherbakov, 2016). We do indeed find that our results are generally reinforced in industries where customer market features are more prominent according to these proxies.

In a complementary way, we also explore information related to additional firm characteristics (in the spirit of Kim, 2021; Lenzu, Rivers, and Tielens, 2019). In particular, we investigate whether firms change their markups in order to deplete their inventories, as predicted by the fire-sales mechanism. Indeed, we find that this may have been the case for some firms. However, this finding turns out to be generally less robust in our data, and we therefore prefer to interpret it more cautiously.

[1] Note that there is a wide set of industry-specific studies that provide support for the presence of customer market features; for instance, Honka (2014) and Browning and Collado (2007) document consumer inertia, search costs and habit formation, while Heiss, McFadden, Winter, Wuppermann, and Zhou (2021) find evidence of switching costs and inattention in specific industries. Evidence is generally more limited in analyses covering a broader set of sectors, while aggregate evidence also seems to be consistent with a setting featuring customer markets, particularly so in the US (Havranek, Rusnak, and Sokolova, 2017).

Finally, we discuss the broader economic implications of our findings. In particular, we show empirically that the interaction of bank-credit supply shocks and firms' exposure to these shocks contributes to a counter-cyclical component in the behavior of the aggregate markup, consistent with the theoretical frameworks that our findings corroborate. In fact, during recessions, problems in raising external funding tend to be aggravated, thereby amplifying firms' vulnerability to adverse financial shocks, which provides incentives for firms to raise markups in order to sustain liquidity. As a consequence, financial shocks can alter the cyclical nature of the aggregate markup.

Our paper relates to several strands of the economics literature. First, it relates to studies that investigate the role of financial frictions in firms' price setting behavior in a framework where the customer base of the firm is sticky. From a theoretical perspective, both the partial equilibrium model of Chevalier and Scharfstein (1996) and the general equilibrium framework of Gilchrist et al. (2017) show that financial frictions can induce firms to raise prices and markups. From an empirical perspective, several studies provide support for positive price effects from financial frictions, exploring mainly (sector-level) price data or qualitative survey data (see, for instance, Gilchrist et al., 2017; Duca, Montero, Riggi, and Zizza, 2017; Antoun de Almeida, 2015; Chevalier and Scharfstein, 1996). We contribute to these studies by analyzing the effect of these shocks on markup policies at the firm level, which is virtually undocumented. To this end, we combine state-of-the-art approaches that allow both markups and financial shocks to be measured at the firm level.

Second, our results are relevant with respect to the debate on the role of firm heterogeneity for the cyclicality of markups, as recently raised by Burstein, Grassi, and Carvalho (2020) and Hong (2019). This strand of the literature tries to explore sources of firm heterogeneity – in particular, in relation to firm size – to shed light on the long-standing debate on the cyclical properties of the aggregate markup. We show that heterogeneity in firms' exposure to liquidity risk and its materialization due to negative bank-loan supply shocks can add a counter-cyclical dimension to the dynamics of the aggregate markup. Thus, we contribute to these studies by showing that other sources of heterogeneity are relevant to this debate.

In addition, there is a large and growing body of empirical evidence documenting the impact of credit supply shocks on firm outcomes. These studies mainly aim at uncovering the effect of these shocks on firm investment or employment (see, for instance, Amiti and Weinstein, 2018; Cingano, Manaresi, and Sette, 2016; Chodorow-Reich, 2014; Bentolila, Jansen, and Jiménez, 2018). This also holds true for studies in this area which focus on Germany (see, for instance, Popov and Rocholl, 2018; Dwenger, Fossen, and Simmler, 2020; Huber, 2018; Bersch, Degryse, Kick, and Stein, 2020). We contribute to this line of research not only by showing that financial frictions can also impact other firm outcomes, such as markup policies, but also by showing how to obtain meaningful estimates of bank-loan supply shifters by combining insights from recent methodological contributions.

Finally, we relate to the many recent studies that aim at uncovering drivers of markup adjustments at the firm level. For instance, previous studies show that firms alter their markups in response to shocks related to foreign competition (De Loecker, Goldberg, Khandelwal, and Pavcnik, 2016), exporting decisions (Garcia-Marin and Voigtländer, 2019), or exchange rate movements (Berman, Martin, and Mayer, 2012). Our work highlights that financial shocks can matter as well.

The rest of the paper is structured as follows. In the next section, we lay out our empirical approach for estimating bank-loan supply shifters, including a description of the underlying data and an assessment of the plausibility of the estimated shifters. Section 3 summarizes our empirical approach to obtaining firm-level markups. It also presents the firm-level data used to estimate markups and shows that bank-loan supply shifters matter for the borrowing of the firms. Section 4 presents the conceptual and empirical frameworks for investigating firms' markup adjustments to bank-loan supply shocks, along with the main results. In section 5, we investigate the main economic channels behind these results, and section 6 discusses the corresponding economic implications. Section 7 offers some concluding remarks.

2 Estimating bank-loan supply shifters

In this section, we first lay out the identification strategy used to estimate the bank-loan supply shifters. Subsequently, we describe the bank-firm-level data used to estimate these shifters and provide evidence regarding their plausibility.

2.1 Empirical strategy

We estimate bank-loan supply shifters from matched bank-firm-level data using the following setup:

$$D(L_{fbt}/L_{fb,t-1}) = \alpha_{ft} + \beta_{bt} + \epsilon_{fbt}, \tag{1}$$

where L_{fbt} refers to total lending of bank b to firm f in year t, $D(L_{fbt}/L_{fb,t-1})$ denotes the growth in lending, and ϵ_{fbt} is an error term.[2] α_{ft} is the firm borrowing channel, which captures factors causing changes in a firm's borrowing, such as investment-specific demand shocks. β_{bt} is the bank-lending channel, which is the parameter of interest. It reflects bank-specific factors that drive changes in a bank's lending activity; e.g. shocks to its liquidity position. Amiti and Weinstein (2018, henceforth AW) emphasize that this empirical setup is agnostic about the specific origins of these shocks.

Importantly, controlling for α_{ft} in equation (1) implies that β_{bt} is purged from demand factors related to bank b's clients, warranting an interpretation of β_{bt} as credit supply shifters. In principle, we can use OLS to estimate the coefficients α_{ft} and β_{bt}. However, some recent studies point to certain caveats to this kind of estimation approach. One concern relates to the fact that equation (1) needs to be estimated using a sample restricted to firms that have relationships with at least two banks. The reason is that β_{bt} is identified by comparing how different banks change their lending behavior towards the same firm, while keeping borrowing demand fixed due to α_{ft}. However, depending on the prevalence of single-bank firms in the matched bank-firm-level data set, it may be questionable whether banks' relationships with multi-bank firms are indeed representative of relationships with single-bank firms. Moreover, equation (1) does not take into account the creation (and – in cases where the growth rate is computed using log differences – also the destruction) of lending relationships. If the latter is an important driver of a bank's lending behavior, the estimated supply shifters may be biased. Relatedly, α_{ft} and

[2] $D(\cdot)$ denotes a general growth rate, since existing studies differ in the definition of growth rates in terms of log differences or percentage changes.

β_{bt} are not necessarily informative about a bank's total loan growth but rather its average growth rate, which may not be the type of shocks the researcher aims to identify.

In this paper, we exploit recent methodological advances in order to address these caveats. First of all, Degryse et al. (2019, henceforth DJJMS) propose a strategy that allows the inclusion of lending to single-bank firms when estimating the bank-loan supply shifters. Their strategy involves using observable firm characteristics to control for demand factors instead of firm-time fixed effects (α_{ft}). Specifically, DJJMS control for $\alpha_{ILS,t}$, where ILS refers to industry-location-size class fixed effects. The approach thus assumes that the credit demand of firms belonging to an ILS group changes in a similar way. Their results indicate that a methodology for estimating bank-loan supply shifters which neglects single-bank firms can lead to a downward bias in the estimated real effects of the bank lending channel.

Second, AW develop a strategy that addresses the two remaining concerns. In particular, they propose moment conditions that allow for: i) the creation and destruction of lending relations and; ii) an aggregation of financial shocks that explains banks' total loan growth. In fact, AW conclude that estimates of bank-loan supply shifters that do not aggregate and ignore new lending are noisy and biased. We provide more details on the implementation of this approach in the Supplementary Appendix.

We combine the approaches of AW and DJJMS in order to estimate the bank-lending channel from a sample that includes loans from single-bank firms, while accounting for the extensive margin of bank lending. As described below, our data set contains a large number of firms that borrow from one bank only. We include the lending to (most of) these firms by forming larger groups of firms according to observable firm-level characteristics in the spirit of DJJMS. Specifically, we collapse the matched bank-firm-level data set to the bank-*ILSR*-level and then apply the AW approach to this data set, where in our case the *ILSR*-level refers to the following observable firm-level characteristics: sector affiliation (i.e. *I*ndustry), regional location (i.e. *L*ocation), indicators of group membership of firms as well as their total bank debt (as indicators of *S*ize), information about the number of banks a firm borrows from (i.e. number of bank *R*elations), and its legal form. We fix all of these variables using pre-sample values. The Supplementary Appendix presents more details about these variables.

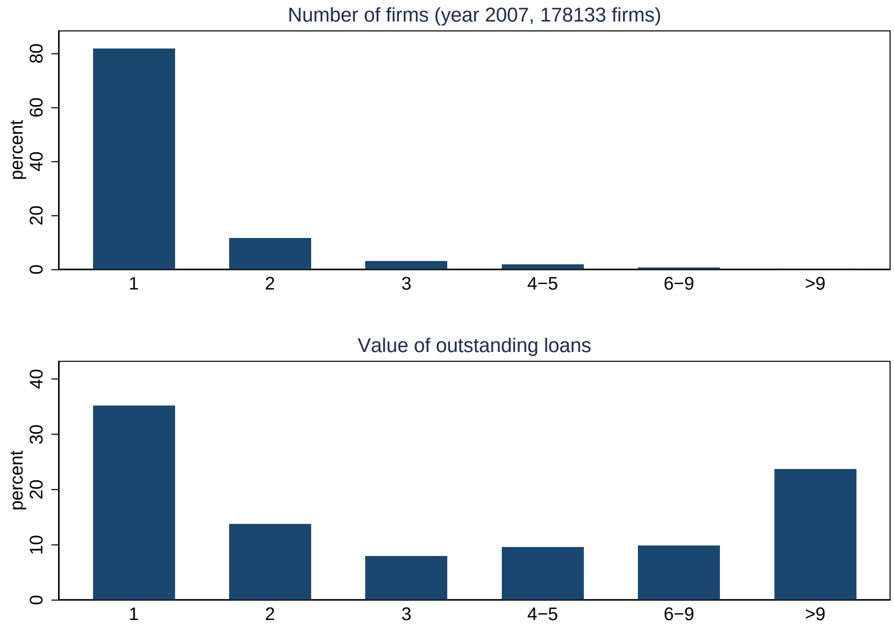
2.2 Bank-firm-level lending data

We use a matched bank-firm-level data known as "MiMiK", available at the Deutsche Bundesbank, to estimate bank-loan supply shifters. This dataset contains credit information at the lender-borrower level on the universe of credit exposures amounting to at least 1.5 million euro per borrower or borrower unit in Germany on a quarterly basis. We rely on credit data in the fourth quarter of every year, since firm-level balance sheet information is available at a yearly frequency only, and the vast majority of firms report their financial statements during this period of the year. Hence, loan growth corresponds to yearly growth rates in the fourth quarter of a given year. We end up with around 1,100 lending institutions and 180,000 borrowers per year, which we use to estimate bank-loan supply shocks. Despite the reporting threshold, the coverage of the database is quite high; for instance, in 2005 it covered around 70% of the total credit volume in Germany. In the Supplementary Appendix, we provide detailed information on characteristics of the data set, including the reasons why we also observe many loans below the reporting threshold, and describe our sample selection and data cleaning procedures.

2.3 Descriptives on the bank-loan supply shifters

Figure 1 depicts the number of firms and their corresponding loan shares according to the number of bank relationships using MiMiK. The upper panel shows that more than 80% of the firms in the sample borrow from one bank only. The lower panel reveals that these firms are relevant in terms of loan volumes, accounting for approximately one-third of the total outstanding corporate loans in 2007. Note that these firms would be excluded from the sample if we had not adopted the adjustment inspired by DJJMS. Instead, by applying our *ILSR* approach, we can include around 98% of outstanding corporate loans reported in MiMiK when estimating the bank-loan supply shifters.[3]

Figure 1: Number of firms' borrowing relationships (in 2007)



Notes: The figure shows the number of borrowing relationships of firms in 2007 (upper panel) and the corresponding loan shares (lower panel).

2.4 Plausibility of shock estimates

In the Supplementary Appendix, we investigate empirically the plausibility of the estimated bank-loan supply shifters in two dimensions. First, we provide supporting evidence that our *ILSR* approach can indeed account for demand effects, corroborating DJJMS findings. On the one hand, we show that bank shock estimates obtained using the *ILSR* approach closely match the ones obtained from an approach that includes firm-time fixed

[3] We may still lose some observations in a given year, since there are cases where an *ILSR* group borrows from one bank only. Moreover, as explained in the Supplementary Appendix, the AW approach does not account for loans of banks (*ILSR* groups) that only dispose of new lending (borrowing) relationships.

effects as a control for borrowing demand in a sample restricted to multi-bank firms. On the other hand, we find more pronounced differences between bank shock estimates obtained from samples excluding single-bank firms compared to estimates obtained when including these firms in the sample (using the *ILSR* approach in both cases). This result points to the importance of incorporating single-bank firms in the analysis. Second, we investigate the external validity of the bank-loan supply shifters. To this end, we explore correlations between these shifters and certain bank characteristics which were associated with more significant cuts in loan supply in Germany, according to studies covering the same period (see, for instance, Puri, Rocholl, and Steffen, 2011; Abbassi, Iyer, Peydró, and Tous, 2016). As expected, we find that the estimated shifters are strongly correlated with such proxy variables.

3 Estimating markups and bank-loan supply shocks at the firm level

In this section, we first describe the firm-level data. Then we discuss how to obtain bank-loan supply shifters and markups that are both time and firm specific. Finally, we present some descriptive statistics.

3.1 Balance sheet - USTAN

We use a data set known as "USTAN", which is available at the Deutsche Bundesbank and contains balance sheet and profit and loss account information for firms in Germany. USTAN contains information for around 25,000 firms per year, almost half of which are part of the manufacturing sector. This data set thus tends to over-represent manufacturing firms, which is why we focus on this sector in the remainder of the paper. In particular, we construct an annual matched loan-firm-level data set for the German manufacturing sector, by merging firms present in the data source MiMiK to the ones in USTAN. The final data set includes around 6,000 manufacturing firms in the period from 2007 to 2013. Since USTAN not only over-represents manufacturing firms but also larger firms, these matched firms account for more than 40% of the aggregate output and wage bill of the manufacturing sector when compared to National Accounts data. In the Supplementary Appendix, we provide further information on the characteristics of this data set along with the data cleaning procedure.

Two additional features of the data are worth noting. First, due to the fact that USTAN over-represents large firms, which are generally considered less likely to be financially constrained (e.g. Beck, Demirgüç-Kunt, and Maksimovic, 2005; Hadlock and Pierce, 2010), any effect of credit supply shocks that we may find should clearly be interpreted as lower bound estimates. This applies all the more since USTAN also tends to include firms which are expected to be, on average, financially sound. In particular, the data set comprises information on firms reported by their respective banks to the Bundesbank in the context of refinancing operations.[4] Second, in the subsequent empirical analysis,

[4] More precisely, credit institutions can submit their credit claims against non-financial corporations as collateral, whereas the Bundesbank accepts only credit claims against "central bank eligible" companies, which tend to have a positive credit worthiness.

we divide the sample into crisis years and the following recovery period. The severity of the recession in the German and the global economy and the role played by the financial system over the crisis period suggests that our findings could change depending on the severity of financial shocks.

3.2 Bank-loan supply shocks at the firm level

We link the bank-loan supply shifters (β_{bt}) to the corresponding firms by using information on a bank's share in a firm's total borrowing. More precisely, we compute:

$$BankShock_{ft} = \sum_b \theta_{fb,t-1}\tilde{\beta}_{bt},$$

where $\theta_{fb,t-1}$ denotes the share of borrowing from bank b in firm f's total borrowing in the previous period, as adopted by the bulk of the literature (e.g. AW and DJJMS). Note that the tilde indicates that the estimated βs are normalized according to the median bank-loan supply shock in a given year.[5]

To estimate the impact of bank-loan supply shocks on firm outcome variables, we require that the firm-specific bank-loan supply shifters can be considered plausibly exogenous from a firm perspective. We mentioned previously that β_{bt} are purged from demand factors related to quite detailed firm characteristics. Moreover, β_{bt} are derived from banks' total loan growth and it is rather unlikely that unobserved firm effects not captured by our *ILSR* approach drive a bank's aggregate lending. A potentially remaining threat to identification could arise if there is systematic sorting between weak banks and weak firms (see, e.g. Berton, Mocetti, Presbitero, and Richiardi, 2018). To mitigate such concerns, we present in Table 1 the mean firm characteristic by quintile of the $BankShock_{ft}$ distribution for a set of variables. Note that we use firms' pre-sample values for this exercise in order to assess whether initially weak firms are more prone to experience negative bank-loan supply shocks during the sample period. For most variables, there is no obvious sorting between pre-sample characteristics and in-sample bank shock realizations. The only exception relates to firm size, since especially ex-ante very large firms appear to have a higher probability of experiencing more favourable credit supply shocks. This may reflect the fact that these firms tend to borrow from several banks such that an unexpected reduction in loan supply by one bank may be more easily offset by borrowing more from another bank. We therefore include control variables for firm size and the number of bank relations in the subsequent regression analysis.

[5] In particular, $\tilde{\beta}_{bt} = \hat{\beta}_{bt} - MEDIAN_t(\hat{\beta}_{bt})$. Hence, the shock estimates are relative to a (yearly) numeraire. Thus, they should be analyzed within one time period only. In a panel regression, this can also be implemented by adding time dummies to the estimated model, which is always adopted in this paper.

Table 1: Mean firm characteristics by quintile of $BankShock_{ft}$

	Quintile of *BankShock*				
	1	2	3	4	5
Log of total assets	-0.020	-0.260	-0.167	0.097	0.351
	(-0.59)	(-7.57)	(-4.86)	(2.81)	(10.24)
Log no. of employees	-0.014	-0.184	-0.131	0.068	0.261
	(-0.49)	(-6.27)	(-4.47)	(2.31)	(8.95)
Log labor productivity	-0.004	-0.007	-0.004	0.004	0.011
	(-1.67)	(-2.90)	(-1.55)	(1.54)	(4.58)
Log no. of banks	0.038	-0.130	-0.123	0.015	0.200
	(2.03)	(-6.91)	(-6.54)	(0.81)	(10.65)
Leverage	-0.001	0.011	0.003	-0.002	-0.011
	(-0.16)	(2.57)	(0.71)	(-0.50)	(-2.62)
Share of total bank loans	0.002	0.007	-0.002	-0.005	-0.002
	(0.47)	(1.68)	(-0.41)	(-1.21)	(-0.53)
Share of bank loans due	0.005	0.003	-0.003	-0.006	0.001
	(1.85)	(0.98)	(-1.16)	(-2.04)	(0.37)
Share of bank loans not due	-0.004	0.004	0.001	0.001	-0.003
	(-1.21)	(1.41)	(0.47)	(0.27)	(-0.93)
Share of cash holdings	0.001	0.002	0.000	-0.001	-0.003
	(0.54)	(1.36)	(0.22)	(-0.30)	(-1.82)
Inventories to sales ratio	-0.001	-0.001	0.003	-0.000	-0.001
	(-0.36)	(-0.21)	(0.87)	(-0.08)	(-0.21)
$\ln(\mu^{cd})$	-0.014	0.009	0.003	-0.000	0.003
	(-1.93)	(1.22)	(0.41)	(-0.06)	(0.34)
$\Delta\ln(\mu^{cd})$	0.003	-0.002	-0.004	0.002	0.001
	(1.24)	(-0.83)	(-2.09)	(1.22)	(0.50)

Notes: The table presents average values of firm characteristics by quintile of the $BankShock_{ft}$ distribution. Note that $BankShock_{ft}$ is purged from industry-year fixed effects before computing quintiles. Similarly, the firm characteristics are purged from industry fixed effects. This is carried out because subsequent regressions always contain industry-year fixed effects. Further note that the firm characteristics are computed as mean values of pre-sample years (2004-2006), or, where a firm joins the sample in 2007, they relate to the year 2007. T-statistics presented in parentheses inform about differences in means with respect to the remaining observations in the sample.

Additionally, we require that the firm-specific bank-loan supply shifters actually have an impact on firm borrowing. If firms can simply switch to other banks when their main lenders are hit by negative shocks, their total borrowing will remain unchanged and, thus, they would not be financially constrained. We investigate the relationship between bank-loan supply shocks and firm-level borrowing by estimating the following model:

$$y_{ft} = \delta BankShock_{ft} + DemandControl + x_{f,t-1} + \gamma_{st} + \gamma_f + \varepsilon_{fbt}, \tag{2}$$

where y_{ft} denotes the logarithm of firm-level bank loans or total debt; $DemandControl$ refers to $\tilde{\alpha}_{ILSR,t}$;[6] $x_{f,t-1}$ contains lagged labor productivity and total assets; and γ_{st} and

[6] This control variable can be important since some recent studies find that bank-loan supply and firm-borrowing demand tend to be negatively correlated at the firm level, implying that the impact of bank-loan supply shocks is underestimated when adequate credit demand controls are absent (e.g. Alfaro, García-Santana, and Moral-Benito, 2021). We also normalize these shocks according to their yearly median. Note also that we account for extreme observations in the distributions of $BankShock_{ft}$ and $\tilde{\alpha}_{ILSR,t}$, by trimming the first and last percentile (computed by year) of these variables.

γ_f are industry-time and firm fixed effects, respectively.

Table 2 presents the estimation results for a sample covering the full sample period (2007-2013) as well as a sample restricted to the crisis period (2007-2010). We start by assessing the effect of the credit supply shifters on firms' bank loans, obtaining a positive and significant coefficient of the bank shock variable for both periods analyzed in columns 1 and 2. The coefficient magnitudes imply that a one-standard-deviation decrease in the bank-loan supply shock is associated with more than a 4 % reduction in firm loans reported in MiMiK. Consequently, firms do not appear to be able to simply switch banks to fully compensate for lending cuts. This finding is usually attributed to information asymmetries between lenders and borrowers which can be mitigated by developing a trusting long-term relationship – a practice which is often considered to be of relevance in Germany and associated with the concept of "house banks" (e.g. Elsas and Krahnen, 1998). In Columns 3 and 4, we repeat this exercise for the same outcome variable but change its source to USTAN. Note that this loan variable may differ from the one in MiMiK as, for instance, firms may have additional loans which fall below the reporting threshold present in MiMiK. Still, the results are broadly similar to the previous ones, even though the coefficient magnitudes are somewhat lower, indicating an effect of between 2% and 3%. In columns 5 and 6, we change the outcome variable to investigate the effect on firms' total debt. Once more, the estimated coefficients are highly statistically significant, pointing to an effect of around 1%. This result suggests that firms cannot fully offset the lending cuts by switching to alternative financing sources such as bonds or funds from affiliated companies.[7]

Table 2: Effect of bank loan supply shocks on firms' loans and debt

	(1) ln(loans) MiMiK	(2) ln(loans) MiMiK	(3) ln(loans) USTAN	(4) ln(loans) USTAN	(5) ln(debt) USTAN	(6) ln(debt) USTAN
BankShock	0.042***	0.044***	0.020***	0.027***	0.006***	0.006**
	(0.007)	(0.008)	(0.006)	(0.007)	(0.002)	(0.003)
Observations	13,961	8,376	13,961	8,376	13,961	8,376
Firms	3,041	2,634	3,041	2,634	3,041	2,634
R^2	0.143	0.137	0.113	0.092	0.290	0.152
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Demand control	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Period	2007-2013	2007-2010	2007-2013	2007-2010	2007-2013	2007-2010

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered (firm-level) standard errors in parentheses. All regressions contain year fixed effects and controls for credit demand ($\tilde{\alpha}_{ILSR,t}$), size (lagged assets) and productivity (lagged value added per employee). R-squared refers to the (adjusted) within R-squared. Dependent variable shown in the second row of the table.

3.3 Estimating firm-level markups

Our baseline approach to estimating markups at the firm level relies on the methodology proposed by De Loecker and Warzynski (2012). Specifically, by rearranging the first order condition of the firm's cost minimization problem, the markup becomes the ratio of the output elasticity of a variable input to its cost share in total output. This relationship

[7] The Supplementary Appendix presents further results regarding the analyses presented in this section. First, evidence regarding the potential sorting between weak banks and weak firms is shown for additional variables. Second, the effects of the bank-loan supply shifters on other firm-level outcome variables are analyzed.

does not depend on a particular type of price competition or functional form of demand. It does, however, require that firms minimize costs and that there is at least one input that is free of adjustment costs. We assume that intermediate inputs (computed as the sum of materials and services used in production) is the variable input.[8] Formally, the markup of firm f in period t (μ_{ft}), defined as the price (P_{ft}) over marginal costs (MC_{ft}), can be written as:

$$\mu_{ft} \equiv \frac{P_{ft}}{MC_{ft}} = \frac{\psi_{ft}^{m}}{\tau_{ft}^{m,adj}}, \tag{3}$$

where ψ_{ft}^{m} is the output elasticity of materials and $\tau_{ft}^{m,adj}$ is the share of material costs in the total revenue of the firm adjusted for unobserved shocks to production. Under this setting, we need to specify a measure for both the numerator and the denominator of equation 3. In fact, one of the challenges in estimating markups is that output elasticities are not observable in the data. To this end, De Loecker and Warzynski (2012) propose to estimate a production function, using a control function approach in the spirit of Ackerberg, Caves, and Frazer (2015).

Alternatively, Brandt et al. (2017) recently proposed to control for ψ_{ft}^{m} using an appropriate fixed effects structure in the empirical setup. Assuming that the production function takes a Cobb-Douglas functional form, ψ_{ft}^{m} becomes constant across time (for firms belonging to the same sector). Consequently, we can capture this term by including sector fixed effects in the outcome equation. We adopt this strategy as our baseline approach for three main reasons. First, we are not interested in the level of markups, but rather their adjustment over time, taking into account the heterogeneity of firms regarding financial factors.[9] Second, we consider a rather short time frame in our empirical analysis, rendering it less likely that firms adjust production technologies over this period. Third, adopting a control function approach to estimating a gross output production function can be particularly challenging when there is no available information on firm-level output and input prices (Ackerberg et al., 2015; Gandhi, Navarro, and Rivers, 2020).[10] While the literature has proposed various ways of addressing such identification problems (see, e.g. De Loecker et al., 2016, 2020; De Loecker and Scott, 2016), these strategies either require additional data which are not available to us or are based on additional assumptions, which we prefer to rely on only in the context of robustness checks.[11]

[8] This assumption is consistent with other recent studies for Germany (e.g. Mertens, 2020).

[9] We aim to discuss how firms adjust their markups in response to bank-credit supply shocks. We acknowledge that sources of bias may still be present in our analysis. However, as long as these sources are not correlated with the financial dependence of the firm, our results remain unchanged. As outlined below and in the Supplementary Appendix, we also experiment with various adjustments of the control function approach to account for such financial factors when estimating the denominator of the markup expression (see equation 3).

[10] In particular, there are identification problems with respect to the output elasticity of the flexible input. Moreover, problems can arise due to the so-called input and output price biases. De Loecker and Goldberg (2014) note that input and output price bias imply contrasting signs so that they may even cancel each other out. Hence, the absence of any control for these two price biases might be the most suitable strategy when no firm-level price data are available.

[11] Moreover, the presence of firm-specific financial distortions adds another layer of complexity in our context. These distortions could, for example, impact a firm's price of capital and thus bias the capital coefficient. Even though that bias would not necessarily affect ψ_{ft}^{m}, by applying an adequate fixed effects structure, we try to avoid such potential issues. Note also that De Loecker and Warzynski (2012) discuss in general a way of addressing such concerns by adjusting the corresponding input demand function. This

In our baseline empirical implementation, we proceed as follows. We assume a Cobb-Douglas production technology and include NACE 3-digit sector fixed effects in the outcome equation, which we, furthermore, interact with year dummies. This specification aims at accommodating potential changes in the production technology occurring within narrowly defined sectors. A remaining concern could relate to production technologies that are firm specific, which we address by also including firm fixed effects in the outcome equation. Hence, only firm-level changes in ψ^m_{ft} that deviate from detailed sector-level developments would not be taken into account by applying this specification. Such changes would be in line with a more flexible production technology such as the translog functional form. In this case, the output elasticity for materials becomes firm and time specific:

$$\hat{\psi}^m_{ft} = \hat{\kappa}_m + 2\hat{\kappa}_{mm} m_{ft} + \hat{\kappa}_{lm} l_{ft} + \hat{\kappa}_{mk} k_{ft} + \hat{\kappa}_{lmk} l_{ft} k_{ft}, \tag{4}$$

where the κs refer to the estimated production function parameters and m_{ft}, l_{ft}, and k_{ft} are the logarithms of firm-level inputs of intermediate goods, labor, and capital, respectively. Note that the firm and time variation in $\hat{\psi}^m_{ft}$ is fully driven by the input choices of the firm. To be more in line with such a specification, we further expand our set of control variables and also add the input use of the firm (i.e. labor and capital along with the corresponding interaction) to the outcome equation.[12] This empirical setup should largely mitigate any concerns related to firm-level changes in $\hat{\psi}^m_{ft}$. Moreover, it is consistent with the approach recently proposed by Bond et al. (2021) to estimating markups when no price data are available and the researcher is not interested in the level of markups.

We still need to specify the denominator of the RHS of equation 3. This term is computed as:

$$\tau^{m,adj}_{ft} = \frac{exp(m_{ft})}{exp(r_{ft} - \hat{\epsilon}_{ft})}, \tag{5}$$

where r_{ft} is the logarithm of revenue and $\hat{\epsilon}_{ft}$ is an adjustment factor obtained from the residual generated in the first step of a production function estimation algorithm in the spirit of Ackerberg et al. (2015) and also adopted by De Loecker and Warzynski (2012) and Brandt et al. (2017). It involves regressing the logarithm of (deflated) revenue (rd_{ft}) on a polynomial expansion of the firms' inputs:

$$rd_{ft} = h(k_{ft}, l_{ft}, m_{ft}, z_{ft}) + \epsilon_{ft}, \tag{6}$$

where $h(\cdot)$ is a polynomial function and ϵ_{ft} is the error term of this equation which corresponds to the adjustment factor. This term purges the expenditure share τ^m_{ft} from variation in output that is not related to input demand, e.g. unobserved shocks to production, certain aspects related to market characteristics, or pure measurement error. As suggested by De Loecker and Warzynski (2012), z_{ft} captures additional factors that potentially impact the firm's optimal input demand. In our setting, the bank-loan supply shocks as well as a firm's reliance on bank financing can potentially affect its input demand for materials. Hence, we further expand the control function for unobserved productivity

strategy has been adopted in the context of financial constraints e.g. by Manaresi and Pierri (2018) and we discuss this further in the Supplementary Appendix.

[12] We thus model the output elasticity as $\psi^m_{ft} = \gamma_{st} + \gamma_f + \lambda_l l_{ft} + \lambda_k k_{ft} + \lambda_{lk} l_{ft} k_{ft}$, where γ_{st} and γ_f are sector-year and firm fixed effects, l_{ft} and k_{ft} are firms' choice of labor and capital inputs, and λ_l, λ_k, and λ_{lk} are coefficients to be estimated.

by including these variables in the vector z_{ft}.[13] The non-parametric regression shown in equation 6 is then run by NACE rev. 2 chapter in order to obtain an estimate for the adjustment factor ϵ_{ft}.

3.4 Descriptives on matched firm-level data

Table 3 contains a set of descriptive statistics for key variables, highlighting some features of the data. For instance, the average number of employees exceeds 150 (i.e. $e^{5.1}$), confirming that, on average, the sample contains rather large firms. Moreover, there is pronounced heterogeneity among firms in terms of their total indebtedness in general (i.e. leverage) and their reliance on bank-loan financing more specifically. For instance, while the share of bank loans in total assets amounts to 26% for the average firm, it ranges from only 2% to as much as 57% when considering the 5th and the 95th percentiles, respectively. There is a comparable degree of heterogeneity regarding their reliance on short-term loans and even more so with respect to cash holdings. While the average firm holds around 5% of cash in total assets, this share reaches around 23% for firms at the 95th percentile and it is below 1% for firms at the 25th percentile. Similarly, the median firm has relations with two banks, while there are also a significant number of firms that borrow from one bank only (25th percentile equals one) or three and more banks (75th percentile equals 3). In addition, we observe that, on average, firms do not change markups during the sample period. However, there is substantial heterogeneity among firms which either raise or lower their markups. This finding holds for our preferred markup proxy that is consistent with a Cobb-Douglas production function as well as for a proxy derived from a translog specification (computed to assess the sensitivity of the results below).

Table 3: Summary statistics

	obs	mean	sd	p5	p25	p50	p75	p95
$\ln(\mu^{cd})$	20826	0.647	0.277	0.249	0.447	0.613	0.814	1.161
$\Delta\ln(\mu^{cd})$	16873	-0.001	0.083	-0.130	-0.045	-0.003	0.040	0.136
$\ln(\mu^{tl})$	19363	0.240	0.165	0.026	0.131	0.214	0.315	0.555
$\Delta\ln(\mu^{tl})$	15593	-0.000	0.049	-0.079	-0.027	-0.001	0.026	0.079
BankShock	25769	0.000	1.000	-1.471	-0.662	-0.099	0.568	1.833
Log no. of banks (t-1)	25750	0.556	0.661	0.000	0.000	0.693	1.099	1.792
Log total assets (t-1)	24518	9.966	1.470	7.919	9.011	9.791	10.761	12.578
Log labor productivity (t-1)	23551	4.134	0.438	3.481	3.888	4.125	4.378	4.831
Leverage (t-1)	24518	0.712	0.183	0.372	0.596	0.732	0.851	0.981
Total bank loans (t-1)	22655	0.260	0.174	0.017	0.120	0.240	0.379	0.572
Bank loans due (t-1)	22489	0.124	0.117	0.002	0.029	0.089	0.187	0.361
Cash holdings (t-1)	24518	0.053	0.082	0.000	0.003	0.018	0.065	0.229
Inventories (t-1)	24445	0.186	0.206	0.033	0.091	0.148	0.226	0.446
Log no. of employees	24947	5.146	1.159	3.401	4.394	5.069	5.814	7.197
Log tangible assets	25440	8.364	1.738	5.438	7.376	8.423	9.449	11.092

Notes: The table presents summary statistics for manufacturing firms in the USTAN data after merging these firms with information contained in the MiMiK data set. Summary statistics are based on around 6,000 firms. μ^{cd} and μ^{tl} refer to markup ratios that are consistent with a Cobb-Douglas and translog production function, respectively.

[13] In particular, we include the bank shocks and firms' lagged share of bank loans in total assets in the control function (see Manaresi and Pierri, 2018, for a similar approach). Moreover, this function controls for firms' mean wages and market shares as well as detailed (NACE 4-digit) industry, region, and year fixed effects (see e.g. Brandt et al., 2017; De Loecker et al., 2016; De Loecker and Scott, 2016; De Loecker et al., 2020). In the Supplementary Appendix, we present further details on this approach, its key underlying assumptions and a wide set of specifications of z_{ft} to ensure the robustness of our findings.

4 Markup adjustments to bank-loan supply shocks

In this section, we first review the theoretical literature on how firms adjust output prices and markups in the presence of financial distortions in order to derive predictions for our empirical analysis. Then, we describe our econometric framework and discuss the main estimation results.

4.1 Conceptual framework

The way firms adjust prices and markups when faced with financial shocks is ambiguous a priori. In fact, both theoretical models and existing empirical evidence suggest that firms may either increase or decrease their output prices and markups in response to these shocks. Below, we briefly review this literature and highlight two prominent channels.

The first channel relates to a specific characteristic of firms' demand, namely the stickiness of a firms' customer base. From a theoretical perspective, this feature can be rationalized by some form of demand rigidity, arising, for instance, due to the presence of switching costs (see, e.g. Klemperer, 1995).[14] In such a setting, firms face a trade-off between current profits and future market shares when making their pricing decision. Specifically, by setting lower prices, firms invest in a future customer base while foregoing higher short-term profits. However, financial frictions can alter this trade-off. Financially constrained firms may have an incentive to raise current prices and thus sacrifice future market shares to obtain higher short-run cash flow and avoid costly external financing. As a result, liquidity constrained firms are expected to raise prices and markups (by more) in response to negative financial shocks. Chevalier and Scharfstein (1996) formalize this idea, while more recently Gilchrist et al. (2017) develop this mechanism in a general equilibrium setup with monopolistically competitive firms.[15] In fact, Gilchrist et al. (2017) emphasize the heterogeneous price setting behavior of firms after being hit by a financial shock, which ultimately depends on their liquidity needs caused by the shock. In particular, while firms in need of liquidity raise their prices for the reasons described above, firms without liquidity shortages (e.g. due to other financing means) tend to cut their prices. The latter effect is also related to customer market considerations, since current demand relative to future demand makes it less attractive to price high in times of crisis for firms without liquidity constraints.

The second channel relates to the possibility that financial shocks force firms to engage in fire sales in order to sustain liquidity. In particular, firms may adjust to a financial shock by reducing output prices (relative to the remaining firms) in an attempt to quickly sell off inventories and raise cash flow. The underlying idea is that fire sales are essentially forced sales at a dislocated price (Shleifer and Vishny, 2011). Kim (2021) develops this mechanism in a dynamic general equilibrium model. The model predicts that firms and sectors that have higher initial inventories or smaller cash holdings decrease their output prices relatively more when hit by financial shocks. He also provides corresponding

[14] However, other mechanisms such as costly search or idiosyncratic preferences (i.e. consumption inertia or habit formation) are also consistent with this setting (Farrell and Klemperer, 2007).

[15] Several authors present consistent evidence, such as Chevalier and Scharfstein (1996), Asplund, Eriksson, and Strand (2005), and Secchi et al. (2016).

empirical evidence.[16]

These two channels thus provide partly conflicting predictions in terms of the price-setting behavior of the firm in response to a financial shock. While the first channel suggests that firms with liquidity needs raise their output prices (and markups) in order to keep the cash flow up, the second channel instead suggests the opposite reaction. Specifically, firms lower their output prices (and markups) in order to quickly sell off inventories and sustain liquidity in the short run.

Below, we investigate how firms adjust their markups to bank-loan supply shocks depending on their share of short-term bank credit in total assets. The intuition for such an interaction effect is that firms with a high share of short-term bank loans in total assets can be considered to be more exposed to bank-loan supply shocks (see, e.g. Custódio et al., 2013). One reason for this is that a strong reliance on short-term loans implies a higher exposure to liquidity risk, which arises when a firm is solvent but unable to receive refinancing. As emphasized by Duchin et al. (2010), long-term debt is less susceptible to this type of risk as compared to short-term loans. For instance, firms with a higher share of short-term loans face larger rollover risks when their bank unexpectedly cuts credit (Acharya, Gale, and Yorulmazer, 2011; He and Xiong, 2012). In this sense, the direction in which firms that are more reliant on short-term bank loans adjust their markups in response to financial shocks already signals which channel dominates in the data.

4.2 Empirical strategy

We aim to understand how firms adjust their markup when facing financial shocks. To this end, we estimate the following main model:

$$\begin{aligned} \ln(\mu_{ft}) =& \lambda_1 + \lambda_2 BankShock_{ft} + \lambda_3 (BankShock_{ft} \times BankLoansDue_f) \\ &+ x_{ft-1}\lambda_4 + \gamma_f + \gamma_{st} + u_{ft}, \end{aligned} \tag{7}$$

where $\ln(\mu_{ft})$ denotes the logarithm of markups. Importantly, we need to distinguish the effect of bank-loan supply shocks from other confounding factors that may drive markup dynamics. To do so, we include detailed (NACE 3-digit) industry fixed effects in equation (7) and their interactions with year dummies (γ_{st}). Hence, we account for unobserved yearly shocks at a detailed industry level, such as demand shocks. Moreover, since we also control for firm fixed effects (γ_f) in the regression, we exploit within-firm variation, comparing developments occurring within industries at a given point in time in order to identify the coefficients of interest. We account for additional heterogeneity at the firm level by including a set of observables. In particular, we add lagged firm-level control variables to the model (x_{ft-1}) – namely, proxies for size (total assets), productivity (value added per employee), the financial stance (leverage, measured as total debt over total assets), and the number of bank relationships.[17]

[16] Relatedly, Borenstein and Rose (1995) find that financial distress in US airline markets induces firms to lower output prices, and Lenzu et al. (2019) find that Belgium firms, on average, reduce output prices when hit by a negative bank-loan supply shock.

[17] As noted before, we also control for a firm's use of labor and capital inputs and the corresponding interaction term whenever we rely on the baseline Cobb-Douglas markup proxy.

In equation (7), we allow the effect of the bank-loan supply shock variable on the markup policy of the firm to vary according to its reliance on bank financing, by including an interaction term between $BankShock_{ft}$ and $BankLoansDue_f$. $BankLoansDue_f$ refers to the share of short-term bank loans in a firm's total assets (i.e. loans with a maturity of less than one year) that we interpret as a measure of exposure to bank-loan supply shocks, as mentioned above. Note that allowing for this type of non-linearity is consistent with other recent studies (e.g. Amiti and Weinstein, 2018; Bucă and Vermeulen, 2017; Lenzu et al., 2019).

During most of the subsequent analysis, we fix the variable $BankLoansDue_f$ according to its pre-sample values. In particular, for each firm, we compute the mean short-term loan to asset share across the pre-sample years 2004 to 2006. This strategy implies that this exposure variable can be credibly considered exogenous, assuming that firms did not anticipate the financial crisis period.[18] However, it has the potential drawback of being a less informative measure of effective exposure for later years in the estimation sample. We therefore also experiment in some specifications with firms' one year lagged short-term loan share.[19]

4.3 Results

We start by discussing the results for the full sample period (2007-2013). In the first column of the Table 4, $BankShock_{ft}$ is not interacted with the exposure variable. The coefficient of interest is very small and statistically insignificant, which does not change when we add firm-level control variables as reported in column 2. However, this is no longer the case if we interact $BankShock_{ft}$ with $BankLoansDue_{ft-1}$ in column 3. In particular, the results indicate that markup responses to bank-loan supply shocks are indeed heterogeneous across firms. Firms that are not reliant on short-term bank loans lower their markups in response to negative bank-loan supply shocks. By contrast, the higher a firm's share of short-term bank credit in total assets, the less it reduces markups in response to negative loan supply shocks, and, in fact, it raises markups if it is highly dependent on this type of bank credit. In column 4, we present corresponding results based on the exposure measure fixed during the pre-sample period, which leads to qualitatively similar results.

[18] For firms first appearing in the sample later, we only consider firms entering up to the year 2007; i.e. the year before the crisis fully unfolded due to the collapse of Lehman Brothers in September 2008. For these firms, we consider their share of short-term loans in 2007 to be pre-sample and we include the firms in the main analysis only from 2008 onwards.

[19] Whenever we include an interaction term in the model, we always add both main effects to the regression; i.e. when using the lagged exposure measure, $BankLoansDue_{ft-1}$ is also included. Note that this is redundant when we rely on pre-sample values since the main effect will be captured by firm fixed effects.

Table 4: Main results: Effect on markups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$
BankShock	0.001	0.000	0.002**	0.002	0.002**	0.002*	0.004***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
BankShock × *Bank loans due (t-1)*			-0.017***				-0.022***	
			(0.006)				(0.008)	
BankShock × *Bank loans due*				-0.012**				-0.027***
				(0.006)				(0.009)
Observations	13,626	13,626	15,522	13,626	8,200	8,200	8,558	8,200
Firms	3,007	3,007	3,718	3,007	2,602	2,602	2,776	2,602
R^2	0.273	0.283	0.271	0.283	0.331	0.342	0.341	0.343
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demand control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Period	2007-2013	2007-2013	2007-2013	2007-2013	2007-2010	2007-2010	2007-2010	2007-2010

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered (firm-level) standard errors in parentheses. All regressions contain (3-digit) sector-year fixed effects and controls for credit demand (i.e. $\tilde{\alpha}_{ILSR,t}$), size (lagged assets), productivity (lagged value added per employee), leverage (lagged total debt in total assets), and lagged number of bank relations. Additionally, we add firms' labor and capital inputs as well as a corresponding interaction term to the models. R-squared refers to the (adjusted) within R-squared. Dependent variable shown in the second row of the table.

A number of empirical studies emphasise the role of credit supply shocks in Germany during the financial crisis period (e.g. Abbassi et al., 2016; Puri et al., 2011). Taking this evidence into consideration, we re-estimate the previous model focusing exclusively on this period (2007-2010). The results in columns 5 to 8 of Table 4 present a broadly similar picture, while the effects are generally reinforced.

Two aspects are therefore key for our main results. The first is the role of the exposure of the firms to bank-loan supply shocks, captured by the share of short-term bank loans in total assets, and the second is related to the crisis period.

4.3.1 The importance of short-term bank loans

One concern regarding the previous findings may be that a high share of short-term loans in total assets is an indication of a general balance sheet weakness of the firm rather than a measure of its exposure to bank-loan supply shocks. If this is the case, the effect that we find should not be restricted to bank-dependent borrowers but be related to a firm's leverage more generally (e.g. Bucă and Vermeulen, 2017). We investigate this possibility by considering, respectively, interaction effects with the shares of total debt, total short-term debt, and non-bank related short-term debt in firms' total assets. The results presented in columns 2 to 4 of Table 5 confirm that only bank-related debt acts as a relevant moderator of bank-loan supply shocks on markups. In the last two columns of this table, we also show that when hit by a bank-loan supply shock, firms adjust their markup according to their reliance on short-term bank loans as opposed to bank-loan financing more generally. In particular, while we also obtain a statistically significant interaction term when considering the sum of firms' short and long-term loans (column 5), results in column 6 show that this effect is fully driven by the share of bank loans due in total assets. We consider this supporting evidence that a high share of short-term loans in total assets makes firms especially vulnerable to bank-loan supply shocks.

Table 5: Role of bank loans due

	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$
BankShock	0.005***	0.011**	0.004	0.002*	0.006***	0.003*
	(0.002)	(0.005)	(0.003)	(0.001)	(0.002)	(0.002)
BankShock × *Bank loans due*	-0.027***					
	(0.009)					
BankShock × *Total debt*		-0.012*				
		(0.007)				
BankShock × *Total debt due*			-0.005			
			(0.006)			
BankShock × *Non-bank debt due*				0.000		
				(0.000)		
BankShock × *Total bank loans*					-0.016**	
					(0.006)	
BankShock × *Bank loans not due*						-0.006
						(0.008)
Observations	8,200	8,200	8,200	8,169	8,200	8,200
Firms	2,602	2,602	2,602	2,592	2,602	2,602
R^2	0.343	0.342	0.342	0.345	0.343	0.342
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Demand control	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Period	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered (firm-level) standard errors in parentheses. All regressions contain (3-digit) sector-year fixed effects and controls for credit demand (i.e. $\tilde{\alpha}_{ILSR,t}$), size (lagged assets), productivity (lagged value added per employee), leverage (lagged total debt in total assets), and lagged number of bank relations. Additionally, we add firms' labor and capital inputs as well as a corresponding interaction term to the models. R-squared refers to the (adjusted) within R-squared. Dependent variable shown in the second row of the table.

4.3.2 The role of the crisis

The empirical literature on bank-loan supply shocks does not provide conclusive evidence on whether these shocks matter for firm outcomes during "normal times" or only during recession periods. While some recent papers show evidence that credit supply shocks do, in fact, play a role during expansions (e.g. AW, DJJMS), other recent studies find real effects mostly in times of crisis (e.g. Greenstone, Mas, and Nguyen, 2020; Gilchrist, Siemer, and Zakrajsek, 2018; Alfaro et al., 2021).[20] The latter finding seems in line with the results of Jiménez, Mian, Peydró, and Saurina (2020), who show that credit supply booms (as opposed to credit supply contractions) have little bearing on firm level outcomes. Consistent with the idea of asymmetric effects of credit supply shocks, Manaresi and Pierri (2018) find that while negative shocks reduce firms' productivity growth, positive shocks do not exhibit significant effects.

In Table 6, we investigate the role of the crisis period and of more positive and more negative bank-loan supply shocks for our results. To this end, we transform the continuous bank shock variable into a dummy variable that equals one if the bank shock is below the median value in a given year. We then consider two empirical frameworks. First, we use the baseline setup from before, where we simply replace the continuous loan supply shock variable with the newly constructed dummy variable. Second, we slightly adjust the empirical approach by replacing the level of the markup as the dependent variable by

[20] Some recent macro studies using VAR models also find that financial shocks matter less or even not at all during expansions (see Colombo and Paccagnini, 2020; Corsello and Nispi Landi, 2020).

its growth rate and removing the firm fixed effects from the model. Thus, we compare the changes in markups of firms with more negative bank-loan supply shocks to those with more positive ones. In both setups, we interact the credit supply shifters with firms' shares of bank loans due in total assets.

Table 6: Role of negative shocks and the crisis period

	(1)	(2)	(3)	(4)
	$\ln(\mu^{cd})$	$\Delta\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\Delta\ln(\mu^{cd})$
$\mathbb{1}(BankShock < p50)$	-0.007**	-0.004	0.000	-0.005*
	(0.003)	(0.003)	(0.003)	(0.003)
$\mathbb{1}(BankShock < p50) \times Bank\ loans\ due$	0.035**	0.033**	0.001	0.031**
	(0.016)	(0.017)	(0.017)	(0.016)
Observations	8,200	7,397	6,416	5,869
Firms	2,602	2,573	2,698	2,586
R^2	0.342	0.356	0.172	0.195
Firm FE	Yes	No	Yes	No
Industry × year FE	Yes	Yes	Yes	Yes
Demand control	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Period	2007-2010	2007-2010	2011-2013	2011-2013

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered (firm-level) standard errors in parentheses. All regressions contain (3-digit) sector-year fixed effects and controls for credit demand (i.e. $\tilde{\alpha}_{ILSR,t}$), size (lagged assets), productivity (lagged value added per employee), leverage (lagged total debt in total assets), and lagged number of bank relations. Additionally, we add firms' labor and capital inputs as well as a corresponding interaction term to the models. R-squared refers to the (adjusted) within R-squared whenever the regressions contain firm fixed effects. Dependent variable shown in the second row of the table.

Considering first the baseline estimation approach and the sample period 2007 to 2010, the results in column 1 suggest that firms with more negative bank-loan supply shocks tend to lower their markups if they are not highly dependent on short-term bank financing. By contrast, the positive and significant interaction term implies that the larger the share of short-run bank debt in firms' total assets, the less they reduce their markups, and instead increase them for high shares of short-term loans in total assets. These results suggest that our previous findings are indeed mainly due to negative movements in the credit supply shifters. The results in column 2 confirm this conjecture using the alternative empirical setup that relies on the growth in markups as the dependent variable.

In terms of quantitative implications, the results in column 1 imply that firms with a low share of short-term loans in total assets reduce their markups by around 0.6% if they experience a relatively more negative bank-loan supply shock. However, this effect turns positive for a loan share of 18%, implying that around a quarter of the firms raise their markups in response to a more negative credit supply shock. In fact, this increase can reach 0.6% for firms that rely heavily on short-term loans. Column 2 reports broadly consistent evidence, but translates into a higher number of firms increasing markups in response to negative financial shocks. While these numbers may appear small, we argue that they are non-negligible. On the one hand, when evaluating the effects against an average change in markups of around zero in the estimation sample, even small numbers can imply large effects in relative terms. On the other hand, and more importantly, our results clearly provide lower bound estimates. This is because, as noted above, firms in USTAN are relatively large and financially sound when compared to the population of firms. Consequently, credit supply shocks are expected to matter less for these firms.

In columns 3 and 4, we repeat these regressions for the post-crisis period (2011 to 2013), where pre-sample values are now based on averages across the years 2008-2010.

Our baseline approach suggests that firms adjusted their markups solely during the crisis period (column 3). However, the alternative empirical setup does not corroborate this result. It suggests that firms relying on short-term bank financing tend to raise their markups when experiencing an unexpected cut in credit supply during the post-crisis period too. Hence, we do not find conclusive evidence for this time period. On the one hand, this may be related to econometric issues, arising from the short post-crisis sample period. On the other hand, it may reflect the characteristics of our firm-level data, which tends to contain larger firms that are usually in a relatively solid financial position. In particular, these firms may in general be better equipped to deal with less severe credit supply shocks during more normal times. We therefore interpret these results with caution and focus on the crisis period in the remainder of the paper.

4.4 Robustness and further results

The Supplementary Appendix presents a series of sensitivity checks. First, we consider a variety of alternative markup proxies, by using measures consistent with a translog production function, by considering indicators obtained from diverse adjustments of the control function, and proxies that do not require any estimation; namely, measures consistent with simple cost shares and price cost margins. Moreover, we adopt different sets of control variables and fixed effects, estimate weighted regressions, experiment with different transformations of the dependent variable and investigate the role of the largest firms for the results. Our main findings are generally robust to these checks. Finally, we investigate whether the effect of bank-credit supply shocks on markups also materialises with a lag and find that the effect of the bank-credit supply shocks occurs contemporaneously.

5 Evidence on economic channels

In this section, we aim to better understand the economic channels that could be driving our findings. The previous results suggest that firms adjust their markups to bank-loan supply shocks according to their reliance on short-term bank credit. We find that firms heavily reliant on this type of financing reduce their markups less intensely, or even increase them, compared to their less exposed peers in response to negative bank-loan supply shocks. In fact, this result is consistent with the customer-market channel. Below, we discuss empirically the plausibility of this hypothesis, also in light of the fire-sales mechanism discussed in section 4.1. To this end, we rely both on information obtainable from firms' balance sheets and on specific industry characteristics.

5.1 Evidence from balance sheet items

As discussed above, firms can adopt a fire-sales strategy when facing a negative bank-credit supply shock. This coping strategy predicts that liquidity constrained firms sell off their inventories at lower prices in order to keep their cash flow up (e.g. Kim, 2021). In particular, it implies that firms with a high initial stock of inventories or small cash holdings reduce output prices (and markups) compared to firms with lower inventories when hit by negative financial shocks. To investigate the role of this mechanism, we interact the bank-loan supply shock variable with firms' lagged inventories and cash holdings, respectively.[21]

In column 1 of Table 7, we first present our baseline results to ease comparisons across models. Columns 2 and 3 show that the interaction term between the bank shock variable and lagged inventories has the expected positive sign, corroborating the prediction above. Thus, firms with a large stock of inventories (relative to their sales) in $t-1$ tend to lower their markup more when hit by a negative bank-loan supply shock than firms with few inventories at their disposal to use for fire sales. Columns 4 and 5 repeat this exercise, but replace inventories with cash holdings. We again obtain a positive and significant coefficient, suggesting that firms that are more liquid in terms of cash holdings tend to lower their markups by more when confronted with an unexpected cut in lending. Hence, while we also find some support in the data for the fire-sales mechanism by looking at firms' inventories, the positive coefficient on the interaction term with cash holdings rather speaks to the relevance of the customer-market channel, as firms with low liquidity in terms of cash holdings actually tend to raise markups.[22]

Table 7: Role of other balance sheet items

	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$
BankShock	0.005***	-0.001	0.003	0.001	0.004**	0.004***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
BankShock × *Bank loans due*	-0.027***		-0.030***		-0.023**	
	(0.009)		(0.009)		(0.009)	
BankShock × *Inventories (t-1)*		0.016*	0.018**			
		(0.008)	(0.008)			
BankShock × *Cash holdings (t-1)*				0.033**	0.024*	
				(0.013)	(0.014)	
BankShock × *Bank loans due adj.*						-0.020***
						(0.007)
Observations	8,200	8,193	8,193	8,200	8,193	8,200
Firms	2,602	2,600	2,600	2,602	2,600	2,602
R^2	0.343	0.344	0.345	0.343	0.345	0.343
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Demand control	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Period	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered (firm-level) standard errors in parentheses. All regressions contain (3-digit) sector-year fixed effects and controls for credit demand (i.e. $\tilde{\alpha}_{ILSR,t}$), size (lagged assets), productivity (lagged value added per employee), leverage (lagged total debt in total assets), and lagged number of bank relations. Additionally, we add firms' labor and capital inputs as well as a corresponding interaction term to the models. R-squared refers to the (adjusted) within R-squared. Dependent variable shown in the second row of the table.

Having said this, it is important to bear in mind that both moderating variables (i.e. inventories and cash holdings) are introduced in the model with one year lagged values, thus making them more prone to endogeneity concerns. For instance, in the course of the sample period, firms may have started to hoard cash in anticipation of the more severe shocks occurring in 2009. In fact, introducing interaction terms based on pre-sample values in the model does not deliver significant coefficient estimates for either

[21] We define inventories as the ratio of inventory stocks to firms' sales and cash holdings as the share of cash and cash equivalents in total assets.

[22] Note that the findings regarding cash holdings are consistent with the idea that firms can use their cash to mitigate refinancing risk, as suggested by Harford, Klasa, and Maxwell (2014).

variable. Hence, the previous results have to be interpreted with some caution, offering only suggestive evidence.

In a complementary way, we use an alternative measure of liquidity to further clarify the interpretation of our results. This measure is obtained by deducting firms' cash holdings from firms' short-term bank loans in total assets. We use this alternative measure in order to capture a broader concept of liquidity by also taking into account the cash holding position of the firm. As before, we interact the bank-loan supply shifters with this alternative measure of exposure using pre-sample values. Reassuringly, we find in column 6 that this interaction term is negative and highly statistically significant, confirming a highly heterogeneous markup response across firms according to their exposure to these shocks. Firms that are more exposed in terms of potential liquidity shortages raise their markups in response to negative bank-loan supply shocks, while less exposed firms reduce them, corroborating the customer-market mechanism.

5.2 Evidence from industry characteristics

According to section 4.1, customer-market-based theories predict that when switching costs are high, firms have additional incentives to build a customer base. Thus, the price (markup) gap between the firms that face a financial shock and the remaining firms is expected to be higher compared to products and sectors which are characterized by low switching costs. In other words, the heterogeneity in markup behavior across firms in the presence of financial shocks is further amplified when switching costs are high. In this subsection, we adopt proxies for switching costs in order to further investigate the economic channels that drive our results in the spirit of Secchi et al. (2016).

Proxies for switching costs are notoriously difficult to find. We gather a set of alternatives from several strands of the economics literature, namely: i) the elasticity of substitution across varieties of differentiated goods; ii) R&D intensity; and iii) a measure of the persistence of firms' market shares.

First, we consider estimates of the elasticity of substitution across varieties of differentiated goods obtained from Broda and Weinstein (2006).[23] This is a measure of horizontal product differentiation, which relates to switching costs under certain conditions (see, e.g. Gehrig and Stenbacka, 2004, where switching costs increase in the degree of horizontal differentiation). More generally, the elasticity of substitution provides some indication of firms' market power in a given industry and it seems plausible to expect that markup increases in response to negative bank-loan supply shocks are only possible if firms have some degree of price-setting power. Second, we resort to R&D intensity as an alternative measure of horizontal product differentiation, as adopted by Kugler and Verhoogen (2012). While R&D intensity can, in general, also relate to vertical product differentiation, Kugler and Verhoogen (2012) provide some evidence of the reliability of this measure in terms of horizontal product differentiation.[24] Third, we adopt a measure of the market share persistency of a firm in a given industry. This indicator aims to cap-

[23] They provide the elasticities at detailed product level. We use the correspondence tables between HS 6-digit and CPA product codes (which directly link to NACE industries) as well as correspondence tables that link HS 6-digit product codes over time, which are available from Eurostat and the UN.

[24] We compute this measure as the ratio of R&D expenditure over gross output at the 2-digit industry level, using data for Germany for the 2004-2006 period from the OECD.

ture state dependence in consumer preferences, which also can relate to switching costs (Shcherbakov, 2016).[25] We introduce these proxy variables in our empirical framework, by dividing industries into two groups: those that are relatively more prone to customer-market features and those less prone to such characteristics. We then re-estimate our baseline specification separately for the two groups of industries. Table 8 does indeed suggest that our results are generally driven by industries that are relatively more likely to feature customer-market characteristics according to these measures. Therefore, we conclude that our findings are mostly consistent with the customer-market channel.[26]

Table 8: Role of industry characteristics

	Elasticity of substitution		Persistence of market shares		R&D intensity	
	High (≥p66)	Low (<p66)	High (≥p33)	Low (<p33)	High (≥p33)	Low (<p33)
	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$
	(1)	(4)	(3)	(4)	(5)	(6)
BankShock	0.003	0.007***	0.006***	0.003	0.006***	0.002
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
BankShock × *Bank loans due*	-0.018	-0.034***	-0.034***	-0.005	-0.033***	-0.004
	(0.014)	(0.011)	(0.010)	(0.018)	(0.011)	(0.013)
Observations	2,743	5,399	5,799	2,397	6,084	2,116
Firms	876	1,722	1,851	765	1,919	687
R^2	0.360	0.336	0.327	0.365	0.343	0.327
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Demand control	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Period	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered (firm-level) standard errors in parentheses. All regressions contain (3-digit) sector-year fixed effects and controls for credit demand (i.e. $\tilde{\alpha}_{ILSR,t}$), size (lagged assets), productivity (lagged value added per employee), leverage (lagged total debt in total assets), and lagged number of bank relations. Additionally, we add firms' labor and capital inputs as well as a corresponding interaction term to the models. R-squared refers to the (adjusted) within R-squared. Dependent variable shown in the third row of the table.

6 Economic implications

In this section, we aim to understand the economic implications of the way German manufacturing firms adjust their markup policies in the presence of bank-credit supply shocks over the financial crisis period. Our findings corroborate the mechanism modelled by Gilchrist et al. (2017) and Chevalier and Scharfstein (1996), which has important implications for the business cycle properties of aggregate markups. In fact, their models predict that financial frictions induce a countercyclical dimension in the behaviour of the aggregate markup, which we empirically test below.

The central idea of the theoretical framework is that liquidity considerations matter for the price-setting behavior of firms operating in customer markets, thereby generating heterogeneity in markup dynamics across firms due to varying liquidity needs. In a

[25] We obtain these measures by regressing the log of firms' current market share on the lagged values (accounting for firm, industry and year fixed effects), using a system-GMM estimator. Market shares are defined at the four-digit industry level, while we estimate the models by three-digit industries. We obtain qualitatively similar results when using OLS both with and without firm fixed effects. These results are available upon request.

[26] In line with this conclusion, in additional regressions presented in the Supplementary Appendix, we find that bank-loan-supply shocks affect firms' markups mostly contemporaneously, while the effects on firms' market shares appear to be more persistent, which seems consistent with a trade-off between current liquidity needs and medium-term market share considerations.

recession, firms are more prone to face liquidity shortages, due to additional challenges in raising external funding. This is particularly the case for firms exposed to liquidity risk, as argued before. Firms that see this risk materialize react by increasing prices and markups at odds with their unconstrained peers in order to sustain liquidity. As a consequence, these heterogeneous markup adjustments can alter the cyclical property of the aggregate markup.

A few recent studies explore the role of firm heterogeneity for aggregate markup dynamics in the context of the long-standing debate on the cyclical properties of the aggregate markup (see Hall, 1988, for instance). The growing availability of granular data sources and the lack of consensus on the cyclical properties of the aggregate markup both from a theoretical and an empirical perspective (e.g. Nekarda and Ramey, 2020) suggest that factors related to firm heterogeneity may provide some indication regarding the sources of this lack of consensus. Burstein et al. (2020) provide one important study in this regard, showing that, besides aspects related to market structure and the level of data aggregation, firm characteristics are a key determinant of markup cyclicality. In particular, the authors find that markups of smaller firms tend to be counter-cyclical, while those of large firms tend to be pro-cyclical, such that the cyclicality of the aggregate markup depends, among other factors, on the firm size distribution in the sector or economy. Empirical findings by Hong (2019) confirm that the cyclicality of the aggregate markup varies by firm size. He rationalizes this finding in a model featuring customer markets, where small firms have a higher exit probability, implying that they become more myopic during recessions. Due to the stickiness of the customer base, these firms thus have an incentive to raise markups during downturns in order to boost short-term profits. Our works ties in closely with these studies by investigating the role of another firm characteristic, namely financial constraints, in generating heterogeneous markup responses and analyzing their implication for the aggregate markup.

While our aim here is not to discuss whether the aggregate markup is in fact countercyclical, we test whether the interaction of bank-credit supply shocks and firms' exposure to these shocks may induce a countercyclical dimension in the aggregate markup. Moreover, we assess whether this is particularly the case in sectors relatively more prone to customer market features, as predicted by the models discussed above. To this end, we include a triple interaction between the bank-loan supply shock variable, the measure of exposure of the firm to the shocks, and a business cycle proxy. Following Burstein et al. (2020), we use output growth at the (2-digit) industry level for this purpose. Since we are investigating aggregate implications, we resort to weighted regressions, using firms' intermediate input expenditures as the weighting variable (which is in line with the work of Edmond, Midrigan, and Xu, 2018) that we fixed during the pre-sample period.

Table 9 reports the estimation results. Column 1 shows that the triple interaction is estimated to have a highly significant and positive coefficient, suggesting that firms exposed to liquidity risks raise markups more strongly in response to negative bank-loan supply shocks if sectoral output is plummeting. Hence, this corroborates the hypothesis postulated above. In Columns 2 to 7 of this table, we repeat the same exercise, but we once more divide industries according to the intensity of customer-market features using the proxy variables described in sub-section 5.2. We find a positive and significant triple interaction effect only in industries that are relatively more prone to customer market features, providing further support for the mechanism under consideration.

Table 9: Markup cyclicality

	All	Elasticity of substitution		Persistence of market shares		R&D intensity	
		High ($\geq$p66)	Low (<p66)	High ($\geq$p33)	Low (<p33)	High ($\geq$p33)	Low (<p33)
		$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$	$\ln(\mu^{cd})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BankShock	0.011***	0.015***	0.003	0.012***	0.006*	0.011***	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
BankShock × *Bank loans due*	-0.071***	-0.105***	-0.024	-0.076***	-0.045**	-0.068***	-0.017
	(0.014)	(0.017)	(0.016)	(0.014)	(0.022)	(0.014)	(0.017)
BankShock × *Industry gowth*	-0.023**	-0.046***	-0.002	-0.033**	0.014	-0.023*	0.007
	(0.011)	(0.017)	(0.014)	(0.013)	(0.020)	(0.012)	(0.022)
BankShock × *Bank loans due* × *Industry gowth*	0.213***	0.192	0.155**	0.261***	0.104	0.212**	0.017
	(0.066)	(0.136)	(0.076)	(0.071)	(0.158)	(0.084)	(0.090)
Observations	8,181	2,737	5,386	5,786	2,391	6,067	2,114
Firms	2,593	873	1,716	1,845	762	1,911	686
R^2	0.561	0.680	0.468	0.579	0.547	0.578	0.592
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demand control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered (firm-level) standard errors in parentheses. All regressions contain (3-digit) sector-year fixed effects and controls for credit demand (i.e. $\tilde{\alpha}_{ILSR,t}$), size (lagged assets), productivity (lagged value added per employee), leverage (lagged total debt in total assets), and lagged number of bank relations. Additionally, we add firms' labor and capital inputs as well as a corresponding interaction term to the models. Regressions are weighted according to firms' pre-sample material expenditures. R-squared refers to the (adjusted) within R-squared. Dependent variable shown in the third row of the table.

7 Concluding remarks

This paper analyzes the role of financial frictions in markup adjustments at the firm level. To this end, we estimate firm-specific bank-loan supply shifters from matched bank-firm-level data. We combine insights from recent contributions regarding the estimation of these shifters (in particular, Amiti and Weinstein, 2018; Degryse et al., 2019) and find that they play a significant role for firm-level loans. Moreover, we estimate firm-specific and time-varying markups, in line with De Loecker and Warzynski (2012), accounting for recent adjustments to the methodology (Brandt et al., 2017; Bond et al., 2021). We uncover new findings regarding the way German manufacturing firms changed their markups when facing credit supply shocks over the period 2007-2013.

In particular, we find that firms adopt heterogeneous markup policies in the presence of exogenous and firm-specific credit supply shocks depending on their exposure to these shocks. Over a period that covers the financial crisis, firms heavily exposed to these shocks raised their markups, while firms less severely affected by credit supply shocks lowered them. After exploring various dimensions of heterogeneity in the data (e.g. in relation to balance sheets and industry characteristics), we conclude that our results are mostly consistent with models featuring customer markets and heterogeneous financial frictions (e.g. Chevalier and Scharfstein, 1996; Gilchrist et al., 2017), suggesting that financially constrained firms may raise markups in order to sustain liquidity in the short run. These findings have important economic implications, for example, with respect to the cyclical behavior of markups. In fact, our results suggest that financial shocks may contribute a countercyclical component to the behavior of the aggregate markup.

Finally, we should mention that our empirical analysis is consistent with a partial equilibrium framework. Consequently, we abstract from potential general equilibrium effects and spillover linkages across firms that could have been at play during our sample period. The presence of such additional channels could imply that there were also indirect effects on prices and markups that could reinforce or mitigate the direct effect that we document. In this regard, our work provides some potentially fruitful avenues for future research. On the theoretical side, combining models that incorporate customer market features with heterogeneous firm models that include variable markups and financial frictions would be highly relevant to achieving a better understanding of firms' markup and price setting behavior. For instance, the debate on the role of the business cycle properties of the markup could benefit from exploring such sources of firm heterogeneity. At the same time, the use of data sets that include information about firm-level prices would allow investigating in more detail firms' markup responses to financial shocks, distinguishing, for instance, between price and marginal cost adjustments.

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